Essays in Public and Urban Economics

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Abstract

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This dissertation uses applied microeconomic tools to study three topics of fundamental importance for the regulation of the urban environment: housing, pollution, and the criminal justice system. The first chapter considers the mortgage market, and analyzes the regulatory tradeoff between optimal credit access and mortgage default. The second chapter examines the difficulties of designing environmental policy in interlinked production processes. In particular, we investigate (i) the impact of regulation on the producers of an upstream good on the pollution practices of the downstream firms that process that good and (ii) the subsequent health impacts on those who live in the cities and towns surrounding the downstream firms. The third chapter explores the bail system used for criminal defendants in the United States, and shows that the requirement that defendants post money bail has profound impacts on case outcomes.

Chapter 1, "Asymmetric Information and the Link Between Leverage and Mortgage Default" begins with the observation that borrowers with large mortgages relative to their home values are more likely to default. This chapter asks whether this correlation is due to moral hazard—larger balances causing borrowers to default—or adverse selection—ex-ante risky borrowers choosing larger loans. To separate these information asymmetries, I exploit a natural experiment resulting from (i) the unique contract structure of Option Adjustable Rate Mortgages and (ii) the unexpected divergence, during the 2008 crisis, of two financial indices used to determine interest rate adjustments for these loans. I find that moral hazard is responsible for 60-70 percent of the baseline correlation between leverage and default, but adverse selection explains the remaining 30-40 percent. I construct and calibrate a simple model of mortgage choice and default with asymmetric information to highlight the policy tradeoff informed by my estimates. I show that optimal regulation of mortgage leverage must weigh losses from defaults against under-provision of credit due to adverse selection.

In Chapter 2, "Interlinked Firms and the Consequences of Piecemeal Regulation", coauthored with Jonas Hjort and Gianmarco León, we note that industrial regulations are typically designed

with a particular policy objective and set of firms in mind. Yet when input-output linkages connect firms across sectors, such “piecemeal” regulations may worsen externalities elsewhere in the economy. Using daily administrative and survey data, we show that in Peru’s industrial fishing sector, the world’s largest, air pollution from downstream (fishmeal) manufacturing plants caused 55,000 additional respiratory hospital admissions per year as a consequence of the introduction of individual property rights (over fish) upstream. By removing suppliers’ incentive to “race” for the resource and enabling market share to move from inefficient to efficient firms, the reform spread production out across time, as predicted by a conceptual framework of vertically connected sectors. We show that longer periods of moderate air polluting production are worse for health than shorter periods of higher intensity exposure. Our findings demonstrate the risks of piecemeal regulatory design in interlinked economies.

In Chapter 3, "The Heavy Costs of High Bail: Evidence from Judge Randomization", coauthored with Ethan Frenchman and Arpit Gupta, considers the bail system in the United States. On any given day, roughly 450,000 people are detained awaiting trial, typically because they have not posted bail. Using a large sample of criminal cases in Philadelphia and Pittsburgh, we analyze the consequences of the money bail system by exploiting the variation in bail-setting tendencies among randomly assigned bail judges. Our estimates suggest that the assignment of money bail causes a 12% rise in the likelihood of conviction, and a 6–9% rise in recidivism. Our results highlight the importance of credit constraints in shaping defendant outcomes and point to important fairness considerations in the institutional design of the American money bail system.

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This dissertation is dedicated to former Federal Reserve Economist Murray Wernick

Chapter 1

Asymmetric Information and the Link Between Leverage and Mortgage Default

1.1 Introduction

The historic rise in household debt in the early 2000s is central to many narratives of the financial crisis, and a key component of these accounts is the role of mortgage leverage.[1] Leading up to 2008, borrowers increasingly took on large mortgages relative to the values of their homes. As housing prices dropped, highly leveraged borrowers were the most likely to default.

This paper separates two potential explanations for the correlation between leverage and default. The first, sometimes called moral hazard,[2] is a causal effect: if housing prices fall, high balances may lead borrowers to default. The alternative is adverse selection: ex-ante riskier borrowers prefer larger loans. Despite a substantial theoretical literature examining these two classical information asymmetries in credit markets, distinguishing moral hazard from adverse selection remains a fundamental challenge for empirical work. Yet the distinction between the two is crucial for mortgage policy.[3] As I show, a policymaker that attributes the correlation solely to moral hazard will (i) *overestimate* the reduction in defaults generated by regulations on leverage and (ii) *underestimate* a significant source of welfare losses, as adverse selection entails safe borrowers taking inefficiently small loans to differentiate themselves from riskier types.

My research design exploits a natural experiment generated by the unique contract structure of Option Adjustable Rate Mortgages (Option ARMs), which traditionally have interest rate adjustments tied to either LIBOR or Treasury rates. The unexpected divergence of these two indices during the 2008 crisis caused borrowers who chose otherwise identical contracts to owe substantially different amounts ex-post. This variation allows me to (i) identify moral hazard effects by comparing borrowers with identical initial leverage choices and different realized balances, and (ii) document adverse selection by comparing borrowers with different initial leverage choices but the same realized balance.

[1]The ratio of household debt to disposable personal income (DPI) peaked in late 2007 at 1.24, up from a historical average of 0.71. See the Financial Accounts of the United States. Mian and Sufi (2015) highlight the role of household debt in the financial crisis, Garriga and Schlagenhauf (2009), Corbae and Quintin (2015), Mayer, Pence and Sherlund (2009) and Campbell and Cocco (2015) discuss the role of mortgage leverage explicitly.

[2]This terminology in credit markets is used, for example, by Adams, Einav and Levin (2009).

[3]The empirical distinction also informs our understanding of the equilibrium form of mortgage contracts. Do lenders require down payments to ensure that borrowers repay ex-post (moral hazard) or to solve an ex-ante screening problem by sorting borrowers with different unobserved risk types (adverse selection)? Berger, Frame and Ioannidou (2011) provide recent empirical evidence on this question in credit markets.

The paper is organized in three parts. First, as a guide, I propose a model of asymmetric information in mortgage markets.[4] This model clarifies the sources of adverse selection and moral hazard and highlights the consequences for contracts in equilibrium. Next, I use the natural experiment described above to empirically disentangle adverse selection from moral hazard in a large sample of Option ARMs. Finally, I suggest and calibrate a simple structural model to quantify the contrasting welfare implications of each asymmetry. As an application of the model, I evaluate a policy that explicitly attempts to limit defaults by restricting leverage: a loan-to-value (LTV) cap.[5]

The theoretical framework, which begins the paper, clarifies the sources of asymmetric information in mortgage markets. In the model, moral hazard is the result of limited access to effective recourse.[6] In practice, mortgage lenders face severe constraints in recovering any loan balance beyond the value of the home itself. As a result, lenders are unable to write contracts that prevent borrowers from choosing to default when it is financially beneficial to do so. The default choice, in turn, may respond endogenously to the loan balance.

Adverse selection arises from heterogeneity across borrowers in their willingness to take advantage of the option to default. A large literature (e.g. Deng, Quigley and Order, 2000) suggests that there are significant differences across borrowers in this domain. Some borrowers walk away as soon as the home is worth less than the mortgage balance, whereas others choose not to default until the balance significantly outweighs the value of the home. If borrowers are privately aware that they are less likely to pay the loan back—a clear determinant of risk to the lender—they will prefer larger loans. This framework can be interpreted as a model of selection on (ex-post) moral hazard, as in Einav et al. (2013). Borrowers' demand for large loans is a function of their privately observed propensity to exercise the default option.

After presenting the model, I turn to disentangling moral hazard from adverse selection. The logic of the exercise comes in recognizing that, as in Karlan and Zinman (2009), the two give distinct empirical predictions. Adverse selection implies a positive correlation between the initial

[4]The model follows a substantial theoretical literature and draws particularly from Brueckner (2000) as well as Jaffee and Russell (1976).

[5]LTV caps limit the size of an initial loan relative to the home value, e.g. 90 percent. They are common worldwide and have recently been considered in the literature, for example by Gete and Reher (2015).

[6]While there is variation across states in recourse laws, the majority of the loans studied here are in states with limited or no recourse. For example, California, the most heavily represented state in my sample, does not allow deficiency judgments for owner-occupied homes (Pence, 2006).

loan size and default, regardless of the balance the borrower actually faces. To identify this effect, the ideal experiment would reassign a large sample of borrowers who have endogenously chosen different loan sizes to identical contracts. With equal balances, any remaining correlation between default and the initial loan choice is attributable to adverse selection.

Conversely, moral hazard implies a positive correlation between a borrower's balance and default, regardless of the initial loan size. The ideal experiment would take a set of borrowers choosing identical loans and randomly assign each borrower a different balance. Any relationship between default and the randomly assigned balance identifies a moral hazard effect.

The natural experiment I utilize features the key property of both experiments: an exogenous change in borrowers' balances *after* the initial contract choice. I isolate changes in ex-post balances that result from plausibly exogenous difference-in-difference variation in monthly interest rates. The variation itself comes as the result of a fine contract detail of all adjustable rate mortgages—the financial index used as a proxy for the cost of funds to the lender (typically a LIBOR or Treasury rate). While there was little reason for a borrower to prefer one index to another when taking a mortgage, the spread between the two increased significantly in late 2007. This led borrowers with otherwise identical loans to face a unique sequence of interest rates as a result of the index they chose and the origination month of their loan.

Two characteristics of Option ARMs translate this interest rate variation to changes *only* in borrowers' balances: fixed payment schedules and variable interest rates. For most other adjustable rate mortgages, the first order impact of an interest rate increase is not a change in the balance owed but rather a rise in the monthly payment. However, because payments are fixed for Option ARMs, at least in the first five years, excess interest accrual is absorbed directly into the loan balance. I use this variation to directly identify the causal effect of borrowers' balances on default—the moral hazard effect—and subsequently back out the role of adverse selection.

I find robust evidence that both moral hazard and adverse selection are present in the mortgage market. I estimate that moral hazard is responsible for 60-70 percent of the baseline correlation between leverage and default, while adverse selection is responsible for the remaining 30-40 percent. The moral hazard effect is directly policy relevant, quantifying how effective ex-post regulations that reduce balances are in preventing defaults.[7] My estimates imply, for example, that a 10-point reduction in a borrower's LTV 24 months after origination would reduce the average probability of default by over 4 percentage points. The policy implications of adverse selection are more difficult to determine. Ex-ante restrictions on mortgage contracts may have profoundly different impacts on equilibrium with and without adverse selection, but there is no standard framework to evaluate such regulations. Even the appropriate characterization of equilibrium in competitive contexts with adverse selection is controversial, and equilibria may fail to exist under conventional definitions.[8]

My final step is to calibrate and simulate a simple structural model to quantify the welfare implications of ex-ante regulation. To ensure the existence of equilibrium, I use the robust equilibrium concept recently proposed by Azevedo and Gottlieb (2016). I consider, as an example, the impact of a reduced LTV cap. I find that this policy is effective in limiting defaults, but the effect is smaller than a naive regulator—one who attributes the full correlation between leverage and default to moral hazard—would expect. Furthermore, for such a regulator, the presence of adverse selection generates significant unexpected welfare losses due to knock-on effects. While borrowers initially above the cap are mechanically forced to take smaller loans, the regulation propagates through the whole distribution: those below the cap also choose to take smaller loans in order to maintain a separation from riskier types. Appropriately accounting for adverse selection, I estimate that default externalities on the order of \$313,000 per default are necessary to make a reduction in the LTV cap from 100 to 90 welfare neutral. A naive regulator would underestimate this by 40 percent.

This paper's foremost contribution is to the growing empirical literature on asymmetric information in credit markets.[9] Complementing Karlan and Zinman (2009), a number of influential papers attempt to distinguish between adverse selection and moral hazard by exploiting ex-ante variation—experimental, regulatory, or institutional—in the set or shape of contracts offered.

[7] e.g. the Home Affordable Refinance Program Principal Reduction Alternative (HAMP PRA).

[8] e.g. Rothschild-Stiglitz.

[9] This paper is also heavily indebted to broader empirical work on asymmetric information in insurance and other markets. Particularly Chiappori and Salanie (2000), Cardon and Hendel (2001), Finkelstein and Poterba (2004), Finkelstein, McGarry et al. (2006), Finkelstein and Poterba (2014), Hendren (2013), as well as recent work examining the welfare implications of information asymmetries such as Einav, Finkelstein and Cullen (2010), Einav et al. (2013), and Einav, Finkelstein and Schrimpf (2010).

These include Ausubel (1999) and Agarwal, Chomsisengphet and Liu (2010) on the US credit card market; Adams, Einav and Levin (2009) and Einav, Jenkins and Levin (2012, 2013) on subprime auto loans; and Dobbie and Skiba (2013) on payday lending. However, separately identifying moral hazard effects in these contexts requires an assumption about why the relevant variation in ex-ante contracts does not also generate selection of borrowers on unobservables. To circumvent such assumptions, I isolate ex-post variation in the loan balance that is *unknown* to borrowers when selecting contracts.[10] Further, I propose and simulate a framework to evaluate policy in the presence of these asymmetries.

I also add to the papers above by studying the largest and arguably most important consumer debt market in the United States.[11] A small handful of empirical papers explicitly consider information asymmetries in mortgage markets, including Edelberg (2004), who uses structural assumptions to test for adverse selection and moral hazard in a broad class of consumer debts, and Ambrose, Conklin and Yoshida (2015) and Jiang, Nelson and Vytlacil (2014), who consider selection into and within low documentation mortgages.[12] Despite the significance of the mortgage market, the well-documented importance of screening in mortgage lending (e.g. Keys et al., 2010), and the quantity of theoretical work on information asymmetries (e.g. Brueckner, 2000; Dunn and Spatt, 1988; Stanton and Wallace, 1998; Harrison, Noordewier and Yavas, 2004; Chari and Jagannathan, 1989), attempts to cleanly separate adverse selection and moral hazard are relatively rare.

The estimated moral hazard effect directly contributes to the literature on the causes of mortgage default, in which the role of home equity is a major concern. Vandell (1995) provides an overview of early research on borrowers' exercise of the default option. More recent work, including Bajari, Chu and Park (2008), Foote, Gerardi and Willen (2008), Elul et al. (2010), Bhutta,

[10]Conceptually, I build on Karlan and Zinman (2009), who experimentally generate ex-post variation in balances in an unsecured debt market. While this environment closely approximates the canonical Stiglitz and Weiss (1981) framework, the majority of lending is secured by some form of collateral. A large theoretical literature (e.g. Bester, 1985) shows that the use of collateral in credit contracts has significant implications for both welfare and the expression of adverse selection in equilibrium. By screening borrowers using contracts that differ along two dimensions—interest rates and collateral amounts—lenders can avoid the credit rationing that characterizes unsecured lending. In contrast to Karlan and Zinman (2009), I explicitly study the richer contract space of collateralized lending.

[11]Mortgage balances represented 68 percent of consumer debt in the first quarter of 2016. See the Federal Reserve Bank of New York's May 2016 Quarterly Report on Household Debt and Credit.

[12]There is also related work in the home equity lending market, in particular Agarwal et al. (2011), who explore dynamic relationships, and Agarwal, Chomsisengphet and Liu (2016), who follow the strategy of Adams, Einav and Levin (2009).

Shan and Dokko (2010), and Gerardi et al. (2015), has stressed the joint importance of triggers such as liquidity and job loss alongside home equity in mortgage default. However, the majority of this literature identifies the impact of home equity on default using variation that results from changes in local home prices. I provide a new source of borrower-level variation in home equity that avoids the potential for measurement error and other endogeneity concerns inherent to the use of home price variation.

The identification strategy complements a series of papers investigating the impacts of interest rate resets on delinquency and other outcomes for borrowers with adjustable rate mortgages. This includes Fuster and Willen (2012), Tracy and Wright (2012), Keys et al. (2014), Di Maggio, Kermani and Ramcharan (2014), and particularly Gupta (2016), who also utilizes the distinction between different indices. Because those papers examine more traditional adjustable rate mortgages, none are able to identify the impacts of loan liability on default, focusing instead on the liquidity impacts of monthly payment shocks that typically accompany rate resets. My primary innovation comes in developing a research design that cleanly translates interest rate resets into variation in borrowers' balances.

The paper is structured as follows: Section 1.2 lays out key definitions and presents a model of information asymmetries in the mortgage market. Section 1.3 provides background information on Option ARMs and the data used in the paper. Section 1.4 describes the contracts offered to borrowers and provides initial tests for information asymmetries. Sections 2.4.1 and 1.6 present the empirical strategy and results, respectively. Section 1.7 shows the results of simulations, and Section 2.9 concludes.

1.2 Definitions and a Model of Information Asymmetries

In this section, I define adverse selection and moral hazard as they pertain to the relationship between mortgage borrowers and lenders. I then discuss why we might expect information asymmetries to exist in mortgage markets, highlighting a particular sort of borrower-level heterogeneity—individual differences in willingness to default—that provides a source of adverse selection. I develop a simple model of mortgage choice and default incorporating this heterogeneity following Brueckner (2000) and show that it gives rise to a Spence-Mirrlees single crossing condition.

Finally, I briefly outline the equilibrium implications of such a model, focusing on the potential for under-provision of credit due to adverse selection.

1.2.1 Definitions of Adverse Selection and Moral Hazard

The definitions of adverse selection and moral hazard that I specify follow largely from those used in Adams, Einav and Levin (2009):

(I) *Moral Hazard:* The mortgage market exhibits moral hazard if there is a causal relationship between the borrower's loan liability and default. That is, amongst homogeneous borrowers, those who face higher balances ex-post default more frequently.

(II) *Adverse Selection:* The mortgage market exhibits adverse selection if unobservably risky borrowers—those who are more likely to default with contract terms held equal—select higher leverage contracts.

Defining adverse selection in this way is fairly standard and adheres closely to the discussion in Chiappori and Salanié (2013) on insurance markets. Adverse selection exists if there is an exogenous correlation between a borrower's demand for leverage and the unobservable credit risk he poses to the lender. While there are a number of possible underlying models that could generate such a relationship, the equilibrium implications of the correlation are largely independent of the source, so I do not specify a mechanism in the baseline definition.

The way I define moral hazard is somewhat broader than usual. Typically, a credit market can be said to exhibit moral hazard if (i) the expected returns to the lender depend on some non-contractable action of the borrower and (ii) that action is itself influenced by the terms of the loan contract. If default is considered a strategic choice, my definition aligns with this traditional notion. Default itself can be thought of as the non-contractable action taken by the borrower. However, default is sometimes not an active choice. Borrowers may be insolvent or credit constrained to the extent that they are mechanically unable to make payments. While the empirical analysis that I conduct is explicitly designed to emphasize the strategic channel, the definition I use does not, in principal, rule out defaults due to a mechanical relationship between the loan balance and default. As in Adams, Einav and Levin (2009), whether the source is mechanical or strategic is not crucial for the policy implications I consider.

1.2.2 The Sources of Information Asymmetries in Mortgage Markets

In this subsection, I suggest potential sources of adverse selection and moral hazard in the mortgage market. While the definitions above are agnostic regarding mechanisms, understanding why we might expect these asymmetries to be present is helpful to frame further discussion.

Limited access to recourse for lenders provides an obvious explanation for the existence of moral hazard. The particular legal restrictions on contracts vary from state to state,[13] with some explicitly prohibiting lenders from recovering any excess balance from the borrower beyond the home itself in the event of default. However, even in states with laws that are favorable to lenders, deficiency judgments are relatively rare in practice (Pence, 2006). As a result, lenders cannot effectively contract against borrowers defaulting when their mortgages are underwater.

What is the source of heterogeneity that generates adverse selection? As a baseline, consider a simple model of mortgage default—often referred to as the frictionless option model—in which borrowers strategically default immediately if the value of their home drops below the value of the mortgage. Unless borrowers and lenders have different beliefs about housing prices, this model leaves little room for private information. All borrowers default according to a uniform rule.

However, a large literature suggests that borrowers do not default according to a frictionless option model (see Vandell, 1995, for a review). There is significant heterogeneity in willingness to exercise the default option (Deng, Quigley and Order, 2000), and a growing consensus that negative equity is a necessary but not sufficient condition for default (Bhutta, Shan and Dokko, 2010; Elul et al., 2010). Borrowers typically do not default until they owe more on their mortgage than the home is worth, and sometimes significantly more. Note that there is no need for a behavioral explanation for this phenomenon. There are real costs associated with default, including credit score reductions, moving costs, and social stigma.[14] These costs may differ in the population.

Heterogeneity in default costs provides a natural source of adverse selection. Borrowers who know that they are unlikely to repay will be less sensitive to the size of the mortgage balance. One way to think about this framework is as a model of selection on (ex-post) moral hazard, as in

[13]California, for example, has laws that explicitly prevent the lender from recovering any balance from the borrower beyond the home itself in the case of default for owner-occupied homes with 1-4 units. Alternatively, Illinois allows deficiency judgments that can only be relieved in bankruptcy.

[14]Even for a given cost of defaulting, borrowers may be heterogeneous in access to liquidity

Einav et al. (2013). Lenders cannot contract on the hidden action of default, the costs of taking that action are heterogeneous in the population, and borrowers are privately informed of their costs.

1.2.3 The Model

To capture the intuition described above, I propose a two-period model of borrowers' leverage demand and default choice, following Brueckner (2000). Borrowers differ in a single dimension, which I refer to as the private default cost. This black box parameter represents all factors that influence the borrower's default decision at a given level of home equity. There are two primary takeaways. First, the distribution of private default costs in the population determines the magnitude of the moral hazard effect, i.e., the increase in defaults generated by a given change in the loan balance. Second, a Spence-Mirrlees single crossing condition holds: borrowers with lower private default costs (i.e. risky borrowers) are relatively more willing to accept large balances.

In period 0, borrowers choose what portion of a risky housing purchase to finance. In period 1, the value of the home is realized, and borrowers choose whether to pay off their loan or to default. Mortgage contracts have two dimensions: the period 0 loan and the period 1 balance. I consider a non-recourse environment: in default, the borrower cedes the right to the home and is relieved of the loan balance.

Formally, let time be indexed by $t \in \{0, 1\}$ and borrowers be indexed by i. Borrowers must purchase a home with initial price H_0 and uncertain period 1 price H_1 distributed on support $[\underline{h}, \bar{h}]$ according to CDF $F(H_1)$. Lenders offer contracts of the form $\{L, B(L)\}$, where L is the value of the loan provided to the borrower in period 0, and $B(L)$ is the balance due on the loan in period 1.[15] In general $B(L)$ is increasing in L, that is, lenders demand higher balances for larger loans. A high leverage mortgage is one with a large L and correspondingly, a large $B(L)$.

Borrowers have per-period utility of consumption $u(\cdot)$, which is increasing and concave, receive income y_t in each period, which is not stochastic, and discount the future according to β. Each borrower i has privately known costs associated with defaulting, C_i, which captures the difference in dollar terms between defaulting and not defaulting.

[15] While other terms are often used to define mortgage contracts, these are usually equivalent to simple transformations of L and B in the two-period case. We could alternatively speak of the down payment $(H_0 - L)$, the interest rate $(\frac{B}{L} = 1 + r)$, and the original loan-to-value $\frac{L}{H_0}$.

Default Choice

In period 1, borrowers realize the value of their home and choose between repaying and defaulting. A borrower who repays retains the value of the home for net income $y_1 + H_1 - B$, while a borrower who defaults avoids paying the mortgage balance but incurs the default cost: $y_1 - C_i$. Borrowers choose to default when

$$H_1 - B < -C_i.$$

Borrowers with a low C_i are quicker to default, that is, for the same B they will default at higher home values.

This default rule demonstrates the importance of private default costs in determining the strength of the moral hazard effect. For a given C_i, the expected fraction of borrowers defaulting at balance B is $F(B - C_i)$, and the marginal effect of an increase in B is $f(B - C_i)$. The calculation becomes even more complicated with heterogeneity in C_i, as one must integrate over the set of borrowers at a given B.

Contract Choice

In period 0, borrowers know C_i but face uncertainty about the period 1 home value. As a result, they choose $\{L, B\}$ to maximize

$$U(L, B; C_i) = u(y_0 - (H_0 - L)) + \beta \left[\int_{\underline{h}}^{B - C_i} u(y_1 - C_i) dF(H_1) + \int_{B - C_i}^{\bar{h}} u(y_1 + H_1 - B) dF(H_1) \right].$$

The term in brackets represents the expected period one utility, with the first term giving utility in the case of default and the second utility with repayment.

Note that the borrower's overall utility is increasing in the loan size L:

$$U_L(L, B; C_i) = u'(y_0 - H_0 + L) \geq 0.$$

Additionally, borrower utility is decreasing in the balance:

$$U_B(L, B; C_i) = -\beta \int_{B - C_i}^{\bar{h}} u'(y_1 + H_1 - B) dF(H_1) < 0.$$

This is likely unsurprising. Holding the balance fixed, borrowers prefer larger loans, and holding the loan size fixed, borrowers prefer a smaller balance.

There are a variety of reasons why borrowers prefer to take out large loans. In the presence of credit constraints, L provides a method of smoothing consumption over time, so borrowers can consume period 1 income and the expected gains from the home in period 0. However, even if it is possible to borrow at the risk free rate, borrowers still value mortgage loans because they provide a form of insurance against low realizations of H_1.[16] An increased B effectively allows borrowers to give up consumption when H_1 is high in exchange for sure consumption (in the form of L) even when H_1 is low.[17]

At actuarily fair prices, borrowers prefer to take advantage of the insurance provided by a mortgage. In a totally frictionless context,[18] borrowers will choose an extreme form of full insurance when offered a fair price. In particular, they will take out as large a loan as possible and default on the loan in all states of the world. While this may seem surprising, it is a standard result: a risk averse agent will be willing to sell a risky asset for its expected value.

Yet borrowers with different values of C_i do not value this insurance equally. In fact, a Spence-Mirrlees single crossing condition holds:

$$\frac{\partial(\frac{U_B}{U_L})}{\partial C_i} = \frac{-\beta u'(y_1 - C_i) f(B - C_i)}{u'(y_0 - H_0 + L)} < 0.$$

Because low C_i are more likely to default, all else equal, they are more likely to take advantage of the insurance provided by the mortgage. As a result, they are willing to accept smaller increases in the loan size L in exchange for the same increase in the balance B.

If borrowers with different levels of C_i, say $C_R < C_S$ (where R and S denote risky and safe borrowers), are offered the same menu of contracts, the single crossing condition constrains the set of contracts chosen. In particular, if these types buy contracts $\{L_R, B_R\}$ and $\{L_S, B_S\}$, respectively, it must be the case that $L_R \geq L_S$. Of course, for borrower C_S to be willing to accept a smaller loan,

[16] Assuming mortgage debt is non-recourse, but other debt cannot be forgiven.

[17] The mortgage literature refers to this as the put option contained in a mortgage: the borrower retains the right to sell the home to the bank in exchange for the balance on the mortgage.

[18] By totally frictionless, I mean a context with (i) borrowing and lending at the risk free rate, (ii) no default costs to the borrower, and (iii) lenders who can perfectly recover the home value after a default.

it must also be the case that $B_R \geq B_S$. Further, if C_R and C_S buy different contracts along one dimension, both inequalities must hold strictly.

1.2.4 Equilibrium Consequences of Single Crossing

In this subsection, I provide a brief graphical discussion of the consequences of single crossing on the equilibrium allocation of credit. The intuition is familiar from Rothschild and Stiglitz (1976) and numerous other works on screening. With two borrower types, single crossing makes pooled contracts unsustainable. Any contract that is sold to both risky and safe borrowers can be undercut by a contract that offers slightly less credit but only attracts safe borrowers. As a result, safe borrowers receive smaller loans in equilibrium (à la Rothschild-Stiglitz) than they would in a world of perfect information. This notion is analogous to the under-provision of insurance to safe types in insurance markets.

I first take a world with perfect competition amongst lenders and a regulatory limit of $L \leq H_0$ (i.e. an LTV cap of 100 percent). I consider borrowers who prefer loans at or above the LTV cap with fair prices.[19] Panels A and B of Figure 1.1 present the perfect information case. These figures show lenders zero profit curves (solid curves, labeled with $\pi = 0$) and borrowers' indifference curves (dashed lines, labeled with U) in the space of contracts (L, B).

Borrowers prefer contracts to the southeast: larger loans with smaller balances. Lenders unambiguously prefer contracts to the west: smaller initial loans. However, the net effect of balance increases is ambiguous for lenders. Profits rise in the case of repayment, but the probability of repayment falls. With perfect information, borrowers choose initial loans right at the LTV cap. This holds whether there is a single borrower type, as shown in Panel A of Figure 1.1, or multiple types, as in Panel B. With two borrowers (risky R and safe S), the riskier type must simply pay a higher balance for the same loan, as shown by $B_R > B_S$ in Panel B.

Panels C and D of Figure 1.1 zoom in on the boxed portion of the graph shown in Panel B and highlight the complications posed by the single crossing property if lenders cannot offer different contracts to different borrowers. The balance necessary to give lenders zero expected profits on a pooled contract lies above the balance paid by safe borrowers with perfect information. Because

[19] For large enough C_i or sufficient difference between y_0 and y_1 with borrowing constraints, borrowers may prefer smaller loans even at fair prices.

the indifference curves of the risky type are steeper than those of the safe borrower—risky types are willing to accept a larger increase in the balance for the same increase in the initial loan—there is a region of contracts that are preferred by the safe borrower to the pooled contract but also make non-negative profits for lenders. In Panel C, this region is shaded in gray. This generates an opportunity for cream skimming. Given any pooled contract, lenders may offer a contract with a slightly smaller initial loan that attracts only the low risk types.

Panel D shows the form of a Rothschild-Stiglitz equilibrium in this context, should it exist. Risky borrowers end up with the same loan they would get in a world with perfect information, while safe borrowers are forced to take a smaller loan to distinguish themselves from riskier types. The welfare loss from adverse selection comes from safe borrowers receiving these inefficiently small loans ($L_S < H_0$) relative to the perfect information outcome.

1.3 Background and Data

In this section, I provide historical background on adjustable rate mortgages generally and the Option Adjustable Rate Mortgage in particular. I focus on the unique features of the Option ARM product that are key to my identification strategy. Because these loans feature fixed payment schedules and variable interest rates, changes in the benchmark financial indices used to determine interest rate adjustments translate directly to changes in borrowers' balances. I then describe the source of variation in interest rates I utilize in the empirical analysis, which is generated by differences between the financial indices used to determine rate adjustments. Finally, I discuss the characteristics of borrowers that chose Option ARMs relative to the larger population of mortgage borrowers and describe the data sources used in the empirical analysis.

1.3.1 Background on the Option ARM

Prior to the late 1970s, regulation effectively limited residential mortgage products in the United States to long term fixed rate loans, with set payments that remained constant over the amortization period. However, in 1978 the Federal Home Loan Bank Board began to allow federal savings and loan institutions to originate adjustable rate mortgages (ARMs) in California, and by the end of 1981, restrictions on adjustable rate products had been significantly relaxed nationwide. Originations of ARMs grew rapidly, representing as much as 68 percent of all new mortgages for certain months in the 1980s (Peek, 1990).

The industry largely settled on what are called Hybrid ARMs. These mortgages feature fixed interest rates and payments for a set initial period, often 5 or 7 years. After the initial period, interest rates begin to adjust according to market conditions, usually changing annually or semi-annually. Monthly payments are designed to be fully amortizing, that is, calculated to exactly pay off the loan over the full term at current interest rates. As a result, payments change to keep pace with interest rates and may unexpectedly increase if interest rates rise.

According to lenders, the potential danger of these unexpected payment increases motivated the creation of the Option ARM.[20] Banks wanted a product that incorporated floating interest rates while protecting borrowers from sharp payment increases and mortgage holders from the associated default risk. The Option ARM is characterized by a series of features that reflect this desire:

(I) *Fixed minimum payment schedule:* Borrowers are offered a relatively low initial payment, often based on the fully amortizing payment for an extremely low "teaser" interest rate. For the first 5 years, this payment adjusts upward once yearly by a fixed amount, usually 7.5 percent.[21] After 5 years, the minimum payment adjusts to the fully amortizing amount. This schedule may be interrupted if the loan balance rises above a fixed proportion of the original home value, often 110 or 125 percent.

(II) *Monthly interest rate changes:* While interest rates for most ARMs adjust annually or semi-annually, Option ARMs update much more frequently, usually monthly. As in typical ARMs, new interest rates are calculated as the sum of a fixed component (referred to as the *margin*), which is determined at origination, and a financial index that proxies for the cost of funds to the lender (hereafter the *index*).

(III) *Negative amortization:* Oftentimes the minimum payment required in a given month will be lower than the amount of accrued interest. In these circumstances, Option ARMs allow for

[20]See Golden West's history of the Option ARM, available at http://www.goldenwestworld.com/wp-content/uploads/history-of-the-option-arm-and-structural-features-of-the-gw-option-arm3.pdf.

[21]In theory, 7.5 percent is a cap, and the minimum payment might adjust by less if a 7.5 percent increase were to exceed the fully amortizing payment. In practice, the cap is nearly always binding.

negative amortization, that is, allow the excess interest accrual to be incorporated into the balance. As a result, the loan balance will typically grow in the early years of the mortgage.

(IV) *Proposed Payment Options:* The name, Option ARM, refers to a menu of payment options offered to borrowers on monthly statements. In addition to the minimum payment, statements offer the possibility of an interest only payment, covering the entirety of the interest accrual, along with amortizing payments calculated according to 15- and 30-year schedules. These possibilities are suggestions. Only the minimum payment is binding, and the borrower may in principle make any payment between the options or in excess of the 15-year amortizing payment (sometimes subject to certain caps). In practice most borrowers make minimum payments every month.

For the purposes of the identification strategy, (I) and (II) are key. Because payments are fixed for the first 5 years,[22] borrowers' balances change as a function of realized interest rates. In the next subsection, I discuss these features in greater depth.

In the 1980s and 1990s, the Option ARM was primarily a niche product directed towards sophisticated borrowers. The flexibility of payments was intended to appeal to borrowers who expected their income to rise in the future or those with high income volatility. With the growth of a secondary market for non-traditional mortgages in the early 2000s, banks began to market Option ARMs as affordability products, allowing borrowers to purchase more expensive homes than they would be able to afford with a traditional mortgage. Borrowers might take out such loans with the intention of refinancing the mortgage or selling the home after several years, and thus never making payments much above the initial minimum. In the years leading up to the crisis, Option ARMs became a significant fraction of the market, representing approximately 9 percent of originations in 2006.[23]

As the crisis hit, borrowers with Option ARMs defaulted at high rates. In the sample studied here, 41 percent of borrowers were seriously delinquent (60 days past due) on their mortgages at some point within the first 5 years, and 33 percent wound up in foreclosure. The combination

[22] All analyses performed here consider outcomes within the first 5 years. Appendix Figure A.1 presents a sample balance and payment trajectory for an Option ARM to highlight these product features from origination through that period.

[23] See the 2008 Mortgage Market Statistical Annual.

of high default rates and non-traditional features made Option ARMs a poster-child for excess in mortgage lending. Their role in the crisis has been highlighted by various media sources and policy makers—Ben Bernanke noted that "the availability of these alternative mortgage products proved to be quite important and, as many have recognized, is likely a key explanation of the housing bubble." Despite these criticisms, recent research has argued that these loans approximate the optimal mortgage contract (Piskorski and Tchistyi, 2010).

1.3.2 Diverging Indices and Interest Rate Resets

In addition to fixed payments and variable interest rates, the identification strategy relies on the financial indices used to determine interest rate adjustments. Interest rates for Option ARMs are typically tied to LIBOR or Treasury rates.[24]

Prior to the crisis, borrowers had little reason to prefer one index to another. Although there tended to be a spread between LIBOR and Treasury rates,[25] the two moved quite closely together, and any fixed difference could be accounted for in the margin. Furthermore, Bucks and Pence (2008) suggest that borrowers tended to be relatively uninformed about their contract terms. When asked what index their loan depended on, only 25 percent of borrowers responded with even plausibly correct indices, while 30 percent of borrowers simply answered that they did not know.

If borrowers were unaware of the distinction between indices, why did some end up with a Treasury index and others with LIBOR? Much of the variation comes as a result of the lender. Appendix Table A.1 shows the proportion of LIBOR-indexed loans for the top originators in the sample. Most originators appear to specialize in either LIBOR or Treasury indices, and some offer only Treasury rates. A similar pattern can be seen among servicers, although slightly less pronounced. According to Gupta (2016), differences across lenders are often a function not of the borrowers they lend to, but rather their intentions on the secondary market.

Panel A of Figure 1.2 shows the spread between the 1-year CMT and 1-year LIBOR. While there were fluctuations in the years preceding the crisis, the difference was contained in a relatively nar-

[24]Treasury rates are usually the 1-year Constant Maturity Treasury (CMT) or the 12-month Moving Treasury Average (MTA). Typically LIBOR refers to the 3-month LIBOR.

[25]For example, the spread between 1-year CMT and 1-year LIBOR was generally below 50 basis points.

row band. However, in mid-2007, Treasury rates began to fall and the spread increased, eventually peaking at over 3 percentage points in late 2008 following the Lehman Brothers bankruptcy filing and news of the AIG bailout. As a result, borrowers taking out similar loans prior to the crisis faced substantially different interest rates when their loans reset.

Panel B of Figure 1.2 displays this phenomenon for a large sample of adjustable rate mortgages, including traditional hybrid ARMs. The black line shows the average difference in interest rates between resetting loans indexed to LIBOR versus Treasury for each month. There is a noticeable spike in the in-sample difference in early 2009, corresponding to the late 2008 spike in Panel A.[26] The red line in Panel B shows the in sample difference in default for resetting loans indexed to LIBOR versus Treasury. In sync with the spike in relative interest rates, there was a sharp spike in relative defaults in early 2009. In the month that LIBOR-indexed loans experienced the most severe difference in interest rates, they also exhibited the most severe difference in defaults. This figure demonstrates the basic idea behind the identification strategy. Borrowers face different interest rates depending on whether their loan is indexed to LIBOR or Treasury and resultantly default at different rates.

But how do differences in interest rates cleanly translate into differences in loan balances? This is where the unique features of the Option ARM come into play. For a traditional adjustable rate mortgage, a change in the interest rate also changes the monthly payment, which adjusts to ensure that payments are fully amortizing at the new rate. These payment shocks are thought to be the first order link between interest rate changes and default (see, e.g., Fuster and Willen, 2012). Alternatively, for Option ARMs, the required monthly payment is fixed for the initial period. As a result, changes in interest rates have no direct impact on monthly obligations. Because the mortgage must account for changes in the interest rate somehow, any additional interest accrual is incorporated directly into the balance. This means that for Option ARMs, the divergence between Treasury and LIBOR rates caused borrowers with otherwise identical loans to have sizable differences in loan balances ex-post. Appendix Figure A.2 provides a stylized example of this pattern. Consider two identical $100,000 loans at origination, one of which faces a high realization of interest rates, while the other faces a low realization. Two years into the loan, the two borrowers will

[26] A lagged value of the index is typically used.

still have the same monthly payment, but the first borrower may owe thousands of dollars more than the second.

The impact of a LIBOR or Treasury index on the loan balance is not uniform across the sample period. Each origination month for each index generates a unique path of interest rates and a unique balance trajectory. Figure 1.3 demonstrates this difference-in-difference variation in borrowers' balances. The plot shows the loan balance over time for four sample $100,000 loans: one LIBOR- and one Treasury-indexed loan originated in January 2005, and one of each originated in January 2007.[27] Each of the four shows a distinct balance trajectory.

1.3.3 Data

The data on Option ARMs used in this paper are taken from a loan-level panel of privately securitized mortgages provided by Moody's Analytics (formerly provided by Blackbox Logic), representing over 90 percent of non-agency residential mortgage backed securities. These data provide detailed information about loans at origination, including borrower information, property characteristics, and contract terms. They also include dynamic information on monthly payments, loan balances, modifications, delinquency, and foreclosure. I focus on a sample of around 500,000 Option ARMs originated between 2004 and 2007, tied to either LIBOR or Treasury rates.

Summary Statistics: Balance Across indices

Table 2.1 shows summary statistics for the primary sample studied here, divided between loans indexed to Treasury and those indexed to LIBOR. Note that Treasury is the dominant index, representing approximately 90 percent of loans. Despite this, the majority of variables are reasonably balanced across the two groups. Borrowers have fairly high FICO credit scores for both indices, with an average of 706 for Treasury loans and 714 for LIBOR loans. Furthermore, the majority of loans are low or no documentation—79 percent of Treasury loans and 77 percent for LIBOR. The original leverage choice, summarized by the original loan-to-value ratio (LTV), is also quite similar across the two indices, at 76.6 for Treasury and 77 for LIBOR.[28] Nearly all loans are subject

[27] These figures are based on simulated loans with a margin of 3.5 for both samples, based on the 3-month LIBOR and 12-month MTA respectively.

[28] I restrict the sample to loans with original LTVs between 50 and 100.

to some form of prepayment penalties, and the majority of both Treasury and LIBOR loans are for primary residences.[29] There is a difference in the average margin for each—3.21 for Treasury loans versus 2.85 for LIBOR—but this gap reflects the baseline spread between the indices themselves.

The four most common states for both indices are California, Florida, Arizona, and Nevada. While Treasury loans are slightly more concentrated in California, the overall geographic patterns are similar across states. Appendix Figure A.3 shows the relatively uniform density: between 5 and 15 percent of loans are indexed to LIBOR in nearly all states. The largest difference between the two samples is in the timing of origination. LIBOR loans are significantly more concentrated in 2004, while Treasury loans are more heavily represented in 2005 and 2006. This pattern is also reflected in the slightly higher average balances for Treasury loans. Overall, the observable details in both groups are reasonably balanced.

In Comparison to the Broader Market

While the unique characteristics of the Option ARM may have attracted a certain sample of the population, the growth of the product was not the result of an inflow of observably low quality borrowers. Option ARM borrowers have relatively good credit scores. The average FICO score in the sample studied here is over 700, and a negligible number of borrowers have scores below 620.[30] In this observable dimension, borrowers with Option ARMs reflect the general pool of borrowers rather than some particularly subprime subset.[31]

The geographic patterns of Option ARM originations also reflect the broader mortgage market. As in the sample of Option ARMs, the top two states for mortgage lending are California and Florida, representing 24 percent and 9 percent of all originations, respectively. Arizona (3.5 percent) and Nevada (1.7 percent) are also prominent nationally. Furthermore, these states all experienced significant growth relative to the market as a whole in the years leading up to the

[29] The stated level of owner occupancy is likely an overstatement due to false reporting by investors (Piskorski, Seru and Witkin, 2013).

[30] The average credit score in the US is below 690, while the average among conforming loans purchased by Freddie Mac is 723 (Frame, Lehnert and Prescott, 2008). 620 is a common threshold to identify subprime borrowers.

[31] Amromin et al. (2011) find that borrowers with complex mortgages tend to be sophisticated, with high incomes and credit scores relative to the subprime population.

crisis.[32]

The initial leverage choices of borrowers with Option ARMs are not out of line with the market as a whole. The average original LTV for Option ARMs, close to 77, is slightly larger than conforming loans purchased by Fannie Mae or Freddie Mac but below the average LTV for subprime adjustable rate mortgages.[33] The average initial loan size for Option ARMs is also larger than that of conforming loans,[34] although still below the conforming loan limit.

One peculiarity distinguishing Option ARMs from conforming loans is the rarity of income verification. Given low payments, protections against payment increases, and generally favorable expectations about housing prices, lenders were relatively unconcerned about borrowers' ability to meet their monthly obligations. This was especially true given that most loans were made to borrowers with high credit scores.[35] This led to the prevalence of low or no documentation loans—nearly 80 percent in this sample. For these loans, borrowers provide little or no formal evidence of sufficient income to meet monthly payments, often simply stating income with no verification. In the market as a whole in 2007, low or no documentation loans represented only 9 percent of outstanding loans. However, nearly 80 percent of Alt-A securitizations in 2006 were low or no documentation, mirroring the pattern in this sample (Financial Crisis Inquiry Commission, 2011).

1.4 Positive Correlation Tests

Before attempting to disentangle adverse selection from moral hazard, I confirm the motivating relationship of the paper: a positive correlation between leverage and default. I first clarify that borrowers do not choose an amount of leverage in isolation. Instead, they choose a contract that entails both the loan size and the interest rate as a pair. I then explicitly conduct positive correlation tests following Chiappori and Salanie (2000) to demonstrate the existence of asymmetric

[32]All gained as a proportion of the market between 1996 and 2006: California by 25 percent, Florida by 60 percent, Arizona by 55 percent, and Nevada by 44 percent. All statistics presented here are available in the 2006 Mortgage Market Statistical Annual.

[33]The average original LTV for Fannie Mae and Freddie Mac in 2007 was 72 and 71, respectively, according to Frame, Lehnert and Prescott (2008). The average LTV for subprime adjustable rate mortgages was over 80 as early as 2004 (Chomsisengphet, Pennington-Cross et al., 2006).

[34]The average conforming loan size was $236,400 for purchases and $233,800 for refinances in 2006 (Avery, Brevoort and Canner, 2007).

[35]See http://www.mortgagevox.com/meltdown/option-arms.html

information. More simply, I show that a positive correlation exists between loan size and default and that this correlation persists conditional on the relevant information available to the bank at the time of contracting.

1.4.1 The Contract Menu

Mortgage contracts often have many features, but the key choice analyzed here is the tradeoff between the amount of leverage—summarized by the original LTV—and the interest rate.[36] A borrower may select a high LTV contract that requires a high interest rate or a low LTV contract with a lower rate. This menu of different interest rates and leverage pairs represents two dimensions of what Geanakoplos (2014) calls the credit surface. The particular slope and curvature of the surface depends on the current economic climate, the borrower's credit score, and other observable characteristics. For a given lender, contracts are usually summarized in a rate sheet, a series of guidelines indicating the required rates for different mortgage products, features, and borrower characteristics.[37]

Two features of the contract menu are suggestive of the existence of information asymmetries. The first is the very presence of multiple contract options for a given borrower, a standard property of markets with separating equilibria. Of course, these options might reflect market segmentation based on unobserved heterogeneity in preferences that is irrelevant to the lender. The second is that the relationship between leverage and interest rate is increasing: larger loans are offered at a higher unitary price. This shows that lenders account for the increase in default risk that comes when observationally equivalent borrowers take larger loans. Figure 1.4 shows this increasing relationship in the data, plotting the empirical schedule between leverage (original LTV) and the margin—the fixed portion of the interest rate—for Option ARMs. I show the median margin offered within each 5-point bin of LTV from 50 to 90.[38] A clear pattern emerges: borrowers who choose higher original LTVs are also choosing higher interest rates.

[36]Just as in the model, borrowers choose an initial loan L and Balance B.

[37]Customarily, lenders specify explicit margin increases associated with each 5-point bin of LTV. For eligible mortgages delivered to Fannie Mae, these increases are explicitly codified. Appendix Table A.2 shows an example of pricing adjustments necessary for mortgages purchased by Fannie Mae.

[38]This figure presents raw data, unconditional on the borrower's credit score or other characteristics, and hence may not represent the actual contract menu offered to any specific borrower.

1.4.2 A Positive Correlation Between Leverage and Default

Figure 1.5 shows the raw positive correlation between leverage and default. For Option ARMs, borrowers who choose large original LTVs are also more likely to default within the first 60 months of the loan. The relationship is nearly monotonic: those with original LTVs close to 50 default less that 10 percent of the time, while those with original LTVs near 90 default more than 50 percent of the time. The black line represents a local linear smoothing through the raw data, while black circles show the proportion of defaults for each 1-point point bin of original LTV.

