Essays in Political Economy and Crisis

Laurence Henry Wilse-Samson

Submitted in partial fulfillment of the

requirements for the degree

of Doctor of Philosophy

in the Graduate School of Arts and Sciences

COLUMBIA UNIVERSITY

2014

ABSTRACT

Essays in Political Economy and Crisis

Laurence Henry Wilse-Samson

My research has two main themes – the link between political economy and economic development, and the causes and effects of economic crises and long recessions. In some cases my interests take me into the field of economic history. This dissertation samples from some of this ongoing research. I find it most informative to approach these issues which are of macroeconomic interest using the techniques of applied microeconomics and the papers here all draw on various of these techniques. I am also very interested in relatively new ways of sourcing of data – including using geographic information systems, methods of transforming large corpuses of text into data, and mining court case records. This dissertation includes the application of some of these methods.

The relationship between economic development and democracy is key in political economy. Many commentators have suggested that economic growth increases support for democracy. One proposed mechanism is that modernization, by reducing the demand for low-skilled labor, increases the willingness of elites, particularly in agriculture, to extend the franchise. In Chapter 1 I use subnational variation in South Africa to test this mechanism. I employ national shocks to the mining sector's demand for native black workers and cross-sectional variation in labor market competition induced by apartheid to estimate the effect of black labor scarcity on wages, capital intensity, and changes in partisan voting preferences. I find that reductions in the supply of foreign mine labor following the sudden withdrawal of workers from Malawi and Mozambique (and the increased demand for native black workers) increased mechanization on the mines and on farms competing with mines for labor. I then show that these induced structural changes resulted in differential increases in pro-political

reform vote shares in the open districts relative to closed districts, even as mining districts became more conservative and voted more to maintain the non-democratic regime.

Chapter 2 also explores issues related to the close relationships between economic and political instituions. In this chapter, together with my coauthor Sébastien Turban, we show how sovereign debt spreads are impacted by news about executive term limits. Political institutions matter for countries' cost of borrowing. We use an event-study to analyze the markets' response to new information about executive term limits over 101 events in seven emerging markets. Investors respond significantly to news about restrictions on those limits, lowering risk spreads. The one day abnormal returns following news about a restriction is 2 percentage points. Over ten days, the cumulative abnormal return is 5 percentage points. News about term limits extensions are not significant in the medium run. The results are robust to a non-parametric test and are confirmed when looking at the behavior of sovereign CDS prices.

Chapter 3 starts the second part of this dissertation which is an investigation into the housing-related aspects of the recent crisis which began as a "subprime crisis" before it became "the Great Recession". In particular, this chapter focuses on the institutional details underpinning these markets. It also serves to set up the analysis in the following chapter which looks at one of the potentially important mechanisms which amplified the severity of the housing crisis. One important feature emerging from this analysis is that it appears that protections for home mortgage creditors were strengthened in the period preceding the subprime crisis. This may have both increased lending, but also the difficulty of modifying home loans ex post. This is more problematic to the extent that there are negative externalities from foreclosures.

Chapter 4, co-authored work with David Munroe, shows that completed foreclosures cause neighboring foreclosure filings. We estimate this relationship using administrative data

on home foreclosures and sales in Cook County, IL, instrumenting completed foreclosures with randomly assigned chancery-court judges. A completed foreclosure causes 0.5 to 0.7 additional foreclosure filings within 0.1 miles, an effect that persists for several years. Contagion is driven by borrowers on the margins of default, not those severely at risk. We find evidence that borrowers learn about lender behavior from neighboring foreclosures. Finally, a foreclosure causes an increase in housing sales among relatively low-quality properties.

Table of Contents

List of Figures

Acknowledgements

In an endeavour such as this one accumulates many debts, some of which one cannot hope to ever repay. Suresh Naidu helped me in innumerable ways: providing constant feedback, valuable criticism and advice, as well as helping to shape my interests and introducing me to fascinating literatures in economics and economic history. Ethan Kaplan's course in Political Economy, and subsequent collaboration on topics around the Great Recession have convinced me of the importance of applying applied microeconomic methods to the study of macroeconomics and political economy. He has also been incredibly supportive of my research and always provided important feedback. I have had the privilege of working for Joseph Stiglitz the last 3 years which has been a constant source of ideas and inspiration. He has led me to work on the Great Depression and constantly motivated me to try and address topics and questions that I really care about. I would also like to thank Patrick Bolton whose timely course on the Financial Crisis I enjoyed most of any while here at Columbia. I was fortunate to be able to work for him on the Sovereign Wealth Fund Research Initiative which again granted me many opportunities. Alessandra Casella patiently helped me to see the value of careful theory and convinced me of the role of lab experiments in economics. I am thankful for all of her support. Perry Mehrling introduced me to exciting and innovative ideas in monetary theory which I would never otherwise have encountered, and I am grateful to him for long discussions and for exposing me to the field of the history of economic thought. A number of Columbia faculty provided important ideas for (and criticisms of) my papers including: Jonas Hjort, Supreet Kaur, Wojciech Kopczuk, Chris Mayer, Brendan O'Flaherty, Tomasz Piskorski, Christian Pop-Eleches, Bernard Salanié, Miguel Urquiola and Eric Verhoogen. I learnt a great deal from the teaching of Yeon-Koo Che, Katherine Ho, Navin Kartik, and Mike Riordan. I was also fortunate to be the TA for Vivette Ancona,

Graciela Chichilnisky, and Bruce Greenwald from whom I learnt a great deal.

My work in South Africa benefited from the input of participants at the Economic Research Southern Africa conference on the Economics of Apartheid held in Cape Town in March 2013, and the Columbia development seminar. My sincere thanks go to Kathy Brookes at the University of Johannesburg for assistance with the TEBA mine recruitment archive, and Celia Matsoma at the National Library in Pretoria for advice on historical production censuses. Japie Kruger at the South African Department of Agriculture was also of great assistance, as was Maurizio of MadMappers who provided help with sourcing and georeferencing historic magisterial district boundaries. My dear friend Jumani Clarke of the University of Cape Town helped hunt down old electoral maps kept in storage at the Department of Rural Development and Land Reform in Cape Town.

My work on the foreclosure crisis was aided by Alicia Horvath at Record Information Services Inc. who helped greatly with data. Financial assistance from the Program for Economic Research at Columbia University is also gratefully acknowledged.

I have learnt most from my collaborators. I have discussed so many ideas around technological unemployment and inequality with Chris Boone that I can no longer distinguish which ideas are mine and which are his. I have been chastened and inspired by David Munroe's fierce work ethic and unflinching academic honesty. He has also taught me most of what I know about housing markets. Sébastien Turban has provided extensive feedback on almost everything I have written, patiently explained to me how various models are constructed and effects estimated, as well as indulged my various digressions and wrong steps. Somehow I think we have had fun. I have also benefited from the kindness and other human capital of Ritam Chaurey, Jonathan Dingel, Jacob Jensen, An Li, Corinne Low, Miguel Morin, Mike Mueller-Smith, Jesse Naidoo, Giovanni Paci, Raul Sanchez de la Sierra, and Hyelim Son. Luke Jordan carefully read and provided valuable feedback on some of these chapters.

My deepest gratitude is to Kate, Laura, Michèle, and Nathan. Their patience and support kept me going even in the darkest times after dad passed away. It is to dad and them that this work is dedicated.

To J.H., in loving memory

Chapter 1

Structural Change and Democratization: Evidence from Rural Apartheid

Laurence Wilse-Samson

The relationship between economic development and political institutions is a long-standing area of interest in political economy. In particular, scholars going back to Barrington Moore have stressed the interest of agricultural landowners in non-democratic political institutions, owing to their dependence on labor-repressive economic institutions that would not survive democratic decision making (Moore 1966). As agriculture modernizes, agricultural elites have less need to maintain control over labor, and thus are more willing to extend the franchise.

Apartheid South Africa provides a good setting in which to examine these issues. It was non-democratic. Its economy remained dependent on agriculture for a long time.[1] And two sectors of the economy — mining and farming — were dependent on low-skilled labor. In its economic content, Apartheid was primarily a large set of tools for black labor allocation and control. As I outline below, the Apartheid state was deeply committed to managing black labor supply. The constraints on the movement of blacks were strict and, importantly, varied by location. Jobs were reserved along racial lines, and the 1913 Natives Land Act denied blacks property rights. In South Africa, therefore, white political preferences were crucially related to preferences over the control of black labor. Changes over demand for labor may, thus, have changed political preferences.

In this paper I focus on one component of the apartheid coalition:[2] the allocation of black labor between white farmers and mining companies that was moderated by successive white governments.[3] I describe a shock to this compact brought about by a sudden reduction in

1. Agricultural employment was 40% all employment as late as 1958 (Atkinson 2007).

2. I do not take the view that the 1948 election, which ushered in the National Party's official policy of 'Apartheid', differed in kind from the system that prevailed before. For that reason I will sometimes use 'segregationist' and 'Apartheid' interchangeably. I do argue, though, that the National Party was relatively more sensitive to the demands of the (especially smaller) farmers when it came to power than the United Party had been.

3. I put to one side the role of manufacturing — this is a political economy of *rural* apartheid.

the supply of foreign mine labor. Using novel datasets I have collected and digitized, I am able to provide some of the first evidence on the economic determinants of the stability of apartheid.

The state was attempting to manage two strategic concerns. On the one hand, there was an ideological long-run worry about being overpowered by the native majority — and hence a pull for geographic separation.[4] On the other, there were the politically powerful mining and farm sectors with their competing demands for black labor. Thus, one way of thinking about Apartheid politics is in terms of the difficulty of marrying the desire for long-run physical separation with the short-run sectoral needs for labor. The solution was a system of migrant labor to the mines, which helped to satisfy the mining demand for labor while accommodating the goal of separation. In addition, the acquisition of mine labor from beyond South Africa's borders helped alleviate some of stress caused by the competition for workers between the mining and agricultural sectors.

A shock to the supply of foreign labor may have contributed to the instability of the Apartheid regime. Specifically, the sudden withdrawal of workers from Malawi and Mozambique between 1974 and 1976 required an immediate increase of recruiting within South Africa. By comparing outcomes in South African districts where historically mine recruiting was permitted to areas where it was not, I show that the increase in mine recruiting generated spillovers onto the farming sector in the years following the shock, inducing higher agricultural wages and the greater adoption of combine harvesters. I then show that these shocks had indirect political consequences by looking at the results of elections — I present suggestive evidence that these changes raised support for parties favoring some reforms of the Apartheid system in the regions forced to modernize.

4. A charitable reading would be that there was an ideological belief that in the long-run, political logic required separate states for separate "nations".

The shock may also have helped to speed up important changes in labor organization on the mines. By examining mine-level outcomes, I show that in the years around the shock, the mines most dependent on foreign labor experienced relatively more strike action. Narrative evidence drawn from the annual reports of the Chamber of Mines and their recruiting company arms suggests that it was in response to these developments that the mining companies decided to raise wages, lengthen contractual terms for mineworkers, and increase mine mechanization. The mining companies took strategic steps to become less reliant on migrant labor, and lobbied the state for a 'stabilized' black labor force that was more permanently at the mines. When black unions were legalized, the National Union of Mineworkers became instrumental in the broader labor movement, which itself became a primary internal vector for the political demands of black citizens.[5] I show that in the elections following the shock that there was increased support for the right wing in the districts where mines were located.

This work informs the literature on democratic transitions. Acemoglu and Robinson (2012) characterize the institutions that prevailed in South Africa throughout most of the twentieth century as "extractive" and as creating a "dual economy" wherein white elites initiated policies designed to reduce competition from black Africans and create a "reservoir of cheap labor". But white elites were not homogeneous,[6] and the study of the effect of shocks to the Apartheid economy allows us to better understand the political equilibria which sustained the state. Democratization can be a way for elites to respond to protest through committing to future redistribution (and thus escape violent revolution). In other

5. Their role was reinforced following the reforms of 1979 which allowed economic protest while still limiting political organization. Note that, while the Natal strikes of 1973 were very important, it was not until 1979 — and in some sectors later — that black employees were recognized within the framework of the Industrial Conciliation Act.

6. The Anglo-Boer war ended in 1902. The four white settler colonies combined into the Union of South Africa in 1910. Afrikaans- and English-speaking whites — mostly descended from separate Dutch and British colonizations — were for some time largely divided socially and politically.

cases, insufficient voice might be met by repression or temporary transfers (Acemoglu and Robinson 2006). From its apex at the start of the 1970s until the democratic transition in 1994, Apartheid was in retreat.[7] During its decline, the government explored all these routes.

This research also speaks to an old question around the relationship between factor endowments and technological adoption and innovation: whether and under what circumstances does the scarcity of a particular factor induce the adoption of new technologies? Acemoglu (2010) shows that labor scarcity promotes technological advance when innovation reduces the marginal product of labor. Indeed, most of the analysis of this issue has arisen in the context of *labor* scarcity due to the number and variety of institutions directly affecting the availability of labor. These include numerous coercive labor systems: feudal institutions,[8] Masters and Servants Laws (Steinfeld 2001), and colonial systems such as the mine *mita* in Peru and Bolivia.[9] Habakkuk (1962) argues that relative technological advance in the United States over Great Britain was due to greater relative scarcity of labor in the former country. Hornbeck and Naidu (2013) argue that having a large amount of low wage labor available limited development in the US South. And controls on labor remain impor-

7. "Grand Apartheid" was the vision of a multi-state South Africa — solving the problem of accommodating black (or "tribal") self-determination — as expressed most forcefully by Prime Minister H.F. Verwoerd. His plan was for a white South Africa, with 10 "bantustan homelands" formed on the basis of the Trust land. Economic opportunity would be provided through "border" industries, that is white firms located just outside the border of a bantustan in which blacks could work on a migrant labor basis. Policies to reach this ultimate goal were implemented with some vigor from the 1960s until the early 1980s when the absurdity became entirely unsustainable. Furthermore, the western part of the Cape Province was designated a "Coloured Labor Preference Area", providing disincentives for firms hiring black labor in lieu of coloured labor. (This is apartheid era racial classification — the Apartheid government used 4 main racial classifications: black/African/Bantu; coloured; Indian/Asian; and white. The term "coloured" refers to South Africans of mixed racial descent).

8. Domar (1970)'s hypothesis is that serfdom arose in Russia in the 16th century to mitigate the scarcity of labor relative to land abundance

9. Dell (2010) shows that these institutions can have persistent effects on household consumption and child health.

tant. There are within country restrictions such as the *hukou* system in China and internal passport regimes within Russia and Ukraine. Even more important are the vast and complex systems governing labor movement across national borders which must shape, in ways we do not understand, development and democratization today.

The paper is organized as follows: In Section 1.1 I present the instruments of labor control — first in general, then particularly as related to the geography of separation between mining and farming recruitment. Section 1.2 introduces the data on which the analysis relies, and outlines the empirical strategy used to identify the effects and mechanism of interest. Section 1.3 presents results related to economic outcomes and section 1.4 discusses political effects. Section 1.5 concludes.

1.1 Apartheid labor controls

Apartheid's controls on labor were very strict. Unlike in some settings where borders are porous, the description in this section suggests that geographic and sectoral mobility was relatively difficult. Secondly, the extreme lengths to which the state went in order to secure black labor for various sectors indicates that this was a politically important policy dimension.

The South African economy, even as late as 1970, was dependent on low-skilled labor in agriculture and mining. As a share of the economically active black population in 1970, about 40% of blacks were employed in agriculture, forestry and fishing (2.26m people), while blacks employed in mining and quarrying comprised 11% (610k people).[10]

The state used a number of tools to maintain the supply of low wage labor. First, we can

10. There were fewer blacks in manufacturing at about 514k while about 1m blacks were employed in the service sector (Lombard and Stadler 1980).

think of tools where the state operates on the extensive margin. These included restricting land ownership, movement, and alternative employment opportunities for blacks — that is, lowering the value of their outside option. Second, there are tools where it operates on the intensive margin (through putting state power behind onerous and unequal contractual relations such as Masters and Servants Laws). Both were used.[11] In addition, policies could be classified in terms of whether the aim was slowing urban to rural flows of labor, or rural to rural flows; and whether the controls were at the urban or rural end. The segregationist regime, again, played on all those levers. The underlying logic was increasing 'reserve labor'.[12]

I distinguish between legislation with the general aim of lowering labor costs, and legislation directly targeted at preventing competition between farming and mining. In order to avoid labor market competition and keep wages low, whites negotiated competing claims on black labor through lobbying the state, rather than through increasing wages and improving working conditions (Lacey 1981). Below I first discuss the extensive and intensive margin before turning to the organization of mine labor recruitment and the specific geography of separation between the mining and farming sectors.[13]

1.1.1 The extensive margin of employment

I now sketch the main laws defining the labor framework. These consisted of laws restricting the lands blacks could own, and laws restricting the places to which, and conditions under which, blacks could move. Throughout this period, except for a few people in the Cape

11. One might also characterize this as increasing the supply of labor and improving labor productivity. A framework here is lowering the value of unemployment to the worker to improve worker discipline in the Shapiro-Stiglitz sense.

12. By "reserve labor" I mean workers that could be idled or used according to demand without shifting wages.

13. I do not focus here on the regulation of rural-urban flows, but the controls there were also strong.

province, for a few years, blacks were entirely politically disenfranchised.

The *Natives Land Act of 1913* restricted the land that blacks could own to "scheduled areas", comprising 7% of the country, whereas blacks were some 68% of the population at the time. The scheduled lands reflected the historical dispossession following wars of colonial conquest in the nineteenth century. These areas, also called "native reserves", would be ruled by "traditional" chiefs on the basis of customary law.[14]

The Natives Land Act restricted blacks to owning land (in common) only in the reserves. It also prohibited whites from owning land in the reserves. Blacks were to have a nominal source of subsistence land outside of white-owned farms to create a 'pool' from which mining companies could recruit migrant labor.[15] The mining companies pushed for the reserves to be extended while white farmers pushed back. Lacey (1981) for example argues that the expansion of the reserves was (p.4) "blocked by the farmers", who were concerned about labor scarcity. Whereas under the *1936 Native Trust and Land Act* the area of the reserves was expanded — the new demarcated territories were called the "released areas" (the total land now allocated to blacks comprised about 13% of the country) — this reflected mainly recognizing places where blacks were already settled.

Blacks could not own land in "white South Africa", but were required to work if living therein. In terms of the *Native Regulation Act of 1911*, black men and women aged 16 or over had to register with a labor bureau. Registered farm workers could not switch to industrial

14. The law also foreshadowed the desire of the state to ensure that blacks either work as wage laborers on (white) farms or relocate to the reserves, through creating limits on renting land to black tenants and imposing stricter controls on black sharecropping (Atkinson 2007). Before World War 2, however, these restrictions were most strongly implemented in the Orange Free State province — elsewhere, systems of black labor tenancy and sharecropping continued. Appendix A.3 describes the timing of the end of the system of labor tenancy in South Africa.

15. The system of migrant labor allowed a mining company to recruit a male worker for 6 or 9 months without having to pay a wage that would also support his family. Women remained subsistence farmers in the reserves.

work (Atkinson 2007, p.35).

The foundation of "influx control", that is, the control of rural-urban migration by blacks, was the *Native (Urban Areas) Act of 1923*. The chief effect of this law was to regulate migration into and residence in towns on the basis of the "pass", a focal point of opposition to Apartheid. Blacks who were staying illegally in urban areas were punished.[16] Savage (1986) reports that more than 9m Africans were prosecuted under the pass laws between 1960 and 1980.

1.1.2 The intensive margin of employment

Employers were also aided by regulations on the conditions of employment. While *Masters and Servants (M&S) Laws* had been on the books since 1880, in 1926 the Pact Government passed an amendment which brought oral and written contracts between farmers and labor tenants under M&S laws.[17] Farmers could now bring criminal charges against labor tenants in breach — a provision that was only repealed in the 1970s with the rest of the M&S laws.

The Pact government also passed the *Native Servant Contract Act of 1932*. Some of its provisions included for instance, (i) strengthening the power of black parents to enter into binding contracts on behalf of their children, and (ii) introducing whipping for contraventions of M&S laws.[18]

16. In addition to this battery of laws controlling the land blacks could own and farm, and the places to which and conditions under which blacks could move, farmers were also able to use foreign migrant labor, particularly in the border districts. Bradford (1990) describes how the migration of black South Africans from rural areas into urban areas was partially offset by increased recruitment of foreign labor from neighboring countries. (Her paper is entitled "Getting away with slavery" which sheds some light on the labor conditions prevailing on those farms.) Farmers were also in some cases able to employ prison labor. For a time, the state provided subsidised credit to farmers to fund the construction of rural prisons, the labor from which would be used on participating farms.

17. The 'Pact' government was a coalition between the farmer friendly National Party and the white worker-backed Labour Party. The law passed covered Transvaal and Natal provinces — 2 of the 4 provinces.

18. Duncan (1995) argues that the egregiousness of the Act, the complexity of administration, and the

The *1936 Native Trust and Land Act*, aside from extending the reserves, also sought to end 'squatting'. Laborers would qualify as 'labor tenants' and not 'squatters' only if they were employed by the white landlord on the farm for 180 days of the year. Blacks could not live on white rural areas, unless registered as 'servant of the owner', a 'labour tenant' or a 'squatter'.

I turn now to the division of the rural ('unskilled') labor between competing white interests, in particular farmers and mine owners. Traditionally, the source for mining capital was English-speaking industrialists, historically in conflict with Afrikaans-speaking farmers. The managed competition over black labor is sometimes referred to in South African historiography as the story of 'gold and maize'.[19]

1.1.3 The organization of mine recruiting

South African economic history is intimately tied to the story of gold mining. In the 20th century, South Africa was the world's primary exporter of gold, a commodity central to global monetary policy. It is the interaction of its *geographic endowment* of minerals — primarily diamonds and gold — with the *institutions* inherited from colonization — Dutch and British — that shaped the country's path of economic and political development in the 20th century.

---

obstruction by Native Affairs Department officials, made it fairly unenforceable. He describes the ambition of the act as one of "unsurpassed repressive qualities"(p.137). However, notes Atkinson (2007) (p.37) "child labour practices were integral to the farm economy until they were outlawed by the Basic Conditions of Employment Act of 1997. It is estimated that there were over 60 000 black child farm labourers in 1990, many between eight and 14 years of age."

19. See Morrell (1988) and Crush (1993). It is important, however, to keep in mind the contest *within* mining also. As described by Bonner and Shapiro (1987), the coal mining companies in Natal and the Transvaal had thinner margins than the gold miners and demanded cheaper labor still. They argue that the desire for lower wage rates resulted in an increased application of coercion — (p.2) "in Northern Natal [...] workers were encouraged to live on neighbouring farms and were deliberately enmeshed in a system of debt bondage."

A monopsonist mine recruiter

Various supervisory roles on the mines were undertaken by white mine workers who were unionized (primarily within the Mine Workers Union) and were a powerful lobby. In contrast the far more numerous black workers — drawn from throughout Southern Africa — were non-unionized and a comparatively weak political force.

There were six major mining houses,[20] but they organized various joint actions within the industry association, the Chamber of Mines. Notably, nearly all recruiting of black labor by the mining industry was done by one of the Chamber's recruiting arms — the Witwatersrand Native Labour Recruting Agency (Wenela) or the Native Recruting Corporation (NRC). The former was tasked with recruiting north of the 22 degree south line of latitude (primarily in Malawi and Mozambique), the latter with recruiting south of it (primarily in South Africa, Lesotho, Botswana, and Swaziland).[21] Foreign black mine recruiting was important. It increased both in absolute and relative terms in the 1960s as mining companies substituted cheaper foreign labor for South Africans. There had been 175,000 black South African mine workers in 1960, but by 1971 that number had been halved. At 1974 foreign migrant mine labor as a share of mineworkers on the South African gold mines was 78%.[22] At the end of 1977, after the shock to foreign labor supply described below, that number was to 47%.[23]

20. Anglo American Corporation (AAC), GENCOR, Goldfields, Rand Mines, JCI, and Anglovaal.

21. As Savage (1986) notes, this decade was also the period of the highest rate of arrests for pass law violations in South African cities. It is possible that former mine workers were trying to find work in the cities.

22. A total of 296,219 out of 382,448 of the total black labor force was from outside South Africa (Lombard and Stadler 1980).

23. In 1977, following the changes I describe below, the two recruiting companies were merged into The Employment Bureau of Africa — which still operates today in Southern Africa as TEBA Ltd. See `http://www.teba.co.za/`

The geography of recruitment

The *Native Labour Regulation Act of 1911* allowed the state to exclude private (including mine) recruiting in magisterial districts in which there were white farms. Under this Act and the *Natives (Urban Areas) Act of 1923*, the Native Affairs Department could assign to local authorities the ability to establish local labor bureaus. As described by Jeeves (Crush and James 1995), the purpose of the Act was to "shelter" the labor supply of important white farming areas from mine recruitment (and other industries). In his assessment this institutional structure "could not have survived [...without the] use of foreign mine labour."

The calls from farmers for even stricter controls on labor movement had increased during and after the Second World War, a time during which black labor had been widely employed in manufacturing. The exigencies of war had required faster migration of blacks into cities. Promising more stronger application of the principles of influx control under their new policy of apartheid, the National Party secured important farming constituencies to come to power in 1948.[24] After 1948 the Native Affairs Department was transformed; a paternalistic ethos was supplanted by a more rigid and extensive labor bureau system.[25] This new system, part of the 1952 *Native Laws Amendment Act*, constituted a vast network of labor allocation and influx control.[26] Local labor bureaus had to approve each application of a black individual to leave a rural district — which would depend on whether local labor supply was deemed adequate. Blacks would only be able to leave the farm and work in the city if the white

24. They lost the popular vote to the United Party by a considerable margin.

25. The transformation of 'native' policy through the extension of the administrative powers of this department is a topic tackled at book length by Duncan (1995) and Evans (1997).

26. The law required also passing the "Natives (Abolition of Passes and Co-ordination of documents) Act of 1952" a wonderfully Orwellian name for a law establishing an expansive Pass system. Influx controls were only finally abandoned in 1986.

farmer agreed.[27] Atkinson (2007) points to the remarkable 'success' of this system in terms of its aim — maintaining a pool of black agricultural labor; including domestic workers, there were some 1.85m workers (black and white) on farms in 1958, accounting for 40% of total workers.

Thus by the start of the 1970s, large parts of the country (containing large numbers of potential mine workers) were closed to the mines (Crush 1993). Restrictions added under the Bantu Labor regulations of 1965 meant that private recruiting was closed even in some districts containing bantustan land.[28]

Crush (1993) maps the regions which were open and closed to mining recruitment in 1970, just before the shocks to mining I describe below (see figure A.1). I will call the parts of South Africa open to mining recruitment "open districts" and the parts closed "closed districts." Note that the open districts include both areas in the Native Reserves (later "homelands" or "bantustans") and areas within "white South Africa".[29] Since I will mainly be concerned with outcomes obtained by surveying white farmers — such as black farm wages and investment in combine harvesters — I will primarily be interested in the difference between the white agricultural land parts of open districts and the white agricultural land parts of closed districts. Figure A.1 also indicates a set of closed districts marked as 'high potential' for recruiting by the mining industry association in internal documents. This set of districts will serve as 'placebos' against which I compare the effects of the shocks to mining on open districts.

---

27. See also Giliomee and Schlemmer (1985) and Greenberg (1980).

28. This applied in particular to the provinces of Natal and Transvaal. By "bantustan" land I mean "scheduled" and "released" land as set out above.

29. The bantustans, or homelands, were generally open to recruiting. As we have noted, there were also some closed parts, but this was relatively rare.

1.2 Data and Empirical Strategy

I first briefly outline the data on mining, agricultural and political outcomes on which my analysis will rely, before turning to a description of the natural experiment and the empirical strategy. It is worth spending some time on the data since in a number of places this is the first time these data have been digitized and/or used in the economics literature.

1.2.1 Data

Mining production. For information about mine output I rely on the annual reports of the Chamber of Mines. These include at the mine level: annual tons produced, working costs, working profits, and (from 1973 onwards) capital expenditure.

Mine labor recruitment. Various sources provide annual data on sources of mine labour by country — I use average number of black workers employed (1920-1989) and black workers received by mines drawn from the appendix to Crush, Jeeves, and Yudelman (1991). The annual memoranda for the Board of Governors of the Native Recruiting Corporation (NRC) and the Witwatersrand Native Labor Association (WNLA) (tasked respectively with recruiting south and north of the 22 degree latitude parallel) contain data on number of recruits by *recruiting station*. These are available in hardcopy format at the TEBA archive at the University of Johannesburg. The collection also contains some information on the country origins of recruits by mine as well as the value of voluntary deferred payments and remittances by station year. There is incomplete data on the recruits by contract type (i.e., recruits for 6 month tours, 9 month tours, etc.).

Agriculture. My primary source for information about agricultural holdings, production, employment, wages and mechanization is the Census of Agriculture prepared by the Cen-

tral Statistical Services (CSS) of South Africa (later called Statistics South Africa).[30] The geographic unit I employ in the construction of the panel is the magisterial district of which there were about 300 (the number for which agricultural data was reported was around 250). The agricultural censuses group magisterial districts into 67 economic regions which are contained within (the 4) provinces. (This classification was done by CSS, and does not correspond to administrative units). I use the classification as at 1972, before the shock, throughout. The main variables of interest include: black wages per worker, total black regular employees, number of tractors, number of combine harvesters, and average farm size. The empirical analysis focuses on a panel of magisterial districts and the years between 1963 and 1983 for which data are available.[31]

Voting. I draw on Schoeman (1977) for electoral division (plurality election) results to 1974. Schoeman's detailed work contains returns for all elections from 1910-1976 as well as commentary on the results. There were between 121 (at Union) and 164 (at 1974) constituencies over this period.[32] The remaining election results (1977, 1981 and 1987) I digitize directly from the relevant government gazettes.

There is considerable difficulty in measuring political consequences given the quality of available data, the chief challenge being mapping electoral divisions to magisterial districts.[33] Fortunately, the 1981 election results published in the government gazette include the home

30. This publication was also known at various times as the*Report on Agricultural and Pastoral Production* and the *Agricultural Survey*.

31. I do not account for district border changes.

32. Another useful source is Nohlen (1999) which explains for example how the military and state security council governance structure was formed in 1978, and how the Westminster system changed with the 1983 tricameral parliament.

33. Maps linking electoral divisions to magisterial districts exist but are extremely difficult to use. They are not complete, located in various national archives all around the country, and are so large that they are impossible to scan with nearly all scanners.

address of each of the constituency winners. I georeference these, assign them to magisterial districts, and (i) drop the urban areas (politicians in cities don't necessarily live in their constituency) and (ii) drop the magisterial districts not containing a politician. Thus the analysis of voting outcomes is for those magisterial districts which we can match to an electoral division.

Strikes. I will discuss also how the shock described in the following section was associated with increased strike action. For mine level outcomes on number of strikes, and number injured or killed, I use Horner and Kooy (1980) who report statistics between 1972 and 1979 and also provide details on changes in living and working conditions on the mines.[34] Following this shock, policy on the mines changed towards higher wages, longer stays at the mines, and more mechanization. I summarize the data used in the main empirical specifications below in Appendix Table A.9.

1.2.2 A shock to the native labor demand to mining

I focus here on the rapid and dramatic reduction in the foreign *supply of labor* starting in 1974. Foreign black mine workers declined from around 336,000 in December 1973 to 208,000 at the end of 1977. Over this period, the number of South African black mine workers increased from around 86,000 to 214,000.

The *heterogeneous geographic incidence of this shock* within South Africa provides the main source of identification in the analysis of the effects on South African agriculture that follows in section 1.3. Additionally, the large increase in the price of gold over the 1970s generally increased the demand for labor. The constraints on geographic sourcing

34. They also provide their own figures for total numbers of Malawians employed on the mines — 110 000 in 1973, but only 163 in April 1977 (this was also the period in which disturbances on the mines were most concentrated).

within South Africa — a result of the institutional structure given by the historical political agreement on labor allocation between gold and maize — meant that different parts of South Africa were affected differently by these shocks.

It is important that this change was exogenous to other conditions on the mines and unplanned by industry. There were two components of the shock. First, following the crash of a plane transporting Malawian miners, President Banda of Malawi decided to entirely stop Wenela's recruiting operations in Malawi. Second, the coup in Portugal in 1974 would lead to the dramatic scaling back of recruiting operations in Mozambique, a former Portuguese colony. (There was a slight lag — while the coup was in April 1974, the reduction would only occur from 1976).[35] Whereas 175,468 new recruits were from Malawi or Mozambique in 1974, by 1977 that number was 40,417 — a change of 135,051.

After the US abandoned the gold standard in 1971, the price of gold increased from a level of $36 at the start of the decade in 1970, to about $300 in 1979 (it would rise still further to $600 in 1980 before the dramatic collapse of commodity prices in the early 1980s). The rate of price increase was not constant. It increased from $36 in 1970 to $59 in 1972, and then $98 in 1973, and $159 in 1974. This increase was important, raising the demand for mine labor throughout Southern Africa. Nonetheless, the increase in total number of recruits in the 1970s was only 28,811.[36] My focus is on the compositional shift, with more recruits from within South Africa (including the Bantustans), and far fewer from Mozambique and

35. As Crush (1996, p. 167) describes, "neither of the major withdrawals of foreign labour in the 1970s were policy measures directed by the mining industry. When Malawi unilaterally withdrew its mineworkers in 1974 and the Chamber of Mines' conciliatory overtures were repudiated by President Banda, the industry was forced to scramble for labour throughout the region [...]." Furthermore, "the dramatic fall-off in Mozambican labour in 1976 (after a jump of 27,000 between 1974 and 1975) coincided with FRELIMO's equivocal attitude to migrancy and its reorganization of the WNLA recruiting apparatus in that country." For a discussion of the relationship between the South African labor recruiters and the Malawian government, see Chirwa (1996).

36. From 370,312 in 1970 to 399,123 in 1979.

Malawi. There were also important increases in recruiting from some of the other neighboring countries (especially Lesotho, but also Zimbabwe and Swaziland), but these were from a far lower base.

The significance of these events was recognized by the industry's recruiting arm, TEBA. Its five-year plan, circulated to the Board of Directors in December 1977, begins by recognizing these events as turning points for the industry:

> April, 1974, was a significant date for South Africa and for its primary industry, mining. The tragic Francistown accident and Spinola's action in Europe heralded a [...] re-adjustment for the mining industry as a major employer of foreign labour [...]

The immediate effect of these shocks was to increase emphasis on recruiting within South Africa rather than from foreign sources. Figure A.2 shows annual recruiting figures by country. The sharp reductions in Malawian recruits from 1974 and Mozambican recruits from 1976 are clear as is the ramping up of recruits from South Africa. While the mines were located in the interior — around Johannesburg and in the Orange Free State province, the recruiting districts were considerable distances away. The Native Recruiting Corporation (NRC) was organized into recruiting stations in the recruiting districts, regional offices (each managing groups of 4 or 5 stations), and receiving depots near the mines at Johannesburg and Welkom. The data presented in Figure A.3 illustrates the median number of recruits across regional recruiting offices within South Africa and was collected from the TEBA archive at the University of Johannesburg. The scaling up of domestic recruiting after the shocks is clear.

As detailed later, the mining industry did not just switch to greater domestic sourcing of labor. It also raised wages, changed contractual terms to incentivize workers to stay longer on a mine during a tour, and provided incentives to return to the same mine on future tours.

There was also a push towards greater mine mechanization.

1.2.3 Empirical Strategy

The discussion in the previous section suggests that the effect of increased mine recruitment within South Africa would be most felt in those districts in which, for historical reasons fixed primarily by the politics of the early Union, mine recruiting was permitted. While mining companies tried to widen the net for recruiting, their political overtures to government to expand geographic labor sourcing were ultimately unsuccessful in the wake of vigorous opposition from the farm lobby (Crush 1993). This informs the basic empirical approach which is to compare outcomes in districts open to mine recruiting to those closed. In terms of *economic outcomes* this means comparing wages, employment and the adoption of combine harvesters. In terms of *political variables* this means comparing voting outcomes for parties favoring some loosening of the labor controls underpinning Apartheid with the outcomes of more conservative parties opposed to any loosening. I add controls and sample restrictions to test the robustness of this basic analysis, and repeat the analysis on a set of "placebo districts" that the mining companies identified as suitable for recruiting (and unsuccessfully lobbied to open). I supplement this analysis with a study of mine level outcomes on output and political activity (strikes), as a function of exposure to this shock to labor supply.

The logic of this paper is to examine how economic shocks are filtered through political institutions to produce structural changes in the economy, which then changes the institutions

themselves.[37] This can be illustrated as

$$shock|\, institutions_0 \Rightarrow technology\ choice \Rightarrow institutions_1$$

$$(labor\ supply|\, open) \Rightarrow (combines) \Rightarrow (voting)$$

The Apartheid institutions of labor control shaped the response to the external shock which then fed back into differential support for institutions (proxied by voting outcomes).

Conceptual Framework

Consider a farmer in a district d in year t producing agricultural goods at a fixed price according to technology $A_d F(K_{dt}, L_{dt})$. Capital is mobile across districts and labor is supplied inelastically. In this setting capital (combine harvesters) and labor are taken to be substitutes.

In open districts blacks can earn an outside wage w_m from working on the mines so that $w_o = w_m$. For farm workers in closed districts there is an added cost of possible punishment, p, should they attempt to work on the mines outside their designated occupation and district, so that $w_c = w_m - p$.

The cost of punishment is determined as a function of the political equilibrium. Farmers lobby their representatives over many dimensions: the provision of credit for capital investment, agricultural extension services, as well as (in the closed districts) the strength of the punishment for labor mobility. Capital choice and labor input are fixed in the usual way but in closed districts there is also a parameter, p, for the level of punishment (exerted through

37. Note also that in important senses, structural change can be an endogenous response to the static political equilibrium — there is evidence that in some cases, innovation and mechanization within South African industry was a response to the strong controls on the labor market. This was self-undermining, in the sense of Greif and Laitin (2004).

the political system, for example via representations by the Agricultural Union). In the closed districts this means:

$$A_c F_K(K, L) = r \tag{1.1}$$

$$A_c F_L(K, L) = w_m - p \tag{1.2}$$

whereas in the open districts the p is dropped from the final equation given that the institutional structure inherited from 1911 does not permit them to exclude mine recruiting.

Following an increase in the mining wage w_m the equilibrium wage in both districts rises, but relatively less in closed districts since they optimally also substitute an increase in political pressure (which raises the equilibrium p) through threats to leave the National Party for the far right which will be realized variably across districts. Hence I expect the observed wage and optimal capital labor mix to rise relatively more in open districts. The increase in punishment p corresponds to an increase in vote share for the right-wing parties in closed districts. Hence I also expect to see relatively higher (lower) support for the right in closed (open) districts.

Comparing open and closed districts

Baseline differences. I first compare the baseline characteristics of districts 'open' and 'closed' to mining recruitment in 1972, before the shocks to the supply of labor from the changes in Malawi and Mozambique. While these types of districts were different on average, when comparing open and closed districts in the same economic regions they are not statistically distinguishable. The location of open and closed districts is illustrated in Figure A.1. The figure shows the districts open to mine recruiting in 1970 with a dark shade.

In the other districts recruiting was entirely prohibited.[38] The grey shaded regions are those districts closed to recruiting, but identified by the mining companies as having 'high potential' for recruiting in the future (provided government could be convinced to open those districts). I thus examine baseline differences by regressing district outcomes in 1972 on open status. I then repeat this regression but include economic region fixed effects. The estimated coefficients are the within-region differences for open districts.

$$y_d = \beta Open_d + \alpha_r + \epsilon_d \tag{1.3}$$

It is clear from Table A.1 Panel A that our "treatment" districts (open) are different on average from the "control" districts (closed). Open districts have smaller average farm size, lower wages, more employees, fewer combine tractors and lower white incomes in 1970. However, conditional on economic region, the districts are indistinguishable — see Table A.1 Panel B — aside from white agricultural incomes in 1970, which are 14 percent lower in the open districts.

Difference in differences. My baseline specification pools over the years 1976-79, of greatest exposure to the shock. I create a single dummy, *Post*, which takes on the value of 1 for those years and I estimate differences between open and closed districts, relative to the excluded base year of 1972. Outcome y in district d and year t is regressed on economic region cross year fixed effects, district fixed effects and open status. My identifying assumption is that open districts would have changed in a way similar to closed districts (in the same economic region), in the absence of the foreign mine labor supply shock which differentially exposed districts open to mine recruiting.

38. There was some leakage. Crush (1993) reports that at 1970, 12% of the 90,322 black mineworkers recruited from white South Africa and the bantustans were recruited from 'closed' districts.

$$y_{dt} = \beta Post_t \times Open_d + \gamma_t X_d \alpha_t + \alpha_{rt} + \alpha_d + \epsilon_{dt} \tag{1.4}$$

In practice I control for other characteristics (X_d) of the district that might explain different changes. In particular, I control for the percent of maize production in 1963 interacted with the outcome year. The specification allows the effect of maize suitability (proxied by 1963 output) to change over time (for example there might be particular price shocks or technology changes affecting maize). I also weight the regression by the share of the population that is black and rural in 1960.[39] Standard errors are clustered at the district level to account for district level heteroskedasticity and within-district time correlation.

To examine dynamics, I also estimate year-specific differences between open and closed districts, relative to the excluded base year of 1972.

$$y_{dt} = \beta_t Open_d + \alpha_{rt} + \alpha_d + \gamma_t X_d \alpha_t + \epsilon_{dt} \tag{1.5}$$

Coefficient β_t can vary by year, so its interpretation is the average difference between our open districts and closed districts in that year, relative to 1972.

In terms of agricultural outcomes I am interested both in the effects of the shock on factor prices (does the cost of farm labor increase on farms competing with mines for labor?), and also in whether higher wages induced the adoption of labor-substituting technology (in particular the combine-harvester). This I define as "agricultural modernization".

For voting outcomes the basic specification I run is similar to the agricultural outcomes; I regress electoral division-year level outcomes (relative to the 1970 elections) on economic region by year fixed effects, year by maize production in 1963 interactions, and a dummy for the 1977 and 1981 elections interacted with open status. Similarly to our baseline spec-

39. This is the last year for which urbanization status is available.

ification above, I weight by the proportion of the black population in that district which is rural. Standard errors are clustered at the electoral division level to account for division level heteroskedasticity and within-division time correlation.

Comparing Mines

The basic specification for information about mine output and political activity is

$$y_{mt} = \beta Post_t \times MMShare_m + \alpha_m + \alpha_t + \epsilon_{mt} \tag{1.6}$$

where $MMShare_m$ is the share of Mozambican and Malawian miners of all miners at mine m in 1967, y_{mt} is the outcome (output or political action) on mine m in year t, α_m is a mine fixed effect and α_t is a year fixed effect. *Post* takes on the value 1 after 1976 and is otherwise 0. Standard errors are clustered at the mine level to account for mine level heteroskedasticity and within-mine time correlation.

1.3 Evidence on Economic Outcomes

I now illustrate how the changes in mine recruiting spilled over onto agriculture particularly in those areas directly competing with mining for labor. I show how in the open districts, agricultural wages increased significantly more between 1976 and 1979 relative to the closed districts. Furthermore, the adoption of combine harvesters similarly increased in the open districts relative to the closed, suggesting substitutability between workers and combines. These findings are robust to various controls, including the status of tenancy 'reform' in that district, the level of white agricultural incomes, and the inclusion or exclusion of the four 'independent' homelands (Transkei, Bophutatswana, Ciskei, and Venda). I also perform a placebo regression on the set of closed districts identified by the mining recruiter as 'high

potential' for recruitment. There was no similar response in those districts to the mining sector shock.[40]

1.3.1 Effects on farms

Baseline results

Wages. My results show wages rise differentially in the open districts. When I collapse the year by open interactions into a single variable for open status for 1976 to 1979, consistent with the main identification assumption, the results in Panel A of Table A.2 show that over this time period the wages in open districts were significantly higher by 0.152 log points relative to closed, conditional on economic region. Column (1) of Table A.3 reports estimated differences in black wages for regular farm workers in open districts relative to closed in various years, following estimating equation 1.5. We see the largest difference by magnitude in 1976, where the difference (conditional on economic region by year fixed effects, and district fixed effects) is 0.217 log points. Though the difference is not statistically significant in 1976, it is significant at the 10% level in 1978. These dynamics are shown in Figure A.4. Thus while agricultural wages across the country rose over this time period, the increase was even greater in those areas exposed to competition from mining following the shock to the supply of Malawian and Mozambican labor.

40. The effects of the shock to foreign labor supply on the South African countryside may have been somewhat attenuated by contemporaneous changes in agricultural organization in South Africa and by the ramping up of recruiting operations elsewhere in Southern Africa (in Lesotho, Swaziland, and Botswana). I detail the changes brought about by the ending of the system of labor tenancy in appendix A.3. These changes, including evictions of blacks from white farms to the Bantustans resulted in an increased number of unemployed blacks willing to work on the mines by the 1970s. Later I show that my findings are robust to these changes.

Employees. The relative number of regular black employees falls in the open districts. In columns (2) and (3) of Panel A of Table A.2 I report the coefficients from the regression with a single open status interaction over 1976–1979. Conditional on economic region the number of regular black employees on farms was 0.087 log points lower in open districts than closed, and this result is significant at the 10% level. The wage bill was also higher but this was not statistically significant. Columns (2) and (3) of Table A.3 similarly report estimated differences in the number of black regular employees on farms, and the total black regular employee wage bill, in open districts relative to closed. As expected, the former decreases from 1976 (and is significantly lower by 0.100 log points in 1978) while the latter increases (0.190 log points in 1979).

Mechanization. Farmers in open districts add more combine harvesters. Columns (1), (2) and (3) of Panel B of Tables A.2 and A.4 report year by open status differences for combines, power combines and all tractors. While combines and power combines increased in open districts relative to closed, the differential impact is not visible when one aggregates over all tractor types. But the difference between tractor types is important — as has been outlined by historians using survey evidence of individual magisterial districts. De Klerk (1984) sets out the important distinction between 'hand harvesting' — by which is meant reaping by hand and threshing using a tractor — and 'combine-harvesting' which means doing both reaping and threshing with the same machine, a 'combine' harvester.[41] De Klerk's work is based on a survey of 6 (closed) districts in a particular economic region

41. As De Klerk (1984) (p.87) describes, "harvesting maize involves both reaping - that is, picking the 'head' or blaarkop from the stalk - and threshing, or removing the grain from the cob. Both can be done by hand or mechanically but it is many years since maize was last threshed by hand in the Western Transvaal. The basic alternatives are therefore: reaping by hand and threshing mechanically - called 'hand-harvesting' - or reaping and threshing with the same machine, i.e. 'combine-harvesting'. In both cases a number of variations are possible."

of South Africa. He finds for those districts an increase between 1968 and 1981 from 25% of combine-harvested maize to 95%, with a particular increase between 1973 and 1977. What Panel B of Tables A.2 shows is that this increase was *relatively larger* in open districts. De Klerk argues that in the period between 1945 and 1970, the adoption of tractors permitted more land to be cultivated and hence increased farmer demands for labor for harvesting, but that in contrast combines and people were substitutes. Schirmer (2004) makes similar points, and suggests also the importance of the increase in agricultural wages (driven by mine wage increases), relative to the increase in the cost of combines. This is consistent with the results for employees and combines just outlined.

De Klerk (1984) also finds (p.91) important changes in the sizes of farms over the time period covered by his sample of 6 districts with the average gross surface area increasing to 1155 ha. in 1981 from 664 ha. in 1968 (with 2/3rds of this increase concentrated between 1973 and 1977). I have insufficient data to analyze average farm sizes over the country as a whole, but it would be surprising if increases in mechanization were not associated with increases in average acreage.

Threats to validity

An important concern is that it may be that inherent differences between open and closed districts resulted in differences between 1976 and 1979, unrelated to the shock from mining. While it is clearly not possible to control for all other potential channels, I can reject a few competing stories.

The open and closed districts were fixed by historical political negotiation and differ on some dimensions which may begin to be important from 1976. I perform a number of robustness checks to explore the importance of inherent differences between open and closed districts. I add controls for whether or not tenancy 'reform' has been completed in

that district, the level of white agricultural incomes in 1970 (and year interactions) and the inclusion or exclusion of the "independent" Bantustans — Transkei, Bophutatswana, Venda and Ciskei (TBVC).

Column (1) of Table A.5 Panel A presents the baseline results for the log differences in black regular wages between open and closed districts as a basis for comparison. Column (2) controls for whether or not the district has undergone tenancy 'reform' (that is, whether or not the institution of tenancy has been restricted in that district) and year interactions. If tenancy reform happened disproportionately in open districts in this time period that might translate into wage increases, but I find that the results are robust to the inclusion of these interactions also.[42] Column (3) includes white farmer incomes in the 1970 census and year interactions. This crudely proxies for political power — we might think that farmers might influence recruiting behavior differentially across districts over this time period. I also run the baseline specification excluding all of the districts intersecting the "independent" TBVC homelands.[43] During this time period these regions gained nominal independence from South Africa, a process which involved some adjustments of borders. In fact Column (4) indicates that doing this does not change our estimates — simply reflecting that there was very limited white agriculture in these districts.

Panels B and C of Table A.5 report similar robustness checks for my results on employees and combines. Again, the results are in line with the baseline specification.

Placebo districts

In September 1974, the Research Department of the Native Recruiting Corporation released a memo in which they identified closed districts from which more labor might be recruited

42. For a discussion of the changes brought about under the label 'tenancy reform' see Appendix A.3.

43. I use the shapefile of the TBVC homelands from the Municipal Demarcation Boards.

— they labeled these 'high potential' districts and noted that "[r]epresentations are being made to Government to relax regulations wherever possible to enable us to recruit from all areas, including urban areas and farming areas."[44] I repeat the baseline specifications where I replace the open districts with these high potential districts. The results are reported in Table A.6, Panels A and B. It is clear that there was no significant response in similar districts which were closed to mine recruiting. There was very limited "opening" of closed districts in response to the shock. As reported in Crush (1993), 4 farming districts were opened to mine recruiting on an "experimental" basis in late 1976. The total number recruited from those districts in 1976 and 1977, however, was only $1,053$ people.[45]

1.3.2 Effects on the mines

I first confirm that there were mines that in the short term were forced to reduce output as a function of the shock. Output drops from 1976 differentially in the mines dependent on these workers after the shock. This confirms the impression conveyed by Figures A.2 and A.3 that 1976 was the year in which the effects of the labor supply shock began to be felt acutely. In Table A.7 Column (1), I present a mine-level regression of mining output on mine fixed effects, and an interaction between the share of Mozambican and Malawian workers at that mine in 1967 and a dummy for the years 1976 to 1979.[46] Output was 1.3% lower in

44. The memo is titled "Recruitment in South Africa".

45. Crush (1993) also indicates that there may have been a few thousand recruits from closed districts in those years also. While it is likely that there were leakages across districts, it is also apparent that the recruiting intensity within open districts was far greater.

46. WNLA referred to mine workers from Mozambique and Malawi as 'East Coast' and 'Tropical' respectively. The latest year for country $\times$ mine recruiting data I was able to locate in the TEBA archive at the University of Johannesburg was 1967. This is problematic because the share of East Coast and Tropical workers rose further between 1967 and 1974. There is thus likely some attenuation bias.

those most exposed mines.

There were a number of changes in policy on the mines, and in terms of mine recruiting, resulting from this shock: Most directly, and as discussed above, the sourcing of black mine labor shifted towards South Africa. But also, wages were increased and working conditions improved. Less directly, there was the implementation of a policy of labor force "stabilization" (keeping mine workers at mines longer, with more returning workers), and greater mine mechanization.

Changes in labor policy

TEBA's five year plan set out the broad pillars of a new recruiting strategy: greater domestic sourcing, wage increases, and improved incentives for longer duration stays on the mines.

Higher Wages. The average annual pay rate for blacks in mining and quarrying increased in real terms from R235 in 1972 to R606 in 1976 (a real increase of 158%. It would rise to R651 by 1979. The nominal increase over the same time was 604%) (Lombard and Stadler 1980). Over the same time period, the increase in real wages for blacks in manufacturing was from R643 in 1972 to R859 in 1976 (a real increase of 34%, and R897 by 1979). As reported by Lombard and Stadler (1980), this sharp increase in black mine wages meant that the ratio of white to black pay on mines fell from 19.8 times in 1970, to 6.7 times by 1979. Nominal wages *per ton milled* increased from R3.77 in 1971 to R9.79 in 1976, in real terms an increase of 53%. The 1977 five year-plan recommended that wages be increased further still, though from then, "in the order of 10% per annum should be sufficient". Furthermore, it recommended improved financial services for remittances and savings.

Stabilization. The effect of the compositional change of labor was important with respect to *turnover*, since the preference of South African workers was for shorter contracts in com-

parison to workers recruited from Malawi and Mozambique. The five year plan notes that "clearly this situation is unsatisfactory and we should plan for greater stability through both incentives to stay and disincentives to go [...]". It also expresses a desire for more selectivity, and recommends building systems which would reward 'good service' and create incentives for workers to return to particular mines. The mining companies and TEBA introduced various contractual changes towards these ends, including *Voluntary Re-engagement Certificates* which rewarded workers through bonuses for renewing employment at the same mine. In addition, there were other developments such as improved retirement benefits for 'long service'.[47] Cobbe (1986) compiles data for mine mine workers from Lesotho showing average contract length increasing from 11 months (the average between 1967 and 1977) to 16 in 1979.

There is evidence that these measures were to a some extent successful in stabilizing the labor supply. As described in Giliomee and Schlemmer (1985), by the early 1980s TEBA was no longer employing its network of labour agents and runners to source workers from remote areas. They argue that while in the past poor conditions for agricultural production in the homelands increased labor supply, seasonal fluctuations by then were less relevant.[48]

Mine Mechanization

A further response of the industry to the combination of increased price of gold and reduced foreign labour supply was to emphasize the importance of capital investment. It is notable

47. The 1976 Annual report of the Chamber describes (p.29), 'The formulation of an improved retirement benefit scheme for Asiatic, Black and Coloured employees referred to in the 1974 and 1975 annual reports was completed during the year with the introduction [...] of a new long service award scheme."

48. They write (Giliomee and Schlemmer 1985, p.72)"in the past [...] bad rains brought increased interest in wage work on the mines. But in 1982, the widespread drought brought almost no increase in the labour available to the mines [...] a TEBA official observes 'the question of seasonal rains and droughts has much less effect than it did 10 years ago.' "

that Capital Expenditure only began to be reported in the annual report of the Chamber from 1973. Capex rose rapidly in the years following.

1.4 Evidence on Political Outcomes

I now look at political consequences from the changes in economic structure identified in the preceding sections. I first consider differences in voting outcomes across open and closed districts. The voting outcomes are more infrequent than the agricultural surveys, and the assignment of electoral divisions to magisterial districts is with some error, so results presented here should be handled with care.

I then present some suggestive evidence that the labor supply shock was associated with greater political activity by black workers on the mines. I discuss how this may have influenced and been reflected in structural changes in labor organization on the mines.

Giliomee (1995) classifies the different potential interpretations of the halting apartheid reforms started in the late 1970s and early 1980s into 'elite initiative', motivated by the skills shortage which had developed; and 'elite response' to black labor protest. Each of these stories likely has some merit. It is clear that this period saw the start of the appearance of opposition to NP rule, noticeably from within the party. During the 1980s, there were important shifts to the (new) rightwing parties, increasing their share from 14% in 1981 to 30% in 1987 (a pickup of 22 electoral seats) (Rooyen 1994). In the election in 1987 the Conservative Party (CP) became official opposition in parliament, in addition to displacing the HNP as the primary far right party. Rooyen (1994) notes that their support was mainly in the Transvaal and Free State, and in particular in *agricultural and mining towns*. The NP shed further seats to the right in the general election of 1989, but in addition lost votes on the left. I will provide evidence in this section that it was precisely in agricultural and

mining towns in closed districts which abandoned the NP to the right.

1.4.1 Comparing open and closed districts

Column (1) of Table A.8 (Panel A) reports estimated differences in the cumulative vote share for parties to the right of the National Party, while Column (2) of Panel A reports the differences in the sum of the vote share for the ruling National Party and parties to its left. The results suggest that following the shock to mining, and the structural changes in agriculture, the 1977 and 1981 may have seen increased support for some of the reforms the National Party was introducing. Charney (1984) gives an essentially class-based, analysis for the loss of some agricultural seats in this period, arguing that the government became perceived as too much in favor of big farmers, "its relaxation of labour and racial policies is regarded as threatening black labour supplies and the whole labour-repressive system on which small farmers, particularly, depend. There has been a big swing to the right, mostly to the CP, among [...] the maize men, who depend heavily on government decisions on the price of inputs and produce."

One might think that instead it could have been the case that the shock to labor availability for farmers in open districts would have prompted them to reject the NP for a party on the right which would have perhaps been more amenable to closing more districts (or otherwise protecting labor supply). I argue that the strong countervailing pressure of mining companies towards reduced constraints on recruiting labor rendered this infeasible. As an internal memo from Chamber of Mines's Native Recruiting Corporation noted in September 1974, "Representations are being made to Government to relax regulations wherever possible to enable us to recruit from all areas, including urban areas and farming areas."[49]

49. This would remain the policy of the miners. Giliomee and Schlemmer (1985), a collection of essays strongly critical of the labor recruitment architecture of Apartheid South Africa, includes articles by 3 then

Charney (1984) also argues that the increase in far right support during the 1980s emanated from white working class opposition to National Party policies. In addition to small farmers, he also notes the strong opposition to any reforms (and an increase in the militant right) among white mine workers for example, who opposed the ending of job reservation, while government sided with mine management. We have seen that important changes had occurred in mining recruitment, capital intensity, and labor organization by the start of the 1980s.

1.4.2 Developments on the mines

Throughout the 20th century white mineworkers jealously guarded their relatively privileged positions, primarily through the Mine Workers' Union (MWU). Their political strength was first felt during the strikes in 1922 (the "Rand Revolt") against the threat of mine owners using more black labor and in more skilled positions. It required the use of the army by Prime Minister Smuts to suppress the rebellion. There was no serious challenge to the white mineworkers (or the fixed 8:1 black:white labor input ratio) until the 1960s.[50] And, in fact, it would only be with the structural changes of the 1970s that the old institutional arrangements would begin to change.

Table A.7 (Columns (2) and (3)) investigates whether the shock to the supply of foreign

executives of Anglo-American, as well as an article from then head of Barlow-Rand. Robert Goodsell, an executive from Anglo American, argued "oscillating migration obstructs the development of a mature employment relationship, and frustrates career-type aspirations on the part of management and workers" Giliomee and Schlemmer (1985) (p.306).

50. But, even then, the changes that were instituted under the new agreement in 1967 were (Wilson 1972) "a triumph for the trade unions ... the lion's share of any increases in productivity due to the elimination of some restrictive practices [i.e. restricting blacks from certain occupations] would go to white worker". Whites would work less, for more pay (the total white wage bill rose by 11%). (White) management and (white) labor could only agree to Pareto-improving bargains over the extra surplus generated by the greater application of black labor.

labor was associated with an increase of strikes and other workplace interruptions (Lemon 1984). Indeed, there is suggestive evidence that the mines most exposed to the shock also had an increase in strikers injured or killed. Horner and Kooy (1980) (p.10) classify the majority of "disturbances" as relating to ethnic differences. In particular, they argue that (p.23) "the sources of conflict are to be found in such factors as differential treatment of workers, different lengths of contract, and differential access to jobs [...] it is in the interests of management to suppress forms of class consciousness in the work force and rather to encourage forms of national or tribal consciousness".[51] The analysis here suggests that the compositional changes in the work force helped to make the strike activity so acute.

The series of strikes is important for helping to understand some of the changes in strategy on the mines. Following the shock there were renewed demands by the mining industry leaders first for a "stabilized" black labor force around the mines, and second, for deregulation of the racial controls on semi-skilled and skilled work. The moment of the entirely transitory, low wage, migrant laborer had passed.[52]

Two commissions. In 1977, A.C. Peterson, member of the council of the Chamber of Mines would argue, "the industry's long-term aim must [...] be to eliminate work restrictions and job reservation and convince our White employees that this, by raising of the whole

51. In his 1975 address, the President of the Chamber of Mines would state, "the strikes and disturbances on the mines resulted in a number of deaths during the year [...] the great majority arose from inter-tribal clashes".

52. Chairman of Anglo American Corporation and De Beers Consolidated Mines, Harry Oppenheimer, argued at a meeting of the Chamber of Mines in 1975, "[t]he time has clearly gone by, never to return, when we sought to control the level of costs in the industry by keeping wages down. We have got to control costs in future, not by low wages, but by high productivity [...] We are not going to be able to count [...] on solving our problems by a great influx of totally unskilled workers, a great many of them foreign [...we should] increase the percentage of Black workers who are South Africans, and we should increase the time they spend in the industry, so that the skills they acquire when working with us are not allowed to go to waste [...] you have got to house them with their families, and there we need the co-operation of the Government[...]" Chamber of Mines South Africa, *85th Annual Report*.

South African economy, can only be to their advantage as well as the advantage of the Blacks." In the same year the government launched two major reviews of aspects of labor legislation in South Africa, the Wiehahn and Riekert Commissions.[53] Part 6 of the final Wiehahn Commission report concerning Labour Legislation and the mining industry was published 30 September 1981. The changes emanating from the Wiehahn Commission were to open up a number of new semi-skilled jobs to blacks on the mines. Black mine workers were now working in a more capital intensive setting, and for longer stays. Various other important regulatory changes happened around this time the effect of which was to improve the bargaining position of black employees. Importantly, the establishment of black trade unions was made possible in 1981.[54]

Resistance to these changes by the unionised white artisans on the mines was fierce. Positions such as "blaster" in mining were some of the last to be removed from the "colour bar". The series of labor reforms following the Wiehahn commission reached mining last (indeed government withheld the mining report until the end). In Table A.8 Panel B I report results from the regression of electoral division outcomes (right share and NP or left) on year by mining district interactions and year by National Party vote share in 1970 interactions. *minedistrict* is simply a dummy for whether the electoral division contained a mine. As is clear from Column (1), the share of the conservative white right increases dramatically in mining districts during the 1980s.