By itself this raw correlation is not conclusive evidence of an information asymmetry. In principle, the correlation could be driven by selection on the basis of characteristics that are observed by the lender and appropriately priced into the contract. As a result, a crucial feature of the tests for information asymmetries suggested by Chiappori and Salanie (2000) are comprehensive controls for the lender's information set. If *observationally equivalent* borrowers who select larger loans default at higher rates, it can only be because (i) those borrowers are more likely to default on the basis of some unobservable (adverse selection), (ii) the larger loans actually *cause* more defaults (moral hazard), or (iii) some combination of the two.

Figure 1.6 shows that the positive correlation between original LTV and default holds conditional on the information available to the lender, providing an affirmative test for the existence of asymmetric information. Each dot in the plot shows the coefficient on original LTV from OLS regressions of a binary indicator of default by 60 months on original LTV, successively controlling for more comprehensive subsets of the information available to the lender. The leftmost coefficient, from a regression with no controls, displays the raw relationship: a 1-point increase in the borrower's original LTV is associated with an approximately 1.2 percentage point increase in defaults. The coefficient labeled *full controls* shows the relationship with the most comprehensive set of controls available. The inclusion of *full controls* drops the coefficient slightly, from just under 1.2 to just under 1.0. It remains large and significant. Note that only the inclusion of month fixed effects causes a meaningful drop, while controlling for the loan purpose and home value actually causes the coefficient to increase. The left-hand side of Panel A in Table 1.2 exhibits the information in Figure 1.6 in table form.

While I would ideally be able to condition exactly on the information available to the lender, the Moody's data is missing two features typically known by mortgage originators. These are the borrower's income as well as soft personal information about a borrower's risk that is not recorded. Fortunately, because of the preponderance of low or no documentation Option ARMs, lenders also did not have information about borrowers' income for the majority of loans in the sample. The fourth column of Panel A displays results limiting the sample only to these loans. The coefficient remains significant, and in fact increases slightly. The problem of soft information is more difficult to deal with. However, any missing soft information is likely to bias the coefficient on the original LTV towards 0. If lenders are aware that borrowers are bad credit risks in some way that is unobservable in the Moody's data, they will be less likely to offer desirable terms for high leverage contracts.

The right-hand side of Table 1.2 shows that whether one considers the borrower's leverage or interest rate as the defining feature of the contract is not crucial. There is a robust positive correlation between the borrower's margin and default. Finally, Panel B shows that the positive correlation holds consistently when looking at defaults by 12, 24, 36, or 48 months.

The final two coefficients shown in Figure 1.6 preview the remainder of the empirical analysis. The first, labeled *Ex-post LTV,* shows the coefficient on original LTV when controlling not just for the information of the lender, but also the (imputed) LTV at 60 months. The drop in the coefficient when including *Ex-post LTV* roughly represents the moral hazard effect. This is the portion of the correlation that is due not to selection, but to the incentives to default provided by the loan liability. The residual coefficient on original LTV represents selection. The final plotted coefficient, labeled *Option Value*, repeats this exercise using a more flexible set of controls in addition to the *Ex-post LTV*, including 6 months of leads and lags in interest rates and zip code level home prices. Note that there is not a significant drop in the coefficient when including this more flexible specification.

1.5 Empirical Strategy

The primary specification to separate adverse selection from moral hazard is a standard binary model of default. In simple terms, I regress an indicator for default on the borrower's current negative equity and original leverage. I argue that this single equation model is sufficient to capture the effects of both information asymmetries. Appropriately instrumented, the impact of current negative equity on default captures the moral hazard effect, while the effect of original leverage captures adverse selection.[39]

More specifically, for borrower i in MSA j at loan age t, I consider models of the form:

$$D_{ijt+1} = \mathbb{1}\{\alpha E_{ijt} + \gamma L_i + x'_{ijt}\beta + \omega_{m_i} + \delta_{index_i} + \zeta_j + u_{ijt} > 0\}. \tag{1.1}$$

Where E_{ijt} is instrumented by a function of the borrower's index choice and origination month:

$$E_{ijt} = f_t(m_i, index_i) + \lambda_{m_i} + \mu_{index_i} + z'_i\pi_t + \phi_j + e_{ijt}. \tag{1.2}$$

Here, D_{ijt+1} is a measure of default by time $t+1$. E_{ijt} is a measure of the borrower's negative equity, measured either as the current difference between a borrower's balance and the value of the home or as the current LTV. L_i is the borrower's initial leverage choice, measured as the original LTV. The vector x_{ijt} contains all time varying covariates relevant to the default decision as well as z_i, the set of all borrower characteristics and loan features known to the bank at the time of contracting. I include fixed effects for the origination month of the loan (ω_{m_i} or λ_{m_i}), the index choice (δ_{index_i} or μ_{index_i}), and the borrower's MSA (ζ_j or ϕ_j). Standard errors are clustered at the MSA level. Most specifications additionally allow for state specific time trends. I estimate the equation separately at different t, so do not include loan age effects. I interpret $\gamma > 0$ as evidence of adverse selection and $\alpha > 0$ as evidence of moral hazard.

In the next subsection, I discuss the challenges of consistently estimating α and γ that necessitate an IV strategy and specify the form of $f_t(m_i, index_i)$. I then justify the use of this single equation model, showing that Equation 1.1 can be derived by collapsing a more comprehensive model that explicitly specifies the borrower's demand for leverage alongside the default choice. Doing so also clarifies the interpretation of α as the moral hazard effect and γ as the adverse selection effect. Finally, I propose a secondary strategy to jointly estimate leverage demand alongside the default choice. While more complex, this approach allows me to recover fundamental parameters more directly relevant for the simulation exercise performed in the next section.

[39] In my basic specifications I use current negative equity to estimate the moral hazard effect, rather than explicitly using the borrower's balance. Negative equity captures any changes in the borrower's balance, but also captures the incentives to default driven by changes in housing prices.

1.5.1 Identification

The basic challenge in separately identifying α and γ—the effects of current equity and initial leverage on default—is the mechanical relationship between L_i and E_{ijt}. In the absence of other differences, borrowers with identical L_i will tend to have identical E_{ijt}. For borrowers consistently making minimum payments, there are only two factors that might cause those with identical L_i to have different E_{ijt}: differences in home prices or differences in interest rates that lead to different balances.

Unfortunately, shocks to home prices may, in general, be correlated with u_{ijt}. For example, a local labor market shock may influence both home prices and, separately, the borrower's probability of default. Additionally, because home prices can never be observed directly but rather must be inferred from the sale prices of surrounding homes, E_{ijt} is measured with error. Similarly, variation across time in interest rates is likely correlated with macro conditions, while cross-sectional variation potentially reflects borrowers' endogenous contract choices. Isolating exogenous variation in E_{ijt} is non-trivial but necessary to accurately estimate α and γ.

I focus on plausibly exogenous variation in E_{ijt} that comes from the interaction of the borrower's index and the origination month of the loan. Each {Index Type, Origination Month} pair generates a unique trajectory of interest rates for a borrower. Utilizing this difference-in-difference variation allows me to control for any origination month-specific cohort effects or trends in the macro-economy, while also accounting for any fixed differences between borrowers with different indices.[40]

Equation 1.2 shows the basic framework for isolating this variation in E_{ijt}. The function $f_t(m_i, index_i)$ is effectively a set of instruments for E_{ijt}. These instruments capture changes in E_{ijt} that result from the interaction between the origination month m_i and the index, but are distinct from fixed month and index effects λ_{m_i} and μ_{index_i}.

Developing an instrument involves choosing a functional form for $f_t(m_i, index_i)$. In what follows, I focus primarily on a specification that exploits all possible variation in the interaction

[40]The difference-in-difference framework allows me to control for fixed lender characteristics, even for originators or servicers who exclusively feature one of the two indices.

between the month of origin and the index, that is:

$$f_t(m_i, index_i) = \lambda_{m_i} \times \mu_{index_i}.$$

In words, I use a full set of fixed effects for every possible {Index Type, Origination Month} pair as instruments for the borrower's home equity. This specification has the advantage of limiting assumptions about functional forms and provides a large number of instruments.

However, because this large set of instruments does not provide an easily interpretable first stage and may suffer from problems associated with many weak instruments, I also consider a secondary option. The basic idea behind this exercise is to produce a strong predictor of a borrower's balance using only the origination month and index. To do so, I mechanically calculate the full balance trajectory for a sample loan for each index type and origination month. The sample loan sets all potentially endogenous terms, which vary for any given loan, to standard values.[41] As a result, the instrument captures the variation in the balance that is driven by the interest rate realizations while excluding any variation due to endogenous contract choices. I refer to the instrument developed using this calculation as the "simulated" instrument.

1.5.2 Leverage Demand and Default Choices

In this section, I show that Equation 1.1 can be derived from a more explicit model of the borrower's leverage and default choices. I begin with the default rule suggested by the theoretical model in Section 1.2. A borrower defaults if the value of the home (H) falls far enough below the balance (B) to justify incurring any default costs C_{ijt}. C_{ijt} is a reduced form parameter that captures any observable and unobservable factors that influence the borrower's decision to default at a given level of home equity. The default condition is then:

$$D_{ijt+1} = \mathbb{1}\{B_{ijt} - H_{ijt} > C_{ijt}\}.$$

A slight relabeling generates a condition that resembles Equation 1.1 above. First, let $E_{ijt} = B_{ijt} - H_{ijt}$, a measure of the borrower's negative equity. E_{ijt} is large when the borrower owes much more

[41] A margin of 3.5, an initial loan size of $400,000, and a minimum payment based on a 1.75 percent teaser rate.

than the home is worth. Next, decompose C_{ijt} into its observable and unobservable components, where $-C_{it} = \sigma_\varepsilon(x'_{ijt}\beta + \omega_{m_i} + \zeta_j + \delta_{index_i} + \varepsilon_{ijt})$ and only $\varepsilon_{ijt} \sim N(0,1)$ is unobservable. Defining $\alpha = \frac{1}{\sigma_\varepsilon}$, we can write the default condition as:

$$D_{ijt+1} = \mathbb{1}\{\alpha E_{ijt} + x'_{ijt}\beta + \omega_{m_i} + \zeta_j + \delta_{index_i} + \varepsilon_{ijt} > 0\} \tag{1.3}$$

While the borrower's contract choice is the result of a complex maximization problem, I abstract from this structure and specify a linear demand model for leverage. Letting L_i represent the original LTV chosen by borrower i:

$$L_i = z'_i\psi + \theta_{m_i} + \eta_{index_i} + v_i. \tag{1.4}$$

Within this framework, moral hazard and adverse selection have straightforward empirical predictions:

(I) *Moral Hazard:* $\alpha > 0$ provides evidence of a moral hazard effect, where α quantifies the impact of the borrower's equity on default.

(II) *Adverse Selection*: $\rho = Corr(v_i, \varepsilon_{ijt}) > 0$ provides evidence of adverse selection. Borrowers who choose higher than average L_{ij} based on unobservables (large v_i) are more likely to default holding home equity constant (large ε_{ijt}).

In the next subsection, I describe an approach to estimating Equations 1.3 and 1.4 jointly. However, to arrive at Equation 1.1, I collapse leverage demand and the default decision based on the correlation between v_i and ε_{ijt}. In particular, I write $\varepsilon_{ijt} = \gamma v_i + u_{ijt}$, where $\gamma > 0$ holds if the two are positively correlated, that is, if there is adverse selection.[42] Replacing v_i using Equation 1.4 gives $\varepsilon_{ijt} = \gamma(L_i - z'_i\psi - \theta_{m_i} - \eta_{index_i})$. Replacing ε_{ijt} in Equation 1.3, collapsing month and index fixed effects, and absorbing z_i into x_{ijt} gives Equation 1.1.

[42]In the normal case, $\gamma = \rho\frac{\sigma_\varepsilon}{\sigma_v}$.

Joint Model

Although the collapsed model in Equation 1.1 is more straightforward, there are also benefits to jointly estimating the leverage demand and default choice. Given the need to instrument for E_{ijt}, doing so actually involves estimating three equations:

$$\begin{aligned}
D_{ijt+1} &= \mathbb{1}\{\alpha E_{ijt} + x'_{ijt}\beta + \omega_m + \zeta_j + \delta_{index} + \varepsilon_{ijt} > 0\} \\
L_i &= z'_i\gamma + \theta_m + \eta_{index} + v_i \\
E_{ijt} &= f(m_i, index_i) + \lambda_m + \mu_{index} + z'_i\pi_t + \phi_{jt} + e_{ijt}.
\end{aligned}$$

I impose a parametric structure on the errors. In particular:

$$\begin{pmatrix} \varepsilon_{imt} \\ v_i \\ e_{it} \end{pmatrix} \sim N\left(\begin{pmatrix} 0 \\ 0 \\ 0 \end{pmatrix}, \begin{bmatrix} 1 & & \\ \rho_{\varepsilon v}\sigma_v & \sigma_v^2 & \\ \rho_{\varepsilon e}\sigma_e & \rho_{ve}\sigma_v\sigma_e & \sigma_e^2 \end{bmatrix}\right).$$

Again, I estimate cross-sectionally at different t and hence make no assumption about the evolution of errors over time. This specification allows a relatively straightforward estimation. I effectively employ a control function approach following Blundell and Powell (2004), incorporating an additional linear equation.

The benefit of this approach is that I am able to recover a few parameters that provide a basis for simulation in Section 1.2. Perhaps most importantly, I directly recover $\rho_{\varepsilon v}$, the correlation between ε_{ijt} and v_i. This correlation determines the strength of the adverse selection effect. Furthermore, under the normality assumption, I am able to recover the underlying distribution of the default costs C_{ijt} for any individual i. Recalling that $-C_{ijt} = \sigma_\varepsilon(x'_{ijt}\beta + \omega_{m_i} + \zeta_j + \delta_{index_i} + \varepsilon_{ijt})$, we have:

$$C_{ijt}|x_{ijt}, \omega_{m_i}, \zeta_j, \delta_{index_i} \sim N\left(\frac{-(x'_{ijt}\beta + \omega_{m_i} + \zeta_j + \delta_{index_i})}{\alpha}, \frac{1}{\alpha}\right).$$

The distribution of C_{ijt} characterizes the moral hazard effect, while $\rho_{\varepsilon v}$ summarizes the degree to which borrowers' knowledge of their place in this distribution impacts leverage demand.

1.6 Results

In this section, I describe the central empirical results of the paper. I begin by defining a few variables used in the analysis. I then describe first stage results: the instruments I use are correlated with borrowers' balances, are directly predictive of default in a reduced form, and do not predict borrower characteristics such as credit scores. I next turn to the results of the primary, single equation model of default. I find strong evidence of both moral hazard and adverse selection, which hold across numerous robustness checks. I explore heterogeneity in these results by state recourse status and initial loan features. Finally, I discuss my estimation of the joint model of leverage demand and default choice, which provides parameters that directly inform the simulations presented in the next section.

1.6.1 Definitions of Key Variables

The empirical analysis revolves around three variables: default D_{ijt+1}, original leverage L_i, and current equity E_{ijt}. Here, I discuss the definitions of each as used below:

(I) **Default (D_{ijt+1}):** The standard definition for default used here is a borrower being 60 or more days past due on monthly payments. Typically, D_{ijt+1} measures the outcome of default between years t and $t+1$. However, when explicitly stated, D_{ijt+1} may also refer to default at any point between loan origination and $t+1$.

(II) **Original Leverage (L_i):** Original leverage is measured as original loan-to-value in percentage terms. While the CLTV (combined loan-to-value), which incorporates any second liens, is sometimes used as a measure of leverage, my focus here is on the leverage contained in a particular contract. I control for the presence of any observable additional liens in all specifications.

(III) **Current Equity (E_{ijt}):** I use two alternative measures of E_{ijt} throughout the analysis. The first is the borrower's *negative equity*. This is defined as the current balance on the loan less the value of the home. The second is the borrower's current *loan-to-value*, the ratio of the current balance to the current value of the home. Both of these measures grow as the borrower's balance increases and fall if the price of the home increases. As home values are generally only recorded when houses are sold, I follow the literature and impute the current home value based on local home price indices.[43]

Unfortunately, I do not observe E_{ijt} for borrowers who exit the sample prior to time t. This prevents me from using the full sample for specifications that incorporate current home equity. However, given the contract terms for a mortgage—the margin, initial monthly payment, initial balance, and index—predicting the balance up to the first delinquency or partial prepayment is a straightforward mechanical calculation. Using just loan terms at origination and interest rates, I am able to predict the observed values of E_{ijt} with a high degree of accuracy ($R^2 > 0.95$). In regressions that incorporate the full sample, I use these imputed values of E_{ijt}, which are available for all borrowers, rather than the observed values.

1.6.2 First Stage

The plots in Figure 1.7 highlight a few features of the instruments for E_{ijt} used in the main analysis. Because the primary specification uses a large number of fixed effects, and hence does not provide an easy to interpret first stage, I use the simulated instrument described above—a single variable—to produce these figures. This variable allows me to address three points.

First, do borrowers actually have significantly higher E_{ijt} when the instrument suggests balances should be high? Panel A of Figure 1.7 shows that this is the case. The plot presents the coefficient on the simulated instrument from the simplest possible specification for considering relevance: a regression of E_{ijt} on the instrument, controlling for origination month and index fixed effects. When the simulated instrument is high, borrowers' E_{ijt} are high. This pattern holds across the first several years of the loan, although the size of the correlation declines over time.

Second, Panel B shows that borrowers also default more when the instrument is high. This is a reduced form and shows coefficients from an identical exercise to that in Panel A, replacing E_{ijt} with default D_{ijt}. Third, Panel C shows evidence of instrument exogeneity. Despite predicting borrowers' balances and defaults, the instrument is not correlated with FICO credit scores, a key measure of borrowers' creditworthiness.

Table 1.3 shows a more formal first stage. The coefficients shown are analogous to those in

[43] I use Zillow's zip code level home price index, available at http://www.zillow.com/research/data/.

Panel A of Figure 1.7, utilizing the simulated instrument. However, because the primary specifications use the full set of $\lambda_m \times \mu_{index}$ fixed effects, I also include F-statistics calculated using the full set of fixed effects alongside those from the simulated instruments. These tables largely mirror the information shown in the figure. At 24 months, predictive power is strong, with F-statistics suggesting that the instruments are relevant in both the fixed effects and simulated instrument specifications (although the F-statistic drops below 10 when using fixed effects to predict negative equity). However, by 48 months, as the sample diminishes, the instruments lose their predictiveness. This is especially so for the simulated instrument, which has effectively no predictive power by 48 months when controls are included. The fixed effects specification, while weak, retains some relevance.[44]

1.6.3 Main Results: Single Equation Model

The primary specifications attempt to isolate adverse selection and moral hazard following Equation 1.1. In the main tables, I use linear probability models and a standard instrumental variables approach. In the appendix, I show probit estimates, accounting for the endogeneity of E_{ijt} following Blundell and Powell (2004).

The main tables are structured to show three versions of each specification of interest: baseline, OLS, and IV. The first is a reference and shows the baseline relationship between original LTV and default, including relevant controls but excluding any measure of current equity. For OLS regressions, I add a measure of E_{ijt} to the baseline regression but do not account for endogeneity in E_{ijt}. Finally, in the IV regressions I explicitly instrument for E_{ijt} with the full set of $\lambda_m \times \mu_{index}$ fixed effects. The coefficient on E_{ijt} gives the moral hazard effect, while the coefficient on original LTV gives the adverse selection effects. Comparing the IV regressions to the baseline regressions gives a sense of the role of moral hazard in the overall correlation between leverage and default.

Table 1.4 presents the primary set of specifications, showing a cross-section of borrowers 24

[44] Because the current loan-to-value or negative equity at 24 or 48 months is only observed for loans that actually survive to those points, the first stage regressions in Panel A are necessarily conducted on a selected sample. Panel B shows identical specifications to those in Panel A but replaces the observed values of E_{ijt} with imputed values, which allows the use of the full sample. While the strength of the instruments is not substantially better at 24 months for either the current LTV or home equity, there are significant improvements at 48 months. While the F-statistics only exceed 10 without covariates, both versions of the instrument are relevant (if weak) with the full sample.

months after origination.[45] This table includes E_{ijt} defined as both current negative equity (Panel A) and current LTV (Panel B). In different specifications I include two levels of controls: a basic set with only origination month and index fixed effects, and a comprehensive set, including MSA fixed effects, flexible controls for the original FICO credit score (dummies for each 20-point bin), state-level time trends, loan originator and servicer fixed effects, and controls for documentation, loan purpose, occupancy, property type, prepayment penalties, private mortgage insurance, second liens, and the original home value.

The left-hand side of Panel A shows that there is strong evidence of both adverse selection and moral hazard when defining E_{ijt} as current negative equity and including only basic controls. The estimated moral hazard effect in the IV specification suggests that a $100,000 increase in negative equity increases the one year default probability by 4.5 percentage points (17 percent). The estimated adverse selection effect suggests that a 10-point increase in the borrower's original LTV is associated with a 3.3 percentage point (12.5 percent) increase in the one year default probability. The OLS results are quite similar, showing slightly larger moral hazard effects and a slightly larger role for adverse selection. Comparing the role of original LTV in the IV regression (0.331) to that in the baseline estimate (0.586) implies that adverse selection is responsible for more than half of the baseline correlation. However, it is crucial not to over-interpret the coefficient on original LTV with this limited set of controls. Without controlling for information available at loan origination, this result pools true selection on unobservables with lenders' steering of riskier borrowers towards smaller loans.

The right-hand side of Panel A includes the full set of controls and shows (i) a larger moral hazard effect and (ii) an adverse selection effect that is similar in levels but smaller as a fraction of the baseline estimate. The estimated moral hazard effect implies that a $100,000 increase in negative equity increases the one year default probability by 8.9 percentage points (33.5 percent). The estimated adverse selection effect shows that, all else equal, borrowers who choose 10-point larger initial LTVs are 2.6 percentage points (10 percent) more likely to default between 24 and 36 months. However, including the full set of controls also leads to a significant increase in the baseline correlation between original LTV and default. As a result, adverse selection accounts for

[45]Other loan ages are shown in the appendix.

approximately 36 percent of the baseline correlation with appropriate controls. This leaves moral hazard responsible for the remaining 64 percent.

Panel B repeats the exercise from Panel A but defines E_{ijt} as current loan-to-value. With full controls, the effects are quite similar to those found in Panel A. Adverse selection is responsible for 32 percent of the baseline correlation between original LTV and default, while the remaining 68 percent is due to moral hazard. These estimates imply that borrowers that choose 10-point higher original LTVs are 2.3 percentage points more likely to default between 24 and 36 months, all else equal, while a 10-point increase in current LTV at 24 months increases the probability of default by just over 4 percentage points. Furthermore, specifications without controls highlight the potential complications of ignoring the information available to the bank. The OLS and IV show negative (although insignificant) coefficients on the original LTV when controlling for the current LTV.

1.6.4 Heterogeneity in Results from the Single Equation Model

Table 1.5 considers how the results in Table 1.4 change across three relevant subgroups: (i) in states with full recourse versus those with limited recourse, (ii) for borrowers providing full documentation versus those providing limited or no documentation, and (iii) for home purchases versus refinances. In each panel, I show the baseline relationship between original LTV and default for the relevant subgroup, then IV regressions with E_{ijt} defined first as negative equity and next as current LTV. All specifications include the full set of controls.

State Recourse Status

The most notable difference between states with full versus limited recourse[46] is in the strength of the estimated moral hazard effect. Both categories show a significant baseline correlation between original LTV and default. However, the impact of E_{ijt} on default—defined either as current negative equity or LTV—is large and statistically significant in limited recourse states, and near zero in full recourse states. This pattern is intuitive: in states where borrowers are responsible for the loan balance even in default, the marginal incentive to default generated by an increase in the current

[46]By state recourse status, I refer to a state's provisions regarding a lender's ability to recover any balance that exceeds the value of the home in the case of default. I categorize states as full or limited recourse on the basis of that in Rao and Walsh (2009), with full recourse referring to states with strong provisions regarding deficiency judgments and limited recourse referring to those with mixed, weak, or nonexistent provisions.

balance is low. Perhaps more surprising is that both types of states show strong evidence of adverse selection across OLS and IV specifications. In both cases, original LTV is strongly associated with default, controlling for current incentives to default. It should be noted that the sample size is much smaller in full recourse states, and the estimates are correspondingly less precise.

Documentation

Dividing borrowers by documentation provided, shown in Panel B of Table 1.5, suggests that income verification may be an important factor in screening borrowers. The results for the low or no documentation sample largely match the full sample. In contrast, in the sample providing full documentation, the entirety of the raw correlation between leverage and default is explained by moral hazard. The optimistic view of this result is that documentation solves the adverse selection problem: the additional information on income allows lenders to distinguish an individual's riskiness before offering a set of contracts. However, because I do not observe income, I am also not perfectly able to control for the information set of the lender in the full documentation sample. As a result, the coefficient on original LTV in the full documentation sample pools an adverse selection effect with any steering of borrowers by lenders on the basis of income.

Purchases vs. Refinances

The differences between those purchasing homes versus those refinancing existing mortgages, shown in Panel C of Table 1.5, are less severe than those in Panels A and B. While the baseline correlation between original LTV and default is higher in the refinance sample, both show comparable moral hazard effects: a 10 point increase in the current LTV causes an average of just over 4 percent more defaults within a year on average in both samples. However, the estimated adverse selection effects are slightly smaller in the purchase sample and only significant when E_{ijt} is defined as current negative equity.

1.6.5 Robustness for the Single Equation Model

The appendix includes a number of tables intended to serve as robustness checks to Table 1.4 and to provide alternative estimates of interest. Here, I briefly discuss these exercises.

Loans at 48 Months

The results for loans at 48 are similar to those at 24 months, if somewhat muted. Appendix Table A.3 presents identical regressions to those in Table 1.4, except with current E_{ijt} defined at 48 months and the dependent variable defined to be default between 48 and 60 months. With full controls, the baseline relationship between original LTV and default is somewhat lower than at 24 months, and the proportion of the correlation due to adverse selection somewhat higher (greater than 50 percent in the IV specifications). Further, the estimated moral hazard effects are smaller, and insignificant when E_{ijt} is defined to be current LTV. Given the weakness of the instrument at 48 months, these estimates should be interpreted cautiously, but they largely support the results found in Table 1.4. Results at other cross-sections are similar.

Cumulative Default Probabilities

The regressions in Table 1.4 take an indicator for default within one year as the dependent variable. Doing so poses two potential issues. First, considering the default probability between 24 and 36 months limits the sample to borrowers who are still active at 24 months. This generates a potential source of bias, as borrowers who default or prepay in the early years of the loan may differ from the larger population, or may be responding endogenously to new knowledge of their anticipated future balance. Second, lenders may be more concerned with whether a borrower defaults at all, rather than a borrower's hazard rate, particularly with loans that feature negative amortization.

To address these issues, Appendix Table A.4 considers the impact of the original LTV and current E_{ijt} on cumulative default outcomes in the full sample. This approach avoids sample selection issues, but requires a slight reinterpretation of the treatment. The moral hazard effect no longer captures a response to the realized balance but rather the borrower's response to the anticipated balance trajectory. Furthermore, because E_{ijt} is not observed directly for those defaulting prior to t, I use the imputed version of the E_{ijt}, based on original contract terms and realized interest rates.

I first estimate specifications meant to mimic those in Table 1.4, this time utilizing the outcome of cumulative default by 36 months. These are shown on the left-hand side of Appendix Table A.4. I include imputed E_{ijt} measured at 36 months. For these estimates the baseline relationship between the original LTV and default is higher than in Table 1.4. However, the portion owing to adverse selection—approximately 17 percent when E_{ijt} is defined as current negative equity, and 29 percent when defined as current LTV—is somewhat lower. Regardless, there is strong evidence that both moral hazard and adverse selection are present.

As a more robust test of the adverse selection effect, the right-hand side of Appendix Table A.4 considers the outcome of default by 60 months. For these regressions, I include a comprehensive set of controls for E_{ijt}, not just at a given point in time, but across the life of the loan. These controls are meant to account for the full impact of the non-linear loan trajectory throughout the first 60 months. Even controlling for the full trajectory of E_{ijt}, the initial leverage choice is strongly predictive of default. In these specifications, adverse selection remains responsible for approximately 30 percent of the baseline relationship between original LTV and default.

Alternate Functional Forms

A potential concern is that the observed effect of original LTV on default when controlling for E_{ijt} does not truly reflect selection, but rather some more complicated functional form relating E_{ijt} to default that is not captured by a linear specification. Appendix Table A.5 examines whether there is still evidence of adverse selection across three more complex specifications: (i) including a cubic specification in current E_{ijt}, (ii) controlling for current and past minimum payments and interest rates, and (iii) interacting E_{ijt} with covariates.[47] The estimated adverse selection effect is persistent across all specifications.

Further Robustness

Appendix Tables A.6 and A.7 explore further robustness. The results are robust to (i) probit and control function specifications, which are potentially more realistic than the linear probability model, (ii) the use of the simulated instrument rather than the full set of fixed effects, and (iii) alternative definitions of default, ranging from mild (30 days past due) to extreme (foreclosure).

[47] In column 5 of Appendix Table A.5, the OLS specification, I fully interact E_{ijt} with all covariates. However, because I do not have sufficient instruments to do so in an IV specification, in column 6 I simply interact E_{ijt} with two covariates: the borrower's credit score and whether the loan was to purchase a home or refinance an existing mortgage.

1.6.6 Joint Model

The final step of my empirical analysis is to estimate a joint model of leverage demand alongside the default choice. Doing so allows me to recover parameters that more directly relate to the model developed in Section 1.2 and that can be used to inform the simulations developed in the next section. Because of the increased computational complexity of this estimation, I slightly reduce the richness of included controls, e.g. substituting MSA fixed effects with state fixed effects. I estimate the model separately at 24, 36, and 48 months, and again define E_{ijt} as both current LTV and home equity. These estimates are presented in Table 1.6 and qualitatively align with estimates from the single equation model.

The primary benefit is in providing estimates of three parameters: (i) $\rho_{\varepsilon}v$, the correlation between the errors in the leverage and default choices, where a positive value indicates adverse selection, (ii) $\sigma_{\varepsilon} = \frac{1}{\alpha}$, the standard deviation of the default error in units of E_{ijt}, where a positive and significant value of α indicates moral hazard (corresponding to a finite positive value of σ_{ε}), and (iii) the mean of borrowers' default costs, conditional on observables. This can also be interpreted as the default threshold, that is, the level of E_{ijt} above which the average borrower (with a given set of observables) defaults. While the estimates at 24 and 36 months show strong evidence of both adverse selection and moral hazard—a positive ρ and α—the estimates at 48 months are less precise.

The first and fourth columns display the estimated parameters at 24 months, with E_{ijt} defined in terms of negative equity and current LTV, respectively. The estimated threshold for default in the first column (at average values of observables) is just under \$100,000, meaning that a borrower will not default until the balance on their loan is \$100,000 above what the home is worth. The standard deviation of the default error in this specification is approximately \$190,000. Similarly, the fourth column suggests that the average borrower must owe 1.34 times what the home is worth before defaulting. The standard deviation of unobserved default costs in the population is just over 50 percent of what the home is worth: $\sigma_{\varepsilon} = 0.55$. Finally, the correlation between unobserved default costs and the unobserved portion of the original leverage (original LTV) choice—which measures adverse selection—is significant, and just under 0.07. I use these estimates to parameterize the model in the next section.

1.7 Simulations and Welfare Analysis

In this section, I consider the policy implications of separately accounting for adverse selection and moral hazard. The estimated moral hazard effect—the causal effect of a change in home equity on the probability of default—provides precisely the relevant parameter for considering the effectiveness of ex-post policies that reduce loan balances in preventing defaults. Understanding the impact of ex-ante regulations is more challenging. I consider the case of an LTV cap and argue that ignoring the role of adverse selection leads policymakers to (i) *overestimate* the reduction in defaults generated by a reduction in the LTV cap and (ii) *underestimate* the welfare loss generated by borrowers taking smaller mortgages. I consider a slightly expanded version of the model suggested in Section 1.2 and use the equilibrium concept proposed by Azevedo and Gottlieb (2016) to address the challenges of evaluating counterfactual policies in competitive markets with adverse selection.

1.7.1 The Impact of Home Equity and Ex-Post Balance Writedowns

The estimated moral hazard effect has direct policy relevance. It captures the causal effect of a change in home equity on the probability of default, which is necessary to predict the effectiveness of ex-post principal writedowns in preventing mortgage defaults. The estimates in Table 1.4 suggest that, for the sample studied here, a 10 percentage point reduction in all borrowers' LTV at 24 months would have reduced defaults within a year by just over 15 percent. Relative to the literature, these estimates are on the large side,but not outside of normal bounds. For example, Bajari, Chu and Park (2008) find that a 25-point increase in the current LTV is necessary to generate a 15 percent increase in the default probability. Alternatively, Elul et al. (2010), find that borrowers with increasing CLTV from between 100 and 110 to between 110 and 120 raises the quarterly default hazard by about 30 percent of the mean.

After the crisis, policies of this form were enacted, for example the Home Affordable Mortgage Refinance Program Principal Reduction Alternative (HAMP PRA). Scharlemann and Shore (2016) use a kink in the schedule for HAMP PRA to analyze the effectiveness of the regulation. They estimate that principal writedowns— balance reductions of 28 percent on average—reduced the quarterly delinquency hazard by 18 percent (from 3.8 percent per quarter to 3.1 percent). However, their study examines only those who participated in the program (and hence were already delinquent), while my estimates consider the full population of active borrowers.

1.7.2 A Model to Evaluate Ex-Ante Regulations

Understanding the effects of ex-ante regulations on welfare requires the specification of a model of borrower and lender behavior, and an equilibrium concept. I begin with the model, which is a minor expansion of the one presented in Section 1.2.

Consumer Preferences

Given a contract $\{L_k, B(L_k)\}$, I characterize the observed portion of a borrower's ex-ante utility exactly as in Section 1.2:

$$U_i(L_k) = u(y_0 - (H_0 - L_k)) + \beta \Big[\underbrace{\int_{\underline{h}}^{B(L_k)-C_i} u(y_1 - C_i) dF(H_1)}_{\text{Default}} + \underbrace{\int_{B(L_k)-C_i}^{\bar{h}} u(y_1 + H_1 - B(L_k)) dF(H_1)}_{\text{Repayment}} \Big].$$

As in the theoretical model, the only source of heterogeneity in U_i is C_i, the borrower's private costs of default. However, in practice, borrowers choose mortgages on the basis of a number of factors beyond just their default costs. Recall that the estimated correlation between the leverage choice and a borrower's private default costs was only 0.07. In a richly specified model, initial mortgage choice might also be a function of heterogeneity in borrower's income, preferences (e.g. risk aversion or intertemporal elasticity of substitution), or period 0 knowledge of future C_i.

I abstract from these details and consider a simplified model in which borrowers' utility for a contract with a particular leverage choice is characterized by an observed portion, as defined above, and an independent, idiosyncratic error ϵ_{iL}:

$$V_i(L_k) = U_i(L_k) + \epsilon_{iL}.$$

This error captures, in a reduced form way, all factors that influence borrowers with the same C_i to choose different contracts. When the variance of ϵ_{iL} is high, there is a weak relationship between C_i and the chosen L. When the variance is low, the correlation increases.

It is convenient to specify ϵ_{iL} to be type 1 extreme value, in which case a borrower's choice probability for a given L can be written as:

$$P_{ik} = \frac{e^{\gamma U_i(L_k)}}{\sum_{k'} e^{\gamma U_i(L_{k'})}},$$

where γ is a viscosity parameter determined by the variance of ϵ_{iL}. Of course, this specification imposes a standard independence of irrelevant alternatives (IIA) assumption, which may not hold in a more sophisticated model of heterogeneity across borrowers.

Lender Profits

With these choice probabilities in hand, computing lender profits is straightforward. I assume lenders are able to recover a fraction $\delta \leq 1$ of what the home is worth in the case of default. The expected profits of a lender selling contract $\{L_k, B(L_k)\}$ to borrower i with private default cost C_i are:

$$\pi(L_k, B(L_k); C_i) = -L_k + \frac{1}{1+r_f}\Bigg[\underbrace{\int_{\underline{h}}^{B(L_k)-C_i} \delta H_1 dF(H_1)}_{\text{Default}} + \underbrace{\int_{B(L_k)-C_i}^{\overline{h}} B(L_k) dF(H_1)}_{\text{Repayment}}\Bigg].$$

The expected profits of a lender are the profits for each individual i, multiplied by the probability that i chooses contract k, integrated over the distribution of C_i (specified here as $G(C)$):

$$\Pi_k = \int P_{ik}\pi(L_k, B(L_k); C_i) dG(C).$$

Equilibrium Concept

There is no clear consensus on the appropriate definition of equilibrium in competitive markets with adverse selection (Chiappori and Salanié, 2013). Furthermore, because equilibria often fail to exist under standard concepts, e.g. Rothschild-Stiglitz, evaluating the counterfactual implications of policy can be difficult. However, a recent development by Azevedo and Gottlieb (2016) characterizes an equilibrium concept that is both robust—an equilibrium always exists—and straightforward to implement in a variety of applications. Equilibria of this form satisfy three requirements: (i) consumers optimize over the available set of contracts, (ii) lenders make zero profits on each contract, and (iii) there is free entry, in the sense that the equilibrium is robust to small perturbations, as defined formally in Azevedo and Gottlieb (2016).

For the purposes of simulation, utilizing this equilibrium concept is straightforward. I calculate equilibrium in what Azevedo and Gottlieb (2016) call a perturbation. I propose a fixed set of contracts (in the example presented, every integer LTV between 50 and 100). I then consider a mass of uniformly distributed behavioral borrowers equal to 1 percent of the population, who always choose a given contract. Behavioral borrowers pay back the loan in all states of the world and, as a result are costless to the lender. I use a fixed point algorithm to determine equilibrium. In each iteration, consumers choose optimally taking prices as given, and interest rates are adjusted up or down for profitable or unprofitable contracts. Convergence is achieved when profits across all contracts fall below a predefined threshold. The existence of behavioral borrowers is crucial for convergence to intuitive equilibria. Because behavioral borrowers are costless, the interest rate on any contract that is only purchased by these types is reduced until either (i) a risky borrower is indifferent between the contract and his current choice or (ii) the interest rate reaches the risk free rate. This rules out equilibria with contracts that have arbitrarily high prices and only make zero profits because they are not chosen.

Calibration

I calibrate three features of the simulation to the estimates from Table 1.6. I define the mean and variance of the private costs of default based on those estimated in Column 4 of 1.6. Furthermore, I choose γ, or equivalently the variance of ϵ_{iL}, so that the correlation between borrowers' choice of L and C_i in Regime I below matches the estimated $\rho_{\varepsilon v}$ in Column 4. All other parameters are set based on the data when possible and explicitly described in the bottom panel of Table 1.7. For the purposes of the simulation, I assume that borrowers have exponential utility, with CARA coefficient a.

1.7.3 Welfare Implications of an LTV Cap

I consider the implications of a decreased LTV cap, that is, a limit on the initial loan provided by lenders. This can be thought of as roughly the mirror image of a standard policy in insurance markets: a mandated minimum level of coverage. I evaluate three policy regimes:

(I) **LTV Cap of 100**: In the first regime, lenders do not observe C_i, and equilibrium is as discussed above, with all loans making zero profits. The set of potential contracts contains all original LTVs between 50 and 100.

(II) **LTV Cap of 90 (No Supply Response)**: The second regime presents a naive view of the impact of an LTV cap of 90, ignoring the impacts of adverse selection. This regime evaluates the choices made by borrowers if an LTV cap of 90 were implemented but lenders did not otherwise adjust their contracts. As a result, lenders may make positive or negative profits under this regime.

(III) **LTV Cap of 90 (With Supply Response)**: The final regime considers the equilibrium allocation of credit when lenders are able to endogenously adjust contracts in response to a change in the LTV cap.

A Naive Evaluation of an LTV Cap: No Supply Response

I first consider a comparison of Regimes I and II, which can be thought of as the anticipated response to an LTV cap for a naive policymaker. For these purposes, I consider a naive regulator to be one who understands borrower preferences and can anticipate the contracts borrowers will choose from any given set, but who disregards adverse selection. Such a policy maker believes that the proportion of defaults for a given contract does not depend on the population purchasing that contract, and hence that there will be no supply response to a change in the LTV cap. The intuition behind this comparison is demonstrated by the dark and light gray bars in Figure 1.8. This figure shows results with an exaggerated degree of adverse selection, to better present the patterns across the three regimes, while Table 1.7 presents numbers based on simulations calibrated to the empirical results.

The black bars illustrate the allocation of original LTV under Regime I and exhibit a basic pattern of adverse selection. While borrowers would prefer initial loans with LTVs of 100 in a world with perfect information, the clustering of the riskiest borrowers raises the interest rate of a 100 LTV loan significantly. As a result, safe borrowers take smaller loans to distinguish themselves from risky types and avoid paying inflated interest rates.

Under the naive view, the only borrowers impacted by the regulation are those initially choosing LTVs above 90. The borrowers who choose contracts with original LTVs below 90 in Regime I will continue to do so, while the majority of those choosing original LTVs above 90 will bunch close to the LTV cap, creating the large mass of borrowers captured by the gray bars in Figure 1.8.[48] Furthermore, the naive view will expect a significant reduction in defaults generated by the regulation. Because it assumes no heterogeneity across borrowers in default propensities, it also expects that those that choose an LTV of 90 under Regime II will default at the same rate as borrowers choosing an LTV of 90 under Regime I.

Columns 1 and 2 of Table 1.7 compare Regimes I and II. There is indeed a reduction in loan size, from \$270,055 to \$246,265, and a corresponding reduction in average interest rates from 8.6 to 7.5 percent. This corresponds to a welfare loss of just over \$8,000. Under the naive view, the expected number of defaults is significantly larger than the true reduction, even without a supply response. The naive view suggests that an LTV cap of 90 would cut the proportion of defaults by 35 percent, from 0.12 to 0.078. Appropriately accounting for the risk of the borrowers initially allocated above 90 reveals the true reduction to be just 24 percent.

Allowing a Supply Response

In addition to overstating the reduction in defaults generated by the regulation, the naive view understates the reduction in mortgage size generated by knock-on effects of the regulation. Reducing the LTV cap does indeed force some risky borrowers to decrease their LTV to 90. However, as a result, the interest rates on 90 LTV loans must also rise. Correspondingly, some borrowers who previously chose LTVs of 90 will choose slightly smaller loans, thereby leading lenders to increase interest rates on those smaller loans, causing further knock-on effects. In the presence of adverse selection, leverage can be seen as a sorting device. Eliminating high LTV loans does not eliminate the incentive of borrowers to distinguish themselves, but instead forces them to do so over a smaller range of loans.

The leftward shift of the white bars in Figure 1.8 relative to the light gray bars demonstrates

[48] Because borrowers have a random component ϵ_{iL} of their preference for contracts, and because of the IIA assumption, borrowers who initially chose LTVs above 90 will not strictly choose contracts at 90. Rather, they will distribute their choices across remaining loans such that the relative choice probabilities are the same before and after the regulation.

the additional reduction in mortgage size due to knock-on effects. In the calibrated simulations of Regime III, shown in the third column of Table 1.7, the knock-on effects cause an additional reduction in loan size of more than $250 on average. Furthermore, because lenders in Regime III appropriately account for the reallocation of risky borrowers, the interest rates of all contracts rise from 7.5 percent on average to 7.9 percent. As a result, the average borrower has a final balance that is nearly $600 larger under Regime III than Regime II. The reduction in loan size and increased borrower balance combine to generate a welfare loss that is $617 larger per borrower in Regime III as compared to Regime II.

Optimal regulation involves balancing reductions in defaults with the welfare loss that results from borrowers taking smaller loans. In the simulations provided here, a naive regulator overstates the number of defaults by 11 percent and underestimates the welfare loss due to reductions in loan size by 7.5 percent. The naive estimates suggest that for this regulation to be welfare neutral, default externalities on the order of $194,000 per default are necessary. When accounting for adverse selection, much larger default externalities are necessary to justify the regulation, on the order of $313,000 per default.

1.8 Conclusion

In this paper, I empirically separate moral hazard from adverse selection in the mortgage market. I begin by developing a theoretical framework to highlight the sources of information asymmetries. In the model, moral hazard exists as the result of limited recourse: lenders cannot contract against borrowers choosing to default when it is in their ex-post interest to do so. Adverse selection, on the other hand, results from borrower heterogeneity in willingness to default. Borrowers differ in access to liquidity, value of future credit access, attachment to the home, and many other factors that influence the default choice. If borrowers know about this heterogeneity when choosing mortgage contracts, riskier borrowers will tend to prefer larger loans.

The primary empirical contribution comes in separating adverse selection from moral hazard. I do so by exploiting a natural experiment resulting from two features of Option ARMs: fixed payments and variable interest rates. Because monthly payments do not change, the balances borrowers owe are a direct function of market interest rates. This creates a distinction between borrowers' initial leverage choices and the balances they owe ex-post. To isolate plausibly exogenous variation in balances, I focus on difference-in-difference variation in interest rates that comes as the result of the financial index used to determine rate adjustments. Because of the unexpected divergence between LIBOR and Treasury rates during the crisis, borrowers experienced substantially different balances as a function of the loan's index and origination month.

This variation in borrowers' balances allows me to construct a series of instruments to identify the causal effect of home equity on default—the moral hazard effect—and subsequently to back out the role of adverse selection. I find significant evidence of both information asymmetries. Moral hazard is responsible for 60-70 percent of the baseline correlation between leverage and default, while adverse selection is responsible for the remaining 30-40 percent. The estimated moral hazard effect at 24 months suggests that a policy that reduced all borrowers' loan-to-values at 24 months would have reduced defaults by over 8 percent.