---

53. The Chamber expressed support for reduced discrimination against blacks on the mines. The Chamber's President, in his annual address of 1978 would report, "[t]he Chamber submitted detailed evidence [...the Chamber] welcomes these urgent enquiries aimed at the removal of discrimination in the workplace [...] non-White workers must be absorbed into the skilled labour pool."

54. This was an outcome of the Labour Relations Amendment Act (No 57 of 1981. We discuss in more detail in appendix A.4 the history of white labor protection and the slow liberalization of these controls.

Black Labor is organized. Ultimately, the employers' association (the Chamber of Mines) and the largest black trade union (the National Union of Mineworkers — NUM) found themselves arguing on the same side for the end of the color bar. NUM was formed in 1982 and quickly became the most important union on the mines.[55] Its membership was 40 percent of the black workforce on the gold mines and collieries, and in the Chamber's words it came to "[dominate] the industrial relations scene, both nationally, through its influence in COSATU, and in the industry."

1.5 Conclusion

This paper reveals some of the fault lines of the Apartheid South African state. The links between extractive political and economic institutions in the stark South African context sheds light on fundamental features of the interplay between economic and political development and, in particular, the role of labor controls. I have collected and digitized important novel datasets and provided some of the first evidence on the economic determinants of the stability of the apartheid regime.

Specifically, the paper has shown how the geography of recruitment between mining capital and white farmers — managed by the use of external unskilled labor in mining — was changed by the shocks to foreign labor supply in the first half of the 1970s. Using the variation in geographic incidence of the shock I show how it changed economic structures and interests, and perhaps influenced political preferences.

Future work will explore the long-run effects emanating from these changes — in terms of persistent economic outcomes and political consequences. In ongoing work, I and collab-

55. After being recognised by the Chamber of Mines in June 1983 it had 6000 employees. This grew to 40,000 by December 1984, and 187,000 by mid-1987.

orators examine in more depth the important role of labor conflict and labor unions during the late 1970s and 1980s. In particular, Naidu, Turban, and Wilse-Samson (2014) study the economic incidence of politically and economically motivated strikes in this period. The powerful National Union of Mineworkers would have an important role in the transition to democracy. As described in the 1987 Annual Report of the Chamber of Mines, "NUM initiated a costly three-week strike [...] the industry sought to view and handle the strike as an industrial dispute, but the evident underlying aim with the strike was to demonstrate wide worker support for an agenda ranging from sanctions to seizure of control of the national economy."[56] What is clear from the analysis in this paper is that the strength of the labor movement in general was aided by the changes on the mines in the mid- to late 1970s.

Much more historical data remains untapped, and many important questions about South Africa's economic and political development remain understudied. South Africa is an important case study for the relatively peaceful transition out of non-democracy. This analysis has suggested that one potential mechanism was the reduced reliance on labor controls resulting from technology substituting for labor on the farm. But, the country's unemployment rate has remained trapped at extraordinarily high levels for decades following transition, perhaps reflecting in part a historically "too" capital intensive growth path.[57] Mechanized mining and agricultural jobs have never been replaced. Understanding why and where development went wrong is the *sine qua non* for effective growth strategies today. This paper suggests that part of the answer might lie in the important changes of the 1970s.

---

56. NUM was headed by Cyril Ramaphosa —who would later become the chief negotiator for the ANC during the constitutional negotiations around the end of Apartheid. Today Ramaphosa is ANC deputy president.

57. As noted in Spandau (1980), the Minister of Economic Affairs reported to Parliament in February 1977 concerns about the economy's increasing capital-intensity.

Chapter 2

Valuing Institutions: A Measure of the Bond Market's Views of Term Limits in Developing Countries

Laurence Wilse-Samson and Sébastien Turban

2.1 Introduction

On 26 February 2010, the Constitutional Court of Colombia rejected a referendum on a constitutional reform that would have allowed Alvaro Uribe to run for a third term in the Colombian presidency. In the following days, Colombia's sovereign bond spread over US Treasuries narrowed. Five years earlier, when the same Court upheld an amendment to the constitution allowing the re-election of Uribe, the bond market did not respond. We look across 101 events related to seven developing countries' executive term limits — 73 of which loosened the constraints on the executive, 28 of which tightened them — and find these responses to be more general. Term limit restrictions lead to a significant reduction in the measured riskiness of dollar-denominated sovereign debt while the effect of extensions is weaker and insignificant. To quantify this riskiness, we use stripped spreads of sovereign debt over Treasuries. Prices of insurance on government bonds from Credit Default Swaps confirm these results, although these markets are less liquid and less data is available. The results are also robust to a non-parametric test where we compare the response on event days to randomly chosen days. We further analyse these events by examining the variation in the response of spreads as a function of the the government branch initiating the action, as well as the level of institutional development of the relevant country.

In the modern literature on economic development, institutions are held to be central (Acemoglu, Johnson, and Robinson 2001). In this paper we ask whether, and in what way, *investors* care about institutions: do sovereign debt spreads respond to news about institutional changes? This paper is a contribution to the still relatively new literature analyzing the impact of institutional changes on high-frequency data. Term limit changes are generally part of a larger sequence of events, which can make approaches — using annual panel data for example — less well identified due to the presence of unobserved confounders.

In this paper we consider the immediate response of financial markets, which gives us an idea of the effect of institutional changes on *expectations* rather than final outcomes[1] — the direct impact on growth, for instance, remains unobserved. However, these outcomes should be correlated. For example, a lower borrowing cost makes borrowing cheaper for the country of interest which makes "positive" outcomes more likely. In the setting of rational expectations this is a distinction without a difference.

We test whether, and how, term limits changes are instantaneously reflected in debt spreads. This analysis thus measures the impact on country-risk as perceived by investors, where fluctuations reflect changes in beliefs about a country's probability of default. A direct effect of an institutional change on borrowing costs would matter for long-run outcomes. Furthermore, by measuring investors' response to potential changes, we infer *investors' beliefs* about the value of term limits in various countries. Since this response should be anticipated by forward-looking leaders and legislators, we also learn something about the political calculus of institutional reform within these countries.

The fall in spreads after restrictions suggests that those events reduce the perceived probability of default. The asymmetry with the non-effect of extensions suggests that some emerging countries live under a form of institutional drag: the interest rate on sovereign debt is high because investors believe that weak institutions are permanent (and that, for example, term limit extensions are likely).

The impact of institutions is further validated by the differing effects we observe when considering the institutional source of the term limits move. A move by the executive branch has a stronger effect than actions taken by the legislative or the judicial branch. However, it appears that restrictions instigated by the judiciary, where it is more likely that the judiciary is then in conflict with the executive's will, have a significant and permanent impact leading

1. Moser (2007), Moser and Dreher (2010) show two recent applications of a similar idea.

to a cumulative abnormal (negative) return of 5% on stripped spreads after 10 days. We analyze further the potential link between the market reactions and a country's level of institutional development by comparing the abnormal returns to various governance indicators, and find some tentative evidence that investors react more strongly in weakly institutionalized countries. The asymmetric absolute response between restrictions and extensions is somewhat reduced when we drop the two countries with the highest quality institutions at the time of their events.

This paper contributes to our understanding of the impact of term limits on both beliefs about and actual economic outcomes. As we have noted, a "shock" to term limits, e.g. a change in the constitutional rule defining their length or their number, may change investors' perceptions of the country's *ability-to-pay* because of the growth implications of the institutional change, or as signal of possible future further institutional changes. But, there are no clear theoretical or empirical implications of term limits on fiscal outcomes emerging from the literature.

Empirical work related to this topic has mostly focused on the link between term limits and Political Business Cycles (PBCs) — incentives for re-election affect the choice of fiscal and monetary policies.[2] The PBC literature notably links political institutions with fiscal outcomes, which might subsequently affect "sovereign risk", and thus a country's bond spreads[3]. Block and Vaaler (2004) suggest that investors and credit rating agencies are election-averse: bond yields rise and downgrades happen more often before an election. Our focus on emerging markets relates to findings that PBCs are more significant in these countries (Brender and Drazen 2005). Besley and Case (1995) show that in US States, incumbent

2. See Persson and Tabellini (2000), ch.16 for a review.

3. The literature is vast. See, for examples, Nordhaus (1975), Alesina, Roubini, and Cohen (1997), and Persson and Tabellini (2000).

governors subject to term limits increase taxes and spending and minimum wages fall in the corresponding term but Besley and Case (2003), with more recent data, find no effect on taxes or GDP growth. Johnson and Crain (2004) extend the analysis to 48 democracies and also find that government spending increases when term limits are binding but Dalle Nogare and Ricciuti (2011) consider 52 countries and find no impact on government spending or deficits, except in presidential systems where a lame duck is correlated with reduced public spending.

These inconsistent empirical results on term limits' impacts may relate to the contradictory incentive effects term limits have and thus some inherent contingency in the empirics. On the one hand, term limits may restrict the possibility of re-electing a competent politician (Smart and Sturm 2006) or reduce the time to build power and network capital, and higher turnover might generate policy instability and reduce the provision of public goods (Tabarrok 1994). They might also create an incentive to shirk (Reed et al. 1998). On the other hand, term limits reduce reputational incentives which might lead a politician to misrepresent her preferences in a first term, and make the screening process for voters more difficult (List and Sturm 2006, Morris 2001). Term limits reduce a politician's time available to acquire the ability to reform institutions to block outsiders, or to benefit from an incumbency advantage, and thus benefit risk-averse ideological voters (Glaeser 1997). They also reduce the time available to become influential and thus generate more logrolling, and reduce the time available to be influenced by lobbies in favor of spending programs (Dick and Lott 1993). Finally, they might also shorten the time and money spent on re-election (Hayek 1979). Initial arguments in favor of term limits focused on US institutions where legislators' ability to direct spending towards their own district generates negative externalities: a district is the only beneficiary of its legislator's ability to increase, say, targeted earmarks, but all districts incur the cost of an increase in spending. Term limits might solve this coordination problem

(Buchanan and Congleton 1994).

More broadly, our paper falls within the literature on institutions and growth. Acemoglu, Johnson, and Robinson (2001) argue that extractive political institutions retard growth by narrowing power to a small interest group which then fosters the persistence of extractive economic institutions. Term limits can be thought of within this framework, since political office can be used by insiders to prevent potential reformers from competing for office (Glaeser 1997). Longer terms could also be used to create connections with private interests through, say, lobbying, encouraging extraction (Lopez 2003). We note also that the impact of term limits themselves should depend on the wider set of institutions present — both particular complementary institutions, as well as the overall level of institutional quality within a country. For example, List and Sturm (2006) provide evidence that the impact of term limits on environmental policies depends on a state's density of environmental organizations, and on the strength of political competition. In the case of US governors as in Besley and Case (1995) and Besley and Case (2003), the impact of term limits on fiscal policies must interact with the institutional constraint imposed by different state balanced budget rules. Finally, Querubin (2011) considers the case of the Philippines and finds that the imposition of term limits for all elected offices did not break the power of dynasties since members of the same families were able to alternate between different political offices.

The rest of the paper proceeds as follows. In Section 2.2 we describe how we select relevant events and model the determination of bond spreads. Section 2.3 reports our main results, and their robustness to a non-parametric test and the substitution of CDS prices for sovereign spreads. Section 2.4 provides potential interpretations of our headline finding discussing heterogeneity with respect to event characteristics and the level of institutional development. Section 2.5 concludes.

2.2 Methodology

We use an event study to analyze the impact of a 'shock' to executive constitutional term limits on a high-frequency series. In this section, we first describe the selection of what constitutes a "term limit event". We then describe the dependent variables we use to analyze the impact of these events. Next, we consider the literature on the determinants of country spreads to build the counterfactual necessary for the event study. We conclude this section by specifying the econometric technique we use for the final results.

2.2.1 Selecting term limit events

The events we are interested in are announcements made or actions taken by a branch of government which impact the institution of executive term limits. Examples of such events include a legislative vote on extending executive term limits, judiciary decisions on the constitutionality of those limits or their extensions, or executive statements of intentions. We select our events by surveying mostly English language newspapers covered by the *LexisNexis* and *Factiva* databases by looking for all articles mentioning "term limits" or its derivatives. In parallel, we closely inspected the timelines of news presented on the website of the International Foundation for Electoral Systems (IFES). Once we find an event, we run a more specific search to determine its exact timing and to properly understand the political context. Events are identified by a country and a specific date. We distinguish the events by whether they *restrict* or *extend* executive terms, and code which branch of the government has instigated the move[4].

4. We recognize however that presidents who want to eliminate term limits may ask their allies in the legislature to pretend it is their own initiative. Furthermore, the powers of each institution are different — the judiciary can very rarely take initiative in expanding term limits, but they may have discretion in deciding on the constitutionality of extensions.

Country	Frequency
Argentina	15
Brazil	19
Colombia	37
Kazakhstan	3
Sri Lanka	4
Nigeria	8
Venezuela	15

Table 2.1: Number of events by country

Type	Consequence	Frequency
Executive	Restriction	7
	Extension	21
Judiciary	Restriction	7
	Extension	5
Legislature	Restriction	10
	Extension	33

Table 2.2: Number of events by type and term limit impact

The final selection consists of 101 events from 7 countries for which we provide summary counts in Tables 2.1 and 2.2. About one third of our events are restriction events, while two thirds are extensions. We classify events into four different categories of initiators, including the three traditional branches of government (Executive, Legislative, and Judiciary). The fourth branch features events that we could not classify in those branches, including important polls and referenda. We are left with 12 events initiated by the judiciary branch (mostly, Constitutional Courts), 28 by the executive branch itself, and 43 from the legislature[5].

2.2.2 Outcomes

Our main outcome of interest is based on JP Morgan's country-level Emerging Market Bond Indices Global (EMBI henceforth) variables. The EMBI is a daily index of emerging market bonds produced by JP Morgan dating back to December 31st 1993[6]. The instruments included in the index have to have a face-value in excess of US$500mn, and a maturity of at least 2.5 years.

JP Morgan provides several variables linked to the EMBI. We focus on the *stripped spread*

5. The list of events, along with a detailed description of them, are available in an online appendix.

6. It aggregates "U.S.-dollar-denominated Brady bonds, Eurobonds, traded loans, and local market debt instruments issued by sovereign and quasi-sovereign entities"(Borri and Verdelhan 2011).

— the traditional measure of sovereign credit risk. This spread corresponds to the difference between a representative bond's yield over that of US Treasuries, stripped of any collateral effects and other potential enhancements. The stripping process means that the level of the stripped spreads reflects directly the change in the value of the bond, while the non-stripped spread would also reflect changes in the value of the collateral, for instance a Treasury bill. The stripping is aimed at measuring specifically the issuer's risk[7].

Second, as a check of robustness, we consider the price of a country's Credit Default Swap (CDS) on sovereign debt, for different maturities. CDS are, by construction, a price on default: the buyer of a sovereign CDS effectively buys insurance against sovereign default by paying a regular premium while the instrument repays in case of a 'credit event' (e.g. default) related to the underlying asset. CDS prices are only available since December 2002, and not for all countries. We settle on the CDS prices at three different maturities for Brazil, Colombia, Kazakhstan and Venezuela: 1-year, 5-year and 10-year instruments.

The EMBI and the CDS prices are directly linked to the perceived country-risk, and in particular the country's perceived default probability, and a rich literature has been developed on the relationship between Sovereign – and US corporate bonds — and corresponding CDS markets (Longstaff et al. 2007).We use the EMBI for our main analysis for several reasons: the EMBIs are available for a larger set of countries and for a longer time period, whereas CDS data for our countries is available only starting in January 2004. In particular, we report the starting dates of the EMBI spread data in Table 2.3 and of the CDS data later in Table 2.5. While Longstaff et al. (2007) argue that in general CDS are more liquid than sovereign debt for institutional reasons, our use of the EMBI alleviates that concern somewhat, since JP Morgan has minimum liquidity requirements for including instruments in the

7. Consider the price p of an asset yielding certain cashflows over some period of time. The stripped spread SS_t of an instrument yielding a cashflow CF_t and with a zero-coupon rate R_t is given by $p = \sum_t \frac{CF_t}{(1+R_t+SS_t)^t}$. The blended spread is $R_t + SS_t$. See Kim (2004).

EMBI. Furthermore, Hilscher and Nosbusch (2010) find that default probabilities explain a large variation of the index and Ammer and Cai (2011) establish that the long-run behavior of bond yields and CDS premia are similar. Our results are also robust to the alternative use of CDS prices.

Table 2.3: Starting date for sovereign stripped spread data in sample countries

Country	EMBIG stripped spread
Argentina	December 31st 1993
Brazil	April 29th 1994
Colombia	February 28th 1997
Kazakhstan	June, 29th 2007
Nigeria	December 31st 1993
Sri Lanka	November 30th 2007
Venezuela	December 31st 1993

We consider only dollar-denominated outcomes to exclude exchange rate variation (Broner, Lorenzoni, and Schmukler 2007). Hund and Lesmond (2008) note that $6.5tn of emerging market debt was traded in 2006, and half of it was denominated in non-local currency.

As usual with financial data, these dependent variables exhibit a high degree of autocorrelation, hence we use their daily growth rates. This is important for the interpretation of our results since we mainly present effects on the estimation of *daily* abnormal returns on the *growth* of stripped spreads and the *growth* of sovereign CDS prices.

2.2.3 A model of bond spreads

There is no standard model for the determination of bond spreads like the factor models for stock market returns. We therefore construct a model which will provide the benchmark against which the country's bond spread (or the CDS price) is compared, around the event. There are two important criteria we use to select the independent variables that model the spreads. First, they must have explanatory power. Second, they should not be impacted by

the country-specific shock to term limits. Fortunately, the international finance literature has found that a small set of global variables can explain a large share of the variation in bond yields. In the absence of strong theoretical structure, we use standard tools of model selection to choose the independent variables in the estimation window.

A standard result on international capital flows is that inflows in emerging markets are driven by the (international) lenders' behavior more than borrowers' conditions[8]. This is important because it allows us to use variables exogenous to the *political environment* in the relatively small emerging countries we consider while still accounting for substantial part of the variation in our dependent variables.

Understanding why this is the case also helps in choosing which variables to include in the estimation. Four channels emerge from the literature.

First, several authors have underlined the link between the US monetary policy and investment flows to emerging markets (Eichengreen and Mody 1998). More generally, US interest rates and others will matter due to arbitrage (Collin-Dufresne, Goldstein, and Martin 2001). But, the effect of US rates on bond spreads is not clearly understood, with different authors reporting different effects (e.g. Cline and Barnes (1997) or Diazweigel and Gemmill (2006)).[9] Others contend that one should instead use swap rates since better proxies for the 'risk-free rate'[10] (e.g. Blanco and Brennan (2005), Zhu (2006)). We also consider the LIBOR, which measures the cost of interbank borrowing (Duffie, Pedersen, and Singleton

8. See among many others Fernandez-Arias (1996), Mauro, Sussman, and Yafeh (2002), and Diazweigel and Gemmill (2006), for the bond market and Chuhan, Claessens, and Mamingi (1998) for determinants of equity flows.

9. A tentative reconciliation of these results is offered by Uribe and Yue (2006) who find that innovations in the short-term US interest rate can explain 20% of the variation in emerging market spreads at quarterly frequencies. After a positive shock to the US short rate, emerging market spreads initially fall (the interest rate increases less than the US interest rate) and then overshoot.

10. The swap rate is the fixed rate paid in an interest rate swap.

2001).

Second, a more recent literature has put great focus on variables proxying for the risk-aversion of international investors. Typically we proxy volatility with the Chicago Board Options Exchange Market Volatility Index (VIX). The VIX measures market volatility by considering the price of options. Greater volatility is associated with higher spreads (Remolona, Scatigna, and Wu 2007). Some authors have used US corporate bond indices to measure risk-aversion with the same findings[11]. Others use the difference between Treasuries and swap rates, and AAA-rated corporate bonds and Treasuries (Favero, Pagano, and Thadden 2009). Finally, Borri and Verdelhan (2011) use the TED spread, which is the difference between the interest rate on interbank loans and Treasury bills. Various authors have built their own indices of risk and also found a significant positive impact of risk aversion on bond spreads (Baek, Bandopadhyaya, and Du 2005).

Third, the economic health of lenders matters. Higher international interest rates can suggest a period of global growth and a steeper US yield curve suggests expectations of higher future short term interest rates and reveals expectations of growth. The yield curve has been found to have a negative impact on emerging market spreads (Martell 2007). Global conditions have also be proxied by the S&P500 index and other global market aggregates with the same effect[12].

Finally, we follow Hilscher and Nosbusch (2010) among others by adding the Commodity Research Bureau commodity index (CRB-CI) which aggregates the prices of future deliveries of a (variable) set of commodities. An indicator of commodity prices is important given that

11. See McGuire and Schrijvers (2003) or Borri and Verdelhan (2011) who use US BBB-rated corporate debt; or Garcia-Herrero and Ortiz (2006) or Wooldridge and Domanski (2003) who use the high-yield spread over treasuries or spreads in bond yields of corporations with different ratings.

12. Garcia-Herrero and Ortiz (2006) find that US growth has a negative, although insignificant impact on spreads. Diazweigel and Gemmill (2006) use the S&P500; Westphalen (2001) the MSCI world index; and McGuire and Schrijvers (2003) the S&P500, Nasdaq, and FTSE.

most of the emerging markets we are interested in are important players in global commodity markets.

Thus, the number of factors that one could use in the estimation window of our event study is large. We employ standard methods of model selection to include the most relevant variables in our estimation. We regress the EMBI Global Composite (EMBI-GC), an aggregation of all the EMBI indices for all emerging countries, on the set of variables mentioned above over a period of 3000 days. Following Groemping (2006), we computed various importance metrics for each variable (for instance the R^2 contribution averaged over the different regressors orderings, the contribution when a variable is included first or last, the product of the standardized coefficient and the correlation between the EMBI-GC and the variable). Finally, we compute bootstrap confidence intervals for these measures of relative importance[13].

The various selection methods yield similar results, and we ultimately include in our model the following: the yield curve for growth, the high yield spread (High yield corporate bonds minus 10-year treasuries), the VIX, the BBB-AAA spread and the TED spread for global volatility, the long term US rate, the swap rate, and the S&P futures for arbitrage variables, and the New York Fed commodity index. The results are not overly sensitive to this specification.

Since our outcome variables are the rate of growth of stripped spreads and CDS price growth, we use the growth rates of the variables above in our regression.

13. The program is available from the authors upon request.

2.2.4 Event Study

Following Corrado (2011) we use estimation windows of 250 days prior to our events to develop a counterfactual. We close the estimation window 10 business days prior to our events. Because events often appear in a cluster (e.g. different government branches take actions for an extension from one to two possible terms), we use the *same estimation window* for events taking place in the same cluster. Formally, an event occurs in cluster C if it happens less than a year after another event in the same country. Thus, our estimation windows correspond to a year before the first event in a cluster of events C. Finally, to measure the event's impact, the "event windows" consist of 10 days before and after each event.

Denote C as an indicator for a "cluster of events", and denote $E_C = \{events \in C\}$ as the set of events in that cluster. For each C, We regress the variable of interest, e.g. the stripped spread's growth rate, on the dependent variables listed above, and a dummy variable to capture the impact of the event(s) in E_C[14]. Hence, if s_t is the stripped spread on date t, the dependent variable we consider at date t is $y_t = \frac{s_t - s_{t-1}}{s_{t-1}}$. For $k \in \{-X, \dots, X\}$ defined below, we run the regression for each cluster of events C:

$$y_{Ct} = \alpha_C + \beta X_{Ct} + \sum_{i \in E_C} \gamma_{C,i}(k)\mathbf{1}(t \in I(k)) + \epsilon_{Ct}$$

where X_{Ct} is the set of independent variables, and k corresponds to the number of days before or after the event. $I(k)$ is the set of dates on which we compute the abnormal stripped spread growth, i.e. the events' impact. Namely, if an event is dated at $t = 0$[15].

14. Sandler and Sandler (2012) use simulations to show that "Allowing multiple event-time dummies to be turned on at once generally produces unbiased estimates."

15. Note the abuse of notation given that we have multiple events in a row.

- if $k \geq 0$, $\mathbf{1}(t \in I(k)) = 1$ iff $t \in \{0, \ldots, k\}$
- if $k < 0$, $\mathbf{1}(t \in I(k)) = 1$ iff $t \in \{k, \ldots, -1\}$

Finally, to test our hypothesis on restrictions versus extension events, the dummies are preceded by a minus in case of restrictions. This means that our hypothesis is that the γ's should be positive in the case that the dependent variable is the stripped spread: a restriction event should decrease the idiosyncratic risk and thus the stripped spread, while an extension should have the opposite effect.

The coefficients of interest are the $\gamma(k)$'s for all k, and represent the cumulative abnormal return over $|k|$ days. When $k \geq 0$, the coefficient represents the daily abnormal return on the variable of interest after the event. If $k < 0$, the coefficient represents the same quantity before the event. We exclude the event day from the negative k's since we want to use those quantities as "placebos" against the post-event abnormal returns.

To assess the significance of this daily abnormal return, we estimate $\gamma_{C,i}(k)$ and obtain the abnormal return for a specific event $\hat{\gamma}_{i,C}(k)$. We compute the robust standard error for the average estimated abnormal return in each cluster $\hat{\gamma}_C(k) = \frac{1}{\#C} \sum_{i \in E_C} \hat{\gamma}_{i,C}(k)$. Finally, to aggregate at a higher level (e.g. over all event clusters), we assume that the γ_C coefficients are independent across clusters of events.

2.3 Results

We find that investors respond to news about term limits. Our results suggest that investors view 'tightenings' more favorably than 'extensions' since bond spreads fall significantly after term limit restrictions while they increase after extensions. However, we uncover an interesting asymmetry by showing that the impact of extensions, while positive, is insignificant. It may be that while restrictions on an executive's term generally improve investor confidence

through signalling de facto separation of powers, a loosening of term limits may already be priced into expectations. These low expectations, therefore, act as an economic drag for the country since making borrowing permanently more costly.

2.3.1 Effect on country spreads

Figure 2.1 shows the daily abnormal growth of the stripped spread over a given event window around an event, when the abnormal returns coefficients are aggregated over all events. In the figure, the event takes place at time 0; on the right, the values are the daily abnormal returns when the event window is between 0 and $x > 0$ days after the event, while on the left of the event, the daily abnormal return is computed over a window excluding the event, between $-x > 0$ and the day before the event. The numbers are daily abnormal returns, so that the overall impact of the level variable (here, the stripped spread) is a compounding of these numbers.

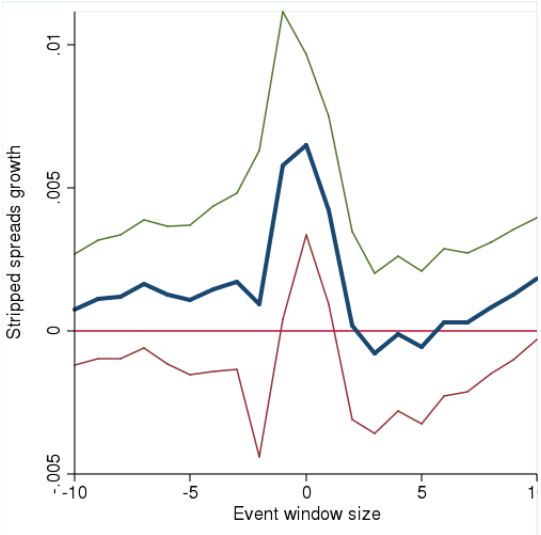


Figure 2.1: Daily abnormal return for different event windows
. The figure shows the daily abnormal slope of the stripped spread between the day of the event and x days after the event (if $x > 0$), or between x and the day before the event (if $x < 0$). The 95% confidence interval under the assumption of normality is displayed.

In the regression, the dummy *was signed negatively for restriction events and positively for extension events*. Given this specification, we expect a positive and significant abnormal return around and after the event, since greater confidence of investors, following a restriction, should be reflected in an abnormally negative stripped spread growth, and thus abnormally lower costs of borrowing. Extension events should be associated with higher growth in spreads if viewed negatively. Figure 2.1 suggests that a change in term limits has the expected impact on stripped spreads on the day of the event, but that the effect dies down, although becoming nearly statistically significant at the 5% level when the event window is extended to 10 days after the event. Specifically, the abnormal return is significant the day before the event, and at the day of the event or a two-day window including the event day and the following business day. The first result tells us that there might be some anticipation by the market, and that the anticipation is correct in the sense that the direction of the abnormality is the same as the post-event abnormality. The second result shows that on the event day, a term limit event leads to an abnormal return of half a percentage point (the daily abnormal return on the day of the event is .65 percentage points, it is .58 the day before, and .42 over a two day window including the day of the event and the following day[16]).

Restrictions vs Extensions.

We consider separately restrictions and extensions in Figures 2.2a and 2.2b respectively. Restrictions of term limits are associated with a significantly higher response of stripped spreads compared to extensions. As shown in Figure 2.2a and in Table B.1, the impact is six times greater on the day of the event: the shock generates abnormal returns on stripped spreads of 2 percentage points. For comparison, the standard deviation of the country EMBI

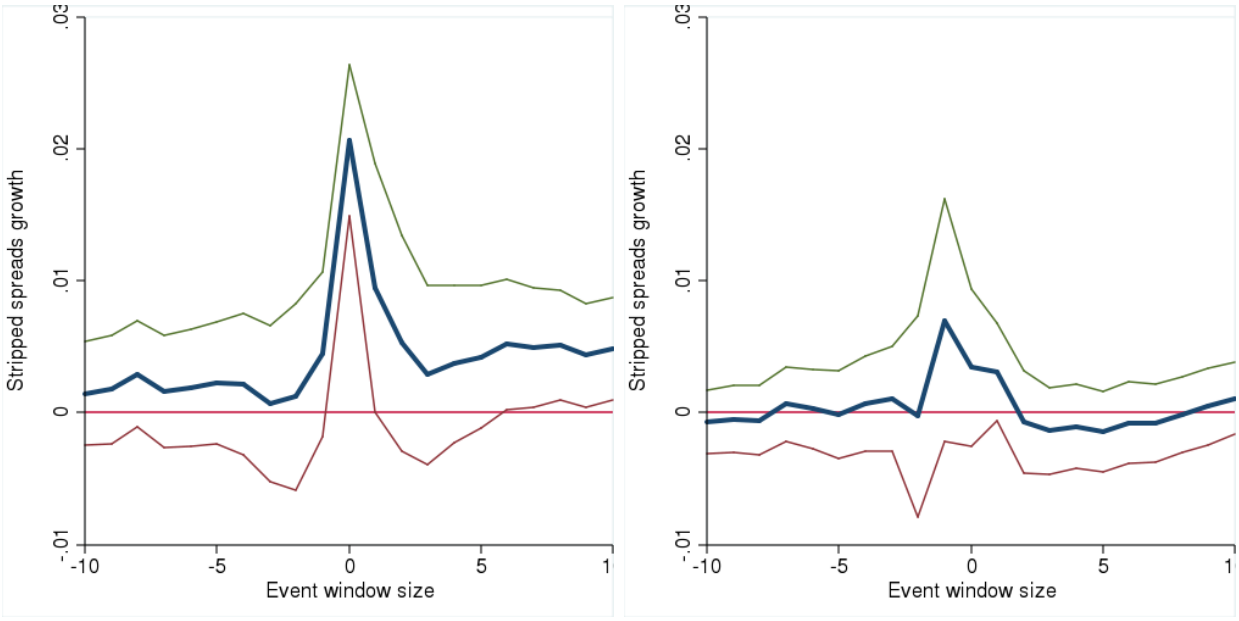
16. The exact numbers for all window sizes are reported in Table B.1.

indices is 3 percentage points. Although the daily abnormal return is insignificant at the 5% level when the event window is extended to 5 days after the event, the extension of the event window further out shows a significant, persistent impact of half a percentage point. In contrast, the daily abnormal return after extension events has the hypothesized sign but is insignificant at the 5% level irrespective of the length of the event window. It is also insignificant at the 10% level except for the day before and after the event.

We cannot rule out entirely that extensions are simply better anticipated, but several factors suggest to us this is not the cause. First, the insignificant abnormal returns as many as 10 business days before the event would imply that the event has been anticipated long before. Second, results are similar when restricting the sample to the first events in the independent cluster of events (recall that we use the same estimation window for events which happen in quick succession). A related consideration is that extensions typically require a large number of procedural steps, so one might think that an individual "extension event" may not contain much new information. Again, our analysis on the *initial observation* of each cluster of events — which are presumably the most newsworthy — which yields similar results suggests this is not what's driving the results.

One interpretation and consequence of these results is that emerging economies live under a permanent *institutional drag* on growth and policy flexibility: borrowing costs are persistently higher than they would be not only if the constraints on the executive were stricter, but higher than they would be if the *expectations* on those constraints were that the status-quo would be preserved as opposed to weakened. The potential impact on GDP and investment from the positive shock to spreads and thus the higher cost of borrowing is the imputed price of this institutional drag, which we might call a 'curse of low expectations'.

To have a better grasp of the potential implications of this result, we run a hypothetical investment exercise. Consider someone investing $100 in an asset which returns the EMBI



(a) Restrictions *(b) Extensions*

Figure 2.2: Daily abnormal return for different event windows for restrictions (left) and extension (right) events.

Restrictions are coded negatively, extensions positively

stripped spread of a country experiencing the event, the day before a term limit event[17]. Using the value of the daily abnormal return found in the estimation for a given window of size k, a compounding over these k days yields the final return at the end of the event window. We display the results, along with confidence intervals generated via the Delta Method, in Figures 2.3a and 2.3b, and the exact numbers are shown in Table B.2 in the appendix. Such an investment generates a significant return of investment of *5 percentage points* after restrictions: 10 days after the event, that investment would be worth \$105. After an extension, the value of the investment over any given window after the event is statistically indistinguishable from the value of the original investment. The values of such an investment looking backwards and using the pre-event abnormal returns are also insignificant

17. Note that we consider, in the case of restrictions, an investor who sells short the asset yielding the stripped spread – the spread actually falls.

independently of the type of event.

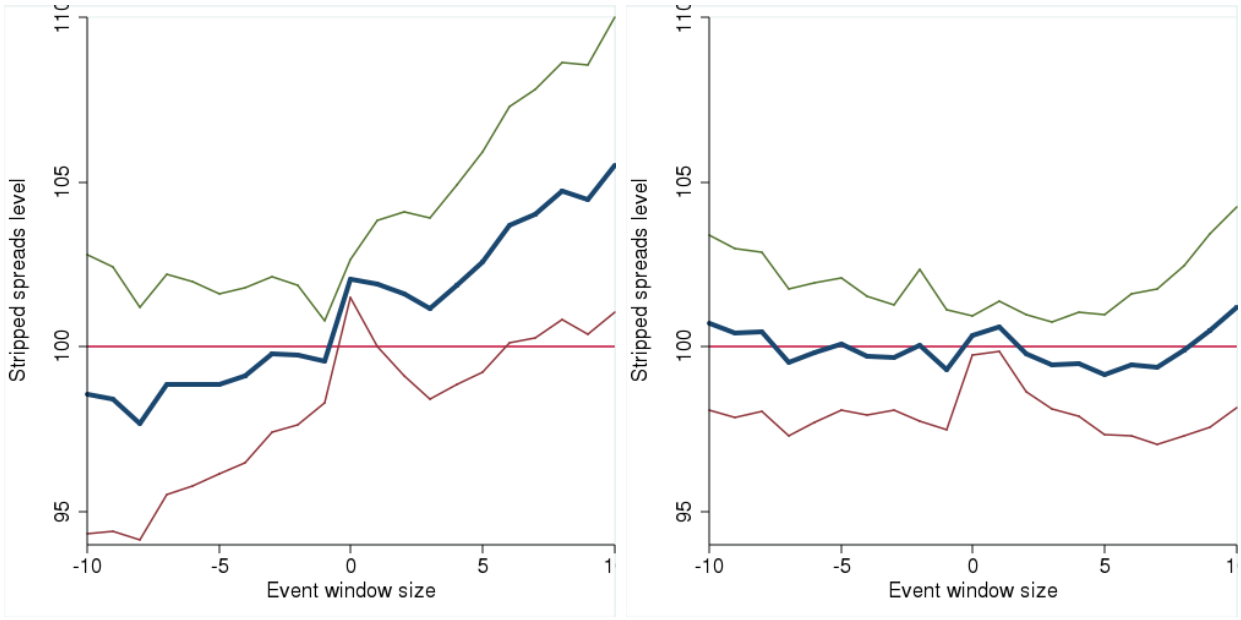
We showed that the costs of borrowing were significantly impacted after restrictions on executive term limits. What effect can this have on macroeconomic variables like growth and investment? Uribe and Yue (2006) find that the impact of a temporary 1 percentage point (pp) shock to a developing country's spreads leads to at least five quarters of output and investment significantly below trend, with a short-term (one-quarter) multiplier of respectively .2 and .6. The 5 pp shock after restriction events is over 10 business days, and Uribe and Yue (2006) uses quarterly data, but the persistence of the increase suggests to us that the events plausibly may be a significant shock on country spreads. If this short-term shock translates into 1 pp shock at the quarterly level, this would imply two years with investment .5 pp below trend and output .1 pp below trend. Restriction events *could* thus have substantial, positive impacts on a country's economy relative to its previous potential.

2.3.2 Robustness of effects

Non-parametric test

Event studies are joint tests of whether the abnormal returns are zero and whether the assumed model of normal returns is correct. For this reason, we develop a non-parametric placebo test of our events which confirms our headline findings and their significance.

We compute, for each dyad $country \cdot event$, a random date, and substitute this date for the actual, original event date. We then perform exactly the same analysis as described in Section 2.2, on this virtual list of events. We repeat this exercise 100 times. For all $b \in [1, 100]$, we estimate the daily abnormal return $\hat{\gamma}^b_{i,C}(k)$. We then look at the share of estimates from those simulations that are below our estimate on the actual set of events. This share can then be interpreted as $1 - p$ where p is the p-value of our initial null hypothesis.



(a) Restrictions *(b) Extensions*

Figure 2.3: Value of a \$100 investment made the day before the event $x \geq 0$ days after the event, or of a \$100 investment made the day of the event looking backwards to $x < 0$ days before the event.
The calculation uses the abnormal returns estimates. Confidence intervals are computed via the Delta Method.

The results of this test are presented in Table 2.4. The first column shows the share of estimates below the value we find in the analysis of the true events when we aggregate all events. The next two columns divide extension and restrictions. The daily abnormal return appears insignificant in extension events irrespectively of the length and direction of the event window. By contrast, the restriction events are significant at the 2% level starting at the day of the event. The daily abnormal returns are significant at the 10% level only when considering a period of 5 days after the event, and at the 5% level again thereafter. When considering the aggregate set of events, the effect of the event is significant at standard levels only one day before to one day after the event.

Importantly, this test also shows that the abnormal returns before the events are statistically insignificant. As in the parametric estimation, the events are not 'anticipated' prior to the event — the behavior of investors is indistinguishable from their usual behavior two weeks before the shock to executive term limits.

Table 2.4: Share of daily AR estimates from 100 simulations below the estimate on the actual table of event.

Event window limit	All Events	Extensions	Restrictions	Event window limit	All Events	Extensions	Restrictions
-10	0.723	0.465	0.673	**0**	**0.921***	**0.772**	**0.98****
-9	0.842	0.485	0.713	1	0.98**	0.713	0.98**
-8	0.822	0.564	0.782	2	0.703	0.376	0.941
-7	0.822	0.723	0.653	3	0.505	0.396	0.901
-6	0.782	0.663	0.693	4	0.673	0.436	0.941*
-5	0.842	0.683	0.752	5	0.525	0.406	0.921*
-4	0.861	0.733	0.792	6	0.752	0.495	0.98**
-3	0.842	0.693	0.673	7	0.743	0.426	0.97**
-2	0.683	0.574	0.634	8	0.871	0.515	0.98**
-1	0.921*	0.891	0.782	9	0.891	0.614	0.98**
				10	0.941*	0.693	0.98**

$***$, $**$ and $*$ significance at 1, 5 and 10% levels, respectively

The robustness of the parametric result to this non-parametric exercise confirms the main implications of our findings. First, there is a significant difference in market response to extension and restriction of executive term limits. More precisely restriction events have a significant, long-lived and negative impact on spreads: it lowers the country-specific risk

priced by investors. On the contrary, extensions do not move markets in the long-run.

Effect on CDS prices

The study of the behavior of sovereign CDS prices provides another robustness check of the headline results on stripped bond spreads. As explained in Section 2.2, CDS prices and stripped spreads, two measures of countries' default risk, are closely related. The primary reason for not using CDS prices in the main analysis is limited data. However, we would expect, at least, an effect of term limit events in the same direction when using CDS prices. We thus perform the same analysis as described in Section 2.2, using the CDS price growth instead as dependent variable.

Table 2.5 first shows the countries and dates for which CDS data is available. The CDS data is more limited than the EMBI spreads — the table below covers 54 events, of which 8 are restrictions. 35 events are from Colombia.

Table 2.5: Starting date for sovereign CDS data in sample countries

Country	CDS 5-year	CDS 1-year	CDS 10-year
Brazil	January 1st 2004	May 3rd 2004	May 3rd 2004
Colombia	January 5th 2004	June 1st 2004	June 1st 2004
Kazakhstan	August 18th 2005	October 3rd 2005	October 3rd 2005
Venezuela	May, 10th 2004	June 1st 2004	June 1st 2004

After estimating the event-study regression, we find that sovereign CDS prices behave similarly to stripped spreads around the events. The cost of insuring against default on a representative country's bonds falls sharply following restriction events. The response of CDS prices to our events are displayed in Table 2.6. The short-term CDS price responds significantly at the time of the event, with a daily abnormal return of 3 percentage points in the price of 1-year CDS and 2 percentage points for the 5-year CDS[18]. There is no significant

18. It is important to note that the 5-year CDS is the most commonly traded CDS contract. Dividing

impact on the price change for the 10-year CDS. This suggests that investors believe that the change in the term limit legislation is going to have an impact in the medium term, but that long-run country-risk is not significantly altered.

Again, the impacts are stronger for restriction events with a drop of 6 percentage points in CDS price growth in a window of 2 days around the event for the 1-year CDS — more than three times the change generated by extension events. In the longer run, the restrictions appear to have a persistent effect on short-term CDS, although the significance is only at the 10% level 10 days after the event – we attribute this difference to our headline result with the smaller set of events for which we have CDS data. In the long term, the restriction events decrease the price of insurance against default by a percentage point every day; a cumulative abnormal return of 10% for this instrument.

When one looks at the 5-year CDS market, the effect of term-limit restrictions differs somewhat; the short-term impact is larger while the long-term impact is muted. We attribute the stronger short-run response to the fact that the 5-year CDS is the most liquid of CDS maturities, while the longer-term response is weaker because investors appear to believe that the effect of the event on long-term country risk is more uncertain than in the short-term. Restriction events yield an event-day impact of 14 percentage points and a 2-day abnormal return of 7 percentage points. However, the effect appears to be short-lived compared to the 1-year CDS, which could, again, be linked to a difference in liquidity. In the long run, the abnormal returns on long-term CDS instruments are insignificant, although they appear higher than in the period before the event[19]. Finally, the effect of extension events is also insignificant, either in the short or long-run for both the 5-year and 10-year CDS.

---

the contracts by maturities means that we do not control for the heterogeneity in the liquidity at different maturities.

19. We do not report the results of the 10-year CDS which are insignificant in both cases.

This, importantly, confirms the result we found for sovereign stripped spreads in that there is a clear asymmetry between restriction and extension events. Moreover, the distinction by CDS maturity also suggests that the reaction of investors is more muted when looking at the impact of events further out.

2.4 Discussion

We saw in the previous section that restriction on executive term limits led to a negative revision on the perception of a country's default risk, as described by its borrowing costs. Extension on those term limits, instead, had no significant impact. Why is that the case? In order to get a better understanding of these striking results, we analyze the directions and sizes of the events' impacts further by disaggregating the events in different categories. Section 2.4.1 distinguishes events by the governmental branch initiating them, Section 2.4.2 analyzes the importance of the variation in the institutional contexts in which they occur.

2.4.1 Event initiators

An important question is whether the reaction is linked to the term limit *institution* itself or new information about the *type* of the leader in charge. To answer this question, one possibility is to distinguish between events initiated by the leader to whom the shock is applied – here, the executive power – and events instigated by the other branches, the legislature or the judiciary. In cases where the president announces a desire for an extended term s/he is revealing something about his or her type, whereas if the parliament or a court moves to extend or limit executive terms, the information concerns the strength of the constitutional separation of powers.

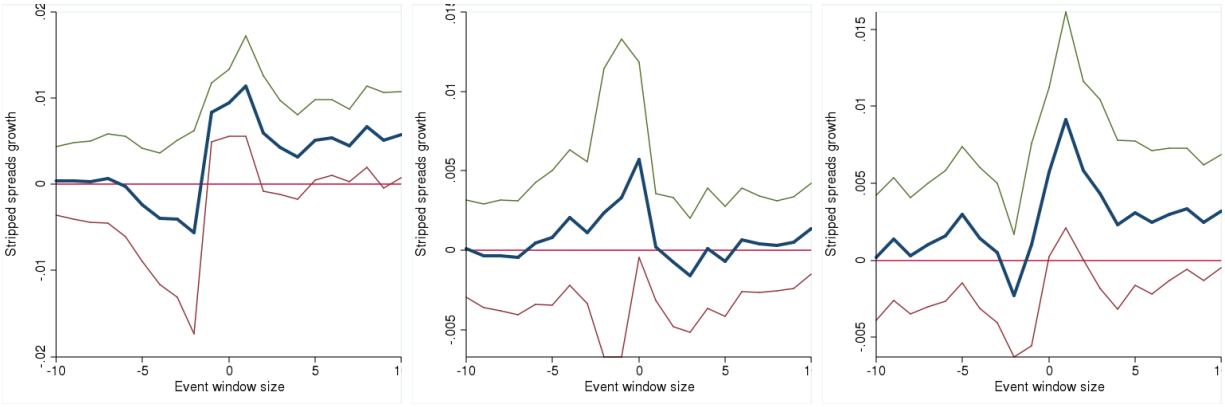
We repeat the analysis described in Section 2.2, separating the estimated effect by the

Table 2.6: CDS Daily abnormal returns, in percentage points, by event window size – A negative number x correspond to a window between $-x$ days and -1 day before the event, a positive number x is a window between the event day and x day after the event.

Event window	1-year CDS			5-year CDS			10-year CDS		
limit	Extensions	Restrictions	All	Extensions	Restrictions	All	Extensions	Restrictions	All
-10	-.06	.04	-.09	.05	-.41	.04	.13	-.38	.1
	(.46)	(.71)	(.43)	(.47)	(1.36)	(.35)	(.21)	(.4)	(.19)
-9	.21	-.1	.15	.03	-.02	.07	.17	-.37	.12
	(.48)	(.76)	(.45)	(.33)	(1.15)	(.27)	(.22)	(.43)	(.2)
-8	-.11	.33	-.11	.21	-.86	.17	.21	-.06	.18
	(.48)	(.86)	(.46)	(.44)	(1.41)	(.35)	(.28)	(.58)	(.24)
-7	-.31	-.03	-.33	.27	-.44	.26	.17	.11	.16
	(.51)	(.94)	(.49)	(.64)	(1.75)	(.49)	(.27)	(.57)	(.25)
-6	-.47	.26	-.45	.63	-.49	.54	.09	.23	.11
	(.56)	(.96)	(.54)	(.83)	(2.27)	(.59)	(.26)	(.55)	(.25)
-5	.39	-.59	.29	-.1	-.15	-.02	.26	.63	.3
	(.53)	(1.01)	(.49)	(.9)	(2.38)	(.66)	(.28)	(.63)	(.26)
-4	.09	.34	.11	.26	.35	.31	.4	.13	.37
	(.53)	(1.04)	(.52)	(.39)	(1.33)	(.37)	(.33)	(1)	(.34)
-3	.05	-.6	-.08	.35	.76	.42	.31	.56	.28
	(.7)	(1.18)	(.63)	(.39)	(1.22)	(.35)	(.34)	(.64)	(.32)
-2	-.81	.79	-.77	.42	2.71**	.85**	.6*	-.11	.56*
	(.81)	(1.13)	(.79)	(.43)	(1.47)	(.38)	(.42)	(.84)	(.4)
-1	-1.69***	.07	-1.67***	.64**	-.38	.46*	.06	.34	.14
	(.41)	(1.16)	(.39)	(.28)	(1.31)	(.3)	(.19)	(.74)	(.21)
0	2.59***	8.05***	3.07***	.38	13.57***	2.01***	-.05	7.77***	.58***
	(.46)	(1.48)	(.41)	(.35)	(1.92)	(.29)	(.22)	(.77)	(.21)
1	1.23*	6.27***	1.64**	-.05	6.51***	.72**	.08	5.29***	.5
	(.92)	(1.33)	(.9)	(.41)	(2.44)	(.39)	(.44)	(1.75)	(.43)
2	-.61	.36	-.59	.01	2.79*	.27	-.13	2.55*	.08
	(.74)	(1.7)	(.71)	(.37)	(2.06)	(.35)	(.36)	(1.63)	(.35)
3	.33	.58	.41	-.49*	1.75	-.29	-.18	2.13*	0
	(.74)	(1.42)	(.73)	(.35)	(1.76)	(.33)	(.35)	(1.47)	(.33)
4	-.49	2.35**	-.15	-.69**	1.97**	-.43*	-.27	1.8**	-.09
	(.71)	(1.13)	(.7)	(.35)	(1.1)	(.32)	(.29)	(.88)	(.27)
5	-.31	.91	-.17	-.73**	1.66**	-.48*	-.41*	.05	-.35
	(.7)	(1.18)	(.68)	(.34)	(.97)	(.31)	(.31)	(1.72)	(.32)
6	-.05	1.09	.07	-.17	.73	-.06	.04	.43	.07
	(.62)	(1.03)	(.6)	(.32)	(.91)	(.27)	(.3)	(1.66)	(.3)
7	-.15	1.25*	0	-.17	.39	-.06	.02	.55	.07
	(.56)	(.92)	(.53)	(.35)	(.91)	(.3)	(.33)	(1.57)	(.31)
8	.11	1.45**	.28	.13	.24	.2	.13	.79	.2
	(.52)	(.77)	(.49)	(.34)	(.85)	(.29)	(.26)	(1.37)	(.26)
9	.15	1.15*	.29	-.25	1.11	-.05	0	.89	.1
	(.47)	(.72)	(.45)	(.4)	(.99)	(.32)	(.25)	(1.25)	(.25)
10	.31	1*	.43	.13	.31	.24	.22	.5	.27
	(.42)	(.68)	(.4)	(.3)	(.77)	(.26)	(.24)	(1.17)	(.24)

Coefficients and standard errors in parenthesis.
$***$, $**$ and $*$ significance at 1, 5 and 10% levels, respectively

event's instigating branch. Figures 2.4a to 2.4c, display the estimated abnormal returns by branch[20].



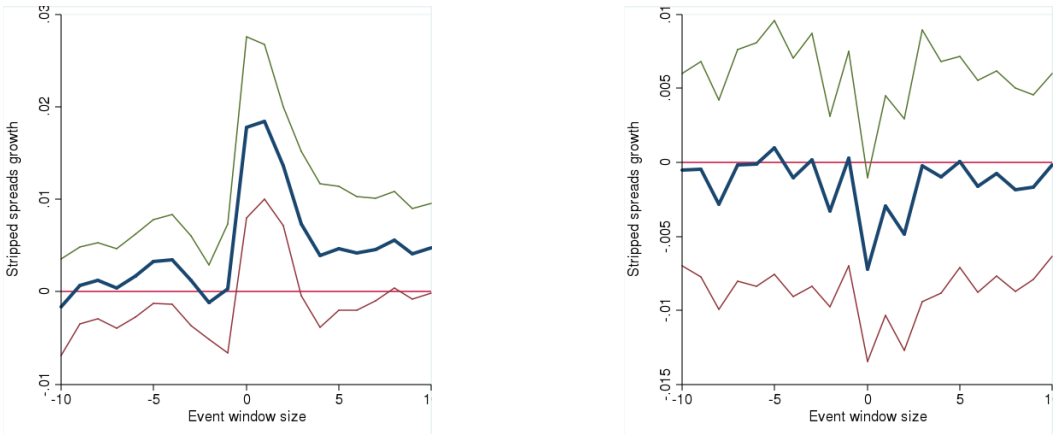
(a) Executive *(b) Legislative* *(c) Judiciary*

Figure 2.4: Daily abnormal return, in blue, for different event windows by instigating branch . 95% confidence intervals are displayed

The results suggest that overall, *actions taken by the executive branch have a stronger, more permanent impact* than actions taken by the other two branches. The judiciary actions show evidence of an impact in line with our hypothesis although power limits its significance (there are only 12 events in this subcategory), while legislative actions show no evidence of having a significant impact on stripped spreads. Quantitatively, an event initiated by the executive generates a one percentage point abnormal return immediately and stabilizes at half a percentage point thereafter. A similar trend can be seen when considering the judiciary, although the abnormal return fails to be persistently significant at the 5% level This variation in the results suggest that, indeed, investors do take some view on the leader currently in power when reacting to the specific change of institutions. However, distinguishing restrictions and extensions lead us to nuance that assertion.

20. The exact numbers for the estimated effects are available in the appendix, Table B.3.

Extensions and restrictions. We divide again the events between restrictions and extensions. When we consider the judicial events in particular, we observe a stark difference between the two. Figures 2.5a and 2.5b show the daily abnormal return in restriction versus extension events respectively. When the judiciary takes an action linked to an extension of executive term limits, the movement of the stripped spreads is insignificant. However, an action restricting executive term limits has a strong, permanent impact on the cost of borrowing of around 2 percentage points per day initially, weakening to half a percentage point per day permanently over 10 days. We interpret this result as evidence that investors respond strongly to evidence of de facto separation of powers, unrelated to the idiosyncratic characteristics of the executive leader. The importance of judiciary decisions is reflected in one particular example, when it came as a surprise that the Colombian Constitutional Court struck down Uribe's path to a third term on February 27th, 2010. More generally, the data provides evidence that the decision of the judiciary has an impact when it is more likely to be in disagreement with the executive branch. On the other hand, investors do not seem to react when the separation of powers is not tested.



(a) Restrictions *(b) Extensions*

Figure 2.5: Daily abnormal return for different event windows judiciary-initiated events

Events linked to the executive branch itself also reveal an interesting contrast, as shown in

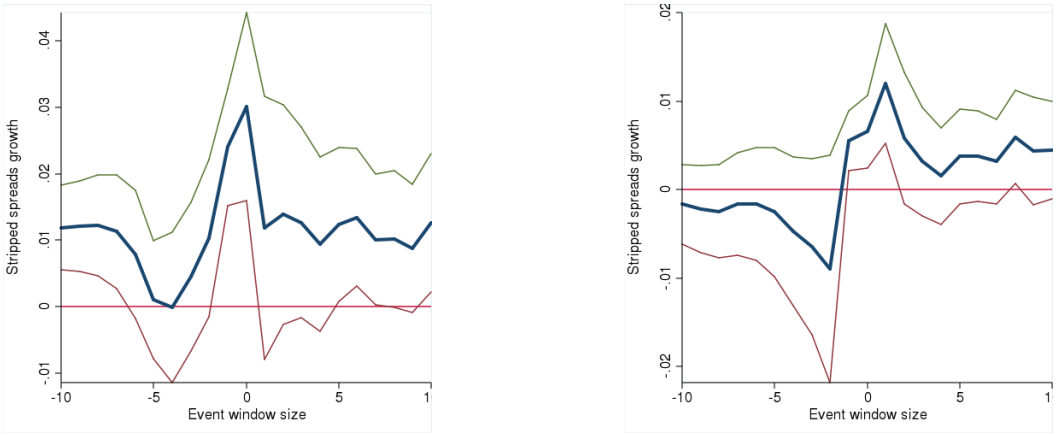
Figures 2.6a and 2.6b[21]. We observe that restrictions have a significant, large, and immediate impact of 3 percentage points on stripped spread growth. This effect dies down over time but remains around one percentage point *daily* 10 days after the event. The effect of extension events appear to be three to four times smaller and barely significant. The shock generated by restriction events launched by the executive also appears to be two to three times as large as when started by the other branches. This suggests that investors tend to see those changes as potentially more credible. The difference at the event day suggests, on the other hand, the fact that investors are more surprised by an executive action than by a legislative or judiciary action. This is intuitive since the executive generally initiates the sequence of procedural steps required to permit him or her to serve another term. Hence the executive events tend to contain more information.

Finally, we do not report the figures for the legislative branch because the abnormal returns are insignificant at the 5% level. However, for the sake of completeness, restriction events yield daily abnormal returns of half a percentage point over an event window of 10 days after the event, and those returns are significant at the 10% level only.

In conclusion, we derive from this analysis that *institutions do matter*, as evidenced by the difference between restrictions and extensions when looking at actions led by the judiciary: only when the judiciary takes a decision that goes counter to the executive (assuming the executive normally wants to extend his term) is there a strong impact.

---

21. It may appear puzzling that the abnormal return is significant 10 days before the event in the case of restriction events, but we view this as simply an effect of the sample size. The abnormal return is also insignificant the week before the event.



(a) Restrictions *(b) Extensions*

Figure 2.6: Daily abnormal return for different event windows executive-initiated events

2.4.2 Institutional quality

As we have noted above, the effects of term limit changes likely depend in important ways on the broader institutional context as well as the overall level of institutional quality in the country. Acemoglu et al. (2008) discussing the "seesaw effect" note that reforms may not be effective in very weakly institutionalized settings since reforms on one dimension often do not do anything to change the underlying political economy and end up being undone along another dimension. They consider Central Bank reforms (looking at effects on inflation) and find that reforms are most effective where constraints are *intermediate*. Reform has instead modest effects where institutions are already strong or very weak. One might think that this is a reason, for example, for the insignificant effects of term limit extensions — the countries where these occur may be weakly institutionalized at the time of the events. In order to analyze the link between institutions and the investors' response to the events, we consider the Worldwide Governance Indicators (WGI, see Kaufmann, Kraay, and Mastruzzi (2010)) provided by the World Bank. The WGI are a set of six indicators aggregating 30 data sources and providing information on dimensions such as "Voice and Accountability", "Government

Effectiveness" or "Political Stability". We compute an aggregate of those 6 dimensions using the first dimension of a principal component analysis, given that those indicators are highly correlated. This yields a rough measure of the institutional quality for each different $country \cdot year$ that we considered for our estimation windows[22]. We then consider the value of this first component against the average absolute value of the cumulative abnormal return over all events corresponding to an estimation window, and against the average actual values when dividing by restrictions and extensions.

The results are fairly striking, as displayed in Figure 2.7. In the graphs, each dot represents one independent event window, labelled with the 3-letter country code for the relevant country, and the last two digits of the relevant year at which we consider the institutional quality of this country.

In contrast to Acemoglu et al. (2008), the short-run percentage change is larger in the more weakly institutionalized settings.[23] When we separate restrictions against extensions, as shown in Figures 2.8a and 2.8b, the result appears again: restrictions have a stronger negative impact on spreads when the institutions are weaker. On the other hand extensions have a stronger positive impact on spreads under weak institutions. Although these results are only tentative for want of power, they underline how important conditioning the effect on institutional strength can be.

We test the significance of the relation between the world governance indices and the $country \cdot year$ average abnormal return at the day of the event. For this purpose, we regress the average abnormal return on the value of one of the six components of the WGI separately

22. Given that we used the same estimation window for some events in different years, in the same country, the $country \cdot year$ considered here are not event-specific but window-specific.

23. Though, we recognize that in order to even have traded EMBI country indices, it must be the case that the country is already somewhat well institutionalized. The implication then would be that the effects may well be non-monotonic in institutional quality.

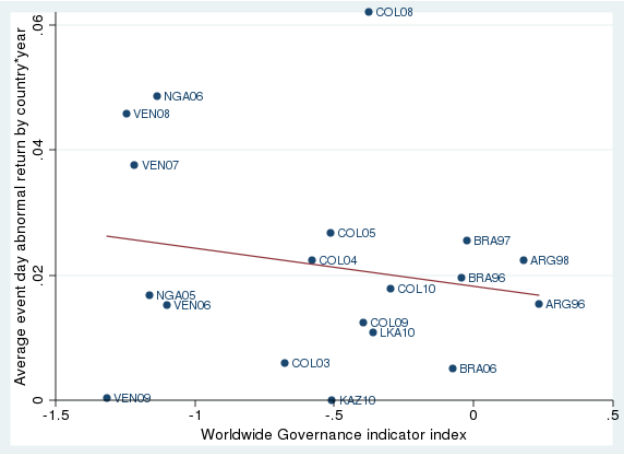
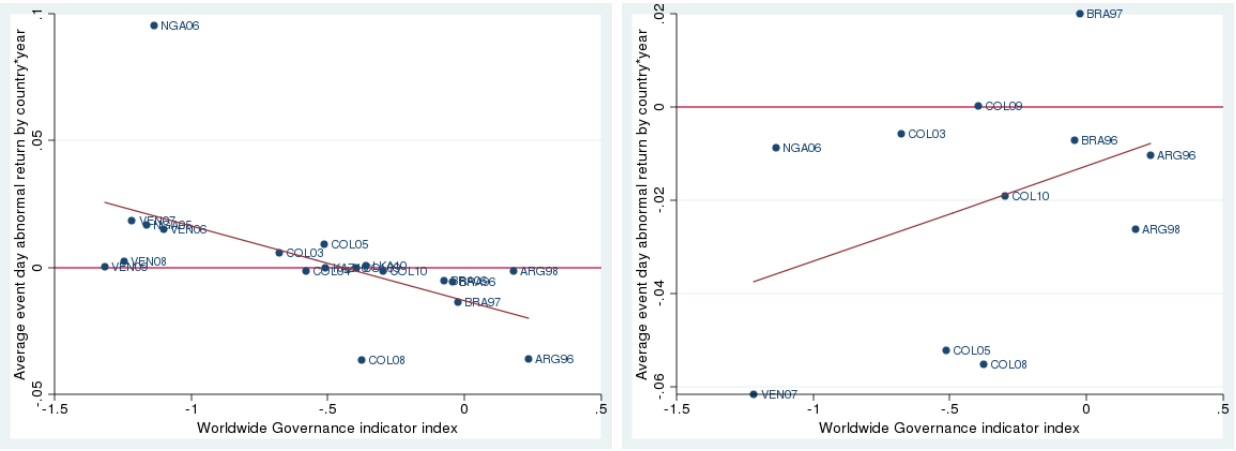


*Figure 2.7: Average event-day absolute abnormal return after events by country*year, against institutionalization measured by an aggregate of 6 World Governance Indicators, with linear fit.*



(a) Extensions *(b) Restrictions*

*Figure 2.8: Average event-day abnormal return after events by country*year, against institutionalization measured by an aggregate of 6 World Governance Indicators, with linear fit.*

for extensions and restrictions and the results are displayed in Table 2.7. While we do not find significant effects for our restriction events, extensions do differ significantly at the 5% level according to 4 out of 6 of the institutional quality measures when we bootstrap the standard errors with 1000 replications or use robust standard errors, while only the coefficient on "Voice and Accountability"(V&A) remains significant at this level when we cluster by country. Overall, it appears that the magnitude of the responses is bigger the weaker the

country's institutional quality. To give some sense of the magnitude of this effect, consider that in 2010, Brazil scored at .5 in the V&A measure while Nigeria and Venezuela scored around -.8. The correlation found here would imply a difference of 5 percentage points in the event-day abnormal return between these countries, which is more than 1.5 standard deviations of the EMBI indices. Interestingly, we can link the significance of the V&A variable to the importance of patronage and clientelism at the electoral level [24]. Extending term limits where accountability is high, the vote is fair, and the franchise is extensive is less likely to have a strong impact on the perpetuation of power and the potential for extraction by a single leader.

Table 2.7: Regression coefficient between WGIs and event-day abnormal return

	Extensions		**Restrictions**	
Governance index	Correlation	Std. err.	Correlation	Std. err.
Political Stability and Absence of Violence	-0.011	0.008	0.008	0.0091
Government Effectiveness	-0.032	0.014**	0.0134	0.0221
Regulatory Quality	-0.019	0.009**	0.0155	0.0161
Rule of law	-0.023	0.01**	0.0211	0.017
Control of Corruption	-0.03	0.015*	0.0217	0.0355
Voice and Accountability	-0.025	0.011**	0.0329	0.022

$***$, $**$ and $*$ significance at 1, 5 and 10% levels, respectively

Part of the reason why extension events appear insignificant is driven by the cases of Argentina (under Menem) and Brazil (under Cardoso). In both cases, the countries have relatively high WGI scores in the event years, in addition to high Unified Democracy Scores (Pemstein, Meserve, and Melton (2010)). Moreover, the two presidents were, at the time, viewed in fairly favorable light by financial market participants. Thus, we can surmise that the reasons for the extensions were understood by investors and perceived as credible and in the continuation of past policies. Hence extension events were associated with slightly reduced country risk and tighter spreads. In contrast, the problem for weakly institution-

24. Shefter (1977), Carey and Shugart (1995) or Baland and Robinson (2008) among many others.

alized countries is a lack of credibility: when the executive leader in Venezuela or Nigeria suggests a term extension, it reflects more information about his intentions to manipulate the country's institutions in his favor. For the same reason, a move towards a tightening of term limits re-assures those same investors.

When we drop Argentina and Brazil from our set of events[25], we indeed observe a stronger impact of the events on the stripped spreads compared to the study on all events, although we still have a stronger impact for restrictions compared to extensions. Interestingly, when we looked at longer windows after the event, it appears that the effects of both sets of events are significant and persistent when dropping these two countries.

Finally, our findings also relate to those of Frot and Santiso (2010) who find that a decrease in the quality of democracy lowers equity flows, but do not find improvements with democratic transitions; we find that investors reward a "reform" involving new restrictions on executive stay. We do not find a response to a "deterioration" in the sense of more power for the executive. However, depending on the circumstances of the country, extension events might be perceived as institutional continuity (e.g. Brazilian or Argentine cases) or institutional deterioration, in their terminology. Our results can therefore be partially reconciled with their findings, that are admittedly based on a more general indicator of institution at a lower frequency.

2.5 Conclusions

This paper has shown that markets respond to news about term limits, and in particular markets react with a lower default premium on dollar-denominated sovereign debt when that country introduces restrictions on executive term limits. On the other hand, exten-

25. The exact results are available upon request

sions of term limits only have a temporary impact on a country's borrowing costs, and are insignificant in the long-run. The findings discussed here offer various further avenues for research.

First, we suggest that in order to understand the impact of a change in a country's political institutions, one might want to consider the status quo ante: the effect differs depending on the institutional quality of that country when that change occurs.

Second, the heterogeneous effects of term limits events in particular are helpful in understanding the contradictory results found in the political business cycle literature concerning the impact of term limits on fiscal policies. In addition, we showed that two indirect channels have consequences for the link between term limits and government's budgets. Term limits do not only affect fiscal policies directly via the various theoretical incentive effects described in previous literature, but also indirectly through financial markets. First, weak term limits might burden a country's spending capabilities through permanently higher costs of borrowing, which we described as an "institutional drag". Second, the move towards stricter term limits lowers these borrowing costs persistently. Therefore, the context in which investors view term limits will affect how the limits interact with fiscal policies.

Third, we argue that such institutional shocks as executive term limits changes are important for the macroeconomic performance of emerging markets through country spreads. As we have discussed, Uribe and Yue (2006) find a substantial multiplier of country spreads on GDP and investment after two quarters. We provide here tentative evidence that institutional shocks such as restrictions of term limits have a fairly persistent impact on stripped spreads, with a decrease of five percentage points in the stripped spread after 10 days. Better identification of institutional shocks, and further analyses of the links between borrowing costs and countries' growth, could lead to better estimates of the institutional drag experienced by countries with political institutions which investors consider risky.

Finally, we argued in this paper that event-studies with high-frequency data are a legitimate, and important, complement to the more developed literature studying the link between institutions and growth using standard economic and political variables at lower frequencies. The use of an event-study methodology cannot yield a precise description of the long-run consequence of an institutional change, but can identify neatly the perception of this change by financial markets. Not only this impact should be correlated with the actual, realized long-term effect, but it might be self-fulfilling: higher borrowing costs, in our case, are likely to hamper growth and vice-versa.

The use of high-frequency data, however, requires a more thorough and precise method to identify events which can be described, for instance, as institutional change (or as discrete movements in the probabilities of institutional changes). To identify such events in a better way, we need to develop new and systematic measures of investor anticipation. Earlier drafts of this paper included measures of expectations using textual analysis, but the results were noisy.[26] In addition, the sentiment analysis literature has found that "surprise" is the hardest sentiment to detect among sentiments such as anger, disgust, fear, joy or sadness (Strapparava and Mihalcea (2010)) . We continue to believe, however, that a promising line of research in understanding the impact of institutional changes is in having some measure for market "surprise" from text, e.g. financial news. Once better solutions to this problem emerge from computational linguistics, the study of the political economy of debt markets will become even richer still.

26. We counted the number of news articles about 'term limits' for each country-event, where an article was measured as 'about term limits' if "close enough" in word space to a reference document.

Chapter 3

The Subprime Mortgage Crisis: Underwriting Standards, Loan Modifications and Securitization

Laurence Wilse-Samson

3.1 Introduction

This chapter reviews some of the literature on the subprime mortgage crisis. I focus on two aspects of the debate around securitization. First, I consider securitization as a possible mechanism for a decline in underwriting standards. Second, I review evidence about its role in inhibiting the modification of loans. These aspects are related, since creating a more rigid debt structure can facilitate better risk management and permit the greater extension of credit. However, it can also result in inefficiencies, through externalities on non-contracting parties. This might justify intervention ex post (Bolton and Rosenthal 2002). I then consider some of the government modification programs and their problems. A concluding section tentatively suggests topics for research.

First I outline the shape of the non-prime mortgage market, tracing its expansion from the mid-1990s and in particular its rapid development since the turn of the century. The first part addresses some of the still unresolved literature on the causes of the crisis in the subprime market. I restrict attention to housing market specific explanations — leaving out broader macroeconomic issues — in order to focus attention on the institutional detail of the mortgage industry.

The first section considers some contending (though not mutually exclusive) explanations for the crisis in subprime. I identify three main strands of the debate. First, the hypothesis that the products themselves were confusing — the allegation that consumers did not understand that rates were going to explode, and hence were 'duped' into loans they could not afford. Next I look at the discussion of securitization and underwriting standards in particular examining the relative importance of securitization as contributor to declining underwriting standards. The innovation of securitization is also linked to powerful institutional and technological changes, such as the automation of underwriting. This can result

in a decline in standards, though on dimensions not measured. Finally, we must recognise that underwriting standards are connected to expectations about house price changes. This is just to say that 'the social contagion in bubble thinking', as discussed in various places by Shiller (2008), may have been important. Notice, the fundamental implication of each of these explanations is the same — houses were sold to individuals who could not afford them.

The second section provides some background on structure of the mortgage finance industry. There appear to be two general and countervailing forces at work: the accounting and regulatory arbitrage incentives for disintermediation; second, the benefits from vertical integration in overcoming agency and transactional costs. The ultimate industry structure results from a firm-specific balance of these two competing forces. I present some basic information on this topic, mainly for context to the discussion around loan modifications which follows in section three. The industrial organization of finance, in particular in terms of understanding the financial crisis, appears to be an area where more research is needed.

The third section is a consideration of the various limitations on performing loan modifications. Following the outline of the Congressional Oversight Panel (2009) report, I consider four different types of constraint. The first two are directly related to securitization. First, is the idea advanced by authors such as Gelpern and Levitin (2009), that the securitization pooling and servicing agreements introduce contracting rigidities and adverse incentives for renegotiation. Linked to this, but a little more general, is the idea that securitization has resulted in various incentive problems and inappropriate fee structures for servicers. Other authors, such as Adelino, Gerardi, and Willen (2009) have argued that in fact the constraints are more prosaic — that concerns about redefault and self-cure (particularly in a setting of rapidly declining house prices) make it unprofitable for servicers to modify loans. Finally, it has been widely observed that loan servicers are capacity-constrained. This is an indirect result of securitization, in that the role of the servicers had altered from one of careful

screening and monitoring, to essentially automated collection and disbursement. The debate here around securitization and modification ties back into the debate around securitization and underwriting in the first part.

Section four turns to an assessment of the various government programs aimed at moderating the speed and extent of foreclosures. I explain some of the criticisms that have been made about these programs, and outline some alternative suggestions. One prominent argument that has been advanced is that the federal government should permit bankruptcy mortgage modifications for single-family principal residence mortgages in Chapter 13. An amendment to this effect was defeated in the House, and the Senate. This illuminates the discussion of the political economy of financial regulation, another under-researched area.

Finally, I conclude with some questions arising out of this survey and some suggestions for research which introduces the analysis of foreclosure contagion in Chapter 4.

3.1.1 Bubble

While in the past 50 years, national nominal housing price growth has never been negative, real price growth often has.[1] It is somewhat surprising therefore, in a low inflation, low interest rate environment, that market participants and regulators do not appear to have anticipated any possibility of future nominal house price declines.

Two features have been identified as underpinning the price dynamics of the US housing market — momentum and reversion (Wilcox 2008). The literature explains momentum as a result of supply frictions and/or informational dynamics. First-time buyers may be unable to purchase based on this momentum effect, which may eventually trigger a reversal (Wilcox 2008, p.8). Case and Shiller (2003) argue that once buyers perceive that prices can no

1. For example, in the 1980s. See (Shiller (2008), fig 2.2 p. 33) for a national real house price index for the US constructed for the period 1890-2008.

longer rise, this belief becomes self-fulfilling and prices may revert to 'fundamentals'. While Himmelberg, Mayer, and Sinai (2005), writing in 2005, argued that fundamentals justified then higher prices[2]; recent experience suggests this was mistaken. In fact, the real economy many have been propped up by a strong real estate market, rather than the reverse.

Between 1997 and 2007 average annual nominal house price growth was 6.5%. Assessing whether or not this formed a 'bubble' requires some notion of fundamental determinants of housing prices. Demand side drivers could include incomes, access to credit, population and preferences; on the supply side we might consider construction costs and zoning restrictions (Wilcox 2008, p.17). However, the empirical basis for such long-run relationships is thin. Alternatively, we might assess house prices by comparison to interest rates and the rental market. Prices might then be viewed as 'overvalued' if high relative to carrying costs, appropriately measured. Shiller (2008, p.34) considers ratios of home prices to building costs, rent and personal income and finds that home prices at 2004 were looking '*very* anomalous' (emphasis in the original).

Shiller (2008) (p.4) argues instead that the boom was driven by 'an epidemic of irrational public enthusiasm for housing investments', what he describes as a 'social contagion of boom thinking, mediated by the common observation of rapidly rising prices' (p.41). This feeds into a *narrative* of a 'new era'. Shiller argues that this was primarily a difference in opinion over time (a 'changing zeitgeist'), as opposed to say across regions. He cites work on median expected price increases in various regions at 2005 which among a third of respondents he found to be 'truly extravagant'.

---

2. As did Ben Bernanke in Congressional testimony in 2005. See Henderson (2005), 'these increases, [Bernanke] said, 'largely reflect strong economic fundamentals,' such as strong growth in jobs, incomes and the number of new households.'

3.1.2 Nonprime mortgages

By nonprime loans we mean those commonly referred to as 'subprime' and 'Alt-A'. The use of these terms by industry participants is not consistent. Ashcraft and Schuerman (2008) refer to the 2001 Interagency Expanded Guidance for Subprime Lending Programs to define these terms[3]. In Chomsisengphet and Pennington-Cross (2006), subprime loans are those which carry a 'premium above the prevailing prime market rate that a borrower must pay'. As noted also by Demyanyk (2008), the denotation of 'subprime', is not solely based on the poor credit characteristics of the borrower (typically a borrower having a FICO score below 620 would result in the loan being designated 'subprime'). A loan could also be classed as 'subprime' if it was originated by a high-cost lender, or if it had certain features (for example if it were a 2/28 hybrid). Furthermore, the process of securitization itself fed into this assignment; the riskiest of the securitized loans would be labelled as 'subprime'. Frame, Lenhert, and Prescott (2008) note that First American LoanPerformance data provides information on loans sold into private label MBS securitization including information on securities marketed as Alt-A, subprime and jumbo. 'Alt-A' loans generally refer to those given to individuals possessing

3. According to these guidelines (Ashcraft and Schuerman 2008),

> 'the subprime borrower [is] one who generally displays a range of credit risk characteristics, including one or more of the following:
>
> - Two or more 30-day delinquencies in the last 12 months, or one or more 60-day delinquencies in the last 24 months;
> - Judgment, foreclosure, repossession, or charge-off in the prior 24 months;
> - Bankruptcy in the last 5 years;
> - Relatively high default probability as evidenced by, for example, a credit bureau risk score (FICO) of 660 or below (depending on product/collateral), or other bureau or proprietary scores with an equivalent default probability likelihood; and/or,
> - Debt service-to-income ratio of 50 percent or greater; or,
> - otherwise limited ability to cover family living expenses after deducting total debt-service requirements from monthly income'

higher credit scores, but with incomplete or no documentation[4]. These also may be loans made where the recipient intends to buy a second home or to purchase for investment.[5]

Bhardwaj and Sengupta (2009), for example, define 'subprime' and 'Alt-A' relative to the First American LoanPerformance database they use in their analysis — 'subprime pools include loans to borrowers with incomplete or impaired credit histories while Alt-A pools include loans to borrowers who generally have high credit scores but who are unable or unwilling to document a stable income history or are buying second home or investment properties' (fn 12, p.10).

3.1.3 Growth in nonprime

In Frame, Lenhert, and Prescott (2008), the first lien subprime loan total at March 2008, was estimated to be 6.7m loans, a total value of $1.2tn. This is in the context of a $10.1tn first lien mortgage loan market. According to Gramlich (2007), subprime mortgage originations totalled $625bn in 2005. The industry publication, *Inside B&C lending* reported subprime origination as growing from $65bn in 1995, to a total of $332bn in 2003 but declining in share of all loans outstanding from 10.2% to 8.8% over the same period.[6] During this time, in line with conventional mortgages, an increasing proportion of subprime mortgages were

4. These 'stated income' loans are also sometimes called 'liar's loans'. An amusing, but perhaps slightly apocryphal story is that WaMu made a second-mortgage loan to O.J. Simpson after a civil court judgment found against him on Simpson's assurance in a letter to the lender that 'the judgment is no good, because I didn't do it' (DeSilver (2009)). Florida state law providing for unlimited homestead exemption is likely to have been more of a factor. *Credit Slips*, 2 November 2009,

5. When using First American LoanPerformance data, to get a sense of 'Alt-A', one usually considers the adjustable-rate prime category. For more details on the available data sources on loan performance, see COP (2009)

6. Chomsisengphet and Pennington-Cross (2006)

securitized (reaching 58.7% in 2003, up from 28.4% in 1995). [7]

The reasons Gramlich (2007) identifies for the rapid increase in subprime lending after 1993 (a time at which essentially no loans were 'subprime') include: the 1980 Depository Institutions Deregulatory and Monetary Control Act of 1980, securitization and automatic underwriting[8], and the Community Reinvestment Act (CRA). To this Chomsisengphet and Pennington-Cross (2006) add the 1982 Alternative Mortgage Parity Act, which allowed balloon payments and interest rate flexibility, and the 1986 Tax Reform Act, prohibiting the tax deductibility of interest on consumer loans.

Gramlich (2007) breaks out shares of origination. While 20% of subprime loans were originated from federally supervised banks and thrifts, and 30% from affiliates of bank holding companies, fully one half of subprime loans originated from unregulated lenders, many of whom had 'no skin in the game' (Gramlich 2007). These were mainly state-chartered banks.

According to Chomsisengphet and Pennington-Cross (2006) (p.31), key dimensions associated with the cost of credit are the down payment, and the borrower's credit history. Prevailing practice before the explosion of subprime was 'nonprice credit rationing' — 'minimum lending standards were based on a borrower's income, prepayment history, down payment and the local underwriter's knowledge of the borrower' (p. 32). In contrast, the growth in the subprime market introduced differential tiers and product types, '[moving] the mortgage market closer to price rationing or risk-based pricing'. They examined Countrywide Home

7. One important feature of this period was the brief collapse in subprime loan securitization in 1998-99, which also coincided with a drop in originations, before the market underwent a period of consolidation and began to recover. Chomsisengphet and Pennington-Cross (2006) (p.40), ascribe this to an under-pricing of these products in the mid-to late 1990s and reduced levels of liquidity in all markets following the East Asian crisis.

8. On automatic underwriting see Browning (2007). On this account, it was this technological development which permitted the variety in subprime mortgages to develop — 'spawned an array of subprime mortgages'. It also reportedly made loans less costly to close (by $916). We consider automatic underwriting in more detail the below, drawing in particular on the work of Poon (2008), although this topic is also considered by Bubb and Kaufman (2009).

loans underwriting matrices and found these to have five categories under which a loan is grouped into one of six loan grades, namely: mortgage delinquency days, foreclosures, chapter 7 or 13 bankruptcy, and debt ratio. This is what these authors characterise as '[active] price discrimination' and 'risk-based pricing'.

They thus identify two periods in the evolution of the subprime market between the mid 1990s and 2004: the first period, until 1998-99 when growth was primarily in the riskiest grades of subprime, and the period from 2000-2004 where subprime volume was increasing, but mainly in the A- grade. Kregel (2008) (p.14) though, citing data from Freddie Mac, finds that the period 2001-2007 was characterised by a rising share in ARM loans as well as loans with low- or no-documentation. In fact, many borrowers were struggling to meet repayments, even before the initial resets.

3.1.4 Crisis

Increasing mortgage rates and slowing house prices from mid 2005 led to a reversal in the growth of subprime and Alt-A originations.[9] The Federal Reserve had begun its tightening cycle in 2004 which brought with it interest rate increases on new mortgage loans and loans with LIBOR-indexed floating rates. Notice though, that since between 2004 and 2006 housing prices were still appreciating, subprime borrowers could still refinance into lower monthly payments (Mayer, Pence, and Sherlund 2009). Once prices stopped rising, defaults began, initially on a regional basis, in places such as Ohio, Michigan and Indiana where the macroeconomic environment was weaker (Mayer, Pence, and Sherlund 2009; for an excellent contemporaneous account of the situation in Ohio, see Katz (2006)).

Subprime and Alt-A mortgages had experienced rapid growth in the first half of the

9. The slowdown was picked up in some parts of the press, e.g. Krugman (2005).

decade (Mayer, Pence, and Sherlund (2009)). The character of this growth was different across the subprime and Alt-A categories — with subprime growth predominantly in the form of short-term hybrid loans (with a yearly share of originations between 68% and 81%) and Alt-A growth more evenly distributed across fixed- and floating-rate and short- and long-term hybrid loans (Mayer, Pence, and Sherlund (2009)). Coupled with the reversal in originations was an explosion in delinquencies. By the second quarter of 2008, seriously delinquent loans — namely those either in foreclosure or on which the borrower is more than 90 days in arrears — were up to 4.5%, from an historical average of 1.7% between 1979 and 2006. Defaults and delinquencies were initially concentrated in the nonprime segment, although as the crisis, recession and unemployment worsened, increasing numbers of prime loans also became 'seriously delinquent'. The factors identified by Bernanke (2008) for an increase in foreclosures include weak underwriting (little documentation, low downpayments), increased inability to refinance, and tighter lending standards as secondary markets softened.

We consider three possible types of explanation for the foreclosure spike commonly called the 'subprime crisis', namely:

- consumers misunderstanding product features such as 'teaser rates' and prepayment penalties, leading to a spike in foreclosures when these ARMs reset;

- a declining standard of underwriting over this period — underwriting standard declines may manifest in a variety of ways: through lower downpayment requirements; inflated appraisals on properties in refinancing transactions; less rigorous documentation requirements (e.g. allowing consumers to state their own income); or through altering credit score eligibility thresholds; and

- a general malaise of bubble psychology related to 'animal spirits' which lead to anticipations of continued appreciation in house prices. Notice, this is related to the second

explanation above since underwriting standards may be allowed to fall because of this 'national mood', and increased access to credit for the purchase of assets may then result in self-confirming asset price increases.

3.2 Causes of the Crisis

3.2.1 Product Complexity, Mortgage Resets, Prepayment Penalties

Smith, Perwien, and Ratcliffe (2009) present survey evidence that borrowers most often cite rate resets as the primary mortgage characteristic leading them to default. For example they quote one foreclosure counsellor as stating simply 'people [...] got hooked into loans they didn't understand'. By contrast, in that same survey, the most commonly cited reason for default for individuals with fixed rate mortgages was loss of income or employment (p.8).

Bucks and Pence (2008) compare consumer reports of loan terms in the Survey of Consumer Finances with how these terms are distributed in the administrative records of the Residential Finance Survey and the LoanPerformance data. They also consider direct reports in the survey of a lack of awareness of mortgage terms. They find among ARM borrowers no knowledge of, or an underestimation of, the magnitude by which their interest rates could change. Lender reported potential ARM interest rate increases were double or triple those reported by consumers while a third were unaware of the extent of their mortgage interest rate caps. They argue that this is best accounted for by a model of rational inattention.[10]

On this account, features which may have contributed to the foreclosure spike include: teaser rates, prepayment penalty clauses, and too little principal down (or even negatively

10. Ben Bernanke revealed that he had to refinance his mortgage because 'we had an adjustable rate mortgage and it exploded, so we had to'. *Time Magazine*, 'Person of the Year 2009',

amortizing loans). These were typical features of non-prime loans whose prevalence expanded over this period. However, Mayer, Pence, and Sherlund (2009) argue that these features do not explain the massive increase in delinquencies. Teaser rates were still quite high; and the spike in foreclosures does not neatly coincide with the reset event. Smith, Perwien, and Ratcliffe (2009) from the same interviews cited, find that households might come before or after the rate reset for counselling on foreclosure mitigation.

Mayer, Pence, and Sherlund (2009) also address a few other issues thought to be relevant to the subprime crisis. They describe as 'relatively uncommon' the situation that prepayment penalties would still be applicable at the time of reset. They also note that by 2006 and 2007 an increasing proportion of subprime and Alt-A loans had payment schedules beyond 30 years and/or interest only or negatively amortizing payment profiles. The implied low equity position meant that default incentives under falling house prices were raised. Households anticipating the recast of Option-ARM payments may choose simply to walk away. However, in the view of these authors, the particular characteristics of these mortgages were less important than factors such as the high loan-to-value ratio of the loan, and the corresponding very thin layer of equity for the borrower.

3.2.2 Declining Underwriting Standards

Dimensions of underwriting standards

The two prime delinquency risk measures that Mayer, Pence, and Sherlund (2009) identify are the 'combined loan-to-value ratio [combining both the primary and secondary lien] and the FICO credit score'. Furthermore, they find that the mere presence of a secondary lien increases delinquency probability, even conditioning on combined LTV.[11]

11. This may relate to common agency problems such as is identified by (Segal (1999)). Mayer, Pence, and Sherlund (2009) notes anecdotal evidence that loans 'in [which] the borrower takes out a second lien without

Using data on subprime mortgages originated in the US between 2003 and 2007 Mayer, Pence, and Sherlund (2009) determine that while median FICO scores were constant over the relevant time period, that underwriting standards had deteriorated in the following senses: there was more origination to no/low documentation borrowers and borrowers with low downpayments (higher loan-to-value ratios). They also find that during this period there was an increasing share of subprime and Alt-A originated loans with second liens attached. Combined loan-to-value loans on subprime mortgages for housing purchases were rising between 2003 and 2007. While refinancing loan-to-value ratios were constant, this latter measure is biased downwards if housing appraisals were biased up over this period (in a refinancing, the house price is based on an appraisal and not an actual sale). They document also an increase in the share of originations for low documentation subprime and, particularly Alt-A, loans and conclude from the fact that default rates are greater for this type of loan that this likely contributed to the increase in foreclosure rates.

Frame, Lenhert, and Prescott (2008) document declining underwriting standards over this period in the following senses: increased subprime mortgage combined loan-to-value ratios (particularly for 2/28s); declining share of fully documented 2/28 subprime mortgages and falling average FICO scores for fixed rate subprime mortgages.

Furthermore, some of the decline in underwriting standards is likely to have been in terms of factors which aren't easily measurable. An important debate has been around the (lack of) incentives for originators and lenders to properly screen borrowers due to the fragmented structure of mortgage bonding under securitization and the fact that originators and lenders no longer needed to necessarily hold these loans on their balance sheets for very long.

---

notifying the original lender [...] may have become more prevalent over this period'.

The securitization screening incentive (originator moral hazard) debate

An important debate centred on the issue of securitization and the role it played (if any) in the decline of underwriting standards. On the accounts of Dell'Ariccia, Igan, and Laeven (2008), Keys et al. (2010), and Mian and Sufi (1999), securitization introduces an important friction in agency between the investors and the servicers of loans.

Keys et al. (2010) use a regression discontinuity design, exploiting the fact that a FICO score just greater than 620 results in a higher probability of being 'treated' (in this case, this means the loan is securitized), than a FICO score of the borrower being just below 620. They find that loans made to borrowers with credit scores just above 620 perform worse than loans made to individuals with FICO scores just below 620, which they argue provides evidence that securitization reduces incentives for originators to undertake proper screening. A problem with the identification strategy, argue Bubb and Kaufman (2009), is that while there appears to be a jump in mortgages at a score of 620, there do not find a corresponding jump in the securitization rate. Hence, while there is a discontinuity in origination, there is no corresponding discontinuity in securitization.

Bubb and Kaufman (2009) model credit score cut-offs as a response by lenders to the fixed cost of screening rather than being chosen exogenously by investors (and hence they are inappropriate as an instrument).[12] In the symmetric information case, incentives to screen are retained through threats of later punishment (Bubb and Kaufman (2009), p.4). Where information is asymmetric, rational securitizers purchase fewer loans below the cut-off to force lenders to undertake screening (since they hold more loans).[13]

12. They argue that lenders were following guidance set out by Freddie Mac and Fannie Mae, who were 'essentially providing a public good by analyzing their data on the relationship between FICO scores and mortgage performance to determine the optimal cutoff rule' (p.14).

13. Bubb and Kaufman do, however, find a discontinuity in the securitization rate for the 'jumbo' sample in which the vast majority of loans is done by private securitizers (p.23). They interpret this as a rational

An important point Bubb and Kaufman need to deal with is the nature of the indivisible cost. They pinpoint this as the decision about whether or not to rely on automated underwriting systems (AUSs), or to perform manual underwriting (p. 15). Automatic underwriting began in the mid 1990s. A key feature of the causes of the crisis may well include things such as technologies like automatic underwriting, with soft knowledge about the borrower being ceded to homogenization. This gives us a reinterpretation of the Keys et al (2008) results which found that loans with FICO scores just above 620 performed better than otherwise identical loans with FICO scores just below 620. On this reinterpretation it is automatic underwriting and the lender cut-off rule which results in less screening being performed on the loan applicant.[14] Thus an important feature of securitization is that it lowers the cost of entry into the underwriting (mortgage origination) market; as Bubb and Kaufman state 'instead of 15 minutes, manual underwriting may occupy days of loan officer's time'. We consider this topic in more detail in the next section. A paper making similar points is Bhardwaj and Sengupta (2009).

Demyanyk and Hemert (2008) find that quality degradation of securitized subprime loans did not experience a structural break between 2001 and 2007; they had instead declined throughout that period, but that this was masked by the housing bubble. As noted in Demyanyk (2008, p. 12) 'default rates have risen for all categories of FICO scores [. . .]'. Her

response by securitizers to induce lenders to hold more loans on their own books, so as to do more screening (although it is unclear how much screening lenders such as New Century Financial did at all). For conforming loans, the GSEs employed various punishment mechanisms not available to private securitizers (hence the jump in securitization probability around the cutoff for jumbo). However, the authors do not separately break out the default probability around the 620 cutoff for jumbo loans — in any event the crucial aspect is whether you think the 620 rule is an exogenous (securitizer) rule, or an endogenous response to the agency problem between servicers/investors. Furthermore, the analysis does not address the bigger question of the inefficiencies resulting from securitization — or even the question of whether it lowered underwriting standards through other mechanisms.

14. See Browning (2007) 'speed became something of an arms race, as software makers and subprime lenders boasted of how fast they could process and generate a loan'.

research indicates that credit score is a poor predictor of default likelihood — for each of 5 different FICO score baskets, in the period 2005-2007, mortgages originated later had higher rates of serious delinquency than those originated earlier.

Elul (2009) examines prime loans over the period 2003-2007 and finds a significantly higher delinquency rate for securitized loans of various classes in comparison to non-securitized loans of the same type, for example, 'for loans originated in 2006, the two-year default rate is at least 15 percent higher, on average' for private securitized prime mortgages than loans held in portfolio. Again, Elul finds no differentially higher rate for subprime securitized loans. Elul ascribes this to either: closer inspection by investors of subprime loans, or, simply the fact that so few subprime loans were held in portfolio at all means that there should be little fear from investors that lenders were engaging in 'cream-skimming'. Elul also argues that the incentives for originators to maintain a good reputation worsened as the outlook for the housing market worsened. The level of subprime securitization was 90% in this period. And, as noted above, on important dimensions, underwriting standards on nonprime loans were falling. This suggests at least a strong correlation between securitization and underwriting.

Thus, fundamentally some writers debate the role of securitization *per se* and argue instead that the reason for lack of proper screening was not so much that the risk could rapidly be offloaded to investors, but rather that it was expected that house prices would continue rising. Notice also, that consistent with this view of the world, most subprime originators are now bankrupt or have been sold to another financial institution.

Automatic underwriting and the 'sociology of knowledge'

Poon (2008) has detailed how the adoption by the GSEs in the 1990s of the consumer risk score $FICO^R$ became 'hardwired' into a 'distributed and collective 'market device", accompanied by the growth into mortgage finance of high-yield seeking investment capital.

On Poon's account, such 'technical apparatuses' are more important for explanation than stories about collective irrationality[15].

Freddie Mac adopted the $FICO^R$ consumer risk assessment tool in 1995 with the aim of standardising prime mortgage underwriting. This development, as well as the subsequent implementation of this methodology by ratings agencies had the effect of stifling 'calculative diversity' and provided the impetus for a shift from 'credit control-by-screening [...] towards credit control-by-risk'. On Poon's account, $FICO^R$ scores are 'manufactured economic information', a market device that co-ordinates lender decision-making. The result, she argues, for consumer credit is a 'risk segmented and saturated U.S. market' (p. 13), and for mortgage finance, a 'bipartite organization [...] into the conventional prime and high-risk subprime'.

It was Freddie Mac initially that shifted to statistical underwriting, using systems that incorporated consumer credit data in the form of $FICO^R$ scores with the aim of preventing broker manipulation of loan eligibility (earlier attempts to develop a Residential Mortgage Credit Report were found to be open to manipulation). A 1995 Freddie letter stipulated that 'a $FICO^R$ score of 660 was the eyeball threshold for their definition of loans eligible for the prime investments') (Poon 2008, p.21). Following this was the diffusion of underwriting software incorporating these standards. Through this means the '$FICO^R$ 660 rapidly became a free standing benchmark of prime investment grade status recognizable among underwriters, securitizing bodies, investors, regulators [...]'

Importantly, this was accompanied by the incorporation of $FICO^R$ scores by the ratings agencies in the development of automatic statistical ratings for securitizations. This way private label securitizations became linked to the GSE segment — through the $FICO^R$ score

15. President Bush reportedly described the subprime crisis in the following terms, 'There's no question about it. Wall Street got drunk [...] and now it's got a hangover. The question is, how long will it sober up and not try to do all these fancy financial instruments?' Reuters, 23 July 2008

as a means to measure consumer credit risk. Poon goes on to argue that the 'sliding scale [...] of risk as measured in the credit score allowed / spurred the proliferation of financial goods' through a shift from screening to risk management. In other words, the credit score permitted the standardization of products. Hence, the growth of private label subprime alongside GSE-guaranteed and owned mortgages. This puts the GSEs in a paradoxical position — the institutions created to provide liquidity for affordable housing were providing liquidity in the 'good risk' segment of the mortgage market.

In summary, Poon (2008) argues that the adoption of GSEs of consumer credit scoring systems in its underwriting guidelines (shifting away from a rules-based underwriting system), and its incorporation in the systems of ratings agencies, led to the development of a risk-based pricing market of subprime finance by private label automated underwriters alongside the GSE-based prime finance market.[16]

MacKenzie (2009) looks more broadly at the sociology of ABS, CDOs and ABS CDOs. He points out that trading requires a commonality of knowledge — in the sense that wide variance in valuation (or 'wild discrepancy') is likely to be unsettling.[17]

---

16. Writers in the financial press have noted that these underwriting systems may also have been poorly adapted for no or low documentation loans (particularly 'borrower-directed' loans), and jumbo loans. See Dungey (2007) 'FICOs and AUS: We Will Add Your Distinctiveness to Our Collective', Calculated Risk, 19 March 2007 at http://www.calculatedriskblog.com/2007/03/ficos-and-aus-we-will-add-your.html Dungey, a former mortgage banker, has an analysis similar to that of Poon, when she wrote, 'a large distortion may have entered the market during the boom because FICO (a kind of derivative or simplification of a complex credit analysis) drove a lot of pricing decisions [...] it made people willing to price [Alt-A] at tiny risk premiums over prime [...] maybe we should give this tech fetish another thought?'

17. Sorkin (2009) recounts that in November 2007 AIG reported its dispute with Goldman Sachs over the value of collateral under various swaps contracts. This disagreement was a broader manifestation of the resensitization of informationally-insensitive debt, the mechanism of the liquidity breakdown, and marked the beginning of the run on wholesale finance (one could also characterize this as a loss in trust). As recounted in Gorton (2009), money market funds 'depositing' at the broker-dealers required increasing haircuts on repo agreements resulting in 'massive deleveraging'. Sorkin (2009, p. 159) provides a relevant anecdote: 'a longtime insurance analyst for Credit Suisse asked pointedly what it meant that 'your assessment of certain super-senior credit default swaps and related collateral ... differs significantly from your counterparties". Later, and still in the context of collateral valuation, Sorkin quotes Joseph Cassano, head of AIG's Financial Products Group as saying 'Just because Goldman says this is the right valuation you shouldn't assume it's

MacKenzie argues that there exist 'evaluation cultures' — by which he means 'pockets of local consensus on how financial instruments should be valued', comprised of ideas, but also 'artefacts and technical systems.' Following Callan, economic models can be performative since shaping market processes, or counterperformative, by which MacKenzie means that using the model makes its predicted outcome less likely (p. 5). The underlying logic is clear: the employment of these models, Mackenzie argues (for example, by the ratings agencies) may result in some change in the *ceteris paribus* conditions underlying the data generating process, hence causing parameter instability. In this way, the very use of a historical model by agents makes it less likely to be true.[18]

MacKenzie sketches a disjunction between the ABS and CDO evaluation cultures. ABS as originally designed had AAA contingencies modelled based on simulations where the stresses considered were the conditions prevailing during the Great Depression (MacKenzie, 2009, p. 22). CDOs arose out of lumpier pools of corporate loans or bonds and were initially employed in the junk bond boom, but later spread more widely following the innovative example of the JPMorgan Bistro deal.[19] Importantly, added to this mix was the credit default swap, which allowed the 'synthetic' transfer of risk — one could build ABS CDO without having to go out and assemble the underlying ABS pool.

The evaluation cultures of ABS and CDOs diverged in focus (prepayment risk versus the correlation of credit risk), a natural divergence given their respective origins — ABS

---

correct [...] My brother works at Goldman, and he's an idiot'.

18. While this is an interesting and plausible idea, more work needs to be done to determine whether anything general can be said about when using a model is likely to make it less accurate. One important factor might be how widespread its use becomes. This is more or less another restatement of the Lucas critique.

19. The most comprehensive account is Tett (2009). Interestingly, she recounts that J.P. Morgan, who had pioneered some of these structures, were themselves reluctant to do CDS deals with mortgage debt, since 'mortgage risk was just too uncharted' — data on defaults were too thin, and correlation structures as a result were not well understood (see pp 76-80)

for residential mortgages, CDOs for lumpier corporate loans and bonds. As also detailed in Ashcraft and Schuerman (2008), differences between ABS as opposed to corporate bond modelling include: more important role for systematic risks over firm-specific; greater reliance on quantitative models over judgement; and an explicit role for forecasting macroeconomic conditions etc.

CDOs of ABS increased greatly from 2001 (MacKenzie, 2009, p. 39), but tradable ABS indices for residential mortgages (the ABX indices) only started in 2006. In the interim, their evaluation was based on the distinct cultures around ABS and CDOs employed by banks' structuring specialists and the ratings agencies, respectively. Correlation was particularly difficult to get a handle on — there was no instrument like share price; defaults were rare (so historical data limited); and the traded ABX index (TABX), was introduced only in February 2007, so correlations could not be inferred from observed prices.

The correlations employed by the ratings agencies and structurers were based either on expert judgment or histories of ratings transitions and were, as it turns out, too modest. Large portions of the ABS CDO were comprised of super senior AAA debt — often held on the books of the issuer, but hedged using monoline or AIG CDS for very low premiums (as little as 12bp, MacKenzie, 2009, p. 48). This gave rise to the ability to book negative basis trade profits by buying AAA rated super-senior debt and 'fully hedging' it with monoline / AIG Financial Products insurance. Banks, therefore, had no need to model correlation — since they were 'hedged' — this was left then to 'ratings agencies, AIG and the monolines' (MacKenzie, 2009, p. 49).

From 2004 successive vintages of ABS CDO performed progressively worse, increasing from 30% of events of default for 2005 to 80% for the 2007 vintage. MacKenzie argues that the key role of the ratings process was not ratings shopping *per se* because this cannot explain why default rates were two orders of magnitude higher than those models (p.

53). Instead, he argues that the ratings process was 'counterperformative' in two senses: First, the increased 'popularity of ABS CDOs caused a structural change in the market for underlying [MBS]': whereas earlier mezzanine tranche insurers had been ABS specialists, new CDO arrangers were 'indiscriminate buyers'. Previously, these buyers had placed a ceiling on acceptable credit risk (and calculated this riskiness themselves) but by 2004, all that mattered was how they would be *rated*. The net effect was a reduced constraint on subprime originators (MacKenzie, 2009, p. 59), causing an amplification of the mortgage origination agency problem. Second, 'the modelling of mortgages [...] changed mortgages again in a way that rendered the models much less accurate'. By this MacKenzie means that the increased reliance on 'hard' criteria such as $FICO^R$ scores and LTVs, resulted in the decreasing accuracy of these variables as predictors (Rajan, Seru, and Vig (2010)). We can think of this as a multitask agency problem, where we would like the firm to screen agents on soft and hard features but can only measure performance on one of these dimensions. Increasing house prices result in a diverging alignment between these two tasks.

The final steps by which the subprime crisis was magnified into a generalised financial crisis were the decisions by many banks to retain, or in some cases purchase, supersenior AAA tranches for their own balance sheets; as well as to move into subprime origination through vertical integration in order to overcome the transactions costs associated with warehousing ABS for the construction of ABS CDOs (MacKenzie, p. 62).[20] Indeed these moves were in many cases completed at precisely the time when subprime lenders were weak on account of the softening of the housing market as I show in the next section.

Then, once the cost of buying ABX protection rose (and the index fell), banks holding ABS (in the warehouse) or ABS CDO supersenior on the balance sheet had large losses which they had to mark-to-market undermining credibility in the system as a whole.

20. See the next part for a discussion of vertical relationships.

3.2.3 On Bubble Psychology

The decline in underwriting standards must be understood in light of the speculation in the housing market. In many cases, the willingness to lend and to borrow was based on expectations of further price increases. Demyanyk and Hemert (2008) find that for each of five categories of credit score, mortgages originated later (2007 vs. 2006 vs. 2005) had higher rates of serious delinquency than mortgages originated earlier. Garriga (2009), studying HMDA data, finds that loan denial rates were increasing dramatically even before 2007 —- they were up 33% by 2004 relative to 2002, and by 75% by 2007 relative to 2002. Most of these denials were for refinancings, which accounted for most new loans by 2003. This is suggestive of speculative pressure from 'below'.

Bhardwaj and Sengupta (2008), examining loan-level data of securitized subprime originations, find that subprime mortgages required housing price appreciation to be viable. Mortgages on earlier vintages were prepayed, but this became impossible for later originations. The evidence they present is fourfold. First they show that 70% of the subprime originations they consider were refinances. Second, a majority were of the hybrid-ARM form. Next, 'teaser' rates were not teasers per se; instead, they were at a similar level to fixed rate subprime mortgages (FRMs). Finally, prepayment penalties were typically applied with the term of application at least as long as the time to reset.

The authors also note two further important features of subprime mortgages — namely that resets always were step-ups and never step-downs and furthermore, that subprime mortgages were at much lower levels than other subprime loans (such as auto), since backed by appreciating collateral. Bhardwaj and Sengupta cite Gorton (2009)'s view of these mortgages as a form of bridge-financing, and argue that this was sustained by prepayments. By 2007, 64% of total 2003-vintage hybrid ARMs had been prepayed (p.3), with a similar number for FRMs. Prepayments drop for later vintages, as the housing market tightened.

Lenders, concerned by the high risk of these borrowers had imposed conditions that would require these borrowers to refinance, but protected themselves with prepayment penalties until reset. On this account, refinances were more likely for cash-constrained consumers, hit by the shock of a lost job or medical necessity (p.7).[21] These loans were ideal for liquidity constrained borrowers with a lack of other sources of credit.

There is a feedback mechanism between declining underwriting standards and expectations of house price increase. Improved credit scores fed into more borrowing for investment into housing, an increased value of collateral and hence higher credit worthiness. And institutional features like 'stated income' fed into this.

Underwriting 'standards' should be thought of not just in terms of measurable factors such as loan-to-value ratios, credit scores, and the presence or not of a second lien, but also as dependent on the anticipated future path of house prices. To make loans which one did not think the borrower could repay (excepting some optimistic expectation of house price appreciation) is to loosen underwriting standards in the common sense of the phrase. A rhetorical confusion can arise depending on whether we are thinking of underwriting standards de jure or in fact. The credit worthiness of refinancing mortgage buyers may have improved as a result of rising house prices (at least before 2005) and so underwriting standards were not 'looser' in this sense, but lenders may have 'known' that this was driven by perhaps unsustainable price appreciation.

On the other hand, writers may deny that participants 'knew' that prices were unsustainable as just described, and it may be that as articulated by Shiller, that the crucial element was the 'social contagion of boom thinking'. Either way, once the contagion had dissipated, and prices were overtaken by 'fundamentals', defaults started to rise. The Bhardwaj and

21. Cash-out refinancings are used in this setting for consumption-smoothing. A different, though not entirely distinct motive to refinance would be to take advantage of lower interest rates ('rate-refinancing'). Bhardwaj and Sengupta observe that proportionately more subprime than prime mortgages involve cash-out.

Sengupta (2008) study concludes that it was not the resets themselves that caused the jump in default, but rather the lack of ability to refinance as the housing market weakened. In other words, nonprime mortgages were never really 'long-term mortgages' in anything but name. They were short-term loans made to liquidity constrained, poor credit borrowers and speculators, who were unable to refinance when prices fell (Tung 2009). Other evidence is provided by Frame, Lenhert, and Prescott (2008) who study LoanPerformance data to find that for subprime mortgages originated between 2001-2004, most were terminated (on account of prepayment or default) within 36 months.

We are left then with a question about the causes of the decline in underwriting standards, as well as the contemporaneous ramp-up in housing prices. Other candidate explanations may include: deregulation, the low interest rate environment, demand for loans for MBS driven by excess liquidity linked to a global savings glut, and affordable housing goals.

Wherever one comes down on these issues, and their relative importance is fundamentally an empirical question, the ultimate unfolding of the crisis clearly points to the important role of leverage, both of the lenders, and of the borrowers (who had placed ever-lower capital down against their loans). Mian and Sufi (2009), studying a dataset of some 70,000 homeowner credit files in the period 1997- 2008, find a doubling of debt-to-income ratios of US households between 2002 and 2007 to a 25-year record level. On their estimates, 1/3rd of 2006-2008 new defaults are explained by home equity borrowing on the back of rising house prices. In particular, default rates were most up in areas where house prices had experienced steepest appreciation, and where credit scores were lowest.

3.3 The Mortgage Finance Industry

As set out in Reiss (2009), innovations in technology, legal changes, and financial innovation have dramatically changed the way mortgages are originated, financed and administered. Reiss describes this as a 'fracturing' — and describes the end product in the following way, 'it is common for a given mortgage to be originated by a mortgage broker contacted by telephone; serviced by a mortgage banker; insured by a mortgage insurance company; legally owned by a trust; and beneficially owned by an institutional investor'.

Ashcraft and Schuerman (2008) draw on the example of a 2006 vintage New Century Financial securitized pool of mortgages to sketch seven informational frictions arising in the subprime mortgage securitization process: mortgagor/originator frictions (predatory lending); originator/arranger (predatory borrowing and lending — arising out of the informational advantage of the originator over the arranger); arranger/third party (potentially adverse selection since the arranger knows better the underlying quality of the originated loans); servicer/mortgager (moral hazard — during delinquency the mortgagor has little incentive to maintain the value of the home, this creates incentives to foreclose quickly); servicer/third parties (moral hazard — incentives for the servicer to inflate expenses in delinquency where it gets paid off the top); asset manager/investor (standard principal/agent type); and investor/credit rating agency (model error). They further recognise that these frictions are likely to be magnified in a crisis. Since we are primarily interested (in Section 4 below) in loan modifications, the 4th and 5th frictions are what are primarily at issue here.

I sketch the roles of the various players in a little more detail below, drawing heavily on Dungey (2007). I then consider the nature of the vertical relationships between these players. These vertical relationships, and the embedded informational and agency constraints, are central to understanding how the subprime crisis originated and magnified, as well as why

now resolution and intervention are so difficult.

3.3.1 Players

Mortgage originators

Kregel (2008) identifies, mortgage originators as either: Financial holding companies, specialised mortgage banks, or independent financial companies. As noted in Aschraft and Schuerman, it is the role of the originator to finance the initial home purchase, and perform the original underwriting. For this the originator receives borrower fees, and the margin on the onward sale of the subprime loans. Important subprime lenders included: Ameriquest Mortgage; New Century; CitiFinancial; Household Finance; Option One Mortgage; First Franklin Financial Corp; Washington Mutual; Countrywide Financial; Wells Fargo Home Mortgage; and GMAC-RFC.

Servicers

Servicers service loans either for themselves, or for investors. The accounting for both of these activities is the same, although the incentives obviously differ. In addition to the basic fee [22], the GSEs incentivize servicers with a bonus payment on performance and for loss mitigation; while private labels generally have servicers hold the equity tranche of the deal. Servicer fees are senior to investor income, but servicers also bear the first expenses in case of delinquency. Once delinquency is dealt with, either through refinancing or foreclosure and sale, again the servicer is paid first, with the investor receiving the remainder. As noted, scheduled interest income in general must be passed on to the investor by the servicer, even if

22. Smith et al (2009) describe the mortgage fee arrangements as '.25% to .50% of each loan's balance per year'.

the borrower is actually in default (but only up to 90 days delinquent). Only actual principal needs to be passed through to the investor.

Servicers administer the loan under pooling and servicing agreements which, for example might limit the number of modifications the servicer can perform (Adelino et al, 2009, p. 3). Ashcraft and Schuerman (2008) — drawing on Dungey (2007) — argue that the core tensions between servicers and investors relate to '(a) reasonable and reimbursable expenses and (b) the decision to modify and foreclose' (p.8). As noted, under delinquency, the servicer advances interest and principal to investors, and is also responsible for property taxes and insurance. Under foreclosure, the servicer is required to cover all expenses until liquidation — but then is compensated 'off the top'. This creates an incentive for servicers to inflate costs between foreclosure and liquidation (as well as fees under delinquency).

The relationship between the servicer and mortgagor is one characterized by moral hazard (Ashcraft and Schuerman 2008). The mortgagor 'has unobservable costly effort that affects the distribution over cash flows shared with [...] the servicer'. When the mortgagor has limited downside liability when in a delinquent state he might not make an effort to maintain the property where he intends (or is likely to) default anyway (p.7). To mitigate this, under delinquency the servicer is required to advance the payments of property taxes and insurance. This confluence of factors can encourage immediate foreclosure by the servicer (to the extent that the incentives of the servicer are aligned with the investor).

Mortgage insurers

Private mortgage insurers insure the lender, not borrower against default — they thus provide a service similar to that of the FHA (the latter has restrictions on maximum loan amounts). They bear the 'first loss' from default (after the borrower, who is the equity holder), typically providing 30% coverage on a 30-year loan with a 95% LTV (Dungey 2007). It should be

noted that private mortgage insurance can be cancelled when there is sufficient equity in the home. Insurer costs are increasing in foreclosure delay. Insurance premiums are funded in one of three ways — on a flow (lender or borrower paid) or bulk (lender paid) basis. Typically in securitizations, the insurance is 'bondholder paid' as a form of credit enhancement, on a 'pool' basis.

Issuers

The arranger / issuer monitors the originator as well as creating the trust and underwrites and structures securities the trust will issue to investors (it co-ordinates this activity with the rating agency to insure timely sequencing). Investors pay issuers fees for this service, and issuers make some margin over the cost of acquiring the underlying assets. Notice that there is a significant informational gap between the issuer and the borrower / originator who may collude to engage in predatory borrowing / lending (Ashcraft and Schuerman 2008). The issuer then sells the loans to a special purpose vehicle — a bankruptcy remote trust (remote from the arranger). These trusts were often implicitly guaranteed by the issuer — once conditions started to deteriorate they were brought back onto the balance sheet.

3.3.2 Vertical Relationships, Integration and Disintermediation

One of the crucial factors that needs to be understood in terms of this crisis, is the disintermediation (through securitization), and reintermediation (through vertical integration) of mortgage finance. In order to better understand the crisis — its origins, as well as the current difficulties around loan modification and the possibility that there are currently an excessive number of foreclosures, one needs to distinguish the vertical integration incentives from the forces pushing towards disintermediation. Williamson (1975, 1985) argued that

integration facilitates adaptation.[23]

Other discussions of the performance implications of vertical integration decisions are found in Grossman and Hart (1986) and Hart and Moore (1990). A situation of ex-post hold-up may arise, since ultimately, the investor needs the servicer healthy, and so may adjust covenants (requirements in the debt that must be maintained, e.g. maximum debt levels, or the interest rates can reset higher) to help the servicer. The servicer also thus ex ante may invest too little in developing appropriate loan modification ability.

To estimate the performance benefits of vertical integration, we might look at default rates among loans serviced by servicers for their own portfolios, as opposed to those serviced for private label securitizations. Levitin (2009d) has argued that redefault rates are significantly worse for the latter.

Since firms vary as to whether they are vertically integrated or not, there must exist some cost to integration (such as, for example managerial capabilities). Empirical measurement is difficult since the firm boundary decision is endogenous). As noted in Ashcraft and Schuerman (2008) (Tables 2-4, 2008, p. 4), Countrywide for example was both the third biggest originator of subprime debt as well as the biggest subprime MBS issuer, and subprime mortgage servicer).

Against the vertical integration incentive is the fragmentation-linked securitization imperative. Securitization has been noted to facilitate 'regulatory arbitrage', and there may be further tax-based, accounting, and bankruptcy-remoteness advantages.

The winter/spring 2007 issue of industry publication, the *American Securitization Journal*, contained an article analyzing the decision by investment banks whether to integrate backwards into origination and/or servicing (Currie 2007). One reason given for why an

23. Forbes and Lederman (2009) have, for example, found evidence in the setting of the airline industry that a vertically integrated airline/regional carrier can have better performance through being able to overcome transactions costs arising from non-contractible services.

investment bank might want to integrate was the elimination of double marginalization ('taking out as many of the middlemen as possible [...or] capturing the spread from the creation value of the loan to the full offer price of the loan'). For example it was the strategy employed by Lehman Brothers. By 2006 when investment banks bought as many as 11 originators and servicers, industry participants were noting that access to product for ABS underwriting was becoming important and there were increasing numbers of bidders on loan sales such that having a 'captive' pool of loans seemed attractive. At the same time, slowing house prices meant that the value of mortgage originators was beginning to appear attractively cheap.

Other benefits cited included accruing all the fees from origination of the loans to selling into the securitization. Furthermore, the better credit of the acquirer relative to the originator would lower the cost of capital for new loan issuance for the merged entity. The final benefit is cited (now perhaps somewhat ironically) as improved risk management, including for instance, having 'much more information about the quality of the loans [...]' which goes to the asymmetric information between originator and issuers.

Companies that bought subprime mortgage originators (and servicers) in 2006 included Barclays Capital, Bear Stearns, Deutsche Bank, Merrill Lynch, and Morgan Stanley. Lehman's presence was particularly big, of the $133bn in MBS it sold in 2005, $86bn it originated itself Currie 2007. Origination was not the only thing Investment Banks targeted though, Currie (2007) goes on to cite economies from 'bringing securities in-house', and for example the potential to use the 'servicing business as a distressed debt platform to buy non-performing loans'.

A further reason for buying a servicing business is related to the agency relationship between the investors/issuers and the servicers. For example Currie (2007)) quotes the co-head of mortgage trading at Bear Stearns as saying 'I believe having a captive servicer is

integral to being in the mortgage-backed security issuance business. In the aftermath of Hurricane Katrina we tried to ascertain the status of certain properties. On loans where we weren't the servicer we had a very difficult time getting this information' and, furthermore, 'better servicing means better loan recoveries [. . .]'

Of course, not all banks aimed to expand vertically. The last wave began in 2006 as subprime originators were falling in value, although Bear and Lehman's were already invested following the previous slump in subprime at the turn of the century. Reasons for not integrating into origination or servicing included not wanting to lose flexibility in purchase (UBS), lack of fit into other business lines (Goldman), and reputational risk from links to predatory lending charges (HSBC, Citigroup).

3.4 Modifications, Foreclosure, and Securitization

Modification data can be difficult to analyze, but reports indicate that attempts to modify the loans of many of the houses in foreclosure have been unsuccessful. Levitin (2009d) describes the various private sector, administration, and industry association modification initiatives, HOPE Now Alliance, FHASecure, Hope4Homeowners, and the Making Home Affordable Program, as having very limited success. Press coverage corroborated these failings (Norris 2009).

3.4.1 Reasons for intervention

First we consider why it might be appropriate to intervene in the contracts so as to mitigate the extent of a foreclosure crisis. M. White (2009) states that since some of the foreclosure cost are external, lenders foreclose 'too often'. This, in her view, is the market failure justifying administration intervention. Levitin (2009b) lists concerns such as pecuniary ex-

ternalities on neighbourhoods and local tax bases; increased blight and potentially crime, and the erosion of 'social bonds', detailed in the Congressional Oversight Panel (2009) as 'community ties are cut, affecting friendships, religious congregations, schooling, transportation and medical care'.

The essential point is that there may be externalities on non-contracting parties. In terms of pecuniary externalities, and by way of historical analogy, we might consider the striking by the Congress under Roosevelt of the gold indexation clause. As Kroszner (1998) details, the debt relief this implied resulted even in the increase in the price of corporate bonds containing gold clauses. This he argues shows that the benefits in some cases of avoiding the costs of bankruptcy more than offset the loss to creditors of trying to recover part or all of the losses from devaluation.[24]

3.4.2 Limitations on intervention

There are various (not mutually exclusive) views of limitations on loan modifications. As summarised in Levitin (2009d) and the Congressional Oversight Panel (2009), these are (i) that the securitization structures, embodied in the pooling and servicing agreements (PSAs), introduce contracting rigidities and adverse incentives for renegotiation (Gelpern and Levitin 2009), (ii) that securitization has resulted in various incentive problems and inadequate fee structures for servicers (COP, 2009; Smith et al, 2009); (iii) that concerns about redefault and self-cure (particularly in a setting of rapidly declining house prices) make it unprofitable for investors to modify loans (Adelino, Gerardi, and Willen 2009); and (iv) that loan servicers are insufficiently experienced and lack capacity to perform these modifications (Congressional

24. See Kroszner (1998) 'the anticipated benefits of enforcement of the gold clause [...] must be more than offset by the expected reduction of payments to bondholders due to bankruptcy and distorted investment incentives [...] a 'debt relief Laffer Curve' exists [...]'

Oversight Panel 2009).

Pooling and Service Agreement Contract rigidities and Renegotiation

Bernanke (2008) has pointed to 'anecdotal evidence' suggesting inefficiencies arising from the diffuse nature of investors holding RMBS which has left renegotiation almost impossible:

> this apparent market failure owes in part to the widespread practice of securitizing mortgages, which typically results in their being put into the hands of third-party servicers rather than those of a single owner or lender. The rules [...] do not always provide them with clear guidance or the appropriate incentives to undertake economically sensible modifications [...] some modifications may benefit some tranches of the securities more than others, raising the risk of investor lawsuits. More generally, the sheer volume of delinquent loans has overwhelmed the capacity of many servicers, including portfolio lenders, to undertake effective modifications.

Eggert (2007) also endorses this view when he states that in the 'tranche warfare of securitization, unnecessary foreclosures are the collateral damage'. Smith et al (2009) cite foreclosure counsellors' experience with many servicers who say '[that because of their] investor guidelines, they're not allowed to do [anything except a repayment]'.

Gelpern and Levitin (2009) present the most detailed exposition[25] of the ways in which the securitization structures may be limiting foreclosure modification. As summarised in Levitin (2009a, p.2), these include 'outright contractual prohibitions and limitations, litigation risk, and adverse incentives for the servicers who make the modification decisions'. The next section sets out in more detail the organizing framework they provide.

A typology of securitization contractual rigidities Gelpern and Levitin (2009) consider the separate question of the immutability of the PSAs from that of the mortgage contracts themselves (rigidities there may include: prepayment penalties, restrictions on

25. But see also, Cordell et al. (2008).

modifying single-family principal residence mortgages in bankruptcy, and the existence of multiple liens).

Instead, they focus on the rigidities imposed by the pooling and servicing agreements governing the RMBS securitization relationship between the servicer and the SPV, and hence investors. These PSAs, they argue, lead to 'excessive' foreclosures and numerous and varied spillover effects. Rigidities, on their account, range from the formal (prohibitions on amendment) to the functional (characterised by collective action problems).

The two formal rigidities from securitization they identify are explicit limitations on loan modifications[26] and statutory and contractual voting thresholds.[27] To this they add a structural rigidity (through bankruptcy remoteness and passive management)[28] and functional rigidities arising out of tranching, resecuritization and insurance which aggravate collective action rigidities.[29] In fact, and as they stress, making this debt non-renegotiable was the purpose of the design ex ante. But while this may have been bilaterally optimal, it now may be socially inefficient. The authors argue that voluntary loan modifications initiatives are unlikely to greatly reduce foreclosures in light of these PSA-induced rigidities. We consider

26. Modification might be prohibited; may be restricted to various forms, may require third party consent, or may be limited in number. Hunt (2009) finds that most deals embed some limitation and in 10% of 2006 subprime RMBS modifications are banned.

27. The effect of the 1939 Trust Indenture Act (TIA) is to require investor unanimity to modify the economic terms of the RMBS (p.17), although it should be noted that this is an area where case law to provide guidance is absent. At the very least, the effect of the Act is to create uncertainty, which is a factor where servicers are risk-averse. In addition to the TIA, there are often explicit supermajority requirements in the terms of the PSAs.

28. In contrast, Gelpern and Levitin (2009) note that in the case of corporate default, the institution of bankruptcy is in place to overcome creditor collective action problems.

29. Aside from the number of diverse investors, the fact that RMBS are tranched can result in further co-ordination problems (p.29). Subordinated trancheholders may veto modification, for example, if they are out of the money. This is the common problem of hold up. Alternatively, senior trancheholders may have no incentive to modify, since assured of repayment regardless. It is only the 'pivotal' or 'fulcrum' tranche which should be decisive.

government interventions in these debt markets in more detail below, but it should be noted here that none of the initiatives thus far have attempted to require contracting parties to infringe on the terms of their PSAs.

As described by the authors, the fact that multiple investors must delegate authority to the servicer to manage the mortgage pool on their behalf results in agency investor risks, in that the servicer 'will renegotiate the underlying loans, reducing payments into the pool' (p.13). However, the exact nature of the agency problem the authors consider is not clear. Gelpern and Levitin (2009) note that the problems identified above are amplified under resecuritization. Furthermore, agreement from net interest margin insurers also may be required to modify underlying mortgages as well as the PSAs.