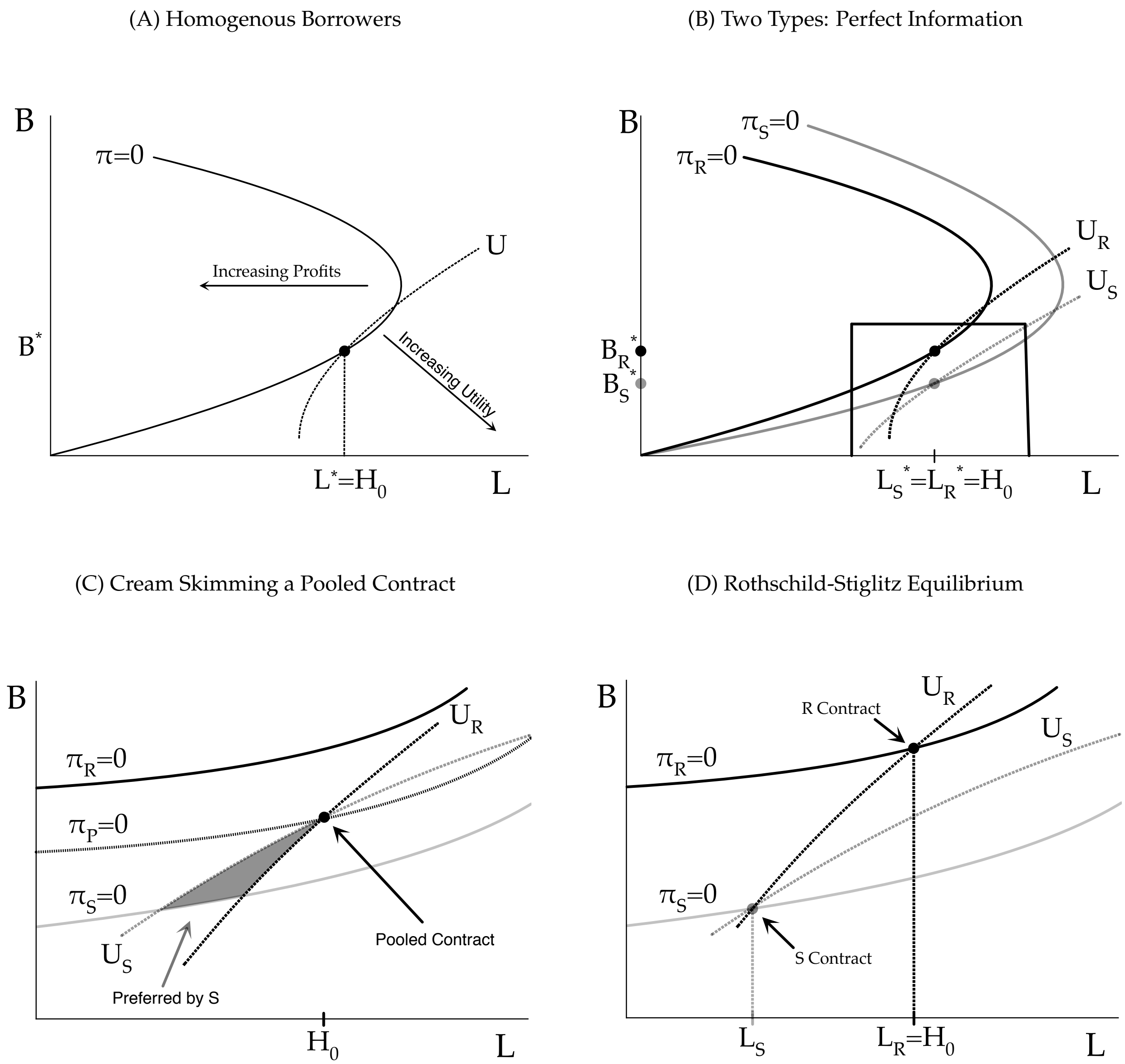
The main welfare implications of adverse selection come in its impact on equilibrium. As in standard insurance models, adverse selection imposes an externality on low risk borrowers, who must take smaller loans than they would in a world with perfect information in order to distinguish themselves from riskier types. The final contribution of this paper is to construct and simulate a model of competitive equilibrium to consider the consequences of this externality for policy. Because even defining competitive equilibrium is a notorious challenge in the presence of adverse selection, I use the robust equilibrium concept recently developed by Azevedo and Gottlieb (2016).

I evaluate the impact of a reduction in an LTV cap, a common policy aimed at reducing defaults by limiting borrowers' initial leverage. I find that a naive policymaker who does not account for adverse selection will significantly *overestimate* the number of defaults prevented by a reduced LTV cap and significantly *underestimate* the welfare losses generated by borrowers' taking smaller loans. The effects of the cap propagate through the distribution. Risky borrowers are forced to take smaller loans, but safer borrowers choose to do so as well in order to differentiate themselves. I estimate that externalities on the order of $313,000 per default are necessary to make a reduction in the LTV cap from 100 to 90 welfare neutral.

This paper separates adverse selection from moral hazard in a particular segment of the mortgage market and examines a single policy. However, the relative role of these information asymmetries is relevant to some of the most important policy questions for the market as a whole. There is significant debate over a number of core mortgage regulations in the US, including the mortgage interest tax deduction and the role of the GSEs. Some argue that the potential magnitude of positive externalities from homeownership is insufficient to justify the current level of intervention in the mortgage market. The existence of adverse selection provides an additional rationale for intervention. Even in the absence of positive homeownership externalities, policies that encourage borrowers to take on larger loans may be welfare enhancing in the presence of adverse selection.

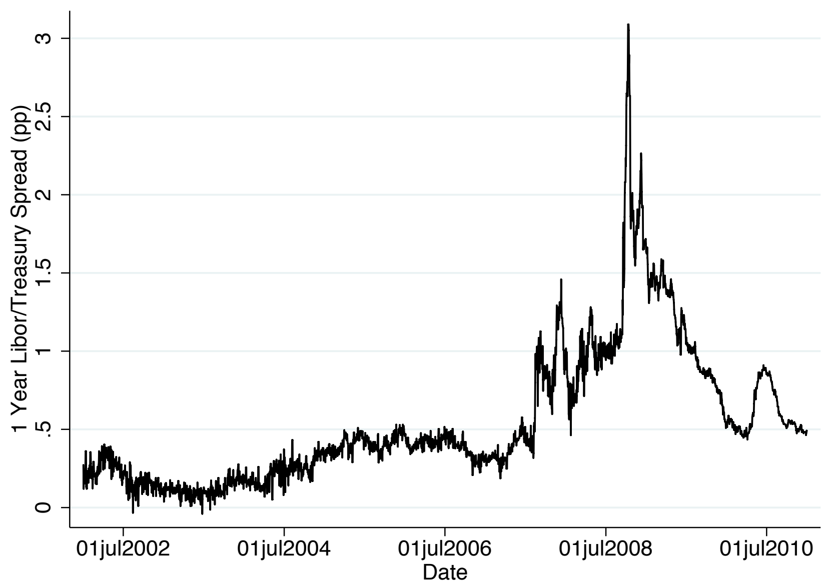
Along these lines, understanding the importance of information asymmetries may help to explain when and why some segments of the mortgage market break down. For some observable portions of the population, mortgage credit is effectively unavailable. If this is due to moral hazard, there is little room for welfare improving intervention. Defaults may simply be so high in those populations that no interest rate is profitable for borrowers, even in the absence of adverse selection. However, if these markets are unravelling due to adverse selection, there may indeed be place for regulation. While this paper provides only a first step, fully understanding the relative roles of adverse selection and moral hazard is key to determining the effectiveness of a broad class of mortgage policies.

FIGURE 1.1
THEORY: EQUILIBRIUM CONTRACTS WITH AN LTV CAP OF 100



Each graph shows the contract space for stylized two period mortgage loans. Balance is shown on the y-axis, the initial loan is shown on the x-axis. Borrowers' indifference curves are marked with U, and lenders' zero profit lines are marked with Π. Borrower utility increases with contracts offered to the southeast, lender profits increase with contracts offered to the west. Panel A displays the equilibrium contract with a single borrower type and an LTV cap of H_0. Panel B shows the equilibrium contract with a risky (R) and safe (S) borrower, and perfect information. Both borrowers receive loans of H_0, but the riskier type pays a higher interest rate. Panel C zooms in on the boxed area of Panel B, and shows the difficulty of sustaining a pooled contract with single crossing when lenders cannot observe risk types. The shaded region shows profitable contracts preferred by the borrower to the pooled contract. Panel D shows the form of a Rothschild-Stiglitz equilibrium, should it exist. The safe borrower takes a smaller loan, $L_S < L_R$, than they would with perfect information.

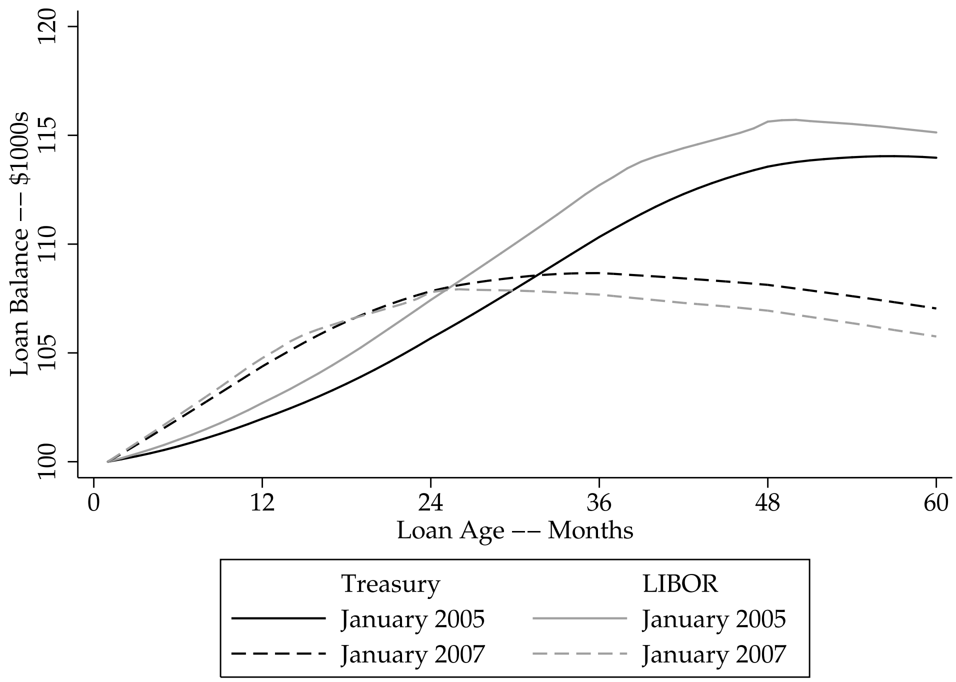
FIGURE 1.2
PANEL A: SPREAD BETWEEN LIBOR AND TREASURY INCREASED DRAMATICALLY DURING CRISIS



PANEL B: SPREAD IS MIRRORED IN INTEREST RATES AND DEFAULT PATTERNS FOR ARMS

Top panel shows spread between 1-year LIBOR and 1-year Constant Maturity Treasury (CMT) between 2002 and 2010. The black line in the bottom panel shows the difference in (reset) rates between LIBOR-indexed loans and Treasury-indexed loans resetting in the corresponding month. The lighter line shows the difference in the one year default probability between LIBOR and Treasury indexed loans resetting in that month. A large sample of adjustable rate mortgages of different types are included. These figures are recreations of Figure IV, Panel B and Figure V, Panel A of Gupta (2016).

FIGURE 1.3
INDEX × ORIGINATION MONTH GENERATES DIFFERENCE-IN-DIFFERENCE VARIATION IN BALANCE TRAJECTORIES FOR OPTION ARMS



Simulated balance trajectories for $100,000 LIBOR- and Treasury-indexed loans originated in January 2005 or January 2007. Trajectories assume margin of 3.5 percent and initial payment based on 1.75 percent teaser. Treasury refers to 1-year MTA, LIBOR refers to 3-month duration.

FIGURE 1.4
INTEREST RATE SCHEDULE IS INCREASING IN ORIGINAL LOAN-TO-VALUE

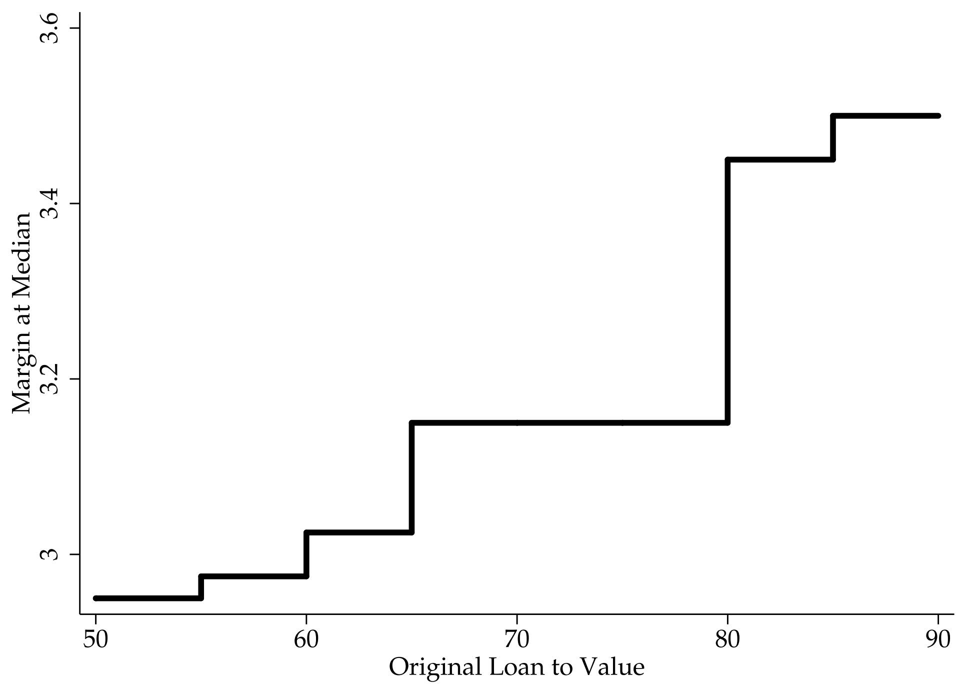
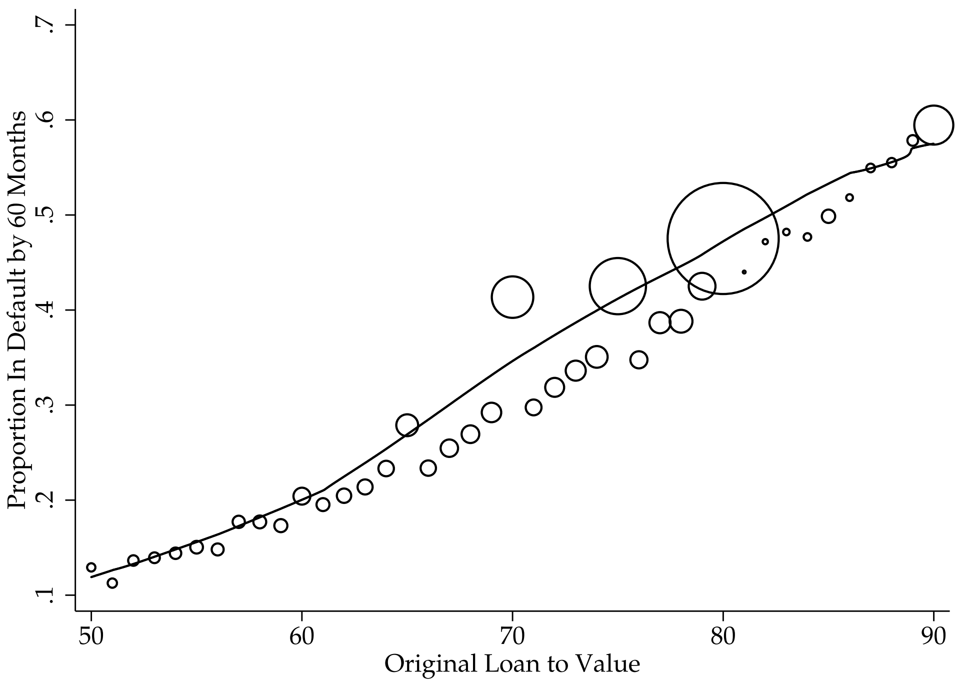


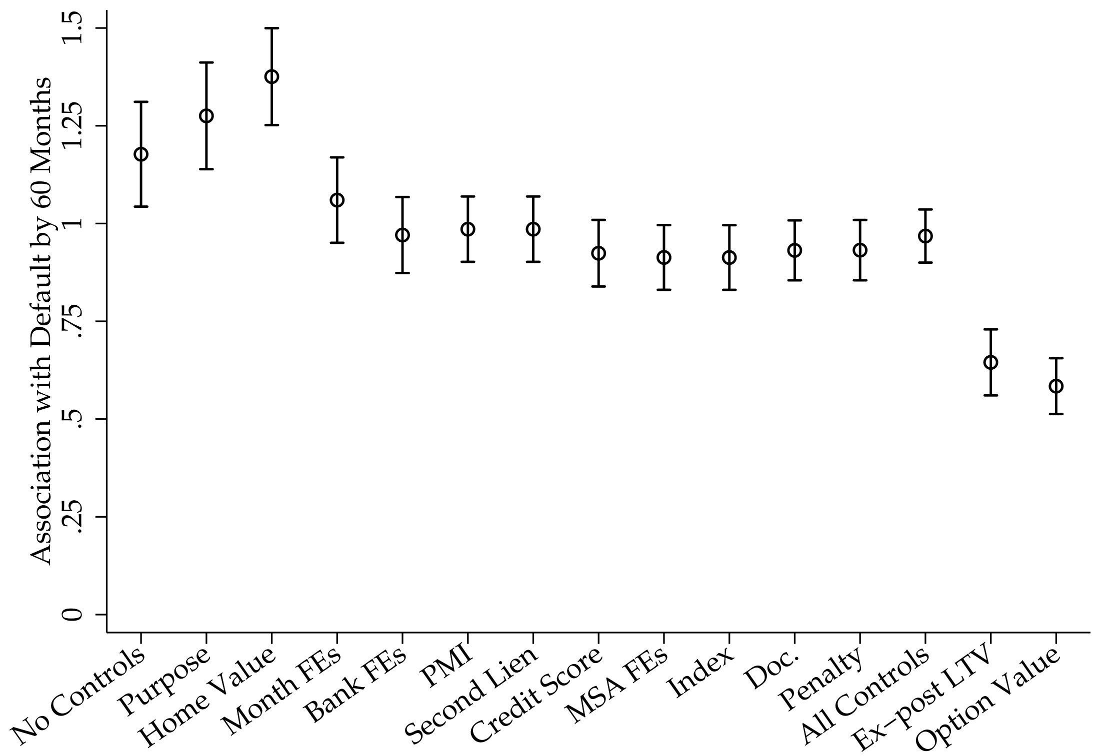
Figure displays the median margin, the fixed portion of each borrower's interest rate, for each 5 point loan-to-value bin. Full sample of Option ARMs is included.

FIGURE 1.5
ORIGINAL LTV IS POSITIVELY CORRELATED WITH DEFAULT WITHIN 60 MONTHS



Hollow dots show the average proportion of loans defaulting within the first 60 months for each 1-point bin of original loan-to-value. Size of dots is proportional to number of borrowers within each bin. Default is defined as 60 or more days past due. The solid line shows a local linear smoothing of the raw data. Full sample of Option ARMs is included.

FIGURE 1.6
CORRELATION BETWEEN ORIGINAL LTV AND DEFAULT HOLDS CONDITIONAL ON INFORMATION AVAILABLE TO LENDER

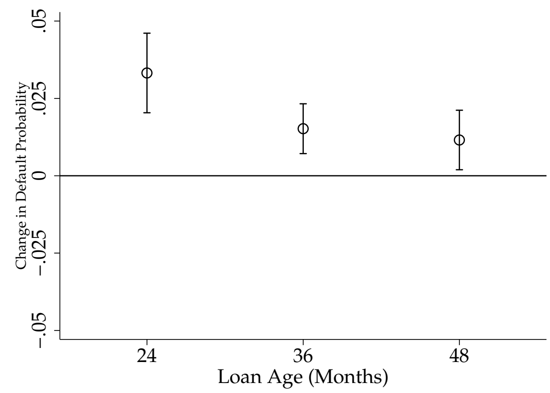


Results from OLS regressions of default within the first 60 months on original loan-to-value. Circles show coefficients on original loan-to-value with 95 percent confidence intervals based on standard errors clustered at the MSA level. The leftmost coefficient includes no controls, and each step to the left increases the set of controls included. Purpose refers to dummies for purchase, refinance, or cash out refinance. Bank FEs include originator and servicer fixed effects. Credit score refers to dummies for each 20-point bin of original FICO, with an additional category for missing values. Index is a dummy for LIBOR. Penalty is equal to one if the loan features a prepayment penalty. Full controls additionally includes a dummy for single family home and investor vs. owner occupant. Ex-post LTV refers to the imputed loan-to-value at 60 months based on initial contract terms. Option value provides a more flexible specification of mortgage value, including six leads and lags of home prices and interest rates at 60 months. Full sample of Option ARMs is included.

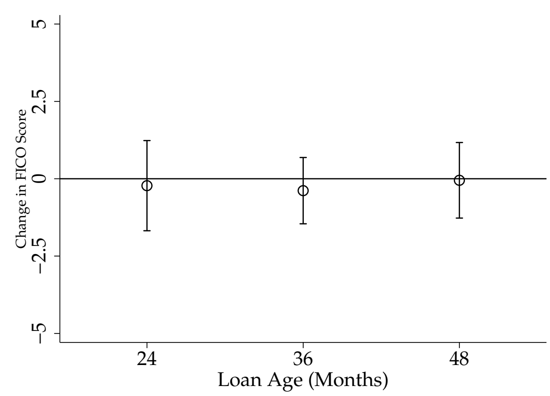
FIGURE 1.7
REGRESSIONS OF DEFAULT AND CREDIT SCORE ON INSTRUMENT FOR HOME EQUITY

PANEL A: INSTRUMENT RELEVANCE—INSTRUMENT PREDICTS CURRENT LOAN-TO-VALUE

PANEL B: REDUCED FORM—INSTRUMENT PREDICTS DEFAULT ACROSS LIFECYCLE

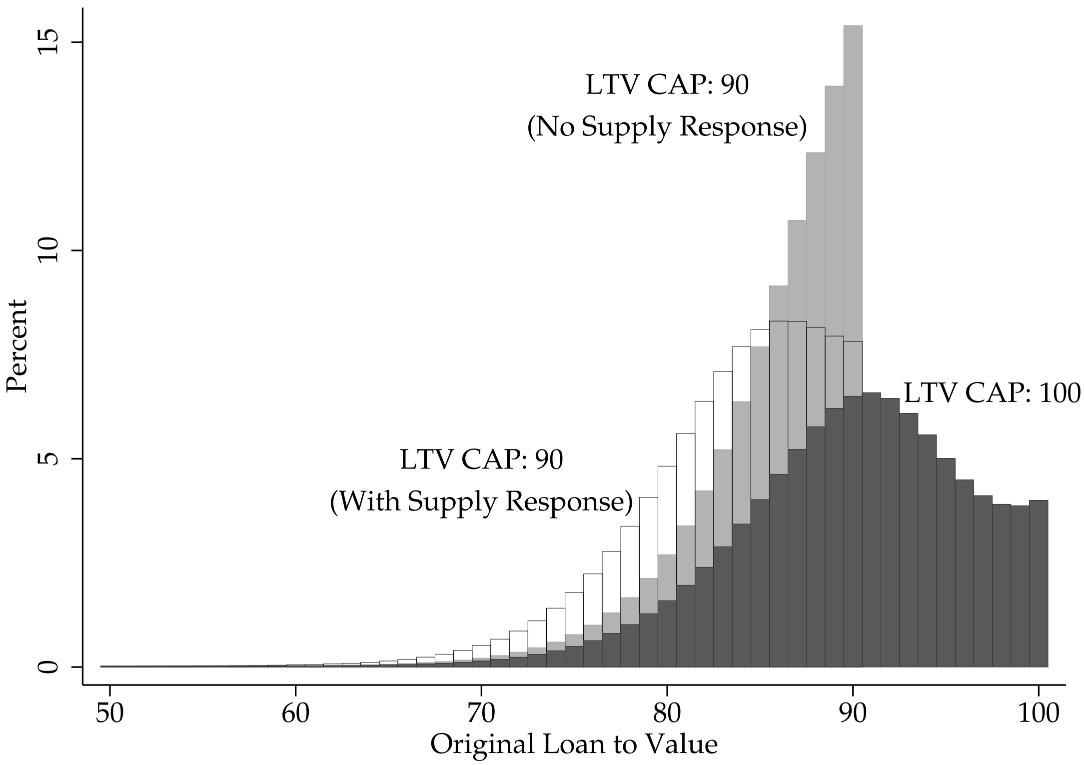


PANEL C: INSTRUMENT EXOGENEITY—INSTRUMENT DOES NOT PREDICT CREDIT SCORE



OLS regressions of outcomes on simulated instrument including origination month and index fixed effects. The simulated instrument is the mechanical calculation of balance based upon the borrowers' index choice and origination month. Margin is fixed to 3.5 for all borrowers, original loan to $100,000 and initial monthly payment is based on 1.75 percent teaser rate. Top panel shows the outcome of default within one year at 24, 36, and 48 months, where default is defined as being 60 or more days past due. Bottom panel shows the outcome of borrowers FICO scores for those remaining at 24, 36 and 48 months.

FIGURE 1.8
EFFECT OF LTV CAP OF 90 ON LEVERAGE: WITH AND WITHOUT SUPPLY RESPONSE



Bars show simulated proportion of borrowers choosing each original LTV under three regimes. The dark gray bars show equilibrium LTV choices at an LTV cap of 100, the light gray show borrowers' LTV choices after a reduction in the LTV cap to 90, but allowing no changes in contracts below 90. White bars show equilibrium LTV choices with an LTV cap of 90 allowing for the supply response. Figure is based on exaggerated level of adverse selection. Table 1.7 shows appropriately calibrated results.

TABLE 1.1
SUMMARY STATISTICS: BALANCE ACROSS INDICIES

	Treasury		Libor	
	Mean	SD	Mean	SD
FICO Score	706.1	45.9	713.8	45.1
Original Balance	370.5	264.4	346.1	282.1
Loan for Purchase	0.33		0.42	
No/Low Documentation	0.79		0.77	
Primary Residence	0.77		0.68	
Condo, Co-op or Multifamily	0.14		0.16	
Prepayment Penalty	.99		0.94	
Margin	3.21	0.53	2.85	0.51
Original LTV	76.6	8.40	77.0	8.30
State:				
- California	0.46		0.35	
- Florida	0.14		0.16	
- Arizona	0.043		0.040	
- Nevada	0.037		0.054	
Origination Year:				
- 2004	0.081		0.31	
- 2005	0.41		0.35	
- 2006	0.43		0.24	
- 2007	0.082		0.089	
Observations	490132		45199	

Summary statistics for full sample of Option ARMs. Treasury refers to loans indexed to Treasury rates, LIBOR refers to those indexed to LIBOR.

TABLE 1.2
POSITIVE CORRELATION TESTS: ORIGINAL LEVERAGE AND INTEREST RATE PREDICT DEFAULT

	Panel A: Association Between Contract Terms and Delinquency by 60 Months							
	No Controls	Month FEs	Full Controls	Low/No Doc	No Controls	Month FEs	Full Controls	Low/No Doc
Original Loan-to-Value	1.177***	0.929***	0.992***	1.122***				
	(0.068)	(0.064)	(0.037)	(0.040)				
Margin					0.222***	0.113***	0.068***	0.071***
					(0.007)	(0.005)	(0.004)	(0.005)
Mean of Dep. Var.	0.42	0.42	0.42	0.47	0.42	0.42	0.42	0.47
Raw Correlation	0.20	0.20	0.20	0.22	0.24	0.24	0.24	0.24
χ^2 Test Statistic	22585	15096	11018	1257	30362	7680	414	618
N	534909	534909	534909	419645	534909	534909	534909	419645
Origination Month FEs	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Index FEs	No	No	Yes	Yes	No	No	Yes	Yes
MSA FEs	No	No	Yes	Yes	No	No	Yes	Yes
Full Controls	No	No	Yes	Yes	No	No	Yes	Yes
	Panel B: Association Between Contract Terms and Delinquency by Year							
	12 Months	24 Months	36 Months	48 Months	12 Months	24 Months	36 Months	48 Months
Original Loan-to-Value	0.097***	0.452***	0.803***	0.956***				
	(0.007)	(0.023)	(0.030)	(0.035)				
Margin					0.013***	0.047***	0.074***	0.076***
					(0.001)	(0.002)	(0.004)	(0.004)
Mean of Dep. Var.	0.035	0.15	0.29	0.38	0.035	0.15	0.29	0.38
Raw Correlation	0.06	0.15	0.19	0.20	0.10	0.21	0.26	0.25
χ^2 Test Statistic	175	1663	1290	1878	172	1008	821	974
N	534909	534909	534909	534909	534909	534909	534909	534909
Origination Month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Full Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Results from OLS regressions of the binary outcome of default on borrowers' original loan-to-value or margin and various controls. Default is defined as 60 or more days past due. Full controls refers to fixed effects for index type, documentation, the loans purpose and occupancy, the existence of prepayment penalties and private mortgage insurance, and single family homes. I also include indicators for each 20-point bin of borrowers' FICO credit scores, loan originator and servicer fixed effects, and controls for second liens. I allow individual state time trends. Raw correlation refers to the pairwise correlation between original loan-to-value or margin and default. The χ^2 test statistic is the test suggested in Chiappori and Salanie (2000), and tests for a correlation between the generalized residual from probit versions of the specifications presented here, and the residual from an OLS regression of original loan-to-value or margin on the listed covariates. Standard errors are clustered at the MSA level. * denotes 10% significance, ** denotes 5% significance, *** denotes 1% significance.

TABLE 1.3
FIRST STAGE: INSTRUMENTS PREDICT REALIZED NEGATIVE EQUITY AND LOAN-TO-VALUE

	Panel A: OLS Regressions of Observed Loan-to-Value and Home Equity on Simulated Instruments							
	24 Months		48 Months		24 Months		48 Months	
Simulated Home Equity ($100,000s)	1.345*** (0.214)	0.795*** (0.197)	0.314** (0.152)	−0.001 (0.108)				
Simulated Loan-To-Value					3.559*** (0.621)	1.736*** (0.471)	1.076** (0.521)	0.090 (0.459)
F (Simulated Instrument)	39.5	16.3	4.3	0.0	32.8	13.6	4.3	0.0
F (Fixed Effects)	10.3	7.1	6.0	3.8	5.0	11.3	4.0	4.6
N	265134	265134	107917	107917	268364	268364	108987	108987
Orig. Month/Index FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	No	Yes	No	Yes	No	Yes	No	Yes
Full Controls	No	Yes	No	Yes	No	Yes	No	Yes
	Panel B: OLS Regressions of Imputed Loan-to-Value and Home Equity on Simulated Instruments							
	24 Months		48 Months		24 Months		48 Months	
Simulated Home Equity ($100,000s)	1.349*** (0.226)	0.339** (0.164)	0.593*** (0.133)	0.273*** (0.105)				
Simulated Loan-To-Value					2.632*** (0.463)	0.545 (0.363)	1.835*** (0.426)	0.287 (0.329)
F (Simulated Instrument)	35.7	4.2	19.8	6.8	32.3	2.3	18.5	0.8
F (Fixed Effects)	11.3	6.8	8.5	5.4	8.1	8.5	7.2	8.3
N	443600	443600	443600	443600	443600	443600	443600	443600
Orig. Month/Index FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	No	Yes	No	Yes	No	Yes	No	Yes
Full Controls	No	Yes	No	Yes	No	Yes	No	Yes

First stage regressions of measures of borrower equity on instruments for equity based on borrowers' index and month of origin. Displayed in each column is the coefficient from regressing borrowers' true equity value, measured either as the loan-to-value ratio (in percentage terms) or as the level of negative equity in $100,000s,on the simulated instrument for that equity. The simulated instrument is calculated using the borrowers true index and origination month to determine interest rates but fixing all other loan terms to standard values: a margin of 3.5%, an initial minimum payment based upon a 1.75% teaser rate, home price appreciation equal to the national average, and the assumption that the borrower always makes minimum payments. With these terms, home equity can be calculated mechanically. The F(simulated instrument) is the F-statistic from this regression, while F(Fixed effects) is the F-statistic from regressions that include the full set of interactions between origination month and index type as instruments. Full controls refers to fixed effects for index type, documentation, the loans purpose and occupancy, the existence of prepayment penalties and private mortgage insurance, and single family homes. I also include indicators for each 20-point bin of borrowers' FICO credit scores, loan originator and servicer fixed effects, and controls for second liens. I allow individual state time trends. Standard errors are clustered at the MSA level. * denotes 10% significance, ** denotes 5% significance, *** denotes 1% significance.

TABLE 1.4
SEPARATING ADVERSE SELECTION AND MORAL HAZARD:
THE IMPACT OF ORIGINAL AND CURRENT LEVERAGE ON 1 YEAR DEFAULT PROBABILITIES

	Panel A: OLS and IV Regressions at 24 Months Including Current Negative Equity					
	Baseline	OLS	IV	Baseline	OLS	IV
Original Loan-to-Value	0.586***	0.252***	0.331***	0.721***	0.407***	0.260***
	(0.046)	(0.054)	(0.118)	(0.026)	(0.037)	(0.047)
Current Negative Equity in		0.059***	0.045**		0.061***	0.089***
$100,000s		(0.006)	(0.021)		(0.005)	(0.010)
Mean of Dep. Var	0.264	0.264	0.264	0.264	0.264	0.264
N	265134	265134	265134	265134	265134	265134
Origination Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	No	No	No	Yes	Yes	Yes
Full Controls	No	No	No	Yes	Yes	Yes
	Panel B: OLS and IV Regressions at 24 Months Including Current Loan-to-Value					
	Baseline	OLS	IV	Baseline	OLS	IV
Original Loan-to-Value	0.586***	−0.059	−0.241	0.721***	0.244***	0.229***
	(0.046)	(0.056)	(0.234)	(0.026)	(0.053)	(0.050)
Current Loan-to-Value		0.573***	0.735***		0.402***	0.415***
		(0.029)	(0.212)		(0.037)	(0.041)
Mean of Dep. Var	0.264	0.264	0.264	0.264	0.264	0.264
N	265134	265134	265134	265134	265134	265134
Origination Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	No	No	No	Yes	Yes	Yes
Full Controls	No	No	No	Yes	Yes	Yes

OLS and IV regressions of default between 24 and 36 months on borrowers original loan-to-value and current equity at 24 months, defined as either the level of negative equity in $100,000s (Panel A), or current loan-to-value (Panel B). Default is defined as 60 or more days past due. Baseline refers to OLS regressions omitting current equity. IV regressions include the full set of interactions between index and origination month as instruments for current equity. Full controls refers to fixed effects for index type, documentation, the loans purpose and occupancy, the existence of prepayment penalties and private mortgage insurance, and single family homes. I also include indicators for each 20-point bin of borrowers' FICO credit scores, loan originator and servicer fixed effects, and controls for second liens. I allow individual state time trends. Standard errors are clustered at the MSA level. * denotes 10% significance, ** denotes 5% significance, *** denotes 1% significance.

TABLE 1.5
HETEROGENEITY IN THE IMPACT OF ORIGINAL AND CURRENT LEVERAGE ON 1 YEAR DEFAULT PROBABILITY

	Panel A: IV Regressions at 24 Months by State Recourse Status					
	Full Recourse			Limited Recourse		
Original Loan-to-Value	0.546***	0.530***	0.494***	0.741***	0.276***	0.239***
	(0.044)	(0.116)	(0.179)	(0.025)	(0.045)	(0.057)
Current Negative Equity in $100,000s		0.003			0.089***	
		(0.034)			(0.009)	
Current Loan-to-Value			0.044			0.421***
			(0.189)			(0.044)
Mean of Dep. Var	0.198	0.198	0.198	0.272	0.272	0.272
N	28565	28565	28565	236569	236569	236569
Origination Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes	Yes
Full Controls	Yes	Yes	Yes	Yes	Yes	Yes
	Panel B: IV Regressions at 24 Months by Original Documentation					
	No/Low Documentation			Full Documentation		
Original Loan-to-Value	0.766***	0.355***	0.336***	0.492***	−0.017	−0.095*
	(0.026)	(0.056)	(0.058)	(0.031)	(0.067)	(0.057)
Current Negative Equity in $100,000s		0.077***			0.113***	
		(0.010)			(0.015)	
Current Loan-to-Value			0.356***			0.538***
			(0.045)			(0.052)
Mean of Dep. Var	0.286	0.286	0.286	0.166	0.166	0.166
N	215366	215366	215366	49768	49768	49768
Origination Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes	Yes
Full Controls	Yes	Yes	Yes	Yes	Yes	Yes
	Panel C: IV Regressions at 24 Months by Loan Purpose					
	Purchase			Refinance		
Original Loan-to-Value	0.587***	0.119**	0.076	0.762***	0.313***	0.277***
	(0.033)	(0.056)	(0.050)	(0.026)	(0.050)	(0.059)
Current Negative Equity in $100,000s		0.101***			0.084***	
		(0.013)			(0.009)	
Current Loan-to-Value			0.452***			0.405***
			(0.042)			(0.048)
Mean of Dep. Var	0.252	0.252	0.252	0.270	0.270	0.270
N	93226	93226	93226	171908	171908	171908
Origination Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes	Yes
Full Controls	Yes	Yes	Yes	Yes	Yes	Yes

OLS and IV regressions of default between 24 and 36 months on borrowers original loan-to-value and current equity at 24 months, defined as either the level of negative equity in $100,000s (or current loan-to-value). Baseline refers to OLS regressions omitting current equity, all other specifications are IV regressions including the full set interactions between index and origination month as instruments for current equity. States are categorized as full recourse if they are considered to have strong provisions regarding deficiency judgments in Rao and Walsh (2009). Full controls refers to fixed effects for index type, documentation, the loans purpose and occupancy, the existence of prepayment penalties and private mortgage insurance, and single family homes. I also include indicators for each 20-point bin of borrowers' FICO credit scores, loan originator and servicer fixed effects, and controls for second liens. I allow individual state time trends. Standard errors are clustered at the MSA level. * denotes 10% significance, ** denotes 5% significance, *** denotes 1% significance.

TABLE 1.6
JOINT ESTIMATES OF THE IMPACT OF ORIGINAL AND CURRENT LEVERAGE ON 1 YEAR DEFAULT PROBABILITIES AND LEVERAGE DEMAND

	Negative Equity			Loan-to-Value		
	24 Months	36 Months	48 Months	24 Months	36 Months	48 Months
Current Negative Equity in $100,000s	0.529*** (0.048)	0.371*** (0.058)	0.232* (0.136)			
Current Loan-To-Value				1.811*** (0.194)	1.046*** (0.199)	0.420 (0.358)
ρ: Correlation of Errors in Default and Leverage Choice	0.036** (0.017)	0.048** (0.020)	0.050 (0.044)	0.067*** (0.016)	0.071*** (0.019)	0.078** (0.037)
Default Threshold	0.906	1.707	3.822	1.338	1.681	3.144
S.D. of Default Error	1.890	2.697	4.313	0.552	0.956	2.378
N	263177	162103	106921	263177	162103	106921
Origination Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Full Controls	Yes	Yes	Yes	Yes	Yes	Yes

Estimates from joint model of leverage demand and default choice. Table shows coefficient on current equity at 24, 36 and 48 months, where current equity is defined as either the level of negative equity in $100,000s (or current loan-to-value). ρ displays the estimated correlation between the errors in the leverage and default equations, capturing adverse selection. Also shown are the default threshold for a borrower at the mean covariate level, and the standard deviation of the error in the default choice in units of current equity. Full controls refers to fixed effects for index type, documentation, the loans purpose and occupancy, the existence of prepayment penalties and private mortgage insurance, and single family homes. I also include indicators for each 20-point bin of borrowers' FICO credit scores, loan originator and servicer fixed effects, and controls for second liens. I allow individual state time trends. Standard errors are clustered at the MSA level. * denotes 10% significance, ** denotes 5% significance, *** denotes 1% significance.

TABLE 1.7

SIMULATION RESULTS: THE IMPACT OF A REDUCTION IN THE LTV CAP FROM 100 TO 90

	Col. 1: LTV Cap of 100	Col. 2: LTV Cap of 90 (No Supply Response)	Col. 3: LTV Cap of 90 (With Supply Response)
Average Loan Amount	\$270,055	\$246,265	\$246,002
Average Interest Rate	8.6%	7.5%	7.9%
Average Balance	\$293,359	\$264,863	\$265,476
Defaults	12.0%	9.1%	9.2%
Naive Defaults	-	7.8%	-
Welfare Loss (CV Rel. to Col. 1)	-	\$8,135	\$8,752
		Parameters	
	Initial Price: $H_0 = \$300000$	Final Value: $H \sim N(500000, 100000)$	Proportion Behavioral: 1%
	CARA Coefficient: a=0.000002	Viscosity: $\gamma = \frac{10}{504}$	$\beta = \frac{1}{1+r_f} = 0.95$
	Prop. Recovered in Default: $\delta = 0.9$	$C_i \sim N(90,000, 190,000)$	Borrowers: $N = 1000$

Simulations from structural model described in Section 7. CARA utility assumed, 1000 simulated borrowers, with 1% behavioral. Viscosity set to match estimated $\rho = 0.067$. The first column shows equilibrium outcomes with an LTV cap of 100. The second column shows borrower responses to the removal of all contracts with initial LTV between 90 and 100, holding fixed all contracts with initial LTV less than 90. Naive defaults refer to expected defaults calculated ignoring borrower heterogeneity and extrapolating from default probabilities at each LTV with an LTV cap of 100. The third column shows equilibrium outcomes with an LTV cap of 90. CV calculated based on expected utility prior to realization of EV1 error.

Chapter 2

Interlinked Firms and the Consequences of Piecemeal Regulation

2.1 Introduction

Firms that generate externalities do not exist in isolation; they interact with other firms through vertical and horizontal interlinkages in the economy. Those other firms may themselves generate externalities, possibly in a different domain. For example, loggers cut down forests and threaten biodiversity while the paper mills they supply pollute the local environment; oil and gas companies emit greenhouse gases while lax safety at the operators they employ put marine life at risk. Yet in practice, regulations are typically designed from a partial equilibrium perspective, with a particular set of firms in mind. If the targeted firms' response affects the extent of externalities generated elsewhere in the economy, such "piecemeal" regulatory design may help account for the frequent and often dramatic regulatory failures we observe (Lipsey and Lancaster, 1956), especially in countries with limited regulatory capacity (Laffont, 2005).[1]

The suboptimality of piecemeal regulatory design was shown theoretically in the 1950s (Lipsey and Lancaster, 1956), but empirical evidence on the welfare costs is lacking.[2] This paper provides a clean empirical demonstration of costly piecemeal regulatory design in an interlinked economy and the potential magnitude of the costs. We do so in the context of one of Latin America's biggest industries – fishmeal production in Peru[3] – which features two textbook externalities: overextraction by upstream suppliers (fishing boats) and air pollution from downstream manufacturers (fishmeal plants). We study the 2009 introduction of individual property rights over fish, an "optimal" policy for preventing overextraction,[4].

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[1]Regulatory failures are common in modern, interlinked economies: recent high profile examples include the 2014 and 2013 Indonesia forest fires (see e.g. The Guardian, 2014), the 2010 Deepwater Horizon oil spill (see e.g. BOEMRE/U.S. Coast Guard Joint Investigation Team, 2011), and the 2006 Ivory Coast toxic waste dump (BBC News, 2010).

[2]As put by Bento et al. (2014), "In the presence of unpriced externalities or other pre-existing distortions, policies levied to correct an externality can exacerbate or alleviate these other distortions in related markets. A priori, theory cannot shed light on the relative importance of the primary welfare effect of the policy – defined by the welfare gain from correcting the externality addressed by the policy – and the interaction effects – defined as the welfare effect that results from the interaction of the new policy with other unpriced externalities." (Bento et al., 2014, p. 2). We cannot do justice to the theoretical literature on regulatory design in the presence of multiple externalities here – see e.g. Bennear and Stavins (2007) and references therein.

[3]Fishmeal is a brown powder made by burning or steaming fish, and often used as animal feed. Peru's fishmeal industry accounts for around 3 percent of the country's GDP (De La Puente et al., 2011) and is the biggest industrial fishing sector in the world (Paredes and Gutierrez, 2008).

[4]See e.g. Boyce (2004, p.1): "In fishery management, an optimal instrument, individual transferable quotas (ITQs), exists".

We show that the introduction of individual property rights upstream, while successful in stemming overextraction, dramatically increased the health impact of air pollution from downstream plants. In documenting the mechanism through which downstream spillover effects arose, we also provide a new finding that underscores the surprising consequences of the *time profile* of production in the presence of externalities: health deteriorated when a given amount of air polluting production downstream was dispersed over time. This, in turn, occurred because upstream boats responded to the reform by spreading their supply out in time. These contributions both have important implications for the regulation of firms and sectors that operate as part of a larger network.

To explore the costs of piecemeal regulatory design, the paper proceeds as follows. We: (a) estimate the health effect of downstream production that was ignored by the architects of the 2009 "individual, tradeable quota" (ITQ) upstream reform using a difference in differences approach comparing near-fishmeal-plants (hereafter "Near plant") locations and further-away (hereafter "control") locations during and outside of the production seasons. (b) Estimate the health impact of the 2009 reform using a difference in difference approach comparing Near plant and control locations pre- and post-reform. (c) Present a conceptual framework of, and document, the upstream and downstream industrial response to the ITQ reform, which serves as a starting point for (d) investigating the mechanism behind exacerbation of downstream externalities post-reform. To do so we view the reform as an exogenous shift in a particular dimension of fishmeal production: it led fishing boats and therefore most manufacturing plants to spread out production across time post-reform. We also exploit the framework's predictions on the local industry characteristics that should predict variation in how individuals' exposure to production changed post-reform and use triple difference strategies to investigate. Finally, (e) we argue that the health impact of fishmeal plants, and the exacerbation of these externalities when production was spread out across time, is due to air pollution generated in the production process, and investigate this hypothesis using a subset of the sample for which air pollution data is available.

The 2009 ITQ reform in Peru is an ideal setting for investigating the consequences of piecemeal regulatory design for several reasons. First, while a handful of influential existing empirical papers explore unforeseen effects of regulations imposed on a given set of firms (e.g. due to plant substitution between different pollutants or effects on market power),[5] we focus instead on a sequential production chain with two sets of firms that generate distinct externalities and a clear link between upstream and downstream firms. (Plants process the fish immediately after it has been offloaded as fishmeal is more valuable when made from fresh fish). This allows a clean separation between the targets of the regulation and the identified unexpected consequences, while highlighting the extent to which input-output linkages in the economy can propagate the impact of "regulatory shocks" into spheres of the economy in which the consequences may be detrimental. In this sense we follow the literature that examines how shocks transmit through the network of an economy.[6]

Second, individual property rights is the most commonly recommended regulatory system for natural resource sectors, including oil and gas, forestry, fisheries and mining (Ostrom, Janssen and Anderies, 2007). Since natural resources are an example of intermediate goods that are typically processed by downstream final good producers, their regulation may affect the impact on welfare of downstream externalities. The findings in this paper show that regulators face a trade-off: individual property rights help to eliminate the "race" for the resource, but tend to spread downstream production – and hence the associated externalities – out over time, which matters for welfare. There are many other examples of common regulatory systems that will tend to spread downstream production out over time.[7] Focusing on a particular downstream industry allows us a precise understanding of its vertical interlinkages, but fishmeal production shares many characteristics – and externalities – with other manufacturing industries.

Third, while recent studies have begun to emphasize the ubiquity and greater challenges of

[5]Sigman (1996); Greenstone (2003); Gibson (2015) explore plant substitution between regulated and unregulated pollutants. Becker and Henderson (2000) find that, in the U.S., environmental regulations favoring small firms led to a shift in industry structure towards single-plant firms, which in turns contributed to environmental degradation. Ryan (2012) and Fowlie, Reguant and Ryan (2014) find that allocative inefficiencies due to changes in market power in the U.S. cement market counteract the social benefits of carbon abatement regulations. Note that because our focus is on interactions between externalities that arise through firms' interlinkages, we do not go into the literature on individuals substituting across regulated versus unregulated appliances and transport modes here.

[6]See e.g. Long and Plosser (1983); Horvath (1998); Jones (2011); Foerster, Sarte and Watson (2011); Acemoglu et al. (2012); Barrot and Sauvagnat (2015); Pomeranz (forthcoming).

[7]For example, Cap and Trade (CAT), some forms of entry barriers, and possibly temporary bans on production due to maximum pollution concentration restrictions. This paper's evidence on unintended consequences of Coasian regulations due to their impact on the distribution of production across time complements the evidence in Fowlie (2010)'s influential study on unintended consequences of CAT programs due to their impact on the geographical distribution of production.

regulating industrial externalities in developing countries, the existing literature has largely focused on rich countries.[8] The task differs for regulators in developing countries in part due to the range and magnitude of interacting externalities they face (Greenstone and Jack, 2015). The reasons why regulatory design typically happens piecemeal – for example, non-coordination between regulating agencies or sequential political regimes with distinct objectives, unobservability of some interlinkages or externalities, and the complexity of optimizing regulations "in equilibrium" – also apply to a greater extent to the developing world. While piecemeal regulation likely leads to significant welfare losses in all countries, we thus focus on the type of context in which the possibility of such losses and the challenges in addressing the problem are of greatest concern.