An important point, not explicitly dealt with by the authors, but hinted at by Bernanke (above) is the fact that different tranches of the securities are often tied to different sources of cash. For instance, some might be comprised of principal repayments, and another against the interest income. This will result in large divergences in interests when it comes to restructuring the loan. This might make it particularly difficult for private label servicers to write down principal or extend term (contractually, the nature of the modification may also differentially impact servicer remuneration).

Contractual immutability and externalities. Gelpern and Levitin identify benefits of immutability as possibly including: enhanced disclosure, ex ante investment, discouragement of holdup and minimization of agency costs. For the borrower, this type of rigidity can lower costs of borrowing[30]. But, as noted, there can be externalities attached to this bilateral

30. We should be clear on who the borrower is we mean here — it is unlikely that homeowners were aware of whether or not their loan would be securitized — the question of pass-through and the industrial organization of the origination business is one that deserves further research. It may be that the competitive variable was quantity of loans — with competition in essentially an unregulated market resulting in a decline in underwriting standards. If this resulted in loans being made which were unsuitable for borrowers, in some

arrangement.

The arguments around flexibility have a similar nature to those advanced for the introduction of a Sovereign Debt Restructuring Mechanism at the start of the decade. The outcome of that process for political economy reasons, as explained in Gelpern and Gulati (2007), was the introduction of Collective Action Clauses into New York law-governed sovereign debt contracts. Gelpern and Levitin (2009) make the point that formal rigidity in sovereign debt contracts has not restricted states from term modifications. But the position of RMBS, with these additional and even stronger structural and functional features render, in their view, these securities 'more effectively immutable than sovereign bonds' (p.35).

In their view, the servicing regime — in which consumer debt is transformed into business debt — is an archetype of 'bankruptcy contracting'. The SIV is remote from the statutory bankruptcy regime, and absolute priority is enforced through the tranching process. However, while bilaterally efficient, there are important externalities from these contracts onto communities, other creditors, financial markets, and the macroeconomy (p. 43). These may involve: higher foreclosure rates which have spillover effects in the region; as well as on holders of other RMBS (and there is then, furthermore, a feedback mechanism here). This then spills over into financial markets more generally through increasing uncertainty, counterparty risk, and illiquidity. In the current crisis, Gorton (2009) has characterised a crucial aspect as a run on the wholesale financing market in the form of lenders requiring ever-increasing haircuts on MBS held as collateral in repo transactions. The effect on the macroeconomy as a whole again feeds into higher foreclosure rates.

The final part of Gelpern and Levitin (2009) considers various possible responses to contractual rigidities. These include: statutory bankruptcy; government carrots (e.g. financing subsidies for renegotiation) and sticks (state foreclosure moratoria). The third method is an

---

sense, then this might be consumer surplus reducing.

invocation of eminent domain (e.g. through nationalization). Furthermore, Congress might pass legislation rendering certain PSA clauses as unenforceable. The authors then examine the use of these mechanisms in the context of various New Deal programs. For example, the effect of the Gold Clause decisions is that legitimate government macroeconomic policy-making legally trumps private contracts (indeed allows governments to rewrite these) (See also Kroszner 1998). Finally, they consider failed efforts to address the farm mortgage crisis — in this instance co-ordination failures among secured creditors resulted in insurmountable 'functional rigidities' — that is, collective action problems. However, these problems were recognised and rectified in the 1986 enactment by Congress creating Chapter 12 of the Bankruptcy Code, which sought to help address the then crisis in farm foreclosures.

Securitization and Servicer Incentives

The lack of incentives of servicers to modify has also been touched on in general terms by Lewis Ranieri, one of the early creators of the private label MBS market in the following terms:[31]

> the cardinal principle [...] is you're always financially better off restructuring a loan around a credible borrower than going into a foreclosure [...] the problem now with the size of securitization [...] so many loans are not in the hands of a portfolio lender but in a security where structurally nobody is acting as the fiduciary. And part of our dilemma here is 'who is going to make the decision on how to restructure around the credible borrower and is anybody paying that person to make that decision' [32]

---

31. Remarks at the 2008 Milken conference on financial innovation, Video at Economist's View `http://economistsview.typepad.com/economistsview/2008/05/financial-innov.html`. See also the discussion by Mike Konczal at `http://rortybomb.wordpress.com/2009/07/31/the-financial-innovation-that-wasnt/`

32. However, Ranieri goes on to argue in this interview that government intervention is not required since 'we know what to do [...] if we're allowed to do it', since technology 'has made it immensely easier' and the relevant know-how exists. This appears to have been overly optimistic.

In much the same way, Levitin's (2009b) survey article describes the agency problem between servicers and investors as fundamental. He provides the example of the FDIC seizure of IndyMac and its decision to modify securitized portfolio loans to increase asset value (Levitin, 2009b, p. 625) to suggest that where that agency problem is absent, investors will find and have found it profitable to modify.

The crux of the issue is compensation. Fee structures have been described as providing insufficient incentive. The fee is generally '.25% or .50% of each loan's balance per year' (Smith et al, 2009). The labor intensive, lengthy and arduous process of modification may result in costs exceeding those fees.[33] The mechanics of payment is important here as well. Cash flow for the servicer may be tight — since they need to advance delinquent payments to investors even if not yet received, but once the delinquency has been 'resolved', they are paid 'off the top'. This may incentivize them to foreclose quickly (Geanakopolos and Koniak (2009)). Under foreclosure, compensation is on a cost-plus basis, and the fees the servicers can charge typically include 'collateral inspection fees, and process serving fees, etc' (Levitin, 2008c).

Geanakopolos and Koniak (2009) have summarised the argument:

> 'Once a homeowner is in default, the servicer must advance that homeowner's monthly payments to the bondholders, getting repaid itself only when the house is sold or the loan is modified. So cash-strapped servicers want to foreclose prematurely or do a quick-and-dirty modification (without due diligence and thus without considering principal reduction) to get their money back fast.'

---

33. Industry commentator David Merkel has observed that it may be useful to contrast CMBS servicing with these RMBS arrangements. In this setting delinquent loans are shifted from servicer to special servicer who receives a premium for successful workouts. The cost is offset against reduced interest payments for the junior certificateholder, who is frequently an originator. This is again suggestive of the agency cost, but it should be stressed that more work is needed here. For Merkel's comments, see his Aleph Blog, http://alephblog.com/2009/12/31/nine-notes-and-comments/

Or, as Levitin (2008) has even more succintly observed in testimony to the House Judiciary Committee:

> 'The choice between modification and foreclosure is a choice between limited fixed-price income and a cost-plus contract arrangement with no oversight of either the costs or the plus components.'

Concerns about Redefault and Self-Cure

Adelino, Gerardi, and Willen (2009) present evidence that the lack of incentive for servicers to renegotiate mortgages is not due to securitization. Their view is that foreclosure is often privately optimal for investors so that the agency frictions are not decisive. Instead they argue that servicers and investors are more concerned with post-renegotiation redefault or self-cure, namely that either borrowers will default again anyway or will become solvent without costly renegotiation.

Adelino, Gerardi, and Willen (2009) compare renegotiation rates of 'private-label loans' to 'portfolio loans' and find no statistically significant, nor economically meaningful difference (they argue that their findings are robust to unobserved heterogeneity). They report reasons why portfolio loans may also be difficult to renegotiate — including accounting rules, staff shortages, and agency problems between loan portfolio managers and investors. They also note the following 'institutional evidence' in further support of their claims: low modifications in earlier housing crashes predating securitization; equal treatment provision statements in PSAs, directing servicers to behave as the mortgage owner; and the absence of lawsuits directed at servicers by investors in mortgage-backed securities.

There is a little bit of conceptual difficulty here — on the one hand they are arguing that there weren't very serious modification difficulties induced by securitization, at least relative to the other factors they model; on the other hand they are saying that what frictions

there were at least matched by the accounting-standards-induced renegotiation frictions on portfolio loans. They relegate to a footnote the possible explanation that the requirement for equal treatment of private label and portfolio loans in PSAs leads servicers to avoid portfolio loan modifications. Instead they concentrate their analysis on arguing simply that expected recovery under foreclosure is higher than under renegotiation.

The two factors they concentrate on are self-cure, namely that 'more than 30 percent of seriously delinquent borrowers 'cure' without receiving a modification' (p. 7) and redefault — namely the fact that many modifications suffer from recidivism which given declining house prices results in even less recovery. These two factors are the basis for their theoretical model. One limitation of their empirical study is that they cannot actually observe modifications and must impute them from information on loan terms. A further problem is that the dataset they employ (LPS) under represents subprime mortgages (p.12).

A similar paper is Foote et al. (2009). They also dispute the contention that foreclosures are not in investor interests (while agreeing they may be socially inefficient). For them, the allegedly inappropriate compensation structures (in a situation of crisis) governing the servicer investor relationship are not decisive. They also emphasise instead redefault and self-cure. As in Levitin (2009) and COP (2009), Foote et al. (2009) find that origination DTI ratios are not a strong default-predictor,[34] in comparison to FICO scores, falling house prices and unemployment. It is important to make a point about interpretation here. We cannot equate origination DTIs with underwriting standards.[35] For the reasons outlined in part I, it appears clear that the mortgage industry originated too many 'unaffordable mortgages'. It is not apparent what it means to say that falling house prices predict defaults, without any

34. This paper also uses LPS data. As noted this under represents subprime mortgages. Furthermore, it does not include any information on secondary loans on properties securing a loan (Foote et al p.11). Thus DTI levels are too low.

35. I do not think the authors do this, but I have seen some writers make this leap.

understanding of why house prices are falling. Similarly, to the extent that unemployment is a result of the collapse of the housing bubble, it is also endogenous. The fact that an individual has a large amount of negative equity is evidence of poor underwriting, which is to say, the underwriting of an unaffordable mortgage.

In any event, Foote et al. (2009) argue that the most important reason for the lack of successful modifications is not skewed servicer incentives, what they label the 'renegotiation failure' theory, but the twin factors of redefault and self-cure. In dismissing the renegotiation failure theory, they cite Cordell et al's (2008) interviews with investors, and Hunt's work showing that outright modification bans are rare (Hunt (2009)). They also contend that fears of investor lawsuits of servicers are overblown, since there have not been many lawsuits. This last argument is unconvincing, since there haven't been many modifications reducing the total amount owed either. Finally, they cite the point made by Eric Maskin in a response to the op-ed by Geanakopolos and Koniak (2009), namely that were there these large gains to be had from renegotiation, mortgage holders, servicers, and borrowers would find a way to do these renegotiations. They state, that the transactions costs would have to be $180bn. To some that might not be an implausibly large number. It is certainly true though that redefaults and self-cures are important considerations.

A model of the decision to foreclose. It is useful to consider the model of Adelino, Gerardi, and Willen (2009) of the foreclosure decision. The version in Foote et al. (2009) is slightly more detailed, but in the same spirit.

There are three periods $t = 0, 1, 2$. In period 0, the lender modifies or not. A payment m is owed at 1, and the balance M at 2. House collateral is P_1 at 1 and P_2 at 2. At 1, under no modification, the homeowner defaults with probability α_0, and the lender forecloses receiving $P_1 - \lambda$. With probability $1 - \alpha_0$, the lender receives $m + M$. The PDV under no modification

is $\alpha_0 * min[P_1 - \lambda, M] + (1 - \alpha_0)[m + (1/R)M]$. Under modification, the lender receives $m*$ at 1, and $M*$ at 2, unless the borrower defaults with probability α_1 at 2. So the PDV under modification is $m * +(1/R)\alpha_1 * min[(P_2 - \lambda), M*] + (1 - \alpha_1)(1/R)M*$. Comparing these two terms one sees the following tradeoffs. Modifications can recover capital for lenders from aiding borrowers who otherwise would default, but it also loses capital by reducing payments for those who would have paid back ('self-cures'). The final group are those who were going to be foreclosed anyway ('redefaults') — for this group, while a modified payment is recovered at $1, m*$, the lender may lose by modifying if housing prices weaken further.

On this account, the crucial point that, for example, M. White (2009) cited earlier, misses is this 'Type II error', namely the value which modified mortgages would have had in the absence of modification — in other words, the possibility of self-cure (which, in their data is about 30% of seriously delinquent mortgages). In addition, if borrowers are going to redefault (in their data, 30-45% of cases, in a 6 month timeframe), then lenders receive lower house prices which may be insufficient to compensate for the receipt of the t=1 payment.

Foote et al. (2009) analysis of the LPS data indicates that portfolio loans are not that more frequently modified than private label securitized loans.[36] Finally, Foote et al argue that modifications in the past were not as frequent as is usually assumed (p.32). They base this claim on a review of foreclosure statistics from the Great Depression. Furthermore, a number of state-enacted foreclosure moratoria were passed during the Depression.[37] Thus, evidence that 'we see far fewer foreclosures than we did in the 1930s' is perhaps suggestive that renegotiation in the past was not as common as imagined. On the other hand, the Great Depression was a wider and deeper crisis so we should anticipate worse foreclosure

36. They note though, that their analysis does not permit controlling for all the characteristics of these loans (p. 31)

37. See for example Levitin (2009, p. 628, fn, 220) 'in 1933, twenty one states enacted legislation that functioned as foreclosure moratoria'.

figures.

Levitin notes that the implication of the Adelino et al paper are two: firstly, that the number of 'preventable' mortgage foreclosures are fewer than is typically assumed; and secondly, that widening 'safe harbour' provisions for servicers — that is granting servicers flexibility to modify loans without fear of litigation — is unlikely to make much difference.[38] Levitin argues though, that while capturing some important reasons why modifications might be impaired (namely redefault and self-cure), these concerns are still not large enough to explain the low rates of modifications we observe.

Levitin also argues that securitization servicers and portfolio lenders view self-cures and redefaults differently. Servicers are paid first following sale, so for them redefault is not a concern, while it obviously is for portfolio lenders (although, if servicers are liquidity constrained it still might be). He also argues that self-cure is also not as great a concern since the amount of servicer compensation is not very elastic to interest reductions (which are the majority of modifications).

Levitin thus concludes that there may be a common factor driving the low rates of securitization servicer and portfolio lender modifications — which he speculates might be capacity related (see below). If this is true, we obviously cannot conclude then that contractual rigidities through securitization do not hinder modification initiatives.

Levitin points out further that in his analysis redefaults vary significantly across securitized loans and portfolio loans,[39] with redefault rates for securitised modifications being significantly worse. This must arise either from differences in the loans, or differences in the

38. Levitin (2009) 'Is Redefault Risk Preventing Mortgage Loan Mods?' at http://www.creditslips.org/creditslips/2009/07/is-redefault-risk-preventing-mortgage-loan-mods-.html ; accessed 17 November 2009.

39. Levitin (2009) 'Does Securitization affect Loan Modifications', at http://www.creditslips.org/creditslips/2009/07/a-few-days-ago-i-wrote-a-long-and-detailed-critique-of-a-boston-federal-reserve-staff-study-that-argued-among-other-things.html ; accessed 17 November 2009.

modifications. Levitin notes that the overwhelming majority of securitized loan modifications do not involve a reduction in principal, and furthermore, servicers extend the term of a loan in far fewer cases for private loan securitizations than for portfolio loans. Furthermore, term extensions are far more frequent in modifications performed where Freddie Mac, Fannie Mae or Ginnie Mae is the servicer. In more detail, he observes (2009, pp7-8) that 'we know that almost no loan modifications address negative equity by reducing principal balances. Of the 185,156 loan modifications in the first quarter of 2009, only 3,389 or 1.8% involved principal balance reductions, and all but four of these were for loans held in portfolio, rather than securitized'. Furthermore, 'we also know many loan modifications do not address affordability by reducing monthly payments. 45.8% of the loan modifications done in the first quarter of 2009 resulted in monthly payments remaining unchanged or even increasing (in 18.5% of cases).' I do not think this important empirical debate has been resolved.

Capacity limitations

In addition to the structure of the PSAs, the Congressional Oversight Panel (2009) identifies various servicer capacity problems (pp39-40). These include a lack of preparedness for the crisis and notably poor staffing. The Panel observes that whereas previously work done by the servicers was routine, 'loan mitigation is slower, more complex, and much less automated.'

The irony of the situation has not been lost on many commentators: loan modification is in many cases far more arduous than taking out the loan was in the first place. A 2008 New York Times piece (Goodman and Morgenson 2008) reports that in many cases WaMu loans 'merely required borrowers to provide an address and Social Security number, and to state their income and assets' whereas in Smith et al's (2009), survey of foreclosure intervention counsellors who mediated between borrowers and servicers over the period October 2008 and January 2009, there is the clear indication that the modification procedure is difficult,

lengthy, and often unsuccessful. Servicers are significantly constrained in their personnel capacity to help negotiate workouts, although some improvements have been noticed since the MHA programme was launched although fundamental problems persist.

On the other hand an industry publication characterises the 'biggest problem' as the lengthy nature of the modification process, 'as borrowers often will not even return phone calls from the servicer seeking to reduce their payments' (Temple 2009). Temple also notes the problems of unaffordable payments and redefault. In his view, modifications raise moral hazard, and hence the costs of borrowing, through lowered cash flow for investors (who thus require higher ex ante returns).

3.5 Government Intervention in the Foreclosure Crisis

First we consider some of the key features of the foreclosure process. Firstly, foreclosure law is state-based, which gives rise to regional variation. In all cases, though, the borrower is the legal owner of the property, what the mortgage holder owns, is a right to force the sale (required to be in a public auction) in order to satisfy the debt.[40] Thus, properties only become real estate owned (REO), if the servicer (typically), buys the property at auction. Foreclosures may also differ along the following dimensions: they may be judicial or non-judicial (the latter is cheaper); the extent of the right of redemption (whereby the borrower can repurchase their former home) can vary; and bankruptcy treatment can be different across states. It would be interesting to consider the impact of these regional differences on

40. An interesting feature of the current crisis is that due to the speed of the origination to securitization process, in some cases, purported mortgage holders are not able furnish evidence of their ownership, 'on Oct. 9 in federal bankruptcy court in the Southern District of New York [r]uling that a lender, PHH Mortgage, hadn't proved its claim to a delinquent borrower's home in White Plains, Judge Robert D. Drain wiped out a $461,263 mortgage debt on the property [...] some of the nuts and bolts of the mortgage game — notes, for example — were never adequately tracked [...] nobody truly knows who owns what.' Morgenson, G (2009), 'If Lenders Say 'The Dog Ate Your Mortgage", New York Times, 24 October 2009.

foreclosure and modification outcomes, as well as to research the public auction design.

3.5.1 Interventions

For this account, we draw, in particular, on Robinson (2009). Traditionally, most modifications do not reduce the total balance due (or even the payment), and instead delay payment while adding fees or past fees (Smith et al, 2009). Various initiatives have been launched at the industry, state and federal levels to try and stem the flow of foreclosures. The Hope Now Alliance was initiated by various stakeholders to try and induce struggling homeowners to contact their servicer (Smith et al, 2009). The alliance helped fund the Hope Hotline Telephone Service in this respect.

Initially, in December 2007, Treasury Secretary Paulson introduced the Streamlined Foreclosure and Loss Avoidance Framework, 'Teaser Freezer', plan which sought to incentivize mortgage servicers servicing ARM securitized loans to freeze resets for a period of five years. Robinson (2009) reports that lack of investor approval for modifications represented an impediment.

The FDIC introduced a Loan Modification Program, 'Mod in a Box', for certain securitized or serviced IndyMac loans, following the bankruptcy of that institution. Modification was to be performed for eligible loans where that modification would yield a positive net present value according to three steps namely: interest rate reduction (to reduce debt-to-income to 38%); extended amortization period; and finally, partial payment forbearance which tacks on a zero interest balloon payment to the end of the loan. Robinson (2009) reports the importance of redefault probability as a constraint, in particular for negative equity borrowers, in a declining house price environment. As a technical point, the presence of mortgage insurance had the mechanical effect of biasing the NPV calculation towards foreclosure — this is something the 'Second Look' Program has been designed to overcome.

Under this program, the servicer would forward the loan failing the NPV test to the mortgage insurer, who would then determine whether or not an advance claim could be provided which might permit loan modification.[41]

FHASecure was designed to assist the refinance of non-FHA ARM loans into FHA fixed-rate mortgages. This aided orders of magnitude less people than expected. Congress established Hope for Homeowners (H4H) which allowed allows homeowners to refinance their distressed mortgages with an FHA-insured mortgage. According to Levitin (2009b) this programme has not been very successful thus far. Under revised Treasury guidelines, servicers receive $2,500 up front for loans modified under this procedure Robinson 2009.

The Streamlined Modification Program was a GSE-owned or securitized variation of the FDIC's 'Mod-in-a-box' programme which included the following aspects: a term extension to reduce the DTI to 38 per cent; incremental interest rate reductions; and again, a deferred principal balloon payment to be added due at maturity or upon sale. The GSEs would pay $800 to the servicers for each modified mortgage.

The Home Affordable Modification Program, which is mandatory for TARP banks and voluntary otherwise, is a similar programme to the Streamlined Modification Programme and Mod-in-a-Box. The pooling and servicing agreements are still binding, however.

The protocol of the program allows servicers to choose the means by which to lower the monthly mortgage payment — i.e. it permits them to pick the combination of principal/ interest/ insurance/ taxes reduction so as to reduce the ratio of payment to current income to 31%; the losses from which investors and government are to share (Treasury covers half the cost of reducing DTI from 38% to 31%). Servicers receive $1000 up front per modified loan, and an annual $1000 per loan for those modified that remain current. Recognising, that

41. 'MICA's 'Second Look' Program To Expand Loan Modifications', MICA Press Release, 27 July 2009 at http://www.privatemi.com/news/pressreleases/detail.cfv?id=152

with declining prices, lenders would like to foreclose as early as possible if redefault is likely; the Home Price Decline Protection (HPDP) initiative compensates owners and servicers for modifications done where prices are declining based on a general house price index Robinson 2009. Modified loans are to be those with positive NPV, according to the following sequential procedure: capitalization of arrearages; incremental reduction of interest rates to reach 31% DTI; term extension to up to 40 years; and principal forbearance.

Thus HAMP is meant to address the following considerations (Robinson, 2009): the cost of modification (with Treasury matching funds); servicer constraints and capacities (with incentives for modification, and incentives for keeping the loan current); investor concerns about the chance of redefault (with a HPDP payment), and uncertainty (with a standardized protocol).

3.5.2 Criticisms

Consider the incentives of a borrower with negative equity. If the cost of the modified loan is lower than the cost of renting, then that borrower would want to modify; even if they fully anticipated defaulting at some point in the future. In this case, the servicer would likely not want to modify ex ante. Indeed, this view has been expressed by Edward Pinto, former Fannie Mae Chief Credit Officer[42], 'This fear has been heightened by the concern of some servicers that borrowers will use the trial period to game the foreclosure process and delay their own foreclosures by another 5 or 6 months.' This may particularly be a concern in an environment where the norms against walking away have been loosened. The importance of norms has been argued by B. White (2009). Indeed, there do not appear to be strong

42. 'Edward Pinto: The Government's Loan Modification Numbers Are A Total Sham', *Clusterstock*, 25 October 2009, http://www.businessinsider.com/edward-pinto-the-governments-loan-modification-numbers-are-a-total-sham-2009-10

moral reasons for arguing that borrowers who own more than their homes are worth should continue to make payments on them. He argues that at work is a desire to 'avoid the shame and guilt of foreclosure' and 'exaggerated anxiety over foreclosure's perceived consequences'. Shiller speculated that the emotional binds would loosen over time.[43] One can dispute the magnitude of the moral constraint, but it seems silly to suggest that it was not present at all.

Geanakopolos and Koniak (2009) criticised HAMP at its inception noting that it insufficiently reduced principal for underwater homeowners. They noted further that for subprime and non-prime mortgages:

> there is room to make generous principal reductions, without hurting bondholders and without spending a dime of taxpayer money, because the bond markets expect so little out of foreclosures. Typically, a homeowner fights off eviction for 18 months [...] the subprime bond market trades now as if it expects only 25 percent back on a loan when there is a foreclosure [...] It is those 'underwater' on their mortgages — with homes worth less than their loans — who are defaulting, but who, given equity in their homes, will find a way to pay [...] This couple could rent a comparable home for $10,000 a year, less than half of their current mortgage payments [...] walking away from their home will further weaken their credit rating and disrupt their lives, but pouring good money after bad on a home they do not really own is costlier still.

As noted in a New York Times editorial of 11 November 2009[44], this plan was designed to reduce foreclosures by financing interest rate reductions. Hence, it appears to have been

43. Robert Shiller is quoted at the Wall Street Journal blog *Real Time Economics* as saying '[s]trategic default on mortgages will grow substantially over the next year, among prime borrowers, and become identified as a serious problem. The sense that 'everyone is doing it' is already growing, and will continue to grow, to the detriment of mortgage holders. It will grow because of a building backlash against the financial sector, growing populist rhetoric and a declining sense of community with the business world. Some people will take another look at their mortgage contract, and note that nowhere did they swear on the bible that they would repay.' 5 January 2010, http://blogs.wsj.com/economics/2010/01/05/2010-predictions-from-shiller-blinder-rajan-and-more/

44. 'More foreclosures to Come', New York Times, 12 November 2009, http://www.nytimes.com/2009/11/12/opinion/12thu2.html

founded on a belief that the proximate cause of increasing foreclosures was default due to exploding interest rates. However, as observed in Levitin (2009), the causes of defaults and foreclosures have changed over time. Initial defaults may have been driven by speculator walk-aways when housing prices first flattened and hybrid ARMs reset, but later defaults included pay-option ARM holders[45] and other negative equity prime borrowers.

Note also that the HAMP programme was structured to operate within the constraints of the pooling and servicing agreements.

3.5.3 Other Possible Interventions

The Smith et al (2009) survey of foreclosure counsellors has the following recommended modifications, 'market level interest rates [...] a fixed term [...] or with principal reductions to reflect the true value of the property'.

Levitin (2009) identifies as the 'only one option' that the federal government permit bankruptcy mortgage modifications for single-family principal residence mortgages in Chapter 13, eliminating the negative equity position through 'cramdown'.[46] [47] Following the 1978 bankruptcy reform principal residence mortgages were exempted from modification in Chapter 13, on the basis that ex ante interest rates for borrowers would be lower through

45. Taken out by prime borrowers with imperfect documentation and hence 'Alt-A' rated.

46. Levitin notes however, that Chapter 13 requires the debtor to possess a regular income — but this will obviously not be the case for those who are unemployed. M. White (2009) also considers cramdown. On her account, the pooling and servicing agreements embedded in the securitization process resulted in socially inefficient rigidities. The policy dilemma identified by White (2009) is that administration loan modification programs have been relatively ineffective on account of requiring lender agreement, but that cramdown modifications in Chapter 13 would lead to 'too many' loan modifications.

47. There are two points here in favour of eliminating the special treatment accorded primary residence debt in bankruptcy. If it is true that this dramatically lowers the cost of access to mortgage finance, then it may be an important reason for 'excessive investment' in housing. Instead, if there is a limited effect, then it is possible to eliminate the adverse externalities from foreclosure without the effect of restricting access to credit in the future.

incentivising ex ante lender competition. In fact, Levitin (2009c) argues that his empirical examination reveals that 'mortgage prices are largely insensitive to bankruptcy modification risk'. The crucial ingredient here is that foreclosure loss be at least as large as bankruptcy modification loss. In which case, ex ante credit costs should not be higher. In support of this, Levitin (2008a) notes that there is no spread between 'conforming mortgages on vacation homes and multifamily properties are currently priced the same as single-family principal residences'. Nor, for example, do Fannie and Freddie 'track the difference in bankruptcy modification risk'.[48]

Furthermore, argues Levitin (2009), this is not too administratively burdensome for the courts to handle. He writes that, 'in an age of a trillion dollars in government bailouts, bankruptcy modification is a rare bargain. Bankruptcy courts are well staffed relative to historic filing levels, and court fees cover the administrative costs of the process.'

There are significant political obstacles to efforts to allow cramdown in bankruptcy. An article in Slate from February 2009 described the Obama administration as 'laying the political groundwork to empower bankruptcy judges to order 'cramdowns' of unpaid mortgage debts — forcing investors to accept those elusive but all-important reductions of principal owed' but goes on to note the 'serious opposition from the securities and banking industries' (Katz 2009). Temple (2009), writing in the *American Securitization Journal*, states the position succinctly, 'such proposals would raise serious questions about the rule of law and the property rights of investors who funded the mortgages while having very little positive

48. However, and as he points out in a brief footnote (70) rationing may be in quantity of credit rather than prices. We cannot conclude without knowledge of this quantity effect. Levitin, in a separate paper, presents historical evidence based on the period 1979-1993 where there was institutional variation across federal districts about whether they permitted strip-down (Levitin (2009c), p. 598). The result here was that for some classes of credit LTVs were lower where strip down was permitted. He argues that his analysis suggests no effect on credit availability or the number of bankruptcy filings, but Levitin (2009c) does not present this evidence. However, his analysis showing that the losses from bankruptcy may in fact be lower than the losses from foreclosure are certainly suggestive.

impact on the number of homes being foreclosed. As with modifications, I believe the single most important flaw related to cram-down is that it requires judicial intervention on a borrower-by-borrower basis.' We should also note that many of the difficulties in performing modifications in general, and performing certain types of modifications in particular, are technical in nature. One reason, for example, why banks may oppose modifications including principal reductions is accounting treatment. Reductions in principal result in an immediate write-down on the balance sheet, whereas interest rate reductions result in a fall in future discounted income.

3.5.4 Observations on the Political Economy of Intervention

Undoubtedly, some of the most important obstacles to any intervention in these debt markets are political. Skeel (2001)'s history of American bankruptcy law might provide a model of the type of analytical narrative approach one could apply to this issue. His is a discussion framed in the interest group theory of public choice, where the relevant interest groups at various stages include creditor groups and their advocates, and bankruptcy lawyers and judges (p. 15). In addition to interest groups, he notes the importance of which committees are active in congressional deliberations; the 'undeniable influence' of ideology; and the important role of particular individuals (most notable is his account of the role of William Douglas and his SEC). It is Skeel's contention that there are three primary forces shaping American bankruptcy law and its development (p. 16). These are: the compromise between creditor interests and the forces of prodebtor movements and other 'populist' forces. Attempting to shape these forces towards accretion of their own prominence have been bankruptcy professionals seeking to expand the law's scope. Roughly speaking, Republicans have aligned with creditor interests and Democrats with prodebtor movements, while bankruptcy professionals have aligned with themselves.

I have spent some space outlining Skeel's history, since it aids in considering the type of approach that one might take to the present case. Mian, Sufi, and Trebbi (2009) consider the relative magnitudes of the effects of constituent interests, special interests, and politician ideology in the context of voting behaviour on two acts — the American Housing Rescue and Foreclosure Prevention Act of 2008 and the Emergency Economic Stabilization Act of 2008. Their findings are: mortgage defaults, particularly of own-party constituents, increased the likelihood of voting for the housing bailout (particularly in competitive districts); financial firm campaign donations increased the likelihood of voting in favour of the bank bailout and; finally, they show that Republican politicians are driven more by ideology than constituent or special interests and argue that 'this [. . .] suggests that politicians, through ideology, can commit against intervention even during severe crises.'

This work provides some empirical basis for the important tension that exists (as suggested by Skeel (2001)), between debtor and creditor interests (and hence debtor and creditor regions) which were again on display in the setting of the mortgage cramdown debate. In March of 2009, House legislation was passed that 'would let federal judges lengthen mortgage terms, cut interest rates and reduce loan balances for homeowners in bankruptcy court, even if the lender objects, on the borrower's primary residence'[49], but this did not come to a vote in the Senate. On 11 December 2009, the House voted 241-188 against an amendment to H.R. 4173, the Wall Street Reform and Consumer Protection Act of 2009, brought by John Conyers, and supported by House Financial Services Committee Chairman Barney Frank, containing the same language.[50] The measure was opposed by banks and what Bloomberg archaically described as 'broker-dealers'. The American Bankers Association chairman, in

49. Bloomberg, 8 December 2009,

50. Bloomberg, 11 December 2009

his discussion of this success, stated[51]

> An amendment to add mortgage bankruptcy cramdown to the bill was defeated on the floor by a strong majority. Given that this amendment had passed the House earlier this year, our win is a strong reflection of the hard work of the state associations and grassroots bankers last week. Since the cramdown concept has now been defeated in both the House and Senate this year, hopefully it will not be brought up again.

This would appear to be the last word on the matter. It may be interesting to examine, in a similar manner to Mian et al, the determinants of voting on this amendment.[52] One way of getting at the tension between servicers and investors would be to examine whether there was a difference in opinion over the bankruptcy cramdown prohibition. Levitin (2009b, p. 625) asserts that opposition has been led by the servicers, who exert greater influence than investors who might benefit from cramdown. On the other hand, Foote et al (2009, pp22-23), cite a survey of investors by Cordell et al (2008) which found that 'investors [...] were not enthusiastic about an idea to reimburse servicers for expenses of loss mitigation'. This suggests that they may not see value in principal reduction, and hence may similarly oppose bankruptcy reform of the sort envisaged.

Finally, it is important to note also that should the number of individuals choosing to walk away from their mortgage dramatically increase, this could produce some pressure for loan modifications (Lowenstein 2010). Certainly, in an environment where regulators are intervening heavily in mortgage markets to support home prices, and there exists a moral constraint on walking away, it is not clear why we should expect a large number of modifications.

---

51. Huffington Post, 16 December 2009,

52. Levitin (2009, p. 649-650) observes that in fact a similarly shaped bankruptcy reform passed the House in 2008 and 'was reported out of the Senate Judiciary Committee, but never came to a floor vote because of the inability to get cloture and avoid filibuster'. The legislation was Helping Families Save their Homes in Bankruptcy Act, S 2136, 110th Cong (2008).

3.6 Conclusions

I conclude by presenting some questions arising out of this survey potentially deserving of further study.

Industrial Organization As a general point, it is important to understand the trade-off between integration and disintermediation. As noted, since the firm boundary is an endogenous decision, this presents some challenging but important empirical problems.

Political Economy The foreclosure crisis can be used to address some classical questions in public choice and political economy, such as the relative importance of interest groups, politician preferences and voter preferences. Firstly, it is unclear why there was such concerted opposition to the votes on bankruptcy cramdown. A useful exercise may be to perform a similar analysis to that in Mian, Sufi, and Trebbi (2009) but applied to the three house votes on cramdown. It would also be interesting to get a sense of why the legislation did not make it out of the Senate Finance Committee. Perhaps linked to this is further investigation of the role of congressional intervention in promoting the mortgage expansion, the extent of congressional and executive interference in administrative decisions, etc. Another instructive example here might be the work of Romer and Weingast (1992).

Barriers to Loans Modifications Further work is needed if we are to understand the reasons for foreclosures and why loan modification programs were so apparently unsuccessful. These are also important questions to consider in the context of regulating the industry going forward.

It should first be noted that an important reason for our lack of understanding of the failures of loan modifications is a shortage of data. The Congressional Oversight Panel's

March 2009 report states that 'the existing data are plagued by inconsistencies in data collection methodologies and reporting, and are often simply unverifiable. Worse still, the data being collected are often not what is needed for answering key questions, namely what are causing mortgage defaults and why loan modifications have not been working' (p.14).

As noted, foreclosure rules differ across states in terms of whether they are judicial or non-judicial; the extent of the right of redemption; and bankruptcy treatment. How do modification rates vary by foreclosure rules, rules around the right of redemption, and bankruptcy treatment? What was the relationship between the bankruptcy reform of 2005 and the foreclosure crisis?

In thinking across borrowers: How important are the social norms against walking away from your mortgage and did those change? When others default in your neighbourhood, does this make it more likely that you will default? What is the size of this effect? Linked to this are incentives across servicers. Do servicers internalize they place on others when foreclosing on a borrower?

Furthermore, we need to better understand the precise nature of the agency relationship between borrower and servicer? Roughly, the servicer would like the borrower to undertake some unobservable effort to pay if able. We can think of this in the moral hazard framework. But there is also an adverse selection element, since the servicer has many borrowers, and would only like to modify the 'good risks'.

Some of these questions are more closely examined in Chapter 4 where I and a coauthor study the question of the effects of one foreclosure on neighboring houses. But there is a lot more work to be done.

Chapter 4

Foreclosure Contagion: Measurement and Mechanisms

Laurence Wilse-Samson and David Munroe

4.1 Introduction

The housing bubble and crisis of the last decade has resulted in an unusually large number of foreclosures in the United States. Completed foreclosures—when a mortgage borrower does not make payments on their loan and the lending institution claims the mortgaged property—increased dramatically starting in 2007 from 404,849 properties per year, peaking at 1.05 million completed foreclosures in 2010.[1] The length and severity of this crisis have increased academic interest in the consequences of home foreclosures and have raised questions about how and why foreclosures spread (e.g., Guren and McQuade (2013)).

In this paper we ask whether home foreclosures are contagious: does one completed foreclosure increase the probability that geographically neighboring borrowers end up in the foreclosure process? The answer to this question informs our understanding of home foreclosures, borrower and lender behavior, and appropriate policy toward mortgages and foreclosure procedures. Foreclosure contagion is suspected of exacerbating the housing crises during the Great Depression and the recent financial crisis (Campbell (2013)). Identifying and understanding contagion in foreclosures will provide a better understanding of how and why such crises spread. Furthermore, the presence of contagion is relevant to policy makers concerned with mitigating the spread of home foreclosures.

Our chief contribution is to develop a randomly assigned instrument for foreclosures, which we apply to administrative data to achieve credible, policy-relevant estimates of foreclosure contagion. To our knowledge, ours is the first study to use a randomly assigned instrument to study the local effects of foreclosures. In Chicago (Cook County), IL, where foreclosure cases are decided in court, we use the randomization of new cases to fixed groups

1. We use "foreclosure filing" to refer to the initiation of the foreclosure process by the lender, and "completed foreclosure" to refer to a foreclosure proceeding ending with the mortgaged property being sold at auction. However, lenders are not always successful in foreclosing on a home, and so not all filings end in completed foreclosure—we refer to such unsuccessful foreclosure attempts as "dismissals."

of judges as an instrument for a completed foreclosure. Intuitively, our estimates compare the neighborhoods around two types of properties going through the foreclosure process (i.e., situations in which a borrower is in default and the lender wants to claim the home as collateral): properties randomly assigned to "difficult" judges that, as a result, are foreclosed upon and sold at auction versus properties randomly assigned to "lenient" judges that dismiss the foreclosure case. Since our empirical strategy necessarily relies upon the comparison of neighborhoods around homes in default that do and do not end in foreclosure, our estimates speak directly to the policy question of how strongly lenders should be incentivized to renegotiate delinquent loans. [2] This instrument allows us to surmount the endogeneity of home foreclosures—a key empirical challenge—present in existing studies of foreclosure externalities that primarily rely on fixed effects analyses of very local neighborhoods (e.g., Campbell, Giglio, and Pathak (2011), who study how real estate sale prices are influenced by foreclosures within 0.1 miles).

We develop a novel data set that matches administrative records of foreclosure *court cases* to records on foreclosure *filings and auctions* for Cook County. This county, which contains most of the city of Chicago, is the second-most populous in the U.S. and was relatively hard hit by the housing crisis: the surrounding MSA experienced the 12th largest decline in city-wide housing prices between 2007 and 2011, while 5.2% of the 1.9 million households in Cook County experienced a completed foreclosure. Our data covers the universe of foreclosure filings and completed foreclosures in Cook County between 2004 and 2011, allowing us to leverage the random assignment of foreclosure judges while observing the precise location

2. There is a developed literature that uses judicial bias as an instrument, as we do herein, including: Kling (2006) (sentencing propensities of judges to instrument for incarceration length); Autor and Houseman (2010) (job placement rates of non-profit contractors to instrument for receiving temporary help jobs); Chang and Schoar (2006) (judicial fixed effects to measure judge-debtor-friendliness); Dobbie and Song (2013) (judge discharge rates to instrument for bankruptcy protection); Doyle (2007) (placement frequency of child protection investigators to instrument for foster care); and Maestas, Mullen, and Strand (2013) (allowance rates of disability examiners to instrument disability insurance receipt).

of the associated property. We also use administrative data on residential housing sales to assess whether a completed home foreclosure lowers neighboring housing values.

Concrete evidence on foreclosure-related externalities and contagion has been elusive, owing to empirical challenges (Frame (2010)). Home foreclosures are known to be correlated with neighborhood characteristics and changes in housing prices and macroeconomic circumstances (Mian and Sufi (2009) and Mian, Sufi, and Trebbi (2012)). Existing studies that find negative housing price effects of foreclosure have relied primarily on local analyses that explicitly control for property and neighborhood characteristics (Campbell, Giglio, and Pathak (2011); Immergluck and Smith (2006); Schuetz, Been, and Ellen (2008); Pennington-Cross (2006); Leonard and Murdoch (2009); and Lin, Rosenblatt, and Yao (2007)) or repeat-sales analyses (Harding, Rosenblatt, and Yao (2009) and Gerardi et al. (2012)). Similarly, existing studies finding evidence of foreclosure contagion rely either on local analyses (Towe and Lawley (2013)) or aggregate analyses controlling for neighborhood and zip code characteristics (Goodstein and Lee (2010)). Few studies have taken a quasi-experimental approach to identifying the externalities associated with foreclosure.[3] One notable exception is Anenberg and Kung (forthcoming), who find a drop in real estate listing prices immediately after foreclosed properties are listed on the market. To our knowledge, ours is the first study to use a randomly assigned instrument to estimate contagion and local price effects of home foreclosures.

We find evidence of foreclosure contagion using our instrumental variables strategy that compares neighborhoods with completed foreclosures to neighborhoods where foreclosure cases are dismissed. Relative to dismissal, a completed foreclosure raises the probability of

3. Mian, Sufi, and Trebbi (2012) exploit changes at state borders in policy toward foreclosure (in particular, the distinction between judicial vs. non-judicial states) to instrument foreclosures, although this instrument is not randomly assigned (for example, Pence (2006) shows substantial changes in housing market conditions at the boundaries between judicial and non-judicial foreclosure states).

any new foreclosure filing within 0.1 miles by 10% per year and leads to about 0.5 new filings per year. This foreclosure contagion effect is robust and persistent, lasting for three to four years after the case is decided. Additionally, our estimates show that substantial contagion in foreclosure filings occurs even in neighborhoods with no recent foreclosures—the "first" completed foreclosure in a neighborhood substantially increases foreclosure filings in the following years. Interestingly, there is no evidence of contagion at the height of the crisis (2009–2011); contagion is present primarily during the peak and initial decline of the Chicago housing market (2004–2008). We interpret this temporal pattern as evidence that contagion operates through borrowers who are on the margin of the default decision (2004–2008), rather than those in dire straits (2009–2011). A neighboring completed foreclosure may not be very meaningful to a borrower who is already in negative equity (thus, relatively insensitive to a foreclosure-induced loss of property value) or when foreclosures are common-place (and so the marginal foreclosure conveys little information).

Contagion is not limited to new foreclosure filings—a completed foreclosure increases the number of neighboring completed foreclosures as well. This result suggests that contagion is costly and plays a role in the geographic spread of foreclosures. In particular, we find that, on average, each completed foreclosures induces an additional 1.5 completed foreclosures within four years. Taken literally, our estimates suggest that in the absence of the contagion externality, Chicago would have experienced more than 50% (roughly 43,000) fewer completed foreclosures between 2004 and 2010.[4] We also find that a completed foreclosure increases the number of completed foreclosures even among neighboring cases that had already begun.

While our estimates suggest that a completed foreclosure lowers neighboring residential sale prices, our estimates are largely driven by selection into sale. Within the first year of

4. These figures are based on a simple back of the envelope calculation. Our estimates suggest that each completed foreclosure causes more than 1 additional completed foreclosure within three years, and so an absence of contagion suggests 50% fewer completed foreclosures.

a case ending in a foreclosure, *relative to a case that ends in dismissal*, the average price of neighboring housing sales drops by up to 40%. However, using a repeat-sales methodology to adjust for property quality, our estimates of this effect fall substantially to zero. We interpret this as evidence that a completed foreclosure disrupts the housing market in terms of the types of homes that sell, causing a larger share of lower-quality homes to transact at correspondingly lower prices than the average home in the neighborhood. At the same time, due to the small size of our housing sales sample and the resulting imprecision of our housing price estimates, we cannot rule out a small negative effect of completed foreclosure on neighboring home values (holding quality constant).

We show evidence consistent with the commonly held belief that foreclosure contagion is driven by an increase in borrowers defaulting on their loans in response to a neighboring foreclosure, rather than lenders filing for foreclosure against already delinquent borrowers. There is substantial evidence that lenders and mortgage servicers—third parties employed by creditors to manage loans—indiscriminately favor pursuing foreclosure on delinquent mortgages, rather than modification (see discussions in Adelino, Gerardi, and Willen (2009) and Foote, Gerardi, and Willen (2008) and Levitin and Twomey (2011)). We argue that in the absence of borrower-driven contagion, lenders would not exhibit positive foreclosure contagion. Given that we do observe positive contagion provides evidence that borrowers respond. Moreover, there is substantial contagion even among mortgages serviced by lenders known for automating foreclosure procedures and who are, thus, unlikely to respond to very local market conditions.

There are two prominent explanations of why a completed foreclosure will increase the probability that neighbors default on their own mortgages. The first hinges on a completed foreclosure lowering neighboring home values, thus increasing the likelihood that borrowers are in negative equity or "underwater" on their loans—i.e., owing more than the mortgaged

property is worth (Campbell and Cocco (2011); Campbell (2013); and Goodstein et al. (2011)). As one becomes further underwater on one's loan, the incentive to default on the mortgage increases: the loss on the asset grows relative to the costs associated with foreclosure (primarily moving costs and a drop in credit score). The second explanation is that a completed foreclosure transmits information to neighbors (Guiso, Sapienza, and Zingales (2013); Towe and Lawley (2013)). Specifically, a completed foreclosure may send a signal to neighbors about the future of the neighborhood (influencing the expected value of the property to the borrower), or about the foreclosure process itself (e.g., neighbors may learn about the likelihood of a mortgage modification if they default on their loans).

Contagion is driven not by borrowers in severe negative equity, but by borrowers who are on the margin of being underwater. We use information about loan principal and outstanding balance to construct a proxy for borrowers being underwater and find that a completed foreclosure induces additional new filings among non-underwater loans only. Moreover, this effect is concentrated among loans who are on the margin of being underwater with outstanding debt at the time of filing within 10% of the initial principal. For example, a neighboring foreclosure may act as a "wakeup call" for borrowers in positive equity, sending a strong signal about the current value of their property, the future of the neighborhood, and/or information about the foreclosure process itself. On the other hand, those who are very underwater on their loans may already be well informed about the foreclosure process and the consequences thereof and/or have a sufficiently large negative equity position that an additional loss in value is negligible with respect to the default decision.

Contagion varies substantially depending on whether neighbors have mortgages serviced by the same lender, which we interpret as evidence that information—in this case, information about lenders—plays an important role in reducing contagion. Specifically, we find that when a completed foreclosure occurs there are significantly fewer new foreclosure fil-

ings among loans serviced by the same lender than loans serviced by different lenders. This difference may be driven by borrowers learning more and different information from the experience of neighbors whose loans are serviced by the same institutions: the neighboring foreclosure may send a signal about their lender's behavior, lowering the perceived probability of a successful renegotiation of the loan and, thus, reducing strategic incentives to default.

Our results suggest that policies that keep delinquent borrowers in their homes, for example by encouraging lenders to modify delinquent loans, may reduce the spillovers associated with home foreclosures. We are able to speak to this question since our empirical strategy identifies foreclosure spillovers for the set of marginally delinquent loans for whom the idiosyncrasies of the overseeing judge matter (i.e., the cases that are likely to comply with the instrument)—these are the cases most likely to be influenced by policy interventions. In principle, our results provide support for government policies that encourage modification, such as the Treasury's Home Affordable Modification Program (HAMP) (even if there have been considerable (and well documented) problems in implementation). For example, as of December 2013 HAMP has achieved 27,525 successful modifications of delinquent mortgages in the Chicago MSA. However, this understates the true benefit of the program—our contagion estimates imply that HAMP prevented an additional 44,040 completed foreclosures (and the spillovers associated with these).[5] Extrapolating our contagion estimates to the national level, by succesfully modifying 1.1 million delinquent mortgages, HAMP has pre-

5. HAMP numbers are from the December 2013 Making Home Affordable Program Performance Report from the U.S. Department of the Treasury (available at www.treasury.gov/initiatives/financial-stability/reports/Pages/Making-Home-Affordable-Program-Performance-Report.aspx). The progress report identifies 46,183 permanent loan modifications for the Chicago-Joliet-Naperville MSA. The report indicates a 40.4% redefault rate (within four years) among HAMP modified loans, leaving 27,525 successful modifications. Summing our estimates of contagion in completed foreclosures from Table C.8 suggests that each completed foreclosure induces, on average, an additional 1.6 completed foreclosures (and 1.6 * 27,525 = 44,040).

vented an aditional 1.76 million completed foreclosures. Of course any program must weigh general equilibrium considerations—these include the effects on ex-ante incentives for loan origination, and incentives for default by other borrowers. Our finding that contagion is minimal or even negative among borrowers with the same creditors, may provide evidence that these incentives for strategic default matter—we interpret this finding as demonstrating that borrowers update the probability of a modification downward and are discouraged from defaulting on their loans. As such, a policy that raises the cost of default (e.g., achieving a reduction in loan principal only by going through bankruptcy, as suggested by Levitin (2009a)), or a more direct policy that targets the vacancy and neglect associated with REO properties, may be preferred.

The rest of the paper proceeds as follows: in Section 1 we outline the judicial foreclosure process in the state of Illinois and randomization of judges to cases in Cook County. Section 2 sketches our data sources—administrative records on court cases, geocoded administrative records on foreclosure filings, and deed transfer records. In Section 3 we outline the empirical strategy, which exploits the random assignment of judges for quasi-experimental identification, the results of which we present in Section 4. Section 5 explores possible mechanisms, and Section 6 concludes.

4.2 Judicial Foreclosure in Illinois

Cook County, Illinois, provides a good context in which to study foreclosure contagion. Firstly, it was badly affected by the foreclosure crisis. Between 2002 and 2011, the county saw 302,166 foreclosure proceedings initiated by lenders ("foreclosure filings"), and 134,924 completed foreclosures. These trends are illustrated in Figure C.1. There is a sharp increase in the number of foreclosure cases filed in Cook County (left axis) from about 1,000 per

month in 2004 to more than 3,000 filings per month in 2008. At the same time, foreclosure proceedings became more likely to end in a completed foreclosure: the completed foreclosure rate (right axis) jumps from 45% for cases filed in 2004 to 65% in 2008. Secondly, the foreclosure process in Cook County, IL, goes through the court system, allowing us to instrument a foreclosure outcome using random assignment of judges to cases.

In Illinois, as in many so-called "judicial foreclosure" states, lenders must take delinquent borrowers to court in order to claim a mortgaged property. When a borrower has missed three mortgage payments (i.e., is in default), a lender or the third party servicing the mortgage may initiate the foreclosure process by filing for foreclosure on the associated property with the chancery court (we refer to this event as a **foreclosure filing**). If after ninety days the borrower has not made up all missed payments, the trial begins and the lender's attorney must establish that the borrower: has borrowed money from the lender; has signed a mortgage note promising the property as collateral; and is behind on payments. At the same time, the borrower may mount a defense, for example by disputing any of these facts or claiming that the lender has violated lending laws (e.g., the Truth-in-Lending Act). After hearing the arguments, the presiding judge decides the case, either dismissing the foreclosure action or filing a judgment of foreclosure. If the case is dismissed, the borrower typically continues to reside in the home. If a judgment of foreclosure is filed, then the case proceeds to a foreclosure auction, which we refer to as a **completed foreclosure**.[6] If the sale price does not cover the outstanding balance of the mortgage then the borrower is still considered in debt to the lender, although it is common for lenders to forgive this remaining debt. In the vast majority of cases (around 95% for Cook County), the lending institution purchases

6. Following a foreclosure judgement, a redemption period begins during which the borrower may pay off the entire outstanding mortgage plus late fees, attorney fees, court costs, and taxes. The redemption period ends either three months after the judgment or seven months after the initial foreclosure complaint is served, whichever is later.

the property at auction for the amount of the outstanding loan—in doing so the lender need not record a loss on their balance sheet.[7]

A dismissal may refer to several possible outcomes, most of which result in the property remaining occupied by the borrower. First, if a borrower makes all missed payments within 90 days of the filing, then the case is dismissed and the mortgage is reinstated. Second, rather than continuing to pursue an ongoing foreclosure case, the lender may modify the terms of the mortgage to make payments more affordable to the borrower. Third, the lender may "lose" the case by failing to adequately establish non-payment of the mortgage or that they are owed the debt, or the borrower's defense may be successful. Fourth, a case may be dismissed because the lender does not take action in pursuing the foreclosure. Fifth, a lender may accept a deed-in-lieu of foreclosure, in which the borrower forfeits the home to the lender without going through the courts. Finally, the borrower may negotiate a short sale of their home: the lender accepts the proceeds from the sale of the home as payment for the mortgage. Deed-in-lieu of foreclosure and short sales are generally not an option when there are multiple liens on the property, a fact we exploit to confirm that our results are not driven by these outcomes in which delinquent borrowers lose their property (Agarwal et al. (2011)).[8] In our data we cannot distinguish which of these outcomes occurs; we only know whether the case ends in dismissal or completed foreclosure. However, with the exception of a deed-in-lieu of foreclosure or a short sale, in all of these dismissal outcomes the house remains occupied by the borrower.

---

7. See statistics for Cook County compiled by the Woodstock Institute: blog.cookcountyil.gov/economicdevelopment/wp-content/uploads/2012/11/Wodstock-Institute-Foreclosure-Filings-2007-2012.pdf

8. Anecdotally, deed-in-lieu of foreclosure and short sales are uncommon in Cook County for the reason that, in both cases, creditors are typically taking a loss, while mortgage servicers will accrue lower fees (relevant in cases where the property is being managed by a mortgage servicer): in Illinois, by accepting a deed-in-lieu of foreclosure the lender must forgive all debt, while short sales typically transact at a price below the outstanding debt (Ghent and Kudlyak (2011)).

Foreclosure cases are randomly assigned to a case calendar, which restricts the set of judges that will ever hear an action on the case. A case calendar is a weekly schedule of court-room/judge pairings, usually made up of two or three judges. Judges typically only hear cases associated with their case calendar. Similarly, since chancery court cases are only assigned to one calendar, only the associated judges will oversee an action on that case. When a case is filed, it is assigned a unique case number, sorted by property type (single-family home, condominium, commercial property, etc.), and randomly assigned to a case calendar.[9] As of 2010, there were 12 chancery court case calendars hearing foreclosure cases (there are additional calendars that hear only other chancery court cases). Judges are assigned to case calendars each year by the Chief Judge of the Circuit Court of Cook County.

There are several ways that a Cook County judge might influence the outcome of a foreclosure case, which is necessary for the validity of our instrument.[10] Firstly, the judge has discretion to determine how long a defendant has to find a lawyer and mount a defense. Secondly, even if a defendant does not mount a defense, the judge determines whether or not the lender successfully establishes that the borrower is behind on payments and that the debt is owed to the lender. Establishing these points is not trivial. Throughout the foreclosure crisis, there have been accounts of mistakes and wrongdoing in the prosecution of foreclosures (Kiel (2012)), including failures of banks to produce proper documentation or lenders

9. Random assignment of cases to case calendars is performed by the Chancery Court computer system. As described on the Chancery Court's FAQ page, "When a case is filed in the Law Division it is randomly assigned via a computer program to a calendar letter. You may contact the Information Desk in Law Division to obtain the Judge's information associated with the calendar letter." See www.cookcountyclerkofcourt.org/?section=FAQSPage

10. There is substantial evidence of judicial bias in many settings: Anderson, Kling, and Stith (1999) illustrate important *differences across judges in decision-making*—sometimes suggestive of bias (e.g., Abrams, Bertrand, and Mullainathan (2008) or Yang (2012)), and sometimes more generally based on "personal assessments" of case-specific information (Iaryczower (2009)). Berdejo and Chen (2010) present evidence suggestive of unconscious judicial bias—illustrating priming effects on judges of wars (which suppress dissents)—as well as more partisan behavior before Presidential elections.

initiating foreclosure proceedings without reviewing the history of the loan ("robosigning"). Similarly, it us up to the judge to evaluate a borrower's defense, for example by determining whether a mortgage is legal in the first place (e.g., is not in violation of (predatory) lending laws). Anecdotal evidence suggests that judges vary substantially in their leniency on these issues.[11]

In what follows, we use the random assignment of foreclosure cases to case calendars to instrument the outcome that the case ends in foreclosure. As discussed above, judges may influence the outcome of a case. At the same time, the case calendar to which a foreclosure case is randomly assigned determines the possible judges who will ever hear the case. If judges vary sufficiently in their biases toward foreclosure, then the case calendar to which a case is assigned may influence whether a case ends in foreclosure or dismissal.[12] Thus, our identification relies on the comparison of two delinquent borrowers going through the foreclosure process, one of whom is randomly assigned to a "lenient" case calendar and ends in dismissal, while the other is randomly assigned to a "strict" calendar and ends in foreclosure. To implement this study we require data on Cook County foreclosure cases, including case calendar assignment and the case outcome (foreclosure or dismissal).

---

11. The *Washington Post* observes, for example, that "[in] Suffolk and Nassau counties on Long Island and Kings County... which have among the highest rates of foreclosure in the state and where the 81 judges handling foreclosures have become infamous over the past few years for scrutinizing paperwork ... *the level of tolerance for document mistakes varies from judge to judge* ..." (emphasis added). "Some judges chastise banks over foreclosure paperwork", *Washington Post*, 9 November 2010.

12. Of course, once assigned to a case calendar, the judge within that calendar who hears a case may is not necessarily random. For this reason, we use the case calendar (group of potential judges) as the unit of randomization to ensure orthogonality of the instrument to unobservable characteristics. As long as there is sufficient bias across judges to ensure that the case calendar (group) to which a case is assigned influences the outcome, then the instrument remains valid.

4.3 Data

We use geocoded administrative data for Cook County from three sources: Cook County chancery court records, foreclosure filings, and deed transfer records. Publicly available chancery court records for 2004–2010 provide us with details of each foreclosure case, including the information necessary to construct our instrument: the case calendar to which the case is randomly assigned and the outcome of the case (dismissal or foreclosure). To study neighborhood outcomes, however, we need to know the location of borrowers' homes. To this end, we match each chancery court foreclosure case record to the associated foreclosure filing record (2002–2011), which has been provided to us by Chicago-based Record Information Services, Inc. (RIS). These records allow us to observe new foreclosure filings that occur around any given delinquent homeowner's property, which we use to study foreclosure contagion. To observe how completed foreclosures affect housing markets—prices and sales volumes—we rely on deed transfer records (1995–2008) provided to us by the Paul Milstein Center for Real Estate at the Columbia Graduate School of Business. These records allow us to observe the state of the housing market around each property going through the foreclosure courts. Finally, we bolster the information about each neighborhood using data from the 2000 Decennial Census and Zillow housing price indices.

The Cook County chancery court makes public all court records, which include details on each foreclosure case. We manually collected data for each of the 217,230 chancery court cases filed between January 1 2004 and June 30 2010 from the court's public electronic docket.[13] Each record identifies the case number (a unique identifier assigned by the court), the type of case (e.g., foreclosure vs. other chancery case), the plaintiff (lending institution or mortgage servicer), the defendant, and the case calendar. The records also include every

13. www.cookcountyclerkofcourt.org

action on the case (and corresponding date), although the action descriptions are terse.

We rely on foreclosure filings from RIS to identify the location of properties going through the foreclosure process. The RIS data span all 307,209 foreclosure cases filed between 2002 and 2011 in Cook County. These records contain the same variables as the online chancery court records, except RIS does not collect the case calendar. However, RIS also collects information not included in the court records, such as the type of property (single-family, condo, etc.), details about the mortgage (type of mortgage, original loan principal, outstanding balance at time of foreclosure filing), any additional lien holders identified on the filing, and the address and latitude/longitude of the home under foreclosure. Finally, RIS also collects a record of each foreclosure auction between 2002 and 2011 (168,577 in total). This allows us to conclusively observe a foreclosure outcome and associated date.

We match the chancery court records to the RIS foreclosure filings by case id.[14] The resulting data set covers 174,187 foreclosure filings in Cook County filed between January 2004 and June 2010. For each record, we observe the date the case is filed, whether and when the case is dismissed or foreclosed, the location of the home under foreclosure, and the above-mentioned details of the property and mortgage. We consider a case as ending in completed foreclosure if the RIS records indicate that a foreclosure auction occurs for that property and mortgage, and we consider the auction date to be the end of the case. We consider a foreclosure case as being dismissed if it does not have an associated foreclosure auction and if the chancery court data records a dismissal action, where we take the date of the dismissal action as the relevant "dismissal date" (see the Data Appendix for details of these variable definitions). For our analysis, we drop 847 filings associated with Veterans Affairs mortgages (VA), 12,755 filings made during the Cook County foreclosure moratorium of 2009,[15] and

14. See the Data Appendix for more details on the cleaning process.

15. Cook County enacted a moratorium on new foreclosure filings on April 16, 2009 to last through Septem-

12,365 filings made during the first or last year in which a case calendar hears foreclosure cases, as cases may be non-randomly assigned as the calendar makes the transition.[16] Our results are robust to these sample restrictions.

The majority of cases end in a completed foreclosure, while a small fraction of cases are unresolved due to right-censoring. As can be seen in Table C.1, which provides descriptive statistics (imposing the above-mentioned sample restrictions), 90,653 (61.2%) cases have an associated foreclosure auction, 50,140 end in dismissal (33.8%), and the remaining 7,427 foreclosure cases remain undecided due to right-censoring. The average length of a case is about 373.6 days, although this is significantly longer for cases that end in foreclosure (428.7 days vs. 274.9 for dismissals). Since the Cook County chancery court records are up to date as of the date of collection (early 2012), and the RIS foreclosure auctions are up to date through 2011, we do not observe the end of particularly long cases. This is especially true for cases filed in 2009 and 2010, from which 79.08% of the undecided cases originate. We omit these undecided cases from our analyses (as well as cases for which we observe the decision, but do not have data on our outcomes for that year).

Among dismissals, we see that only 12.0% of the borrowers "redefault", suggesting that the dismissal outcome does not merely delay a completed foreclosure. We define redefault as

---

ber 1, 2009. This moratorium applied to all new filings except those in which the borrower agreed not to mount a defense prior to filing. The effect of the foreclosure moratorium can be seen in Figure C.1: we see that the number of cases filed dips sharply during the moratorium (left axis), while the foreclosure rate jumps up (right axis). Interestingly, it seems the moratorium finished early—new filings spike before September 1, 2009.

16. Case calendars have been added over time to ease the burden on existing calendars. In our data, we observe the addition of six new calendars to the foreclosure roster and the phase-out of 16 calendars (that move from hearing foreclosure cases to hearing exclusively other chancery cases). Unfortunately, the details of these phase-in and phase-out processes are not well publicized and we observe unusually low case assignment to these calendars during the phase-in periods. Our concern is that as new calendars are introduced to the foreclosure process they are restricted in the type of foreclosure cases that they hear. Indeed, including cases assigned to new case calendars in our sample brings our IV estimates closer in line with the OLS estimates, suggesting some non-random assignment to newly introduced calendars.

a new foreclosure filing occurring against the same loan after the first case has been decided; this definition excludes future defaults to the same borrower on different loans and future defaults from different borrowers at the same property. Since dismissed cases make up our counterfactual in studying the neighborhood-level effects of completed foreclosures, this low rate of redefault is reassuring—in most instances dismissing a case does not merely delay the foreclosure (for example, while the lender finds a missing mortgage note), but provides a concrete resolution of the mortgage default within the time frame that we observe.

To study the neighborhood-level effect of completed foreclosure on housing sales and prices we rely on deed transfer records for Cook County from 1995–2008. These records cover the universe of real estate transactions and indicate the date of sale, the sale price, and the property type (residential, commercial, etc.). We restrict this data to residential real estate transactions between 2000 and 2008,[17] which leaves us with 862,215 residential real estate sales. The mean sale price for these transactions is $276,401, while the median is $215,000. We geocode the transactions using the reported property address (using Yahoo! Placefinder), allowing us to observe transactions near properties associated with foreclosure cases.

Finally, we add data from the 2000 Decennial Census matched by tract and the IRS Statistics of Income (SOI) and Zillow (matched by zipcode). The Census provides us with details (as of 2000) on the population density, race, and median of the census tract in which each property is located. The IRS Statistics of Income provide a measure of zip-code-level income (mean adjusted gross income) derived from aggregated tax returns. These data are available for the 1998, 2001, 2002, and 2004–2008 tax years (for 2003, we use the mean of 2002 and 2004, while for 2009+ we use the observed adjusted gross income in 2008). Zillow provides zip-code-level housing price indices for 2000–2011.

17. See the Data Appendix for more details.

4.4 Empirical Strategy

Our primary objective is to estimate whether and to what extent a completed home foreclosure is contagious, which we define in terms of the question: does one *completed* foreclosure cause new foreclosure *filings*? To this end, we compare the number of new foreclosure filings in neighborhoods around properties going through the foreclosure process that end in a completed foreclosure to properties that end in dismissal. An obvious concern is that there is non-random selection into completed foreclosure (versus dismissal); we deal with this endogeneity and omitted variable bias by instrumenting a completed foreclosure using the random assignment of foreclosure cases to chancery court case calendars.

For each property that goes through the foreclosure courts, we measure all outcomes annually within an x-mile radius of the property. We measure outcomes relative to the date that the case is decided (either the date of the foreclosure auction or the date of the court action in which the case is dismissed). For case i, let $d(i)$ be the time period in which the case is decided and $Y_{i,d(i)+t}$ be the outcome for property i measured within an x-mile radius of the property, t periods from the decision date. In practice, we measure time in terms of years: $d(i)$ is the year in which case i is decided, $d(i)+1$ is the year after the case is decided, and so on.[18] In our baseline specification we use a 0.1-mile radius around each property, although our results are not sensitive to taking smaller or larger radii (of the same order of magnitude). We choose this radius both for comparability with existing literature (e.g., Campbell, Giglio, and Pathak (2011)) and to reduce the extent of observations with overlapping neighborhoods (see the discussion in the following paragraph). As an example of how we construct our outcomes, one measure of contagion we consider is the number of

18. We have also tried months and quarters. However, since home sales and foreclosure filings in small geographic areas are low-frequency events, estimates using these finer units of time end up being low-powered and imprecise.

new foreclosure filings within a 0.1-mile radius of each property every year since the case is decided.

One consequence of using foreclosure cases (rather than mortgages) as the unit of observation is that the neighborhoods around cases may overlap. Ideally, we would use mortgages as the unit of observation and relate the probability of each mortgage defaulting to the number of neighboring foreclosures (instrumented by the expected number of neighboring foreclosures). However, in our data we only observe mortgages when they have an associated foreclosure filing; we do not observe mortgages that never enter into the foreclosure process. With our specification, cases within $2x$ miles of one another will have overlapping neighborhoods of observation. The completed foreclosure (versus dismissal) treatment will be imperfectly assigned—e.g., the neighborhood around a case that is dismissed may overlap with the neighborhood around a case that ends in completed foreclosure.[19] However, our instrument is still randomly assigned and will not be correlated with neighborhood overlap. Additionally, the geographic clustering of our standard errors (discussed below) will account for correlated shocks between observations with overlapping neighborhoods. Finally, we test the sensitivity of our contagion estimates to this potential overlap by restricting our sample to cases with no neighboring foreclosures or foreclosure filings within recent years of the case's decision (we discuss this exercise in Section 4.5.1).

To achieve our goal of comparing cases filed at the same time that have different outcomes (owing to the random assignment of case calendars) we include several sets of fixed effects in our baseline specification. Filing-month fixed effects, $M_{m(i)}$, where $m(i)$ is the filing month associated with case i, allow us to compare foreclosure and dismissal among cases filed at roughly the same time (and, as explained below, we construct our instrument at

19. The extent of overlap is not trivial given the high volume of foreclosure filings in Chicago during the crisis and the possibility of foreclosure contagion. The median observation in our sample has four neighboring filings within 0.2 miles in the two years prior to the end of the case and one neighbor within 0.1 miles.

the filing-month level). However, cases filed in the same month may be decided in different years. Since we do not want our estimates to be based on the comparison of cases decided in drastically different times (e.g., the onset of the financial crisis in 2008 versus the peak of the boom in 2006) we include year-of-observation fixed effects, $\psi_{d(i)+t}$.[20] In our baseline specification, we also include property-type fixed effects, Φ_i (single-family home, condo, etc.), as cases are sorted by property type prior to randomization to case calendar. Finally, we include a vector of covariates, X_i (loan principal at origination, a dummy variable for the lender/plaintiff being a "large" plaintiff (six largest plaintiffs each representing $\geq$ 7000 filings), a dummy variable for the plaintiff having a "large" attorney (three largest attorneys each representing $\geq$10,000 cases), whether the census tract has an above-median share of white residents, a set of dummy variables for the quartile of median census-tract income, and census tract population density). While these controls improve precision, our estimates are robust to excluding both the property-type fixed effects and the covariates. The resulting relationship we estimate is:

$$Y_{i,d(i)+t} = \beta_0 + \beta_1 F_i + \beta X_i + M_{m(i)} + \Phi_i + \psi_{d(i)+t} + u_{i,d(i)+t} \tag{4.1}$$

where F_i is an indicator for case i ending in foreclosure. Our goal is to estimate β_1 from Specification 4.1 separately for each value of $t \in \{0, 1, 2, 3, 4, 5\}$ for contagion and $t \in \{0, 1, 2\}$ for price and sales effects (due to data limitations).

We cluster our standard errors along two dimensions: filing month and census tract (Cameron, Gelbach, and Miller (2011)). Clustering on filing month captures correlation due

20. One concern is that the length of the case is itself endogenous. We have explored this in several ways: in Table C.14 we estimate the baseline effects measuring the outcome as of the date that the foreclosure case is filed (rather than decided). While this leads to somewhat noisier estimates (the treatment is diluted by cases that have not yet been decided) the results are generally consistent with our baseline estimates. In Table C.15 of the appendix we add controls for the length of the case and find that our contagion results hold.

to macroeconomic trends—cases filed in the same month may experience similar shocks. Since we also expect correlation between properties that exist in the same geographic area, we cluster at the census tract level. One issue with multi-way clustering that we occasionally encounter is invalid negative variance terms (and a non-positive-definite variance matrix). As suggested in Cameron, Gelbach, and Miller (2011), we conservatively address this by taking the maximum of the standard errors clustered only on filing month, clustered only on census tract, and clustered on filing month and census tract (and the minimum of the corresponding first-stage F-statistics).

Measuring Local Contagion and Prices

We define two outcomes to test for contagion. Firstly, we consider an indicator for whether any new foreclosure filing occurs within x miles of property i in year $d(i) + t$—how does a completed foreclosure affect the probability of observing any new foreclosure filing? This outcome is of interest when there are few filings in the neighborhood and speaks to the question of whether a completed foreclosure influences the general state of nearby mortgages. However, this measure is of limited interest in neighborhoods or time periods with high filing rates filing—when all neighborhoods have at least one filing, a completed foreclosure will have no effect on the probability of any new filing. Secondly, we consider the count of new foreclosure filings within x miles of property i in year $d(i) + t$—how does a completed foreclosure affect the total number of new filings? In both cases, we omit new foreclosure filings at the same address or associated with the same foreclosure case, but at a different address (e.g., a loan taken with multiple properties as collateral). We also consider the effect of a completed foreclosure on the probability of any and total number of neighboring completed foreclosures.

We also examine the effect of a completed foreclosure on local housing prices, although

our estimates are hampered by sample size. While important in its own right, understanding the pecuniary externality associated with completed foreclosure helps assess whether loss of home equity as a channel of foreclosure contagion. We take the log of the average sale price of all properties that sell within the x-mile radius of property i in the year of observation $d(i)+t$. Importantly, we omit the delinquent property itself to ensure that our price estimates are not influenced by an own-price discount of foreclosure (as found by Campbell, Giglio, and Pathak (2011)). This measure does not account for selection into sale—the types of homes that sell after a completed foreclosure may be different from the types of homes that sell after a dismissal. This selection may drive any observed price effects. Unfortunately, a hedonic approach is not possible since we do not observe property characteristics in our data.

While we cannot estimate selection into sale in terms of the types of homes that sell, we can observe whether the volume of sales itself changes. For each property going through the foreclosure process, i, we take as an outcome the count of sales, as of $d(i) + t$, that have occurred within x miles of property i since the year of decision, $d(i)$. We omit sales at the delinquent property i itself to avoid the mechanical effect of foreclosure on sales. A change in the quantity of sales in response to a completed foreclosure suggests that some sellers (or buyers) are selecting into or out of the market. At the same time, observing no response of sales volume does not prove that there is no selection.

To further explore selection into sale, we study the subset of repeat-sales (about 44% of the sample) in our data to adjust for fixed property characteristics. We estimate the quality-adjusted home value by netting out property-specific fixed effects—details are in the appendix. Our estimates of the effect of a completed foreclosure on the log of the mean quality-adjusted price will not be biased by selection into sales under two assumptions. Firstly, we assume that there is not differential occurrence of repeat sales around properties

that end in completed foreclosure vs. dismissal (i.e., no selection into sample). Secondly, we assume that the property characteristics that determine sale price are not changing differentially for properties near a completed foreclosure vs. a dismissal (i.e., the error in the repeat-sales adjustment is invariant to the case outcome). Our repeat-sales estimates may suffer from imprecision due to measurement error in the repeat-sales adjusted measure of home value.

The means of the various outcomes, as displayed in Table C.2, suggest foreclosure contagion and a foreclosure price effect. These averages are constructed using all concluded cases (with the above-mentioned sample restrictions) observed annually for up to five years after the case decision for contagion outcomes, and up to two years after for price and sales; each observation represents a case-year. The means in the upper panel of Table C.2 suggest foreclosure contagion: on average, there are 0.435 fewer new foreclosure filings per year around properties whose cases are dismissed than properties associated with foreclosures. There is also evidence that completed foreclosures disrupt the housing market (see the lower panel): properties that end in foreclosure see a higher volume of neighboring sales (3.099 per year relative to 2.962 near dismissed homes). At the same time, these sales occur at a lower average price—$157,181.90 vs. $184,212.50—although this difference is not apparent in the repeat-sales-adjusted price. While these descriptive statistics suggest negative externalities of home foreclosure, these comparisons of means suffer from omitted variable bias and endogeneity of home foreclosure.

Instrumental Variables Approach and First Stage Regression

There are several reasons home foreclosures may be endogenous to neighborhood-level characteristics. A completed foreclosure is not a random event—it is the product of the choice of a borrower to default on a loan, the choice of a lender to pursue a foreclosure, and the actions

of the associated attorneys and judges. The borrower default decision may be influenced by local housing prices, the type of mortgage a borrower has, and the borrower's financial position (both in terms of balance sheet, and cash flow). For example, foreclosures may be more likely to occur in neighborhoods with lower housing price levels and negative price growth (Campbell and Cocco (2011)). Similarly, the lender's decision to pursue a foreclosure versus a loan modification depends on the home value, the probability that the borrower re-defaults on a modified loan, the probability that the borrower brings him/herself out of delinquency without a modification, and, if the loan is serviced by a company that is not the creditor, the potential fees associated with foreclosure (Foote, Gerardi, and Willen (2008), Levitin and Twomey (2011)).

Descriptive empirical evidence suggests that observable borrower and neighborhood characteristics are correlated with home foreclosures. Table C.1 shows means for various covariates broken down by case outcome, where the fourth column contains the p-value on the test of equality between the foreclosure and dismissal. Cases that end in foreclosure are significantly less likely to be single-family homes (59.0% vs. 68.4%), more likely to have a plaintiff that is a "large institution" (47.3% vs. 47.2%) or have a plaintiff represented by a "large attorney" (68.7% vs. 68.2%), are less likely to have a conventional fixed-rate mortgage (65.3% vs. 66.5%), and tend to be in neighborhoods with lower median income (43,748.26 vs. 46,409.49), a lower share of white residents (43.3% vs. 46.8%), and a lower population density. While studies have attempted to control for omitted variable bias using very local fixed effects analyses (see summaries in Foote et al. (2009), Towe and Lawley (2013)), ours is the first study to directly address the endogeneity of home foreclosure with a randomly assigned instrument.

We use a measure of the propensity to foreclose for each chancery court case calendar as an instrument for completed foreclosure. We construct our instrumental variable to capture

the notion of judicial bias—the judges on some case calendars are more likely to foreclose than others, all else equal—by taking the "jackknife" or "leave-one-out" foreclosure rate for each case calendar, as is common in studies that use judicial random assignment as an instrumental variable (e.g., Kling (2006), Doyle (2007), Dobbie and Song (2013)). Specifically, for each case i, filed in month $m(i)$ and randomly assigned to calendar k, we take the foreclosure rate among all other cases j filed in that month and assigned to that calendar:

$$Z_i = \frac{\sum_{j \in K_{m(i)}, j \neq i} F_j}{n(K_{m(i)}) - 1} \tag{4.2}$$

where $K_{m(i)}$ is the set of all cases filed in month $m(i)$ and assigned to calendar k, $n(K_{m(i)})$ is the cardinality of set $K_{m(i)}$, and $F_j = 1$ if case j ends in a completed foreclosure. A case calendar with "strict" judges whose cases end often in foreclosure will have a high value of the instrument, Z_i, while a calendar with "lenient" judges will have a low value. By omitting case i when constructing the instrument, we ensure that we are not regressing the outcome of the case on itself (resulting in a mechanical correlation in the first stage). Calculating this instrument at the filing-month level accommodates changing case-calendar rosters and attitudes of judges over time.[21] Failing to account for these changes may violate monotonicity of the instrument.

Our first-stage regression relates an indicator for a case ending in foreclosure to our measure of case-calendar strictness. For each case, we regress an indicator for the case ending in foreclosure (F_i) on the instrument. As with the second stage described in Specification 4.1, we include filing month fixed effects, $M_{m(i)}$, property-type fixed effects, Φ_i and year of observation fixed effects ($\Psi_{d(i)+t}$). The resulting first-stage regression is:

21. For example, we see in Figure C.1 that the foreclosure rate changes over time.

$$F_i = \alpha_0 + \alpha_1 Z_i + \alpha X_i + M_{m(i)} + \Phi_i + \Psi_{d(i)+t} + v_i \tag{4.3}$$

We rely on the usual instrumental variables assumptions: the instrument influences the outcome of the foreclosure case (instrument relevance), the instrument is randomly assigned (instrument exogeneity), the instrument does not itself influence neighborhood outcomes except through foreclosure of the house in question (exclusion restriction), and an increase in the instrument is associated with an increase in the probability of the case ending in foreclosure (monotonicity). We check the instrument relevance by examining the first-stage F-statistic for all of our regressions (presented in the Tables discussed in subsequent sections) and find a strong relationship between the instrument and completed foreclosure (Table C.13 in the Appendix presents the coefficients from the baseline first-stage relationship: a one percentage-point increase in the case-calendar foreclosure rate increases the probability of a completed foreclosure by 0.556 percentage points).

If the rules of the Chancery Court are followed, then the instrument should be randomly assigned and appear independent of case characteristics. We run two sets of regressions to check the assumption that the instrument, Z_i, is exogenous. First, we regress Z_i on a set of pre-treatment covariates (controlling for property type and filing month):

$$Z_i = \gamma_0 + \gamma X_i + M_{m(i)} + \Phi_i + e_i \tag{4.4}$$

where Z_i is the instrument, X_i is a vector of fixed or pre-treatment property and case characteristics, and $M_{m(i)}$ and Φ_i are filing month and property type fixed effects. Random assignment (conditional on filing month and property type) implies that none of the covariates predict the value of the instrument ($H_0 : \ \gamma_i = 0$) and nor do they jointly determine the value of the instrument ($H_0 : \ \gamma_1 = \gamma_2 = ... = \gamma_k = 0$). Second, we regress each of these

covariates on a full vector of case calendar dummies:

$$X_{ji} = \rho_0 + \sum_k \rho_k \kappa_{ki} + M_{m(i)} + \Phi_i + u_i \tag{4.5}$$

where X_{ji} is a given pre-treatment characteristic j observed for case i, and κ_{ki} is a vector of calendar-specific dummy variables such that $\kappa_{ki} = 1$ if case i is assigned to calendar k. We then test the joint significance of these dummy variables: $H_0 : \rho_1 = \rho_2 = ... = \rho_k = 0$.

The first column of Table C.3 presents the coefficient estimates from Specification 4.4 and the p-value for the joint significance test of the covariates. We see no evidence of systematic correlation between pre-treatment covariates and the instrument, and cannot reject the hypothesis that the covariates are jointly insignificant. The second column displays the p-value for the joint significance test of case calendar dummies for Specification 4.5, where the outcome variable is given by the row. Again, there is no systematic relationship between case calendar assignment and pre-treatment covariates, with the exception of loan principal.[22] Importantly, we see no relationship between the instrument and total filings and total completed foreclosures (our two main contagion outcomes) in the year prior to the case being filed. We conclude that, conditional on filing month and property type, case calendars are randomly assigned.

The assumption that the instrument does not itself influence neighborhood-level outcomes is reasonable. The outcomes we are studying are the result of the decisions of those not involved in the court case (e.g., neighboring home owners). Moreover, while foreclosure cases span many months, defendants will have minimal direct contact with the presiding judges.

22. Given that we are conducting 19 significance tests in this table, we would expect to observe significance at the 5% level about once (or 0.95 times). Moreover, the relationship between loan principal and the instrument is economically small: a one percentage-point increase in the case-calendar-specific foreclosure rate is associated with a drop of 0.0045 standard deviations in the instrument.

Finally, we find no evidence of a failure of monotonicity. The assumption maintains that a higher value of the instrument—i.e., being assigned to a stricter case calendar—weakly increases the probability of foreclosure for all cases. One can imagine a prejudiced judge who is lenient toward delinquent wealthy borrowers, for example, but push for foreclosure against defendants of lower social class. Then if there are disproportionately more of one type of borrower, a higher value of the instrument will not mean a higher probability of foreclosure for all cases. We explore this possibility by relating group-level foreclosure rates (e.g., foreclosure rate among cases in predominantly white vs. non-white census tracts) within each case calendar to the overall foreclosure rate for each calendar and find that foreclosure rates for sub-groups are all increasing with the overall case calendar foreclosure rate. A discussion of these results can be found in the appendix.[23]

Interpretation of the Two-Stage Least Squares Estimate

Our estimate captures the local average treatment effect (LATE) for foreclosure cases in which judges are influential, compounds the effect of all subsequent completed foreclosures caused by the initial foreclosure, and is representative of neighborhoods with many foreclosure filings. The estimate does not represent the effect of a completed foreclosure relative to a mortgage that is in good standing; rather, the estimate represents the effect of a completed foreclosure relative to the effect of a foreclosure case being dismissed. We argue that this parameter is of interest to policy makers.

Firstly, as discussed in Doyle (2007), if there are heterogeneous treatment effects the parameter identified by a judicial random assignment instrumental variable (or in Doyle's case,

23. As suggested by Mueller-Smith (2013), we have also estimated our baseline specification by constructing the instrument separately for various sub-groups. If monotonicity is violated, then these results may differ substantially. While we do not see a substantial difference in our baseline results (see Table C.16 in the Appendix), these "monotonicity-robust" estimates are imprecise; as splitting the data into filing-month/characteristics cells often yields few observations per cell.

rotationally assigned case workers) is the LATE for "marginal" cases—those where the judge is likely to have an influence. Intuitively, there are cases that will always end in foreclosure and cases that will always end in dismissal; the set of "compliers" with our instrument are the marginal cases where the judges on the case calendar have influence on the outcome.[24] We find that the characteristics of the sub-population of loans that comply with our instrument are consistent with cases on the margin of foreclosure or dismissal, representing individuals who have a higher ability to pay than the typical delinquent borrower, but are facing difficult circumstances that could be mitigated through loan modification. We stratify the sample along several margins: tract-level quartile of income (from the 2000 Decennial Census), whether the loan is from a "large" lender, whether the mortgage is conventional, whether the zip code experiences positive price growth in the year that the case is filed, and a proxy for whether the property is worth less than the loan ("underwater").[25] Our goal is to proxy characteristics of borrowers who are likely to benefit from loan modification; creditors may be more willing to modify in such situations (Adelino, Gerardi, and Willen (2009)), making them more responsive to judicial input. For each sub-sample, G, we estimate the first-stage:

$$F_i = \alpha_0 + \alpha_{1G} Z_i + \alpha X_i + M_{m(i)} + \Phi_i + \Psi_{d(i)+t} + v_i \quad \forall i \in G \tag{4.6}$$

We then take the ratio of the estimate of the first-stage relationship for group G to the estimate for the full sample from Specification 4.3: $\frac{\hat{\alpha_{1G}}}{\hat{\alpha_1}}$. As described by Angrist and Pischke

24. We can conceive of situations where judges will not matter. Sophisticated borrowers may always be able to renegotiate the terms of their mortgages (and a dismissal of the case), regardless of who the judge is. Other borrowers may resign themselves to walking away from their home and mortgage and choose not to appear in court at all.

25. We define a proxy for a borrower being underwater as whether or not the outstanding debt at filing is larger than the initial loan principal.

(2008), the ratio of the sub-group-specific first stage to the full-sample first stage represents the relative likelihood that a complier belongs to the given subgroup.

We interpret our estimates of these ratios, presented in Table C.4, as demonstrating that compliant cases are likely to be on the margin of completed foreclosure. The upper panel shows that compliers are more likely to be in the upper two quartiles of income than the general population of foreclosure cases. Taking income as a measure of a borrower's ability to repay their loan, these estimates suggest that compliers are more likely than the typical borrower to be able to resume payments if the case is dismissed.

At the same time, the compliant sub-population may benefit from a mortgage modification. Compliant borrowers are less likely to be in a zip code with positive price growth. Falling house prices may be largely responsible for the default crisis (Mayer, Pence, and Sherlund (2009)): borrowers may be in default because they expect to lose money on their mortgages as housing prices fall and the value of the asset drops below the cost of the debt. A modification reducing the loan principal or the interest rate may reduce the anticipated loss, making default and foreclosure less appealing. At the same time, compliant borrowers are not in dire straits—they are less likely to be underwater on their loans, so a modification may be more effective (home value is not so low that the mortgage is a lost cause) and may result in smaller losses to lenders than modification of more severely underwater loans. Additionally, compliers are less likely to have conventional loans. There is some suspicion that unconventional mortgages are responsible for many defaults during the crisis. For example, borrowers with low "teaser" interest rates or balloon payments may have been expecting to refinance their loans to avoid higher monthly payments, but found themselves without this option during the financial crisis. In such cases, a modification may be particularly effective (by mimicking the effect of a refinance).[26] Finally, it is interesting to note that the

26. At the same time, there is debate about the importance of unconventional loans in the default decision

differential characteristics of the compliant population appear borrower specific—compliers are no more or less likely to have a loan from a "large" lender.

Secondly, our LATE estimate does not simply identify the effect of a single completed foreclosure, but compounds the effects of all subsequent induced foreclosures. If foreclosures are contagious, then a completed foreclosure will lead to subsequent foreclosure filings. In turn, some of these filings will become completed foreclosures and themselves cause new filings. Since our empirical strategy compares the neighborhoods around cases in the foreclosure courts each year after the case is decided and does not control for the effects of subsequent foreclosures, our estimates will compound the effects of these subsequent foreclosures. We believe that this parameter is relevant to policy makers since it represents broad consequences of the marginal foreclosure, although is imperfect, as the 0.1 mile radius will only partially capture the effect of successive foreclosures.

Similarly, if there is foreclosure contagion, our LATE represents neighborhoods with several completed foreclosures. There is selection into our sample: we only observe neighborhoods around properties going through the foreclosure courts. If completed foreclosures induce subsequent filings resulting in additional observations in our data, neighborhoods with previous completed foreclosures will be over-represented in our sample. This does not affect the validity of the instrument—case calendars are still randomly assigned—but influences the interpretation of the LATE. Nonetheless, we find that our contagion estimates persist when we restrict the sample to properties with no foreclosures in the past two years within 0.1 miles.

Finally, our estimates are conditional upon a foreclosure filing having occurred in the neighborhood. Our empirical strategy and data set necessarily rely on comparing neighborhoods around properties that are already going through the foreclosure process. Our

(c.f., Mayer, Pence, and Sherlund (2009)), so this channel may be less relevant.

estimates will not account for any externalities associated with a borrower default or a foreclosure filing. Many have argued that it is a completed foreclosure and subsequent real-estate ownership of the associated property that drives foreclosure-related externalities. While we cannot speak to any spillovers from borrower default, our estimates provide a well-identified answer to whether there are negative spillovers associated with the completed foreclosure itself.

The LATE represented by our estimates is a relevant parameter for the policy question of how best to address the problems of delinquent borrowers. Policymakers concerned with foreclosures can focus on several stages of the lending process: how easy it is to originate/obtain mortgages, how to prevent borrowers from defaulting, and what to do once a borrower has defaulted. Our parameter, which is estimated conditional on foreclosure filing, focuses directly on the latter question.[27] Moreover, the LATE is relevant for cases on the margin of foreclosure and dismissal, and who are influenced by foreclosure court judges. These cases are also likely to be influenced by policies discouraging foreclosure on delinquent loans.

4.5 Neighborhood-Level Effects of Completed Foreclosure

We find robust evidence of foreclosure contagion that persists over several years. Neighborhoods around a completed foreclosure are 10% more likely to have at least one foreclosure filing in a given year relative to neighborhoods around a dismissed property and experience around 0.5 to 0.7 more total filings per year. We also find that residential properties that

27. Of course, the usual partial-equilibrium caveat applies: any change to foreclosure policy may affect ex-ante incentives (e.g., Mayer et al. (2011)) and housing market outcomes (e.g., Pence (2006)), which are not captured in our reduced-form estimates.

transact around completed foreclosures do so at a price discount (on the order of 30–40%), although this effect may be largely explained by negative selection into sale.

4.5.1 Contagion in Foreclosure Filings

Our estimates demonstrate that completed foreclosures are contagious. Table C.5 presents our baseline 2SLS estimates of the effect of a completed foreclosure on the probability of observing any neighboring foreclosure filing in a year and on the annual count of neighboring foreclosure filings within 0.1 miles of the at-risk property. The 2SLS estimates show that a completed foreclosure increases the probability of observing any new filing within 0.1 miles by 0.052 percentage points in the year of the decision (a 7.4% increase in the mean for all dismissed cases). This effect increases over time to 8.2 percentage points (11.7%) in the second year after the decision, 9.0 percentage points (12.8%) in the third, and 24.7 percentage points (35%) in the fourth year out. Similarly, the 2SLS estimates show that a completed foreclosure causes 0.54 to 0.70 new foreclosure filings per year in the year the case is decided and the following three years. This contagion represents a 25–32% increase in total annual filings relative to an average of 2.161 filings per year around dismissed properties. Note that the instrument is strong in the year of the decision through the second year after the decision (F-stats around 200), although is relatively weak three, four and five years out owing to the smaller sample for these periods.

Our contagion estimates are generally not sensitive to the specification, sample, or geographic measurement of the outcome. The results are robust to excluding the covariates, omitting the property fixed effects, dropping cases decided in the summer months (the court automatically dismisses inactive cases during this time), using a monotonicity-robust instrumental variable, using the full sample and including the foreclosure moratorium, omitting each filing year one by one, and dropping neighborhood-years with foreclosure filings above

the 99th percentile (see Tables C.16, C.17, C.18, and C.19 in the Appendix). We also estimate our baseline results measuring outcomes within 0.25 miles of the delinquent property and find that contagion (and price effects, discussed below) persist—See Table C.20.[28] Finally, we confirm that our estimates are driven by dismissals where the defendant retains possession of the property (rather than deeds-in-lieu of foreclosure or short sales). We estimate our baseline results on the sample of cases in which the plaintiff identifies that there are additional liens against the property. Although less precise, the point-estimates for this sample are comparable to the full-sample estimates and are not significantly different (see Table C.21 in the Appendix).

We further explore the validity of our estimates by applying the same 2SLS procedure to our contagion outcomes measured in the three years prior to the case being filed. If our instrumental variable is truly randomly assigned, we should not expect to see any effect of a case ending in foreclosure before the case has even started. We present these "pre-treatment" estimates in Table C.7. Reassuringly, when instrumented by case calendar leniency, a case ending in foreclosure appears to have no relationship to local housing prices prior to the start of the case—the point estimates are close to zero and generally insignificant.

We examine the cumulative effect of a completed foreclosure in order to appreciate the full extent of contagion. Rather than using as an outcome the number of new foreclosure filings per year for each year since the decision, we instead consider the total number of new filings

28. The estimates for price generally decline as we increase the radius. We find minimal effect for any new foreclosure filing—this is due to the fact most cases have at least one new filing per year within this expanded radius (and so there is little variation in the outcome). On the other hand, the effects for total filings tend to be larger. There are two explanations for this. First, as the radius expands, the compounding effect of neighboring completed foreclosures (induced by the initial observation) grows—the larger radius allows for capturing more of the neighborhood around neighboring foreclosures. Second, as the radius grows the total base number of properties that might file for foreclosure grows. Increasing the radius 2.5 times increases the area of the neighborhood 6.25 times, thus dramatically increasing the potential number of properties that may be affected by the completed foreclosure. However, the estimates themselves only grow by a factor between about 2 and 4, suggesting that the effect declines with distance.

since the decision. These estimates are presented in the second panel of Table C.5 and show that a completed foreclosure leads to a significant divergence in foreclosure filings relative to a dismissal. As noted above, in the year of the decision a completed foreclosure causes 0.691 new filings. However, neighborhoods around completed foreclosures have experienced 2.09 more foreclosure filings by the second year after the decision, and 6.45 more filings by the fourth year after the case ends. One completed foreclosure may have a substantial impact on the composition of a neighborhood, at least in the short and medium term.

One concern with our findings is that they are specific to neighborhoods that are experiencing a wave of foreclosures. Firstly, our period of study (2004–2011) is largely made up of the housing crisis. Secondly, since we only observe neighborhoods where a foreclosure filing has occurred, and since we do find that foreclosures are contagious, there is likely selection into our sample—foreclosure filings (and, thus, observations in our data) are likely to be in neighborhoods with recent completed foreclosures. From a policy perspective, it is especially important to understand the cumulative impact of the first foreclosure in a neighborhood.

A completed foreclosure is contagious even in a neighborhood that has not experienced a foreclosure in recent years. We restrict our sample to cases where there have been no completed foreclosures within 0.1 miles in the two years prior to the decision (the results are similar if we restrict to cases with no filings within two years) and estimate the cumulative contagion effect of a completed foreclosure, presented at the bottom of the second panel of Table C.5. We find clear evidence that completed foreclosures are contagious even in neighborhoods with no other recent completed foreclosures, although these results are less precise than when we use the full sample owing to a smaller sample size: a completed foreclosure leads to 1.3 more filings by the end of the first year after the decision, and almost four more filings by the third year out. Even the first completed foreclosure in a neighborhood has externalities. Moreover, these results suggest that the contagion we observe is not an

artifact of selection into the sample (cases induced into the sample because of neighboring prior completed foreclosures).

Similarly, these results suggest that overlapping treatment and control neighborhoods (i.e., properties around a case that ends in dismissal may also be within the neighborhood of a case that ends in foreclosure) is not a serious problem with our baseline estimates. No new filings within 0.1 miles in the past two years reduces the potential for such overlapping neighborhoods. That contagion persists in these neighborhoods is reassuring. Of course, this is an imperfect test: cases that have no new filings within 0.1 miles in the past two years have a median of 1 new filing per year within 0.2 miles (whose 0.1 mile radius neighborhood will overlap). However, the extent of overlap for such cases is not too large. For example, the overlapping region of a case that is 0.15 miles away represents 14.4% of the total neighborhood area. When we restrict the sample to cases with no new filings within 0.2 miles in the past two years, thus eliminating all potential for overlap, we suffer from small sample size. The (unreported) point estimates still suggest foreclosure contagion (around 0.6 new filings per year in the first three years), but the estimates are very imprecise.[29]

Finally, we find that contagion is strongest during the peak of the housing bubble and beginning of the crash, and disappears at the height of the foreclosure crisis. We estimate the baseline specification restricting the data to a constant sample of cases for which we observe a full three years after the decision.[30] We present estimates for the constant sample

29. An additional concern relates to the length of cases—as seen in Table C.1, cases ending in dismissal are significantly shorter than cases that end in completed foreclosure. A possible explanation is that foreclosure externalities are driven by borrower behavior while in default, and the effect is larger for cases ending in completed foreclosure since these cases are longer. To rule out this explanation, we estimate our baseline 2SLS estimates, adding flexible controls for the length of the case. We try three different sets of controls—log of the number of months, a quadratic in number of months, and dummy variables for the number of quarters of length—and present these results in Table C.15 in the Appendix. These estimates show contagion effects that are comparable to our baseline estimates, although the addition of these length-of-case controls reduces the precision of the estimates.

30. This also addresses the issue of interpretation with the baseline estimates in the top panel of Table C.5

(cases decided in 2004 through 2008 and observed for three years after the decision) in Table C.6. These estimates show strong evidence of contagion—a completed foreclosure increases the probability of any neighboring filing by between 6 and 17 percentage points. Similarly, a completed foreclosure during this period increases the total number of new filings by 1.2 in the year of the decision, increasing to 1.6 the year after, and dropping down to 1.1 and 0.7 new filings in the subsequent two years. Interestingly, contagion is stronger for this sample than for the baseline sample. In the bottom panel of Table C.6 we present estimates of contagion for the complementary sample—cases decided in 2009 through 2011, which we observe for two or fewer years after the decision. There is little evidence of contagion during this time period; the point estimates are very small (-0.018 to 0.027 for any filing, 0.099 to 0.239 for new filings). It is not surprising that during this period of heightened foreclosure activity we find no effect on any new filing per year—if most neighborhoods are already experiencing a foreclosure filing (regardless of neighboring foreclosure cases), then there will be little movement in this outcome. However, the fact that we find no effect of a completed foreclosure on the total number of new filings suggests that foreclosure contagion is not a strong force during this time period.

We interpret this finding of differential contagion by time period as evidence that foreclosure contagion acts on marginal borrowers—those on the threshold of being able to stay in their homes. In particular, at the peak of the housing bubble and beginning of the bust, it may be that a completed foreclosure sends a stronger signal to mortgage holders at risk of default—for example, a signal about the future of the neighborhood and local property values or conveys information about the foreclosure process. Conversely, during the height of the crisis (2009–2011) it may be that mortgage holders are already well informed about

---

that the sample changes each year (e.g., for cases decided in 2010, we only observe the year of decision and one year out).

the state of their mortgages and the foreclosure process itself. Indeed, neighborhoods in the latter period experienced 16% more filings in the year the case is decided than in the earlier period. Similarly, in Section C.4 of the Appendix, we extend our empirical method to relate counts of new filings to lagged counts of completed foreclosures for small neighborhoods. We find a smaller, although non-zero, contagion effect in the latter period, and also find evidence that the marginal contagion effect of a completed foreclosure diminishes as a neighborhood experiences more foreclosures.

4.5.2 Contagion in Completed Foreclosures

To better understand the costs of foreclosure contagion, we look for contagion in completed foreclosures. Above, we established contagion in foreclosure filings—the result of new borrower defaults and lenders pursuing foreclosure action (we take up the discussion of these two actions in more depth in Section 4.6). However, if we do not see contagion in completed foreclosures, then contagion in filings is unlikely to be a large contributor to the spread of a foreclosure crisis. Moreover, the costs of new filings that end in dismissal are, perhaps, smaller than the costs of new completed foreclosures (for example, owing to pecuniary externalities of completed foreclosure, moving costs associated with the displacement of homeowners, etc.).[31]

We find contagion in completed foreclosures. We estimate the baseline contagion IV regressions replacing the outcomes with an indicator for any neighboring completed foreclosure

31. One difficulty in studying contagion in completed foreclosures is that the response may be driven by judges. While this is not an issue when studying contagion in foreclosure filings—an event that depends only on the actions of the borrower and lender—judges have an influence over the outcome of a foreclosure case. We cannot explicitly rule out judge behavior as driving contagion in completed foreclosures. However, we do not expect judge contagion to be a dominant force—this would require judges to be well informed about recent events in the neighborhoods around the delinquent properties associated with their cases, which we find unlikely given the volume of cases and judicial random assignment (judges do not specialize in neighborhoods).

(within 0.1 miles of the property in the given year since the case is decided) and the count of completed foreclosures. We present these estimates in Table C.8 and find that a completed foreclosure moderately increases the probability of observing any neighboring completed foreclosure (by 13.8 percentage points three-years out). Moreover, there is a notable increase in the number of neighboring completed foreclosures: one completed foreclosure causes between 0.28 and 0.56 additional completed foreclosures annually (or between 40 and 93 percent off of the mean). Thus, contagion appears to play an important role in the spread of foreclosures; mitigating completed foreclosures may reduce the depth and costs of a housing crisis.

The timing of contagion in completed foreclosures suggests that borrowers and/or lenders who are already involved in the foreclosure process respond to nearby events. Given how long the foreclosure process takes—from default to filing to completed court case—it may seem strange that we find contagion in completed foreclosures in the year of the decision. However, borrowers and lenders may respond at any stage of the foreclosure process. For example, a neighboring completed foreclosure may influence the effort a borrower puts into fighting an ongoing foreclosure case by conveying information about the costs of fighting foreclosure or the probability of a successful loan modification.[32]

Indeed, we find that completed foreclosures influence cases that are ongoing. For each case, i, we split neighboring completed foreclosures into two groups: completed foreclosures among cases filed before case i is decided and cases filed after case i is decided. By focusing on the former group, we can observe how the outcome of case i influences ongoing foreclosure cases. We define as an outcome the count of completed foreclosures among cases filed before the decision (and an indicator for any completed foreclosure in this group as another outcome)

32. Of course, this need not be a question of effort—given a binary choice (with no effort necessary) between keeping their home and walking away, delinquent borrowers may be swayed toward the latter by neighboring completed foreclosures (e.g., the neighboring event reduces the borrower's perception of the value of the neighborhood).

and estimate our contagion model—the results are presented in the lower panel of Table C.8.[33] The estimates show that a completed foreclosure causes 0.11 to 0.60 new completed foreclosures per year among cases that were filed before the decision in the first two years after the decision (these cases drive the contagion in the first two years, during which there is minimal contagion among newly filed cases). These results provide evidence that contagion acts not only through influencing the behavior of borrowers and lenders before filing (e.g., encouraging borrower default), but also by changing how borrowers/lenders approach an ongoing foreclosure case.

4.5.3 Housing Markets

Our baseline 2SLS estimates, presented in the first panel of Table C.9, suggest that a completed foreclosure lowers the average neighboring sale price over several years. The columns of Table C.9 present the baseline price and sales effects for the year in which the case is decided, and one and two years after. The estimates suggest that a completed foreclosure depresses neighboring residential sale prices by 12.7% in the year of the foreclosure, 41.1% in the year after and 35.8% two years out.[34] However, the precision of these estimates suffers from a smaller sample size than our contagion results (we only observe housing sales through

33. We present similar estimates for the complementary sample—completed foreclosures among cases filed after case i is decided—in Table C.22 of the Appendix. These estimates (in the upper panel) show a *negative* effect of a completed foreclosure on neighboring completed foreclosures for the year of decision and the year after, with a positive effect by the 2nd year out. However, the negative effect is not a result of more cases being dismissed—estimates in the lower panel show no (or sometimes negative) effect of a completed foreclosure on neighboring dismissals for this sample. Thus, it appears that the negative estimate for completed foreclosures is a result of cases taking longer to be decided after a neighboring completed foreclosure, perhaps because those cases that are induced into filing by the neighboring foreclosure are different in some sense (e.g., more complicated, borrower puts up a stronger defense, etc.).

34. That the effect becomes more pronounced one year out is consistent with the theory that foreclosures lower neighboring prices through a disamenity effect, and with existing studies that find that the housing-price effects of foreclosure are driven by the supply-effect of the property being listed, keeping in mind that banks generally do not list foreclosed properties on the market immediately.

June 2008 and only observe prices when a home sells): they are only significant in the year after the decision and the first-stage F-statistics are around 20. Nonetheless, these estimates are not sensitive to the same robustness checks described in Section 4.5.1 (see Tables C.17, C.23, and C.24 in the appendix). Similarly, we see no pre-filing relationship between local housing prices and the eventual outcomes of the cases (see Table C.7).

Our 2SLS estimates show suggestive evidence that a completed foreclosure influences the volume of residential housing transactions in a neighborhood. The point estimates of the effect of a completed foreclosure on the cumulative number of neighboring residential sales since the case decision shows a large increasing trend—while this effect is not statistically significant, it appears as though a completed foreclosure may induce additional home sales. Keep in mind that we omit sales of the delinquent home itself from the count of sales—this is not a mechanical increase in sales due to foreclosure auctions and REO sales. This increase in number of sales raises the question of whether the drop in sale price after a completed foreclosure is caused by selection into sale of lower quality (and thus, lower price) homes or a drop in the value of neighboring properties (conditional on quality). In particular, if this drive to sell in response to a completed foreclosure is stronger among those with lower-quality houses (for example, because they are in a more precarious financial situation), then we would expect a decline in average neighboring sale price.

Our repeat-sales-adjusted price results suggest that there is negative selection into residential sales after a completed foreclosure, which may explain much of the negative sale-price effect associated with completed foreclosure. As explained in Section 4.3, we use a repeat-sales methodology to adjust reported sale prices for property quality. We then estimate the effect of a completed foreclosure on the log of the average neighboring quality-adjusted sale price. The point estimates of the effect of home foreclosure on the log of the mean repeat-sales adjusted price tend to be small: 0.059 in the year of the decision, 0.003 the following

year, and are not significantly different from zero; controlling for property quality yields smaller price effects of completed foreclosure.[35] By pooling the unadjusted (i.e., baseline) and repeat-sales-adjusted price regressions (allowing all fixed effects to vary by group) and testing the cross-equation restriction that the effects of completed foreclosure are different (i.e., $H_0 : \beta_{\text{raw prices}} = \beta_{\text{repeat sales}}$), we find that the treatment effects for adjusted prices are significantly smaller (in absolute value) than for the unadjusted prices in the year after the decision (p-values for this test are presented in the adjusted-price panel of Table C.9). We examine the relationship between completed foreclosure and quality more explicitly by using as an outcome the log of the mean price at previous sale (adjusted for year-of-sale effects) for all neighboring repeat sales observed in the given year. If completed foreclosures induce more low-quality properties to sell, we would expect these properties to have sold at a lower price in the past. We find a large negative, although insignificant, effect of completed foreclosure on the price at previous sale (see the bottom of Table C.9).

We conclude that there is negative selection into sale—when a neighboring foreclosure occurs, the properties that do sell tend to be of a lower quality. The difference between our repeat-sale adjusted and unadjusted baseline sale price estimates, along with the earlier estimates that suggest an increase in sales volume after a completed foreclosure, suggest that lower-quality homes are more likely to transact after a completed foreclosure. This may be the case if owners of lower quality homes in a neighborhood are those who have lower income or wealth. Given a signal that the neighborhood is declining (i.e., a neighboring completed foreclosure), these owners may be eager to sell before the neighborhood "falls apart" in order to avoid the liquidity shock, both from the difficulty of selling an underwater

35. In Table C.25 in the appendix, we confirm that the difference between the baseline and repeat-sales adjusted estimates is not driven by a selected sample of repeat sales. We estimate the baseline price effects for two measures of neighboring unadjusted price: mean log sale price for all non-repeat sales, and mean log (unadjusted) sale price for all repeat sales. In both cases, the point estimates are comparable to the baseline price effects—large and negative—although the smaller sample size reduces precision.

property and the inability to borrow against an underwater home. Of course, we cannot conclude that all foreclosure-related pecuniary externalities are driven by this selection into sale. Existing studies of foreclosure-price externalities (e.g., Campbell, Giglio, and Pathak 2011) find estimates one order of magnitude smaller than our own, and, although our quality-adjusted point estimates are generally zero or positive, given the imprecision of our estimates, we cannot rule out that there are negative effects of completed foreclosure on the quality-adjusted value of neighboring properties.

4.6 Evidence of Contagion Mechanisms

While our reduced-form estimates provide clear evidence of contagion, interpreting the causes of contagion is difficult since a foreclosure filing is the result of a joint decision of the borrower, who defaults on his/her loan, and a lender or servicer that chooses to pursue foreclosure on the borrower's home. Both parties may be influenced by a neighboring completed foreclosure, which may lower the value of the asset and alter the incentives for the borrower to repay and the lender to file for foreclosure. We study heterogeneity in the treatment effect of foreclosures—across loan types, lender identity, and market conditions—to gain insight into the importance of borrower and lender behavior.

4.6.1 Distinguishing Borrower and Lender Response

Our goal is to distinguish whether contagion is driven by borrowers or by lenders. We argue that lenders would exhibit no response to a local completed foreclosure or even anti-contagion—fewer foreclosure filings in response to a completed foreclosure. This provides a weak test for the presence of borrower-driven contagion. We then explicitly test for borrower-driven contagion by examining contagion among loans from lenders known for automation of

foreclosure proceedings (who are, thus, unlikely to respond to very local market conditions).

Lenders and mortgage servicers—third parties who are paid by creditors to collect mortgage payments and manage defaults—have three options when dealing with a delinquent borrower: do nothing, pursue foreclosure, or renegotiate the loan. Through foreclosure, lenders acquire the mortgaged property and gain the benefit of the value of the property (servicers collect fees for managing the foreclosure). Lenders or servicers may instead modify the terms of a mortgage with the goal of making the payments more affordable—e.g., by reducing the principal or interest rate on a loan. The benefit of modification is ensuring that loan payments continue, at the cost of a lower lifetime value of repayment. Finally, when faced with a borrower who is not making payments, a lender or servicer can always do nothing, although this is generally not optimal.[36] Existing empirical studies have found that mortgage modification is uncommon (Adelino, Gerardi, and Willen (2009), Ding, Quercia, and White (2009)). There are several (non-exclusive) explanations for why lenders and servicers prefer foreclosure to modification: borrowers may redefault on a modified loan (Adelino, Gerardi, and Willen (2009)); frequent modification encourages strategic default among borrowers who can pay, but would benefit from modification (Foote, Gerardi, and Willen (2008)); foreclosing and purchasing the property at auction allows creditors to delay recognizing a loss on their balance sheets; modification requires coordination and agreement among all creditors (e.g., multiple holders of a mortgage-backed security or a second lien on the property), some of whom may need to accept a loss (Gelpern and Levitin (2009)).

These incentives to file for foreclosure against delinquent borrowers are unlikely to respond to neighboring completed foreclosures (unless borrowers themselves change their behavior),

36. Inaction may be a good strategy if renegotiation and foreclosure are costly to the lender and if there is a high probability that the default will "self cure"—i.e., the borrower will resume making payments. Additionally, accounting rules adopted in April 2009 allow creditors to keep a delinquent loan at face value on balance sheets if there is a reasonable chance that the loan may be repaid.

making lender-driven contagion unlikely. In the Appendix, we adapt the simple framework of Foote, Gerardi, and Willen (2008) and Adelino, Gerardi, and Willen (2009) to explore how the lender's incentive to modify or foreclose might change with a neighboring completed foreclosure. Intuitively, suppose that a completed foreclosure does not influence the behavior of neighboring borrowers (i.e., no borrower-driven contagion), but lowers neighboring home values.[37] A lender would be weakly less inclined to foreclose since the collateral is worth less relative to the value of the modified loan, although the lender may still foreclose for the reasons discussed in the preceding paragraph. Moreover, mortgage servicers are not likely to respond to a neighboring foreclosure at all. Servicer compensation in foreclosure depends on fees incurred during the foreclosure process and not on the value of the home itself—servicers are indifferent to the value of the collateral. On the other hand, since servicer compensation on a loan in good standing is typically a function of the monthly payments collected, modifying a mortgage entails lower future payments to the servicer and servicers do not typically recover fees for modifying a loan. Thus, when faced with a delinquent loan, pursuing foreclosure is generally more valuable to servicers, and the decision to take this action is orthogonal to the value of the property. This is consistent with the discussion of Levitin and Twomey (2011) who note that servicers have no stake in the value of the property under consideration, and will take the action that maximizes the fees they collect, which is typically foreclosure.[38] In summary, under the assumption that borrowers do not

37. If a completed foreclosure has no influence on neighboring home prices and if we maintain the assumption that borrowers are unresponsive to a completed foreclosure, then lenders and servicers should not respond—the conditions of the neighboring loans are unchanged.

38. Levitin and Twomey (2011) also point out that, although foreclosure is preferable to modification from the servicer's perspective, there is incentive to delay the foreclosure process. Servicers must forward missed mortgage payments to the creditor and are repaid when the borrower resumes payment or the property is sold at foreclosure auction. However, servicers charge late fees that are paid when payments resume or the foreclosure occurs. Levitin and Twomey (2011) show that servicers can benefit by waiting several months to accrue late fees before beginning the foreclosure process. Since this wait period is unlikely to be influenced by a neighboring completed foreclosure, it does not relate to contagion.

respond to completed foreclosures, a neighboring completed home foreclosure should (weakly) discourage foreclosure filings—the collateral is less valuable to the lender and servicers are indifferent to home value.

The above-outlined servicer and lender incentives provide a simple test for the hypothesis of no **borrower-driven** contagion, which we reject. Specifically, this null hypothesis can be rejected if we observe positive contagion (since lenders would display weak anti-contagion). That we observe contagion in Table C.5 suggests that borrowers respond. Note, that while this confirms the presence of borrower contagion, this does not rule out lender response—when borrower default probabilities are changing, lender response is ambiguous.

To further establish that contagion is driven primarily by borrowers, we study contagion among loans held by lenders and servicers who are known to have automated foreclosure filing processes. We identify foreclosure filings by all lenders/servicers investigated in the Independent Foreclosure Review Settlement conducted by the Office of the Comptroller of the Currency and the Board of Governors of the Federal Reserve System.[39] We then redefine our contagion outcomes in terms of new foreclosure filings among automating lenders only and re-estimate our baseline 2SLS contagion models. The new outcomes are: the count of new foreclosure filings among automating lenders and an indicator for any new foreclosure filing from these lenders. Because this restriction reduces our power—we are studying filings among a subset of lenders, which are lower frequency events—we pool all post-decision years ($d(i) + t$ where $t \in \{0, 1, 2, 3, 4\}$) and restrict the contagion effect to be constant across

39. Throughout the mid- and late-2000s, many lenders and servicers adopted automated foreclosure filing procedures (Levitin and Twomey (2011)). In many cases the delinquent borrower's situation and background were not given close consideration. As investigations have revealed, employees of several large mortgage servicers and financial institutions falsely testified that they had personally inspected delinquent borrowers' information, even though the processing speeds made it impossible for this to be true (this was the so-called "robosigning" controversy, settled for $25bn in April 2012 between the federal government, 49 state attorneys general and the five largest servicers). See Kiel, P. "The Great American Foreclosure Story: The Struggle for Justice and a Place to Call Home", ProPublica, 10 April 2012.

all years (see Table C.26 in the Appendix for annual estimates). Clustering our standard errors on the census-tract dimension accommodates correlation between the same property across different years of observation. If contagion is driven largely through lender response, then we should find no effect of a completed home foreclosure on the number of foreclosure filings among lenders likely to be automating foreclosures. At the same time, if contagion is driven primarily through borrower response, then our estimates should be comparable to our baseline full-sample contagion estimates.

We find that contagion is primarily borrower-driven—contagion in foreclosure filings persists when studying loans among automating lenders. Table C.10 presents the pooled estimates for all filings (upper panel) and for filings among automating lenders. One concern is that the total number of loans in a neighborhood from automating lenders is necessarily smaller than the total among all lenders (as in the baseline estimates). If new filings are proportional to the existing number of loans, then we would expect a larger response in the total count of new filings for the baseline than for automating lenders. To address this, we focus on the estimates of the log of total filings in the third column. We observe similar contagion effects for the two outcomes: an increase in foreclosure filings of 9.4% per year in the baseline sample versus an increase of 8.4% among filings from automating lenders.[40]

We also observe similar contagion in the probability of observing any new filing (0.062 versus 0.039). Thus, given the incentives of lenders and servicers and the finding that contagion persists among automating lenders, we conclude that contagion is mainly borrower driven.

40. Similarly, while the point estimates for total filings are different, 0.74 for the baseline and 0.115 for the automating lenders, relative to the mean number of filings for each group (2.44 filings per year within a 0.1 mile radius for all filings, 0.37 for automating lenders), these estimates are remarkably similar (30.3% versus a 29.7%).

4.6.2 Foreclosure Contagion and Negative Equity

We investigate whether foreclosure contagion is driven by mortgages held by individuals who face high debt relative to the value of their property. Mortgage default theory (e.g., Campbell and Cocco (2011), Deng, Quigley, and Order (2000)) suggests that borrowers will only default when the value of their home falls below the balance of their mortgage, putting the borrower "underwater." Intuitively, if the market value of a home is greater than the outstanding debt, a homeowner who is having difficulty making mortgage payments may sell the property and use the proceeds to pay off the debt. Conversely, underwater borrowers who are having difficulty making payments do not have the option to sell. To the extent that foreclosures lower or send a signal about neighboring home values, foreclosure contagion may operate through pushing borrowers (further) underwater (Campbell (2013)).[41] We use a proxy for equity—relating the size of the initial loan to the outstanding debt at foreclosure filing—to determine whether contagion is driven by individuals who are likely to be underwater on their loans. Understanding which borrowers are most influenced by a neighboring completed foreclosure sheds light on the mechanism of contagion and the borrower default decision more generally.

We examine whether foreclosure contagion is more prevalent among loans with large outstanding debt relative to the initial balance. For each foreclosure filing, we proxy the borrower being underwater at the time of filing by whether or not the lender's claim against the borrower (i.e., the outstanding debt at filing) is larger than the initial loan principal.

41. The benefit of foreclosure is muted when borrowers have more than one mortgage taken against the property (e.g., a mortgage and a home equity loan). In Illinois, mortgages are "recourse" debt—if the foreclosure auction generates less revenue than the value of the outstanding debt, the borrower still owes the creditor the balance. In the majority of cases the lender purchases the property in the amount of the outstanding debt to avoid writing down a loss (i.e., the property becomes REO), in which case borrowers no longer owe. However, a borrower is still on the hook for any additional liens on the property. Thus, a foreclosure will not always render borrowers debt free.

Although we observe the lender's claim at the time of filing, we do not observe a direct measure of the property value and so we proxy the value using initial loan principal.[42] For each foreclosure case we split the count of neighboring foreclosures in two—filings among borrowers that are underwater according to our proxy ($N_{i,u,d(i)+t}$) and filings in positive equity ($N_{i,p,d(i)+t}$). We define contagion outcomes in terms of these two counts: $Y_{i,j,d(i)+t} = 1\left[N_{i,j,d(i)+t} > 0\right]$, $Y_{i,j,d(i)+t} = N_{i,j,d(i)+t}$, and $Y_{i,j,d(i)+t} = \log(N_{i,j,d(i)+t})$ for $j \in \{u, p\}$, and estimate our baseline 2SLS specification jointly allowing the time-specific fixed effects to vary with the type of filings under consideration:

$$Y_{i,j,d(i)+t} = \beta_0 + \beta_u \cdot F_i \cdot 1[j = u] + \beta_p \cdot F_i \cdot 1[j = p] + \beta X_i + \Phi_i + M_{j,m(i)} + \psi_{j,d(i)+t} + u_{i,d(i)+t} \quad (4.7)$$

Pooling the estimates for both outcomes allows us to test the null hypothesis that a completed foreclosure has the same effect on underwater borrowers as borrowers in positive equity ($H_0 : \beta_u = \beta_p$).[43] We assume that, were it not for their differing loan to value ratios, borrowers in positive and negative equity respond similarly to a neighboring completed foreclosure, and that lender behavior does not vary along these margins (i.e., any observed heterogeneity in estimates is driven by whether or not the neighbors are underwater on their loans).

We find that contagion is more prevalent among borrowers who are *not* in negative equity. We present the 2SLS estimates of Equation 4.7 in the upper panel of Table C.11. We pool

42. We have experimented with alternative definitions of underwater, for example by making assumptions about loan-to-value ratios at origination to determine home value and adjusting this using local price indices or comparing outstanding balance at filing to local home values. Our results are not very sensitive to these changes.

43. Using log count of filings and the probability of any filing for each group avoids the problem that we do not observe the base number of loans in each category. Additionally, having a randomly assigned instrument ensures that, in expectation, neighborhoods with completed foreclosures have similar numbers of underwater borrowers as neighborhoods with dismissals.

five years of post-decision observations ($t \in \{0, 1, 2, 3, 4\}$), restricting the treatment to be constant across all years (we present the yearly estimates in Table C.27 of the Appendix). These estimates show that a completed foreclosure increases the probability of observing any non-underwater filing by 6.6 percentage points and total non-underwater filings by 9.6 percent, while having little effect on filings from underwater borrowers (0.023 and -0.3%). The difference between the two groups is significant.[44]

We suspect that this finding that contagion is more prominent among borrowers who are not underwater is driven by borrowers who are on the margin of negative equity. Since being in negative equity is generally considered a necessary condition for default, it is unlikely that contagion is driven by borrowers who have lots of equity in their homes. At the same time, using loan principal at origination to proxy housing value in defining underwater and non-underwater loans may have some error on the margins—we may be classifying borrowers who are "just" underwater as being in positive equity. In this case of misclassification, our estimates suggest that the response may be coming from those who are in slight negative equity, rather than those who are very underwater. Either way, our estimates do not imply that those who are severely underwater will not default on their loans, but that they are not responsive to neighboring foreclosures, whereas those who are on the margin of negative equity are responsive. For example, a borrower who is severely underwater may default regardless of what happens to their neighbors. However, for a borrower with debt close to

44. We also examine heterogeneity by local price growth, loan type and by zip-code income level (a proxy for ability to pay). Deng, Quigley, and Order (2000) argue that the decision to default depends on expectations of home value. We adjust our baseline contagion specification by interacting the foreclosure effect with an indicator for positive zip-code-year price growth, but find no evidence that price growth matters for contagion—see Table C.28 in the Appendix. Campbell and Cocco (2011) argue that the probability of default is decreasing in ability to pay (income relative to loan payments) and that incentives for default vary depending on the structure of loan payments. We proxy ability to pay with zip-code-level median income (from the IRS SOI), but find no discernible difference in contagion by income quartile. These results are plotted in Figure C.5 of the appendix. We also find no significant difference between the response among neighboring borrowers with conventional fixed-rate mortgages and those with alternative mortgage products—see Table C.29.

100% of their home value, a nearby foreclosure may be quite important: not only might it push them into negative equity, but it might also convey information about the foreclosure process, lender behavior, or the future of the neighborhood (signals of less import to a severely underwater borrower who has thought through the default decision).

We further explore if contagion is driven by individuals on the margin of negative equity by comparing our 2SLS contagion estimates for filings among borrowers who have close to zero equity in their home (as measured by our proxy) to contagion for filings among borrowers with lots of equity or who are severely underwater. We define filings on the margin of underwater as those where the absolute difference between loan principal at origination (ρ) and the outstanding debt at filing (d) is less than 10% of principal: $\frac{\rho-d}{\rho} \in [-0.1, 0.1]$. As above with underwater and non-underwater filings, we split the count of neighboring filings in two—marginal and non-marginal filings—and estimate the analogue to Specification 4.7 for these two groups. The results, presented in the lower panel of Table C.11, demonstrate that contagion is driven by borrowers on the margin of negative equity: a completed foreclosure induces a 6.3 percentage point increase in the probability of observing any filing for those on the margin versus an insignificant 0.8 for other filers, and a 10.7% increase in the number of new filings among those on the margin, versus an insignificant drop of 3.5% (in all cases the difference between the two groups is significant). Thus, we conclude that contagion is operating through borrowers who are on the margin of negative equity, rather than those who are reasonably well off or in dire straits. This interpretation is consistent with our finding from Section 4.5.1 that contagion is pronounced for cases decided in 2004–2008 when borrowers are less likely to be severely underwater, but not for cases decided at the depth of the foreclosure crisis (2009–2011) when foreclosures are common and many mortgage holders experience financial difficulty.