Fourth, the Peruvian setting allows us to exploit sharp variation in downstream production due to government-imposed, irregularly timed, semi-annual production ban periods,[9] and in the introduction of ITQs upstream. Among developing countries, Peru also has exceptional data coverage. We link uniquely detailed hospital admissions records, repeated cross sections of household health and labor market surveys, administrative data on all production of fishmeal at the day$\times$plant level, and ground-station measurements of air pollutant (PM^{10}, $PM^{2.5}$, NO_2 and SO_2) concentrations.

We begin by documenting the downstream health externality that was ignored by the ITQ reform's architects (The Ecologist, 2008). Difference in difference estimates comparing Near plant and control locations during production and ban periods show that plant production in the last 30 or 90 days increases respiratory (and total) hospital admissions, reported health issues and medical expenditures among adults, and reported health issues and coughs among children. The estimated health effects survive extensive robustness checks, and are not driven by changes in incomes or labor markets during production periods.

[8]See, among others, Hanna and Oliva (2014); Ebenstein (2012); Chen et al. (2013); Rau, Reyes and Urzua (2013); von der Goltz and Barnwal (2014); Greenstone and Hanna (2014) on the often extremely high pollutant concentrations in developing countries. Several innovative recent papers also illustrate the need to take regulatory capacity and the prevailing incentive structures into account when designing regulation (Laffont, 2005; Estache and Wren-Lewis, 2009; Burgess et al., 2012; Duflo et al., 2013, 2014; Jia, 2014; Greenstone and Jack, 2015). The primary focus in the literature on how to design regulation of industrial externalities has been on rich countries and comparing (i) the magnitude of decreases in the targeted type of externalities (e.g. pollution or overextraction of a resource – see Costello, Gaines and Lynham (2008) for convincing evidence in the case of ITQs for open access resources) to (ii) the economic costs of compliance (see e.g. Gray and Shadbegian, 1993; Greenstone, 2002; List et al., 2003; Greenstone, List and Syverson, 2012; Natividad, 2014).

[9]Boats were not allowed to fish during periods when the fish reproduce.

To identify how firms' response to the 2009 regulatory reform affected the downstream plants' impact on health, we compare Near plant and control locations before and after the reform came into effect. We find that the plants' production was dramatically more harmful to adult and child health post-reform, for example causing 55,000 additional respiratory hospital admissions per year. The estimated reform effects survive extensive robustness checks,[10] are not driven by changes in incomes or labor markets or confined to those who work in the sector, and are consistent in magnitude with the estimated health effects of plant production.

To begin investigating the mechanism underlying the downstream health impact of the 2009 reform, we first present a two-sector conceptual framework. The framework predicts that exposure to fishmeal manufacturing will spread out in time when individual property rights over fish are introduced upstream, as boats' incentive to rapidly capture as much as possible of the "total allowable catch" (TAC) is removed, less efficient plants decrease production or exit the industry, and more efficient plants spread their production across time. These predictions find support in the data. While there was a small decrease in the total *amount* of fishmeal produced post-reform, the average individual in our sample was exposed to 53 percent more *days* of production per year post reform.[11]

Following the conceptual framework and the observed industrial response to the introduction of individual property rights over fish, we hypothesize that plants' impact on health worsened primarily due to the change in the time profile of production. To test this hypothesis, we first instrument for days of production with the reform, and find that days produced is indeed an important determinant of the extent to which a given amount of production harms individuals' health. Geographical heterogeneity in the estimated reform effects further supports the time profile hypothesis. Where the extension across time of production was more extreme – the north (97 percent increase) and locations with efficient plants (134 percent increase) – the exacerbation of the industry's impact on health post-reform was significantly worse. But where fishmeal produc-

[10]We show direct evidence supporting the identifying assumption of no differential trends in fishmeal locations, and that the estimates are robust to including location-specific trend terms and to varying the time window compared before/after the reform. We also show that the estimated health and reform effects are not driven by pollution from the fishing boats.

[11]Boats in the North/Central region spread out fishing in time as the ITQ reform came into effect. (Boats in the previously unregulated southern region fished for fewer days of the year after the reform due to the introduction of ban periods there in conjunction with the reform.) Fishmeal production days increased in the North/Central region and in locations with efficient plants. Production days decreased in the South and in locations with inefficient plants.

tion days decreased with the reform – e.g. the smaller southern region (46 percent decrease) – the estimates of the effect on health are insignificant or significantly *positive* (favorable).

Convincing empirical evidence on the potential for, and possible magnitude of, a worsening of externalities elsewhere in the economy due to the introduction of piecemeal regulation is the primary objective of this paper. But the mechanism driving such adverse effects in the case analyzed is important for the specific but common scenario of natural resource suppliers supplying downstream manufacturers, and potentially any regulation that impacts the time profile of pollution. Why is plant production harmful to individuals' health, and why is the impact greater when production is spread out over time? We address this question in the final part of the paper using data on the sub-sample of individuals and hospitals in the Lima region, where ground-station measurements of air pollutants are available. We show that plants' impact on health, and the increased impact post-reform, is due to air pollution emitted in the production process. Our results suggest that increases in the duration of exposure to pollution are harmful to health, even when accompanied by proportional decreases in the intensity of exposure. Cost/benefit calculations that are suggestive but conservative indicate that the monetized cost of the reform's impact on health is of the same order of magnitude as the increase in sector profits due to the decrease in overextraction.

While the harmful effects of air pollution on adult and child (especially respiratory and pulmonary) health outcomes are convincingly documented in the existing literature,[12] this finding to our knowledge represents the first causal evidence on the health consequences of *simultaneous* changes in duration and intensity of exposure to air pollution (see e.g. Pope III et al., 2011) – a trade-off faced by policymakers whenever regulations that affect the time profile of production can be used. The finding is consistent with extensive existing evidence from economics and, especially, epidemiology on respectively (a) concavity in dose response at the levels of pollution seen in developing countries (Chay and Greenstone, 2003; Krewski et al., 2009; Crouse et al., 2012; Clay, Lewis and Severnini, 2015; Hanlon, 2015; Pope III et al., 2015), and (b) the importance of concur-

[12]See e.g Brook RD et al. (2010); Moretti and Neidell (2011); Schlenker and Walker (forthcoming); Chen et al. (2013); Currie et al. (2014) on adult health and Chay and Greenstone (2003); Case, Fertig and Paxson (2005); Chay and Greenstone (2005); World Health Organization (2006); Jayachandran (2006); Currie and Almond (2011); Currie and Walker (2011); Gutierrez (2013); Roy et al. (2012); Currie et al. (2014, 2015); Isen, Rossin-Slater and Walker (forthcoming) on child health.

rent exposure and the duration of exposure (Pope III et al., 2011; Beverland et al., 2012; Chen et al., 2013; Anderson, 2015; Barron and Torero, 2015).[13] Exploring the generality of our findings on the shape of the health production function is an important direction for future research.

We conclude (a) that the cost of the exacerbation of "interlinked externalities" elsewhere in the economy that are ignored when (otherwise successful) regulatory reforms are designed can be of first order magnitude; and (b) that the health impact of air polluting plant production appears to be worse if spread out in time, which may alter the cost-benefit calculus for individual property rights and other regulatory regimes that affect the time profile of production in interlinked polluting industries downstream.

The paper is organized as follows. In Section 2.2 we discuss background on the setting, why fishmeal production may affect health, and the 2009 ITQ reform. In Section 2.3 we present the data. In Section 2.4 we explain our empirical strategy and provide evidence on plants' impact on health and how the ITQ reform affected this externality. Section 2.5 analyzes, theoretically and empirically, the industry's responses to the 2009 ITQ reform, and Section 2.6 tests the time profile hypothesis. In Section 2.7 we investigate *why* the time profile of production might matter for health. Section 2.8 discusses the total costs and benefits of the reform and regulatory design, and Section 2.9 concludes.

2.2 Background

2.2.1 Two interlinked sectors, downstream production, and health

The industrial fishing boats supplying Peru's fishmeal plants account for around 10 percent of global fish capture (Paredes and Gutierrez, 2008). The fishmeal plants, all located at ports, are

[13]Note that we use the term "health production function" to mean the three-dimensional relationship relating health at a given point in time to both the duration of exposure to air pollution and the intensity of exposure, though the existing literature typically analyzes the two underlying relationships (duration and dose response) separately. Pope III et al. (2015) summarize the epidemiological evidence on dose (concentration) response: "recent research suggests that the C-R [concentration response] function [between $PM^{2.5}$ and health risk] is likely to be supralinear (concave) for wide ranges" (Pope III et al., 2015, p. 516). The fishmeal locations in our sample are well into the higher ranges of PM concentration for which Pope III et al. (2015) argue that concavity in concentration response is increasingly uncontroversial (though many epidemiologists argue that concentration response may be concave also at lower concentrations (Crouse et al., 2012; Krewski et al., 2009)). Pope III et al. (2011) summarize the epidemiological evidence on duration response for cardiovascular mortality risk of air pollution and conclude that "the evidence suggests that...longer duration exposure has larger, more persistent cumulative effects than short-term exposure, but the highest marginal effects occur with relatively short-term exposures most proximal in time" (Pope III et al., 2011, p. 1).

present in around 22 towns with a suitable port and produce about a third of the global supply of fishmeal. Both the industrial fishing sector and the fishmeal sector are very capital intensive. Paredes and Gutierrez (2008) estimate that there were only about 26,500 jobs in the two sectors as a whole in 2008: 1,194 active industrial fishing boats employed around 17 workers each on average, and 110 fishmeal plants employed around 60 workers each on average (see Christensen et al., 2014; Paredes and Gutierrez, 2008). Five percent of our adult sample in fishmeal locations reports to work in the "fishing" sector. Because jobs in industrial fishing and fishmeal production are quite stable – many fishmeal firms keep the (relatively high-skill) plant workers on payroll outside of the production season[14] – there is little seasonal work migration, as discussed in more detail in Sub-section 2.4.2

Fishmeal is more valuable when made from fresh fish. Most fishing boats therefore go out for at most one day at a time, and plants process the fish immediately after it has been offloaded, leading to a direct link between plant production and supply of fish. The fish is transported from the boat into the plant through a conveyor belt. After cleaning, the fish is dried and converted into fishmeal by either exposure to direct heat or steaming. Fishmeal is storable for 6 – 12 months (but fishmeal companies report that they rarely store for long).

Air pollution may occur in the form of chemical pollutants (such as carbon dioxide (CO_2) and nitrogen dioxide (NO_2)) from the plants' heavy use of fossil fuels, in the form of noxious gases (e.g. sulfur dioxide (SO_2) and hydrogen sulfide (H_2S)) released as fish decompose, and in the form of microscopic natural particles (PM^{10} or $PM^{2.5}$) released during the drying and burning processes. Case studies have found high levels of air pollution near fishmeal plants during production periods, as discussed in detail in the appendix.[15] As also discussed at greater length in the appendix, air pollution in the form of particulate matter, chemical pollutants and gases associated with fishmeal production has been shown to cause a range of health problems in adults and children, especially respiratory disease episodes.

[14] In a country-wide survey of workers in the sector conducted by the consulting firm APOYO in May 2007, 87 percent report having worked for the same company or fishing boat owner throughout their career, on average for about 14 years (APOYO, 2008). 40 percent report not working at all outside of the production seasons; a large proportion of the remainder work as artisan fishermen intermittently.

[15] In developed countries, filters and scrubbers are usually required by law and reduce emissions from manufacturing plants; in Peru, the regulatory authorities have unsuccessfully attempted to force the powerful fishmeal industry to install such technologies (De La Puente et al., 2011).

2.2.2 Regulations and the 2009 upstream reform

The regulations imposed on the Peruvian industrial fishing/fishmeal industry are aimed at preserving fish stocks while maintaining industry profitability. In the North/Central marine ecosystem (down to the -16 °S parallel), irregularly timed, semi-annual fishing/production bans were in place during the Peruvian anchoveta's reproductive periods throughout the years covered in our data. In addition, before the 2009 reform, industrial boats in the North/Central region operated under a sector-wide "Total Allowable Catch" (TAC) set at the beginning of each season. In the smaller southern marine ecosystem, fishing was allowed throughout the year and no aggregate quota was in place before the 2009 ITQ reform.[16]

In 2008, officials estimated excess capacity in the combined sector (the industrial fleet and fishmeal plants) of 35–45 percent and declining fish stocks (Tveteras et al., 2011). The goverment announced a new law introducing a system of individual, transferable quotas (ITQs) for industrial fishing boats on June 30th, 2008. An extensive media search reveals no mention of the downstream plants' impact on health in the deliberations leading up to the law, though clear indications of such externalities had received considerable attention in the Peruvian and foreign media for years and must have been known to Peruvian regulators.[17] The ITQ law came into effect in the North/Central region on April 20th, 2009 and in the South on July 7th, 2009. In the South, the new ITQ system also meant that a quota and fishing ban periods were introduced for the first time.

Individual boat quotas were specified as a share of the regions's aggregate quota for the relevant season. The quota-share was based on historical catches and a boat's hull capacity. Within regions, the quotas could be transferred between boats, subject to certain rules.

[16]This was due to fears that Chilean fishing activity would offset any environmental or industrial benefits of regulation.

[17]Travelers passing by fishmeal locations during production season can easily see and smell the severity of air pollution, an observation that helped motivate this paper. In a 2008 article, *The Ecologist* magazine reported that "When we visited one heavily afflicted community [in the fishmeal town of Chimbote], more than a dozen women and children gathered [...] to vent their anger at the fishmeal plants. They claim the plants that loom over their houses are responsible for asthma, bronchial and skin problems, particularly in children. 'We know the factories are responsible for these [problems], because when they operate the illnesses get worse', says one young woman [...] Another says when the plants are operating the pollution is so thick you cannot physically remain on the street. Footage [...] seen by *The Ecologist* illustrates typical conditions when fishmeal plants are operational: billowing black smoke drifts through the streets, obscuring vision and choking passers-by [...] Pupils at a Chimbote school [...] also complain of health problems. 'It causes fungal growths, breathlessness, we cannot breathe', says one boy." Such complaints were supported by case studies (e.g. Cerda and Aliaga, 1999), and local doctors (The Ecologist, 2008).

2.3 Data

We combine five different types of data: hospital admissions records, individual- and household-level survey data, administrative regulatory data, administrative production and transaction registries, and data on pollution.

Hospital admissions records. Information on hospital admissions was provided by the Peruvian Ministry of Health and consists of counts of all patients admitted to any public health facility between 2007 and 2011. The data is at the facility$\times$month level and gives information on the cause for admission (using the International Classification of Diseases (ICD) system).

Individual- and household-level survey data. The nationally representative Encuesta Nacional de Hogares (ENAHO) is the Peruvian version of the Living Standards Measurement Study (LSMS). Since 2004 surveying has taken place throughout the year, and the order in which sampling clusters are surveyed is randomly determined. A subset of clusters are re-surveyed every year and information on the "centro poblado" where each respondent is interviewed is recorded.[18] In our analysis, we use the GPS coordinates of the centro poblado's centroid. The survey focuses on labor market participation, income and expenditures, self-reported health outcomes, etc., as in other LSMSs.

We also use the nationally representative Encuesta Demografica y de Salud Familiar (ENDES), which is the Peruvian version of a Demographic and Health Survey (DHS). The sampling framework is similar to ENAHO. A subset of clusters are re-surveyed every year.[19] GPS coordinates for sample clusters are recorded. Women between 15 and 49 years old are interviewed, and information on the women themselves and their children (five years old and under) recorded. The survey is comparable to other DHS surveys, focusing on self-reported and measured health outcomes. For both surveys, we use the years 2007–2011.

[18]Centros poblados are villages in rural areas and neighborhoods in urban areas. After the sample restrictions we impose, 2096 sampling clusters with on average 77 households each are present in our sample. 710 centros poblados are present, with on average 228 households each.

[19]From 2004 to 2007, a fixed set of clusters were used, the survey order of which was randomized (as was the trimester of surveying). The definition of clusters changed somewhat in 2008 when Peru's statistical bureau updated the sampling frame with the 2007 national census. Furthermore, 2008 was unusual in that fewer clusters were surveyed. From 2009 to 2011, the number of survey clusters was the same as in 2004-2007, and about half were part of a panel of clusters surveyed every year.

Administrative regulatory data. We coded the dates of all fishing seasons from 2007 to 2011 and the size of each season's aggregate quota from the government gazette *El Peruano*.

Administrative production and transaction registries. The registry of all transactions between industrial fishing boats and fishmeal plants from 2007 to 2011 was provided by the Peruvian Ministry of Production.[20] All offloads by industrial boats are included, i.e., all (legal) input into fishmeal production. Information on the date of the transaction, and the boat, plant and amount of fish involved (though not the price), is included.

We also have access to the ministry's records of fishmeal plants' production/output, recorded at the monthly level, from 2007 to 2011.

Pollution data. Unlike for most developing countries, daily ground-station measurements of air pollutants are available for a significant period of time for Peru, though only for the area around the capital city. Information on the daily concentration, from 2007 to 2010, of four air pollutants at each of five stations in the Lima region was provided by the environmental division (DIGESA) of the Ministry of Health. The measured air pollutants – PM^{10}, $PM^{2.5}$, NO_2 and SO_2 – have been shown to correlate with factory production in many contexts and are commonly used in the health literature.

We construct five primary outcome variables, with a particular focus on the health issues that are most likely to be affected by short-term variation in air pollution from plant production (see e.g. Chen et al., 2013) – respiratory issues. The outcome "Respiratory Admissions" is a count at the hospital level of all admissions due to diseases of the respiratory system (ICD codes J00-J99). As no explicit question on respiratory issues is asked in the ENAHO survey, for adults we construct an outcome labeled "Any Health Issue" as the complement to "No health issue in the last month". We also use expenditure data to construct an estimate of the individual's total medical expenditures. For children, we use ENDES survey data to construct a measure of "Any Health Issue",[21] and also separately report the outcome of the child experiencing a cough. The survey

[20] This includes "within-firm" transactions. Some boats are owned by the firms that own the plants.

[21] This variable is equal to one if the surveyed parent reported that the child had experienced any of the health issues the survey covers in the last two weeks. The covered health issues are cough, fever, and diarrhea. These have all been linked to air pollution in the existing epidemiological literature (see e.g. Peters et al., 1997; Kaplan et al., 2010), although the evidence linking air pollution and cough is more extensive.

based outcomes capture adverse health episodes of a wider range of severity than those leading to hospital admission.

2.4 Fishmeal Manufacturing's Impact on Health

2.4.1 Empirical strategy

The primary goal of this paper is to identify the impact of the introduction of a new regulatory system upstream—individual property rights—on the externalities generated by downstream plants, and the mechanism through which such spillover effects occur. There are three parts to our analysis. First we estimate how exposure to fishmeal production affects health. At this stage we are flexible in our specification of the extent of production activity: we show results from using both the amount produced and days of production within a given time window. We then go on to estimate the impact of the regulatory reform on health outcomes for those exposed to fishmeal production. We briefly lay out the approach we take in each of these steps here.

We consider the health outcomes y_{ijt} of an individual or hospital i in location j at time t. To estimate how exposure to fishmeal production affects health, we compare y_{ijt} for those located within a given radius of fishmeal plants,[22] $NearPlant_j = 1$, to those located further away, at times of varying production intensity in the cluster of plants closest to the individual or hospital in question $Production_{jt}$:

$$y_{ijt} = \alpha + \beta_1 Production_{jt} + \beta_2 NearPlant_j \times Production_{jt} + \beta_3 X_{ijt} + \gamma_{c(j)} + \delta_{m(t)} + \varepsilon_{ijt} \tag{2.1}$$

$$y_{ijt} = \alpha + \beta_1 Production_{jt} + \beta_2 NearPlant_j \times Production_{jt} + \beta_3 X_{jt} + \psi_i + \delta_t + \varepsilon_{ijt} \tag{2.2}$$

where t indicates a specific date for the individual level outcomes in (2.1) and a year×month

[22] As we do not have GPS points for surveyed individuals' homes, nor shape files for the sampling clusters and centros poblados, we define the location of i as the centroid of j (the centro poblado (in ENAHO) or sampling cluster (in ENDES)) to which he/she belongs.

for the hospital level outcomes in (2.2). X are covariates that include $NearPlant_j \times \theta_{n(t)}$, where $\theta_{n(t)}$ is a month fixed effect to control for possibly differential seasonality in $NearPlant_j$ locations. In (2.1) X also includes individual-level covariates.[23] $\gamma_{c(j)}$ is a centro poblado or district fixed effect, ψ_i a hospital fixed effect, and $\delta_{m(t)}/\delta_t$ a year×month fixed effect. We thus compare individuals/hospitals who are within the same town or district, but close to versus less close to fishmeal plants, during periods when production is higher versus lower. β_2 measures the marginal effect of additional production exposure. Standard errors are clustered at the centro poblado or district level.[24]

To explore how the 2009 ITQ reform affected health outcomes for those exposed to fishmeal production in reduced form, we compare individuals and hospitals in Near plant and control locations before and after the reform as follows:

$$y_{ijt} = \alpha + \beta_2 NearPlant_j * Reform_{jt} + \beta_3 X_{ijt} + \gamma_{c(j)} + \delta_{m(t)} + \varepsilon_{ijt} \tag{2.3}$$

$$y_{ijt} = \alpha + \beta_2 NearPlant_j * Reform_{jt} + \beta_3 X_{jt} + \psi_i + \delta_t + \varepsilon_{ijt} \tag{2.4}$$

$Reform_{jt}$ is a dummy variable equal to one after the reform took effect in the fishmeal port (cluster of plants) nearest to location j. In some specifications we additionally include centro poblade/district time trends or allow a $NearPlant$ specific trend.

How is the difference in difference specification on the reform in (2.3) and (2.4) related to that on exposure to production in (2.1) and (2.2)? As we show below, the reform led to a stark change in the fishmeal sector: fish capture and plant production were spread out in time. Suppose that

[23]The individual covariates are gender, age, mother tongue, years of education, and migration status for adults, and gender, age, mother's years of education, and the ENDES household asset index for children. These control for possible changes in the sample surveyed across time/space.

[24]While we use centro poblado fixed effects in regressions using ENAHO data, the lowest geographical unit we can condition on when using ENDES data is districts. The reason is that the ENDES sampling framework changed in 2008/2009. While district information is included in all rounds of ENDES, the data key necessary to link specific sampling clusters/centros poblados before and after 2008/2009 was not stored. Note that Peruvian districts are small; there are 1838 districts in the country.

this spread was the only or main mechanism through which the reform changed the fishmeal sector's impact on health. If so, (2.3) and (2.4) can be thought of as the reduced form corresponding to a hypothesis on the "structural" relationship between fishmeal plants' production and health. In particular, the hypothesis that the impact of production on health depends on the time profile of production—the number of days of exposure to production—holding constant the level of production. After we present the evidence from running (2.1)-(2.4), we test this hypothesis.

For outcomes drawn from surveys, in which we have precise village/cluster GPS data, we use five kilometers as the baseline "treatment" (Near plant) radius within which any health effects of fishmeal production are hypothesized to be greatest, based on the literature on air pollution (see e.g. Currie et al., 2015; Schlenker and Walker, forthcoming). For hospital admissions outcomes, we use 20 kilometers as the baseline treatment radius so as to include the facilities used by those living near fishmeal plants in the "treatment group."[25] Note that our specification is conservative in that we compare locations inside the treatment radius to locations outside the radius, allowing the "control locations" to also be affected by production in the nearest port. We simply allow production to have a differential effect in locations close to the fishmeal plants. As a robustness check, we also investigate how our estimates vary with the treatment radius used.

We initially consider two natural measures of fishmeal production: the number of days on which fishmeal production took place and log total input into fishmeal production reported in 10,000s of metric tons, in the previous X days in the port (i.e., cluster of plants) nearest to the individual or hospital (we use input rather than output to measure fishmeal production because we have data on input at the daily level and output only at the monthly level. As seen in Figure B.1, the output of fishmeal very closely tracks the input of fish). Our baseline lookback window—30 days—matches the way the ENAHO survey questions are asked. To capture health responses to more persistent exposure to production, we also show results for a 90 day window—approximately the longest period of continuous exposure observed in our data period—and also investigate how our estimates depend on the exact lookback window used. It is important to note that β_2 in (2.1) and (2.2) captures the health response to exposure to fishmeal production in the recent past – the marginal effect of an additional day or amount of production in the last 30

[25] The geographical spread of health facilities is much greater than that of sampling clusters. In many fishmeal locations, the nearest hospital is more than 10 kilometers away.

or 90 days. There may additionally be health consequences of long-term exposure to fishmeal production that we do not capture.

Figure 2.1 is a map of Peru illustrating our identification strategy by showing five kilometer radii around fishmeal ports alongside ENAHO and ENDES sampling clusters. The assumption necessary for (2.1) and (2.2) to identify the impact of exposure to fishmeal production on health is that trends in health outcomes across periods with more versus less fishmeal production in the nearest cluster of plants would have been similar in Near plant and control locations in the absence of fishmeal production. In Table 2.1 we display the means and standard deviations of both health outcomes and covariates in Near plant and control locations during and outside of production periods. When the plants are not operating, respiratory hospital admissions and medical expenditures are higher in Near plant locations, whereas child health issues occur more frequently in control locations. Most household demographic characteristics are similar in Near plant and control locations, but education levels and assets are somewhat higher and the proportion of adults speaking an indigenous language is somewhat lower in Near plant locations. We include these variables as controls in all of our regressions. The numbers also indicate that there is little seasonal work migration to the fishmeal locations, probably because jobs in the industrial fishing sector are quite stable, as discussed above.

Similarly, the identifying assumption necessary for (2.3) and (2.4) to estimate the causal effect of the ITQ reform on health is that trends in health outcomes across the date when the reform took effect would have been similar in Near plant and control locations in the absence of the reform. Table 2.2 is identical to Table 2.1, except that we now compare Near plant and control locations before and after the ITQ reform. Unsurprisingly, the differences in health outcomes and covariates between Near plant and control locations before the reform are similar to those for the non-production periods discussed in the previous paragraph.

Location fixed effects control for time invariant differences between Near plant and control locations, including the average level of air pollution. We include all covariates shown in Tables 2.1 and 2.2 for adults and children as controls when estimating (2.1), (2.2), (2.3), and (2.4) for adults and children respectively. In sub-sections 2.4.2 and 2.4.3 we investigate possible violations of the identifying assumptions in depth.

2.4.2 Results on fishmeal manufacturing and health

In addition to summary statistics, Table 2.1 shows the "raw" difference in differences, i.e., without any fixed effects or controls included, in health outcomes between Near plant and control locations during and outside of production periods. These are positive—indicating that health is relatively worse in Near plant locations during fishmeal production—and sizeable for all five categories of adverse health outcomes, and significant for respiratory hospital admissions and adult health issues.

Table 2.3 shows the effect of fishmeal production on adult and child health from estimating (2.1) and (2.2). We find that fishmeal production during the previous 30 or 90 days, whether measured as production days or total input into production, negatively affects adult and child health. A 50 percent increase in fishmeal production during the previous month leads to 1.6 (1 percent) more hospital admissions for respiratory diseases; a 0.77 percentage point (1.3 percent) higher incidence of "Any Health Issue" among adults; and a 3.8 percent increase in medical expenditures.[26] For these outcomes the estimated effects are similar when using a 90 day window. We also find that a 50 percent increase in fishmeal production during the last 90 days leads to a 1.7 percentage point (3.7 percent) increase in the incidence of "Any Health Issue" and a 1.6 percentage point (4.2 percent) increase in the incidence of having a cough among children ≤ 5. We do not find significant effects for children of production in a 30 day window. The reason may be that our statistical power to detect effects on child health is lower than for adult health due to much smaller sample sizes.[27] The last two panels of Table 2.3 show the estimated effect of days of production on health. The patterns are similar to those found in the top panels; for example, 10 additional days of production during the last 90 days increases the incidence of "Any Health Issue" by 8.9 percent for children ≤ 5. Overall, the results in Table 2.3 indicate that exposure to fishmeal production leads to worse health outcomes for both adults and children.

In the appendix we show that the results are robust to instrumenting for production and pro-

[26] As we estimate the effects of log production on health outcomes, we compute the effects shown here, the impact of a 50% change in production, as $\beta \times \ln(150/100)$. For medical expenditures, which is in logs, we report $e^{[ln(150/100)\times\beta]}$.

[27] The results indicate a decrease in hospital admissions (and in some specifications also weaker indications of improvement in child health) in non-fishmeal locations during the periods when production takes place. The explanation is most likely that differences in health between regions have changed over time in a way that happens to correlate with the extent of fishmeal production in the region. Such a pattern is not a concern for our estimates as it would lead us to understimate the impact of plant production on health.

duction days using non-ban days (Appendix Table B.1); to specifying hospital admissions in logs (Appendix Table B.2); to varying the treatment radius and look-back window used (Appendix Figure B.2);[28] to restricting the sample to the period prior to the ITQ reform (Appendix Table B.3);[29]and that a falsification exercise shows no significant effects on health outcomes that we would not expect to respond to plant production (Appendix Table B.4). In Table 2.1 we see that average educational attainment, the proportion of immigrants, and the proportion speaking an indigenous language are lower in Near plant locations during the production periods. While these changes are unlikely to explain a deterioration in health outcomes, to be cautious we include all covariates shown in Table 2.1 as controls when estimating (2.1) and (2.2).

In the appendix we show that fishmeal production affects the health of whole communities (not just those who work in the sector, see Appendix Table B.5), and that the effect is not driven by labor market responses (average incomes and labor market outcomes are not significantly different during production periods, see Appendix Table B.6). We also show that the adverse impact on health is not driven by ocean pollution or direct fish consumption (see Appendix Table B.7). We return in Section 2.7 to the hypothesis that fishmeal production affects the health of the local population primarily through air pollution emitted by the manufacturing plants.

2.4.3 Results on the introduction of individual property rights upstream and health

Recall that the 2008 ITQ reform introduced individual property rights over the resource for the fishmeal plants' suppliers—industrial fishing boats—so as to de-incentivize boats racing to capture fish early in the season. In addition to summary statistics, Table 2.2 shows the raw difference in differences in health outcomes between Near plant and control locations before versus after the ITQ reform. These are positive and sizable for all five categories of adverse health outcomes—indicating that health is relatively worse in Near plant locations after the reform—and significant

[28]Note that we can also compare individuals/hospitals in fishmeal locations only to individuals/hospitals in locations that are contiguous to the fishmeal locations; this gives very similar results to those in Table 2.3.

[29]Recall that we in (2.1) and (2.2) estimate the effect on health of a marginal increase in exposure to fishmeal production. As discussed in Sub-section 2.4.1, we are intentionally flexible in how we specify the extent of production activity at this stage: we simply wish to establish if there is an effect of plant production on health or not. (When we analyze if and why the impact on health changed after the reform, we will instead attempt to establish which specific dimensions of production that changed and thereby altered the impact on health). For this reason we find it most natural to include the whole sample period in Table 2.3, which also helps to maintain power. Appendix Table B.3 is provided for the reader who instead would prefer the impact on health to be estimated using only data from the pre-reform period. The estimates are similar to those in Table 2.3, but less precisely estimated.

for respiratory hospital admissions, adult health issues, and medical expenditures.

Table 2.4 presents the results from estimating (2.3) and (2.4). The top panel is our preferred specification, in which we limit the sample to the last year before and first year after the reform. We see respiratory hospital admissions increase by 7.2 percent in Near plant locations, relative to control locations, after the reform. For adults, we see large and significant effects on health, with the likelihood of reporting a health issue increasing by over 10 percent, and medical expenditures by 23.9 percent, after the reform. We see even bigger effects for children, with the incidence of "Any Health Issue" increasing by 40 percent and coughs increasing by 39 percent.[30] We discuss the magnitude of the estimates below.

The other five panels of Table 2.4 show results from robustness checks in which we control for $NearPlant$ specific time trends; control for centro poblado or district specific time trends; expand the sample to include data from the last two years before and first two years after the reform; restrict the sample to include data from only the first fishing season of the year[31] and restrict the sample to include only locations that are relatively near (within 50 kilometers of) fishmeal plants in the control group.[32] The significance and magnitude of the estimated coefficients is very similar to that found in the top panel throughout, with some changes for specific outcomes. Overall the results in the bottom five panels of Table 2.4 provide strong support for the identifying assumptions for our estimation.

Finally, Figure 2.2 shows trends in health outcomes in Near plant and control locations before and after the reform took effect. We see similar trends in the two groups before the reform, again suggesting that the identifying assumption of parallel trends holds. The significant, differential increase in adverse health outcomes in fishmeal locations when the reform takes effect, estimated formally in Table 2.4,[33] is also apparent in Figure 2.2, for all five health outcomes. We conclude that the estimated worsening of the downstream plants' impact on health after the 2009 ITQ reform is robust and likely reflects a causal relationship.

[30]The latter is imprecisely estimated and not significant in the main specification, but is significant in all the other specifications.

[31]We conduct this test to make sure that our results are not driven by the effect of El Niño in late 2009.

[32]The estimated reform effects are also robust to varying the "treatment radius" around ports used to define fishmeal locations.

[33]We do not have enough observations around the cut-off (the date then the reform took effect) to estimate the effect of the reform as in a regression discontinuity approach.

A possible concern is that the seriousness of health issues may have changed after the reform. While we ultimately cannot fully test for this possibility, it is important to keep in mind that (a) respiratory disease episodes have to be fairly serious to lead to a hospital admission (pre- or post-reform), and, perhaps more importantly, (b) the estimates for medical expenditures suggest that the total health costs to individuals increased significantly post-reform.

2.5 Plants' response to the introduction of individual property rights upstream

We now present a theoretical argument for how we should expect the introduction of individual property rights over intermediate goods to affect the spatial and temporal distribution of final good production. The purpose is two-fold. First, the framework informs which particular dimension(s) of fishmeal production we should expect to change *on average across locations* after the ITQ reform and therefore to potentially drive the increased impact on health. Second, the framework will ultimately help us *test* why the fishmeal industry's impact on health increased when individual property rights were introduced upstream. This is because it delivers predictions on which characteristics of the fishmeal industry *in a particular location* should predict a large or small local response. We present the basic framework and predictions of the model here; a full presentation is in the appendix.

The basic intuition of the model is as follows. An industry wide quota regime encourages boats to "race" for fish early in the season. The resulting high per-period fish capture early in the season decreases the price of fish and thereby allows less efficient fishmeal plants to survive. The introduction of individual quotas removes boats' incentive to fish intensely early in the season; now they instead minimize extraction costs, which requires spreading out fishing across time. This in turn increases the price of fish available for fishmeal production, forcing less efficient plants to reduce their production or exit the industry.

We now consider the industrial response to the 2009 ITQ reform in light of the model's predictions. Overall, the reform is widely seen as as a success. The downstream plants reported an increase in profits, and boats an improvement in the fish stock (International Sustainability Unit, 2011). Because the reform did not target total capture, the positive effect on fish stocks can be attributed mainly to changes in the *intensity* of fishing – capture of juvenile fish fell (Paredes and Gutierrez, 2008). In fact, most clusters of plants saw minor decreases in production after the reform came into effect, while two ports expanded considerably, as seen in Figure 2.3. Still, on the whole fishmeal production fell on average post-reform, reflecting a combination of factors.[34]

Natividad (2014) documents a rise in the price of anchovy after the reform. In order to evaluate whether suppliers and plants responded to the new regulations along the lines of our theoretical predictions, we make use of administrative production registries. The most noteworthy change in the industry after the reform was in the time profile of production downstream. Consistent with our framework's predictions, the introduction of ITQs led to longer production seasons, as seen in Figure 2.4. Fish capture and therefore production of fishmeal spread out in time as boats' incentive to rapidly capture as much as possible of the TAC early in the season was removed. The sample-weighted across-port average increase in days of production post-reform was 26 days per year, or 53 percent. Production early in the season was considerably greater before the reform, but the decline in output over time was less steep after the reform.[35]

Figure 2.5 shows that the reform also led to consolidation in the industry. As seen in the top panel, the number of active plants began a steady decline in 2009. It thus appears that the increase in the price of fish after the ITQ reform came into effect led some plants to exit the market. The bottom panel of Figure 2.5 shows the intensive margin corresponding to the extensive margin in the top panel. Before the reform, the longest– and shortest– producing plants produced for about the same period of time. After the reform, bottom-quartile plants began to decrease or stop production mid-season, while top-quartile plants continued to produce. These findings are consistent with the framework above.

The top panel of Figure 2.6 shows the average number of production days before and after the reform for efficient versus inefficient *ports*, noting that a plant's costs are partly determined by its

[34]The total allowable catch continued to be set by the regulatory authorities after the reform, using the same criteria as before the reform – primarily estimates of fish stocks. Production was unusually low in 2010 due to El Niño. Consolidation in the industry, and how the boats and plants that exited or expanded production were selected, may also have affected total production.

[35]Note that the pause in fishing mid-season in the pre-reform regime was due to a regulatory rule that was removed with the ITQ reform. Before the reform, the seasonal TAC had two components; a total amount that could be fished before a specified "pause date" (this sub-quota was reached long before the pause date due to the race for fish), and a second amount that could be fished only after a specified "recommence" date. The removal of the pause rule contributed to production being spread out in time after the reform, along with the forces highlighted in our theoretical framework.

location.[36] It is clear from the figure that plants in efficient ports greatly stretched out production across time after the reform, while plants in inefficient ports did so to a much lesser extent.

We conclude that, from the perspective of local communities, the two sectors' response to the 2009 ITQ reform first and foremost led exposure to fishmeal manufacturing to be spread out in time. How should we expect such a change in the "temporal distribution" of the downstream industry's production to affect its impact on health? *If* – as we hypothesize, and test below – plants' impact on health is driven by air pollution, this will depend both on (a) plants' "pollution production function" and (b) the health production function. We are aware of no existing evidence on (a), but find it most plausible to generally expect the amount of pollution emitted at a given point in time to be either concave or linear in the level of plant production.

When it comes to the health production function, the existing literature generally analyzes the response to duration and dose separately. The few existing studies that overcome the formidable challenges of estimating the causal effect on health of *sustained* exposure to air pollution generally find much bigger effects on health (mortality and respiratory infections) than (the effects found elsewhere of) short-term exposure.[37] Moreover, Chay and Greenstone (2003) and Clay, Lewis and Severnini (2015) both find evidence consistent with concavity in the dose response function relating infant mortality to the intensity of air pollution, and Hanlon (2015) finds the same for all-ages mortality.[38] No existing research convincingly compares the health effects of a *given* amount of pollution when concentrated versus spread out in time (in their review of the literature, Pope III et al. (2011) flag that "there are likely important risk trade-offs between duration and intensity of exposure" (Pope III et al., 2011, p. 13)), despite their importance for policy making. Consider, for example, pollution regulation based on thresholds. If our hypothesis is true, perhaps it is not such a good idea to stop sources of pollution (eg. cars or factories) when the concentration hits certain levels, but rather try to concentrate the same amount of pollution in fewer days. Overall, the evidence from the economics literature is thus consistent with a health production function

[36] We define efficiency formally below.

[37] Examples include Chen et al. (2013), Anderson (2015) and Barron and Torero (2015) (see also Isen, Rossin-Slater and Walker, forthcoming). The level of exposure differs considerably across these studies, but they all large effects of sustained exposure.

[38] It is reasonable to expect a similarly shaped production function for respiratory diseases and other diseases that are affected by air pollution and (eventually affect mortality) (see e.g. Pope III et al., 2011).

shape in which dispersing air pollution across time can exacerbate the impact on health.

2.6 Plants' Response to the Introduction of Individual Property Rights Upstream and their Impact on Health

The reform led to significant changes in suppliers' (boats') organization of production, which in turn led downstream production, on average across locations, to be spread out in time. Put loosely, individuals beforehand faced a "short, sharp" profile of production: a large amount of plant production concentrated in a relatively short period of time. Post-reform, individuals instead faced a "long, low" profile of production: the same amount of production distributed across a longer production season. We hypothesize that, within the range of port level production profiles observed during out data period in Peru, it is how many days production is spread out over that matter most for health, and that the reform's impact on health was therefore due to the move from "short, sharp" to "long, low" production. To investigate this hypothesis, we begin by estimating an adjusted version of (2.1) and (2.2) in which fishmeal production is no longer seen as a "black box". Instead we use the introduction of the reform as an exogenous shift in the number of days of production within the last 30 or 90 days for those in Near plant locations.

Before we present the results from this "structural" specification, it is important to note that we do not expect the exclusion restriction to hold in a literal sense. Relative to the change in the time profile of production, however, other changes in the production environment post-reform were either minor or arguably unable to explain a deterioration in health. First note that total production *decreased* after the reform so the observed impact of the reform cannot be explained by an overall increase in production. To address the possibility that the impact is due to a shift in production across ports, Table 2.5 shows results from regressions that are identical to those in the first panel of Table 2.4, except that we now include various measures of port level production. Controlling for production in the last 30 days, the last 90 days, or the season has a negligible effect on the size and significance of the estimated impact of the reform on health. These results suggest that, regardless of how it is specified, reallocation of market share across ports cannot explain the estimated reform effect. [39]

Second, while Appendix Table B.6 shows that average incomes and labor market outcomes are not significantly different during fishmeal production periods, it is nevertheless possible that the impact of the reform on health was due to changes in labor markets post-reform. As seen in Table 2.6, however, the reform increased the probability of having a job for fishing workers, but had no significant effects in the sample as a whole. We thus rule out the possibility that the aggregate health effect is explained by income effects or labor market responses to the reform. Third, in Appendix Table B.8, we show that the adverse health impact of the reform estimated in the full sample is not driven by impacts on fishing workers' health. Finally, it is also clear that the impact of the reform on health is not explained by pollution from the fishing boats,[40] nor by production expanding into periods of the year in which the impact of air pollution differs.[41]

The results from instrumenting for the time profile of production using $Reform \times NearPlant$ are presented in Table 2.7. The top panel shows the results of first stage regressions of days of production on $Reform \times NearPlant$. For adults and hospitals near plants, there is a strong relationship between the reform and production days in the last 30 or 90 days. On average those in the hospital sample faced just under four additional days of production in the last 30 days, and about 8.5 additional days of production in the last 90 days, while those in the adult sample saw just over five additional production days in the last 30 days, and about 9.5 additional production days in the last 90 days. In our sample of children, the first stage is less clear. The bottom panels show the results of the second stage, the impact of production days in the last 30 or 90 days—instrumented by $Reform \times NearPlant$—on health. In our hospital and adult samples, the effects of these addi-

[39]The estimated reform effects are also robust to excluding the two ports that saw an increase in total, yearly production after the reform.

[40]The boats spend little time in the ports with their engines on and thus probably do not contribute noticeably to the worse health of those who live near the plants/ports, relative to others, during production. Additionally, however, there was a considerable decrease in port queuing times post-reform (as expected (International Sustainability Unit, 2011)), indicating that post-reform changes in pollution from boats should, if anything, counteract the adverse reform effects we identify.

[41]While ex ante unlikely due to the fact that production takes place during two different periods of the year, both of which expanded across time after the reform and corresponded with worsening health outcomes, we formally investigate this possibility as follows. We construct a "New Period" variable equal to one for those periods of the year in which non-negligible production took place after the reform but not before. We then estimate specifications (2.1) and (2.2), additionally interacting "Fishmeal production $\times$ Near Plant" with "New Period" and using only post-reform data. We do not find worse health effects post-reform of fishmeal production during the "new" production periods relative to the periods on which production took place also before the reform.

tional days of production on health are positive (adverse) and significant in both the 30 and 90 day windows. Taking the results from our "structural" specification at face value thus suggests that the reform impacted health by increasing production days. Further, the magnitude of the IV results is significantly larger than those estimated in Table 2.3. This points towards possible non-linearities in the relationship between health and exposure to production: additional days of production impact health to a greater degree than the average day. We find no significant results for children in these specifications, which is unsurprising given the weakness of our first stage in the child sample.

The results in Table 2.7 are consistent with the hypothesis that the introduction of individual property rights upstream exacerbated plants' impact on health by increasing the number of days of production in Near port areas. To provide further evidence on this possibility, we exploit the fact that the average change in the time profile of production seen in Figure 2.4 masks considerable heterogeneity across locations. We ask whether the impact of the reform on health is worse in the areas that see greater increases in production days post-reform. The first source of variation in the effect of the reform on the time profile of production we exploit is regulatory: the reform effectively differed in the North/Central region and the South. The second source of such variation we exploit is based on a prediction of the framework presented above: areas with different production costs should see different changes in the time profile of production post-reform.

The North/Central region covers the large majority of the country (as seen in the map in Figure 2.1). For this reason the theoretical framework above was built to match the regulatory system in place in the North/Central region before (and after) the reform, and we expect the full-sample industrial response to the reform to largely reflect what occurred there. Indeed, fishmeal locations in the North/Central region saw a striking 97 percent (sample-weighted) increase in the average number of days of plant production per year, as predicted by the model and illustrated in Figure 2.6.Conversely, in the smaller southern region, fishing and fishmeal production instead became more concentrated in time – a 48 percent decrease in the average number of days produced per year – with the introduction of fishing ban periods there in conjunction with the ITQ reform.

The top panel of Table 2.8 shows results from a difference in differences in differences specification in which we interact the double difference term in specification (2.3) with an indicator for the household residing in the North/Central region. For respiratory hospital admissions and medical expenditures, the estimated coefficient on "Post-reform×Near Plant" is negative (beneficial) and significant for the South, and positive (adverse) and significant for the North/Central region. We similarly see a differential increase in "Any Health Issue" in the North/Central region (although the coefficient on "Post-reform×Near Plant" is positive also for the South).[42] Overall, the results in Table 2.8, with a deterioration in health in the North/Central region after the reform and signs of improvement in the South, support the hypothesis that the introduction of ITQs upstream exacerbated the downstream industry's impact on health by changing the time profile of production.

In a third and complementary test of the time-profile-of-production hypothesis, we exploit another key prediction of our model, namely that inefficient plants should exit or reduce production after the reform and efficient plants should expand. To relate changes in plants' production to health effects of the reform estimated at the location level, we need a proxy for plants' costs at the location level. We take advantage of the fact that we observe both input of fish and output of fishmeal and construct pre-reform, plant-level "efficiency" (output/input ratio) and associate each fishmeal location with the maximum efficiency observed among plants in the location before the reform.[43] As shown in Figure 2.6, days of production increased by 134 percent in more efficient locations and increased by only 46 percent in less efficient locations when the ITQ reform took effect.

The bottom panel of Table 2.8 shows results from a difference in differences in differences specification in which we interact the double difference term in specification (2.3) with port level efficiency. The adverse health effects of the reform for adults are concentrated in locations with efficient plants; beneficial, though insignificant, health effects are seen for adults in locations with inefficient plants. Similarly, we see a large (but imprecisely estimated) increase in respiratory hospital admissions in locations with more efficient plants, but not in locations with less efficient plants.

The majority of locations with efficient plants are located in the North/Central region. Note,

[42]Child outcomes are not included in Table 2.8 because we have insufficient observations in ENDES to estimate standard errors in difference in differences in differences specifications.