4.6.3 Foreclosure Contagion and Information

Contagion may also occur because borrowers learn about the foreclosure process, including the behavior of lenders and servicers, by observing neighboring foreclosure cases. Recent survey evidence suggests that borrowers learn from defaults and foreclosures within their social networks (Guiso, Sapienza, and Zingales (2013)). For example, a neighbor may learn about the foreclosure process, including the costs of default (and completed foreclosure), how long the process takes, and the probability of a positive resolution (e.g., mortgage modification) by observing his/her neighbor's experience.[45] A priori, it is not clear whether the foreclosure event would increase or decrease the probability of neighboring filings (contagion vs. anti-contagion). For instance, a foreclosure may lower neighbors' perception of the probability of renegotiating one's loan by defaulting, thus lowering the expected value of default and discouraging this behavior (Mayer et al. (2011) find evidence of borrowers defaulting in response to an increase in the probability of modification).

We investigate a specific social network—neighbors with loans from the same lender.[46] Individuals with the same lender may be more likely to discuss the foreclosure (and/or mortgage renegotiation) process with one another. At the same time, a successful (or failed) mortgage renegotiation provides a stronger signal to individuals with loans from the same institution; it may be that a neighbor's foreclosure discourages default among those with

45. Guiso, Sapienza, and Zingales (2013) also find that social norms may matter for contagion. Many (82.7%) respondents to their survey feel they have a moral obligation to repay their debts. But a neighboring default may weaken respondents' sense of moral responsibility and increase their probability of default. We do not expect this social morality channel to be particularly strong in our context, as our estimates compare neighborhoods around properties where the borrower has already defaulted—the event that sends a signal about the moral obligation to repay.

46. We have also examined heterogeneity in the treatment effect across neighborhoods where we expect weaker and stronger social networks. We study how contagion varies with geography-based proxies for neighbor connectivity, including racial homogeneity, population density, and housing type (drawing on the notion of social capital outlined by Glaeser and Sacerdote (2000)), although we find no systematic relationship between contagion and these proxies. We include a discussion of these estimates in the Appendix.

loans from the same servicer/lender by lowering the perceived probability of a modification. To test for the presence of learning-based contagion, we test whether contagion is stronger/weaker among neighboring loans from the same lender. Under the assumptions that i) individuals are aware of (at least some of) their neighbors' lenders and ii) lenders are not reacting to local conditions, then the difference between contagion among same-lender and different-lender loans provides evidence of the importance of learning from neighboring foreclosures.[47] Note that this signal may be present as long as a borrower knows who his/her neighbor's lender is and observes the outcome of the case. Since a borrower may learn about lender behavior by simply observing the outcome of his/her neighbor's case, heterogeneity in contagion along this margin provides evidence that borrowers learn from their neighbors' experiences, but not necessarily that communication through social networks matters.

To test for a difference in contagion between same-lender and different-lender borrowers, we estimate our 2SLS contagion effects for each subset of neighboring filings. We split the count of neighboring foreclosures in two—filings with the same lender listed as the plaintiff ($N_{i,s,d(i)+t}$) and filings with any other lender listed as the plaintiff ($N_{i,o,d(i)+t}$), and redefine our outcomes in terms of these two counts: $Y_{i,j,d(i)+t} = 1\left[N_{i,j,d(i)+t} > 0\right]$, $Y_{i,j,d(i)+t} = N_{i,j,d(i)+t}$, or $Y_{i,j,d(i)+t} = \log(N_{i,j,d(i)+t})$ for $j \in \{s, o\}$. We then estimate our baseline 2SLS specification jointly allowing the time-specific fixed effects to vary with lender type:

$$Y_{i,j,d(i)+t} = \beta_0 + \beta_s \cdot F_i \cdot 1[j = s] + \beta_o \cdot F_i \cdot 1[j = o] + \beta X_i + \Phi_i + M_{j,m(i)} + \psi_{j,d(i)+t} + u_{i,d(i)+t} \quad (4.8)$$

Pooling the estimates for both outcomes (same-lender filings and other-lender filings)

47. Our same-lender and different-lender estimates are robust to restricting the sample to lenders known for automating the foreclosure process, suggesting that lenders are not driving the patterns we observe in these estimates.

allows us to test the null hypothesis that a completed foreclosure has the same effect on mortgages held by the same lender as mortgages held by other lenders ($H_0 : \beta_s = \beta_o$).

The 2SLS estimates of β_s and β_o from Specification 4.8, presented in Table C.12 (pooling all years; see Table C.33 for yearly estimates), suggest that lender-specific local networks matter. The estimates show that contagion is primarily driven by foreclosure filings among loans held by different lenders—0.061 percentage point increase in the probability of any new filing on a loan from a different lender versus an (insignificant) increase of 0.006 for loans from the same lender, 0.770 additional foreclosure filings from different lenders versus a drop in filings of 0.184 among loans held by the same lender, and an increase of 0.085 in the log of total filings among different lenders versus 0.008 for filings on loans from the same lender. In the first two cases, the difference between the estimates are statistically significantly different from zero.

We interpret these same-plaintiff results as evidence that borrowers learn about lenders from the experience of their neighbors.[48] Given that there is general contagion among loans from other lenders, it appears that borrowers experience anti-contagion when a neighbor from the same lender ends up in a completed foreclosure. A possible explanation for this difference is that the neighboring foreclosure sends different information to different individuals: contagion among those with different lenders is consistent with the foreclosure lowering neighboring home values or sending a broad signal about the general direction of the neighborhood (i.e., the value of the neighborhood is deteriorating). Anti-contagion among

48. Another possibility is that lenders are aware of the externalities of completed foreclosures and do not pursue new filings in neighborhoods where they have had a recent successful foreclosure (while other lenders do not react). However, we find this to be a less likely explanation for two reasons. Firstly, we still observe, on average, a positive and significant number of foreclosure filings around foreclosed properties in the year of and years after the foreclosure decision. Thus, it does not appear that lenders are avoiding foreclosure externalities altogether. Secondly, the anti-contagion effect among filings from the same lender persist for several years after the decision, which represents a long time for lenders to be waiting to avoid foreclosure externalities (while other lenders continue to file in spite of these externalities.

borrowers with the same plaintiff is consistent with borrowers revising downward the probability of a positive outcome (i.e., not losing their home) when a neighboring borrower with the same lender is unsuccessful. This lowered expectation of a positive outcome decreases the value of (strategic) defaults.[49]

4.7 Conclusion

We provide clean estimates of the effect of a completed foreclosure on neighboring residential sale prices and on neighboring foreclosure filings in Cook County, IL. We exploit a randomly assigned instrument—the set of judges who hear a foreclosure case—to compare neighborhood-level outcomes around a delinquent property that ends in completed foreclosure to a delinquent property whose foreclosure claim is dismissed.

We find robust evidence of foreclosure contagion. A completed foreclosure leads to about 0.5 to 0.70 more foreclosure filings per year within 0.1 miles and increases the probability of observing any neighboring foreclosure filing by about 10%. Moreover, a completed foreclosure causes between 0.25 and 0.5 new completed foreclosures per year. These contagion effects persist for at least four years after the case is decided.

Contagion is primarily driven by borrowers who are on the threshold of default. We find contagion among loans held by lenders who are known to automate foreclosure filings and are likely unresponsive to very local conditions, which we interpret as evidence that foreclosure contagion is driven by borrowers. Contagion is strongest not during the depths of the crisis,

49. Given our instrument, it is also conceivable that borrowers learn about the impact of a given judge on foreclosure outcomes. However, we do not expect that foreclosure contagion is driven by a completed foreclosure revealing information about a specific judge. Firstly, given the random assignment of judges to foreclosure cases, the probability that a given borrower ends up with a given judge is low—thus, the neighboring borrower's expectations about the outcome of default should not change substantially. Secondly, if learning about judges is the primary driver of foreclosure contagion, then we would not expect to observe differential contagion among individuals with the same lenders.

but at the end of the housing boom and beginning of the crash. Moreover, contagion is most prevalent among borrowers who are on the cusp of being underwater and not those who are severely underwater on their loans. We interpret this as evidence that a neighboring foreclosure has the greatest impact on borrowers who are on the margin of defaulting, and not those who are severely at risk. Finally, we find that contagion is minimal when borrowers have the same lender, perhaps because the neighboring foreclosure sends a signal about their lender's behavior, lowering the perceived probability of a successful renegotiation of the loan, thus reducing strategic incentives to default.

We find evidence that completed foreclosures disrupt local housing markets. After a completed foreclosure, the mean residential sale price dips by as much as 40%. However, this drop is largely explained by selection into sale—in the wake of a completed foreclosure, the composition of residential sales is skewed toward lower quality (and thus, lower price) homes. Nonetheless, we cannot rule out that foreclosures do influence the value of homes, conditional on quality.

While our instrumental-variables method provides clean identification of the effect of foreclosure, the resulting estimates are of a particular parameter that helps to inform foreclosure policy. Our estimates represent the effect of a completed foreclosure on the neighborhoods around properties that are most likely to be influenced by foreclosure judges. These are the cases that are most likely to be influenced by policy. At the same time, our estimates compare the neighborhood-level effect of a completed foreclosure relative to a delinquent mortgage that does not end in foreclosure. This is the relevant parameter for assessing policy that addresses how easily and how often lenders should be able to foreclose on delinquent borrowers. Finally, while there may be concerns about the external validity of our estimates, which are derived from a housing crisis in one of the worse-hit cities, it is in exactly such circumstances that policymakers and economists must worry most about foreclosure contagion.

In sum, our estimates of foreclosure contagion suggest room for policy that seeks alternative solutions for delinquent borrowers.

Bibliography

Abrams, D.S., Marianne Bertrand, and Sendhil Mullainathan. 2008. "Do Judges Vary in Their Treatment of Race?" *American Law and Economics Association Annual Meetings* 93.

Acemoglu, D. 2010. "When does labor scarcity encourage innovation?" *Journal of Political Economy* 118 (6): 1037–1078.

Acemoglu, D, Simon Johnson, and JA Robinson. 2001. "The colonial origins of comparative development: An empirical investigation." *American Economic Review* 91 (5): 1369–1401.

Acemoglu, D., and J. Robinson. 2006. *Economic Origins of Dictatorship and Democracy.* New York: Cambridge University Press.

———. 2012. *Why Nations Fail: The Origins of Power, Prosperity, and Poverty.* New York: Crown Publishers.

Acemoglu, Daron, Simon Johnson, Pablo Querubin, and James A. Robinson. 2008. "When Does Policy Reform Work?: The Case of Central Bank Independence." *Brookings Papers on Economic Activity,* NBER Working Papers Working Paper, no. 1:351–418.

Adelino, Manuel, Kristopher Gerardi, and PS Willen. 2009. "Why don't lenders renegotiate more home mortgages? redefaults, self-cures and securitization." *NBER Working Paper,* no. 15159.

Agarwal, Sumit, Gene Amromin, Itzhak Ben-David, Souphala Chomsisengphet, and Douglas D. Evanoff. 2011. "The role of securitization in mortgage renegotiation." *Journal of Financial Economics* 102, no. 3 (December): 559–578.

Alesina, Alberto, Nouriel Roubini, and Gerald D. Cohen. 1997. *Political Cycles & the Macroeconomy.* MIT Press.

Alston, LJ, and KD Kauffman. 1997. "Agricultural chutes and ladders: new estimates of sharecroppers and 'true tenants' in the South, 1900—1920." *The Journal of Economic History* 57 (2): 464–475.

Ammer, John, and Fang Cai. 2011. "Sovereign CDS and bond pricing dynamics in emerging markets: Does the cheapest-to-deliver option matter?" *Journal of International Financial Markets, Institutions and Money* 21, no. 3 (July): 369–387.

Anderson, J.M., J.R. Kling, and Kate Stith. 1999. "Measuring interjudge sentencing disparity: Before and after the federal sentencing guidelines." *Journal of Law and Economics* 42 (February): 271–307.

Anenberg, Eliot, and Edward Kung. Forthcoming. "Estimates of the size and source of price declines due to nearby foreclosures." *American Economic Review.*

Angrist, Joshua D, and Jörn-Steffen Pischke. 2008. *Mostly harmless econometrics: An empiricist's companion.* Princeton University Press.

Ashcraft, A., and Til Schuerman. 2008. "Understanding the Securitization of Subprime Mortgage Credit." *Federal Reserve Bank of New York Staff Report* May (No. 318.).

Atkinson, Doreen. 2007. *Going for broke: The fate of farm workers in arid South Africa.* Human Sciences Research Council.

Autor, DH, and SN Houseman. 2010. "Do temporary-help jobs improve labor market outcomes for low-skilled workers? Evidence from "Work First"." *American Economic Journal: Applied Economics* 2 (July): 96–128.

Baek, In-Mee, Arindam Bandopadhyaya, and Chan Du. 2005. "Determinants of market-assessed sovereign risk: Economic fundamentals or market risk appetite?" *Journal of International Money and Finance* 24, no. 4 (June): 533–548.

Baland, Jean-Marie, and James a Robinson. 2008. "Land and Power: Theory and Evidence from Chile." *American Economic Review* 98, no. 5 (November): 1737–1765.

Berdejo, Carlos, and Daniel L Chen. 2010. "Priming Ideology: Electoral Cycles Among Unelected Judges." *Working Paper,* no. January.

Bernanke, B. 2008. "Housing, Mortgage Markets, and Foreclosures." *Speech at the Federal Reserve System Conference on Housing and Mortgage Markets, Washington, D.C* 4 December 2008.

Besley, Timothy, and Anne Case. 1995. "Does electoral accountability affect economic policy choices? Evidence from gubernatorial term limits." *The Quarterly Journal of Economics* 110 (3): 769.

———. 2003. "Political Institutions and Policy Choices: Evidence from the United States." *Journal of Economic Literature* 41 (1): 7–73.

Bhardwaj, G., and R. Sengupta. 2008. "Where's the Smoking gun? A Study of Underwriting Standards for US Subprime Mortgages." *Working Paper.*

Bhardwaj, G., and R. Sengupta. 2009. "Did Prepayments Sustain the Subprime Market?"

Blanco, Roberto, and Simon Brennan. 2005. "An Empirical Analysis of the Dynamic Relation between Investment-Grade Bonds and Credit Default Swaps." *The Journal of Finance* LX (5).

Block, S., and Paul M. Vaaler. 2004. "The price of democracy: sovereign risk ratings, bond spreads and political business cycles in developing countries." *Journal of International Money and Finance* 23, no. 6 (October): 917–946.

Bolton, P., and Howard Rosenthal. 2002. "Political Intervention in Debt Contracts." *Journal of Political Economy* 110 (October).

Bonner, P., and K. Shapiro. 1987. "Company estate, company town: Pilgrim's Rest 1910-1932." *In: University of Witwatersrand. African Studies Seminar Paper* 223.

Borri, Nicola, and Adrien Verdelhan. 2011. "Sovereign risk premia." *AFA 2010 Atlanta Meetings Paper.*

Botha, S. 1996. "South Africa's Party System." *Journal of Theoretical Politics* 8, no. 2 (April): 209–225.

Bradford, Helen. 1990. "Getting Away with Slavery: Capitalist Farmers, Foreigners and Forced Labour in the Transvaal, c. 1920 - 1950." *University of Witwatersrand,* History Workshop.

Brender, A, and A Drazen. 2005. "Political budget cycles in new versus established democracies." *Journal of Monetary Economics* 52, no. 7 (October): 1271–1295.

Broner, F., G. Lorenzoni, and S.L. Schmukler. 2007. *Why do emerging economies borrow short term?*

Browning, L. 2007. "The Subprime Loan Machine." *The New York Times* March 23.

Bubb, R, and Alex Kaufman. 2009. "Securitization and Moral Hazard: Evidence from a Lender Cutoff Rule." *Working paper.*

Buchanan, J.M., and R.D. Congleton. 1994. "The incumbency dilemma and rent extraction by legislators." *Public Choice* 79 (1): 47–60.

Bucks, B., and Karen Pence. 2008. "Do Borrowers Know Their Mortgage Terms?" *Journal of Urban Economics* 64 (2): 8–33.

Cameron, Colin, Jonah B. Gelbach, and Douglas L. Miller. 2011. "Robust Inference With Multiway Clustering." *Journal of Business & Economic Statistics* 29, no. 2 (April): 238–249.

Campbell, John Y. 2013. "Mortgage Market Design." *Review of Finance* 17 (1): 1–33.

Campbell, JY, and JF Cocco. 2011. "A Model of Mortgage Default." *NBER Working Papers Working Paper,* NBER Working Papers Working Paper, no. 17516.

Campbell, J.Y., Stefano Giglio, and Parag Pathak. 2011. "Forced sales and house prices." *American Economic Review* 101 (5): 2108–31.

Carey, JM, and MS Shugart. 1995. "Incentives to cultivate a personal vote: A rank ordering of electoral formulas." *Electoral studies* 14 (4): 417–439.

Case, A., and A Deaton. 1999. "School Inputs and Educational Outcomes in South Africa." *Quarterly Journal of Economics* 114 (3): 1047–1084.

Case, K.E., and Robert Shiller. 2003. "Is there a bubble in the housing market?" *Brooking Papers on Economic Activity* 2.

Chang, Tom, and Antoinette Schoar. 2006. "The effect of judicial bias in Chapter 11 reorganization." *Unpublished manuscript. Massachusetts Institute of Technology.*

Charney, C. 1984. "Class conflict and the National Party split." *Journal of Southern African Studies* 10 (2): 269–282.

Chirwa, WC. 1996. "The Malawi Government and South African Labour Recruiters, 1974-92." *The Journal of Modern African Studies* 34 (4): 623–642.

Chomsisengphet, S., and Anthony Pennington-Cross. 2006. "The Evolution of the Subprime Mortgage Market." *Federal Reserve Bank of St. Louis Review* January.

Christopher, AJ. 1983. "Parliamentary delimitation in South Africa , 1910-1980." *Political Geography Quarterly* 2, no. 3 (July): 205–217.

Chuhan, Punam, Stijn Claessens, and Nlandu Mamingi. 1998. "Equity and bond flows to Latin America and Asia: the role of global and country factors." *Journal of Development Economics* 55:439–463.

Cline, WR, and KJS Barnes. 1997. *Spreads and risk in emerging markets lending.* Institute of International Finance.

Cobbe, James. 1986. "Consequences for Lesotho of changing South African labour demand." *African Affairs* 85 (338): 23–48.

Collin-Dufresne, Pierre, Robert S. Goldstein, and J. Spencer Martin. 2001. "The Determinants of Credit Spread Changes." *The Journal of Finance* 56, no. 6 (December): 2177–2207.

Cordell, L., K Dynan, A Lehnert, N Liang, and E Mauskopf. 2008. "The Incentives of Mortgage Servicers: Myths and Realities." *Federal Reserve Working Paper* 2008-46.

Corrado, Charles J. 2011. "Event studies: A methodology review." *Accounting & Finance* 51, no. 1 (March): 207–234.

Crankshaw, Owen. 1997. *Race, Class and the Changing Division of Labour under Apartheid.* London: Routledge.

Crush, J. 1993. "'The Long-Averted Clash': Farm Labour Competition in the South African Countryside." *Canadian Journal Of African Studies* 27 (3): 404–423.

Crush, J., and W. James. 1995. *Crossing Boundaries: Mine Migrancy in a Democratic South Africa.* Edited by Jonathan Crush and W James. IDASA / IDRC.

Crush, J., A. Jeeves, and D. Yudelman. 1991. *South Africa's Labor Empire: A History of Black Migrancy to the Gold Mines.* Boulder: Westview Press.

Currie, A. 2007. "Buy or Build: The Vertical Integrator's Dilemma." *the American Securitization Journal* Winter/Spring.

Dalle Nogare, Chiara, and Roberto Ricciuti. 2011. "Do term limits affect fiscal policy choices?" *European Journal of Political Economy* 27, no. 4 (December): 681–692.

Davies, Robert, David Kaplan, Mike Morris, and D. O'Meara. 1976. "Class struggle and the periodisation of the state in South Africa." *Review of African Political Economy* 3, no. 7 (September): 4–30.

De Klerk, M. 1984. "Seasons that will never return: the impact of farm mechanization on employment, incomes and population distribution in the Western Transvaal." *Journal of Southern African Studies.*

Dell, Melissa. 2010. "The Persistent Effects of Peru's Mining Mita." *Econometrica* 78 (6): 1863–1903.

Dell'Ariccia, G., D. Igan, and Luc Laeven. 2008. "Credit Booms and Lending Standards: Evidence from the Subprime Mortgage Market." *IMF Working Paper* April.

Demyanyk, Y. 2008. "Did Credit Scores Predict the Subprime Crisis?" *Federal Reserve Bank of St. Louis* October.

———. 2009. "Quick Exits of Subprime Mortgages." *St. Louis Review* March/April (91:2): 79–93.

Demyanyk, Y., and Otto Van Hemert. 2008. "Understanding the Subprime Mortgage Crisis." *Review of Financial Studies.*

Deng, Y, JM Quigley, and R Order. 2000. "Mortgage terminations, heterogeneity and the exercise of mortgage options." *Econometrica* 68 (2): 275–307.

DeSilver, D. 2009. "Reckless strategies doomed WaMu." *the Seattle Times* 25 October.

Diazweigel, D, and G Gemmill. 2006. "What drives credit risk in emerging markets? The roles of country fundamentals and market co-movements." *Journal of International Money and Finance* 25, no. 3 (April): 476–502.

Dick, A.R., and J.R. Lott. 1993. "Reconciling voters' behavior with legislative term limits." *Journal of Public Economics* 50 (1): 1–14.

Ding, Lei, Roberto G. Quercia, and Alan M. White. 2009. "State Anti-Predatory Lending laws: Impact and Federal Preemption Phase I Descriptive Analysis" (January).

Dobbie, Will, and Jae Song. 2013. "Debt Relief and Debtor Outcomes: Measuring the Effects of Consumer Bankruptcy Protection." *Revise and Resubmit at the American Economic Review.*

Domar, ED. 1970. "The causes of slavery or serfdom: a hypothesis." *The Journal of Economic History* 30 (1): 18–32.

Doyle, JJ. 2007. "Child protection and child outcomes: Measuring the effects of foster care." *The American Economic Review* 97 (5): 1583–1610.

Duffie, Darrell Darrell, L.H. Pedersen, and Kenneth J. Singleton. 2001. "Modeling Sovereign Yield Spreads: A Case Study of Russian Debt." *SSRN Electronic Journal.*

Duncan, David. 1995. *The Mills of God: The State and African Labour in South Africa, 1918-1948.* 2nd. Johannesburg: Witwatersrand University Press.

Dungey, Doris 'Tanta'. 2007. "The Compleat UberNerd." *Calculated Risk* at `http://www.calculatedriskblog.com/2007/07/compleat-ubernerd.html` (2): 337–388.

Eichengreen, Barry, and A. Mody. 1998. *What explains changing spreads on emerging-market debt: fundamentals or market sentiment?*

Elul, R. 2009. "Securitization and Mortgage Default: Reputation vs. Adverse Selection." *Federal Reserve Bank of Philadelphia.*

Evans, I. 1997. *Bureaucracy and Race: Native Administration in South Africa.* Berkeley: University of California Press.

Favero, Carlo, Marco Pagano, and Ernst-Ludwig von Thadden. 2009. "How Does Liquidity Affect Government Bond Yields?" *Journal of Financial and Quantitative Analysis* 45, no. 01 (November): 107.

Fernandez-Arias, Eduardo. 1996. "The new wave of private capital inflows: Push or pull?" *Journal of Development Economics* 48, no. 2 (March): 389–418.

Foote, Christopher L., Kristopher Gerardi, and Paul S. Willen. 2008. "Negative equity and foreclosure: Theory and evidence." *Journal of Urban Economics* 64, no. 2 (September): 234–245.

Foote, Christopher, K. Gerardi, L. Goette, and Paul Willen. 2009. *Reducing foreclosures: No easy answers.* Technical report. National Bureau of Economic Research.

Forbes, S.J., and Mara Lederman. 2009. "Does Vertical Integration Affect Firm Performance? Evidence from the Airline Industry." *July.*

Frame, Scott, Andreas Lenhert, and Ned Prescott. 2008. "A Snapshot of Mortgage Conditions with an Emphasis on Subprime Mortgage Performance."

Frame, WS. 2010. "Estimating the effect of mortgage foreclosures on nearby property values: A critical review of the literature." *Economic Review, Federal Reserve Bank of Atlanta,* no. 3.

Frot, Emmanuel, and J. Santiso. 2010. "Portfolio Managers and Elections in Emerging Economies: How investors dislike political uncertainty."

Garcia-Herrero, Alicia, and Alvaro Ortiz. 2006. "The Role of Global Risk aversion in explaining Latin American Sovereign Spreads." May.

Garriga, C. 2009. "Lending Standards in Mortgage Markets." *Federal Reserve Bank of St. Louis Economic Synopses* May.

Geanakopolos, J., and Koniak. 2009. "Matters of Principal." *New York Times* 5 March.

Gelpern, A, and Mitu Gulati. 2007. "Public Symbol in Private Contract: A Case Study." *Washington University Law Reviews* 84 (7): 1627–1715.

Gelpern, Anna, and Adam Levitin. 2009. "Rewriting frankenstein contracts: The workout prohibition in residential mortgage-backed securities." *Southern California Law Review* 82:1077–1152.

Gerardi, Kristopher, Eric Rosenblatt, PS Willen, and Vincent W Yao. 2012. "Foreclosure externalities: Some new evidence." *Federal Reserve Bank of Atlanta,* no. Working Paper 12.

Ghent, Andra C, and Marianna Kudlyak. 2011. "Recourse and residential mortgage default: Evidence from US states." *Review of Financial Studies* 24 (9): 3139–3186.

Giliomee, Hermann. 1995. "Democratization in South Africa." *Political Science Quarterly* 110 (1): 83–104.

Giliomee, Hermann, and L Schlemmer, eds. 1985. *Up against the fences.* New York: St Martin's Press.

Glaeser, EL. 1997. "Self-imposed term limits." *Public Choice* 93 (3): 389–394.

Glaeser, EL, and B Sacerdote. 2000. "The Social Consequences of Housing." *Journal of Housing Economics* 3:1–23.

Goodman, P., and Gretchen Morgenson. 2008. "Saying Yes, WaMu Built Empire on Shaky Loans." *New York Times* 27 December.

Goodstein, R, and Y Lee. 2010. "Do Foreclosures Increase Crime?" *Available at SSRN 1670842.*

Goodstein, Ryan, Paul E. Hanouna, Carlos D. Ramirez, and Christof W. Stahel. 2011. "Are Foreclosures Contagious?" *SSRN Electronic Journal:*1–34.

Gorton, G. 2009. "Slapped in the Face by the Invisible Hand: Banking and the Panic of 2007." *Prepared for Federal Reserve Bank of Atlanta's Financial Markets Conference: Financial Innovation and Crisis* May 11- 13.

Gramlich, E. 2007. "Booms and Busts: The Case of Subprime Mortgages." *Federal Reserve Bank of Kansas City Economic Review,* no. September:105–113.

Greenberg, Stanley. 1980. *Race and State in Capitalist Development: South Africa in Comparative Perspective.* Yale University Press.

Greif, Avner, and David D. Laitin. 2004. "A Theory of Endogenous Institutional Change." *The American Political Science Review* 98 (4): 633–652.

Groemping, U. 2006. "Relative importance for linear regression in R: the package relaimpo." *Journal of statistical software* 17 (1): 1.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales. 2013. "The Determinants of Attitudes toward Strategic Default on Mortgages." *The Journal of Finance* 68 (4): 1473–1515.

Guren, Adam, and Tim McQuade. 2013. "How Do Foreclosures Exacerbate Housing Downturns?" April.

Habakkuk, H.J. 1962. *American and British Technology in the Nineteenth Century.* London: Cambridge University Press.

Harding, John P. J.P., Eric Rosenblatt, and Vincent W. V.W. Yao. 2009. "The contagion effect of foreclosed properties." *Journal of Urban Economics* 66, no. 3 (November): 164–178.

Hayek, FA. 1979. "The Political Order of a Free People. Vol. 3 of Law, Legislation, and Liberty." *London and.*

Henderson, N. 2005. "Bernanke: There's no housing bubble to go bust: Fed Nominee has said 'cooling' won't hurt." *Washington Post,* no. 27 October.

Hilscher, J., and Y. Nosbusch. 2010. "Determinants of Sovereign Risk: Macroeconomic Fundamentals and the Pricing of Sovereign Debt." *Review of Finance* 14, no. 2 (March): 235–262.

Himmelberg, C., Christopher Mayer, and T. Sinai. 2005. "Assessing High House Prices: Bubbles, Fundamentals, and Misperceptions." *Federal Reserve Bank of New York Staff Reports,* no. 218.

Hornbeck, R., and S. Naidu. 2013. "When the Levee Breaks: Labor Mobility and Economic Development in the American South." *American Economic Review (forthcoming).*

Horner, D, and A Kooy. 1980. "Conflict on South African mines, 1972-1979." *SALDRU Working Paper,* no. 29:1972–1979.

Hund, John, and D. Lesmond. 2008. "Liquidity and credit risk in emerging debt markets." *Traders.*

Hunt, J. P. 2009. "What do subprime securitization contracts actually say about loan modification?" *Berkeley Center for Law, Business and the Economy* working paper.

Iaryczower, M. 2009. "The Value of Information in the Court—Get it Right, Keep it Tight." *American Economic Review* 102 (1): 202–237.

Immergluck, Dan, and G. Smith. 2006. "The external costs of foreclosure: The impact of single-family mortgage foreclosures on property values." *Housing Policy Debate* 17 (1): 57–80.

Johnson, J.M., and W.M. Crain. 2004. "Effects of term limits on fiscal performance: Evidence from democratic nations." *Public Choice* 119 (1): 73–90.

Katz, A. 2006. "Prime Suspect." *Mother Jones,* no. September.

———. 2009. "The Loan Ranger." *Slate: the Big Money* 17 February.

Kaufmann, Daniel, A Kraay, and M Mastruzzi. 2010. "The worldwide governance indicators: methodology and analytical issues."

Keys, B., Tanmoy Mukherjee, Amit Seru, and Vikrant Vig. 2010. "Did Securitization Lead to Lax Screening? Evidence from Subprime Loans." *Quarterly Journal of Economics* 125 (1).

Kiel, Paul. 2012. "The American Foreclosure Story: The Struggle for Justice and a Place to Call Home." *ProPublica* (April).

Kim, Gloria M. 2004. *Emerging Markets Bond Index Plus (EMBI+).* Technical report December. JP Morgan.

Kling, JR. 2006. "Incarceration length, employment, and earnings." *American Economic Review* 96 (3): 863–876.

Kregel, J. 2008. "Changes in the U.S. Financial System and the Subprime Crisis." *Levy Economics Institute Working Paper,* no. April.

Kroszner, Randall S. 1998. "Is it Better to Forgive than to Receive? Repudiation of the Gold Indexation Clause in Long-Term Debt During the Great Depression." *Graduate School of Business, University of Chicago.*

Krugman, P. 2005. "That hissing sound." *New York Times,* no. August.

Lacey, Marian. 1981. *Working for boroko: The origins of a coercive labour system in South Africa.* Johannesburg: Ravan Press (Pty) Limited.

Lemon, Anthony. 1984. "State Control over the Labor Market in South Africa." *International Political Science Review* 5 (2): 189–208.

Leonard, Tammy, and James C. Murdoch. 2009. "The neighborhood effects of foreclosure." *Journal of Geographical Systems* 11, no. 4 (May): 317–332.

Levitin, Adam. 2008. "Written testimony of Adam J. Levitin." *Hearing on H.R. 200 and H.R. 225 Hearing before H. Comm. on Judiciary, 110th Cong, 23* 22 January.

———. 2009a. "Helping homeowners: Modification of mortgages in bankruptcy." *Harvard Law & Policy Review Online* 3.

Levitin, Adam. 2009b. "Helping Homeowners: Modification of Mortgages in Bankruptcy." *3. Harv. L. & Pol'y Rev. (online)* Jan. 19.

———. 2009c. "Resolving the Foreclosure Crisis: Modification of Mortgages in Bankruptcy." *WISC. L. REV. 565.*

———. 2009d. "The Worsening Foreclosure Crisis: Is It Time to Reconsider Bankruptcy Reform?" *Written Testimony for the Senate Committee on the Judiciary Subcommittee on Administrative Oversight and the Courts* 23 July.

Levitin, Adam, and Tara Twomey. 2011. "Mortgage Servicing." *Yale J. on Reg.* No. 11.

Lin, Zhenguo, Eric Rosenblatt, and Vincent W. Yao. 2007. "Spillover Effects of Foreclosures on Neighborhood Property Values." *The Journal of Real Estate Finance and Economics* 38, no. 4 (November): 387–407.

List, J. A., and Daniel M. Sturm. 2006. "How Elections Matter: Theory and Evidence from Environmental Policy." *The Quarterly Journal of Economics* 121, no. 4 (November): 1249–1281.

Lombard, J.A., and J.J. Stadler. 1980. *The Role of Mining in the South African Economy: A Study prepared for the Chamber of Mines of South Africa.* Bureau of Economic Policy / Analysis, University of Pretoria.

Longstaff, F.A., J. Pan, L.H. Pedersen, and K.J. Singleton. 2007. *How Sovereign is Sovereign Credit Risk?*

Lopez, Economic Journal. 2003. "Term limits: Causes and consequences." *Public Choice* 114 (1): 1–56.

Lowenberg, Anton D. 1997. "Why South Africa's Apartheid Economy Failed." *Contemporary Economic Policy* 15, no. 3 (July): 62–72.

Lowenstein, R. 2010. "Walk away from your Mortgage!" *New York Times* 7 January.

MacKenzie, D. 2009. "The Credit Crisis as a Problem in the Sociology of Knowledge."

Maestas, Nicole, Kathleen Mullen, and Alexander Strand. 2013. "Does Disability Insurance Receipt Discourage Work? Using Examiner Assignment to Estimate Causal Effects of SSDI Receipt." *American Economic Review* 103 (5): 1797–1829.

Magruder, JR. 2012. "High unemployment yet few small firms: The role of centralized bargaining in South Africa." *American Economic Journal: Applied Economics* 4 (3): 138–66.

Mariotti, M. 2010. "Estimating the elasticity of substitution in the South African manufacturing industry during Apartheid, 1950 — 1985." *Economic History of Developing Regions* 27 (2): 47–60.

Martell, R. 2007. "Understanding Common Factors in Domestic and International Bond Spreads." *Review of Finance* 12, no. 2 (March): 365–389.

Mauro, P., N. Sussman, and Y. Yafeh. 2002. "Emerging market spreads: then versus now." *The Quarterly Journal of Economics* 117 (2): 695.

Mayer, Christopher, Edward Morrison, Tomasz Piskorski, and Arpit Gupta. 2011. "Mortgage Modification and Strategic Default: Evidence from a Legal Settlement with Countrywide." *NBER Working Papers Working Paper.*

Mayer, Christopher, Karen Pence, and Shane Sherlund. 2009. "The Rise in Mortgage Defaults." *Journal of Economic Perspectives* 23 (1): 23–50.

McGuire, P., and M.A. Schrijvers. 2003. *Common factors in emerging market spreads.* Technical report December. Bank of International Settlements.

Mian, Atif, and Amir Sufi. 1999. "House Prices, Home Equity-Based Borrowing, and the U.S. Household Leverage Crisis." *NBER Working Paper* 15283.

———. 2009. "The consequences of mortgage credit expansion: Evidence from the US mortgage default crisis." *The Quarterly Journal of Economics* 124 (4): 1449.

Mian, Atif, Amir Sufi, and Francesco Trebbi. 2009. "The Political Economy of the US Mortgage Default Crisis." *papers.ssrn.com,* no. January.

———. 2012. "Foreclosures, House Prices, and the Real Economy." *Chicago Booth Research Paper,* nos. 13-41.

Moore, Barrington. 1966. *Social origins of dictatorship and democracy: lord and peasant in the making of the modern world.* Boston: Beacon Press.

Morrell, Robert. 1988. "The Disintegration of the Gold and Maize Alliance in South Africa in the 1920s." *The International journal of African historical studies* 21 (4): 619–635.

Morris, Stephen. 2001. "Political Correctness." *Journal of Political Economy* 109, no. 2 (April): 231–265.

Moser, Christoph. 2007. *The Impact of Political Risk on Sovereign Bond Spreads. Evidence from Latin America.*

Moser, Christoph, and A. Dreher. 2010. "Do Markets Care About Central Bank Governor Changes? Evidence from Emerging Markets." *Journal of Money, Credit and Banking* 42 (8): 1589–1612.

Mueller-Smith, Michael. 2013. "Program Evaluation with Randomized Screeners: Estimating Heterogenous Response Instrumental Variable (HRIV) Models."

Naidu, S., S. Turban, and L. Wilse-Samson. 2014. "Labor Conflict and the Economic Incidence of Institutional Change: Evidence from South Africa."

Nohlen, Dieter. 1999. "Elections in Africa, A Data Handbook — South Africa":1–54.

Nordhaus, W.D. 1975. "The political business cycle." *The Review of Economic Studies* 42 (2): 169–190.

Norris, F. 2009. "Why Many Home Loan Modifications Fail." *New York Times* 3 December.

O'Meara, Dan. 1996. *Forty Lost Years: The Apartheid State and the Politics of the National Party, 1948-1994.* Ravan Press.

Onselen, C Van. 1996. *The seed is mine : the life of Kas Maine, a South African sharecropper, 1894-1985.* New York: Hill / Wang.

Panel, Congressional Oversight. 2009. "The Foreclosure Crisis: Working Toward a Solution." *Journal of Urban Economics.*

Pemstein, D., S. a. Meserve, and J. Melton. 2010. "Democratic Compromise: A Latent Variable Analysis of Ten Measures of Regime Type." *Political Analysis* 18, no. 4 (August): 426–449.

Pence, Karen. 2006. "Foreclosing on opportunity: State laws and mortgage credit." *Review of Economics and Statistics* 88 (1): 177–182.

Pennington-Cross, A. 2006. "The value of foreclosed property." *Journal of Real Estate Research* 28 (2): 193–214.

Persson, Torsten, and Guido Enrico Tabellini. 2000. *Political Economics: Explaining Economic Policy.* 533. MIT Press.

Pickles, J. 1988. "Recent changes in regional policy in South Africa." *Geography* 73 (3): 233–239.

Poon, M. 2008. "From New Deal Institutions to Capital Markets: Commercial consumer risk scores and the making of subprime mortgage finance." *Accounting, Organizations and Society.*

Price, TW. 1953. "The South African General Election, 1953." *Parliamentary Affairs* 6 (3): 258.

Querubin, Pablo. 2011. "Political reform and elite persistence: Term limits and political dynasties in the Philippines." *Harvard Academy for International and Area Studies.*

Rajan, Uday, Amit Seru, and Vikrant Vig. 2010. "The Failure of Models that Predict Failure: Distance, Incentives and Defaults." *Chicago GSB Research Paper* No. 08-19.

Reed, W.R., D.E. Schansberg, James Wilbanks, and Z. Zhu. 1998. "The relationship between congressional spending and tenure with an application to term limits." *Public Choice* 94 (1): 85–104.

Reiss, D.J. 2009. "Regulation of Subprime and Predatory Lending." *Brooklyn Law School Legal Studies, Research Papers* No.142.

Remolona, E., M. Scatigna, and Eliza Wu. 2007. *Interpreting sovereign spreads.* Technical report March. Bureau of International Settlements.

Robinson, B. 2009. "An Overview of the Home Affordable Modification Program." *Philadelphia Fed* Consumer Compliance Outlook, Third Quarter.

Romer, T, and Barry Weingast. 1992. "Political foundations of the thrift debacle." *in J. Barth and D. Brumbaugh, Eds: The Reform of Federal Deposit Insurance* 167-202.

Rooyen, Johann Van. 1994. *Hard Right: The New White Power in South Africa.* Cape Town: David Phillip Publishers.

Sandler, DH, and Ryan Sandler. 2012. "Multiple Event Studies in Public Finance: A Simulation Study with Applications."

Savage, Michael. 1986. "The Imposition of Pass Laws on the African Population in South Africa 1916-1984." *African Affairs* 85 (339): 181–205.

Schirmer, Stefan. 2004. "Motives for mechanisation in South African agriculture, 1940—1980." *African Studies* 63, no. 1 (July): 3–28.

Schoeman, B.M. 1977. *Parlementere verkiesings in Suid-Afrika 1910-1976.* Pretoria: Aktuele Publikasies.

Schuetz, Jenny, Vicki Been, and Ingrid Gould IG Ellen. 2008. "Neighboring Effects of Concentrated Mortgage Foreclosures." *Journal of Housing Economics,* no. 212:–36.

Segal, I. 1999. "Contracting with Externalities." *Quarterly Journal of Economics* 114 (2): 337–388.

Shefter, M. 1977. "Party and Patronage: Germany, England, and Italy." *Politics & Society* 7, no. 4 (January): 403–451.

Shiller, R. 2008. *The Subprime Solution.* Princeton: Princeton University Press.

Siebert, WS. 1987. "Black trade unions and the wage gap in South Africa." *Managerial and Decision Economics* 8 (1): 55–65.

Skeel, D. 2001. *Debt's Dominion: A History of Bankruptcy Law in America.* Princeton: Princeton University Press.

Smart, Michael, and D. Sturm. 2006. "Term limits and electoral accountability." May.

Smith, S., L. Perwien, and J. Ratcliffe. 2009. "Mortgage Servicer Response to Borrowers in Crisis: A Report from the Front Lines." *Center for Community Capital, UNC, Working Paper* 14 September.

Sorkin, Andrew Ross. 2009. *Too Big to Fail.* London: Viking Penguin.

Spandau, A. 1980. "Mechanization and labour policies on South African mines." *South African Journal of Economics* 40 (1): 110–120.

Steinfeld, Robert J. 2001. *Coercion, Contract, and Free Labor in the Nineteenth Cen- tury.* Cambridge, UK: Cambridge University Press.

Strapparava, Carlo, and Rada Mihalcea. 2010. "Annotating and Identifying Emotions in Text." *Intelligent Information Access:*21–38.

Tabarrok, Alexander. 1994. "A Survey, Critque, and New Defense of Term Limits." *The Cato Journal* 14 (2).

Temple, R. 2009. "How to Underpin Tottering House Prices." *the American Securitization Journal* May.

Tett, G. 2009. *Fool's Gold.* London: Little Brown.

The Surplus People Project. 1983. *Forced Removals in South Africa: Vols 1-5.* Cape Town: Surplus People Project.

Towe, Charles, and Chad Lawley. 2013. "The Contagion Effect of Neighboring Foreclosures." *American Economic Journal: Economic Policy* 5 (2): 313–335.

Tung, F. 2009. "The Great Bailout of 2008—09." *Emory Bankruptcy Developments Journal* 25 (2).

Uribe, M, and V Yue. 2006. "Country spreads and emerging countries: Who drives whom?" *Journal of International Economics* 69, no. 1 (June): 6–36.

Westphalen, Michael. 2001. "The determinants of sovereign bond credit spreads changes." *unpublished paper, Universite de Lausanne:*1–27.

White, B. 2009. "Underwater and not walking away: Shame, Fear, and the Social Management of the Housing Crisis." *Arizona Legal Studies Discussion Paper* No 09-35.

White, Michelle. 2009. "Talk at the National Conference of Bankruptcy Judges." *Las Vegas, October.*

Wilcox, J. 2008. "House Price Dynamics." *Synopses of Selected Research on Housing, Mortgages and Foreclosures, Federal Reserve.*

Wilson, F. 1972. *Labour in the South African Gold Mines, 1911-1969.*

Wooldridge, P, and Dietrich Domanski. 2003. *Changing links between mature and emerging financial markets.* Technical report September. Bank of International Settlements.

Yang, Crystal S. 2012. "Free At Last? Judicial Discretion and Racial Disparities in Post Booker Sentencing." *Working Paper.*

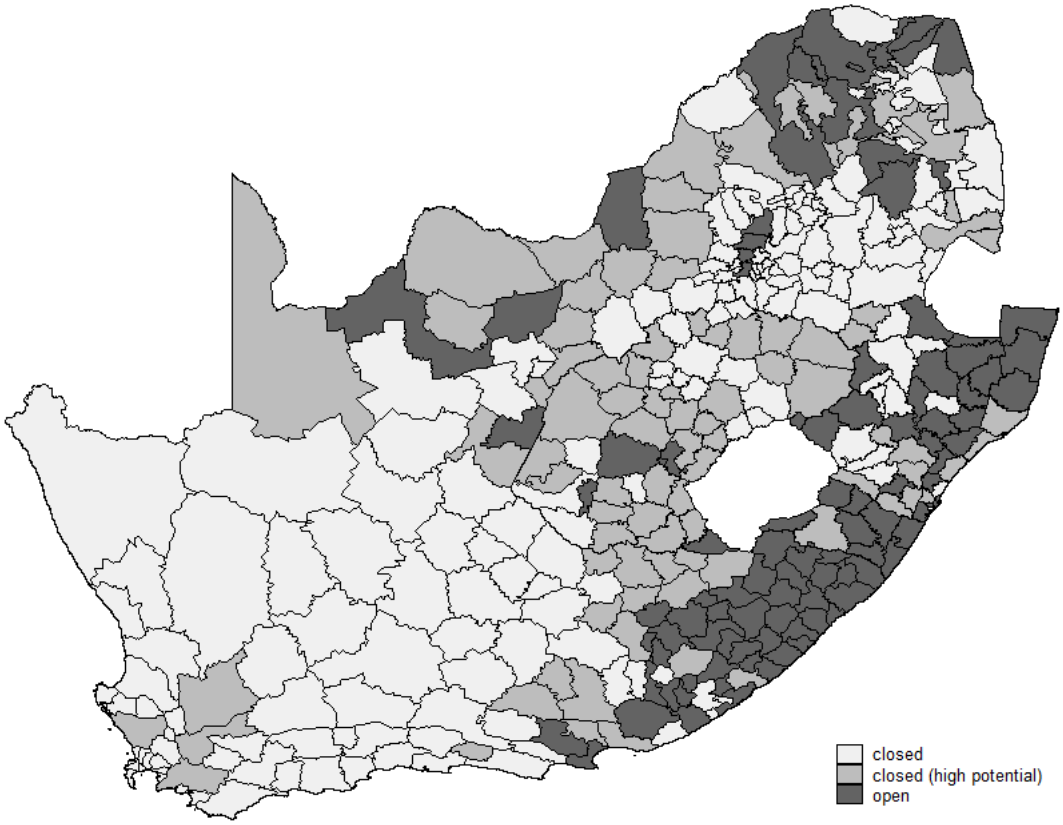
Zhu, Haibin. 2006. "An Empirical Comparison of Credit Spreads between the Bond Market and the Credit Default Swap Market." *Journal of Financial Services Research* 29, no. 3 (May): 211–235.

Appendices

Appendix A

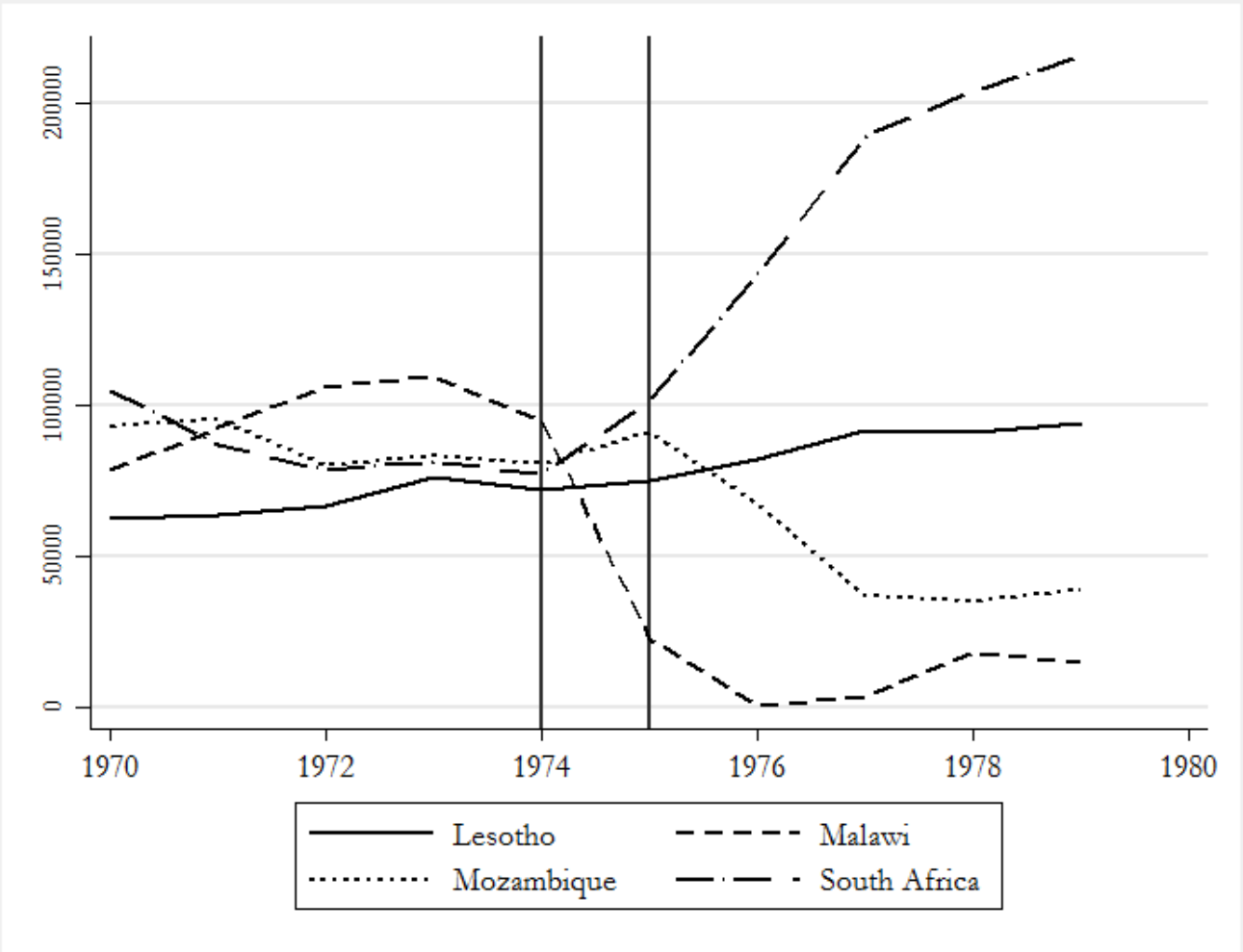
Appendix for Chapter 1

Figure A.1: Open and closed districts (Source: Crush (1993).)



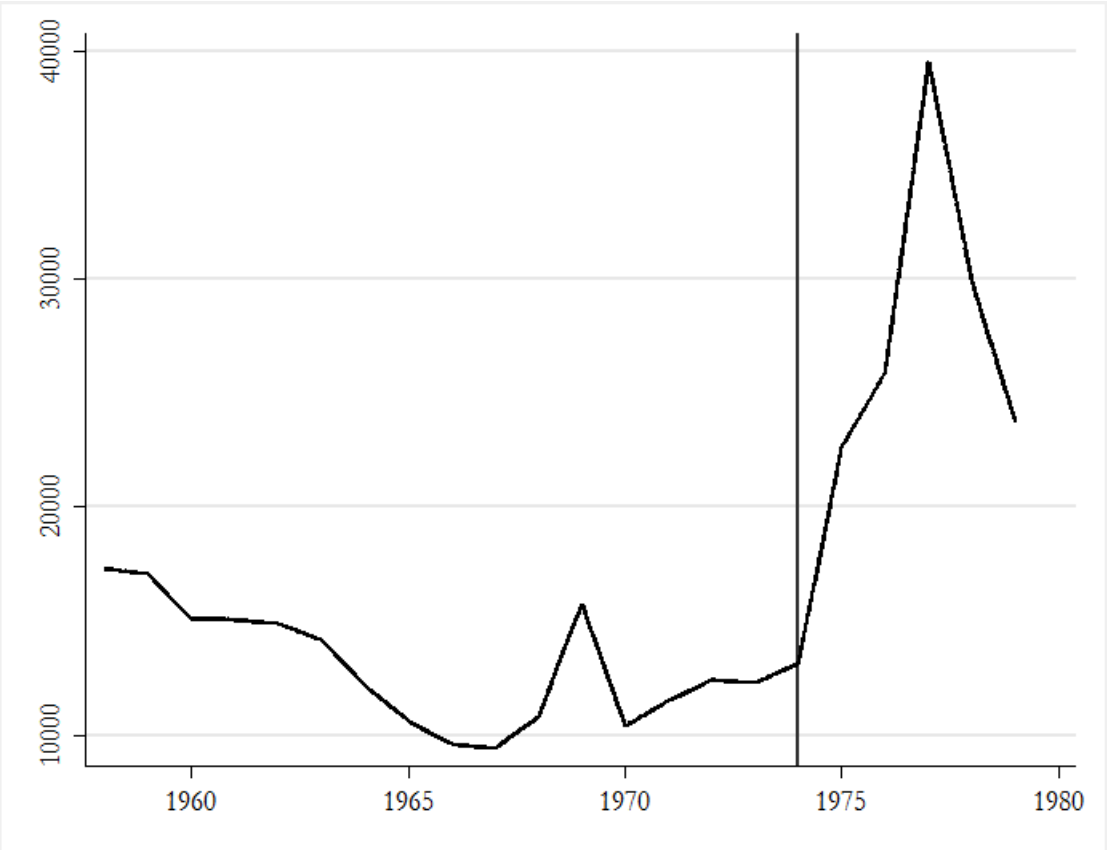
Notes: I georeferenced the map from Crush (1993) using Map Warper (`http://mapwarper.net/`). I intersected it with the district map shapefile to obtain the set of open/closed/high potential districts. I checked the resulting set of open districts against lists of recruiter licenses (issued under the 1911 Native Labour Recruiting Act) I was able to locate in the TEBA archive at the University of Johannesburg as well as the station level recruiting data available there. I checked the list of high potential districts against TEBA's 1977 memo "Recruiting in South Africa".

Figure A.2: Mining Recruits by Country



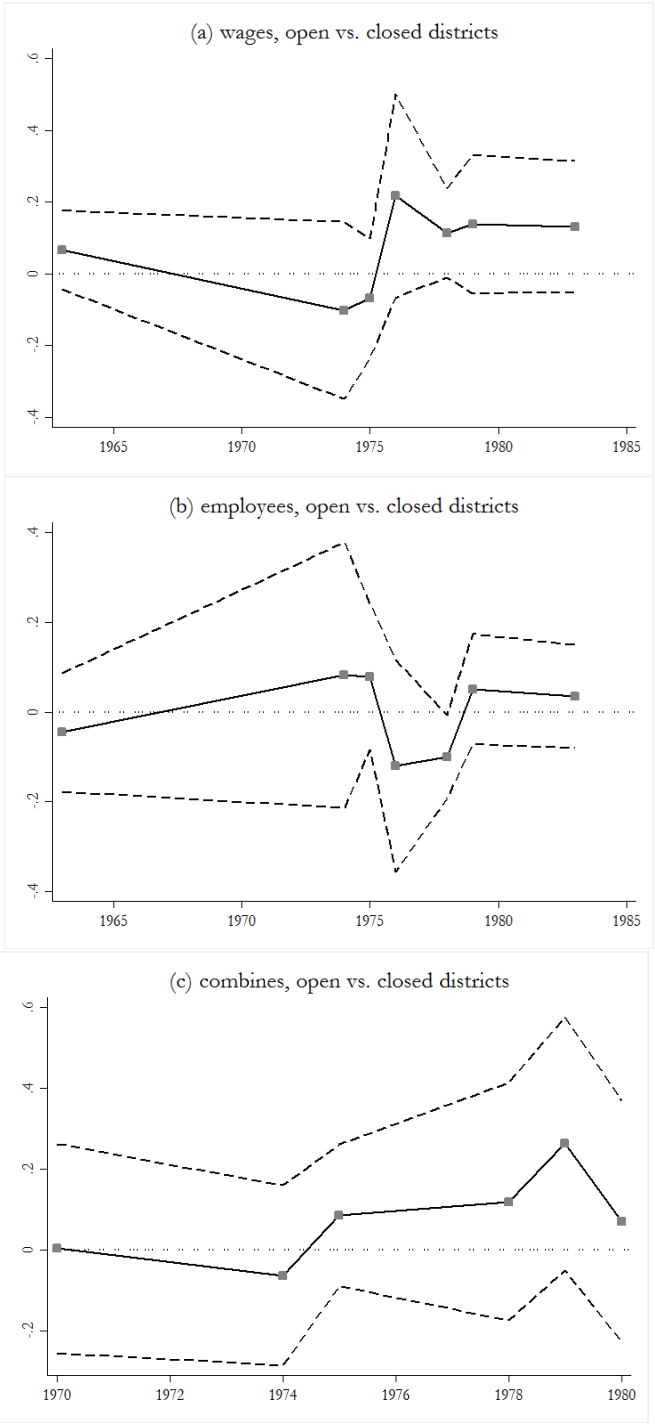
Notes: Data are drawn from the Annual Reports of the Chamber of Mines. Graphed are the series for the 4 major source countries. Not shown are Botswana, Swaziland, Zimbabwe and 'others'.

Figure A.3: Median Mining Recruits at South African Recruiting Stations



Notes: Graph shows the median number of recruits from NRC regional offices in South Africa forwarded to the mines. The data was compiled from the TEBA archive at the University of Johannesburg.

Figure A.4: Adjustment Dynamics



Note: The coefficients are graphed according to estimating Equation 1.5. Confidence intervals correspond to 5% bands. Outcomes are for black regular farm workers and are relative to 1972. Data are from the Censuses of Agriculture 1963-1983.

Table A.1: Difference open v closed, 1972

Panel A: Baseline comparison						
	(1)	(2)	(3)	(4)	(5)	(6)
	Farm size	Wages per worker	Farm workers	Tractors	Combines	Ag incomes
Open	-0.459**	-0.284***	0.569**	-0.201	-0.656***	-0.218***
	(0.192)	(0.109)	(0.221)	(0.143)	(0.180)	(0.0705)
Observations	255	250	256	240	240	294
R^2	0.024	0.047	0.020	0.008	0.044	0.052
Panel B: Comparison including economic region fixed effects						
	(1)	(2)	(3)	(4)	(5)	(6)
	Farm size	Wages per worker	Farm workers	Tractors	Combines	Ag incomes
Open	-0.0516	-0.0578	-0.127	0.0410	-0.0774	-0.141**
	(0.169)	(0.0637)	(0.226)	(0.156)	(0.254)	(0.0676)
Observations	255	250	256	240	240	294

Robust standard errors. * p < 0.10, ** p < 0.05, *** p < 0.01

Note: 'Open' is a dummy for whether the district was open to mine recruiting at 1970. The regressions control for maize production in 1963 and year interactions, and weights for the 1960 black rural population. The regression controls for economic region fixed effects. Errors are clustered at the district level.

Table A.2: Estimated differences in Wages and Workforce by Open status

Panel A: Wages and Workers

	(1) Per Cap Wage	(2) Black Employees	(3) Wage Bill
Post × Open	0.152**	-0.0868*	0.0646
	(0.0723)	(0.0507)	(0.0658)
Observations	1738	1749	1753
R^2	0.924	0.973	0.979

Panel B: Combines and Tractors

	(1) Combines	(2) Power Combines	(3) All tractors
Post × Open	0.172*	0.137	0.0170
	(0.0938)	(0.131)	(0.0264)
Observations	1487	1060	1708
R^2	0.969	0.958	0.989

Note: Errors clustered at district level. * p < 0.10, ** p < 0.05, *** p < 0.01. All regressions have log outcomes. Outcomes for Panel A are for black regular employees on farms. Post × Open is a dummy for whether the district was open to mine recruiting in 1970 interacted with the years 1976 to 1979. Each regression includes district and economic region × year fixed effects, and year by maize production in 1963 interactions. Errors are clustered at the district level. Outcomes are relative to 1972.

Table A.3: Estimated differences in Wages and Workforce by Open status, relative to 1972.

	(1) Per Cap Wage	(2) Black Employees	(3) Wage Bill
1963 × Open	0.0669	-0.0456	0.0263
	(0.0547)	(0.0662)	(0.0844)
1974 × Open	-0.102	0.0824	-0.0179
	(0.124)	(0.148)	(0.0827)
1975 × Open	-0.0680	0.0792	0.0162
	(0.0832)	(0.0815)	(0.0648)
1976 × Open	0.217	-0.120	0.101
	(0.142)	(0.118)	(0.111)
1978 × Open	0.114*	-0.100**	0.0171
	(0.0621)	(0.0465)	(0.0725)
1979 × Open	0.139	0.0512	0.190*
	(0.0965)	(0.0617)	(0.114)
1983 × Open	0.131	0.0344	0.168*
	(0.0916)	(0.0575)	(0.101)
Observations	1738	1749	1753
R^2	0.925	0.973	0.979

Errors clustered at district level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Note: All regressions are log outcomes for black regular employees on farms. Open is a dummy for whether the district was open to mine recruiting in 1970. Each regression includes district and economic region × year fixed effects, and year by maize production in 1963 interactions. Errors are clustered at the district level. Outcomes are relative to 1972.

Table A.4: Estimated differences in Combines and Tractors by Open status, relative to 1972

	(1) Combines	(2) Power Combines	(3) All tractors
1970 × Open	0.00320		0.00884
	(0.130)		(0.0407)
1974 × Open	-0.0634	-0.143	-0.0133
	(0.111)	(0.139)	(0.0280)
1975 × Open	0.0856	-0.0447	0.0159
	(0.0877)	(0.197)	(0.0310)
1978 × Open	0.120	0.00442	-0.0142
	(0.147)	(0.170)	(0.0447)
1979 × Open	0.263*	0.141	0.0740
	(0.157)	(0.232)	(0.0520)
1980 × Open	0.0707		-0.00393
	(0.150)		(0.0519)
Observations	1487	1060	1708
R^2	0.969	0.958	0.989

Errors clustered at district level. * p < 0.10, ** p < 0.05, *** p < 0.01

Note: All regressions are log outcomes. Open is a dummy for whether the district was open to mine recruiting in 1970. Each regression includes district and economic region × year fixed effects, and year by maize production in 1963 interactions. Errors are clustered at the district level. Outcomes are relative to 1972.

Table A.5: Estimated differences in Wages by Open status: Robustness to alternative specifications

Panel A: Wages per worker				
	(1) Baseline	(2) Tenancy end	(3) Ag incomes	(4) ex. TBVC
Post × Open	0.152**	0.154**	0.158**	0.152**
	(0.0723)	(0.0727)	(0.0733)	(0.0721)
Observations	1738	1738	1725	1718
R^2	0.924	0.925	0.924	0.923
Panel B: Employment				
	(1) Baseline	(2) Tenancy end	(3) Ag incomes	(4) ex. TBVC
Post × Open	-0.0868*	-0.0848*	-0.0840	-0.0868*
	(0.0507)	(0.0505)	(0.0515)	(0.0505)
Observations	1749	1749	1736	1729
R^2	0.973	0.973	0.971	0.970
Panel C: Combines				
	(1) Baseline	(2) Tenancy end	(3) Ag incomes	(4) ex TBVC
Post × Open	0.172*	0.170*	0.170*	0.172*
	(0.0938)	(0.0917)	(0.0927)	(0.0936)
Observations	1487	1487	1483	1482
R^2	0.969	0.969	0.968	0.968

Note: Errors clustered at district level. * p < 0.10, ** p < 0.05, *** p < 0.01. All regressions are log outcomes for black regular employees on farms. The 'treatment', is an interaction of open status with the years 1976 to 1979. Each regression includes district and economic region × year fixed effects. Column (1) is our baseline specification. Column (2) includes year interactions with a dummy for tenancy 'reform' as set out in appendix A.3. Column (3) for the level of white agricultural incomes in 1970 and year interactions. Column (4) runs the baseline specification excluding districts within Transkei, Bophutatswana, Venda or Ciskei. Errors are clustered at the district level. Outcomes are relative to 1972.

Table A.6: Placebo: Estimated differences in Wages and Workforce by High potential status, relative to 1972.

Panel A: Wages and Workers

	(1) Per Cap Wage	(2) Black Employees	(3) Wage Bill
Post × highpot	-0.0563	-0.00311	-0.0596
	(0.0406)	(0.0293)	(0.0414)
Observations	1738	1749	1753
R^2	0.923	0.972	0.979

Panel B: Combines and Tractors

	(1) Combines	(2) Power Combines	(3) Tractors
Post × highpot	-0.0678	-0.105	-0.0197
	(0.0562)	(0.101)	(0.0194)
Observations	1487	1060	1708
R^2	0.968	0.958	0.989

Note: Errors clustered at district level. * p < 0.10, ** p < 0.05, *** p < 0.01. All regressions are log outcomes. The 'treatment', is an interaction of 'high potential for recruiting' status with the years 1976 to 1979. High potential status is as defined by the mine labor recruiter. Each regression includes district and economic region × year fixed effects, maize suitability fixed effects, and weights by black rural population. Errors are clustered at the district level. Outcomes are relative to 1972.

Table A.7: Mine-level regressions.

	(1) Ore mined	(2) Number of Strikes	(3) # Injured or killed
Post $\times MM67$	-0.0128**	0.00639	0.197*
	(0.00628)	(0.00520)	(0.102)
Observations	450	416	416
R^2	0.768	0.274	0.204

Note: Standard errors clustered at gold mine level. * p < 0.10, ** p < 0.05, *** p < 0.01. All regressions are log outcomes. The 'treatment', Post $\times MM67$, is the share of workers at that mine in 1967 originating from Mozambique or Malawi interacted with a dummy for the years 1976–1979. The outcome in the first column is the tons of ore mined (in 000s). The second column outcome is a count of the number of strikes at that mine, the third is the total number injured or killed in strikes at that mine. Each regression includes mine and year fixed effects. Outcomes are relative to 1973.

Table A.8: Election outcomes.

Panel A: Open vs closed districts		
	(1) Right Share	(2) NP and left
Post × Open	-0.0324**	0.0340***
	(0.0138)	(0.0126)
Observations	655	655
R^2	0.949	0.832
Panel B: Mine districts vs all others		
	(1) Right Share	(2) NP and left
1966× *minedistrict*	0.252	-0.00160
	(0.588)	(0.0155)
1974 × *minedistrict*	-0.267	0.0221
	(0.186)	(0.0156)
1977× *minedistrict*	-0.198	0.0170
	(0.162)	(0.0138)
1981× *minedistrict*	0.318**	-0.0298
	(0.140)	(0.0338)
1987× *minedistrict*	0.425***	-0.118**
	(0.149)	(0.0507)
Observations	354	766
R^2	0.915	0.729

Note: The 'treatment', Post × Open, is an interaction between open to recruiting status, and dummies for the 1977 and 1981 election years. *minedistrict* is a dummy taking on 1 if the electoral division (magisterial district) contained a gold mine. The outcome in the first column is the vote share of parties defined as right of the National Party, the second is the vote share of the National party and parties to its left. Each regression includes electoral division and year fixed effects. The regressions also control for maize production in 1963 and year interactions, and weights for the 1960 black rural population. Errors are clustered at the electoral division level. Outcomes are relative to 1970.

A.1 Data Appendix

Table A.9: Summary of data sources.

Data Source	Dates:	Key Variables	Geography
TEBA annual reports to board ($T = 23$)	1957-1979	· recruiting numbers, · remittances	regional office
Annual report Chamber of Mines ($T = 14$)	1964 - 1983	· Mine output · Capex · Profit	mine
Censuses of Agriculture ($T = 11$)	1963 - 1983	· Wages, Employees · Output by Crop · Machines	district
Stats SA Pop Trends ($T = 7$)	1904 - 1960	· population by urban-rural status by race	district
Gov Gazettes, Schoeman (1977) ($T = 7$)	1961-1987	·vote share by party	electoral division
Horner(1980)	1972-79	· Strikes, Injuries	mine
Map of open or closed districts	1970	· Open or closed · Closed with potential	district

A.2 Notes on White Politics

A.2.1 Historical Background

I briefly outline some of the main cleavages in white politics while noting that undoubtedly the most important aspect of South African politics before 1994 was that the majority of the population was entirely disenfranchised.[1]

The Union of South Africa was formed in 1910 when the four British colonies (two of which had been Boer republics before the Anglo-Boer War of 1899-1902) were joined. The South African Party was in power from 1910 to 1924[2] until it was defeated by the National Party - Labour Party 'Pact' government (Nohlen 1999, p. 818).[3] The Pact governed until 1932 when Hertzog's National Party and Smuts' South African Party formed the 'fusion government', which would form into the United Party. This lead the hardliner Nationalists to break away as the 'Purified' National Party under D.F. Malan.[4] South Africa entry into WWII on the side of the British again led many Afrikaners to split from the United Party in 1939, but Smuts became Prime Minister under a United Party-Dominion Party-South African Labour Party coalition government from 1939-1948. The Purified National Party members joined with refugees from the United Party to form the reunited NP from 1940.

1. South Africa at the turn of the 20th century was not unique in having a restricted franchise. Compare reconstruction in the US South; that women were widely still denied the vote; and the various Colonial empires throughout the world. There were some important (non-monotone) developments in suffrage over this time period. White women could vote from 1930. All Cape 'Europeans' received equal, direct and secret suffrage from 1931. Black voters in the Cape province were removed from the roll in 1936 and 'coloureds' in 1956 (the South Africa Act Amendment Act was passed in 1956 after the Supreme Court threw out the first such act in 1951). The age of voting was reduced to 18 from 21 in 1958.

2. Though it was part of a minority government between 1920 and 1921.

3. The National Party was formed on the basis of Afrikaner opposition to South African support for the British in WW1 (Nohlen 1999).

4. "Left-leaning" pro-British SAP members broke away to form the "Dominion Party".

It was this National Party which won power (in coalition with the Afrikaner Party) in 1948 and initiated the policy of "Apartheid". The NP would govern until 1994. The United Party, which received more votes than the National Party in 1948, but won fewer constituencies, weakened over time and dissolved in 1977. Opposition to NP rule from further left came in the shape of the Liberal Party (from 1953), which was succeeded by the Progressive Party (from 1959), the Progressive Federal Party (from 1977) and the Democratic Party (from 1989). Opposition to NP rule from the right came in the shape of the Reformed National party (HNP) (formed 1969) and the Conservative Party (CP) (formed in 1982). These parties would split the right-wing vote in the 1980s. Davies et al. (1976) and O'Meara (1996) are important references for an analysis of intrawhite political economy. Botha (1996) provides a review of the party system.

A.2.2 Geography and gerrymandering.

Information on the delimitation of political boundaries is provided by Christopher (1983) who provides a history of the work of the various Delimitation Commissions and reapportionments to the four provinces based on census returns. Delimitation was based on the number of white adults (white males, then white adults, white union nationals, and from 1952 number of registered white voters). Christopher (p.216) states that in the period in which Black and 'Coloured' voters were on the roll in the Cape, they were 'of limited influence [...] as they rarely formed more than one-fifth of the electorate of any constituency'. Note that delimitation happened in two stages: first, apportionment to provinces. Second constituency boundary drawing for each province. Christopher notes various delimitation battles waged resulting in apportionments, 'first to the benefit of Natal and the Transvaal, and more recently to the benefit of the Cape Province. The implications of this state of affairs are evident in the present struggles between the Transvaal and Cape wings of the

National Party over the interpretation of party policy.' Christopher (1983) (p. 210). Particularly noteworthy was the power of the Delimitation Commission to 'load' or 'unload' a constituency by up to 15%, based on a set of factors, with a net effect of 'favoring' rural constituencies.[5] A particular concern was the "unloading" of rural Cape seats.[6] Another important consideration is that when Transkei, Bophutatswana and Venda received 'independence' in 1977, constituency boundaries had to be redrawn (see the work of the Fourteenth Delimitation Commission).

---

5. See also Price (1953)

6. Note, according to the Constitution Amendment Act of 1965, 'special electoral divisions' could receive a 30% loading. Christopher (1983) (p.214) observes 'the unloading of the rural constituency and the corresponding loading of the urban constituency is a markedly persistent and vital factor in all delimitations since Union.' As he goes on to observe, while the 1961 Republic referendum returned 52.1% in favour of leaving the Commonwealth and becoming a Republic, in constituency terms the results corresponded to 65.3% in favor.

A.3 Ending Labor Tenancy on Farms

White farmers were an important political constituency in Apartheid South Africa. According to Crush and Jeeves, "every South African government after Union taxed the gold mines in order to subsidize farming". It was not enough that the 1913 Land Act denied blacks the rights to own land outside the reserves — an area comprising 7.5% of the country alone. A constant refrain from white farmers throughout the first half of the 20th century was the need for the South African government to provide them with more 'cheap labor'.

Various institutional arrangements were created and elaborated towards this end. The forms of tenure were still rooted in the paternalist relations inherited from the colonial era — blacks on white farms were tenants and squatters, sharecroppers, or migrants (there was no way 'up the ladder' (Alston and Kauffman 1997) since blacks could not own land).[7] Frequently the compensation provided by the farmers was far inferior to that offered by industry or mining, and farmers relied instead on the system of labor controls to sustain the viability of their low proffered pay (hence many marginal farms were kept afloat). This was particularly the case for farmers using labor-intensive techniques such as those operating the sugar plantations in Zululand, and maize and potato farmers. Farmers would also tend to favor the use of migrant labour where: production processes were labor intensive[8]; where land was more valuable; and where migrant labour was accessible — that is, the borders with (then) Rhodesia, Nyasaland and Mozambique.

Labor tenancy was an important institutional feature of many parts of the country. Notes Atkinson (2007) (p.30), "[t]he system of labour tenancy did not take hold very extensively in the Cape Province or the Free State, but it was a pre-eminent form of labour organisation

7. See Onselen (1996) for a description of the hardships black farmers endured during this time.

8. The maize fields of Eastern Transvaal, and the sugar plantations of Northern Natal.

in northern Natal and most of the Transvaal. Labour tenancy involved workers residing on white farms, with a period of service of up to 180 days per year on the farm. During the rest of the year, the tenant was free to seek outside employment."

As agriculture mechanized, aided by generous state assistance in the form of cheap credit, and wage labor became more widespread, farmers began to not have use of large amounts of "surplus labor". The state's drive for mechanization of agriculture starting from the 1960s included the incremental (across districts) outlawing of the "labor tenant" relationship on farms. Under the terms of section twenty-two of the *Bantu Laws Amendment Act* (Act No. 42 of 1964), Bantu Affairs Commissioners were authorized to choose to stop new tenancy contracts, and/or outlaw tenancy altogether. The process for the ending of tenancy was finally completed in 1979. These changes led to farm evictions, the elimination of 'black spots' and the deepening of unemployment in the homelands. Once it was no longer possible to be a labor tenant, the *Illegal Squatters Act of 1951* and the *Trespass Act of 1955* provided for summary eviction from a farm. Between 1960 and 1983, more than 3.5m people were forcibly moved into the Bantustans (The Surplus People Project 1983). As Atkinson (2007) explains:

> Official policy was aimed at tying much-needed workers to the farms; at the same time, the National Party government endeavoured to remove 'surplus' people (typically, independent black labour tenants and their families) to the homelands. There have been various estimates of the scale of forced removals. It is possible that around 1.4 million labour tenants and squatters were removed between 1960 and 1974 [...] Between 1964 and 1970, labour tenants decreased from 163,000 to 27,585 [...] the changeover from squatting to labour tenancy, and thereafter to wage labour, brought in each instance diminished access for black rural labourers to subsistence production, whether in the traditional black farming areas or in the reserves, and to grazing and cultivation rights on European farms.

A.4 Protecting white labor

A bestiary of controls. The controls protecting white labor were numerous and wide-ranging. Various authors date the start of the labor architecture to the white mining strike of 1922 against proposals of the mining companies to increase the use of 'non-white' labor. The strike was suppressed – by the military — and wages fell. In 1924 the incumbent SAP party was defeated by a coalition 'Pact' government of the farmer-friendly NP and the white-worker-protecting Labour Party. A corporatist labor structure was then introduced in the *Industrial Conciliation Act (1924).* This Act introduced an architecture of bargaining councils between employer associations and (white) trade unions. The IC Act became the mechanism for the color bar (job reservation for whites) fixed on an industry by industry basis between the relevant white employer and white union. Argues Mariotti (2010), "The IC Act allowed for industries and trade unions to come together to negotiate on general work conditions including the reservation of work for whites and on the relevant wage rate to be paid." Collective bargaining agreements negotiated between a representative trade union and employer association were made binding on non-union members through application of the *ergo omnes* principle.[9] The Pact government also introduced the *civilized labor policy* which entrenched affirmative action for whites in particular within government and parastatal jobs.

Later, the *Factories, Machinery and Building Work Act (1941)* required separate at-work amenities for different racial groups (e.g. cafeteria, restrooms, entrances were all separated by races). Mariotti (2010) argues that these added fixed costs dissuaded many small firms from hiring multiracial workforces. She also notes that the *The Apprenticeship Act of 1944* excluded blacks from artisanal careers. A linked (and crucial) component of discrimination against blacks, coloureds and indians by the white state was the radically inferior provision

9. For estimates of the effects of this legislation (which persists today, but without the racial elements), see for example Magruder (2012) who finds large and significant negative impacts on employment.

of public education relative to whites. This policy began to be reversed initially at the beginning of the 1970s, but the largest changes only followed the Soweto Riots of 1976 (sparked by the introduction of compulsory Afrikaans language instruction in black schools). The number of black female school teachers increased between 1985-1990 from around 30,000 to 80,000 (and black male school teachers increased from about 20,000 to 50,000). All other teacher series are flat (and, in fact white school teachers decline) (Crankshaw 1997). The discussion in Case and Deaton (1999) is also highly informative.

Labor reform from the 1970s and the Wiehahn and Riekert Commissions. The end of Apartheid is overdetermined. Pressures on Apartheid spatial geography towards the end of the 1970s included: local and domestic opposition to the homeland system; economic deterioration within the homelands; pressures for migration emanating from 'severe droughts in the early 1980s'; increased urban black militancy; and stronger black trade unions (Pickles 1988). Lowenberg (1997) and Mariotti (2010) argue that white support for job discrimination declined over the apartheid period as whites moved from semi-skilled to skilled work — they suggest that increased education led Afrikaans whites into more skilled occupations, and skilled workers supported allowing African workers into semi-skilled occupations. We note though that a large number (as many as 40%) of Afrikaners in the labor force worked in the state sector and likely may have opposed political change (on these grounds alone). The Riekert and Wiehahn Commissions recommended in 1979 that government end job reservation (companies were increasingly flouting these restrictions in the 1970s).

The *Riekert commission's* mandate was to consider the institutional framework for manpower utilization — excluding those aspects that would be dealt with under the terms of reference of the Wiehahn Commission into labor legislation. An important outcome of the

commission, as detailed by Pickles (1988), was the renewed push for industrial decentralization through incentives organized according to the logic of 'regional labour markets'. Pickles (p.236) reports that to some extent the high incentives offered were 'effective' — between 1978 and 1984 'the proportion of manufacturing industry located in decentralised areas' increased from 12.9 per cent to 19.3 per cent. Siebert (1987) documents how the real black/white wage gap in mining and manufacturing narrowed after 1975.

Appendix B

Appendix for Chapter 2

Appendix

B.1 Tables

Table B.1: Daily abnormal returns, in percentage points, by event window size – A negative number x correspond to a window between $-x$ days and -1 day before the event, a positive number x is a window between the event day and x day after the event.

Event window limit	Extension	Restrictions	All events	Event window limit	Extension	Restrictions	All events
-10	-.07	.15	.07	**0**	**.34**	**2.07*** **	**.65*** **
	(.12)	(.2)	(.1)	(.3)	(.29)	(.16)	
-9	-.05	.18	.11	1	.31*	.95**	.42***
	(.13)	(.21)	(.11)	(.19)	(.48)	(.17)	
-8	-.06	.29*	.12	2	-.07	.53	.02
	(.14)	(.2)	(.11)	(.2)	(.42)	(.17)	
-7	.07	.16	.16*	3	-.14	.29	-.08
	(.14)	(.22)	(.11)	(.17)	(.35)	(.14)	
-6	.03	.19	.13	4	-.1	.37	-.01
	(.15)	(.23)	(.12)	(.16)	(.3)	(.14)	
-5	-.01	.23	.11	5	-.14	.42*	-.06
	(.17)	(.24)	(.13)	(.16)	(.28)	(.14)	
-4	.07	.22	.15	6	-.08	.52**	.03
	(.18)	(.27)	(.15)	(.16)	(.25)	(.13)	
-3	.11	.07	.17	7	-.08	.5**	.03
	(.2)	(.3)	(.16)	(.15)	(.23)	(.12)	
-2	-.03	.12	.09	8	-.01	.51***	.08
	(.39)	(.36)	(.27)	(.15)	(.21)	(.12)	
-1	.7*	.44*	.58**	9	.05	.44**	.13
	(.47)	(.32)	(.27)	(.15)	(.2)	(.12)	
				10	.11	.49***	.18**
					(.14)	(.2)	(.11)

Coefficients and standard errors in parenthesis.
$***$, $**$ and $*$ significance at 1, 5 and 10% levels, respectively

Table B.2: Value of $100 investment on both sides of the event. If the event window limit is negative, we considered the value of an investment of $100 at the day of the event, looking backwards until the corresponding day before the event. If the event window limit is positive, we considered the value of an investment of $100 the day before the event, looking forwards until the corresponding day after the event. The variance has been computed via the delta-method.

Event window limit	Restrictions	Extensions	All
-10	98.6	100.7	99.26
-9	98.4	100.4	99.02
-8	97.7*	100.5	99.05
-7	98.9	99.5	98.87
-6	98.9	99.8	99.25
-5	98.9	100.1	99.47
-4	99.1	99.7	99.42
-3	99.8	99.7	99.48
-2	99.8	100.1	99.81
-1	99.6	99.3	99.42
0	102.1***	100.3	100.65***
1	101.9**	100.6*	100.84***
2	101.6	99.8	100.05
3	101.2	99.4	99.68
4	101.9	99.5	99.94
5	102.6*	99.1	99.65
6	103.7**	99.5	100.2
7	104**	99.4	100.23
8	104.7***	99.9	100.72
9	104.5**	100.5	101.26
10	105.5***	101.2	102.02**

Coefficients and standard errors in parenthesis.

$***$, $**$ and $*$ significance at 1, 5 and 10% levels, respectively

Table B.3: Daily abnormal returns depending on the source branch for the executive term limits change, in percentage points, by event window – A negative number x correspond to a window between $-x$ days and -1 day before the event, a positive number x is a window between the event day and x day after the event.

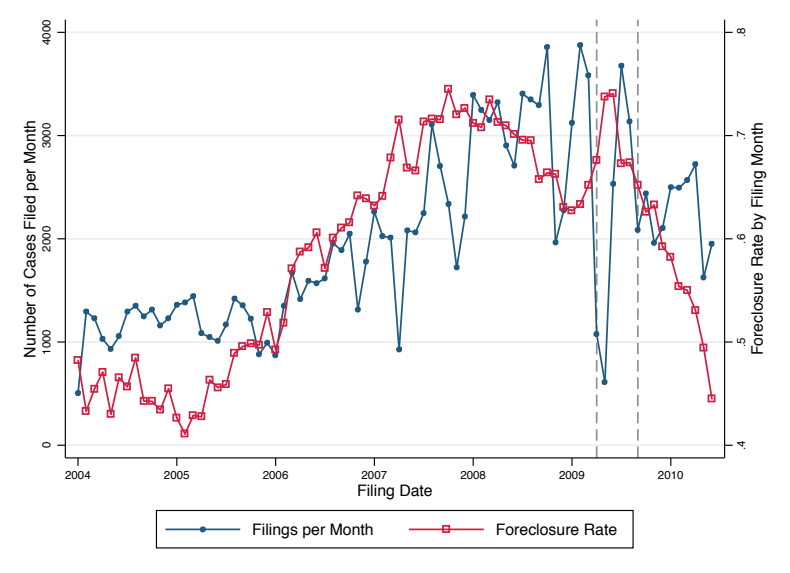
Event window limit	Executive			Legislative			Judiciary		
	Extensions	Restrictions	All	Extensions	Restrictions	All	Extensions	Restrictions	All
-10	-.17	1.19***	.04	.03	-.47	.01	-.05	-.17	.02
	(.23)	(.33)	(.2)	(.16)	(.47)	(.16)	(.33)	(.27)	(.21)
-9	-.22	1.21***	.04	-.02	-.62	-.03	-.04	.07	.14
	(.25)	(.35)	(.22)	(.17)	(.51)	(.17)	(.37)	(.21)	(.2)
-8	-.25	1.22***	.03	-.03	-.61	-.03	-.29	.12	.03
	(.27)	(.39)	(.24)	(.18)	(.54)	(.18)	(.36)	(.21)	(.19)
-7	-.16	1.13***	.06	.02	-.79*	-.05	-.02	.04	.1
	(.29)	(.44)	(.26)	(.19)	(.58)	(.18)	(.4)	(.22)	(.2)
-6	-.16	.79*	-.02	.07	-.36	.04	-.01	.17	.16
	(.33)	(.49)	(.3)	(.2)	(.6)	(.19)	(.42)	(.23)	(.22)
-5	-.25	.11	-.24	.04	-.25	.08	.1	.33*	.3*
	(.37)	(.45)	(.33)	(.23)	(.65)	(.22)	(.44)	(.23)	(.23)
-4	-.47	-.01	-.4	.16	.05	.21	-.1	.35*	.15
	(.43)	(.58)	(.39)	(.23)	(.73)	(.22)	(.41)	(.25)	(.23)
-3	-.64	.45	-.4	.1	-.14	.11	.02	.12	.05
	(.5)	(.57)	(.46)	(.29)	(.91)	(.23)	(.44)	(.25)	(.23)
-2	-.89*	1.03**	-.56	.23	-.43	.24	-.33	-.11	-.23
	(.66)	(.6)	(.6)	(.81)	(1.34)	(.46)	(.33)	(.2)	(.2)
-1	.55***	2.4***	.83***	.93	.1	.33	.03	.03	.1
	(.17)	(.45)	(.17)	(1.15)	(1.05)	(.51)	(.37)	(.36)	(.34)
0	.65***	3.01***	.95***	1.01*	1.25*	.57**	-.72**	1.78***	.57**
	(.21)	(.72)	(.2)	(.77)	(.78)	(.31)	(.32)	(.5)	(.28)
1	1.2***	1.18	1.14***	.05	.68	.02	-.29	1.84***	.91***
	(.34)	(1.01)	(.3)	(.22)	(.6)	(.17)	(.38)	(.43)	(.36)
2	.58*	1.38*	.59**	-.09	.55	-.08	-.49	1.36***	.58**
	(.38)	(.84)	(.34)	(.2)	(.66)	(.21)	(.4)	(.33)	(.3)
3	.32	1.27**	.43*	-.17	.34	-.16	-.02	.74**	.43*
	(.31)	(.73)	(.28)	(.19)	(.59)	(.18)	(.47)	(.4)	(.31)
4	.15	.94*	.32	.01	.55	.01	-.1	.4	.23
	(.28)	(.67)	(.25)	(.21)	(.51)	(.19)	(.4)	(.4)	(.28)
5	.37*	1.23**	.51**	-.1	.7*	-.07	.01	.47*	.31*
	(.27)	(.59)	(.24)	(.19)	(.48)	(.18)	(.36)	(.34)	(.24)
6	.38*	1.34***	.54***	.12	.62*	.06	-.16	.42*	.25
	(.26)	(.53)	(.23)	(.21)	(.45)	(.17)	(.37)	(.32)	(.24)
7	.32*	1.01**	.45**	.06	.61*	.04	-.07	.46*	.3*
	(.24)	(.5)	(.21)	(.19)	(.42)	(.16)	(.35)	(.28)	(.22)
8	.59**	1.01**	.67***	.02	.52*	.03	-.18	.56**	.34**
	(.27)	(.53)	(.24)	(.18)	(.37)	(.14)	(.35)	(.27)	(.2)
9	.44*	.87**	.51**	.05	.47*	.05	-.17	.41*	.25*
	(.31)	(.49)	(.28)	(.18)	(.35)	(.15)	(.32)	(.25)	(.19)
10	.45*	1.26***	.58**	.13	.52*	.13	-.02	.47**	.32**
	(.28)	(.53)	(.25)	(.17)	(.34)	(.14)	(.32)	(.25)	(.19)

Coefficients and standard errors in parenthesis.
$***$, $**$ and $*$ significance at 1, 5 and 10% levels, respectively

Appendix C

Appendix for Chapter 4

Figure C.1: Foreclosure Cases Over Time in Cook County, IL



Notes: Monthly count of new foreclosure filings in Cook County over time (left axis) and share of cases filed in a given month that end in a completed foreclosure (right axis). Dashed vertical lines indicate Cook County suspension of all new foreclosure filings starting April 16, 2009, except for so-called "consent foreclosures:" foreclosure filings in which lender and borrower had already agreed to foreclosure prior to filing. This "moratorium" was scheduled to end on September 1, 2009, although appears to have ended earlier, given the spike in filings prior to Sept. 1.

Table C.1: Descriptive Statistics: Pre-Treatment Characteristics

	Mean			P-Value
	All	$D^{\dagger}$	$F^{\dagger}$	$(H_0 : D = F)$
Case Resolved	0.950	1.000	1.000	.
Ends in Foreclosure	0.612	0.000	1.000	.
Days to Decision	373.554	274.931	428.665	0.000
Prob Redefault	0.049	0.120	0.013	0.000
Single-Family Property	0.623	0.684	0.590	0.000
Conventional Mortgage	0.647	0.665	0.653	0.000
Loan Principal	237328.100	219520.200	237733.000	0.892
Complaint Amount	229621.000	211420.400	231905.600	0.449
Large Plaintiff	0.471	0.472	0.473	0.065
Large Attorney	0.685	0.682	0.687	0.032
Median Income (tract)††	44859.720	46409.490	43748.260	0.000
Share White (tract)††	0.449	0.468	0.433	0.000
Population (tract)††	5434.558	5472.077	5411.712	0.000
N	148220	50140	90653	

Notes: Data from matched court records and foreclosure filings (one obs per case) with baseline sample restrictions as described in text.
$^{\dagger}D$ = dissmissed cases, F = completed foreclosures.
††Data from 2000 Census (tract-level).

C.1 Data Appendix

Cleaning Court Records We collected all chancery court case records filed between January 2004 and June 2010 (inclusive). We extract from each record the associated case number and the case calendar to which the case is assigned. The records also contain a list of case actions, the lawyer who initiated this action, the associated judge, and the date. We extract this list of actions (simple text descriptions, e.g., "Amend complaint or petition - allowed" or "Dismiss by stipulation or agreement") and the corresponding dates.

We identify a case as ending in a dismissal if an action occurs containing one of the following descriptions: "mortgage foreclosure motion plaintiff dismissed", "mortgage foreclosure voluntary dismissal, non-suit or dismiss by agreement", "mortgage foreclosure motion defendant dismissed", "mortgage foreclosure dismissed for want of prosecution", "dismissed

Table C.2: Descriptive Statistics: Outcomes

	Means			P-Value	
	All	$D^\dagger$	$F^\dagger$	$(H_0 : D = F)$	N
Neighboring Filings	2.423	2.161	2.596	0.000	475127
Any Neighboring Filing	0.734	0.702	0.755	0.000	475127
Neighboring Foreclosures	0.748	0.603	0.844	0.000	475127
Any Neighboring Foreclosure	0.365	0.326	0.390	0.000	475127
Neighboring Sales	3.038	2.962	3.099	0.006	133176
Mean Neighboring Sale Price	169326	184213	157182	0.000	81371
Mean Repeat-sale Adjusted Price	122896	123240	122621	0.205	56306

Notes: Outcome variables (0.1 mile radius) measured annually for five years (two years for sales outcomes) after case is decided (observation = one case-year).
$^\dagger D$ = dissmissed cases, F = completed foreclosures.

for want of prosecution", "general chancery - dismissed for want of prosecution", "general chancery - voluntary dismissal, non suit, dismiss by agreement", "mortgage foreclosure voluntary dismissal, non-suit or dismiss by agreement", or "mortgage foreclosure judgment for defendant"; or an action containing any of the following: "case dismissed", "voluntary dismissal", "declaratory judgment voluntary dismissal", "dismiss entire cause" and not "denied", or "dismiss by stipulation or agreement" and not "denied". For dismissed cases, we consider the end of the case to be the date of this "dismissal" action (in the case of multiple such actions, we take the final).

Cleaning RIS Data Record Information Services, Inc. provided us with details of foreclosure filings and foreclosure auctions for Cook County from 2002 through 2011. RIS is a private data provision company that collects publicly available records on all foreclosure filings in the five counties of Chicago. RIS employees manually input data on each foreclosure filing. From the foreclosure filings, we extract the associated chancery court case number, unique loan ID, the filing date, details of the associated loan (origination date, principal at origination, outstanding claim at time of foreclosure filing, a general indication of mortgage

Table C.3: Balance of Covariates

	Coefficient†	p Value††
Adj. Rate Mortgage	0.0005100 (0.0006240)	0.3837
Loan Principal†††	-0.0000033 (0.0000025)	0.0343**
Large Plaintiff	-0.0005790 (0.0003840)	0.9438
Large Attorney	-0.0007020 (0.0007410)	0.1370
Median Income (tract)†††	-0.0002780 (0.0001730)	0.6566
Share White (tract)	-0.0000439 (0.0005620)	0.4025
Population Density (tract)	0.0454000 (0.0355000)	0.1364
Price (Zip code)†††	0.0000541 (0.0000410)	0.8564
Total Foreclosure Filings††††	0.0000266 (0.0000570)	0.3578
Total Completed Foreclosures††††	0.0001580 (0.0001230)	0.5866
p Value	0.4280	
N	143276	

Notes: **Indicates significance at the 5% level. Standard errors are the max of the SE clustered on census tract, clustered on filing month, and multi-way clustered on tract and filing month. †Coefficient from regression of instrument (case-calendar-filing-month foreclosure rate) on given pre-treatment covariates, controlling for filing month and property type fixed effects. P value in first column is from a joint significance test for the given covariates. ††Given covariate (for that row) is regressed on full set of case calendar dummies (plus filing month and property type fixed effects); p value for a joint significance test of the case calendar dummies. †††In $10,000 of dollars. ††††Outcomes as defined in Section 4.4 measured the year before the case is filed.

Table C.4: Complier Characteristics: Ratio of Subgroup First Stage Estimate to Overall First Stage

Quartile of Income†			
1	2	3	4
0.784	1.011	1.060	1.066
Loan Characteristics††			
Large Lender	Conventional Mortgage	Positive Zip Code Price Growth	Underwater
1.002	0.896	0.795	0.907

Notes: †Income quartile is given by the tract-level quartile of median tract income from the 2000 Decennial Census. ††Large lender is an indicator for the plaintiff being one of the six most prominent banks in the sample, each representing $\geq 10\%$ of filings. Zip-code-level annual price growth is taken from Zillow housing price indices for Cook County. Underwater is a proxy for the outstanding balance of the mortgage being greater than the estimated value of the home, as described in the text.

Table C.5: Baseline Contagion Estimates: 2SLS Coefficient of Effect of Completed Foreclosure on Given Outcome in Given Year

Years After End of Case (t):	0	1	2	3	4	5
Baseline Estimates						
$Y_{id(i)+t}$ = Any Filing per Year	0.052*	0.012	0.082***	0.090*	0.247**	0.140
	(0.028)	(0.027)	(0.027)	(0.053)	(0.112)	(0.133)
1st-stage F	238.300	224.200	205	24.620	19.060	12.970
N	130199	118566	93143	67379	41958	23831
$Y_{id(i)+t}$ = Total Filings per Year	0.691*	0.670*	0.536***	0.657**	1.551	0.538
	(0.393)	(0.368)	(0.183)	(0.319)	(0.983)	(0.987)
1st-stage F	238.300	224.200	205	24.620	19.060	12.970
N	130199	118566	93143	67379	41958	23831
Cumulative Count of Filings						
$Y_{id(i)+t}$ = Cumulative Filings	0.691*	1.395*	2.090***	4.522***	6.446**	5.610
	(0.393)	(0.732)	(0.794)	(1.169)	(3.105)	(4.610)
1st-stage F	238.300	224.200	205	24.620	19.060	12.970
N	130199	118566	93143	67379	41958	23831
$Y_{id(i)+t}$ = Cumulative Filings	0.622	1.266*	1.594**	3.820***	4.076	2.693
No Recent Foreclosures	(0.380)	(0.667)	(0.703)	(1.429)	(2.918)	(4.611)
1st-stage F	172.500	159.700	138	20.910	12.790	7.515
N	71925	66995	55105	43186	27395	14948

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates of the effect of complected foreclosure on given outcome (measured within 0.1 miles of the property in the given year since the case is decided), on an indicator for the case ending in foreclosure (instrumented by the leave-one-out case-calendar-filing-month-specific foreclosure rate), filing month, property type, and year of observation fixed effects, and case-level controls:share of tract that report race as white in 2000 decennial census, income quartile from decennial census, whether plaintiff is a "large plaintiff" (six largest plaintiffs each representing $\geq$ 7000 filings) or attorney is a "large attorney" (three largest attorneys each representing $\geq$10,000 cases), whether mortgage is adjustable rate, size of initial loan, and census tract population. Cumulative filings respresent the total number of new filings since decision through the given year.

Table C.6: Constant Sample Contagion Estimates

Years After End of Case (t):	0	1	2	3	45
Constant Sample Observed for 3 Years after Case Decision (Decided in 2004–2008)					
$Y_{id(i)+t}$ = Any Filing per Year	0.094**	0.059	0.168***	0.090*	
	(0.048)	(0.046)	(0.061)	(0.053)	
1st-stage F	24.620	24.620	24.620	24.620	
N	67379	67379	67379	67379	
$Y_{id(i)+t}$ = Total Filings per Year	1.243**	1.567***	1.056***	0.657**	
	(0.552)	(0.395)	(0.391)	(0.319)	
1st-stage F	24.620	24.620	24.620	24.620	
N	67379	67379	67379	67379	
Cases Observed for 2 Years or Fewer after Decision (Decided in 2009–2011)					
$Y_{id(i)+t}$ = Any Filing per Year	0.027	-0.018	0.026		
	(0.037)	(0.038)	(0.038)		
1st-stage F	72.890	69.410	66.700		
N	62820	51187	25764		
$Y_{id(i)+t}$ = Total Filings per Year	0.239	0.099	0.202		
	(0.447)	(0.515)	(0.263)		
1st-stage F	72.890	69.410	66.700		
N	62820	51187	25764		

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates of the effect of complected foreclosure on given outcome (measured within 0.1 miles of the property in the given year since the case is decided), on an indicator for the case ending in foreclosure (instrumented by the leave-one-out case-calendar-filing-month-specific foreclosure rate), filing month, property type, and year of observation fixed effects, and case-level controls:share of tract that report race as white in 2000 decennial census, income quartile from decennial census, whether plaintiff is a "large plaintiff" (six largest plaintiffs each representing $\geq$ 7000 filings) or attorney is a "large attorney" (three largest attorneys each representing $\geq$10,000 cases), whether mortgage is adjustable rate, size of initial loan, and census tract population. Constant sample includes only cases for which we observe three years post decision.

Table C.7: Estimates in Years Before Filing

Years Before Case is Filed	3	2	1
Any Filing	0.003	0.009	0.018
	(0.029)	(0.029)	(0.022)
1st-stage F	400.000	400.000	400.100
N	140672	140672	140672
Total Filings per Year	0.020	0.084	0.214
	(0.087)	(0.122)	(0.139)
1st-stage F	400.000	400.000	400.100
N	140672	140672	140672
Any Completed Foreclosure	-0.027	0.045*	-0.004
	(0.023)	(0.025)	(0.030)
1st-stage F	159.400	182.100	164.300
N	140672	140672	140672
Total Completed	-0.057*	0.075	0.110
Foreclosures per Year	(0.033)	(0.065)	(0.071)
1st-stage F	159.400	182.100	164.300
N	140672	140672	140672
log(price)	0.007	0.037	0.007
	(0.038)	(0.042)	(0.049)
1st-stage F	164.900	166.500	136.400
N	109123	102220	84126

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates as in Table C.5, with outcomes measured in the given year prior to the case being filed.

Table C.8: Contagion in Completed Foreclosures

Years After End of Case	0	1	2	3	4	5
	Baseline Estimates					
Any Completed Foreclosure	-0.048	0.002	0.045	0.138***	0.033	0.174
	(0.031)	(0.030)	(0.038)	(0.051)	(0.106)	(0.144)
First-stage F	238.300	224.200	205	24.620	19.060	12.970
N	130199	118566	93143	67379	41958	23831
Total Completed Foreclosures	0.280*	0.435***	0.312***	0.558***	0.243	0.063
	(0.164)	(0.160)	(0.117)	(0.185)	(0.316)	(0.334)
First-stage F	238.300	224.200	205	24.620	19.060	12.970
N	130199	118566	93143	67379	41958	23831
	Contagion in Completed Foreclosures Filed Prior To Decision					
Any Completed Foreclosure	-0.043	0.135***	0.073***	0.025	-0.010	0.003
	(0.031)	(0.028)	(0.019)	(0.016)	(0.017)	(0.015)
First-stage F	238.300	224.200	205	24.620	19.060	12.970
N	130199	118566	93143	67379	41958	23831
Total Completed Foreclosures	0.303*	0.609***	0.111***	0.031	-0.010	0.003
	(0.164)	(0.118)	(0.028)	(0.019)	(0.017)	(0.015)
First-stage F	238.300	224.200	205	24.620	19.060	12.970
N	130199	118566	93143	67379	41958	23831

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates for given completed foreclosure outcome are as in Table C.5.

Table C.9: Baseline Housing Market Estimates

Years Since End of Case		0	1	2
log(price)	2SLS	-0.127	-0.411**	-0.358
		(0.105)	(0.203)	(0.395)
	1st-stage F	22.500	18.840	14.390
	N	43079	26047	12241
log(price) (repeat-sales)	2SLS	0.059	0.003	-0.251
		(0.066)	(0.146)	(0.201)
	1st-stage F	11.130	6.314	7.355
	p-value	0.107	0.030	0.763
	N	30482	17916	7904
Total Sales (Cumulative Over Years)	2SLS	0.220	6.562	9.964
		(0.914)	(4.574)	(9.255)
	1st-stage F	24.620	19.060	12.970
	N	67379	41958	23831
log(price at last sale)† (repeat sample)	2SLS	-0.031	-0.373	-0.186
		(0.116)	(0.230)	(0.323)
	1st-stage F	21.000	12.400	13.910
	N	29620	17597	7852

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates for given completed foreclosure outcome are as in Table C.5. Sales outcomes represent residential transactions within the given radius and time period only (total sales represents the total count of sales since the decision year), while price represents the mean sale price of these transactions. Repeat-sales adjusted prices are estimated as outlined in the text; associated p-value is for the cross-equation test of equality between the repeat-sales adjusted and unadjusted (first panel) price effects. †Price at previous sale (for repeat sales) adjusted for annual price growth. Fewer observations here since sample is restricted to the latter of all repeat sales (need to observe a previous sale).

Table C.10: Contagion Among Loans with Lenders Implicated in Independent Foreclosure Review Settlement

	Outcome Variable	Any Filing	Total Filings	Log Filings
Baseline†	Effect of Completed Foreclosure	0.062***	0.735**	0.094*
		(0.022)	(0.320)	(0.050)
	First-stage F	131.900	131.900	147.100
	N	311116	311116	235325
Automating Lenders†	Effect of Completed Foreclosure	0.039*	0.115	0.084*
		(0.022)	(0.073)	(0.049)
	First-stage F	131.900	131.900	138.000
	N	311116	311116	86054

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS specification as in Table C.5, although all five years post-decision are pooled and the effect of completed foreclosure is fixed.
†Outcomes are measured based on new foreclosure filings by lenders implicated in the Office of the Comptroller of the Currency and the Board of Governors of the Federal Reserve System Independent Foreclosure Review Settlement, while "Baseline" includes new foreclosure filings among all lenders. Both samples are restricted to cases decided between 2007 and 2010 (during which automation of foreclosure filings is thought to be most common).

type—conventional, adjustable rate, etc.), details of the associated property (latitude and longitude, census tract, zip code, property type—condo, single family, etc.), and the parties involved (defendant name, plaintiff—the lender or servicer—identity, plaintiff law firm).

We identify a case as ending in a completed foreclosure if there is an associated foreclosure auction record in the RIS data. For completed foreclosures, we use the date of the foreclosure auction as the end-date of the case. If there is both a dismissal action in the court records and an associated foreclosure auction, we consider the case to have ended in a completed foreclosure, although, our results are not sensitive to this decision. Relatedly, there is a field in the RIS data that indicates the outcome of the auction, including if the auction is canceled. Since this information is missing for half of the years and since it is not indicated why a cancellation occurs, we do not code canceled auctions as dismissals in our analysis sample. Again, however, our baseline results are not sensitive to coding canceled auctions as dismissals. We consider a borrower to have redefaulted if a foreclosure filing is brought

Table C.11: Contagion Estimates By Proxy for Borrower Equity

Outcome Variable	Any Filing	Total Filings	Log Filings
Neighboring Filings from Underwater versus Non-Underwater Borrowers			
Effect of Foreclosure on Filings of Non-Underwater Borrowers†	0.066***	0.696***	0.096**
	(0.020)	(0.230)	(0.043)
Effect of Foreclosure on Filings of Underwater Borrowers†	0.023	-0.066	-0.003
	(0.019)	(0.062)	(0.035)
First-stage F	84.220	84.220	86.940
p val for difference between groups	0.053	0.000	0.006
N	902490	902490	441734
Neighboring Filings from -10% – 10% Equity versus ¡ -10% or ¿ 10%			
Effect of Foreclosure on Filings with Equity ¡ -10% or ¿ 10%††	0.008	-0.159***	-0.035
	(0.017)	(0.054)	(0.035)
Effect of Foreclosure on Filings with Equity between -10% and 10%††	0.063***	0.789***	0.107**
	(0.020)	(0.245)	(0.045)
First-stage F	84.220	84.220	89.350
p val for difference between groups	0.017	0.000	0.001
N	902490	902490	416141

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates as in Table C.5, pooling observations for five years after the case is decided (fixing the effect of completed foreclosure to be constant).

†Outcomes are for two separate counts of new foreclosure filings by either borrowers in negative equity or borrowers in positive equity (as defined in the text). P-value tests the significance between the two responses (positive vs. negative equity)—estimates are performed simultaneously (pooling both outcomes and allowing for differing filing month and year of observation fixed effects).

††Outcomes are for two separate counts of new foreclosure filings by either borrowers where the difference between outstanding debt and loan principal is within 10% of initial loan principal and all other borrowers (see definition in the text). P-value tests the significance between the two responses—estimates are performed simultaneously (pooling both outcomes and allowing for differing filing month and year of observation fixed effects).

Table C.12: Contagion Estimates By Lender Identity

Years Since Decision	Any Filing	Total Filings	Log Filings
Filings from Different Lender†	0.061***	0.770***	0.085**
	(0.022)	(0.245)	(0.041)
Filings from Same Lender†	0.006	-0.184***	0.008
	(0.015)	(0.056)	(0.045)
First-stage F	87.490	87.490	93.560
p val for difference between groups	0.052*	0.000***	0.109
N	796112	796112	330880

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates as in Table C.5, pooling observations for five years after the case is decided (fixing the effect of completed foreclosure to be constant).

†Outcomes are for two separate counts of new foreclosure filings by either different lenders than that in the observed case or the same lender. P-value tests the significance between the two responses (same lender vs. other lender)—estimates are performed simultaneously (pooling both outcomes and allowing for differing filing month and year of observation fixed effects).

against the same loan ID. Note that this will not count new filings at the same property for different loans (e.g., if the home owner has filings against mortgage and a home equity loan, we count these as distinct filings). However, if a second filing against the same loan ID occurs within 180 days, we consider this to be the same case, taking the first date as the true filing date (and merging info from the two filings). The descriptive statistics in Table C.1 show a non-zero number of redefaults (1.3%) among loans that end in foreclosure. This is likely due to miscoding in the RIS data—for example, a foreclosure auction is scheduled and recorded by RIS, the case is dismissed before the auction takes place (and RIS misses this) and the borrower subsequently redefaults on the loan. Our results are not sensitive to discarding these observations.

We construct a unique ID for each plaintiff and attorney as follows. For plaintiffs and attorneys who are on more than ∼100 cases, we manually checked the names for consistency and constructed a unique ID number. We then identified "large plaintiffs" as those plaintiff-IDs associated with greater than 7000 cases, and "large attorneys" as those attorney-IDs

associated with greater than 10000 cases. We identify by name all plaintiffs implicated in the Independent Foreclosure Review Settlement conducted by the Office of the Comptroller of the Currency and the Board of Governors of the Federal Reserve System.

We match the RIS and chancery court records by case number. We discard non-matches, which arise due to several factors: non-foreclosure chancery court cases (e.g., name changes, mechanic's liens) will not appear in the RIS foreclosure filings; differing date ranges between the two data sources (2004–2010 for court records, 2002–2011 for RIS); and differing geographies (RIS data includes some cases in neighboring counties).

Cleaning Census Data We merge in the following census-tract-level data from the 2000 Decennial Census: median tract income, population, land area, and share of population that identifies as each census-designated race. We construct tract-level income quartiles (i.e., what quartile of median income does a given tract fall into), an indicator for being a predominantly white tract (share white is greater than the median share), population density (and associated quartiles), and a Herfindahl-Hirschman index for each race (i.e., the sum of the squared share of each race in the tract). We merge the census tract data to the RIS data using census tract FIPS codes.

Cleaning Deeds Records These data are collected from the county recorder for Cook County, IL. The records were collected by an anonymous private firm and made available to us by the Paul Milstein Center for Real Estate at the Columbia Graduate School of Business. These data include the date of each sale property transaction, the type of property, the address, the price of the sale, and an indicator for the property being residential. We drop all transactions with sale price or address missing. We drop duplicate records—multiple sales with identical sale prices that occur at the same property within 30 days of one another. We keep only residential sales. We geocode these deeds records based on the property address

using Yahoo! Placefinder.

Defining Outcomes Using the cleaned and matched RIS and court records, for each foreclosure case we calculate the distance between the associated property and the properties associated with all other foreclosure filings. We then count the number of new filings around each property (i.e., within the given radius; 0.1 miles in the baseline) in each calendar year, omitting from the count new filings at the same property or filings associated with the same case—e.g., a given loan may be tied to multiple properties, which we do not want to include in the count. If there are multiple foreclosure filings at a neighboring property, we include each of these in the count (although we have found that our results are not sensitive to treating these as a single new filing). For each calendar year, we then construct an indicator for there being any new foreclosure filing. We then identify the year that the case is decided and define our contagion outcomes relative to that year: number of filings and indicator for any filing in the year the case ends, number and indicator one year after the case ends, two years after, and so on. We follow the same procedure to get a count of new completed foreclosures in each calendar year: calculate the distance between the property associated with each case and each completed foreclosure (from the RIS auction data), and count the auctions that occur within the given radius in the given calendar year (where this date is based on the auction date).

We also construct several sub-counts of new foreclosure filings. Foreclosure filing records in the RIS data have a field reporting additional lien holders that are listed on the foreclosure claim (reporting additional lien holders is optional for plaintiffs). For each filing we create an indicator for additional lien holders. We then construct the same contagion outcomes, but only counting filings with multiple lien holders. Similarly, we construct our contagion outcomes using the annual count of new filings from plaintiffs implicated in the Independent

Foreclosure Review Settlement and, as described in the text, use this to investigate contagion among lenders known for the bulk processing of delinquent loans into foreclosure filings. Thirdly, we construct an indicator for the borrower being underwater on their loan: we take the principal of the mortgage at origination, conservatively assume an 80% loan-to-value ratio to back out the value of the property, adjust the value of the property using Zillow monthly zip code housing price indices for the year of origination and the year of the foreclosure filing, and compare the adjusted value of the property to the claim made against the borrower by the plaintiff. If the claim is larger than the value of the property, then we consider the borrower to be "underwater". For each property associated with a foreclosure filing, we then find the annual count of filings against underwater borrowers within the given radius. We have experimented with other ways to construct this underwater indicator—claim larger than loan principal at origination, claim larger than 110% of estimated value, and so on—and find little difference in our results. Finally, we construct contagion outcomes for filings from the same lender. We restrict our sample to the set of cases for which we cleaned the plaintiff name (i.e., filings with plaintiffs who appear on approximately more than 100 filings). For each case within this subsample, we identify all filings within the given radius and create an indicator for the neighboring filing having the same plaintiff. We then find the annual count of new filings from the same plaintiff and new filings from other plaintiffs.

We construct our housing market outcomes in a very similar way. For each foreclosure filing, we calculate the distance between the associated property and all residential sales in the deeds records. As with the filings, we want to exclude sales at the property associated with the completed foreclosure. However, in this case (and unlike the filings) we do not have a perfect match of address—we cannot precisely identify a sale associated with the property going through foreclosure. Instead, we drop sales within 0.01 miles. For each calendar year, we take the mean sale price of all sales occurring within that year within the given radius

(0.1 miles in the baseline) of the property associated with the foreclosure filing. We then use as an outcome the log of this sale price. In the following section, we discuss how we use repeat sales do adjust this average sale price for fixed property quality. We also use as an outcome the count of residential sales that occur within the given radius in the given calendar year and an indicator for any sale occurring.

C.2 Adjusting Price Data for Property Quality Using Repeat Sales

In our deeds records, we first identify all repeat sales: of the 1,330,949 residential sales we observe between 1995 and 2008, there are 585,756 (44.01%) properties that transact more than once, which leaves us with 216,068 transactions during the relevant period of 2004 – 2008 (43.56% of the 496,055 residential transactions in this period). For each property, k, we assume that the sale price in year t, P_{kt}, is a function of the property's time-invariant characteristics, δ_k, the year of sale, and whether or not there is a recent foreclosure nearby, $F_{i(k)t}$:

$$\log(P_{kt}) = \alpha_0 + \alpha\delta_k + \Psi_t + \alpha_F F_{i(k)t} + \epsilon_{kt} = \alpha_0 + \alpha\delta_k + \Psi_t + e_{kt} \tag{C.1}$$

where Ψ_t is a year-specific fixed effect and we denote for convenience $e_{kt} \equiv \alpha_F F_{k(i)t} + \epsilon_{kt} = \log(P_{kt}) - \alpha_0 - \alpha X_k - \Psi_t$. We want a measure of the sale price, P^*_{kt}, that removes the influence of property characteristics, but allows price to vary with foreclosure:

$$\log(P^*_{kt}) = \beta_0 + \phi_t + e_{kt}$$

To achieve this, we estimate a simple price regression that controls for property and year of

sale for all repeat sales in our sample: $\log(P_{kt}) = \beta_0 + \theta_k + \Psi_t + e_{kt}$, where θ_k is a vector of property fixed effects and Ψ_t is a vector of year-of-sale fixed effects. Property fixed effects absorb the influence of the (time-invariant) property characteristics. Using the OLS parameter estimates and residuals from this model, we then estimate $P^*_{kt} = \exp\left(\hat{\beta}_0 + \hat{\Psi}_t + \hat{e_{kt}}\right)$. Using these quality-adjusted sales prices, we then construct a quality-adjusted measure of sale price for each property i going through the foreclosure courts by taking the log of the average of all P^*_{kt} that transact within x miles of property i in the relevant year of observation.

C.3 Monotonicity of Instrument

A failure of monotonicity occurs if a higher value of the instrument means a higher probability of foreclosure for some cases, but a lower probability for others. As discussed in the main text, a failure of monotonicity may arise if judges treat different types of borrowers and lenders differently.

We examine this possibility by relating foreclosure rates for each case calendar for different subgroups to the overall value of the instrument for that case calendar. We want to check that a higher value of the instrument for the case calendar is associated with a higher foreclosure rate for the sub-groups. We first calculate the overall foreclosure rate by case calendar and filing year, and de-mean these estimates by filing year.[1] We then take a given covariate (e.g., the borrower is from a predominantly white neighborhood) and calculate the foreclosure rate by case calendar, filing year, and the value of the covariate (e.g., foreclosure rate by case calendar, filing year, and whether predominantly white neighborhood), and

1. Recall that our estimates all include filing date fixed effects, so the relevant comparison is within filing date, although the figures are similar if we do not de-mean. We use filing year for this exercise to decrease noise in the foreclosure rate estimates; the story does not change if we use filing month, although the associated figures are noisier.

again de-mean by filing year. We plot the de-meaned group-specific foreclosure rates against the de-meaned general case-calendar-filing-year foreclosure rate and display these plots in Figures C.2, C.3, and C.4. A failure of monotonicity as we described above suggests that for certain subgroups a higher general case-calendar foreclosure rate is associated with a higher group-specific foreclosure rate, while for other subgroups a higher general case-calendar foreclosure rate is associated with a lower group-specific foreclosure rate. We construct six such plots: i) comparing foreclosure rates between properties in census tracts where the share of white residents is greater than the median to those below the median, ii) comparing properties in each quartile of median tract-level income, iii) comparing foreclosure rates among conventional (fixed-rate) mortgages vs. unconventional mortgages (adjustable rate, interest only, etc.), iv) comparing foreclosure rates by property type, v) comparing cases where the plaintiff is a large lender to those with smaller lenders, and vi) comparing foreclosure rates among cases where the lender's attorney is a large vs. smaller attorney (as previously defined). Figures C.2, C.3, and C.4 shows that there is no evidence of a failure of monotonicity. In all cases, there is a clear positive relationship between the overall case-calendar-filing-year foreclosure rate and the group-specific case-calendar-filing-year foreclosure rate—a higher value of the instrument is associated with a higher foreclosure rate in each subgroup. Thus, in terms of observables—property type, loan type, whether the plaintiff is a large bank or employs a large attorney, and census tract demographics—the monotonicity assumption appears valid.

C.4 Nonlinearities in Foreclosure Contagion

Our regressions are all at the foreclosure-case level, which raises two issues. Firstly, the treatment is imperfectly assigned since neighborhoods around two (or more) delinquent properties

may overlap. Secondly, and relatedly, it is difficult to investigate non-linearities in the effect of a completed foreclosure. To explore nonlinearities and work with a cleaner (albeit limited) treatment, we look at the effect of the lagged foreclosures in a small neighborhood on the new filings.

We define small neighborhoods by partitioning Cook County into squares and examining foreclosure behavior within. We assign each property associated with our foreclosure cases to a 0.0625-square-mile square (0.25x0.25). Within each square, i, denote by N_{it} the count of new filings that occur in each year, t. Similarly, we count the total number of completed foreclosures in each year, F_{it}. We discard squares that have no foreclosure activity (i.e., no filings) over the period 2004–2010.

Our goal is to relate the lagged number of completed foreclosures in square i to the number of new filings in a given year. We include fixed effects for the number of ongoing foreclosure cases for properties in square i as of year t (η_{it})[2] and for the year of observation (ψ_t):

$$N_{it} = \beta_0 + \beta_1 F_{it-1} + \beta_2 F_{it-2} + \eta_{it-1} + \eta_{it-2} + \psi_t + \beta X_i + e_{it} \tag{C.2}$$

where X_i is a vector of square-specific controls (census-tract demographics).[3] We instrument the lagged number of completed foreclosures using the expected number of completed foreclosures in square i in year $t-1$ or $t-2$ (conditional on the number of ongoing cases, $\eta_{i,t-j}$), where we take the probability of a filing ending in completed foreclosure to be the leave-one-out case-calendar/filing-month probability of foreclosure as described in Section

2. Since our data only covers filings from 2004 through 2010, we only consider cases filed in this period.

3. We have also experimented with additional lags, although these are rarely significant and require a further reduction in sample size.

4.4.[4] In this way, we are comparing neighborhoods with the same initial foreclosure filing activity, but with different completed foreclosure outcomes owing to random assignment of these filings to different case calendars.

Aggregating in this way helps address the two issues listed above. Firstly, neighborhoods here are well defined entities that do not overlap, and so there is no mis-assignment of the treatment as defined (while this is a contained definition of the treatment, completed foreclosures on the edges of these neighborhoods may nonetheless have spillover effects to neighboring squares that we are not accounting for). Secondly, by aggregating counts of foreclosures we can more readily explore non-linearities in the effect of filings.

The results from the 2SLS estimation of Equation C.2 are presented in Table C.34 and show evidence of contagion consistent with our baseline estimates from Section 4.5.1. We split the sample by pre-crash (2006–2008) and post-crash (2009–2010) observations to reflect the differences seen for these samples in Section 4.5.1. Column 1 shows a strong positive relationship between lagged completed foreclosures and new foreclosure filings—a completed foreclosure one year prior causes 0.281 new filings, while a foreclosure two-years prior causes 0.913 new filings. Off of a mean of 2.54 filings per year for this sample, this represents an increase of 11.1% to 35.9%, which is on par with the baseline estimates in Section 4.5.1. At the same time, the linear estimates for 2009–2010, in Column 4, are substantially smaller: 0.115 and 0.481 (5.2% and 21.8% relative to the mean of 2.21 filings for this sample). Columns 2 and 5 show estimates allowing for a quadratic relationship between lagged foreclosures and new filings. Interestingly, the quadratic relationship is significant—as the lagged number of foreclosures grows, the marginal effect of a completed foreclosure in the neighborhood

4. We treat each case filed within square i as of the given period as an independent random draw that may or may not foreclose in the given year. We construct the probability of foreclosure in the given year for each case as the leave-one-out share of cases filed in the same month and assigned to the same calendar that foreclose in the given year. Then the expected number of foreclosures in square i in a given year is the sum over these probabilities.

diminishes. Again, this is consistent with the findings in Sections 4.5.1 that split the sample by pre-crisis and crisis—exposure to more foreclosure activity diminishes the contagion effect, perhaps because the marginal foreclosure conveys less information (about house prices or the foreclosure process itself).

Finally, in Columns 3 and 6 we allow for a more flexible relationship between lagged foreclosures and new filings by regressing new filings on a set of indicators for the lagged number of foreclosures. We instrument the lagged foreclosure indicators by the probability of observing that number of foreclosures in that year, conditional on the number of ongoing foreclosure cases.[5] These estimates seem to suggest that the contagion effect is strongest for the first two completed foreclosures. However, we do not put too much stock in these results as the first-stage F statistic for these regressions suggests that the instrument here is weak.

C.5 Lender Response to a Completed Foreclosure

For any given mortgage, divide the remaining life of the loan into three periods ($t \in 0, 1, 2$). Consider a mortgage in which the borrower owes a payment of m_t in each period, t and the mortgage is fully paid back as of $t = 2$. Suppose the borrower misses their payment in the current period ($t = 0$). With probability α_1 the borrower will still be delinquent the following period and the lender can foreclose on the property, in which case the lender recovers the value of the home, P, less the costs of foreclosure, λ (e.g., legal fees). However, with probability $1 - \alpha_1$, the borrower will recover in period 1 and will resume making payments. Then the value of the unmodified loan to the lender is:

5. For example, if there are four ongoing foreclosure cases in a given year, then the instrument for the indicator of one completed foreclosure is the sum of the probabilities that each of the four cases ends in foreclosure, where these probabilities are given by the case-calendar-filing-month foreclosure rate in that year. We restrict the sample to observations with no more than five ongoing cases in the lagged years, since the calculation of these probabilities grows drastically with the number of outstanding cases.

$$V_u = \alpha_1 \cdot (P - \lambda) + (1 - \alpha_1) \cdot \left[m_1 + \frac{m_2}{R} \right] \tag{C.3}$$

where R is the discount rate. The lender may instead choose to modify the loan, which reduces subsequent loan payments to $m_1' < m_1$ and $m_2' < m_2$.