[43]This maximum is based on the overall input/output ratio in the year 2008. For ports with only one plant, it is simply the 2008 input/output ratio for that plant. This measure serves as a proxy for the limits on efficiency imposed by the geography of that port, and hence provides a measure of the port specific component of costs.

however, that efficiency predicts both the response in days produced and in the health consequences of the reform also *within* the North/Central region as seen in Appendix Table B.9. Further, the strikingly different effects of the reform on health outcomes in the North/Central region and the South, and in locations with efficient versus inefficient plants, are not driven by differential effects on incomes or labor market outcomes, nor on fishing workers' health.[44] We conclude that the concentration of adverse health effects in fishmeal locations where production days increased after the introduction of individual property rights upstream supports the hypothesis that the downstream plants' exacerbated impact on health post-reform was due to changes in the time profile of production.

In sum, the battery of tests presented in this section are strongly supportive of the view that Peruvian fishmeal plants' impact on health increased after the introduction of property rights among their suppliers because the regulatory change affected the plants' time profile of production due to the interlinkage between the two sectors. While the across-location movements in plants' market share after the reform intensify location level changes in the time profile of production and thus help us test our hypothesized explanation for the deterioration in health post-reform, most fishmeal locations saw negligible changes in the *level* of production post-reform, as seen in Figure 2.3. On average the ITQ reform can thus be thought of as spreading out downstream production over time without changing the total amount of production. Our findings indicate that such a dispersion worsens the impact of polluting plant production on health.[45]

[44]Appendix Table B.8 shows that there is no significant differential effect across regions or high versus low cost ports on either health or labor market outcomes for those who work in the fishing industry.

[45]The port-level average change in the time profile of fishmeal production after the reform is affected "directly" by boats spreading out fishing in time to a much greater extent than by the movements in market share. As seen in Figure 2.3, only two ports saw a non-negligible increase in the level of production after the reform, six saw a considerable decrease, while almost all ports (in the North/Central region) saw a significant increase in days produced after the reform. We estimate very similar reform effects if we limit the sample to those 15 ports that saw a negligible change in the level of production after the reform, but lose significance because 2/3 of the sample live near the ports that saw bigger changes in levels of production.

2.7 Why Plants' Response to the Introduction of Individual Property Rights Upstream Matters for Health

2.7.1 Fishmeal production, air pollution, and health

We have shown that exposure to fishmeal production is harmful to individuals' health; that the impact on health increases after the upstream ITQ reform; and that the reason is that plant production spreads out over time when suppliers no longer face incentives to "race" for the resource early in the season. We hypothesize that air pollution generated by the plants during the production process explains these three sets of findings. To investigate this hypothesis, we first return to the flexible specification of production in (2.1) and (2.2).

We start, in Appendix Table B.10 by disaggregating respiratory hospital admissions into ICD sub-categories. Doing so shows that the overall effect on health is driven primarily by a higher incidence of "Acute Upper Respiratory Infections" during production periods, consistent with air pollution as the underlying mechanism.[46]

To investigate more directly, we estimate (i) the effect of fishmeal production on air pollution, and (ii) the effect of plant-generated air pollution on adult and child health. This can be done for the part of our sample that live in the Lima region (27 percent), where, as discussed above, daily data on ground-level concentration of four air pollutants – PM^{10}, $PM^{2.5}$, NO_2 and SO_2 – from five measuring stations is available. For each date we construct the average concentration of each of the measured air pollutants during the last 30 days in the port/cluster of plants closest to Lima as an average over the pollutant concentration at each of the five measuring stations weighted by inverse distance between the station and cluster of plants as in Schlenker and Walker (forthcoming)[47] We then run a location-level regression with year$\times$month fixed effects in which we regress the average pollutant level in the Lima area during the 30 days prior to the date in

[46]Using specifications identical to those in Table 2.3 with different subcategories of respiratory admissions as dependent variables, we find a coefficient on "Fishmeal Production in Last 30 Days x Near Port" of 3.192 for "Acute Upper Respiratory Infections." The estimated effect is significant at the 5 percent level, and suggests that the subcategory explains about 80 percent of the total effect on respiratory admissions.

[47]This is done after using the empirical distribution at other stations to impute missing values of a given pollutant at a given station, also following Schlenker and Walker (forthcoming). Note that using a single station and imputing missing values using this technique gives similar results (see Appendix Table B.11), as does using the mean, max or median across stations.

question on fishmeal production by the six plants that are located at the port that is closest to the five stations – Callao – during the same 30 days. As seen in the top panel of Table 2.9, we find that fishmeal production is significantly positively correlated with all four air pollutants. A 50 percent increase in production in the last 30 days increases PM^{10} by just under 1 percent, $PM^{2.5}$ by 1.3 percent, NO_2 by 0.5 percent, and SO_2 by 1.1 percent.[48] Figure 2.7 directly plots the data on air pollution concentration levels on a given day against the level of fishmeal production on that day, controlling for month fixed effects. The relationship between the two time series is clearly increasing and appears approximately linear.

In the bottom panel of Table 2.9, we merge the air pollution data with outcome data for the respondents and hospitals in the Lima area. We regress respiratory hospital admissions and adult health outcomes on the 30 day average level of an air pollutant, separately for each of the four pollutants, and instrument each by fishmeal production.[49] We present these IV regressions to illustrate the magnitude of the component of fishmeal production's impact on health that may arise through air pollution, acknowledging that the exclusion restriction is likely violated.[50] While distinguishing the relative contributions of different air pollutants is not the goal of this exercise, it is important to note that PM is regarded by many as a general indicator of air pollution, receiving contributions from fossil fuel burning, industrial processes, and other underlying sources (see e.g. Greenstone and Hanna, 2014). Restricting attention to the PM regressions thus provides a (very) conservative interpretation of the impact of pollution generated by fishmeal production estimated in Table 2.9.[51]

The results in Table 2.9 show that a one standard deviation (10 $\mu g/m^3$) increase in PM^{10}, as instrumented by fishmeal production, gives an increase in respiratory admissions of 1.3 percent (0.7 percent). A one standard deviation (10 $\mu g/m^3$) increase in $PM^{2.5}$ gives an increase in respiratory admissions of 3.2 percent (2.7 percent). A one standard deviation (10 $\mu g/m^3$) increase in NO_2

[48]Once again, given that we estimate the effect of log fishmeal production on pollutants, we display the impact of a 50% change in fishmeal production as $\beta \times \ln(150/100)$.

[49]Child health outcomes are not included because the ENDES data does not have sufficient treatment observations in the vicinity of Callao to estimate standard errors.

[50]PM^{10}, $PM^{2.5}$, NO_2 and SO_2 have all been linked with adverse health outcomes in the existing literature. The exclusion restriction is violated in each of these regressions in the sense that fishmeal production likely affects health also through (at least) three other air pollutants. For a similar approach, see e.g. Malamud and Pop-Eleches (2011).

[51]The correlation between PM^{10} and $PM^{2.5}$, NO_2 and SO_2 is 0.83, 0.39 and 0.37, respectively. The correlation between $PM^{2.5}$ and NO_2 and SO_2 is 0.37 and 0.48 respectively.

gives an increase in respiratory admissions of 6.6 percent (11.2 percent). Finally, a one standard deviation (10 μg/m^3) increase in SO_2 gives an increase in respiratory admissions of 13.4 percent (16.2 percent). All pollutants, as instrumented by fishmeal production, also significantly increase "Any Health Issue". These effect sizes are comparable to those that have been found in epidemiological studies relating health outcomes to air pollution.[52] Note that while the fact that some of the pollutants produced are so fine as to penetrate homes (e.g. $PM^{2.5}$) complicates "avoidance behavior", any such behavioral response to the pollution generated by plant production would lead us to underestimate the direct health effects of production.[53]

In sum, the evidence presented in this sub-section is strongly supportive of air pollution emitted by plants being the primary mechanism through which fishmeal production affects adult and child health.[54] In the next section we explore why the impact of plant-generated air pollution on health is increased when production is spread out over time.

2.7.2 The time profile of production and the impact of air pollution on health

There are two obvious possible reasons why spreading out manufacturing over time could worsen its impact on health: that the total amount of air pollution generated in the "long, low" production scenario is greater than that in the "short, sharp" scenario, and/or that prolonged exposure to low levels of air pollution is worse for health than short-term exposure to higher pollution levels.

All four measured air pollutants decreased in concentration post-reform in the Lima area,[55].

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[52]In their review of the (primarily correlational) epidemiological literature on particulate matter and health outcomes, Anderson, Thundiyil and Stolbach (2012) cite studies that for example associate a 10 $\mu g/m^3$ (14.8 $\mu g/m^3$) increase in PM^{10} with a 2.28 percent (3.37 percent) increase in respiratory hospital admissions, and a 10 $\mu g/m^3$ increase in $PM^{2.5}$ with a 2.07 percent increase in respiratory admissions. Our estimated effect sizes thus appear plausible in light of the epidemiological literature, though is of course important to keep in mind that the IV results presented here may overestimate the health effect of *each specific* air pollutant by "loading" the health effect of the other air pollutants onto the one in question. Restricting attention to the PM results avoids this possibility, but likely yields an underestimate of the total component of the impact on health that is driven by air pollution.

[53]The final panel of Table 2.5 shows the effects of reform on health in Lima. The effects for hospital admissions and log medical expenditures are remarkably similar to our main effects, although neither are significant given the relatively small sample size.

[54]We additionally attempted to compare individuals and hospitals located downwind from the fishmeal plants to those located upwind. The estimated coefficient on "Fishmeal production $\times$ Near Plant $\times$ North of Plant" is positive in almost all specifications (indicating a more adverse health impact of fishmeal production north of the plants) and for some health outcomes also significant. While winds are reported to blow north most of the time along the coast of Peru, we do not have wind maps that would allow us to precisely define downwind/upwind locations and exploit time variation in wind directions.

[55]PM^{10}, $PM^{2.5}$, NO_2 and SO_2 decreased by in 5, 12, 43 and 18 percent in average concentration during the first year

We should not overinterpret this evidence – other factors may also have contributed to changes in air pollution in the Lima area after the point in time when the reform was introduced – but it is difficult to reconcile with a hypothesis in which an increase in overall pollution levels explains the exacerbated impact of downstream plants on health post-reform.

We posit that the primary explanation for why "long, low" plant production is worse for health than "short, sharp" lies in the shape of the health production function, or more specifically, the three-dimensional relationship relating health at a given point in time to both the duration of exposure to air pollution and the intensity of exposure. Though the existing literature typically analyzes the two underlying relationships (duration and dose response) separately, there is in fact a considerable body of evidence in the epidemiological literature indicating that air pollution in high-concentration contexts may have worse health consequences if dispersed over time. Pope III et al. (2015) summarize the evidence on dose (concentration) response: "recent research suggests that the C-R [concentration response] function [between $PM^{2.5}$ and mortality risk] is likely to be supralinear (concave) for wide ranges" (Pope III et al., 2015, p. 516). The authors point out that air pollution in low and middle income countries is frequently in the (higher) part of the concentration range where concavity in dose response is now uncontroversial – as are the fishmeal locations in our sample (though many epidemiologists argue that concentration response may be concave also at lower concentrations (Crouse et al., 2012; Krewski et al., 2009)). The literature on cardiovascular disease risk of exposure to tobacco smoke similarly finds a concave dose response function (California Environmental Protection Agency, 1997; Law, Morris and Wald, 1997; Smith and Ogden, 1998; Smith, Fischer and Sears, 1999). Law et al. (1997) finds the same for lung cancer risks of tobacco. Note that there is considerable biological overlap between the types of health issues considered in this paper and those analyzed in the epidemiological literature summarized in this Sub-section. For example, Pope III et al. (2011) point out that cardiovascular and pulmonary ("of or affecting the lungs") diseases have "substantial common co-morbidity" and argue for conceptualizing a shared health production function for "cardiopulmonary" diseases.

Pope III et al. (2011) summarize the epidemiological evidence on duration response for cardiovascular mortality risk of air pollution and conclude that "the evidence suggests that...longer

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post-reform respectively.

duration exposure has larger, more persistent cumulative effects than short-term exposure, but the highest marginal effects occur with relatively short-term exposures most proximal in time" (Pope III et al., 2011, p. 1). Beverland et al. (2012), for example, find that "short-term [black smoke] exposure-mortality associations were substantially lower than equivalent long-term associations".

This paper is the first to provide direct evidence on the three-dimensional relationship between health, dose, and duration of air pollution. We do so via the natural experiment represented by Peru's ITQ reform, in which the level of air pollution-generating activity remained essentially constant but was spread out over time. In Figure 2.8 we take advantage of the detailed information on cause of admission available in the hospital data, and the fact that, considering all ports and seasons observed in our datasets, we observe many different combinations of production levels and time profiles. We relate the total number of hospital admissions for a given cause in a location and production season to the number of days of production that season, controlling for the total amount of production. The figure shows two important results. First, the point estimates are positive for almost every hospital admission category, and significant also for about half. This highlights a central argument of the paper: the number of days of production is harmful to health, even after conditioning on the total (seasonal) level of production. Second, and perhaps more importantly, the disease categories that respond most to how spread out production is across time are exactly the ones we a priori expect to be most influenced by air pollution, such as respiratory issues, digestive issues, and skin issues. While ultimately suggestive, the evidence in Figure 2.8 is strongly indicative that a given amount of air pollution is more harmful to health if occurring at low concentrations for long periods of time, at least within the range observed in Peru.

2.8 Quantifying the Risks of Piecemeal Regulation

In this section we analyze what our estimates imply about the magnitude of the risks of piecemeal regulatory design by comparing the cost of the estimated worsening of downstream externalities to the benefit of the decrease in the targeted upstream externality. We have seen that the introduction of individual property rights upstream exacerbated downstream plants' impact on the health of the local population, but that fishmeal companies reported an increase in profits and their suppliers an increase in fish stocks post-reform.[56]

In the costs and benefits of the ITQ reform we include the (monetized) value of the deterioration in health and the increase in sector profits after the reform.[57] We obtained data on the profits of the fishmeal companies that are publicly listed from publicly available financial statements. Since not all companies are listed, we scale these up by extrapolating based on the share of production the publicly listed firms account for in each year to arrive at a yearly, sector-wide estimate. The resulting estimate of the increase in sector-wide profit in the first post-reform year is USD 219 million. (The details of the cost/benefit calculations are in the notes of Table 2.10).

We consider only the increase in disease episodes associated with a respiratory hospital admission and medical expenditures in the total health costs of the reform.[58] We start with the 55,516 additional respiratory hospital admissions caused each year as estimated in Table 2.4. To quantify the cost of these respiratory disease episodes, we first convert to the equivalent number of "years lived with disability (YLDs)", using standard weights from the Global Burden of Disease Study 2010 (Murray, 2012; U.S. Environmental Protection Agency, 2010). Assuming conservatively that the estimated additional disease episodes did not result in increased mortality, our results imply that in the first post-reform year 5,681 disability-adjusted life year equivalents were lost due to the reform's impact on respiratory diseases. Finally, we use a conventional "value of statistical life (VSL)" method to monetize the DALYs lost.[59] As there are no existing convincing estimates of the VSL in Peru, we present estimates from using both the value estimated for Africans in Leon and Miguel (2015)—the only existing paper to estimate VSL in a developing country setting with revealed preference methods and using a sample fairly close to ours in average income levels—and the VSL for Americans estimated and used by the U.S. Environmental Protection Agency (Murray, 2012; U.S. Environmental Protection Agency, 2010). To scale these VSL estimates, we use the GNI

[56]The increase in fish stocks was likely due to lower juvenile fish capture after the reform, when boats no longer "raced" for fish early in the season. There were likely several reasons for the increase in profits. These include, for example, a decrease in overcapacity. See also Natividad (2014).

[57]Local incomes are not considered in our cost/benefit calculations as we find no significant effect of the reform on average incomes.

[58]We do not count the health issues measured in the ENAHO and ENDES surveys because it is difficult to estimate the monetary cost of "Any Health Issue", and because the extent to which the health issues reported in the surveys also led to hospital admissions and hence would be double counted if included is unclear.

[59]See e.g. Ashenfelter and Greenstone (2004); Ashenfelter (2006); Hall and Jones (2007); Greenstone, Ryan and Yankovich (2012); Leon and Miguel (2015).

per capita in Sub-Saharan Africa, the U.S., and Peru with the commonly used elasticity recommended by Hall and Jones (2007). The per-year costs of the 2009 ITQ reform due to its impact on respiratory disease episodes estimated using this methodology is between USD 297 million (with the Leon and Miguel (2015) VSL) and USD 128 million (with the EPA VSL). To this we add the additional medical expenditures caused to finally arrive at a total, yearly health cost of the reform of USD 174-343 million.[60]

Comparing these cost estimates to the estimated yearly benefits of the reform to the industry of USD 219 million, it appears that the costs of the 2009 introduction of individual property rights among industrial fishing boats in Peru, due to the unintended add-on effect on downstream plants' impact on health, are of the same order of magnitude as the benefits of the reform. While our calculation probably underestimates the total health costs, as we include only the impacts on respiratory diseases, the methodology used to monetize health costs rests on strong assumptions. Our goal is not to conclusively say whether the costs of the reform exceeded the benefits, but simply to illustrate that the unexpected health impacts of the reform are a first order concern.

2.9 Conclusion

This paper considers the interplay of externalities generated in different parts of the economy due to the interlinkages between firms, and how regulation designed from a partial equilibrium perspective affects the total externalities generated in a production chain. We analyze how a Coasian solution – individual property rights – to overextraction among suppliers in one of the world's largest natural resource sectors affected the impact on health of the downstream manufacturing plants that process the resource.

Using hospital admissions records and survey data on individual health outcomes, and exploiting government-imposed, irregularly timed semi-annual production ban periods in a difference in differences approach, we first document that production by the downstream plants that

[60]To consider also the reform's impact on fish stocks, we can potentially use government data on stocks to inform how far into the future we should "project" the additional, yearly profits and health costs due to the ITQ reform. There is suggestive evidence that the reform succeeded at slowing the decline in the fish stock. We expect the health costs to be more persistent than the increase in profits, and thus the net cost of the reform to grow over time. (For example, some of the increase in profits in the first year post-reform likely came from a one-time sale of excess plant capacity. Comparing 2011 to 2006, Paredes and Gutierrez (2008) estimate that sector-wide profits increased by USD 144 million.) But we prefer to be conservative and count only the per-year gap.

convert fish from Peru's industrial fishing boats into fishmeal harms adult and child health. We then analyze how the impact on health changed with a 2009 reform that introduced individual, transferable quotas (ITQs) upstream so as to sustain fish stocks. We find that, on average across locations, plants' adverse impact on health increased substantially after the reform, leading to e.g. 55,000 additional respiratory hospital admissions per year and a total, yearly health cost of the reform exceeding USD 174 million.

While total downstream production fell slightly, the quotas removed boats' incentive to "race" for fish early in the season and led inefficient plants to decrease production or exit the market and efficient plants to expand production across time, as predicted by a two-sector model with heterogeneous plants. As a result, downstream production was spread out in time on average across locations. We show that the plants' exacerbated impact on health after the reform was due to this change in the time profile of production.

We use a sub-sample for which air pollution data is available to explore why "long, low" production is worse for health "short, sharp" exposure. We find suggestive evidence that the explanation lies in the shape of the health production function, i.e. that longer periods of exposure to moderate air pollution levels are worse for health than shorter periods of higher intensity exposure. While this paper is the first to consider the health consequences of simultaneous changes in duration and intensity of exposure to polluting plant production, our findings are thus in line with the existing epidemiological evidence, which points to concavity in dose response and importance of concurrent exposure and the duration of exposure to air pollution (Pope III et al., 2015; Pope III and Dockery, 2013; Crouse et al., 2012; Pope III et al., 2011; Krewski et al., 2009; California Environmental Protection Agency, 1997).

These results highlight that the exacerbation of externalities elsewhere in the economy that are ignored when regulatory reforms are designed can be very large, and that regulations' effect on the time profile of production – often ignored by researchers and regulators – can be crucial for industries' impact on welfare. In the particular and common case of natural resource suppliers supplying downstream manufacturing plants, policymakers face a trade-off. On the one hand, the objective of preventing depletion of the resource suggests "internalizing the externality" by giving upstream market participants individual property rights. Such Coasian solutions will tend to spread out production in time as the incentive to "race" for the resource is removed. On the other hand, the evidence in this paper suggests that the impact of pollution on health may be minimized by concentrating downstream production in time.[61] The case analyzed in this paper illustrates a general take-away: the importance of the method and "level" of regulation used to restrict each externality being optimally chosen *in equilibrium*, taking into account the input-output links that connect different firms in the economy.

[61]Our findings do not speak to the relative merits of the many regulatory methods that can be used to restrict or influence the time profile of production.

Tables and Figures

FIGURE 2.1
LOCATION OF FISHMEAL PORTS AND SAMPLING CLUSTERS

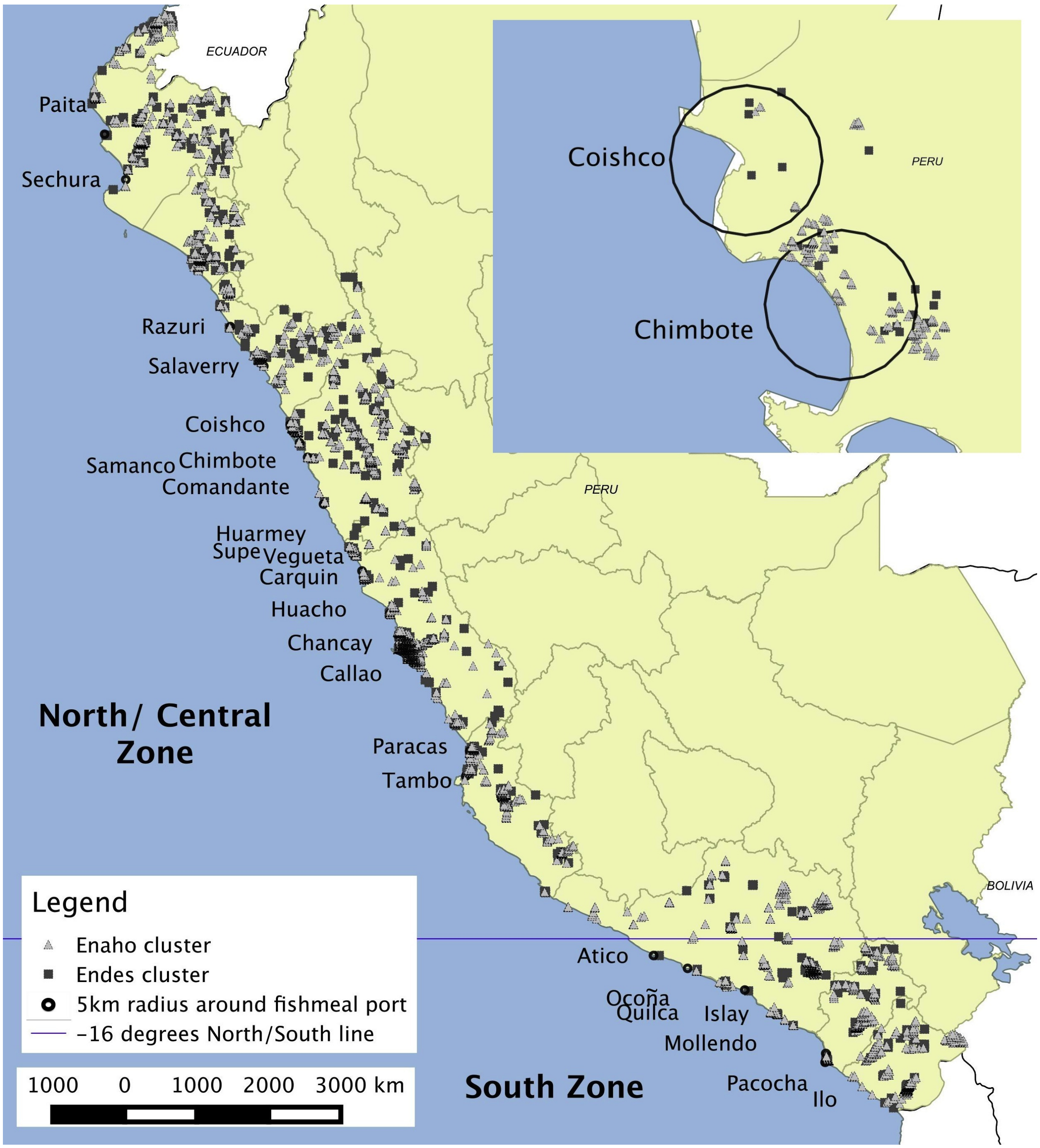
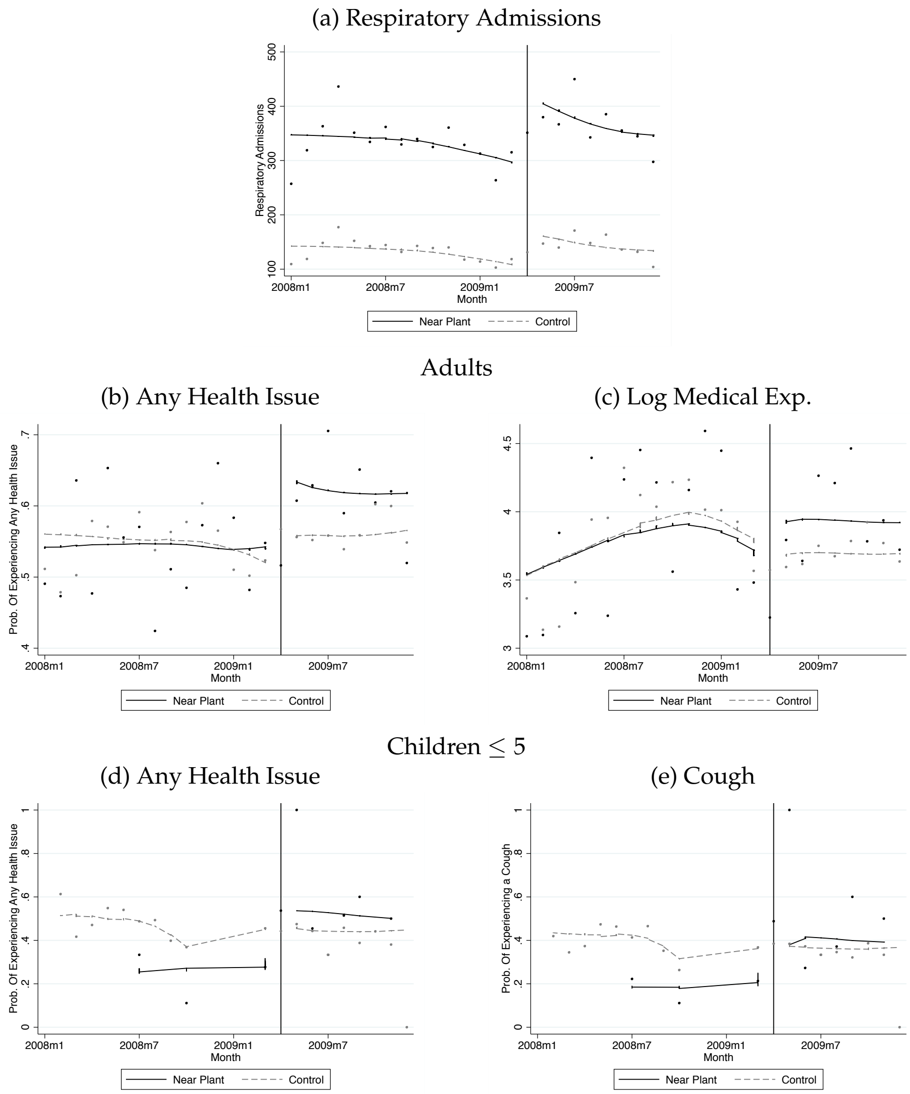
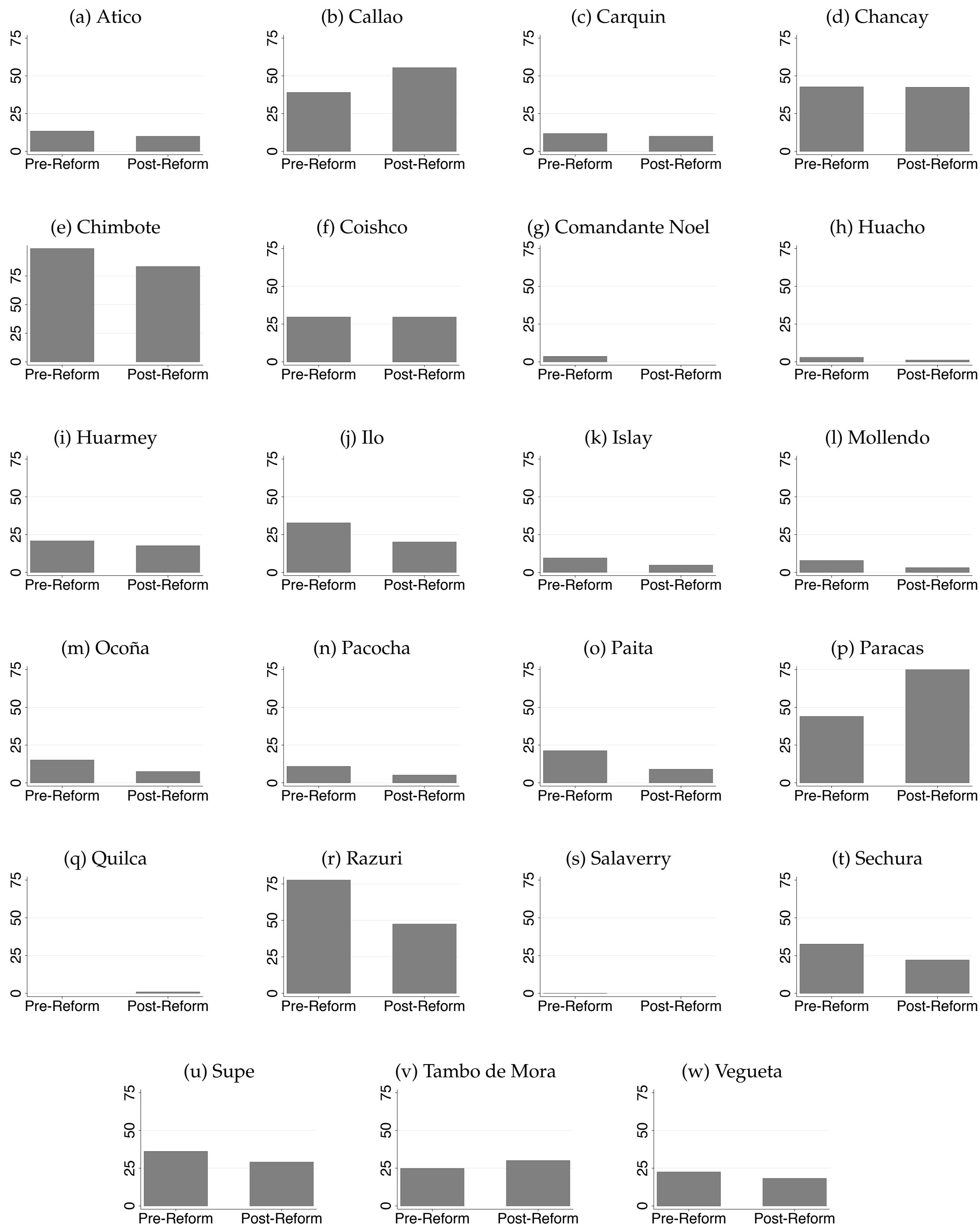


FIGURE 2.2
PLOTTING HEALTH OUTCOMES ACROSS TIME PRE- AND POST-REFORM



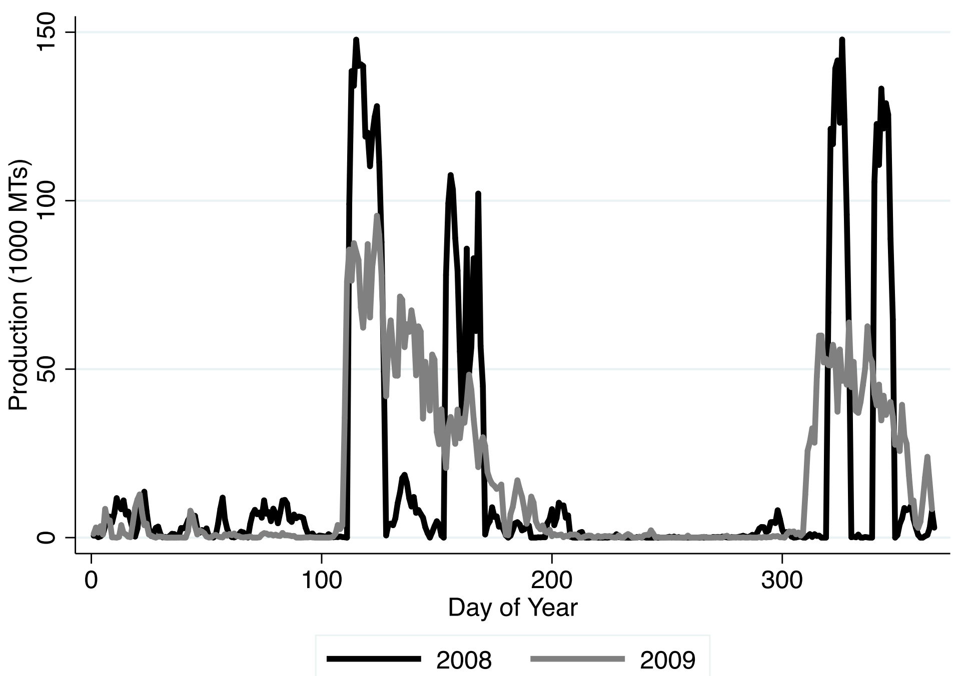
Scatter plots and lowess smoothing of health outcomes across months. Black lines and dots are based on data for those living near plants, gray lines and dots are based on data for all others. Dots are monthly mean levels for each group. Adult data includes those over 13 years of age living in costal regions sampled in ENAHO (2008-2009), child data includes those under 6 years old living in coastal regions sampled in ENDES (2008-2009). Note that no clusters in ENDES sampled in the early part of 2008 were near a plant. Noisier graphs for child outcomes are in general due to smaller sample sizes for children. Smoothed separately before and after the start of the reform in the north region (April 2009). The small South region is omitted due to a later reform starting date and different regulatory change.

FIGURE 2.3
PORT-LEVEL FISHMEAL PRODUCTION PRE- AND POST-REFORM



Average yearly production levels by port in 1000s of metric tons, pre-and post-reform. There was no production in Quilca pre-reform.

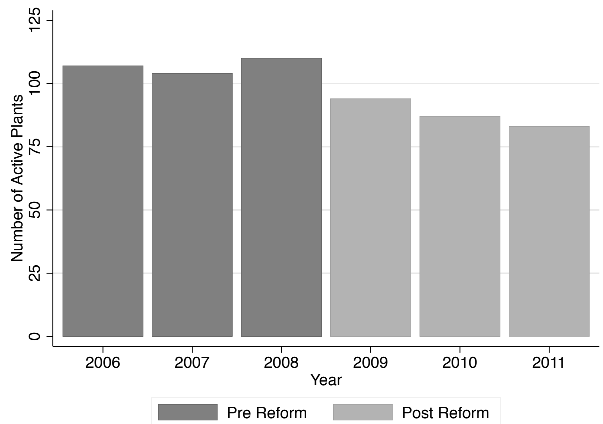
FIGURE 2.4
TIME PROFILE OF FISHMEAL PRODUCTION



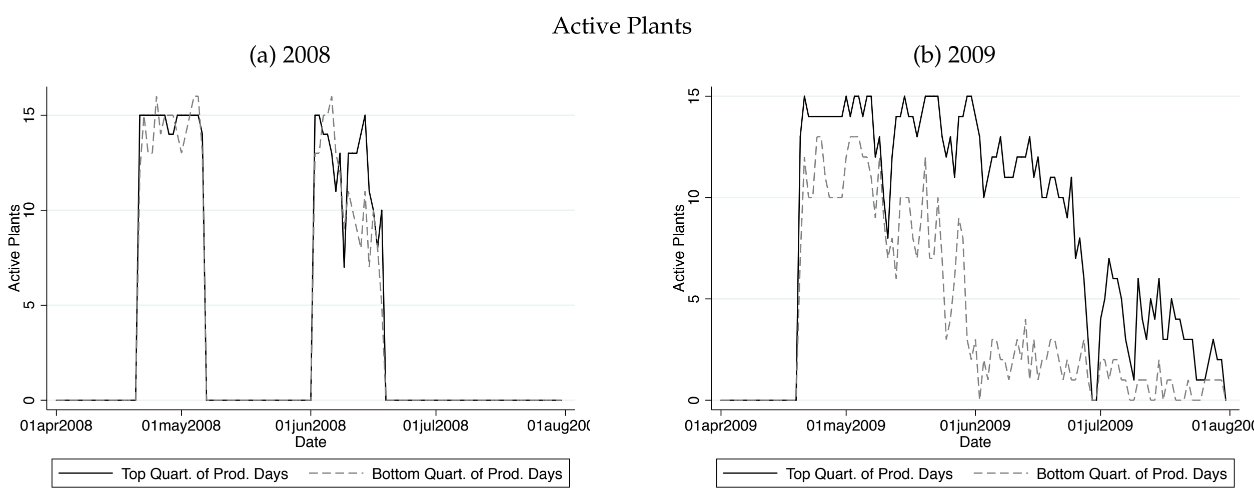
Comparisons of production (measured as fish inputs) in 1000s of metric tons in 2008 and 2009. Before the reform, the seasonal regulation (TAC) had two components; a total amount that could be fished before a specified "pause date" (note that this sub-quota was reached long before the pause date due to the race for fish) and a second amount that could be fished only after a specified "recommence" date. The removal of the pause rule contributed to production being spread out in time after the reform, along with the forces highlighted in our theoretical framework.

FIGURE 2.5
PLANT ACTIVITY PRE- AND POST-REFORM

NUMBER OF ACTIVE PLANTS ACROSS YEARS



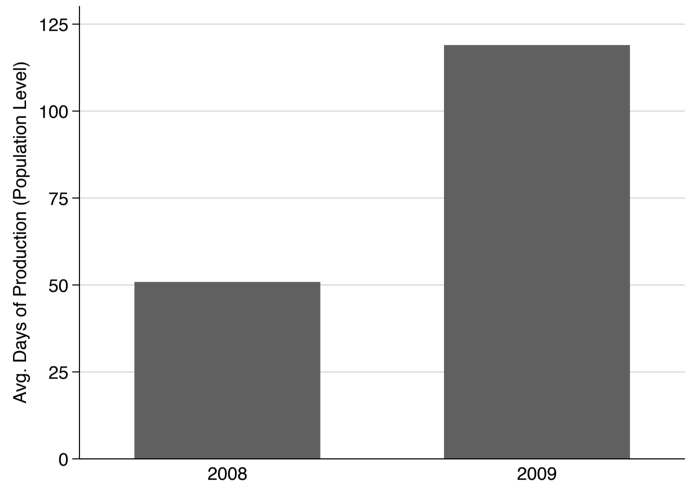
NUMBER OF ACTIVE PLANTS DURING THE SEASON: TOP VS. BOTTOM QUARTILES OF 2009 PRODUCTION DAYS



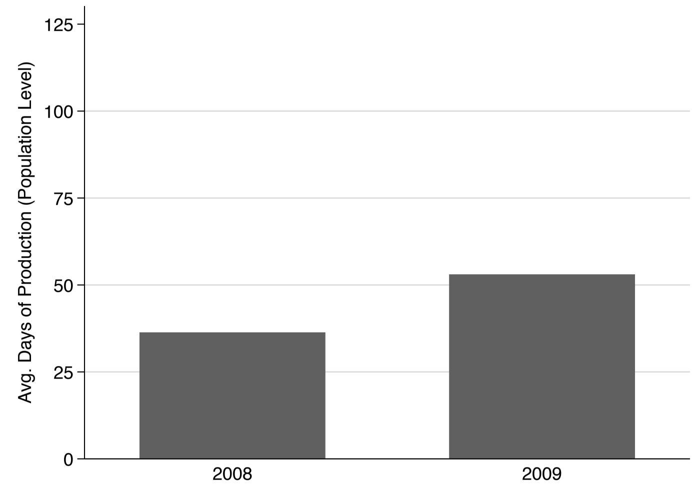
Top figure plots total number of active plants by year, where a plant is considered active if it purchases fish input any day of the year. The lower figures plot the number of active plants during the first production seasons in 2008 and 2009. The solid line in each shows plants in the top quartile of production days in 2009, while the dashed line shows plants in the bottom quartile of production days in 2009.

FIGURE 2.6
AVERAGE PORT LEVEL DAYS OF PRODUCTION BY EFFICIENCY: PRE-REFORM VS. POST-REFORM

(a) Ports in Top Quartile of Efficiency

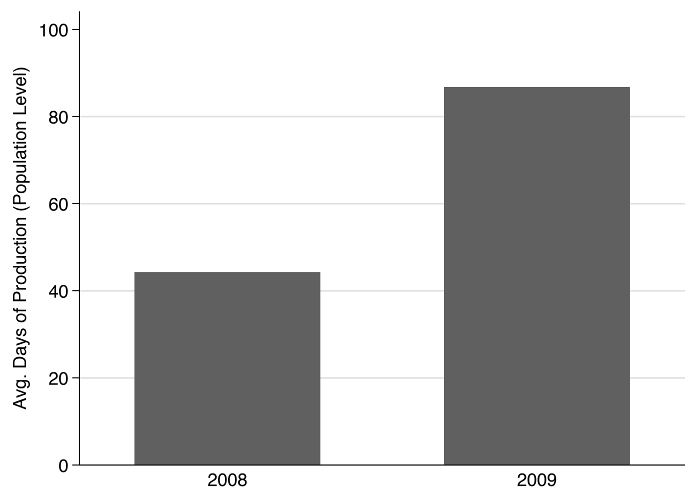


(b) Ports in Bottom Quartile of Efficiency

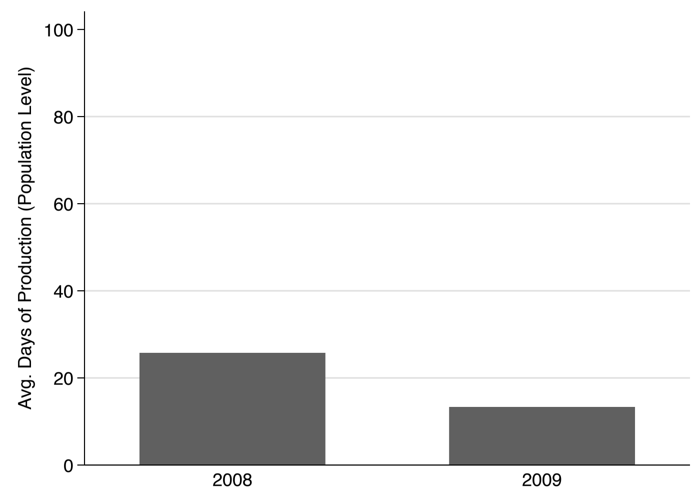


AVERAGE PORT LEVEL DAYS OF PRODUCTION BY REGION: PRE-REFORM VS. POST-REFORM

(a) North/Central Region

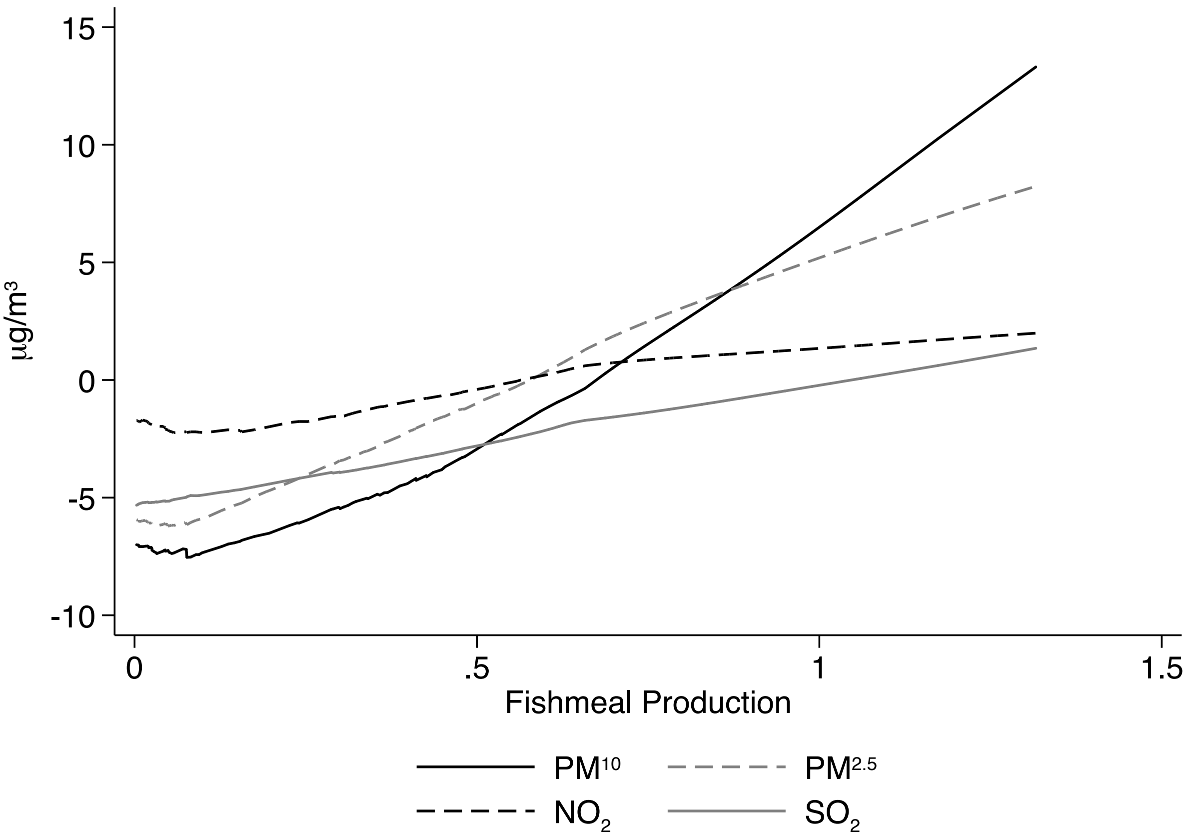


(b) South Region



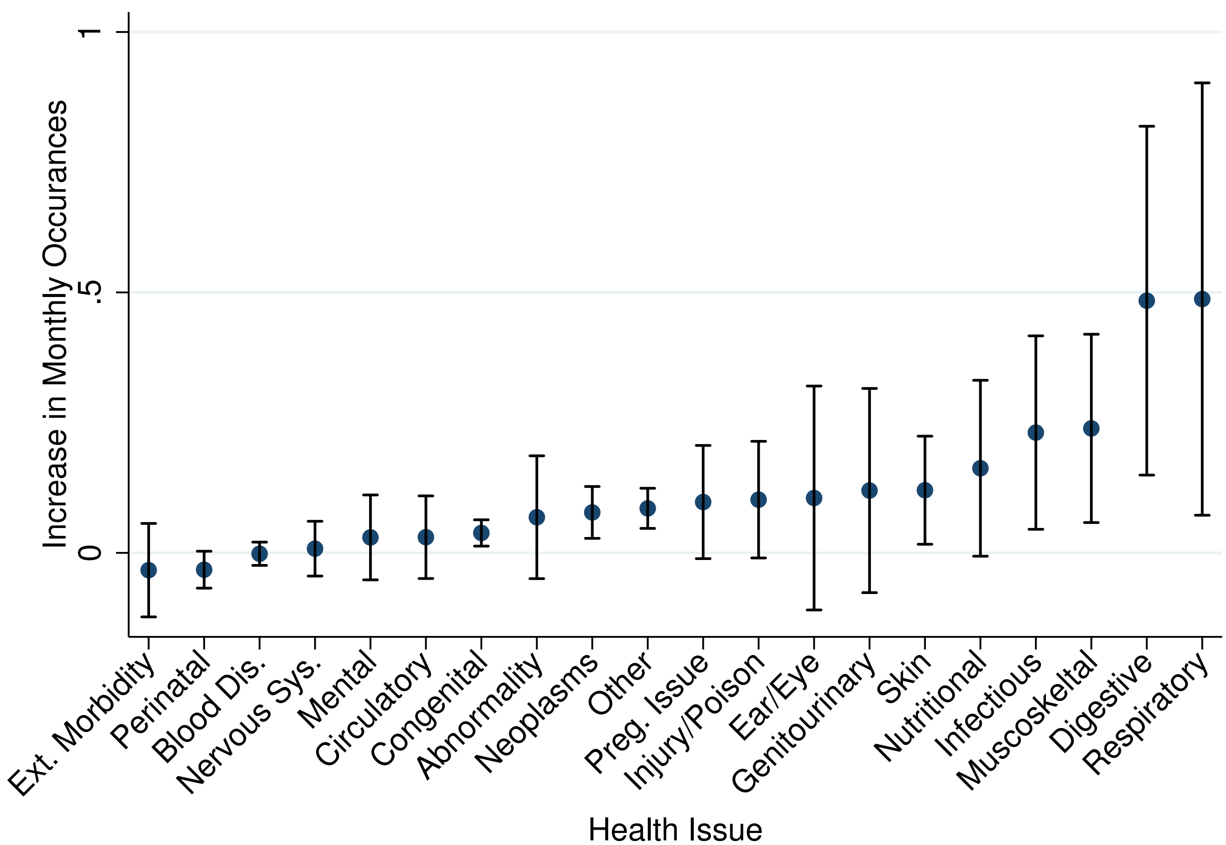
Top figures show average yearly days of production at the port level (weighted by representation in our adult sample) pre-and post- reform, split by port level efficiency. Bottom figures show average yearly days of production at the port level (weighted by representation in our adult sample) pre-and post- reform in the North/Central and South regions. A production day is defined by $>$ 1000 MTs of input at the port level. Efficiency is measured as the maximum port level yearly output/input ratio.