Lenders will be willing to modify a mortgage when modification is very effective in reducing the probability of non-payment or when the necessary reduction in the value of the loan is small. By lowering payments, modification reduces the probability of default to $\alpha_1' < \alpha_1$.[6]Then the value of the modified loan is:

$$V_M = \alpha_1'(P - \lambda) + (1 - \alpha_1') \left[m_1' + \frac{m_2'}{R} \right]$$

Thus, the lender will choose modification when the value of the modified loan is larger than the unmodified loan, or the difference between the two is positive:

$$V_M - V_u = (\alpha_1 - \alpha_1') \cdot \left[m_1' + \frac{1}{R} m_2' - (P - \lambda) \right] - (1 - \alpha_1) \cdot \left[m_1 + \frac{1}{R} m_2 - (m_1' + \frac{1}{R} m_2') \right] > 0 \tag{C.4}$$

Since $\alpha_1 - \alpha_1' > 0$, $1 - \alpha_1 > 0$, and $\left[m_1 + \frac{1}{R} m_2 - (m_1' + \frac{1}{R} m_2') \right] > 0$, even if the probability of re-default under modification is zero (i.e., even if $\alpha_1' = 0$), modification is still not optimal if the modified payments are too low. For example, lenders will never modify if the net-of-foreclosure-cost value of the property is greater than the present value of the modified mortgage payments (i.e., $m_1' + \frac{1}{R} m_2' < (P - \lambda)$). When the value of the modified payments

6. Adelino, Gerardi, and Willen 2009 allow home prices to change across periods, however, for the purpose of this paper little generality is lost by assuming no price growth. Similarly, Adelino, Gerardi, and Willen 2009 operate under the assumption that modification guarantees payment in period 1 with a non-zero probability of default in period 2. Again, this does not fundamentally change the implications of the framework.

are high enough, lenders are more inclined to modify when it is very effective in reducing the probability of redefault (i.e., the smaller is α_1').

Assuming that the default probabilities are constant (which shuts down the borrower contagion channel), a neighboring completed home foreclosure—which lowers the value of the property under consideration—discourages new foreclosure filings by lenders. Taking the derivative of $V_M - V_u$ with respect to the value of the home, we see that $\frac{\partial V_M - V_u}{\partial P} = \alpha_1' - \alpha_1 < 0$, relying on the assumption that modification lowers the probability of future default. A drop in the value of the home encourages mortgage modification—selling the property at auction is relatively less appealing to the lender than the modified mortgage.

We derive similar conditions for modification by mortgage servicers. Mortgage servicers are typically employed by lenders to collect mortgage payments and to manage mortgage defaults. When a mortgage is current, servicers receive a share of the interest payments that they collect. However, when a borrower is delinquent, servicers are required to forward payments to the holder of the debt while they manage the default (either by modifying the loan or seeking foreclosure). While managing a default, the servicer must incur all associated costs (e.g. legal fees). If the default ends in foreclosure, the servicer is reimbursed for all foreclosure-related expenses and fees. Thus, the value to the servicer of foreclosing on the delinquent loan (as in the above framework) is:

$$V_u^s = \alpha_1 \cdot \Pi + (1 - \alpha_1) \cdot \left[\rho \cdot m_1 + \rho \cdot \frac{m_2}{R} \right] \tag{C.5}$$

where Π is the total value of all foreclosure-related fees charged by the servicer and where ρ is the share of the mortgage payment that is returned to the servicer. When the loan is modified, the servicer is generally not reimbursed for any related expenses, and so the value of the modified loan to the servicer is:

$$V_M^s = \alpha_1' \cdot \Pi + (1 - \alpha_1') \left[\rho \cdot m_1' + \rho \cdot \frac{m_2'}{R} \right] - C_M$$

where C_M is the cost of modification (e.g., time/labor spent in negotiations). Notice that if the modification is successful, the servicer receives a lower monthly payment for servicing the mortgage. Then the servicer will prefer to modify when:

$$V_M^s - V_u^s = (\alpha_1' - \alpha_1) \cdot \left[\rho m_1' + \frac{\rho}{R} m_2' - \Pi \right] - (1 - \alpha_1) \cdot \rho \left[m_1 + \frac{1}{R} m_2 - \left(m_1' + \frac{1}{R} m_2' \right) \right] - C_m > 0$$

The servicer's incentives are similar to the lender's incentives, although as pointed out by Levitin and Twomey 2011, since servicers' fees have seniority over all other claims against a property, servicers are "indifferent to the amount of the [foreclosure] sale proceeds." In other words, since the payoff to a servicer of a completed foreclosure is the foreclosure fees, the servicer does not care about the value of the property (as long as it is high enough to cover their fees). Moreover, high costs of modification (which are not reimbursed) will push the servicer in favor of foreclosure. Notice, then, that if the probabilities of foreclosure (α_1 and α_1') are invariant to a neighboring completed foreclosure, the servicer will not experience foreclosure contagion ($\partial \left(V_M^s - V_u^s \right) / \partial P = 0$).

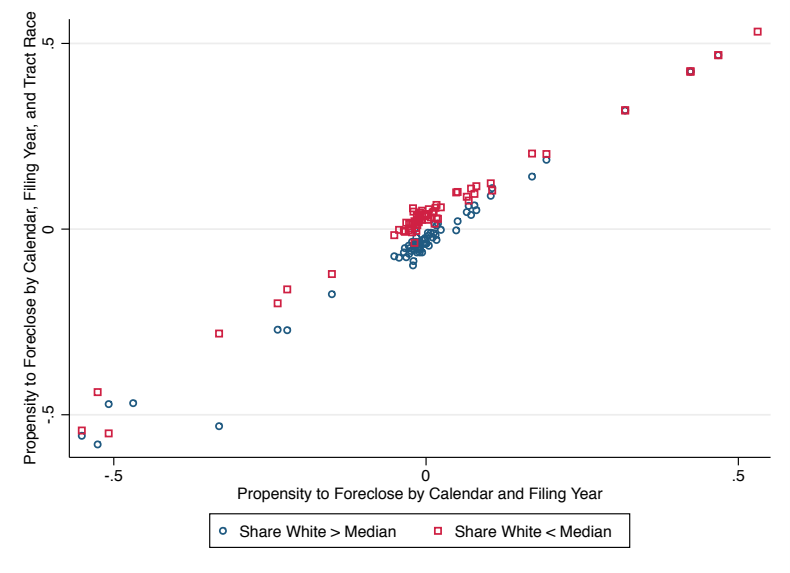
In summary, under the assumptions that a neighboring completed home foreclosure lowers the housing value P and that borrowers are unresponsive to the neighboring foreclosure so that α_1 and α_1' are unchanged, we should observe "anti-contagion" in home foreclosures: if only lenders and servicers are responding, a completed home foreclosure should discourage neighboring foreclosure filings. Of course, if a neighboring completed foreclosure has no influence on housing values and borrowers are unresponsive, then lenders and servicers should not respond (we should see no contagion at all).

C.6 Estimates by Proxies for Social Connectedness

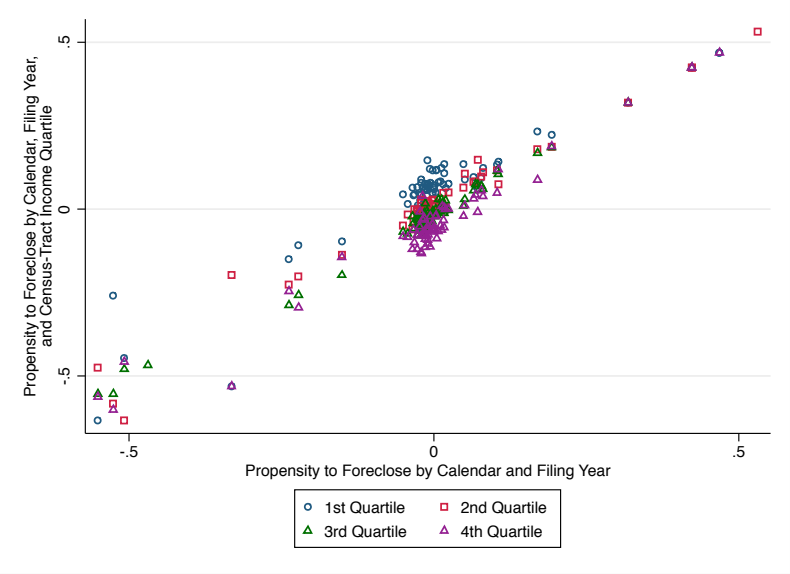
We find little systematic relationship between the extent of foreclosure contagion and several proxies for social connection. Our first attempt to proxy social connectedness is to stratify by neighborhood diversity, operating under the assumption that neighbors with similar background maintain stronger social ties. For each census tract, we calculate a Herfindahl index of neighborhood diversity using race-population shares from the 2000 Decennial Census. We then estimate the baseline treatment effect for each diversity quartile to examine whether contagion is stronger in less diverse (high-index) census tracts. We also estimate the baseline specification and interact the treatment effect with an indicator for the foreclosure taking place in a census tract where a single race makes up more than 75% of the population. Our second set of proxies draw on the notion of social capital outlined by Glaeser and Sacerdote 2000, who argue that social connections are higher when residents live in close proximity to one another. For example, studying survey data, Glaeser and Sacerdote 2000 find that there are higher levels of civic participation among residents of large condo buildings than single-family housing. We proxy neighbor proximity in two ways. First, we interact the foreclosure treatment effect with an indicator that the home undergoing foreclosure (the unit of observation) is a condominium unit. Second, we stratify census tracts by population density (again using data from the 2000 Decennial Census). In both of these cases, while we continue to find evidence of contagion, there is no systematic relationship between foreclosure contagion and our proxies for social connection. Our social-contagion estimates can be found in Table C.31 and C.32, and Figures C.5 and C.6 of the Appendix.

Figure C.2: Calendar-Group-Specific Foreclosure Rates vs. Calendar-Specific Rates

(a)



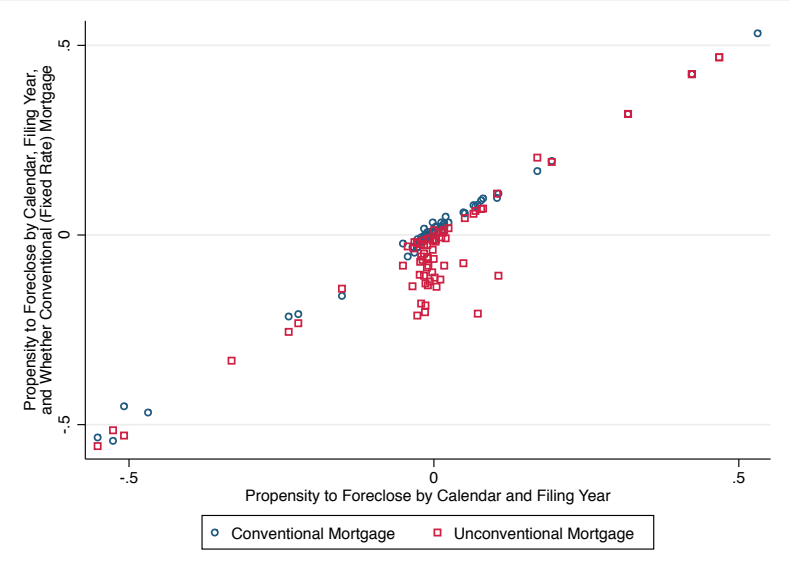
(b)



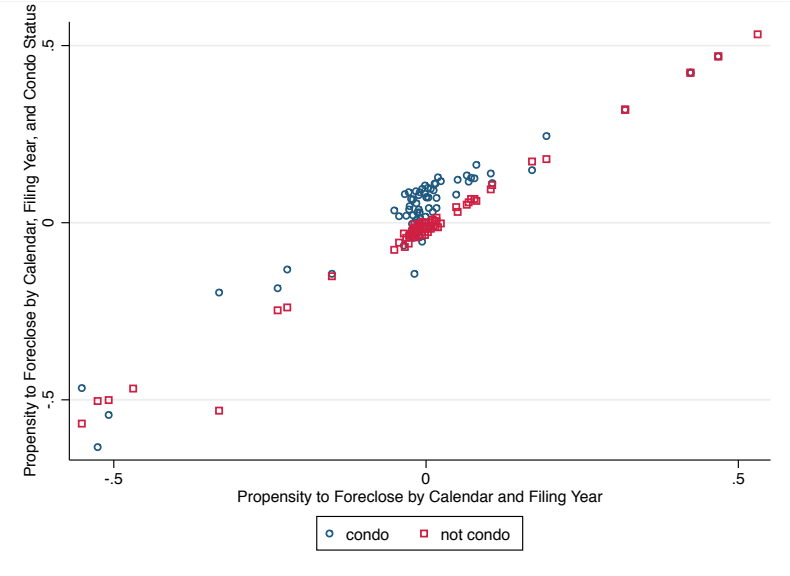
Notes: Filing-year foreclosure rates are calculated for each indicated sub-group by case calendar, demeaned by filing-year, and plotted against overall foreclosure rates for the given calendar and filing year (again, demeaned by filing year). A predominantly white census tract has an above-median share of white residents as of the 2000 Decennial Census, and income quartiles are calculated at the census tract level using median income from the 2000 Decennial Census.

Figure C.3: Calendar-Group-Specific Foreclosure Rates vs. Calendar-Specific Rates

(a)



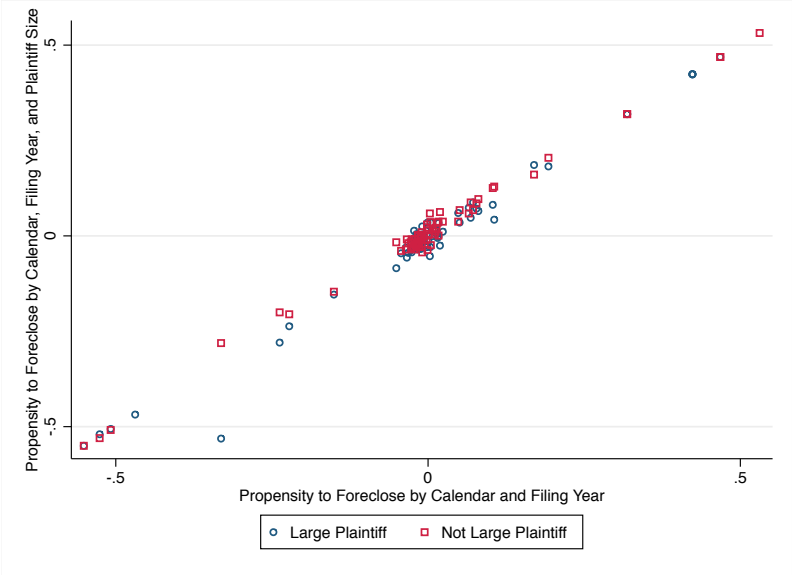
(b)



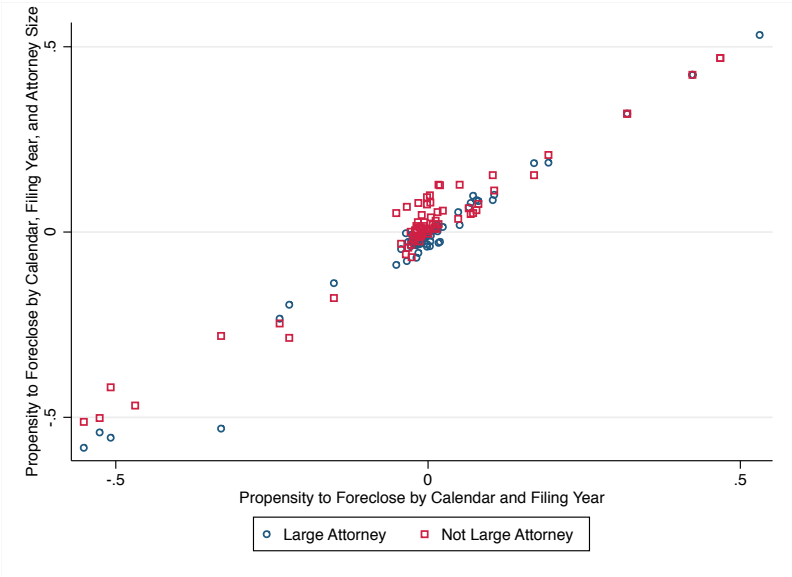
Notes: Filing-year foreclosure rates are calculated for each indicated sub-group by case calendar, demeaned by filing-year, and plotted against overall foreclosure rates for the given calendar and filing year (again, demeaned by filing year). Conventional mortgage includes all standard fixed-rate mortgages (while unconventional mortgages includes adjustable-rate mortgages, balloon-payment mortgages, reverse mortgages, and interest-only mortgages; we exclude VA mortgages). Condo status is as reported in the court documents.

Figure C.4: Calendar-Group-Specific Foreclosure Rates vs. Calendar-Specific Rates

(a)



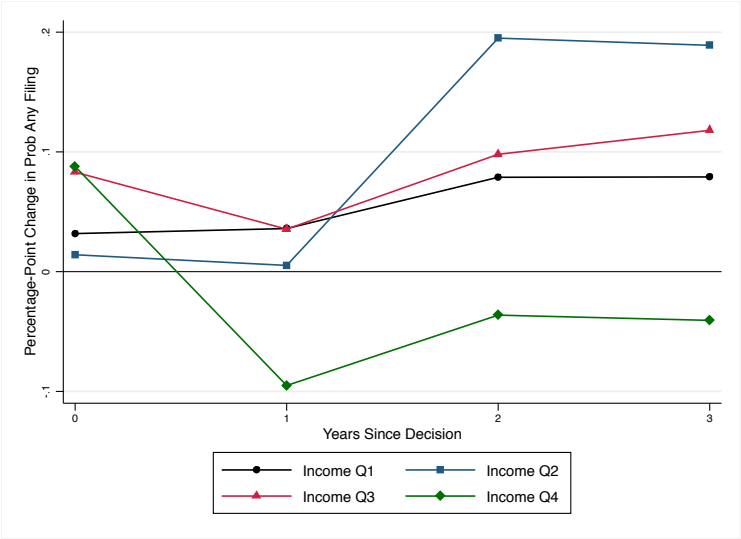
(b)



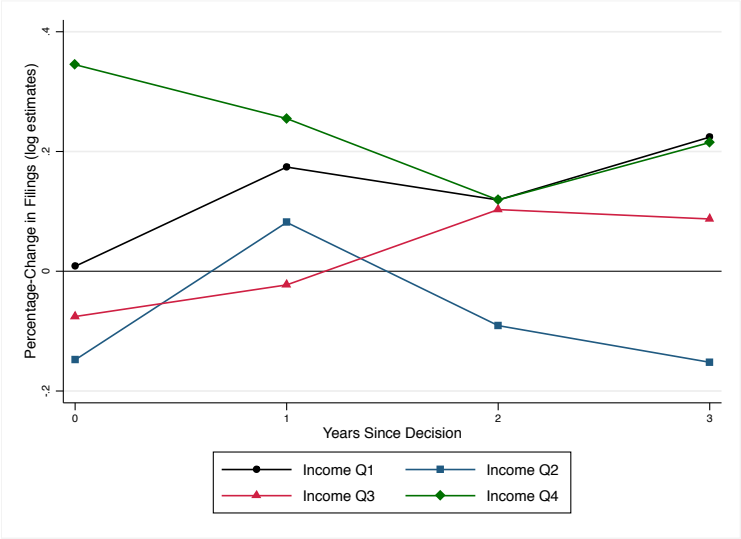
Notes: Filing-year foreclosure rates are calculated for each indicated sub-group by case calendar, demeaned by filing-year, and plotted against overall foreclosure rates for the given calendar and filing year (again, demeaned by filing year). A large lender is a plaintiff who appears on more than 7000 of the foreclosure cases filed in cook county, while a large attorney appears on greater than 10,000 cases.

Figure C.5: Contagion Estimates by Income Quartile

(a)



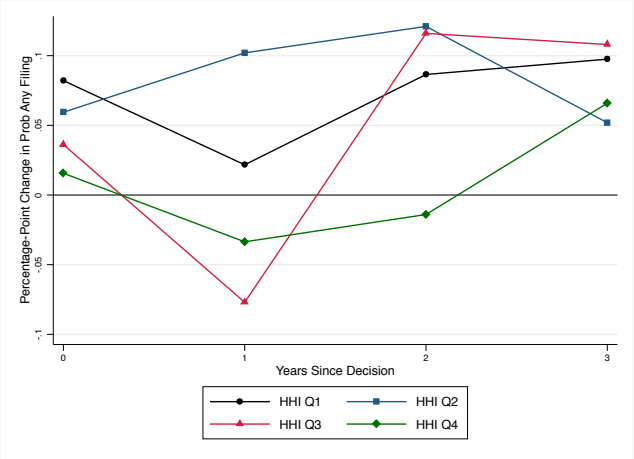
(b)



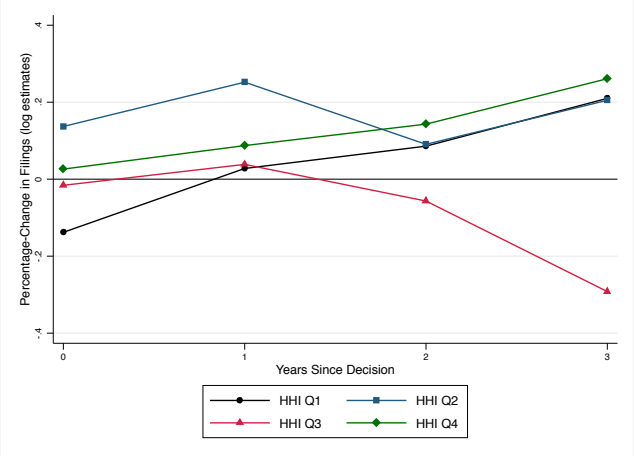
Notes: 2SLS estimates (as described in Tables C.9 and C.5) performed separately for each value of the given neighborhood quartile of income: income quartiles are calculated at the census tract level using median income from the 2000 Decennial Census.

Figure C.6: Contagion Estimates by Social Connection Proxy

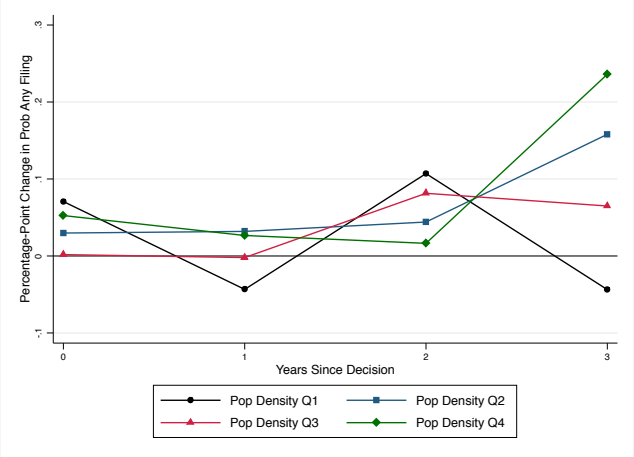
(a)



(b)



(c)



(d)

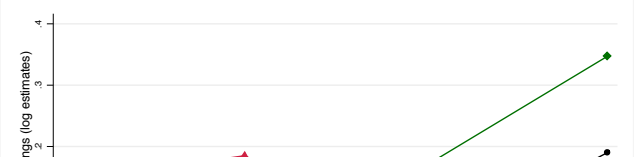


Table C.13: First Stage Regression of Foreclosure on Propensity to Foreclose

	No Property Controls	Controls†
Coefficient on Z_i	0.556***	0.554***
	(0.045)	(0.046)
1st-Stage F	150.000	147.400
N	140667	140667

Notes: ***Indicates significance at the 1% level. Reported standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. First-stage regression of indicator for case ending in foreclosure on leave-one-out case-calendar-filing-month foreclosure rate, and filing month and property type fixed effects. †Unreported controls include share of tract that report race as white in 2000 decennial census, income quartile from decennial census, whether plaintiff is a "large plaintiff" (six largest plaintiffs each representing $\geq$ 7000 filings) or attorney is a "large attorney" (three largest attorneys each representing $\geq$10,000 cases), whether mortgage is adjustable rate, size of initial loan, and census tract population.

Table C.14: Contagion Estimates Measured Since Case Filing

Years Since Filing		0	1	2	3	4	5
Any Filing	2SLS	0.057***	0.032	0.026	0.025	0.076	0.024
		(0.021)	(0.021)	(0.022)	(0.026)	(0.076)	(0.087)
	1st-stage F	388.100	388.100	399	255.500	30.450	22.310
	N	140683	140683	121171	98032	59967	36685
Total Filings	2SLS	0.631*	0.867**	0.334	0.276*	0.023	0.456
		(0.354)	(0.343)	(0.260)	(0.152)	(0.440)	(0.728)
	1st-stage F	388.100	388.100	399	255.500	30.450	22.310
	N	140683	140683	121171	98032	59967	36685

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates for given completed foreclosure outcome are as in Table C.5. Outcomes (and year-of-observation fixed effects) are measured as of the year that the case is filed (rather than the year that the case is decided).

Table C.15: Controlling for Length of Case

Years Since Decision		0	1	2	3	4	5
Control for Log Length of Case	Any Filing	0.049	-0.006	0.107**	0.125	0.357**	0.167
		(0.042)	(0.040)	(0.043)	(0.086)	(0.177)	(0.163)
	First-stage F	149.500	133.900	114.600	24.810	10.740	8.804
	N	129834	118201	92804	67076	41733	23666
	Total Filings	0.728	0.782	0.669**	0.928*	2.308	0.627
		(0.553)	(0.562)	(0.299)	(0.511)	(1.522)	(1.196)
	First-stage F	149.500	133.900	114.600	24.810	10.740	8.804
	N	129834	118201	92804	67076	41733	23666
	log(Price)	-0.102	-0.516	-0.390			
		(0.176)	(0.341)	(0.442)			
	First-stage F	19.320	7.391	12.570			
	N	42880	25890	12155			
Control for Quadratic in Length of Case	Any Filing	0.058	0.008	0.105***	0.124	0.401**	0.245
		(0.036)	(0.036)	(0.037)	(0.076)	(0.196)	(0.264)
	First-stage F	175.100	163.900	163.200	27.100	14.840	4.805
	N	130199	118566	93143	67379	41958	23831
	Total Filings	0.783	0.791	0.677***	0.890**	2.497	0.883
		(0.496)	(0.501)	(0.255)	(0.451)	(1.599)	(1.895)
	First-stage F	175.100	163.900	163.200	27.100	14.840	4.805
	N	130199	118566	93143	67379	41958	23831
	log(Price)	-0.113	-0.638	-0.511			
		(0.158)	(0.427)	(0.685)			
	First-stage F	21.270	6.922	7.338			
	N	43079	26047	12241			
Control for Quarterly Length Dummies	Any Filing	0.053	0.001	0.106**	0.141	0.463**	0.214
		(0.042)	(0.040)	(0.041)	(0.096)	(0.226)	(0.225)
	First-stage F	127	123.400	104.800	24.880	14.800	8.341
	N	130199	118566	93143	67379	41958	23831
	log(# of Filings)	0.808	0.842	0.672**	0.990*	2.819	0.803
		(0.567)	(0.556)	(0.288)	(0.576)	(1.956)	(1.660)
	First-stage F	127	123.400	104.800	24.880	14.800	8.341
	N	130199	118566	93143	67379	41958	23831
	log(Price)	-0.105	-0.619	-0.462			
		(0.197)	(0.404)	(0.562)			
	First-stage F	19.450	9.194	12.400			
	N	43079	26047	12241			

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates for given completed foreclosure outcome are as in Table C.5, including the indicated controls for length of the case—log of the total months, a quadratic in total months, or number-of-quarter fixed effects.

Table C.16: Robustness of Contagion Estimates By Specification

Years Since Decision		0	1	2	3	4	5
No Covariates	Any Filing	0.066**	0.029	0.096***	0.113**	0.253**	0.160
		(0.028)	(0.027)	(0.027)	(0.052)	(0.107)	(0.130)
	First-stage F	242.600	225	212.400	25.470	20.900	14.130
	N	130199	118566	93143	67379	41958	23831
	Total Filings	0.739*	0.741**	0.603***	0.796**	1.616*	0.621
		(0.388)	(0.375)	(0.196)	(0.320)	(0.954)	(0.972)
	First-stage F	242.600	225	212.400	25.470	20.900	14.130
	N	130199	118566	93143	67379	41958	23831
No Property-Type FEs	Any Filing	0.071**	0.029	0.095***	0.096*	0.244**	0.150
		(0.028)	(0.027)	(0.027)	(0.053)	(0.112)	(0.133)
	First-stage F	232.700	220.200	201	24.080	18.910	13.190
	N	130199	118566	93143	67379	41958	23831
	Total Filings	1.190***	1.065***	0.876***	0.868**	1.488	0.805
		(0.453)	(0.398)	(0.197)	(0.338)	(0.939)	(0.941)
	First-stage F	232.700	220.200	201	24.080	18.910	13.190
	N	130199	118566	93143	67379	41958	23831
Drop Cases Decided in Summer	Any Filing	0.068**	0.018	0.104***	0.089	0.171	0.085
		(0.034)	(0.032)	(0.034)	(0.058)	(0.117)	(0.137)
	First-stage F	179.200	168.800	162.300	25.500	19.180	14.600
	N	105638	95487	75004	55342	34719	19464
	Total Filings	0.914**	0.851**	0.633***	0.563	1.126	0.100
		(0.430)	(0.406)	(0.230)	(0.354)	(0.932)	(1.078)
	First-stage F	179.200	168.800	162.300	25.500	19.180	14.600
	N	105638	95487	75004	55342	34719	19464
Monotonicity-Robust IV	Any Filing	0.083	0.118**	0.026	-0.088	-0.130	-0.417
		(0.062)	(0.057)	(0.062)	(0.109)	(0.296)	(0.662)
	First-stage F	45.180	43.800	37.200	14.260	4.733	1.432
	N	127521	115977	90795	65515	40534	22821
	Total Filings	3.471***	2.067***	1.041	-0.263	-1.259	-7.345
		(1.113)	(0.675)	(0.674)	(1.149)	(3.539)	(9.501)
	First-stage F	45.180	43.800	37.200	14.260	4.733	1.432
	N	127521	115977	90795	65515	40534	22821

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates for given outcome are as in Table C.5, with the following adjustments: "No covariates" omits the case-level controls as outlined in Table C.5; "No Property-Type FEs" omits the property-type (condo/single-family/multi-family/apartment) fixed effects; "Drop Summer Decisions" drops cases that are decided in June, July, or August (during which the courts dismiss inactive cases); "Monotonicity-Robust IV" indicates that the instrument is constructed by calendar/filing month/income quartile/large plaintiff/"white" census tract cells.

Table C.17: Robustness of Estimates by Sample

Years Since Decision		0	1	2	3	4	5
Include Moratorium	Any Filing	0.056**	0.009	0.083***	0.093*	0.280**	0.160
		(0.028)	(0.028)	(0.027)	(0.054)	(0.115)	(0.137)
	First-stage F	188.700	179.600	202.400	24.060	18.630	12.440
	N	140912	127802	94495	67749	42249	24048
	Total Filings	0.649	0.649*	0.553***	0.700**	1.757*	0.693
		(0.399)	(0.375)	(0.184)	(0.318)	(1.019)	(1.013)
	First-stage F	188.700	179.600	202.400	24.060	18.630	12.440
	N	140912	127802	94495	67749	42249	24048
	log(Price)	-0.122	-0.418**	-0.378			
		(0.106)	(0.210)	(0.409)			
	First-stage F	21.490	18.220	14.660			
	N	43343	26236	12343			
Full Sample	Any Filing	0.078***	0.064***	0.071***	0.058***	0.035	0.017
		(0.022)	(0.019)	(0.018)	(0.021)	(0.025)	(0.022)
	First-stage F	202.200	201.500	225.100	144.800	125	127.600
	N	152559	139324	105628	77785	50704	31508
	Total Filings	0.402**	0.363**	0.410***	0.376**	0.360**	0.241
		(0.204)	(0.177)	(0.124)	(0.163)	(0.157)	(0.204)
	First-stage F	202.200	201.500	225.100	144.800	125	127.600
	N	152559	139324	105628	77785	50704	31508
	log(Price)	-0.159***	-0.199***	-0.222***			
		(0.038)	(0.036)	(0.036)			
	First-stage F	131.300	121.900	132.700			
	N	50879	32750	17952			
Filings ¡ 99th Percentile	Any Filing	0.048*	0.008	0.081***	0.089	0.251**	0.140
		(0.028)	(0.027)	(0.028)	(0.055)	(0.115)	(0.135)
	First-stage F	247.600	229.100	207.200	24.470	18.120	12.920
	N	127046	116434	91674	66454	41416	23533
	Total Filings	0.144	0.234	0.395***	0.318	0.887	0.231
		(0.194)	(0.174)	(0.128)	(0.225)	(0.561)	(0.568)
	First-stage F	247.600	229.100	207.200	24.470	18.120	12.920
	N	127046	116434	91674	66454	41416	23533

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates for given outcome are as in Table C.5, with the following sample adjustments (recall that the baseline sample omits cases filed during the foreclosure moratorium of 2009, omits filings against VA loans, and omits cases filed during the first year of a case-calendar's existence): "Include Moratorium" maintains the baseline sample, but includes cases filed during the 2009 foreclosure moratorium; "Full Sample" relies on the set of all matched foreclosure cases in Cook County for 2004–2010; "Filings ¡ 99th Percentile" drops observations where the number of new foreclosure filings near a case in a given year is greater than the 99th percentile.

Table C.18: Contagion in Any New Filing, Omitting Each Filing Year

Years Since Decision	0	1	2	3	4	5
No 2004	0.044	0.014	0.078***	0.076	0.233*	0.167
	(0.028)	(0.027)	(0.027)	(0.054)	(0.130)	(0.174)
First-stage F	221.700	208.300	190.800	21.330	15.180	10.010
N	124342	112717	87309	61592	36265	18391
No 2005	0.046*	0.009	0.072**	0.109*	0.349**	-0.735
	(0.028)	(0.027)	(0.028)	(0.063)	(0.171)	(1.183)
First-stage F	258.600	248	247.400	22.270	8.089	0.937
N	116682	105064	79676	54020	28854	11860
No 2006	0.053*	0.006	0.078***	0.060	0.189*	0.233*
	(0.027)	(0.026)	(0.027)	(0.047)	(0.103)	(0.132)
First-stage F	267.700	255	249	28.580	26.780	13.700
N	112963	101380	76056	50592	26403	17411
No 2007	0.061**	0.015	0.073***	0.072	0.316*	0.140
	(0.029)	(0.028)	(0.028)	(0.052)	(0.185)	(0.133)
First-stage F	176.900	167.300	172.700	21.770	8.266	12.970
N	107066	95565	70614	46462	34353	23831
No 2008	0.079	0.003	0.224***	0.242*	0.247**	0.140
	(0.073)	(0.072)	(0.081)	(0.126)	(0.112)	(0.133)
First-stage F	62.950	55.030	34.530	16.530	19.060	12.970
N	93508	83050	62869	56850	41957	23831
No 2009	0.055*	0.018	0.078***	0.090*	0.247**	0.140
	(0.028)	(0.028)	(0.029)	(0.053)	(0.112)	(0.133)
First-stage F	232.200	218	173.100	24.620	19.060	12.970
N	110228	101633	89191	67379	41958	23831
No 2010	0.043	0.012	0.082***	0.090*	0.247**	0.140
	(0.028)	(0.027)	(0.027)	(0.053)	(0.112)	(0.133)
First-stage F	245.500	224.400	205	24.620	19.060	12.970
N	116405	111987	93143	67379	41958	23831

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates for any new neighboring filing are as in Table C.5, although omitting cases filed in the given year.

Table C.19: Contagion in Total New Filings, Omitting Each Filing Year

Years Since Decision	0	1	2	3	4	5
No 2004	0.658	0.642*	0.462**	0.531*	1.464	-0.253
	(0.403)	(0.376)	(0.182)	(0.318)	(1.181)	(0.253)
First-stage F	221.700	208.300	190.800	21.330	15.180	20.300
N	124342	112717	87309	61592	36265	13557
No 2005	0.667	0.653*	0.531***	0.840**	1.609	0.571
	(0.407)	(0.386)	(0.189)	(0.341)	(1.157)	(1.206)
First-stage F	258.600	248	247.400	22.270	8.089	1.423
N	116682	105064	79676	54020	28854	8716
No 2006	0.695*	0.650*	0.501***	0.597**	1.524	-0.003
	(0.396)	(0.373)	(0.175)	(0.286)	(0.946)	(0.231)
First-stage F	267.700	255	249	28.580	26.780	24.660
N	112963	101380	76056	50592	26403	12983
No 2007	0.659	0.686*	0.528***	0.529*	1.882	-0.062
	(0.427)	(0.405)	(0.187)	(0.314)	(1.594)	(0.217)
First-stage F	176.900	167.300	172.700	21.770	8.266	24.500
N	107066	95565	70614	46462	34353	17628
No 2008	1.118*	0.635	1.192**	1.178	1.551	-0.062
	(0.654)	(0.517)	(0.573)	(1.012)	(0.983)	(0.217)
First-stage F	62.950	55.030	34.530	16.530	19.060	24.500
N	93508	83050	62869	56850	41957	17628
No 2009	0.749*	0.768**	0.524***	0.657**	1.551	-0.062
	(0.394)	(0.378)	(0.199)	(0.319)	(0.983)	(0.217)
First-stage F	232.200	218	173.100	24.620	19.060	24.500
N	110228	101633	89191	67379	41958	17628
No 2010	0.627*	0.667*	0.536***	0.657**	1.551	-0.062
	(0.375)	(0.367)	(0.183)	(0.319)	(0.983)	(0.217)
First-stage F	245.500	224.400	205	24.620	19.060	24.500
N	116405	111987	93143	67379	41958	17628

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates for count of filings per year are as in Table C.5, although omitting cases filed in the given year.

Table C.20: Baseline Estimates for 0.25 Mile Radius

Years Since Decision		0	1	2	3	4	5
Any Filing per Year	2SLS	0.017	-0.003	0.016	0.026	0.032	-0.064
		(0.012)	(0.011)	(0.011)	(0.016)	(0.041)	(0.052)
	1st-stage F	238.300	224.200	205	24.620	19.060	12.970
	N	130199	118566	93143	67379	41958	23831
Total Filings per Year	2SLS	2.187***	2.358***	1.693***	1.219	2.665	0.762
		(0.777)	(0.785)	(0.520)	(0.764)	(1.792)	(2.217)
	1st-stage F	238.300	224.200	205	24.620	19.060	12.970
	N	130199	118566	93143	67379	41958	23831
log(price)	2SLS	-0.161**	-0.269*	-0.410*			
		(0.067)	(0.144)	(0.220)			
	1st-stage F	24.760	17.870	16.860			
	N	63639	39508	21860			

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates for any new neighboring filing are as in Table C.5, although defining outcomes using a 0.25 mile radius around delinquent property.

Table C.21: Estimates for Loans with Multiple Claimants

Years Since Decision	0	1	2	3	4	5
Any Filing	0.076*	0.007	0.087**	0.071	0.112	0.058
	(0.039)	(0.038)	(0.039)	(0.075)	(0.143)	(0.164)
First-stage F	210.100	213.600	175.800	23.360	16.810	12.410
N	65366	59917	48271	34865	21252	11377
95% CI Lower Bound	0.154	0.081	0.163	0.217	0.392	0.379
Total Filings per Year	0.820	0.669	0.454	0.470	2.026	0.707
	(0.575)	(0.511)	(0.286)	(0.495)	(1.634)	(1.267)
First-stage F	210.100	213.600	175.800	23.360	16.810	12.410
N	65366	59917	48271	34865	21252	11377
95% CI Upper Bound	1.947	1.671	1.015	1.440	5.229	3.190
log(price)	0.035	-0.172	-0.198			
	(0.147)	(0.231)	(0.346)			
First-stage F	19.580	24.230	18.530			
N	22348	13423	6071			
95% CI Upper Bound	-0.254	-0.625	-0.876			

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates for any new neighboring filing are as in Table C.5. Sample is restricted to cases where multiple lien holders are listed in the foreclosure filing.

Table C.22: Contagion in Completed Foreclosures/Dismissals Filed After Decision

Years After End of Case	0	1	2	3	4	5
	Contagion in Completed Foreclosures Filed After Decision					
Any Completed Foreclosure	-0.015	-0.150***	0.054	0.186***	-0.023	0.119
	(0.011)	(0.032)	(0.035)	(0.055)	(0.115)	(0.154)
First-stage F	238.300	224.200	205	24.620	19.060	12.970
N	130199	118566	93143	67379	41958	23831
Total Completed Foreclosures	-0.028*	-0.285**	0.323**	0.705***	-0.044	0.180
	(0.014)	(0.129)	(0.160)	(0.211)	(0.389)	(0.539)
First-stage F	238.300	224.200	205	24.620	19.060	12.970
N	130199	118566	93143	67379	41958	23831
	Contagion in Dismissals Filed After Decision					
Any Dismissal	-0.018**	-0.029	-0.011	0.034	0.047	0.077
	(0.008)	(0.023)	(0.017)	(0.030)	(0.072)	(0.095)
First-stage F	238.300	224.200	205	24.620	19.060	12.970
N	130199	118566	93143	67379	41958	23831
Total Dismissals	-0.026**	-0.050	-0.026	0.049	0.003	0.216
	(0.011)	(0.036)	(0.028)	(0.059)	(0.127)	(0.176)
First-stage F	232.700	234.500	172.700	25.030	15.020	8.676
N	94866	84906	66291	46383	28310	15230

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates for given completed foreclosure or dismissal outcome are as in Table C.5. Dismissal outcomes are counted in the same way as foreclosure outcomes: number of dismissals or indicator for any dismissal (among cases filed after observation case is decided).

Table C.23: Robustness of Price Estimates By Specification

Years Since Decision		0	1	2
No Covariates	log(Price)	-0.153	-0.439**	-0.374
		(0.113)	(0.214)	(0.394)
	First-stage F	23.290	20.600	15.210
	N	43079	26047	12241
No Property-Type FEs	log(Price)	-0.113	-0.416**	-0.361
		(0.106)	(0.204)	(0.397)
	First-stage F	22.140	19.140	14.580
	N	43079	26047	12241
Drop Summer Decisions	log(Price)	-0.127	-0.482**	-0.549
		(0.122)	(0.240)	(0.361)
	First-stage F	22.860	18.330	9.793
	N	31977	19176	8837
Monotonicity-Robust IV	log(Price)	-0.200	-0.929*	-0.337
		(0.256)	(0.502)	(0.678)
	First-stage F	12.080	5.523	3.730
	N	41593	24902	11517

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates for given outcome are as in Table C.5, with the following adjustments: "No covariates" omits the case-level controls as outlined in Table C.5; "No Property-Type FEs" omits the property-type (condo/single-family/multi-family/apartment) fixed effects; "Drop Summer Decisions" drops cases that are decided in June, July, or August (during which the courts dismiss inactive cases); "Monotonicity-Robust IV" indicates that the instrument is constructed by calendar/filing month/income quartile/large plaintiff/"white" census tract cells.

Table C.24: Price Effects, Omitting Each Filing Year

Years Since Decision	0	1	2
No 2004	-0.068	-0.418*	-0.207
	(0.111)	(0.252)	(0.464)
First-stage F	17.390	12.490	12.150
N	38223	21575	8701
No 2005	-0.140	-0.282	-2.818
	(0.127)	(0.405)	(2.370)
First-stage F	15.530	5.944	1.824
N	32114	16558	6011
No 2006	-0.143	-0.393**	-0.156
	(0.101)	(0.182)	(0.341)
First-stage F	28.770	29.790	13.780
N	30601	17345	9770
No 2007	-0.113	-0.531*	-0.358
	(0.112)	(0.292)	(0.395)
First-stage F	17.230	9.746	14.390
N	32369	22664	12241
No 2008	-0.206	-0.411**	-0.358
	(0.206)	(0.203)	(0.395)
First-stage F	13.530	18.830	14.390
N	39009	26046	12241

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates for log of neighboring residential real estate sale prices are as in Table C.5, although omitting cases filed in the given year.

Table C.25: Sub-Sample Price Effects

Years Since Decision		0	1	2
log(price)	Repeat Sample, Unadjusted	-0.073	-0.395	-0.322
		(0.115)	(0.247)	(0.440)
	1st-stage F	22.070	11.470	13.980
	N	30482	17916	7904
log(price)	Non-Repeat Sample	-0.235	-0.291	-0.705
		(0.146)	(0.274)	(0.518)
	1st-stage F	24.040	16.870	9.938
	N	30522	17925	8143

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates for given completed foreclosure or dismissal outcome are as in Table C.5, although using the given sample. "Repeat sample" includes all properties for which we observe at least one other sale; "non-repeat" is all other sales.

Table C.26: Contagion Among Loans with Lenders Implicated in Independent Foreclosure Review Settlement

Years Since Decision	0	1	2	3	4	5
Any Filing per Year	0.024	0.038	0.068	0.091	0.059	0.009
	(0.030)	(0.030)	(0.051)	(0.111)	(0.153)	(0.680)
First-stage F	269.300	161.600	25.060	19.060	12.970	2.765
N	94735	83564	67028	41958	23831	9579
Total Filings per Year	0.121	0.115	0.139	0.033	0.226	0.357
	(0.103)	(0.091)	(0.086)	(0.224)	(0.283)	(1.240)
First-stage F	269.300	161.600	25.060	19.060	12.970	2.765
N	94735	83564	67028	41958	23831	9579
Log of Filings per Year	0.094	0.089	0.064	0.034	0.174	0.024
	(0.068)	(0.068)	(0.084)	(0.281)	(0.225)	(0.396)
First-stage F	312.200	139.700	18.160	8.790	10.440	5.390
N	27570	23426	17780	11119	6159	2551
Log of Cumulative Filings since Decision	0.083	0.155**	0.258***	0.252	0.680**	1.911
	(0.079)	(0.070)	(0.083)	(0.228)	(0.319)	(1.454)
First-stage F	221.700	167.300	22.240	20.410	15.760	3.238
N	27929	36395	34539	23998	14714	6317

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS specification as in Table C.5. Outcomes for "Automating Lenders" are measured based on new foreclosure filings by lenders implicated in the Office of the Comptroller of the Currency and the Board of Governors of the Federal Reserve System Independent Foreclosure Review Settlement, while "Baseline" includes new foreclosure filings among all lenders. Both samples are restricted to cases decided between 2007 and 2010 (during which automation of foreclosure filings is thought to be most common).

Table C.27: Contagion among Borrowers in Positive vs. Negative Equity

Years Since Decision	0	1	2	3	4	5
Any Filing From Non-Underwater Borrower	0.044	0.037	0.105***	0.131**	0.175	0.191
	(0.028)	(0.028)	(0.029)	(0.056)	(0.111)	(0.145)
Any Filing From Underwater Borrower	0.043	-0.003	0.010	0.073	0.189	-0.251
	(0.030)	(0.029)	(0.027)	(0.052)	(0.153)	(0.155)
First-stage F	119.200	112.100	102.500	12.310	9.534	6.486
p val for difference between groups	0.985	0.187	0.000	0.286	0.917	0.010
N	260398	237132	186286	134758	83916	47662
Total Filings From Non-Underwater Borrowers	0.785**	0.736**	0.619***	0.650**	0.921	0.836
	(0.352)	(0.319)	(0.179)	(0.268)	(0.748)	(0.822)
Total Filings From Underwater Borrowers	-0.094	-0.066	-0.082	0.007	0.629*	-0.298
	(0.089)	(0.084)	(0.058)	(0.109)	(0.362)	(0.329)
First-stage F	119.200	112.100	102.500	12.310	9.534	6.486
p val for difference between groups	0.006	0.005	0.000	0.006	0.605	0.077
N	260398	237132	186286	134758	83916	47662
Log of Filings From Non-Underwater Borrowers	0.059	0.149**	0.106**	0.119	0.097	0.086
	(0.067)	(0.060)	(0.053)	(0.089)	(0.217)	(0.228)
Log of Filings From Underwater Borrowers	-0.078	0.025	0.030	-0.004	0.546**	-0.020
	(0.053)	(0.050)	(0.052)	(0.073)	(0.272)	(0.199)
First-stage F	127.300	107.400	83.690	15.110	4.309	14.370
p val for difference between groups	0.080	0.018	0.281	0.217	0.103	0.641
N	127218	116391	91160	65799	41166	23303

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates as in Table C.5. Outcomes are for two separate counts of new foreclosure filings by either borrowers in negative equity or borrowers in positive equity (as defined in the text). P-value tests the significance between the two responses (positive vs. negative equity)—estimates are performed simultaneously (pooling both outcomes and allowing for differing filing month and year of observation fixed effects).

Table C.28: Interaction with Positive Price Growth

Years Since Decision	0	1	2	3	4	5
Any Filing	0.046	0.017	0.068**	0.076	0.227**	0.121
	(0.028)	(0.028)	(0.027)	(0.051)	(0.112)	(0.134)
Interaction of Foreclosure with Positive Price Growth	-0.042	-0.052	0.528*	0.094	-0.125	-0.087
	(0.065)	(0.045)	(0.308)	(0.080)	(0.141)	(0.109)
First-stage F	75.470	98.680	3.115	12.140	9.705	6.768
N	127980	116456	91358	65987	41063	23280
Total Filings	0.697	0.605*	0.255	0.609*	1.566*	0.385
	(0.549)	(0.364)	(0.232)	(0.324)	(0.946)	(0.999)
Interaction of Foreclosure with Positive Price Growth	-0.342	1.648	17.110*	2.593	-7.903*	-1.292
	(2.215)	(1.268)	(10.130)	(2.196)	(4.072)	(4.832)
First-stage F	75.470	98.680	3.115	12.140	9.705	6.768
N	127980	116456	91358	65987	41063	23280
Log of Filings in Zip Codes/Years with Negative Price Growth	-0.003	0.135**	0.069	0.176*	0.202	-0.093
	(0.074)	(0.061)	(0.054)	(0.091)	(0.225)	(0.217)
Interaction of Foreclosure with Positive Price Growth	0.156	0.084	1.805	0.328	-0.879	-0.021
	(0.281)	(0.150)	(1.182)	(0.238)	(0.606)	(0.386)
First-stage F	51.130	96.730	2.502	14.250	8.214	13.970
N	93583	85496	67258	48491	30449	17273

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates as in Table C.5, but allowing for the interaction of the foreclosure treatment with an indicator for positive price growth in that zip code in that year (from Zillow price indices).

Table C.29: Response Among Borrowers with Conventional Mortgages

Years Since Decision	0	1	2	3	4	5
Any Filing Among Borrowers with Non-Conv. Mortgages	0.022	0.050*	0.034	0.087	0.302**	0.060
	(0.028)	(0.030)	(0.030)	(0.054)	(0.133)	(0.158)
Any Filing Among Borrowers with Conventional Mortgages	0.019	0.023	0.094***	0.074	0.131	0.189
	(0.033)	(0.032)	(0.031)	(0.048)	(0.117)	(0.187)
First-stage F	119.200	112.100	102.500	12.310	9.534	6.486
p val for difference between groups	0.952	0.528	0.088*	0.821	0.209	0.529
N	260398	237132	186286	134758	83916	47662
Total Filings Among Borrowers with Non-Conv. Mortgages	0.226	0.391**	0.145	0.365*	0.894	0.223
	(0.212)	(0.181)	(0.093)	(0.193)	(0.598)	(0.491)
Total Filings Among Borrowers with Conventional Mortgages	0.465**	0.278	0.392***	0.292*	0.656	0.315
	(0.210)	(0.222)	(0.120)	(0.173)	(0.434)	(0.586)
First-stage F	119.200	112.100	102.500	12.310	9.534	6.486
p val for difference between groups	0.099*	0.503	0.017**	0.623	0.448	0.831
N	260398	237132	186286	134758	83916	47662
log(# of Filings) Among Borrowers with Non-Conv. Mortgages	-0.032	0.119**	0.075	0.119	0.242	-0.084
	(0.054)	(0.049)	(0.051)	(0.081)	(0.251)	(0.210)
log(# of Filings) Among Borrowers with Conventional Mortgages	0.125**	0.065	0.089*	0.134	0.231	-0.060
	(0.058)	(0.066)	(0.052)	(0.083)	(0.201)	(0.187)
First-stage F	117.800	96.760	72.820	14.810	8.370	13.850
p val for difference between groups	0.001***	0.357	0.791	0.838	0.959	0.885
N	136995	126760	100375	73225	45688	25633

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates as in Table C.5. Outcomes are for two separate counts of either new foreclosure filings among borrowers with conventional mortgages or filings among borrowers with unconventional mortgages. P-value tests the significance between the two responses (conventional vs. non-conventional)—estimates are performed simultaneously (pooling both outcomes and allowing for differing filing month and year of observation fixed effects).

Table C.30: Contagion among Filings with -10% – 10% Equity versus ¡ -10% or ¿ 10%

Years Since Decision	0	1	2	3	4	5
Any Filing with Equity ¡ -10% or ¿ 10%	0.024	-0.019	0.033	-0.019	0.150	-0.076
	(0.025)	(0.027)	(0.028)	(0.044)	(0.103)	(0.151)
Any Filing with Equity between -10% and 10%	0.042	0.038	0.083***	0.156***	0.188	0.161
	(0.028)	(0.027)	(0.032)	(0.053)	(0.115)	(0.150)
First-stage F	119.200	112.100	102.500	12.310	9.534	6.486
p val for difference between groups	0.582	0.077	0.187	0.001	0.727	0.267
N	260398	237132	186286	134758	83916	47662
Total Filings with Equity ¡ -10% or ¿ 10%	-0.233***	-0.212***	-0.085	0.028	0.228	-0.178
	(0.068)	(0.069)	(0.062)	(0.091)	(0.273)	(0.332)
Total Filings with Equity between -10% and 10%	0.924**	0.881***	0.621***	0.629**	1.323*	0.716
	(0.391)	(0.337)	(0.178)	(0.270)	(0.789)	(0.853)
First-stage F	119.200	112.100	102.500	12.310	9.534	6.486
p val for difference between groups	0.004	0.001	0	0.014	0.085	0.251
N	260398	237132	186286	134758	83916	47662
Log of Filings with Equity ¡ -10% or ¿ 10%	-0.168**	-0.012	-0.031	0.167**	0.037	0.105
	(0.079)	(0.052)	(0.051)	(0.081)	(0.237)	(0.209)
Log of Filings with Equity between -10% and 10%	0.070	0.155**	0.117**	0.107	0.264	-0.028
	(0.062)	(0.064)	(0.053)	(0.089)	(0.228)	(0.198)
First-stage F	58.860	101.600	90.690	14.320	3.189	10.910
p val for difference between groups	0.015	0.013	0.008	0.539	0.290	0.602
N	116814	108614	86569	63587	40557	23479

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates as in Table C.5. Outcomes are for two separate counts of new foreclosure filings by either borrowers where the difference between outstanding debt and loan principal is within 10% of initial loan principal and all other borrowers (see definition in the text). P-value tests the significance between the two responses—estimates are performed simultaneously (pooling both outcomes and allowing for differing filing month and year of observation fixed effects).

Table C.31: Contagion by Proxy for Neighborhood Homogeneity

Years Since Decision	0	1	2	3	4	5
Main Effect of Completed Foreclosure on Any Filing	0.037	0.031	0.090**	0.105	0.241*	0.185
	(0.040)	(0.038)	(0.038)	(0.066)	(0.141)	(0.185)
Interaction with Single Race ¿ 75% of Tract	0.025	-0.038	-0.020	-0.034	-0.008	-0.095
	(0.054)	(0.052)	(0.053)	(0.093)	(0.212)	(0.260)
First-stage F	108.600	99.770	84.300	9.773	9.330	5.559
N	130199	118566	93143	67379	41958	23831
Main Effect of Completed Foreclosure on Total Filings	0.404	0.834	0.570*	1.346**	1.677	-0.096
	(0.558)	(0.662)	(0.296)	(0.529)	(1.047)	(1.512)
Interaction with Single Race ¿ 75% of Tract	0.534	-0.338	-0.132	-1.385**	-0.394	1.216
	(0.813)	(0.650)	(0.369)	(0.630)	(1.416)	(1.810)
First-stage F	108.600	99.770	84.300	9.773	9.330	5.559
N	130199	118566	93143	67379	41958	23831
Main Effect of Completed Foreclosure on Log of Filings	0.011	0.172*	0.090	0.276**	0.252	-0.277
	(0.094)	(0.088)	(0.082)	(0.121)	(0.273)	(0.345)
Interaction with Single Race ¿ 75% of Tract	0.009	-0.082	-0.009	-0.257	-0.119	0.380
	(0.110)	(0.108)	(0.104)	(0.166)	(0.391)	(0.423)
First-stage F	95.590	96.070	72.730	11.620	7.097	10.310
N	95155	86961	68482	49422	31016	17628

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates as in Table C.5, but allowing for the interaction of the foreclosure treatment with indicator for property being in a census tract where one race makes up greater than 75% of the population.

Table C.32: Contagion by Condo Status

Years Since Decision	0	1	2	3	4	5
Main Effect of Completed Foreclosure on Any Filing	0.060**	0.014	0.074**	0.074	0.250**	0.155
	(0.029)	(0.028)	(0.029)	(0.057)	(0.116)	(0.137)
Interaction with Property Being a Condo	-0.045	-0.013	0.042	0.092	-0.017	-0.074
	(0.041)	(0.035)	(0.044)	(0.066)	(0.109)	(0.085)
First-stage F	120	112.400	102.400	12.400	9.603	6.414
N	130199	118566	93143	67379	41958	23831
Main Effect of Completed Foreclosure on Total Filings	0.563	0.261	0.079	0.549*	1.529*	0.248
	(0.392)	(0.296)	(0.192)	(0.331)	(0.916)	(1.032)
Interaction with Property Being a Condo	0.704	2.236**	2.446***	0.615	0.117	1.453
	(1.656)	(0.986)	(0.869)	(1.326)	(2.005)	(1.776)
First-stage F	120	112.400	102.400	12.400	9.603	6.414
N	130199	118566	93143	67379	41958	23831
Main Effect of Completed Foreclosure on Log of Filings	0.082	0.125**	0.024	0.106	0.112	-0.051
	(0.075)	(0.063)	(0.057)	(0.094)	(0.227)	(0.231)
Interaction with Property Being a Condo	-0.321	0.036	0.352**	0.307	0.501	-0.061
	(0.196)	(0.146)	(0.161)	(0.219)	(0.456)	(0.240)
First-stage F	139.100	107.200	97.200	14.660	8.312	12.390
N	95155	86961	68482	49422	31016	17628
	95155	86961	68482	49422	31016	17628

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates as in Table C.5, but allowing for the interaction of the foreclosure treatment with the given indicator that the property is a condo.

Table C.33: Contagion Among Borrowers with the Same Lender

Years Since Decision	0	1	2	3	4	5
Any Filing (Different Lender)	0.062**	0.037	0.073***	0.092*	0.287**	0.070
	(0.029)	(0.027)	(0.028)	(0.054)	(0.119)	(0.163)
Any Filing (Same Lender)	0.001	0.014	0.020	0.013	0.048	0.029
	(0.021)	(0.021)	(0.019)	(0.029)	(0.074)	(0.094)
First-stage F	124.700	112.400	112.700	13.060	8.506	4.524
p val for diff. = same	0.104	0.488	0.126	0.146	0.040	0.794
N	231920	211018	164628	117636	70910	38210
Total Filings (Different)	0.816**	0.887***	0.683***	0.742**	1.112	-0.081
	(0.362)	(0.343)	(0.182)	(0.347)	(0.700)	(1.108)
Total Filings (Same)	-0.194***	-0.185***	-0.160***	-0.116	0.024	0.063
	(0.061)	(0.062)	(0.050)	(0.083)	(0.190)	(0.254)
First-stage F	124.700	112.400	112.700	13.060	8.506	4.524
p val for diff. = same	0.007	0.002	0.000	0.012	0.018	0.860
N	231920	211018	164628	117636	70910	38210
log(# of Filings) (Different)	0.023	0.127**	0.121**	0.182**	0.145	-0.053
	(0.067)	(0.054)	(0.053)	(0.090)	(0.226)	(0.246)
log(# of Filings) (Same)	0.046	0.001	0.033	-0.154	-0.425	0.952
	(0.087)	(0.080)	(0.072)	(0.103)	(0.394)	(3.326)
First-stage F	55.050	107.600	41.270	15.800	5.857	0.056
p val for diff. = same	0.838	0.162	0.157	0.000	0.121	0.750
N	98694	88191	67543	47654	28798	15282

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates as in Table C.5. Outcomes are for two separate counts of new foreclosure filings by either different lenders than that in the observed case or the same lender. P-value tests the significance between the two responses (same lender vs. other lender)—estimates are performed simultaneously (pooling both outcomes and allowing for differing filing month and year of observation fixed effects).

Table C.34: Neighborhood-Level Aggregate Contagion Regressions

	2006–2008			2009–2010		
	(1)	(2)	(3)	(4)	(5)	(6)
F_{it-1}	0.281**	-0.051		0.115	0.725***	
	(0.123)	(0.225)		(0.075)	(0.148)	
F^2_{it-1}		0.066			-0.078***	
		(0.045)			(0.020)	
F_{it-2}	0.913***	1.177***		0.481***	0.742***	
	(0.108)	(0.163)		(0.064)	(0.096)	
F^2_{it-2}		-0.062*			-0.026***	
		(0.035)			(0.009)	
$F_{it-1} = 1$			1.793			-0.192
			(2.381)			(0.288)
$F_{it-1} = 2$			3.895			0.096
			(4.288)			(0.470)
$F_{it-1} = 3$			5.209			0.844
			(9.861)			(1.025)
$F_{it-1} = 4$			7.004			-0.841
			(13.671)			(2.557)
$F_{it-2} = 1$			1.296***			0.259
			(0.414)			(0.238)
$F_{it-2} = 2$			10.262			1.062**
			(8.890)			(0.503)
$F_{it-2} = 3$			-2.192			1.687
			(5.702)			(1.613)
$F_{it-2} = 4$			-2.160			0.006
			(11.965)			(2.853)
N	30291	30291	25317	19807	19807	12967
First-Stage F Stat	351.578	205.429	0.060	244.386	145.119	0.071

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month. 2SLS estimates as in Table C.5. Outcomes are for two separate counts of new foreclosure filings by either different lenders than that in the observed case or the same lender. P-value tests the significance between the two responses (same lender vs. other lender)—estimates are performed simultaneously (pooling both outcomes and allowing for differing filing month and year of observation fixed effects).