FIGURE 2.7
DAILY FISHMEAL PRODUCTION AND AIR POLLUTION IN LIMA



Lowess smoothing of month demeaned pollutant levels (in $\mu g/m^3$) against daily fishmeal production in Callao (measured as inputs in 10,000s of MTs) for days with positive production. Pollutant levels at the port of Callao are calculated as the inverse distance weighted mean of 5 air quality measurement stations in Lima. Missing values at individual stations are imputed using the following method: (i) construct the empirical distributions for each of the five stations. (ii) On days that data is missing at a given station, find the value of the empirical distribution on that day for each of the other stations. (iii) Take the inverse distance weighted mean of those values. (iv) Replace the missing data with the concentration corresponding to the point in the empirical distribution found in (iii).

FIGURE 2.8
IMPACT OF DAYS OF FISHING ON HOSPITAL VISITS: CONTROLLING FOR PRODUCTION LEVEL



Results from regressions of hospital visits at the season level for various health issues on total seasonal days of fishing and the total level of seasonal production, as well as hospital and season fixed effects. Coefficients on days of fishing are plotted with 95% confidence intervals. Standard errors are clustered at the hospital level. Only hospitals within 20km of ports are included.

TABLE 2.1
SUMMARY STATISTICS: HEALTH OUTCOMES IN NEAR PLANT AND CONTROL LOCATIONS

	Health Outcomes								
	Near Plant				Control				
	No Prod.		Prod. Season		No Prod.		Prod. Season		
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Diff-in-Diff
Respiratory Admissions	317.8	331.9	334.9	348.9	129.7	173.4	132.7	183.0	14.1***
									(4.49)
Any Health Issue (Adults)	0.58	0.49	0.62	0.49	0.59	0.49	0.59	0.49	0.041***
									(3.99)
Log Medical Expend.	3.88	2.88	3.88	2.86	3.71	2.86	3.68	2.88	0.027
									(0.45)
Any Health Issue (Children)	0.40	0.49	0.46	0.50	0.44	0.50	0.48	0.50	0.019
									(0.54)
Cough	0.32	0.47	0.38	0.49	0.36	0.48	0.40	0.49	0.022
									(0.64)
	Covariates								
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Diff-in-Diff
Age (Adults)	35.8	21.3	37.2	20.0	35.7	20.6	36.3	20.2	0.85*
									(2.08)
Age (Children)	2.44	1.42	2.54	1.42	2.50	1.43	2.50	1.43	0.095
									(0.94)
Male (Adults)	0.49	0.50	0.48	0.50	0.49	0.50	0.48	0.50	0.00049
									(0.05)
Male (Children)	0.52	0.50	0.52	0.50	0.51	0.50	0.50	0.50	0.0017
									(0.05)
Years of Education (Adults)	9.87	4.21	9.69	4.29	9.21	4.60	9.47	4.48	-0.44***
									(-4.59)
Mothers Years of Educ. (Children)	10.8	3.51	11.6	3.04	9.54	4.14	9.81	3.99	0.54
									(1.89)
Current. Lives in Birth Prov. (Adults)	0.43	0.49	0.47	0.50	0.39	0.49	0.40	0.49	0.031**
									(2.99)
Indigenous Language (Adults)	0.078	0.27	0.11	0.31	0.13	0.34	0.13	0.34	0.038***
									(5.32)
HH Asset Index (Children)	0.83	0.67	0.90	0.65	0.29	0.93	0.44	0.91	-0.080
									(-1.24)
Observations (Adults)	5172		4563		93852		58225		
Observations (Children)	631		319		9203		4531		
Observations (Hospitals)	13563		8979		77463		41976		

Adult data from ENAHO (2007-2011), child data from ENDES (2007-2011) and hospital admissions from administrative data. Adults older than 13 and children under 6 living in coastal regions are included. All health outcomes excluding "Log Medical Expenditure" and counts of hospital admissions are binary. Medical expenditure is measured in Peruvian Soles. Production seasons are periods in which there has been a production day ($>$ 1000 MTs of input at the port level) in the last 30 days.

TABLE 2.2
SUMMARY STATISTICS: HEALTH OUTCOMES PRE- AND POST-REFORM

	Health Outcomes								
	Near Plant				Control				
	Pre-Reform		Post-Reform		Pre-Reform		Post-Reform		
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Diff-in-Diff
Respiratory Admissions	327.5	352.5	322.3	327.2	136.5	183.6	124.7	169.4	6.70*
									(2.19)
Any Health Issue (Adults)	0.55	0.50	0.64	0.48	0.57	0.50	0.60	0.49	0.059***
									(5.73)
Log Medical Expend.	3.66	2.89	4.06	2.84	3.59	2.86	3.79	2.88	0.21***
									(3.52)
Any Health Issue (Children)	0.39	0.49	0.43	0.50	0.47	0.50	0.45	0.50	0.063
									(1.69)
Cough	0.32	0.47	0.35	0.48	0.39	0.49	0.37	0.48	0.056
									(1.52)
	Covariates								
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Diff-in-Diff
Age (Adults)	37.7	20.0	35.4	21.3	36.2	19.7	35.7	21.0	-1.72***
									(-4.19)
Age (Children)	2.39	1.40	2.50	1.43	2.51	1.44	2.49	1.43	0.13
									(1.19)
Male (Adults)	0.49	0.50	0.48	0.50	0.49	0.50	0.49	0.50	-0.011
									(-1.06)
Male (Children)	0.50	0.50	0.53	0.50	0.50	0.50	0.50	0.50	0.026
									(0.68)
Years of Education (Adults)	9.64	4.27	9.90	4.22	9.32	4.54	9.30	4.57	0.28**
									(2.94)
Mothers Years of Educ. (Children)	10.9	3.36	11.1	3.38	9.69	4.19	9.60	4.05	0.35
									(1.15)
Current. Lives in Birth Prov. (Adults)	0.45	0.50	0.45	0.50	0.40	0.49	0.38	0.49	0.014
									(1.35)
Indigenous Language (Adults)	0.099	0.30	0.088	0.28	0.13	0.34	0.13	0.34	-0.0083
									(-1.18)
HH Asset Index (Children)	1.00	0.68	0.80	0.64	0.60	0.90	0.21	0.91	0.19**
									(2.81)
Observations (Adults)	4388		5347		7013		9176		
Observations (Children)	255		695		4558		9176		
Observations (Hospitals)	10210		12332		55136		65773		

Adult data from ENAHO (2007-2011), child data from ENDES (2007-2011) and hospital admissions from administrative data. Adults older than 13 and children under 6 living in coastal regions are included. All health outcomes excluding "Log Medical Expenditure" and counts of hospital admissions are binary. Medical expenditure is measured in Peruvian Soles. Post-reform refers to the 2009 ITQ reform, which began on April 20th, 2009 in the North/Central region and July 7th, 2009 in the South.

TABLE 2.3
IMPACT OF FISHMEAL PRODUCTION ON HEALTH

	Hospitals	Adults		Children: ≤ 5	
	Respiratory Admissions	Any Health Issue	Log Medical Expenditure	Any Health Issue	Cough
	Log Fishmeal Production in Last 30 Days				
Log Fishmeal Prod. in Last 30 Days	-2.340^{***}	0.010^{***}	0.006	0.002	0.000
	(0.555)	(0.003)	(0.014)	(0.009)	(0.010)
Log Fishmeal Prod. in Last 30 Days x Near Plant	3.952^{**}	0.019^{***}	0.092^{**}	0.014	0.014
	(1.591)	(0.006)	(0.043)	(0.028)	(0.029)
	Log Fishmeal Production in Last 90 Days				
Log Fishmeal Prod. in Last 90 Days	-1.800^{***}	0.006^{**}	0.017	-0.001	-0.005
	(0.483)	(0.003)	(0.014)	(0.007)	(0.007)
Log Fishmeal Prod. in Last 90 Days x Near Plant	4.374^{**}	0.010^{*}	0.073^{**}	0.041^{***}	0.039^{**}
	(2.047)	(0.006)	(0.033)	(0.015)	(0.019)
	Production Days in Last 30 Days				
Production Days in Last 30 Days	-0.268^{***}	0.001^{***}	0.001	0.000	0.000
	(0.066)	(0.000)	(0.002)	(0.001)	(0.001)
Production Days in Last 30 Days x Near Plant	0.228	0.003^{***}	0.010^{**}	0.000	0.000
	(0.174)	(0.001)	(0.005)	(0.003)	(0.003)
	Production Days in Last 90 Days				
Production Days in Last 90 Days	-0.172^{***}	0.000^{**}	0.000	-0.000	-0.001^{**}
	(0.038)	(0.000)	(0.001)	(0.000)	(0.000)
Production Days in Last 90 Days x Near Plant	0.219^{*}	0.001^{**}	0.006^{***}	0.004^{***}	0.003^{**}
	(0.116)	(0.001)	(0.002)	(0.001)	(0.001)
Mean of Dep. Var.	161.6	0.59	3.71	0.45	0.37
N	141981	161773	161806	14684	14678
Hospital/Centro Poblado/District FEs	Yes	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes	Yes
HH Controls	No	Yes	Yes	Yes	Yes

OLS regressions. Hospital admissions measure total monthly admissions at the hospital level. Adult data includes those over 13 years of age living in costal regions sampled in ENAHO (2007-2011), child data includes those under 6 years old living in coastal regions sampled in ENDES (2007-2011). Last 30 or 90 days is calculated as last 1 or 3 months for hospital data. "Near Plant" is defined as 5 kilometers for survey data and 20 kilometers for hospital data. All specifications include a dummy variable for living near a plant. Adult regressions include controls for age, gender, native language and level of education. Child regressions include controls for age, gender, household assets and mother's level of education. Hospital, adult and child specifications include hospital, Centro Poblado and district fixed effects respectively, with standard errors clustered at the same level. A "Production Day" is defined by $>$ 1000 MTs of input at the port level. Fishmeal production is based on daily inputs of fish, measured in 10,000s of MTs. "Respiratory Admissions" is a count, medical expenditure is measured in Peruvian Soles and all other dependent variables are binary. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.4

Impact of Fishmeal Industry on Health Before and After 2009 ITQ Reform

	Hospitals	Adults		Children: ≤ 5	
	Respiratory Admissions	Any Health Issue	Log Medical Expenditure	Any Health Issue	Cough
	Baseline (2008-2009)				
Post-Reform x Near Plant	12.239**	0.059**	0.239*	0.184**	0.146
	(5.245)	(0.027)	(0.140)	(0.092)	(0.090)
Mean of Dep. Var.	170.5	0.57	3.70	0.45	0.37
N	57554	62158	62167	6602	6599
	Treatment/Control Specific Time Trends				
Post-Reform x Near Plant	19.483***	0.061*	0.198	0.241**	0.206*
	(6.364)	(0.033)	(0.174)	(0.116)	(0.121)
Mean of Dep. Var.	170.5	0.57	3.70	0.45	0.37
N	57554	62158	62167	6602	6599
	Centro Poblado Specific Time Trends				
Post-Reform x Near Plant	1.417	0.066***	0.243*	0.280***	0.346***
	(7.908)	(0.025)	(0.135)	(0.082)	(0.083)
Mean of Dep. Var.	133.2	0.57	3.70	0.43	0.36
N	48631	62158	62167	4785	4782
	Sample Explanded to 2007-2010				
Post-Reform x Near Plant	9.681*	0.056***	0.181**	0.099***	0.083**
	(5.408)	(0.018)	(0.084)	(0.036)	(0.038)
Mean of Dep. Var.	167.2	0.58	3.68	0.46	0.37
N	114755	125084	125106	11112	11107
	Sample Restricted to First Season of 2008 and 2009				
Post-Reform x Near Plant	17.136***	0.093***	0.317*	0.288***	0.260***
	(5.839)	(0.028)	(0.168)	(0.074)	(0.096)
Mean of Dep. Var.	188.7	0.57	3.73	0.46	0.38
N	28776	31504	31510	5059	5059
	Sample Restricted to Within 50 Kilometers of Port				
Post-Reform x Near Plant	10.319*	0.023	0.155	0.189**	0.167**
	(6.018)	(0.027)	(0.145)	(0.084)	(0.073)
Mean of Dep. Var.	279.8	0.55	3.99	0.46	0.39
N	18620	29042	29049	2450	2448
Hospital/Centro Poblado/District FEs	Yes	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes	Yes
HH Controls	No	Yes	Yes	Yes	Yes

OLS regressions. Hospital admissions measure total monthly admissions at the hospital level. Adult data includes those over 13 years of age living in costal regions sampled in ENAHO (2007-2010), child data includes those under 6 years old living in coastal regions sampled in ENDES (2007-2010). The reform began on April 20th, 2009 in the North/Central region and July 7th, 2009 in the South. All specifications include a dummy variable for living near a plant. Time trends refers to the inclusion of a treatment or Centro Poblado specific monthly linear trend. Adult regressions include controls for age, gender, native language and level of education. Child regressions include controls for age, gender, household assets and mother's level of education. Hospital, adult and child specifications include hospital, Centro Poblado and district fixed effects respectively, with standard errors clustered at the same level. "Respiratory Admissions" is a count, medical expenditure is measured in Peruvian Soles, all other dependent variables are binary. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 2.5
IMPACT OF FISHMEAL INDUSTRY ON HEALTH BEFORE AND AFTER 2009 ITQ REFORM – CONTROLLING FOR PRODUCTION

	Hospitals	Adults		Children: ≤ 5	
	Respiratory Admissions	Any Health Issue	Log Medical Expenditure	Any Health Issue	Cough
	Controlling for Log Production in Last 30 Days				
Post-Reform x Near Plant	11.389**	0.052**	0.223	0.188**	0.150*
	(5.302)	(0.026)	(0.144)	(0.081)	(0.087)
Mean of Dep. Var.	171.2	0.57	3.70	0.45	0.37
N	57035	62158	62167	6602	6599
	Controlling for Log Production in Last 90 Days				
Post-Reform x Near Plant	11.519**	0.052**	0.241*	0.222***	0.178**
	(5.357)	(0.025)	(0.140)	(0.063)	(0.080)
Mean of Dep. Var.	171.2	0.57	3.70	0.45	0.37
N	57035	62158	62167	6602	6599
	Controlling for Log Seasonal Production				
Post-Reform x Near Plant	7.880	0.059**	0.212	0.216***	0.172**
	(5.762)	(0.027)	(0.141)	(0.059)	(0.068)
Mean of Dep. Var.	171.2	0.57	3.70	0.45	0.37
N	57035	62158	62167	6602	6599
	Controlling for Levels of Seasonal Production				
Post-Reform x Near Plant	11.225**	0.061**	0.257*	0.192***	0.144**
	(5.512)	(0.027)	(0.141)	(0.056)	(0.059)
Mean of Dep. Var.	171.2	0.57	3.70	0.45	0.37
N	57035	62158	62167	6602	6599
	Sample Restricted to Lima				
Post-Reform x Near Plant	11.406	−0.010	0.238		
	(8.554)	(0.061)	(0.535)		
Mean of Dep. Var.	328.5	0.52	4.17		
N	10420	17227	17234		
Hospital/Centro Poblado/District FEs	Yes	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes	Yes
HH Controls	No	Yes	Yes	Yes	Yes

OLS regressions. Hospital admissions measure total monthly admissions at the hospital level. Adult data includes those over 13 years of age living in costal regions sampled in ENAHO (2007-2010), child data includes those under 6 years old living in coastal regions sampled in ENDES (2007-2010). The reform began on April 20th, 2009 in the North/Central region and July 7th, 2009 in the South. All specifications include a dummy variable for living near a plant. Time trends refers to the inclusion of a treatment or Centro Poblado specific monthly linear trend. Adult regressions include controls for age, gender, native language and level of education. Child regressions include controls for age, gender, household assets and mother's level of education. Hospital, adult and child specifications include hospital, Centro Poblado and district fixed effects respectively, with standard errors clustered at the same level. "Respiratory Admissions" is a count, medical expenditure is measured in Peruvian Soles, all other dependent variables are binary. Fishmeal production is based on daily inputs of fish, measured in 10,000s of MTs. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 2.6
IMPACT OF FISHMEAL INDUSTRY ON LABOR MARKET OUTCOMES
BEFORE AND AFTER 2009 ITQ REFORM – BY JOB CATEGORY

	Panel A: All Adults			
	Has Any Job	Has 2nd Job	Total Labor Hours	Log. Total Income
Post-Reform x Near Plant	0.023	−0.001	−0.111	−0.675
	(0.020)	(0.015)	(0.110)	(0.973)
Mean of Dep. Var.	0.63	0.10	3.44	30.3
N	62104	62104	62104	62104
	Panel B: Non-Fishing Workers			
	Has Any Job	Has 2nd Job	Total Labor Hours	Log. Total Income
Post-Reform x Near Plant	0.022	−0.002	−0.110	−0.148
	(0.022)	(0.014)	(0.127)	(1.067)
Mean of Dep. Var.	0.62	0.10	3.40	30.0
N	60832	60832	60832	60832
	Panel C: Fishing Workers			
	Has Any Job	Has 2nd Job	Total Labor Hours	Log. Total Income
Post-Reform x Near Plant	0.097***	0.085	0.453	−3.334
	(0.036)	(0.090)	(0.330)	(6.480)
Mean of Dep. Var.	0.93	0.12	5.67	43.8
N	1272	1272	1272	1272
Hospital/Centro Poblado FEs	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes
HH Controls	Yes	Yes	Yes	Yes

OLS regressions. Data from ENAHO (2007-2011). Adults older than 13 living in coastal regions are included. All specifications include a dummy variable for living within 5 kilometers of a port and controls for age, gender, native language and level of education. Standard errors, clustered at the Centro Poblado level, are included in parentheses. A "Production Day" is defined by $>$ 1000 MTs of input at the port level. Fishmeal production is based on daily inputs of fish, measured in 10,000s of MTs. Total income is measured in Peruvian Soles. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. Labor categories are based on 3 digit job codes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 2.7
IMPACT OF FISHMEAL PRODUCTION ON HEALTH - PRODUCTION INSTRUMENTED BY REFORM – 2008 AND 2009

	First Stage					
	Hospitals		Adults		Children: ≤ 5	
	Production Days In Last		Production Days In Last		Production Days In Last	
	30 Days	90 Days	30 Days	90 Days	30 Days	90 Days
Post-Reform x Near Plant	3.705***	8.572***	5.048***	9.548***	0.415	-2.385
	(0.108)	(0.267)	(1.174)	(2.842)	(0.516)	(4.792)
Mean of Dep. Var. (Near Plant)	5.06	14.4	5.83	14.4	1.87	12.3
N	57035	57035	62167	62167	6755	6755
Hospital/Centro Poblado/District FEs	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes	Yes	Yes
HH Controls	No	Yes	Yes	Yes	Yes	Yes

	Second Stage - Production in Last 30 Days				
	Hospitals	Adults		Children: ≤ 5	
	Respiratory Admissions	Any Health Issue	Log Medical Expenditure	Any Health Issue	Cough
Production Days in Last 30 Days x Near Plant	3.061**	0.011**	0.051*	0.602	0.489
	(1.380)	(0.004)	(0.026)	(0.955)	(0.867)

	Second Stage - Production in Last 90 Days				
	Hospitals	Adults		Children: ≤ 5	
	Respiratory Admissions	Any Health Issue	Log Medical Expenditure	Any Health Issue	Cough
Production Days in Last 90 Days x Near Plant	1.349**	0.006**	0.027*	-0.068	-0.054
	(0.601)	(0.002)	(0.015)	(0.146)	(0.115)
Mean of Dep. Var.	171.2	0.57	3.70	0.45	0.37
N	57035	62154	62163	6600	6597
Hospital/Centro Poblado/District FEs	Yes	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes	Yes
HH Controls	No	Yes	Yes	Yes	Yes

Hospital admissions measure total monthly admissions at the hospital level. Adult data includes those over 13 years of age living in costal regions sampled in ENAHO (2007-2011), child data includes those under 6 years old living in coastal regions sampled in ENDES (2007-2011). Last 30 or 90 days is calculated as last 1 or 3 months for hospital data. The top panel shows first stage regressions of production days in the last 30 or 90 days interacted with "Near Plant", on an indicator for the post reform period interacted with "Near Plant." The bottom panels show second stage IV regressions of health outcomes on production days interacted with "Near Plant" instrumented by the post reform period interacted with Near Plant." All specifications also include a dummy variable for "Near Plant," which is defined as 5 kilometers for survey data and 20 kilometers for hospital data. Adult regressions include controls for age, gender, native language and level of education. Child regressions include controls for age, gender, household assets and mother's level of education. Hospital, adult and child specifications include hospital, Centro Poblado and district fixed effects respectively, with standard errors clustered at the same level. A "Production Day" is defined by $>$ 1000 MTs of input at the port level. "Respiratory Admissions" is a count, medical expenditure is measured in Peruvian Soles and all other dependent variables are binary. The reform began on April 20th, 2009 in the North/Central region and July 7th, 2009 in the South. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 2.8
IMPACT OF FISHMEAL INDUSTRY ON HEALTH BEFORE AND AFTER 2009
ITQ REFORM – NORTH VS. SOUTH AND EFFICIENT VS. INEFFICIENT PORTS

	Hospitals	Adults	
	Respiratory Admissions	Any Health Issue	Log Medical Expenditure
		North vs. South	
Post-Reform x Near Plant	-15.472	-0.080	-0.315^{*}
	(11.603)	(0.054)	(0.178)
North/Central Region x Post-Reform	-20.047^{***}	0.040^{**}	-0.263^{*}
	(3.399)	(0.019)	(0.146)
North/Central Region x Post-Reform x Near Plant	31.151^{**}	0.134^{**}	0.547^{**}
	(12.976)	(0.055)	(0.221)
p-value (Row 1+Row 3=0)	0.182	0.051	0.152
Mean of Dep. Var.	169.8	0.56	3.73
N	56570	58143	58152
		Efficient vs. Inefficient Ports	
Post-Reform x Near Plant	-2.135	-0.072	-0.330
	(22.528)	(0.055)	(0.350)
Pre-Reform Max. Efficiency x Post-Reform	-49.622^{***}	-0.016	-1.333^{***}
	(12.454)	(0.068)	(0.479)
Pre-Reform Max. Efficiency x Post-Reform x Near Plant	56.634	0.356^{***}	1.802^{**}
	(85.399)	(0.129)	(0.813)
p-value (Row 1+Row 3=0)	0.392	0.001	0.005
Mean of Dep. Var.	172.3	0.56	3.74
N	54323	57250	57259
Hospital/Centro Poblado FEs	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes
HH Controls	No	Yes	Yes

OLS regressions. Hospital admissions measure total monthly admissions at the hospital level, limited to 2008/2009. Adult data includes those over 13 years of age living in costal regions sampled in ENAHO (2008-2009). The reform began on April 20th, 2009 in the North/Central region and July 7th, 2009 in the South. All specifications include a dummy variable for living near a plant. Adult regressions include controls for age, gender, native language and level of education. Children are excluded due to a lack of observations in Southern ports. Hospital and adult specifications include hospital and Centro Poblado fixed effects respectively, with standard errors clustered at the same level. "Respiratory Admissions" is a count, medical expenditure is measured in Peruvian Soles, all other dependent variables are binary. The port of Ilo is excluded from both specifications due to production outside of designated seasons. Efficiency is determined by the maximum 2008 output/input ratio for any plant within the port. Efficiency is included as a continuous variable interacted with both living near a plant and post-reform. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 2.9
IMPACT OF FISHMEAL PRODUCTION ON HEALTH THROUGH AIR POLLUTION IN LIMA

	Port Level Correlation Between Fishmeal Production and Air Pollution			
	PM^{10}	$PM^{2.5}$	NO_2	SO_2
Log Fishmeal Prod. in Last 30 Days	1.631*** (0.284)	1.418*** (0.202)	0.328** (0.140)	0.536*** (0.150)
Mean of Dep. Var.	77.9	45.1	25.2	19.2
N	1231	1414	1416	1416
Month x Year FEs	Yes	Yes	Yes	Yes

	Impact of Air Pollution Instrumented by Fishmeal Production on Health		
	Hospitals	Adults	
	Respiratory Admissions	Any Health Issue	Log Medical Expenditure
		PM10	
Avg. PM^{10} Level in Last 30 Days x Near Plant	0.260 (0.526)	0.001*** (0.000)	−0.001 (0.001)
		PM2.5	
Avg. $PM^{2.5}$ Level in Last 30 Days x Near Plant	0.889** (0.434)	0.001*** (0.000)	−0.000 (0.001)
		NO_2	
Avg. NO_2 Level in Last 30 Days x Near Plant	3.699** (1.808)	0.002*** (0.000)	−0.000 (0.001)
		SO_2	
Avg. SO_2 Level in Last 30 Days x Near Plant	5.325** (2.602)	0.003*** (0.001)	−0.000 (0.002)
Mean of Dep. Var.	329.2	0.54	4.11
N	19976	33570	33583
Month x Near Plant FEs	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes
HH Controls	Yes	Yes	Yes

Hospital admissions measure total monthly admissions at the hospital level for hospitals whose closest port is Callao. Adult data includes those over 13 years of age whose closest port is Callao sampled in ENAHO (2007-2011). The top panel presents pollutant levels regressed on "Log Fishmeal Production" and month fixed effects. The bottom panel presents IV regressions of health outcomes on average pollutant levels in the last 30 days and average pollutant level in the last 30 days interacted with an indicator for "Near Plant" instrumented by "Log Fish Capture in Last 30 Days" and "Log Fish Capture in Last 30 Days × Near Plant." All pollutants are measured in $\mu g/m^3$. Daily pollutant levels are inverse distance weighted averages of readings at 5 pollution stations in Lima. Missing values at individual stations were imputed using the following technique: (i) construct the empirical distributions for each of the five stations. (ii) On days that data is missing, find the value of the empirical distribution on that day for each of the other stations. (iii) Take the inverse distance weighted average of those values. (iv) Replace the missing data for the station with the concentration corresponding to the point in the empirical distribution found in (iii). Outcomes for children are excluded due to a lack of observations near the port of Callao. Last 30 days refers to the calendar month for hospital data and to the 30 days preceding the survey date for survey data. "Near Plant" is defined as 5 kilometers for survey data and 20 kilometers for hospital data. All specifications include a dummy variable for living near a plant. Adult regressions include controls for age, gender, native language and level of education. Hospital and adult specifications include hospital and Centro Poblado fixed effects respectively, with standard errors clustered at the same level. A "Production Day" is defined by $>$ 1000 MTs of input at the port level. Fishmeal production is based on daily inputs of fish, measured in 10,000s of MTs. Medical expenditure is measured in Peruvian Soles, all other dependent variables are binary. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 2.10
COST BENEFIT ANALYSIS OF 2009 ITQ REFORM

Panel A: Increase in Sector Profits		
Increase in net income for listed companies (USD)	$58,526,966	
Estimated sector wide increase in net income (USD)	$219,237,448	
Panel B: Health Costs		
Medical Expenditures:		
Estimated increase per person/year	$38	
Estimated total increase (USD)	$45,523,379	
Respiratory Hospital Admissions:		
Estimated increase in total hospital admissions	55,516	
Estimated increase in years lived with disability (YLDs)	5,681	
Estimated cost of years lived with disability (YLDs) A:	$297,455,874	(Leon and Miguel)
Estimated cost of years lived with disability (YLDs) B:	$128,097,109	(US EPA)
Panel C: Total Costs and Benefits		
Estimated benefit to sector (USD)		$219,237,448
Estimated total cost A: (medical exp. + cost of YLDs)		$342,979,253
Estimated total cost B: (medical exp. + cost of YLDs)		$173,620,488

Net income from public available firm financials, calendarized for April-April fiscal years. Sector wide estimates based on 2008 proportion of fishmeal production represented by publicly listed firms. Population estimates are based on total 2009 population living in locations with fishmeal plants from the Peru Institute of National Statistics and Information. Medical expenditure is annualized and extrapolated to the population based on estimates in Table 2.4. Disability weights translate health conditions over a given duration into an equivalent number of years lived with disability (YLDs). We estimate YLDs using the average disability weight for respiratory diseases (from the Global Burden of Disease Study 2010), and assume a total duration per disease episode of one year. VSL (value of statistical life) estimates for Peru are estimated as $5.42 million, based on an African VSL of $577,000 (from Leon and Miguel (2015)), scaled to Peru GNI using the elasticity in Hall and Jones (2007). We calculate the value of a statistical life year by dividing our VSL estimates by the average life expectancy in the relevant population (40.88, based on remaining life expectancy in Peru for the average individual experiencing a respiratory disease). We alternatively conduct our calculation using a United States VSL estimate of $7.87 million, per US EPA recommendations, again scaled by GNI. All numbers reported are in 2009 USD, calculated using the USA BLS inflation calculator. Scalings use World Bank estimates of GNI per capita (PPP).

Chapter 3

The Heavy Costs of High Bail: Evidence From Judge Randomization

3.1 Introduction

Roughly 450,000 people in the United States are held in jail awaiting trial on any given day.[1] These individuals have not been convicted of a crime and are presumed to be innocent of the charges for which they have been jailed. For the majority of defendants the barrier to release is financial: they are unable or unwilling to post bail. Due to limited judicial resources, defendants often remain incarcerated for months or years awaiting trial.[2] Many defendants who are detained on money bail before trial eventually choose to plead guilty in exchange for release, rather than risking continued detention or an uncertain trial outcome.

There is significant evidence of a correlation between pretrial detention and both conviction and recidivism, consistent with a direct impact of bail assessment on defendant outcomes (for instance, Lowenkamp, VanNostrand and Holsinger (2013*a*), Lowenkamp, VanNostrand and Holsinger (2013*b*), Phillips (2007), and Phillips (2008)). However, prior research has struggled with causally estimating the impact of money bail due to the endogenous nature of detention hearings.[3] When judges determine whether to release an arrestee and the conditions of such release, they consider, among other things, the facts of the case, the strength of the evidence, and the arrestee's criminal history, ties to the local community, and financial resources. These factors may be related to factual guilt and render correlations between money bail assessments and outcomes like convictions and recidivism difficult to interpret.

This paper investigates the causal impact of money bail on convictions and recidivism using comprehensive court data from the two largest cities in Pennsylvania: Philadelphia and Pittsburgh. By money bail, we refer to the requirement that criminal defendants post a cash amount as bail in exchange for freedom before trial.[4] In Philadelphia, defendants are assigned bail at a centralized, 24-hour-a-day court presided over by arraignment court magistrates, whom we refer to as judges for convenience. These judges differ in what we call severity, or the propensity to assess bail. All else being equal, some judges assess money bail frequently, while others do so sparingly.

[1] See `http://www.bjs.gov/content/pub/pdf/jim14.pdf`.

[2] In our data, the median time between bail arraignment and trial is 200 days.

[3] A notable exception is Abrams and Rohlfs (2011) who exploit an experiment in Philadelphia in the 1980s.

[4] Other forms of bail may require non-monetary conditions, or only require the defendant to pay in the event of a non-appearance.

The Philadelphia system assigns defendants to bail judges in an effectively random manner, creating a natural experiment that we exploit to determine the role of money bail in determining defendant outcomes. We document that defendants' assignment to more severe judges raises the probability of being assessed money bail for reasons unrelated to other case factors, including defendant characteristics. This natural experiment allows us to then study the implications of effectively exogenous impositions of money bail on further defendant outcomes.

We find that the assessment of money bail is a significant, independent cause of convictions and recidivism. In Philadelphia, criminal defendants who are assessed money bail are 12% (6 percentage points) more likely to be convicted. These effects appear to be driven by the subset of cases where arrestees are detained due to their inability to post bail. We also investigate money bail assessment and outcomes in Pittsburgh, where judicial assignment is based on arrest location, and find similar results. We combine the Philadelphia and Pittsburgh samples to gain statistical precision in examining the lasting negative effects of money bail after the conclusion of the underlying criminal case. We document that the assessment of money bail increases recidivism in our sample period by 6-9% yearly (0.7 percentage points).

Our results are primarily driven by whether money bail is required and not by the amount of money bail. In other words, the assessment of money bail, rather than the bail size, appears to cause convictions. A key implication of this finding is that simply lowering required bail amounts will not ameliorate harms imposed by money bail. Our findings persist among a number of subgroups—non-white defendants, those assigned a public defender, and male defendants. We find estimates that are even larger among defendants charged with felonies, though we do not reach statistical significance in that sample. This suggests that our effects are not merely driven by convictions for petty crimes.

We do not attempt to isolate the exact channel by which money bail causes convictions and recidivism. Money bail, as a source of pretrial detention, imposes significant costs on defendants. As the Supreme Court wrote in *Gerstein v. Pugh* (420 U.S. 103, 114 [1975]) pretrial detention "may imperil the suspect's job, interrupt his source of income, . . . impair his family relationships [and affect his] ability to assist in preparation of his defense." Many defendants who are detained on money bail before trial may consequently choose to plead guilty to avoid or minimize further detention. Prosecutors commonly offer detained defendants a plea of "time-served," where defendants will receive credit for time already spent in detention and will therefore be released immediately upon conviction. Other potential channels include the difficulty detained defendants have communicating with their counsel and properly preparing a defense; changes in behavior among various institutional actors such as prosecutors, defense attorneys, judges, and jurors toward defendants who are incarcerated pretrial; the limited opportunity for detained arrestees to participate in diversionary programs and other resolutions not resulting in convictions; and the financial strain of making bail.[5] Money bail may also directly influence recidivism through the harms of pretrial incarceration imposed upon those those unable to make bail, post-trial incarceration following conviction, or the stigma of conviction.[6]

Despite the multiplicity of possible channels, we emphasize that our results provide novel evidence of a causal role of money bail and pretrial detention on defendant outcomes. The relationship between money bail, conviction, and recidivism suggests a strong interaction between poverty and the criminal justice system. A large literature has examined the credit constraints facing American households that make even small money bail amounts difficult to post (see Lusardi, Schneider and Tufano (2011)). While it is feasible that money bail could impact convictions among those with sufficient liquid assets to post bail, it is more likely that these effects come primarily from the credit-constrained. It is important to note that a large majority of arrestees in our sample qualified for representation by the public defender, and therefore are presumably indigent.

The interactions between money bail and subsequent defendant outcomes pose substantive legal issues. From a liberty perspective, these relate to the incarceration of presumptively innocent people and the basic assumption that convictions reflect only the merits of the underlying case. Bail also raises equality issues related to the requirement of equal access to justice and the prohibition against wealth discrimination. Racial is a further concern, and we find evidence consistent with racial discrimination in bail setting: non-white defendants are more likely to be assessed money bail, yet less likely to be found guilty. However, this correlation is suggestive, and may reflect unobserved factors that are correlated with race.

Our findings also raise institutional design questions regarding the American money bail sys-

[5]Though bail bondsmen can front bail amounts in exchange for a collateral value which is typically 10%, even these relatively smaller collateral values may be out of reach for criminal defendants facing liquidity constraints. In Philadelphia, the court may accept 10% of the bail amount.

[6]See, e.g., Baylor (2015); Appleman (2012); Phillips (2008).

tem as a whole. The money bail system in Philadelphia and Pittsburgh has a lot in common with the money bail systems used in many cities around the country, such as New York and Baltimore. Arrestees see a judicial officer who determines whether to release a person pending trial or impose money bail. Those people who are unable to pay their bail have the opportunity to plead guilty or remain in jail until trial. In systems to the one in Philadelphia and Pittsburgh, our research suggests that money bail causes convictions and recidivism.[7]

One suggested solution to the perceived inequities of pretrial detention is the adoption of empirical pretrial risk assessments. Such tools, based on multivariate models built from large sets of defendant data, create recommendations for release or conditions of release. Despite the use of such assessment tools in Philadelphia and Pittsburgh in the time period covered by our analysis, judges varied widely in assessing bail amounts for similar defendants, calling into question the ability of such tools to rein in judicial discretion.

To contextualize our findings on guilt and recidivism, we examine whether the assessment of money bail induces defendants to appear at trial, the stated purpose of the money bail system. As we are unable to explicitly observe defendants failing to appear, we construct two proxies based on the issuance of bench warrants. While these proxies are imperfect—both likely understate the true number of failures to appear—we find no evidence that money bail increases the probability of appearance. These results should be interpreted as preliminary, and a more nuanced study of court appearances using more complete date is necessary. Nevertheless it is notable that we are unable to find an obvious impact of money bail. Pretrial detention is expensive. Philadelphia spent an estimated $290 million on jailing in 2009, and 57% of the daily jailed population was detained awaiting trial (Eichel, 2010). Rationalizing the costs imposed by money bail (via detention costs, convictions and recidivism) requires substantial compensating public benefits, and we find no evidence that such benefits exist.

Our research has a close connection to the literature on pretrial justice.[8] There is a large body of evidence suggesting that pretrial custody status is associated with the ultimate outcomes of

[7] Of course, the impact may differ depending on the population. For instance, in certain places, defendants may be relatively well-off and have the general ability to pay money bail. In such a place, we would expect that the causal impact of money bail would be lower than in Philadelphia, where many people are too poor to pay their bail.

[8] The Pretrial Justice Institute has created an exceptionally detailed bibliography, available at: `http://www.pretrial.org/wpfb-file/pji-pretrial-bibliography-pdf/`.

criminal cases, with detained defendants consistently faring worse than defendants at liberty (See ABA Standards for Criminal Justice: Pretrial Release 29 (3d. ed. 2007)). Past work has uncovered the correlation between money bail, pretrial detention, and conviction (e.g. Phillips (2007), Phillips (2008)), and examined other policy considerations regarding the design of pretrial detention systems (See Lowenkamp, VanNostrand and Holsinger (2013*a*), Lowenkamp, VanNostrand and Holsinger (2013*b*), Bechtel et al. (2012), and Phillips (2012)).

In the economics literature, beyond Abrams and Rohlfs (2011) and Helland and Tabbarok (2004), our work is most closely related to papers utilizing random assignment of judges within the criminal justice system such as Kling (2006), Doyle Jr (2007), Doyle Jr (2008), Mueller-Smith (2016) and Aizer and Doyle (2015), as well as in other contexts, such as Chang and Schoar (2007), and Dobbie and Song (2015). Especially relevant is concurrent and complementary work by Stevenson (2016), which uses a similar approach in Pennsylvania to examine the impacts of pretrial detention on case outcomes. Our work differs in that we also examine recidivism and establish a long-term negative outcome of incarcerations spells. We also differ in that our approach focuses on the decision of judges to set money bail, rather than the detention status of defendants.[9]

Our paper is structured as follows: Section 2 presents legal background on the money bail system in Philadelphia and Pittsburgh, Section 3 explains our data and empirical strategy, Section 4 contains estimation results, and Section 5 concludes.

3.2 Legal Background and Bail Hearings

3.2.1 Legal Background

Any person who is arrested without a warrant is entitled to a hearing within 48 hours of arrest, see *Cnty. of Riverside v. McLaughlin* (500 U.S. 44, 56 [1991]) and (*Gerstein*, 420 U.S. at 114). At this hearing, a judicial officer must determine whether there is probable cause for the arrest prior to the imposition of “any significant pretrial restraint of liberty.” (*Gerstein*, 420 U.S. at 125). Across the country, this initial appearance has evolved into a “hearing at which the magistrate informs

[9] In principle, guilty pleas may be affected by bail setting even when bail is posted due to the financial cost of making bail. Our Table 5 examines the consequence of bail setting on the full interaction of outcomes of pre-trial detention and case guilty.

the defendant of the charge in the complaint, and of various rights in further proceedings, and determines the conditions for pretrial release." *Rothgery v. Gillespie Cnty, Tex* (554 U.S. 191, 199 [2008]).

At a bail hearing, judges have a number of options available to them:

1. *Release on Recognizance (ROR)* — Requires the defendant only to agree to appear at a later date

2. *Non-Monetary Conditions* — Allows some non-monetary restriction to be placed on the defendant, such as pretrial supervision or a curfew

3. *Unsecured Monetary Condition* — Written agreement to be liable for a fixed financial payment, akin to a promissory note.

4. *Secured Monetary Condition* — Defendant must satisfy a financial condition paid to the court either directly, through a bail bondsman, or other collateral such as real property, in order to secure release

5. *No Bail* — Defendant is to be held pending trial

A variety of constitutional and legal protections constrain the discretion of judicial officers in determining whether to detain or release a defendant and what conditions to place on such release. First, pretrial liberty is a fundamental right independently guaranteed by the Constitution. *See Foucha v. Louisiana* (504 U.S. 71, 80 [1992]); *U.S. v. Salerno* (481 U.S. 739, 750 [1987]). "In our society liberty is the norm, and detention prior to trial or without trial is the carefully limited exception." (*Salerno*, 481 U.S. at 755). Therefore pretrial detention must be "narrowly focus[ed]" to the government's "compelling" interests in public safety and return to court. (*See Salerno*, 481 U.S. at 750-51) and *Stack v. Boyle* (342 U.S. 1, 4 [1951]); ABA Standards for Criminal Justice: Pretrial Release 37 (3d. ed. 2007). In determining whether to release a defendant, and what conditions to place on such release, the judicial officer must make an individualized assessment of the case and defendant. (*See Stack*, 342 U.S. at 5).

Bail also raises issues covered under the Equal Protection Clause of the Fourteenth Amendment to the Constitution, which has been interpreted to prohibit "punishing a person for his poverty." *Bearden v. Georgia* (461 U.S. 660, 671 (1983)). Persons may not be incarcerated solely due to their inability to make a payment. (*See Bearden*, 461 U.S. at 671) and *Tate v. Short* (401 U.S. 395 [1971]); *Williams v. Illinois* (399 U.S. 235 [1970]); and *Smith v. Bennett*, (365 U.S. 708, 709 [1961]). For this reason such payments must take into account a person's financial resources.

These guarantees find a statutory parallel in the Pennsylvania Rule of Criminal Procedure 523, which explicitly requires magistrates to consider arrestees' financial resources when setting money bail.

3.2.2 Bail Hearings

In Pennsylvania, a magistrate presides over the initial appearance of an arrestee. In Philadelphia particularly, a centralized bail court operates 24 hours a day. Defendants from across the city appear before one of a team of appointed magistrates who conduct the initial detention hearing. Magistrates generally preside via CCTV over satellite locations in the city where arrestees are held. The centralized location, high case load, constant process, and rotating magistrate calendar result in the effectively random assignment of defendants to magistrates (an assumption we test). Importantly for our purposes, magistrates in Philadelphia only preside over the initial appearance; they do not preside over subsequent hearings or trials. As a result, magistrates only impact the case at the bail assessment, and not at later stages.

In Pittsburgh, magistrates are elected to a six-year term to serve in a district court, which administers a particular geographic section of Allegheny county. A single magistrate handles the majority of the arrests that occur within their jurisdiction, although many arrestees are seen by other magistrates during weekends, nights, and other periods when the presiding magistrate is not in service. As a result, defendants in Pittsburgh are assigned to judges in part based on the location and time of their arrest.

At the pretrial detention hearing in both Pittsburgh and Philadelphia a magistrate hears information from the defendant (or the defendant's counsel) and the prosecutor relevant to the defendant's flight risk and public safety. This information includes the many factors set forth in Pennsylvania Rule of Criminal Procedure 523, such as: the nature of the offense, the strength of the evidence, the defendant's financial resources, family and community ties, criminal record, and prior failures to appear. These hearings typically last only a few minutes. In Philadelphia and Pittsburgh, magistrates also employ a empirical risk assessment tool, meant to standardize decisions regarding pretrial detention.[10]

Should money bail be set, detainees may only secure their release through the satisfaction of its financial terms. In Philadelphia, detainees may post 10% of the money bail amount directly to the court.

Detainees who cannot afford the financial condition of their release remain incarcerated for months or even years awaiting trial. Detainees have the opportunity to move for a reduction in their money bail after the initial hearing. We focus on the initial assessment of money bail, as it is the product of a randomized judicial decision, and find this decision is influential in determining the final amount the defendant is required to pay, regardless of later modifications.

The timeline of defendant actions around the release determination varies from state to state. In Pennsylvania, the detention hearing precedes the entry of the plea, ensuring that the magistrate's assessment of money bail is a factor in the defendant's plea decision from the beginning.

3.3 Data and Empirical Strategy

3.3.1 Data Summary

We obtained comprehensive criminal data from the Administrative Office of the Pennsylvania Courts for 2010–2015. These include records from the local magistrate courts as well as subsequent judicial and defendant decisions from the higher Court of Common Pleas. In Philadelphia, a separate municipal court system typically handles initial arraignments.

Table 1 summarizes the data for our focal region of Philadelphia, where we are best able to establish judicial randomization, as well as Pittsburgh—the second largest jurisdiction in the state. Our data contain information about the entire history of detention determinations and money bail assessments on criminal defendants (although we focus on the money bail amount resulting from the initial hearing); disposition information on the list of charged offenses; bench warrant information; and final sentencing outcomes for individual defendants. Our first appendix table, Table A1, contains the top 10 most common offenses and basic characteristics of the cases associated

[10] Although we find that these tools do not eliminate the exercise of wide judicial discretion.

with those offenses.

3.3.2 Empirical Strategy

A simple approach to addressing the role of money bail would be to run the OLS regression:

$$Guilt_{it} = \alpha + \beta Bail_{it} + \varepsilon_{it}$$

where $Bail_{it}$ is an indicator for whether or not individual i is assigned money bail in time t. Table 2 illustrates this strategy. Column 1 suggests that being assessed money bail results in a 1.4 percentage point increase in the probability of pleading guilty. As shown in column 3, this goes up to 4.3 percentage points after adding a battery of additional controls, including gender, race, age, and offense fixed effects. This relationship is confirmed in column 4, where we focus on the log of the bail amount instead of the indicator for money bail assessment. Figure 1 provides an illustration of this correlation for one offense: possession of marijuana. Defendants charged with this offense are substantially more likely to be found guilty when assessed money bail.

While these estimates are consistent with a causal interpretation that higher bail amounts induce convictions, they are also consistent with a spurious correlation resulting from the endogenous bail assessment. Recall that bail assessments are not made randomly, but are intended to be calibrated against the nature of the offense, the flight risk of the individual, and even the strength of the case. As these factors are also likely to be associated with the underlying guilt of the defendant, the results from Table 1 may not reflect a causal role of bail.

Concerns about the endogenous assignment of bail are heightened by the results shown in Panel A of Figure 2, which displays the coefficients from a regression of money bail on various covariates. While there is a raw univariate correlation with guilt, the assessment of money bail is also associated with gender, race, and prior cases. The correlation of money bail with these covariates is indicative of the endogenous initial assignment of money bail.

The goal of our empirical strategy is to address this endogeneity concern using the effectively random assignment of defendants to judges. Bail judges differ widely in how they treat similarly situated defendants. Some judges are far more likely to impose money bail, and to impose money bail in greater amounts, than other judges. In other words, certain judges over time tend to set bail when other judges would not, all else being equal. We refer to each judge's propensity to set money bail as the judge's severity. Therefore, a defendant's chances of receiving money bail depend on the severity of the bail judge, not just the characteristics of the case and the defendant. Because defendants are close to randomly assigned to bail judges, the judicial assignment serves as the treatment in a natural experiment. We isolate the effect of the severity of the bail judge in setting money bail to determine the role of money bail on defendant outcomes.

The coefficients plotted in Panel B of Figure 2 reflect our attempt to isolate the impact of random judicial assignment on guilt. This figure shows the relationship between a battery of covariates and the component of money bail that is due only to judicial severity. They are created by regressing several covariates on the linear prediction of money bail on a judicial severity measure described below. None of the covariates appear to be related to the fraction of variation in money bail that is driven by judicial variation, indicating random assignment. By contrast, our outcome variable of guilt *is* associated with our instrument—showing how the judicial assignment of bail can produce causal estimates of the impact of money bail.

Conceptually, our identification strategy is to isolate the impact of the judge on the probability that an individual is assigned money bail. One approach would be to use judge-specific fixed effects to instrument for whether a defendant is assigned bail. This would involve estimating a first stage, for individual i in court c with judge j, of:

$$Bail_{icjt} = \alpha + \gamma_c + \delta_j + v_{it}$$

and estimating the effect of $Bail_{icjt}$ on guilt in a second stage, where δ_j are judge fixed effects. However, the assumptions required for IV estimation via two-stage least squares may be violated in finite samples because of a mechanical correlation in the first stage. The estimated judge fixed effects are essentially an average across defendants, and with a small number of cases each defendant contributes significantly to the average. As discussed above, a defendant's own bail assessment is likely to be correlated with unobserved factors that are associated with guilt. If this is true, then averaging that bail assessment with a finite number of other defendants' assessments will not in general eliminate the correlation.

A solution to this problem in the literature (e.g., Dobbie and Song (2015)) involves estimating a leave-out mean for each defendant:

$$Z_{icjt} = \frac{1}{n_{cjt}-1}\left(\sum_{k=1}^{n_{cjt}}(Bail_k) - Bail_i\right) - \frac{1}{n_{ct}-1}\left(\sum_{k=1}^{n_{ct}}(Bail_k) - Bail_i\right)$$

which we refer to as judicial severity. The first term of Z_{icjt} is simply the average of $Bail_{kcjt}$ for all individuals faced by judge j except for i (all $k \neq i$). The second term subtracts out the average of $Bail_{kcjt}$ at the court c, once again omitting individual i. Intuitively, Z_{icjt} is simply judge j's average relative to the court's average, computed using everyone but i. Because Z_{icjt} is computed without using individual i, there is no mechanical correlation. This leave-out mean is then used as an instrument in place of judge fixed effects.

While the exposition above demonstrates a judge-level leave-out mean, our preferred instrument is slightly more granular. To account for possible non-random assignment by offense we compute a leave-out mean at the offense-judge level. That is, the average for a judge for a given offense type, relative to the court average for that offense. For this instrument, we need only assume that individuals of the same offense category are randomly assigned to judges. Our primary specifications depend on a version of the instrument in which $Bail_{it}$ is defined as the binary decision of whether to assign bail or not. However, we also examine alternative continuous measures, including $\log(1+$ bail amount$)$.

Panel A of Figure 3 illustrates our estimate of judicial severity against the log bail amount, showing that judge severity is highly predictive of bail amounts faced by criminal defendants[11]. Panel B shows that our judge severity measure is consistent over time, suggesting that judge severity is driven by idiosyncratic personal factors rather than temporary shocks or case characteristics (judge severity is even consistent across different offices when judges move to serve in other jurisdictions).

In our main specifications, we instrument for $Bail_{icto}$ with Z_{ictjo}, our measure of judge severity

[11] We measure judicial severity using a leave-out-mean of the log[1 + Money Bail Amount] at the judge-year level, relative to the leave-out-mean average at the court in the same year. These computed judicial measures are then regressed against individual measures of log bail with fixed effects for the month of arraignment. The resulting residuals are averaged at the judge-year level and the average log bail amount is added to each residuals. Panel A contrasts the averaged measure of judicial severity against average log bail amounts at the judge-year level. Panel B compares the averaged measure of judicial severity in one year against the same judge's measure the previous year.

taken from a within offense measure:

$$\begin{aligned} Guilt_{icto} &= \alpha + \beta Bail_{icto} + X'_{icto}\delta + \eta_{cto} + \varepsilon_{ictjo} \\ Bail_{icto} &= \alpha + \gamma Z_{ictjo} + X'_{icto}\zeta + \rho_{cto} + v_{ictjo} \end{aligned}$$

with errors clustered at the jurisdiction-judge-year level. Our identifying assumption, taken from judge randomization, is that:

$$\text{corr}(Z_{ictjo}, \varepsilon_{ictjo}) = 0.$$

In the next section, we provide supporting evidence for this assumption.

It is important to note that these results are created using an instrumental variable approach that focuses on criminal defendants induced to pay money bail as a result of judicial severity. In other words, we estimate a local average treatment effect (LATE) identified on the basis of individuals for whom changes in bail assessment resulting from variation in judicial severity impact guilty pleas. These defendants are more likely to represent criminal cases for which there is more scope for judicial variation in bail setting. Nevertheless, we do find that our results persist in a number of important subcategories (including defendants facing felonies), and our results are quite comparable in both Philadelphia and Pittsburgh. These checks suggest that our results have external validity outside of the precise jurisdictions we examine.

3.3.3 Randomization Check

Though our analysis of the judicial assignment process in Philadelphia leads us to expect close-to-random assignment of cases across judges, we check this assumption by examining the association between our leave-out-mean estimator and a series of defendant covariates in Table 3. Column 1 illustrates the means of the covariates we analyze. Column 2 regresses our instrument against each covariate in isolation with no additional controls and reports the coefficient. Column 3 regresses our instrument against all covariates and includes fixed effects for the most severe offense among the defendant's charges. Column 4 adds additional month-of-arraignment fixed effects.

Across all specifications, we find strong evidence for random assignment. F-statistics of the joint significance of covariates we test against our instrument are 0.54 with only month fixed effects and 0.34 when including both month fixed effects and offense controls.

3.4 Results

3.4.1 IV Results

Table 4 presents our main results from Philadelphia. The first column shows the first stage—a regression of our instrument of judicial severity against a binary indicator of whether the defendant was assessed money bail. While defendants are on average likely to receive money bail (62%), we find that judicial factors also play a large role. Our first stage suggests strong instrumental validity: being assigned to a more severe judge results in defendants facing a higher likelihood of being assessed money bail. Given the close-to-random assignment to judges and the lack of correlation between our instrument and observable defendant characteristics, we interpret this first stage as indicating that judicial severity provides effectively exogenous variation in money bail.

Column 2 presents the reduced form—a direct regression of our instrument of judge severity against the outcome of guilt. Although this relationship will be attenuated—because not all people who receive a severe judge are impacted by way of higher bail amounts—the strong and significant relationship in the reduced form indicates a causal relationship between judge severity and conviction.

The third column scales the reduced form by the first stage to produce our instrumental variables estimate of the relationship between money bail and conviction. Our estimate suggests that defendants who are required to pay money bail as a result of being assigned to a severe judge are 6 percentage points more likely be convicted. Given a baseline guilt level of 50% in our sample, our estimate suggests that the presence of money bail increases the likelihood that a defendant is found guilty by about 12%.

This estimate is large, tightly identified through our measure of judicial severity, and suggests a powerful role for money bail in inducing convictions. Our data do not permit complete analysis of whether convictions result from plea bargains or trials. However, we have strong results when focusing on cases in which we can explicitly observe plea behavior, and cases proceeding to trial appear in our sample only rarely. We believe our estimates are primarily driven by defendant plea behavior.

Table 4 also provides suggestive estimates regarding the role of race in case outcomes. Column 1 shows that non-white defendants are 1.4 percentage points more likely to be assessed money bail. However, columns 2 and 3 show that non-whites are *less* likely to be found guilty of crimes. While these results should not be interpreted causally, as they do not exploit judicial randomization and may reflect non-racial factors associated with race, they are consistent with racial bias in the criminal justice system. They are also consistent with other mechanisms of the legal process. For instance, prosecutors or judges may correct for an initial bias in arrest by dismissing or differentially pursuing cases involving non-white defendants. While our data do not permit a complete analysis of racial bias in bail setting, this remains an interesting avenue for future research.

We next detail the relationship between money bail, pretrial detention, and convictions. There are a number of paths a defendant may take following the initial bail assessment. We consider a categorization of four possible paths in a criminal case: defendants may be detained and found guilty, detained and found not guilty, released and found guilty, or released and found not guilty. Table 5 attempts to analyze the impact of money bail on the flow of defendants between these four categories. Each of the four columns presents an IV regression (as in column 3 of Table 2) with one of those categories as the dependent variable. As the dependent variables across the four columns are mutually exclusive and exhaustive, one of the columns is redundant, in the sense that the coefficients in any row must sum to 0. However, examining all four columns provides a useful picture of the impact of money bail on the path defendants take from bail assessment to their ultimate case outcome.

Since defendants not receiving money bail are presumptively released, we can assume that the imposition of money bail is unlikely to *increase* the number who are released. Fort this reason, we observe that the judicial assignment of money bail reduces the outcome of release both in column 2 (release and guilty) and column 4 (release and not guilty). Although we are not able to precisely estimate the effects in either column, Table 5 suggests that money bail decreases the probability that a defendant is released and ultimately found guilty by nearly 10 percentage points and decreases the probability that a defendant is released and found not guilty by nearly 8 percentage points.

The reduction in the released population must be matched by an increase in the detained population. Nearly the entire increase falls into the first column: we see a 16 percentage point increase in the outcome of detention and guilt. In other words, money bail increases the probability of detention for those who would be counterfactually released, and the majority of the population that is detained as a result of money bail is ultimately convicted.

3.4.2 Robustness

For robustness, we provide a number of additional checks. Table 6 explores our main IV specification as illustrated in column 3 of Table 4 for different subsamples—being charged with a felony, having a public defender, being male, and being non-white. While none of these estimates are statistically different from our main estimates, it is noteworthy given our findings on race discussed above that our IV point estimate for non-whites is higher, at 8.3 percentage points. Our finding on felonies, an 8.1 percentage point increase, is not precisely estimated but is high in magnitude and suggestive that instances of guilt induced by higher bail are not for low-level crimes exclusively. Being convicted of a felony typically results in severe long-term impacts on defendant outcomes, including opportunities for future employment and voting status.[12]

Tables 7 and 8 explore alternate specifications of our judge severity measure. Table 7 uses the log of 1 plus the bail amount, effectively using both the intensive and extensive margins. Table 8 uses the log of the bail amount, conditional on being assigned money bail—that is, only the intensive margin. In Philadelphia, we find no evidence that the intensive margin matters, only the extensive margin of being assessed money bail.

Next, we turn to Pittsburgh. As discussed in section 2, the nature of judicial assignment in Pittsburgh and the rest of the state is not as clean and does not permit a straightforward causal estimate. Rather than a central courtroom that handles all cases, individual magistrate judges are elected to districts in the city and are principally responsible for cases within that jurisdiction. Our judge measure therefore captures the variation arising from the difference between the principal judge and other judges, which account for 20–30% of cases in districts, typically due to the principal judge being absent on a weekend, night, vacation, or for some other reason. Our identifying assumption is that case loads, conditional on observables, do not differ between the principal

[12]Though convicted felons can vote in Pennsylvania.

judge and other judges in a given district.

A randomization check in Appendix Table A2 suggests that there is non-random judicial assignment in Pittsburgh, with an F-statistic of 4.74 for various defendant characteristics regressed against a measure of judicial severity within Allegheny County. Nonetheless, to establish robustness of our primary finding outside of the city of Philadelphia, we attempt a version of our main specification in Pittsburgh in Table 9. Remarkably, given the extent of non-random assignment, we find estimates that are virtually identical in Pittsburgh—in column 3, we see a 6.4 percentage point increase in guilt as a result of money bail assessment. Due to the comparability of the Pittsburgh and Philadelphia samples, in subsequent analysis on recidivism we combine the two samples in order to maximize statistical power.

Appendix Table A3 examines how our results vary across the distribution of bail amounts. To avoid an endogenous assignment of bail amounts, we first categorize offense categories according to the average bail amount into quartiles. Next, we estimate our main IV analysis within each quartile of bail assessment. Interestingly, we find that our results appear to be largest in the first quartile, where bail amounts are lowest. This suggests that the imposition of money bail, even when bail amounts are low, is sufficient to cause convictions.

Appendix Tables A4 and A5 focus on the top five most common offenses within felonies and misdemeanors. Table A4 highlights the extensive margin and shows coefficients from our main IV specification run on individual offenses (without offense fixed effects). Interestingly, our results appear among various categories of theft—retail theft, receiving stolen property, and retail theft (misdemeanor). Our results are somewhat lower and do not reach significance for drug offenses, DUIs, and gun possession charges. These results are comparable to Stevenson (2016), who also finds substantial results among those categories and lower effects on drug and other charges.

Table A5 examines the intensive margin—whether changes in the intensity of bail matter given that bail was set. Interestingly, we only find effects here among gun possession misdemeanors. It is possible that the effect could be driven by the relatively high average bail in this category (around $11,000). Though other offense categories also carry high bail amounts (such as aggravated assault), they typically also carry greater consequences which may deter defendants from pleading guilty. Future work will attempt to analyze why responses to bail setting appear to be particularly high in some offenses more than others, which may assist in adjusting pretrial detention standards.

3.4.3 Other Outcomes

Recidivism

We next look at recidivism, which we explore in Table 10. Existing literature has documented the role of incarceration on future criminal activity.[13] There are a variety of mechanisms which appear to drive this relationship, including the negative impact of incarceration on labor market outcomes (encouraging illegal income seeking), family disruption, loss of human capital, and peer effects resulting from associations with other detainees.

We extend this literature by examining the role of money bail on future criminal activity. There are a number of channels through which money bail in particular might cause recidivism. As our results from section 4.1 show, money bail causes convictions, which in turn may entail incarceration and subsequent effects. Even without additional convictions, money bail may impact future criminal activity via job loss during pretrial detention, financial hardship caused by raising funds to make bail, or other factors. Our data do not permit, and we do not attempt, a complete separation of the various mechanisms linking bail assessment to future criminal activity.

We follow some of the prior literature in this area by restructuring our data into a yearly panel format. Our main specification follows the first criminal offense committed by defendants in our data and estimates:

$$Recidivism_{i,t+y} = \alpha + X_{i,t} + \mu_y + \mu_t + \beta Bail_{i,t} + \varepsilon_{i,t+y}$$

Where $Recidivism_{i,t+y}$ is a binary indicator equal to one if the defendant is charged with a crime in the yth calendar year after his or her initial charge (where the initial charge year is denoted by t). $X_{i,t}$ is the full list of defendant controls previously included (these include the age, race, and gender of the defendant; along with controls for the criminal charge) which are taken in the calendar year of criminal charge. μ_y is a calendar year fixed effect; μ_t controls for the month of arraignment. $Bail_{i,t}$ is an indicator for whether the defendant was required to post money bail,

[13] See for instance Mueller-Smith (2016) and Aizer and Doyle (2015).

and is instrumented for using our judicial severity measure. β remains the key causal variable of interest, capturing the role of exogenous bail assessments on future recidivism. Our yearly defendant panel begins in the calendar year in which defendants enter our data as a result of initial charge, and ends in 2015 (the last year for which we have criminal charge data). Standard errors are clustered at the defendant level.

The base rate of yearly recidivism in our sample (around 12% in Philadelphia) along with the standard errors of our IV estimates result in some statistical imprecision in our estimate in Philadelphia. In column 1 of Table 10, we examine the role of money bail assessment on the yearly probability of future criminal behavior. Though the estimate of 0.007 is quite large economically (corresponding to a 0.7 percentage point yearly increase in the probability of committing future crime, or a 6% increase), we are unable to statistically distinguish this result from zero. In order to gain statistical precision, in column two we expand our sample to a "Combined Sample" which includes data from both Pittsburgh and Philadelphia. Though the judicial assignment process is not as random in Pittsburgh as in Philadelphia, we find quite comparable results in both localities in most of our specifications—including recidivism. We estimate an identical effect of 0.007 in the Combined Sample (or an increase of 9%), an effect which is statistically significant at a 5% level.

In columns 3 and 4, we separate future criminal charges into felonies and misdemeanors on the combined sample. We find that the bulk of our recidivism result is driven by money bail causing defendants to be charged with misdemeanors, a finding consistent with prior literature and the intuition that incarceration spells should raise the chances of committing minor crimes more than severe ones.

Our effects can be compared with the literature examining the role of incarceration spells on future criminal activity. Our results are somewhat lower than Aizer and Doyle (2015), who find juvenile incarceration increases adult incarceration by 23 percentage points, consistent with a larger role for incarceration spells on the future criminal behavior of younger defendants. Our finding is more comparable to Mueller-Smith (2016), who finds that each year of incarceration results in a 4–7 percentage point quarterly increase in post-release criminal activity. While these studies examine the role of incarceration spells on criminal behavior directly, we examine the role of money bail—which is unlikely to be a binding constraint for many defendants, but leads to sizable financial costs or detention for some defendants. It is unsurprising our results are somewhat smaller or attenuated as a result, but remain striking in that we find evidence that money bail causes recidivism. Though we emphasize the statistical imprecision of our estimates, our results suggest that the assessment of money bail yields substantial negative externalities in terms of additional crime.

Failure to Appear

We finally analyze whether money bail impacts the probability that a defendant appears in court. While we do not explicitly observe failures to appear, we construct a series of proxies. The first, which we label "Explicit FTA", is our most conservative. It reflects an explicit entry in the court calendar files of a warrant being issued as a result of the defendant failing to appear. While this surely captures instances in which the defendant failed to appear, the files lack a standard coding procedure, and so this measure may underreport the true number of failures to appear.[14] Our second measure, "Warrant" indicates whether a warrant was issued at a scheduled court calendar event. This event is consistently coded when it occurs in the calendar files, but may capture warrants issued for reasons other than failures to appear. This measure has a higher mean than Explicit FTA, occurring in approximately one out of a hundred cases, but still may underreport the true number of failures to appear.

Table 11 presents IV regressions as in column 3 of Table 4, but with Explicit FTA and Warrant as the dependent variables. The left columns restrict the sample to Philadelphia, while the right columns include the Combined Sample of Philadelphia and Pittsburgh. The coefficients on money bail are positive and insignificant in all specifications. While the imprecision of these estimates prevents us from drawing much from these results, we note that the goal of money bail is to ensure appearance at trial, that is, to have a substantial negative effect on failures to appear. Our results suggest that money bail has a negligible effect or, if anything, increases failures to appear.

Of course, a substantial caveat to these results is imposed by the limitations of our data, which rely on proxies to measure defendants' failures to appear. By contrast, prior research has found different estimates of appearance rates. For instance, Abrams & Rohlfs (2011) document that, in 2000, 22% of U.S. defendants failed to appear while 16% of those released on bail were rearrested.

[14]The average of the binary indicator for this measure is extremely small: 0.001, likely reflecting this underreporting.

They also document in Philadelphia defendants failed to appear around 10–13% of the time. Our analysis focuses on later periods, and measures failures to appear among all defendants. Primarily, however, we use administrative court data to trace either: 1) when court appearances were shifted due to defendant non-appearance, or 2) when a warrant was issued. It is possible that defendants who fail to appear may be warned prior to a warrant being issued, though we expect that the court appearance data will still count that event as a failure to appear.

We emphasize the incompleteness of our available data on failures to appear. It is possible in particular that our estimates may under-estimate the role of non-appearances in the criminal justice system. We examine the role of money bail assessment on defendants' probability of appearing in court the best we can, and find little evidence of a connection.

3.5 Conclusion

Our findings raise substantial questions about the nature of the money bail system. We find substantial variation among individual magistrates in setting money bail, suggesting that the imposition of money bail, and therefore pretrial detention, is a function of the judge one receives. We exploit the random assignment of defendants to judges to examine the causal implications of money bail. Defendants assessed money bail have a 6 percentage point (12%) higher chance of conviction and a 0.7 percentage point higher yearly probability of being charged with further crimes (or a 6–9% increase). Our results are robust to alternative specifications and examining different subgroups. Our results tend to be higher on the extensive margin—whether money bail was set at all—than the intensive amount of different bail amounts. Broadly, our results seem to be highest among relatively minor offenses: those with low average bail amounts or offenses related to retail theft. However, we do find effects which are sizable, if not significant, among defendants charged with felonies.

These results have implications for both our understanding of criminal defendants' economic circumstances and the institutional design of the American money bail system. Existing research shows that a quarter of Americans report that they cannot come up with $2,000 in 30 days (Lusardi, Schneider and Tufano (2011)), and we demonstrate how these liquidity issues have real impacts on household outcomes. The demands of money bail are quite low for those with easy access to cash, so we expect our findings are largely driven by those facing severe liquidity constraints.

We also document how money bail impacts the later outcome of recidivism, potentially through channels of pretrial detention, the financial imposition of paying bail, or the impact of post-conviction incarceration spells. Our work complements other literature demonstrating how incarceration causally influences future criminal behavior (for instance, Mueller-Smith (2016) and Aizer and Doyle (2015)), but differs by providing a link to the pretrial process.

From a legal perspective, our work raises both conceptual and practical issues. Examining the pretrial detention phase of the criminal justice system is particularly topical given the recent policy focus on reducing the incarcerated population in the United States. While sentencing decisions may involve tradeoffs between harms to criminal defendants and the goals of punishment, our analysis indicates a much weaker tradeoff regarding the imposition of money bail on criminal defendants. Money bail imposes many costs on society—including those stemming from pretrial detention, convictions, and recidivism—yet we find no evidence that money bail results in positive outcomes, such as an increase in defendants' rate of appearance at court. Reducing the number of arrestees held pretrial may be a relatively low-cost way of decreasing the size of the incarcerated population.

The system of money bail also raises substantive issues related to equal protection. Past work has noted the potential for racial discrimination in the bail system (e.g. Ayres and Waldfogel, 1994) and we find suggestive evidence consistent with this notion: non-white defendants are assessed bail more frequently, despite being convicted less often. However, our primary result highlights the importance of wealth in access to justice. Many defendants appear to be found guilty simply due to an inability to pay money bail, indicating two systems: one for the rich and one for the poor.

TABLE 3.1
SUMMARY STATISTICS

	Philadelphia		Pittsburgh	
	Mean	SD	Mean	SD
Age	33.5	11.6	33.4	11.7
Non-White	0.56	-	0.42	-
Race Missing	0.12	-	0.027	-
Male	0.81	-	0.77	-
Prior Cases	0.42	-	0.33	-
Total Offenses	3.42	2.95	4.68	3.48
Case Guilty	0.50	-	0.77	-
Total Bail	24083	74891	12964	28697
Money Bail	0.62	-	0.53	-
Posted Money Bail	0.31	-	0.24	-
Bench Warrant	0.019	-	0.15	-
Charged With Future Crime	0.43	-	0.33	-
Sample Size	203188		57145	

THE SAMPLE INCLUDES CRIMINAL CASES IN PHILADELPHIA AND PITTSBURGH IN THE PERIOD 2010–2015. BAIL INFORMATION IS REPORTED FROM THE MAGISTRATE LEVEL; CASE DISPOSITION INFORMATION IS TAKEN FROM THE MOST SEVERE OFFENSE FOR WHICH THE DEFENDANT WAS CHARGED; AND BENCH WARRANT INFORMATION IS TAKEN FROM A MERGED DATASET OF ALL BENCH WARRANTS FILED IN ASSOCIATION WITH A PARTICULAR DOCKET. PRIOR CASES ARE TAKEN WITHIN OUR SAMPLE, SO THE MEASURE DOES NOT ACCOUNT FOR CRIMES COMMITTED IN THE PERIOD PRIOR TO OUR SAMPLE. DEFENDANTS ARE RECORDED AS HAVING POSTED MONEY BAIL IF MONEY BAIL WAS INITIALLY SET AND THEIR BAIL STATUS WAS AT SOME POINT LISTED AS POSTED.

TABLE 3.2
OLS REGRESSIONS OF GUILT ON ASSIGNED BAIL

	No Controls (1)	Offense FEs (2)	Full Controls (3)	Full Controls (4)
Any Money Bail	0.014^{+}	0.092^{***}	0.043^{***}	
	(0.008)	(0.007)	(0.006)	
Log(Money Bail)				0.004^{***}
				(0.001)
Proportion Guilty	0.498	0.498	0.498	0.498
N	200643	200643	200617	200617
Case Controls	No	No	Yes	Yes
Offense FEs	No	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes	Yes

OLS regressions of a binary indicator of a case disposition of guilt on a binary indicator equal to 1 if money bail is initially assigned to the case (Columns 1-3) or the continuous measure log[1+money bail amount] (column 4). Case controls include age, age^2, prior cases, number of offenses, and indicators for race, gender and out-of-state.Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge-year level. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3.3
RANDOMIZATION TESTS

	Means	Pairwise	Joint Regressions: No Controls	Joint Regressions: Controls
	(1)	(2)	(3)	(4)
Non-White	0.56	0.00035	0.00037	0.00020
		(0.000)	(0.001)	(0.001)
Race Missing	0.12	-0.00026	-0.000015	-0.00014
		(0.001)	(0.001)	(0.001)
Male	0.81	0.00053	0.00043	-0.000066
		(0.001)	(0.001)	(0.001)
Age	33.5	-0.0000010	-0.00000041	0.000016
		(0.000)	(0.000)	(0.000)
Out of State	0.031	0.0018	0.0019	0.0026
		(0.001)	(0.001)	(0.002)
Prior Cases	0.42	0.00013	0.00013	0.00037
		(0.000)	(0.000)	(0.001)
N. of cases			200617	200617
F-Statistic			0.54	0.34
Offense FEs		No	Yes	Yes
Month FEs		No	No	Yes

OLS regressions of our judge severity measure on case characteristics for the Philadelphia sample. Column 1 presents means of case characteristics. Column 2 presents coefficients of separate bivariate regressions of the judge severity measure on each case characteristic. Column 3 contains the coefficients from a single regression of the judge severity measure on all case characteristics and month fixed effects. Column 4 shows the coefficients from a regression identical to column 3, but additionally including offense fixed effects. F-statistics are reported for the test of joint significance of all shown case characteristics. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3.4
IV REGRESSIONS OF GUILT ON MONEY BAIL

	First Stage	Reduced Form	IV
	Any Money Bail	Case Guilty	Case Guilty
	(1)	(2)	(3)
Severity	0.587^{***}	0.036^{**}	
	(0.028)	(0.017)	
Any Money Bail			0.061^{**}
			(0.028)
Non-White	0.014^{***}	-0.026^{***}	-0.027^{***}
	(0.003)	(0.003)	(0.003)
Male	0.077^{***}	0.026^{***}	0.021^{***}
	(0.006)	(0.003)	(0.003)
Mean of Dep. Var.	0.623	0.498	0.499
N	200617	200617	200615
Case Controls	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes

IV regressions of a binary indicator of a case disposition of guilt (Case Guilty) on a binary indicator equal to 1 if money bail is initially assigned (Any Money Bail) instrumented by our judge severity measure based on Any Money Bail. Only the Philadelphia sample is included. The first column presents the first stage, an OLS regression of Any Money Bail on our judge severity measure. The second column presents the reduced form: a regression of Case Guilty on our judge severity measure. The final column presents the IV regression itself. Case controls include age, age^2, prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge-year level. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3.5
IV REGRESSIONS OF GUILT × DETENTION STATUS ON MONEY BAIL

	Guilty		Not Guilty	
	Detained (1)	Released (2)	Detained (3)	Released (4)
Any Money Bail	0.161^{***}	-0.098^{+}	0.014	-0.077
	(0.059)	(0.060)	(0.050)	(0.053)
Non-White	-0.006^{**}	-0.021^{***}	0.029^{***}	-0.003
	(0.002)	(0.003)	(0.003)	(0.004)
Male	0.029^{***}	-0.008	0.028^{***}	-0.049^{***}
	(0.005)	(0.005)	(0.006)	(0.006)
Mean of Dep. Var.	0.226	0.272	0.178	0.323
N	200615	200615	200615	200615
Case Controls	Yes	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes	Yes

IV regressions of a binary indicator of a new measure of full defendant outcomes on a binary indicator equal to 1 if money bail is initially assigned (Any Money Bail) instrumented by our judge severity measure based on Any Money Bail. Only the Philadelphia sample is included. Outcomes for defendants are split into four categories corresponding to the interaction of being detained and a case disposition of guilty. Detained defendants were either remanded without the ability to post bail, or failed to post bail given the assessment of money bail. Released individuals either did not receive money bail or posted money bail. Each of the four columns presents an IV regression with one of those category as the dependent variable. Case controls include age, age^2, prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge-year level. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3.6
IV REGRESSIONS OF GUILT ON MONEY BAIL BY CASE CHARACTERISTICS

	Felony (1)	Public Defender (2)	Male (3)	Non-White (4)
Any Money Bail	0.081	0.054^{+}	0.060^{+}	0.083^{**}
	(0.061)	(0.029)	(0.032)	(0.034)
Non-White	-0.045^{***}	-0.026^{***}	-0.026^{***}	
	(0.003)	(0.003)	(0.003)	
Male	0.020^{***}	0.024^{***}		0.024^{***}
	(0.006)	(0.004)		(0.004)
Proportion Guilty	0.541	0.492	0.509	0.515
N	94658	126757	162691	112280
Case Controls	Yes	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes	Yes

IV regressions of a binary indicator of case dispositions on a binary indicator equal to 1 if money bail is initially assigned (Any Money Bail) instrumented by our judge severity measure based on Any Money Bail. Only the Philadelphia sample is included. Each column restricts to the subsample indicated in the column header. Felony refers to defendants are who are charged with a felony offenses, public defender refers to defendants represented by public defenders. Case controls include age, age^2, prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge-year level. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3.7
IV REGRESSIONS OF GUILT ON LOG(MONEY BAIL)

	First Stage	Reduced Form	IV
	Log(Money Bail)	Case Guilty	Case Guilty
	(1)	(2)	(3)
Severity	0.561^{***}	0.004^{+}	
	(0.027)	(0.002)	
Log(Money Bail)			0.006^{**}
			(0.003)
Non-White	0.153^{***}	-0.026^{***}	-0.027^{***}
	(0.024)	(0.003)	(0.003)
Male	0.829^{***}	0.026^{***}	0.021^{***}
	(0.058)	(0.003)	(0.004)
Mean of Dep. Var.	5.695	0.498	0.499
N	200617	200617	200615
Case Controls	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes

IV regressions of a binary indicator of a case disposition of guilt (Case Guilty) on the continuous measure log[1+money bail amount] (Log(Money Bail)) instrumented by our judge severity measure based on Log(Money Bail). Only the Philadelphia sample is included. The first column presents the first stage, an OLS regression of Log(Money Bail) on our judge severity measure. The second column presents the reduced form: a regression of Case Guilty on our judge severity measure. The final column presents the IV regression itself. Case controls include age, age^2, prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge-year level. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3.8
IV REGRESSIONS OF GUILT ON LOG(MONEY BAIL) – INTENSIVE MARGIN

	First Stage	Reduced Form	IV
	Log(Money Bail | Bail>0)	Case Guilty	Case Guilty
	(1)	(2)	(3)
Severity	0.489***	−0.006	
	(0.035)	(0.008)	
Log(Money Bail | Bail > 0)			−0.013
			(0.016)
Non-White	0.047***	−0.037***	−0.036***
	(0.007)	(0.002)	(0.002)
Male	0.344***	0.019***	0.023***
	(0.021)	(0.004)	(0.006)
Mean of Dep. Var.	9.143	0.506	0.499
N	124352	124352	124338
Case Controls	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes

IV regressions of a binary indicator of a case disposition of guilt (Case Guilty) on the continuous measure log[money bail amount], instrumented by our judge severity measure based on log[money bail amount]. Only the Philadelphia sample is included, and defendants with no money bail are excluded. The first column presents the first stage, an OLS regression of log[money bail amount] on our judge severity measure. The second column presents the reduced form: a regression of Case Guilty on our judge severity measure. The final column presents the IV regression itself. Case controls include age, age^2, prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge-year level. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3.9
IV REGRESSIONS OF GUILT ON MONEY BAIL – PITTSBURGH

	First Stage	Reduced Form	IV
	Any Money Bail	Case Guilty	Case Guilty
	(1)	(2)	(3)
Severity	0.391^{***}	0.025^{+}	
	(0.026)	(0.013)	
Any Money Bail			0.064^{**}
			(0.031)
Non-White	0.107^{***}	−0.004	−0.011
	(0.006)	(0.006)	(0.007)
Male	0.084^{***}	0.053^{***}	0.047^{***}
	(0.006)	(0.006)	(0.006)
Mean of Dep. Var.	0.495	0.777	0.766
N	38149	38149	38141
Case Controls	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes

IV regressions of a binary indicator of a case disposition of guilt (Case Guilty) on a binary indicator equal to 1 if money bail is initially assigned (Any Money Bail) instrumented by our judge severity measure based on Any Money Bail. Only the Allegheny county (Pittsburgh) sample is included. The first column presents the first stage, an OLS regression of Any Money Bail on our judge severity measure. The second column presents the reduced form: a regression of Case Guilty on our judge severity measure. The final column presents the IV regression itself. Case controls include age, age^2, prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the office-judge-year level. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3.10
IV PANEL REGRESSIONS OF RECIDIVISM ON MONEY BAIL

	Philadelphia	Combined Sample		
	All Charges (1)	All Charges (2)	Felony (3)	Misdemeanor (4)
Any Money Bail	0.007	0.007^{**}	0.002	0.006^{**}
	(0.008)	(0.004)	(0.003)	(0.003)
Non-White	-0.012^{***}	-0.008^{***}	0.003^{***}	-0.012^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
Male	0.026^{***}	0.017^{***}	0.018^{***}	0.002^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
Mean of Dep. Var.	0.117	0.0811	0.0442	0.0424
N	522395	862163	862163	862163
Case Controls	Yes	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes	Yes
Month of Initial Offense FEs	Yes	Yes	Yes	Yes
Calendar Year FEs	Yes	Yes	Yes	Yes

IV regressions of recidivism on a binary indicator equal to 1 if money bail is initially assigned (Any Money Bail) instrumented by our judge severity measure. Recidivism is a binary indicator equal to one if the defendant is charged with a new offense in the current calendar year following the case in question. Only the IV regression is displayed in each column. Defendants are included in a yearly panel starting with the calendar year of offense until 2015 (the last year for which we have criminal charge data). Case controls are taken from the first case in our records only, and include age, age^2, prior cases, number of offenses, and an indicator for out-of-state. Subsequent charges are included only as instances of recidivism. Offense and month of arraignment fixed effects are also included, as are controls for the calendar year. The first column includes data only from Philadelphia (episodes of recidivism may reflect future crimes committed anywhere else in the state); the remaining columns include combined data from Pittsburgh and Philadelphia (the "Combined Sample"). Column 3 uses as a dependent variable only future crimes which are classified as felonies; column 4 focuses on future misdemeanor offenses. Standard errors are clustered at the defendant level. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3.11
IV REGRESSIONS OF FAILURE TO APPEAR ON MONEY BAIL

	Philadelphia		Combined Sample	
	Explicit FTA (1)	Warrant (2)	Explicit FTA (3)	Warrant (4)
Any Money Bail	0.003	0.018	0.002	0.005
	(0.003)	(0.021)	(0.002)	(0.008)
Non-White	−0.000	−0.001	−0.000	−0.001
	(0.000)	(0.001)	(0.000)	(0.001)
Male	−0.000	−0.000	−0.000	0.000
	(0.000)	(0.002)	(0.000)	(0.001)
Mean of Dep. Var.	0.001	0.010	0.001	0.007
N	200615	200615	238614	299779
Case Controls	Yes	Yes	Yes	Yes
Offense FEs	Yes	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes	Yes

IV regressions of binary indicators for failing to appear (FTA) at court dates on a binary indicator equal to 1 if money bail is initially assigned (Any Money Bail) instrumented by our judge severity measure based on Any Money Bail. The two columns present two different variables indicating that the defendant failed to appear. Calendar FTA is an indicator equal to one if the defendant is explicitly listed as having failed to appear at a scheduled calendar event in the data. Bench Warrant FTA is an indicator if a bench warrant was issued for the defendant. Only the Philadelphia sample is included. Case controls include age, age^2, prior cases, number of offenses, and an indicator for out-of-state. Offense and month of arraignment fixed effects are also included. Standard errors are clustered at the judge year level. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figures

FIGURE 3.1
GUILT BY BAIL STATUS: POSSESSION OF MARIJUANA

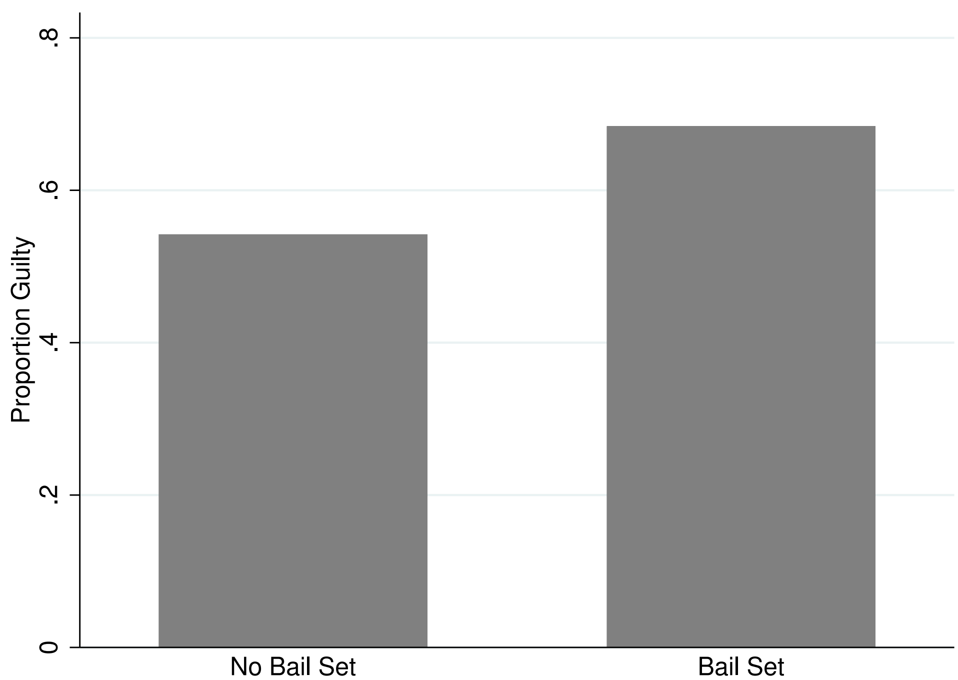
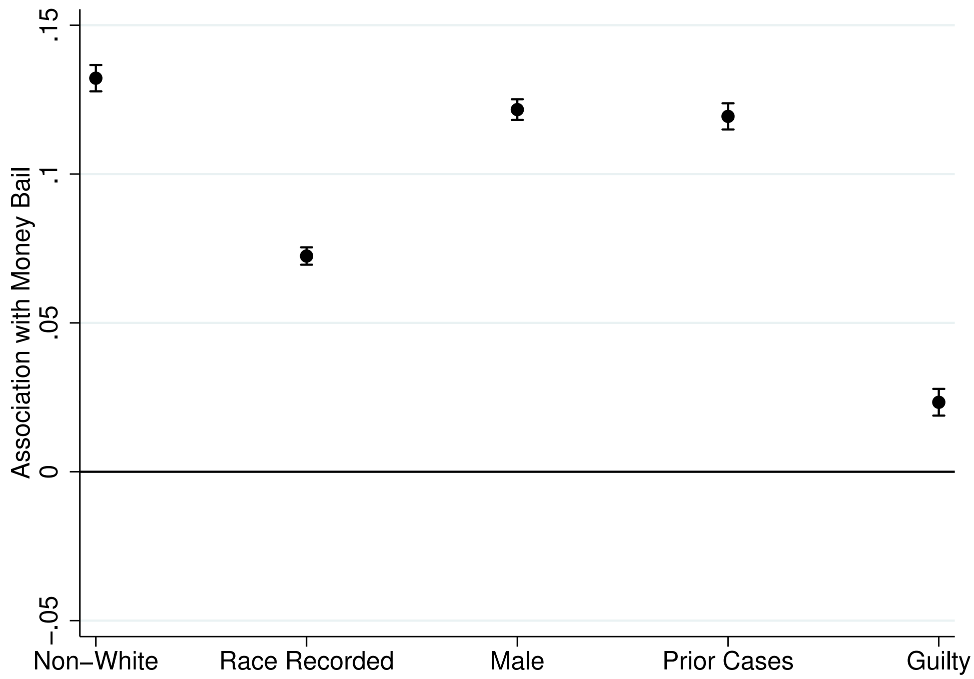


FIGURE 3.2
RANDOMIZATION CHECK: REGRESSION COEFFICIENTS OF COVARIATES ON MONEY BAIL

Panel A: RAW ASSOCIATION WITH MONEY BAIL



Panel B: MONEY BAIL AS INSTRUMENTED BY JUDICIAL SEVERITY

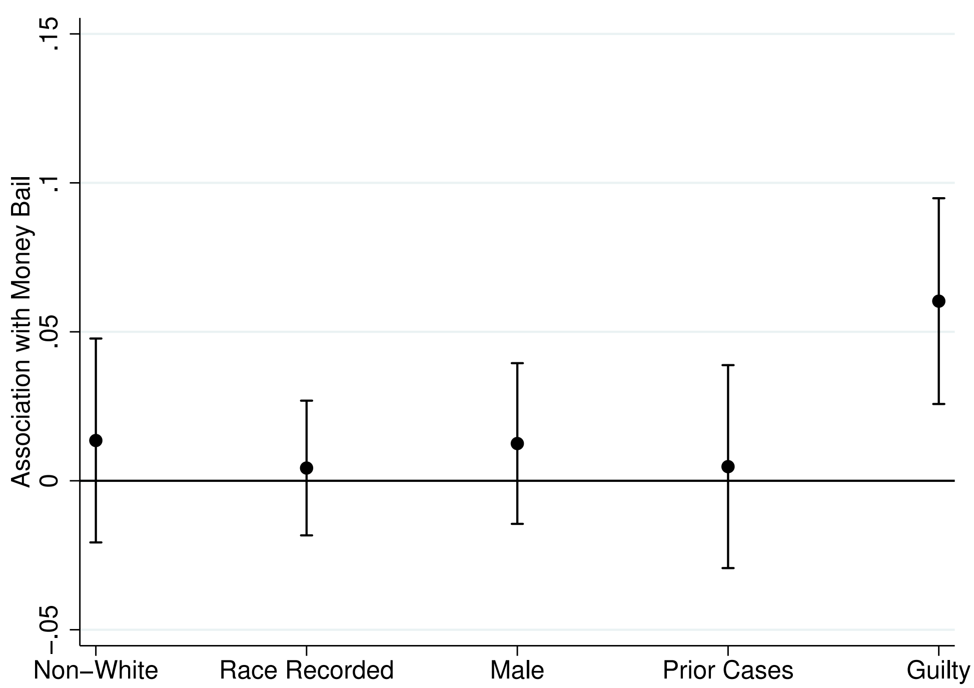
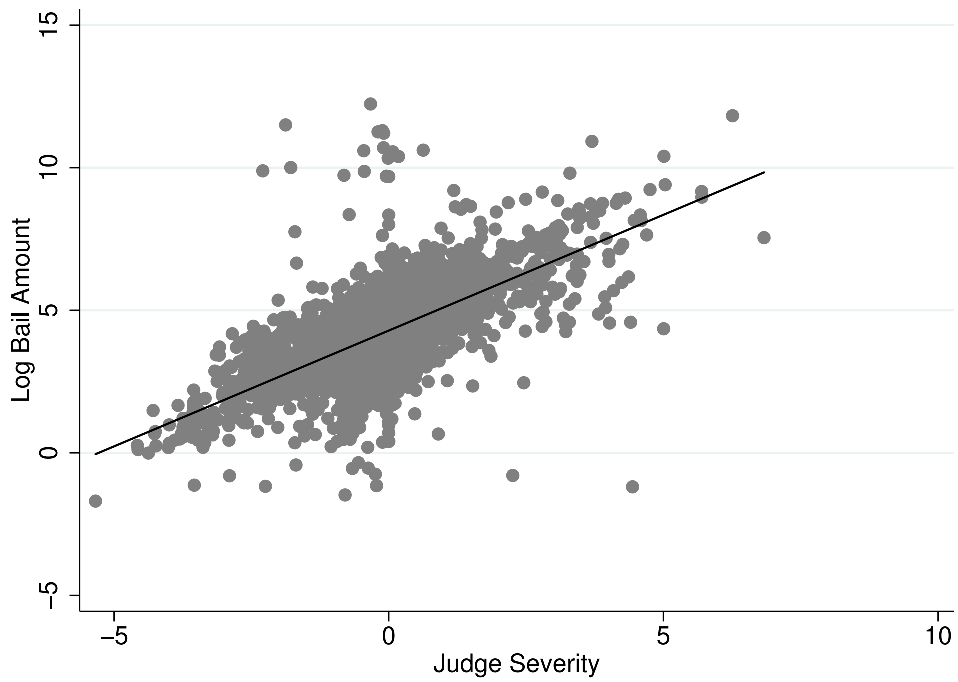
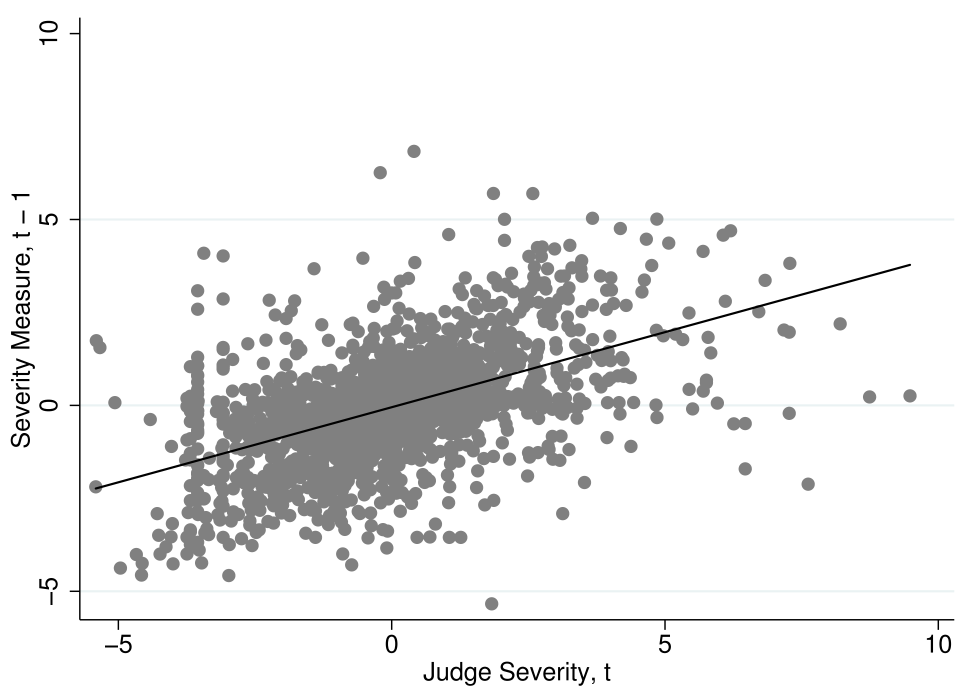


FIGURE 3.3
Panel A: JUDICIAL SEVERITY VS. LOG BAIL AMOUNT



Panel B: JUDICIAL SEVERITY T-1 V. SEVERITY IN T



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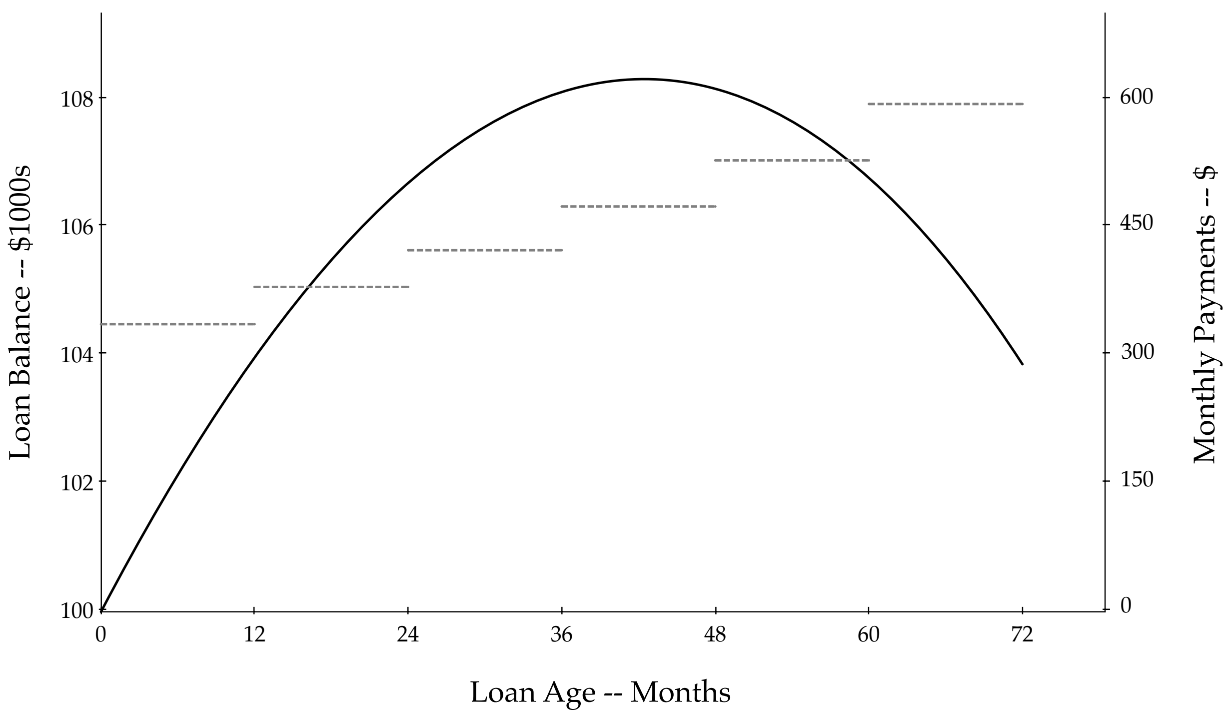
Appendix A

Appendix: Chapter 1

FIGURE A.1
STYLIZED MONTHLY PAYMENT AND BALANCE TRAJECTORY FOR OPTION ARM



The solid line shows the balance trajectory for a stylized Option ARM with an initial loan of $100,000. The balance is initially increasing, demonstrating negative amortization. Monthly payments, shown by the dashed lines, increase by 7.5% per year regardless of balance. As payments grow, the balance begins to decrease, as shown by the parabolic shape of the balance trajectory. At 5 years the monthly payment jumps to the fully amortizing amount.

FIGURE A.2
STYLIZED EXAMPLE OF IMPACT OF INTEREST RATE VARIATION ON OPTION ARM BALANCE

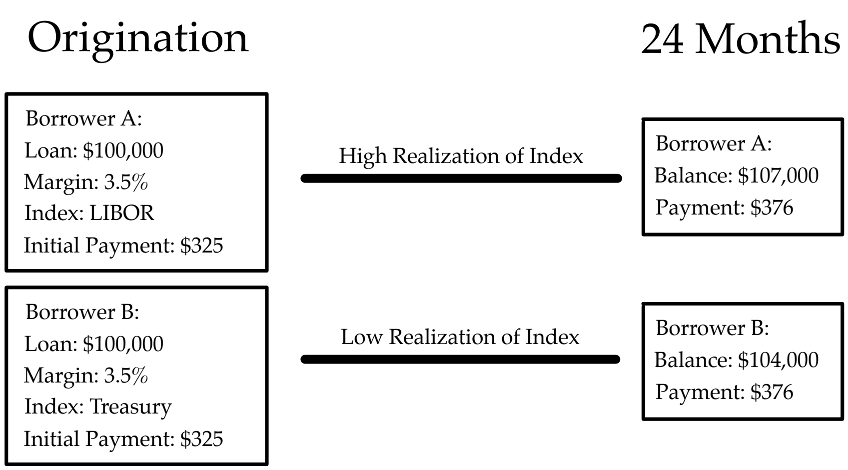
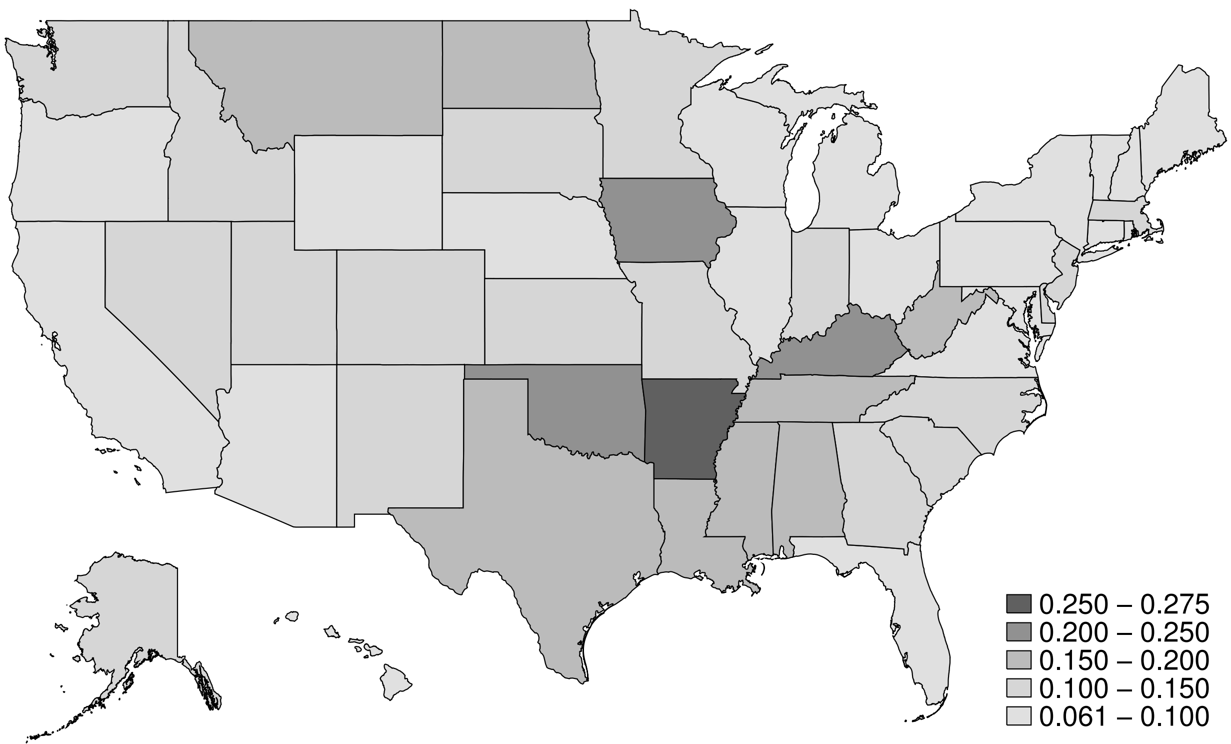


FIGURE A.3
UNIFORM DENSITY OF LIBOR INDEXED OPTION ARMS ACROSS STATES



Plot shows number of LIBOR indexed Option ARMs as a proportion of all LIBOR- or Treasury-indexed Option ARMs. The minimum is 6.1 percent, while the max is 27.5 percent. In the majority of states, between 5 and 15 percent of Option ARMs are indexed to LIBOR. A subsample of Deeds data shows average sales prices in Maryland and Pennsylvania in 2001 was $94,027

Table A.1
Fraction of LIBOR-Indexed Loans by Lender

Originator	Percent of Loans Indexed to LIBOR
American Home Mortgage	$< 1\%$
Bank United	$< 1\%$
Bank of America	85%
Countrywide	3%
Downey	0%
EMC	0%
Greenpoint	91%
IndyMac	$< 1\%$
MortgageIT	5%
Residential Funding	9%
Servicer	**Percent of Loans Indexed to LIBOR**
American Home Mortgage	$< 1\%$
Bank of America	10%
Central Mortgage	1%
Countrywide	15%
EMC	7%
IndyMac	$< 1\%$
JP Morgan Chase	2%
Nationstar	31%
Ocwen	2%
Washington Mutual	$< 1\%$

Table displays percent of LIBOR-indexed loans for the top 10 originators and servicer in the sample. Servicer is available for 99 percent of loans, while originator is only available for 27 percent of loans.

TABLE A.2
FANNIE MAE LOAN-LEVEL PRICING ADJUSTMENTS

Table 1: All Eligible Mortgages (excluding MCM) – LLPA by Credit Score/LTV Ratio

Representative Credit Score	LTV Range								
	Applicable for all mortgages with terms greater than 15 years								
	≤ 60.00%	60.01 – 70.00%	70.01 – 75.00%	75.01 – 80.00%	80.01 – 85.00%	85.01 – 90.00%	90.01 – 95.00%	95.01 – 97.00%	SFC
≥ 740	0.000%	0.250%	0.250%	0.500%	0.250%	0.250%	0.250%	0.750%	N/A
720 – 739	0.000%	0.250%	0.500%	0.750%	0.500%	0.500%	0.500%	1.000%	N/A
700 – 719	0.000%	0.500%	1.000%	1.250%	1.000%	1.000%	1.000%	1.500%	N/A
680 – 699	0.000%	0.500%	1.250%	1.750%	1.500%	1.250%	1.250%	1.500%	N/A
660 – 679	0.000%	1.000%	2.250%	2.750%	2.750%	2.250%	2.250%	2.250%	N/A
640 – 659	0.500%	1.250%	2.750%	3.000%	3.250%	2.750%	2.750%	2.750%	N/A
620 – 639	0.500%	1.500%	3.000%	3.000%	3.250%	3.250%	3.250%	3.500%	N/A
< 620(1)	0.500%	1.500%	3.000%	3.000%	3.250%	3.250%	3.250%	3.750%	N/A

Loan-level interest rate increases necessary for different categories of original LTV and credit scores for loans delivered to Fannie Mae.

TABLE A.3
IMPACT OF ORIGINAL AND CURRENT LEVERAGE ON 1 YEAR DEFAULT PROBABILITY AT 48 MONTHS

	Panel A: OLS and IV Regressions Including Current Negative Equity					
	Baseline	OLS	IV	Baseline	OLS	IV
Original Loan-to-Value	0.283***	−0.005	−0.136	0.452***	0.187***	0.231***
	(0.038)	(0.025)	(0.219)	(0.028)	(0.021)	(0.074)
Current Negative Equity in $100,000s		0.073***	0.106**		0.054***	0.045***
		(0.004)	(0.053)		(0.002)	(0.015)
Mean of Dep. Var	0.213	0.213	0.213	0.213	0.213	0.213
N	107917	107917	107917	107917	107917	107917
Origination Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	No	No	No	Yes	Yes	Yes
Full Controls	No	No	No	Yes	Yes	Yes
	Panel B: OLS and IV Regressions Including Current Loan-to-Value					
	Baseline	OLS	IV	Baseline	OLS	IV
Original Loan-to-Value	0.283***	0.027	−0.067	0.452***	0.165***	0.371***
	(0.038)	(0.028)	(0.160)	(0.028)	(0.026)	(0.068)
Current Loan-to-Value		0.195***	0.266**		0.180***	0.050
		(0.008)	(0.122)		(0.010)	(0.038)
Mean of Dep. Var	0.213	0.213	0.213	0.213	0.213	0.213
N	107917	107917	107917	107917	107917	107917
Origination Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	No	No	No	Yes	Yes	Yes
Full Controls	No	No	No	Yes	Yes	Yes

OLS and IV regressions of default between 48 and 60 months on borrowers' original loan-to-value and current equity at 24 months, defined as either the level of negative equity in $100,000s (Panel A), or current loan-to-value (Panel B). Default is defined as 60 or more days past due. Baseline refers to OLS regressions omitting current equity. IV regressions include the full set of interactions between index and origination month as instruments for current equity. Full controls refers to fixed effects for index type, documentation, the loans purpose and occupancy, the existence of prepayment penalties and private mortgage insurance, and single family homes. I also include indicators for each 20-point bin of borrowers FICO credit scores, loan originator and servicer fixed effects, and controls for second liens. I allow individual state time trends. Standard errors are clustered at the MSA level. * denotes 10% significance, ** denotes 5% significance, *** denotes 1% significance.

TABLE A.4
IMPACT OF ORIGINAL AND CURRENT LEVERAGE ON CUMULATIVE DEFAULT PROBABILITIES

	Panel A: Current Negative Equity					
	36 Months			60 Months: Controlling for Trajectory		
	Baseline	OLS	IV	Baseline	OLS	IV
Original Loan-to-Value	0.857***	0.712***	0.148**	1.041***	0.892***	0.338***
	(0.030)	(0.034)	(0.064)	(0.037)	(0.032)	(0.127)
Imputed Negative Equity at 36 Months in \$100,000s		0.026***	0.128***		0.004	−0.157*
		(0.004)	(0.016)		(0.005)	(0.081)
Imputed Negative Equity at 24 Months in \$100,000s					−0.006*	0.174***
					(0.004)	(0.050)
Imputed Negative Equity at 48 Months in \$100,000s					0.012	0.174**
					(0.008)	(0.089)
Imputed Negative Equity at 60 Months in \$100,000s					0.016**	−0.060
					(0.007)	(0.054)
Mean of Dep. Var	0.310	0.310	0.310	0.454	0.454	0.454
N	443600	443600	443600	443600	443600	443600
Origination Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	No	No	No	Yes	Yes	Yes
Full Controls	No	No	No	Yes	Yes	Yes

	Panel B: Current Loan-to-Value					
	36 Months			60 Months: Controlling for Trajectory		
	Baseline	OLS	IV	Baseline	OLS	IV
Original Loan-to-Value	0.857***	0.509***	0.255***	1.041***	0.583***	0.299**
	(0.030)	(0.037)	(0.066)	(0.037)	(0.049)	(0.124)
Imputed Loan-to-Value at 36 Months		0.231***	0.402***		0.006	−0.341
		(0.016)	(0.043)		(0.022)	(0.228)
Imputed Loan-to-Value at 24 Months					0.217***	0.806***
					(0.048)	(0.187)
Imputed Loan-to-Value at 48 Months					0.088***	−0.109
					(0.032)	(0.356)
Imputed Loan-to-Value at 60 Months					0.019	0.263
					(0.024)	(0.292)
Mean of Dep. Var	0.310	0.310	0.310	0.454	0.454	0.454
N	443600	443600	443600	443600	443600	443600
Origination Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	No	No	No	Yes	Yes	Yes
Full Controls	No	No	No	Yes	Yes	Yes

The left columns show OLS and IV regressions of default by 36 months on borrowers' original loan-to-value and imputed current equity at 36 months, defined as either the level of negative equity in \$100,000s (Panel A) or current loan-to-value (Panel B) at 36 months. Right columns show OLS and IV regressions of default by 60 months on borrowers' original loan-to-value and imputed current equity at 24 months, 36 months, 48 months and 60 months, defined as either the level of negative equity in \$100,000s (Panel A) or current loan-to-value (Panel B). Default is defined as 60 or more days past due. Baseline refers to OLS regressions omitting current equity. IV regressions include the full set of interactions between index and origination month as instruments for current equity. Full controls refers to fixed effects for index type, documentation, the loans purpose and occupancy, the existence of prepayment penalties and private mortgage insurance, and single family homes. I also include indicators for each 20-point bin of borrowers FICO credit scores, loan originator and servicer fixed effects, and controls for second liens. I allow individual state time trends. Standard errors are clustered at the MSA level. * denotes 10% significance, ** denotes 5% significance, *** denotes 1% significance.

TABLE A.5
IMPACT OF ORIGINAL LEVERAGE WITH FLEXIBLE CONTROLS FOR CURRENT LEVERAGE AND TIME-VARYING COVARIATES

	Panel A: Current Negative Equity					
	Cubic in Neg. Equity		Current Rates and Payments		Neg. Equity × Covariates	
	OLS	IV	OLS	IV	OLS	IV
Original Loan-to-Value	0.332***	0.296***	0.339***	0.204***	0.372***	0.309***
	(0.041)	(0.053)	(0.035)	(0.050)	(0.040)	(0.055)
Current Negative Equity in $100,000s	0.107***	0.146***	0.061***	0.089***	0.413**	−0.082
	(0.008)	(0.015)	(0.005)	(0.010)	(0.171)	(0.064)
Current Negative Equity^2	0.016***	−0.016				
	(0.002)	(0.019)				
Current Negative Equity^3	0.001***	−0.010***				
	(0.000)	(0.003)				
Minimum Payment in $			0.000***	0.000***		
			(0.000)	(0.000)		
Interest Rate			0.047***	0.047***		
			(0.003)	(0.003)		
Current Negative Equity × Fico Score						−0.000
						(0.000)
Current Negative Equity × Purchase						0.086***
						(0.028)
Mean of Dep. Var	0.264	0.264	0.275	0.275	0.264	0.264
N	265134	265134	240189	240189	265134	265134
Origination Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	No	No	No	Yes	Yes	Yes
Full Controls	No	No	No	Yes	Yes	Yes
	Panel B: Current Loan-to-Value					
	Cubic in LTV		Current Rates and Payments		LTV × Covariates	
	OLS	IV	OLS	IV	OLS	IV
Original Loan-to-Value	0.168***	0.175***	0.157***	0.127**	0.241***	0.229***
	(0.055)	(0.060)	(0.054)	(0.058)	(0.054)	(0.070)
Current Loan-to-Value	−0.542***	0.556***	0.389***	0.414***	−0.033	0.308***
	(0.144)	(0.067)	(0.038)	(0.043)	(0.751)	(0.049)
Current Loan-to-Value^2	1.128***	0.000				
	(0.176)	(1.866)				
Current Loan-to-Value^3	−0.391***	−0.038				
	(0.059)	(0.170)				
Minimum Payment in $			0.000***	0.000***		
			(0.000)	(0.000)		
Interest Rate			0.043***	0.043***		
			(0.003)	(0.003)		
Current Loan-to-Value × Fico Score						0.000
						(0.001)
Current Loan-to-Value × Purchase						0.001
						(0.189)
Mean of Dep. Var	0.264	0.264	0.275	0.275	0.264	0.264
N	265134	265134	240189	240189	265134	265134
Origination Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	No	No	No	Yes	Yes	Yes
Full Controls	No	No	No	Yes	Yes	Yes

OLS and IV regressions of default between 24 and 36 months on borrowers' original loan-to-value and current equity at 24 months, defined as either the level of negative equity in $100,000s (Panel A), or current loan-to-value (Panel B). The first two columns include a cubic in current equity. The third and fourth columns include current and original minimum payments, as well as the current interest rate. The 5th column interacts current equity with all control variables in an OLS specification. The final column includes current equity interacted with each borrowers FICO score and an indicator equal to one if the loan was used to purchase a home. Default is defined as 60 or more days past due. IV regressions include the full set of interactions between index and origination month as instruments for all terms including current equity. Full controls refers to fixed effects for index type, documentation, the loans purpose and occupancy, the existence of prepayment penalties and private mortgage insurance, and single family homes. I also include indicators for each 20-point bin of borrowers FICO credit scores, loan originator and servicer fixed effects, and controls for second liens. I allow individual state time trends. Standard errors are clustered at the MSA level. * denotes 10% significance, ** denotes 5% significance, *** denotes 1% significance.

TABLE A.6
IMPACT OF ORIGINAL AND CURRENT LEVERAGE ON 1 YEAR DEFAULT PROBABILITY: PROBIT ESTIMATES AND ALTERNATIVE INSTRUMENTS

	Panel A: Probit and Control Function				
	Baseline	Probit	Control Function	Probit	Control Function
Original Loan-to-Value	3.282***	2.028***	0.556**	1.602***	1.047***
	(0.100)	(0.134)	(0.260)	(0.169)	(0.296)
Current Negative Equity in $100,000s		0.251***	0.529***		
		(0.020)	(0.051)		
Current Loan-to-Value				1.347***	1.808***
				(0.111)	(0.216)
Mean of Dep. Var	0.264	0.264	0.264	0.264	0.264
N	265128	265128	265128	265128	265128
Origination Month FEs	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes
Full Controls	Yes	Yes	Yes	Yes	Yes
	Panel B: OLS and IV Incorporating Simulated Instrument				
	Baseline	OLS	IV	OLS	IV
Original Loan-to-Value	0.721***	−0.641*	0.256***	−0.541	0.229***
	(0.026)	(0.339)	(0.047)	(0.348)	(0.050)
Current Negative Equity in $100,000s		0.216***	0.090***		
		(0.060)	(0.010)		
Current Loan-to-Value				1.002***	0.415***
				(0.314)	(0.041)
Mean of Dep. Var	0.264	0.264	0.264	0.264	0.264
N	265134	265134	265134	265134	265134
Origination Month FEs	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes
Full Controls	Yes	Yes	Yes	Yes	Yes

Top panel shows probit and control function specifications of default between 24 and 36 months on borrowers' original loan-to-value and current equity at 24 months, defined as either the level of negative equity in $100,000s or current loan-to-value. Default is defined as 60 or more days past due. Baseline refers to probit regressions omitting current equity. Control Function regressions include the full set of interactions between index and origination month as instruments for current equity, and are estimated following Blundell and Powell (2004). The bottom panel includes OLS regressions as in Table **??**, but uses the simulated instrument. Full controls refers to fixed effects for index type, documentation, the loans purpose and occupancy, the existence of prepayment penalties and private mortgage insurance, and single family homes. I also include indicators for each 20-point bin of borrowers FICO credit scores, loan originator and servicer fixed effects, and controls for second liens. I allow individual state time trends. Standard errors are clustered at the MSA level. * denotes 10% significance, ** denotes 5% significance, *** denotes 1% significance.

TABLE A.7
IMPACT OF ORIGINAL AND CURRENT LEVERAGE ON ONE YEAR DELINQUENCY, DEFAULT, AND FORECLOSURE RATES

	30 Days Past Due			90 Days Past Due			Foreclosure		
	Baseline	Equity	Loan-to-Value	Baseline	Equity	Loan-to-Value	Baseline	Equity	Loan-to-Value
Original Loan-to-Value	0.688***	0.379***	0.211***	0.710***	0.407***	0.256***	0.494***	0.315***	0.213***
	(0.026)	(0.034)	(0.049)	(0.026)	(0.037)	(0.051)	(0.019)	(0.029)	(0.038)
Current Negative Equity in $100,000s		0.060***			0.059***			0.035***	
		(0.005)			(0.005)			(0.004)	
Current Loan-to-Value			0.404***			0.381***			0.234***
			(0.034)			(0.035)			(0.022)
Mean of Dep. Var	0.304	0.304	0.304	0.248	0.248	0.248	0.177	0.177	0.177
N	213535	213535	213535	278761	278761	278761	294636	294636	294636
Origination Month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Index FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Full Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

OLS and IV regressions of default between 24 and 36 months on borrowers' original loan-to-value and current equity at 24 months, defined as either the level of negative equity in $100,000s (Panel A), or current loan-to-value (Panel B). Default is defined as 30/90 or more days past due or as foreclosure. Baseline refers to OLS regressions omitting current equity. IV regressions include the full set of interactions between index and origination month as instruments for current equity. Full controls refers to fixed effects for index type, documentation, the loans purpose and occupancy, the existence of prepayment penalties and private mortgage insurance, and single family homes. I also include indicators for each 20-point bin of borrowers FICO credit scores, loan originator and servicer fixed effects, and controls for second liens. I allow individual state time trends. * denotes 10% significance, ** denotes 5% significance, *** denotes 1% significance.

Appendix B

Appendix: Chapter 2

Appendix

B.1.1 Background on fishmeal production, pollution and health in Peru

Case studies have found high levels of air pollution near fishmeal ports during the production seasons. Sueiro (2010) investigated the environmental situation in 2008 in the city surrounding the port of Chimbote, the largest in the country with 27 fishmeal plants operating at the time. The Swedish Meteorological and Hydrological Institute (SMHI) monitored the air quality in the same port area between April 2005 and April 2006. These studies found very high levels of air pollution. (SMHI found that the annual levels of SO_2 were around 110 $\mu g/m^3$ – exceeding the international standard of 80 $\mu g/m^3$. Monthly concentrations of hydrogen sulfide (H_2S) fluctuated between 20 and 40 $\mu g/m^3$ during the fishing seasons, and the hourly concentrations reached 80 to 90 $\mu g/m^3$, again exceeding the WHO standard of seven $\mu g/m^3$). In their reports, focusing especially on Ferrol Bay, the Ministry of the Environment (MINAM) cite investigations that found levels of sulfur dioxide near twice the level of international standards, hydrogen sulfide levels beyond international standards, and PM^{10} levels that vary dramatically over time and can at times reach more than twice the international standard. PM^{10} levels were higher near fishmeal plants (MINAM, 2010, 2011). A study by Consejo Nacional del Medio Ambiente (2010) of air pollution levels in Chimbote from April to August 2006 found a high correlation between PM^{10} and fishmeal production. The concentration of PM^{10} exceeded international standards throughout the study period.

Air pollution in the form of particulate matter has been shown to cause respiratory diseases, cardiovascular diseases and affect mortality in adults (see e.g. Brook RD et al., 2010; Moretti and Neidell, 2011; Schlenker and Walker, forthcoming; Chen et al., 2013; Currie et al., 2014). Some PM components are also associated with heartbeat irregularities, arterial narrowing, issues with lung function and increased emergency room visits (Stanek et al., 2011). PM has also been shown to cause respiratory diseases, skin diseases, eye diseases, and affect lung growth and mortality in children (see e.g. Currie et al., 2014; Currie and Walker, 2011; Gutierrez, 2013; Roy et al., 2012; Jayachandran, 2006; Chay and Greenstone, 2005; World Health Organization, 2006). Chemical pollutants and gases associated with fishmeal production have been linked to respiratory complications, heart disease, low blood cells counts and increased mortality (see e.g. Mustafa and Tierney, 1978; World Health Organization, 2006; Reiffenstein and Roth, 1992; Clarke et al., 2000). (Nitrogen oxide exposure is linked to respiratory effects, airway irritation and lung injury (Mustafa and Tierney, 1978). Short-term sulfur dioxide exposure is associated with higher hospital admissions due to heart disease and pulmonary complications and greater mortality (World Health Organization, 2006). Most organ systems are susceptible to hydrogen sulfide, including the nervous and respiratory systems (Reiffenstein and Roth, 1992). Clarke et al. (2000) found that dogs had reduced blood cell counts when exposed to sulfur).

We are aware of one study of the health effects of air pollution generated by fishmeal plants in Peru. The Regional Health Offices found that, among children 3 to 14 years of age, those in schools located near fishmeal plants had a 10 percent incidence of respiratory diseases in 2003; much higher than in comparable populations (see Sueiro, 2010).

Peru's fishmeal plants are also alleged to pollute the ocean by releasing "stickwater" onto the beaches or into the ocean (see e.g. Rivas, Enriquez and Nolazco, 2008). Stickwater can cause skin- and gastrointestinal diseases and conjunctivitis in humans (a) through direct exposure and (b) indirectly, by stimulating the growth of pathogens in the ocean, which can enter seafood and thus, ultimately, humans (Pruss, 1998; Fleming and Walsh, 2006; , 2009).

B.1.2 Robustness

We include a number of alternative specifications as robustness checks of the impact of fishmeal production on health. Here, we discuss those that receive limited attention in the main text.

Instrumental Variables: As the timing of fishmeal production is determined by government-mandated, semi-annual fishing ban periods (which "bind"), we consider the variation in production to be exogenous. However, we can alternatively explicitly instrument for production and production days during the last 30 or 90 days using the number of non-ban days during the same period. The resulting estimates are very similar to those in Table 2.3 when using survey-measured health outcomes, as seen in Appendix Table B.1.[1]

[1]The lack of cross-sectional variation in the instrument leads to imprecise estimates for the hospital admissions outcome variable. While survey-measured outcomes vary by day (and production and the instrument can therefore also be measured at the daily level), hospital admissions is measured only at the monthly level.

Log of Hospital Admissions: While our primary specifications include the count of hospital admissions as a dependent variable, we alternatively present our specifications with ln(hospital admissions) as a dependent variable in Appendix Table B.2. The results are qualitatively similar in terms of sign and significance to our primary specifications.

Varying Treatment Radius and Lookback Window: The treatment radius and lookback windows used in Table 2.3 were informed by the existing literature and the window used in the ENAHO survey questions[2], but nevertheless involved a degree of choice. In Figure B.2, we plot treatment effects estimated for all radii between 0 and 30 kilometers from fishmeal ports, for all outcomes.[3] For survey outcomes, the impact on health decays with distance from the nearest plant, although effects on "Any Health Issue" persist even at larger radii. For hospitals the effects become large and precisely estimated with radii that allow the inclusion of hospitals at most ports, as expected. In Figure B.3, we plot treatment effects for production days estimated with a lookback window varying from 0 to 120 days. For production days within the lookback window, the point estimates are generally biggest in short windows for adults. For children, the effects are imprecisely estimated at short windows, but become precisely estimated and significant with larger windows. The estimates in Figures B.2 and B.3 support the choice of 5/20 kilometer treatment radii and 30/90 lookback windows, and a causal interpretation of the estimates in Table 2.3.

Falsification Exercise: In Appendix Table B.4 we show estimates from a falsification exercise using hospital admissions due to health issues that should not be affected by plant production as dependent variables: "Congenital Disorders", "External Factors such as injury and poisoning", and "Mental, Behavioral, and Neurodevelopmental disorders." We find no significant effects.

Alternative Channels for the Health Impact of Fishmeal Production: Whether the estimated adverse health effects in the full sample are due to worse health during the production periods for those who work in the sector, or if instead whole communities are affected, is informative about the underlying mechanism. Recall that fishmeal production is a capital intensive industry. Only five percent of the adult sample in fishmeal locations report to work in "fishing", a broader

[2] A typical ENAHO question reads "Did you experience X in the past 30 days?".

[3] Production here is defined as the number of production days in the last 90 days, as this is the time window in which we find significant effects of fishmeal production on the health also of chidren.

category that includes the fishmeal sector. In Table B.5, we show results from estimating equation (2.1) separately for those who work in fishing. We see that fishing workers display health effects that are similar to those of other individuals.[4] One notable exception is a bigger increase in medical expenditures for fishing workers during production seasons, which may partly reflect an income effect. Overall, these results suggest that the estimated adverse health effect in the full sample are not driven by effects on the health of workers in the industry.

Another possible mechanism is that industrial fishing/fishmeal production affects health through labor market responses. In Table B.6 we investigate the impact of fishmeal production on labor market outcomes. As expected, we do see increases in the likelihood of having a job and in total income for workers in the industry during production seasons. However, fishmeal production does not affect average incomes and labor market outcomes in the full sample of adults. This suggests that the observed health effects are not due to changes in local labor markets during the production seasons.

A third possibility is that a part of the observed effect of fishmeal production on "Any Health Issue" operates through pollution of the ocean.[5] However, as seen in Table B.5, we do not observe bigger health effects for those who work in fishing, who presumably have greater direct exposure to the ocean. Moreover, in Appendix Table B.7, we show that (a) the estimated health effects are not of greater magnitude for individuals who consume more fish, and (b) fishmeal production does not increase pollution at beaches near ports relative to those further away. We conclude that ocean pollution is unlikely to contribute noticeably to the estimated health effects of fishmeal production.

Alternative Hospital Outcomes: In Appendix Table B.10, we expand the set of health outcomes to consider hospital admissions not only for respiratory issues (the type of disease episodes that we hypothesize to be most likely to respond to short-term variation in air pollution), but also for other health issues that the previous literature has found to correlate with air pollution. We find that

[4] The small number of fishing workers in our sample gives us limited power to detect differential effects but also suggests that fishing workers do not drive the aggregate effects we find.

[5] If greasy "stickwater" is released onto the beaches or directly into the ocean, a process of eutrophication can lead organisms (e.g. algae) and bacteria to grow excessively. Toxins can in turn affect human health either through direct exposure or through the consumption of seafood (World Health Organization, 2002; Committee on Nutrient Relationships in Seafood, 2007). (Effects on respiratory hospital admissions and coughs are unlikely to be due to ocean pollution).

fishmeal production increases total hospital admissions, admissions for digestive diseases (see also Kaplan et al., 2010), and for pregnancy complications. These results underline the seriousness of the fishmeal industry's impact on the health of Peru's coastal population.

B.1.3 Theoretical framework

In this section, we present a simple two-sector model with homogeneous suppliers (boats) upstream and heterogeneous final good producers (plants) downstream. The model predicts how the introduction of individual property rights over intermediate goods will tend to affect the spatial and temporal distribution of final good production. With an added hypothesis on how the distribution of final good production matters for the impact of downstream externalities, the model thus delivers a prediction for upstream Coasian solutions' downstream consequences. As explained in the body of the paper, the model's predictions will help us test hypotheses on why the fishmeal industry's impact on health may have changed as a result of Peru's ITQ reform.

The intuition of the model is as follows. An industry wide quota regime encourages boats to "race" for fish early in the season. A high per-period fish capture early in the season in turn decreases the price of fish and thereby allows less efficient fishmeal plants to survive. When boats' incentive to race for fish is removed with the introduction of individual quotas, fishing is spread out in time, the price of fish increases and less efficient plants are forced to reduce their production or exit the industry.

The model consists of two sectors: homogeneous fishing boats, who capture and sell fish, and heterogeneous fishmeal plants, who buy fish to use as an intermediate good and sell fishmeal on the international market. We assume that the price of fishmeal is fixed, and that the price of fish is determined in equilibrium based on the contemporaneous demand for and supply of fish.

Fishing boats. Our specification of the boat sector follows Clark (1980) and subsequent research. There are N identical boats, who capture fish (q_i) as a function of (costly) effort e_i and the stock of fish x, according to $q_i = \gamma x e_i$, where γ is a constant. Boats face an increasing and convex cost of effort $c(e_i)$, and a decreasing inverse market demand $p(q)$. Within each season, the fish stock declines according to the amount captured, that is $x(t) = x_0 - \int_0^t \gamma x(t') \sum_i^N e_i(t') dt'$.

Let the maximum length of the season under any regulatory regime be T. We first consider the case of an industry wide total allowable catch (TAC) quota, with magnitude H.[6] We take boats to be small relative to the industry, and assume they take the path of prices $p(t)$ and the fish stock $x(t)$ as given. Each boat chooses $e_i(t)$ for all t to maximize:

[6] We focus on situations where the quota binds. The season ends when the total quantity of fish captured is equal to the industry quota H.

$$\pi_i = \int_0^{t^*} [p(t)\gamma x(t)e_i(t) - c(e_i(t))]dt \tag{B.1}$$

which gives optimal effort $e_i^*(t)$ defined by the first order condition $c_i'(e_i^*(t)) = p(t)\gamma x(t)$. Under the TAC regime, boats simply choose effort to equate marginal revenue and marginal costs, without internalizing their impact on the fish stock.

We next turn to the individual quota regime (ITQ). We assume that each boat is assigned a quota of H/N. There is no fixed t^*; instead each boat implicitly chooses a path of effort that determines when their quota is exhausted (time $\tilde{t}$) – an optimal control problem for each boat's cumulative catch, $y_i(t)$. Each boat solves:

$$\max \int_0^{\tilde{t}} [p(t)\gamma x(t)e_i(t) - c(e_i(t))]dt \tag{B.2}$$

subject to $\frac{dy_i}{dt} = \gamma x(t)e_i(t)$ for $0 \leq t \leq \tilde{t}$, $y_i(0) = 0$, $y_i(\tilde{t}) = H/N$, and $\tilde{t} \leq T$. This gives $c'(e_i(t)) = (p(t) - \lambda_i)\gamma x(t)$ and $\frac{d\gamma_i}{dt} = -\frac{\partial \mathcal{H}}{\partial y_i} = 0 \Rightarrow \lambda_i$ constant.[7] If the quota binds, $\lambda_i > 0$.

λ_i represents each boat's internalization of the reduction in season length generated by an additional unit of effort. We can write the inverse demand in equilibrium in terms of the individual effort decision and stock of fish. We can then rewrite the first order conditions (with e^* representing the optimal effort level of a boat under the TAC regime, and $\tilde{e}$ representing the optimal effort level under the ITQ regime) as $c'(e_i^*(t)) = p(\gamma x(t)e_i^*(t))\gamma x(t)$ for $t \leq t^*$ and $c'(\tilde{e}_i(t)) = [p(\gamma x(t)\tilde{e}_i(t)) - \lambda_i]\gamma x(t)$ for $t \leq \tilde{t}$.

With λ_i in hand the effort decision at any t is determined by $x(t)$ at all points. It is thus helpful to consider each boat as simply solving a static problem (at any t) that differs under the two regimes as follows:

$$c'(e_i^*) = p(\gamma x e_i^*)\gamma x \tag{B.3}$$

$$c'(\tilde{e}_i) = [p(\gamma x \tilde{e}_i) - \lambda_i]\gamma x \tag{B.4}$$

[7] The Hamiltonian is: $\mathcal{H} = p(t)\gamma x(t)e_i(t) - c(e_i(t)) + \lambda_i \gamma x(t)e_i(t)$.

These two equations imply that (a) facing an equal stock of fish x, effort at any t must be weakly higher in the TAC regime, and (b) fish capture is decreasing in the stock of fish under both regimes.[8] Together (a) and (b) imply that the highest fish capture, and lowest price, occur under the TAC regime (when the stock of fish is at its initial x_0). Finally, (c) the fish stock must always be weakly higher under the ITQ regime than under the TAC regime. Hence, the season must be longer under the ITQ regime.[9]

Fishmeal plants. We now turn to the plant sector. There is a mass M of fishmeal plants with heterogenous marginal costs that require one unit of intermediate good q to produce each unit of the homogeneous final good q^f. The price of the final good is normalized to one. The price of the intermediate good at time t is $p(t)$. Let plant j's marginal cost be given by:

$$MC_j(q^f, p(t)) = MC(q^f) + \alpha_j + p(t) \tag{B.5}$$

where α_j is a plant-specific constant. If firms share common technology outside of the α_j, the minimum average cost for each firm can be described as $r + \alpha_j + p(t)$, where r is the minimum average cost for a firm with $\alpha_j = 0$ and facing 0 cost of the intermediate good. Firm j produces some positive amount so long as $r + \alpha_j + p(t) < 1$. This means that as firms face higher input prices $p(t)$, the less efficient firms – those with high α_j – decrease production and eventually drop out of the market. Each firm has a threshold price

$$p_j^* = 1 - r - \alpha_j \tag{B.6}$$

above which it will not produce. Let p_j^* be distributed among firms in the industry on [0,1] according to $F(\cdot)$. For firm j, denote demand by $\tilde{q}(p(t), p_j^*)$ (where demand is 0 for $p(t) < p_j^*$). We can then describe the market demand $q(p(t))$ by:

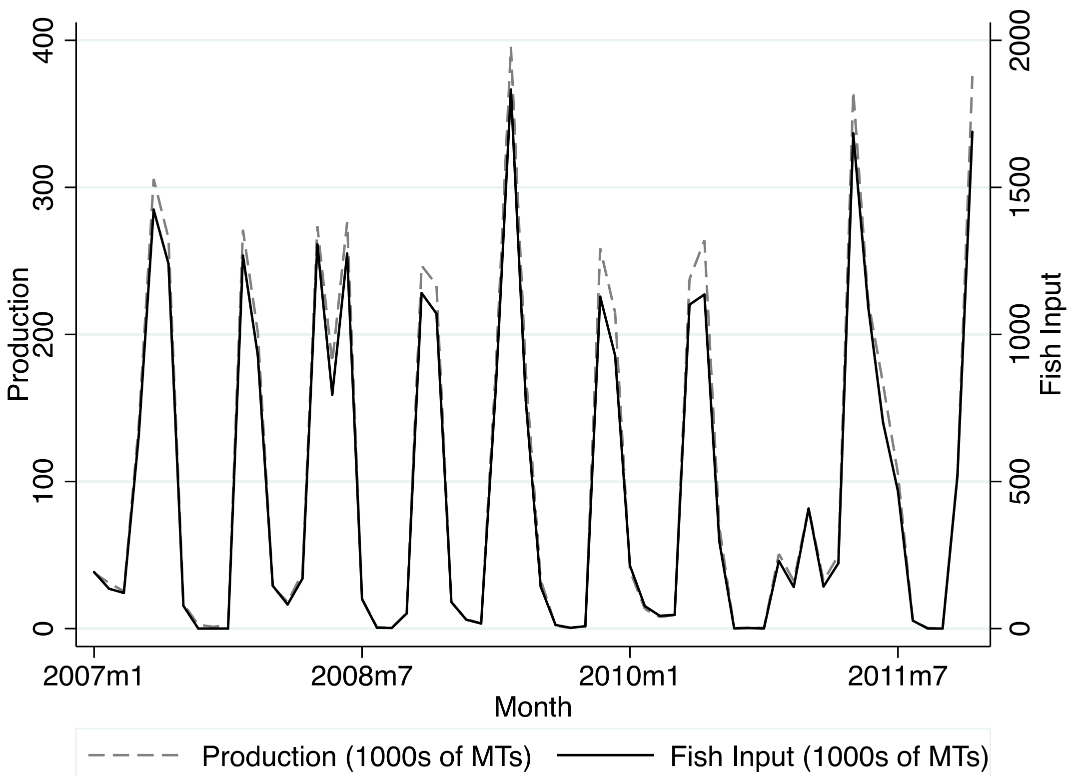
$$q(p(t)) = M \int_{p(t)}^{1} \tilde{q}(p(t), p_j^*) dF(p_j^*) \tag{B.7}$$

[8]Suppose, for the TAC regime, that $x > x'$, but $\gamma x' e_i' \geq \gamma x e_i$. Then $e_i' > e_i$, so $c'(e_i) < c'(e_i') = p(\gamma x' e_i')\gamma x' < p(\gamma x e_i)\gamma x = c'(e_i)$. An identical argument holds for the ITQ regime.

[9]Note that a necessary condition for $x^*(t) > \tilde{x}(t)$, for some t, is that there be some x such that the equilibrium effort at fish stock x is higher under the ITQ regime than under the TAC regime.

Under standard assumptions, this gives decreasing market demand. As discussed above, the highest per-period production, and lowest price, occur under the TAC regime. For fishmeal plants, this implies that (d) a greater mass of plants have non-zero production (at some point in the season) in the TAC regime than in the ITQ regime, and (e) the plants that produce in the TAC regime but not in the ITQ regime are those with the lowest p_j^*, that is, those with the highest marginal cost. We test the model's predictions in the next section.

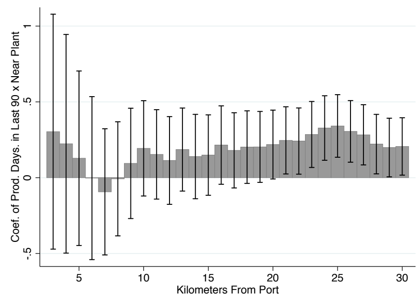
FIGURE B.1
RELATIONSHIP BETWEEN FISHMEAL PRODUCTION AND INPUT OF FISH



Monthly port level fishmeal production (dashed line) and fish input (solid line), measured in 1000s of metric tons. Input based on daily boat level fish capture as weighed at fishmeal plants. Production based on monthly plant level reports.

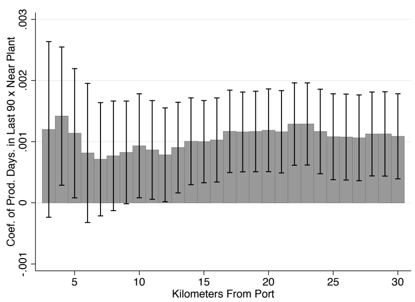
Figure B.2
Impact of Fishmeal Production on Health: Varying Treatment Radius

(a) Respiratory Admissions

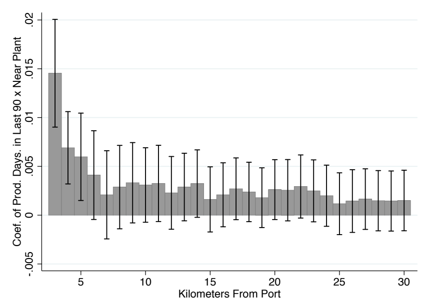


Adults

(a) Any Health Issue

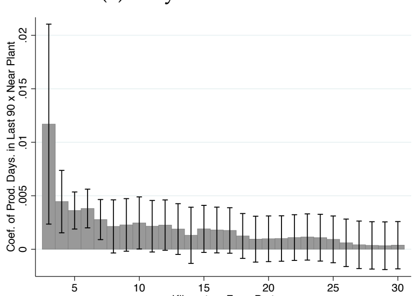


(b) Log Medical Expenditure

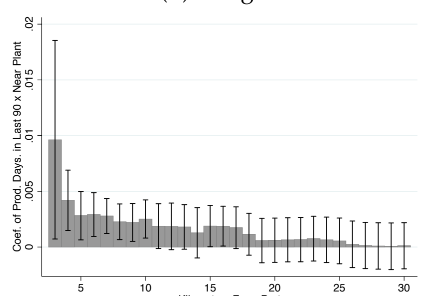


Children ($\leq$ 5)

(c) Any Health Issue



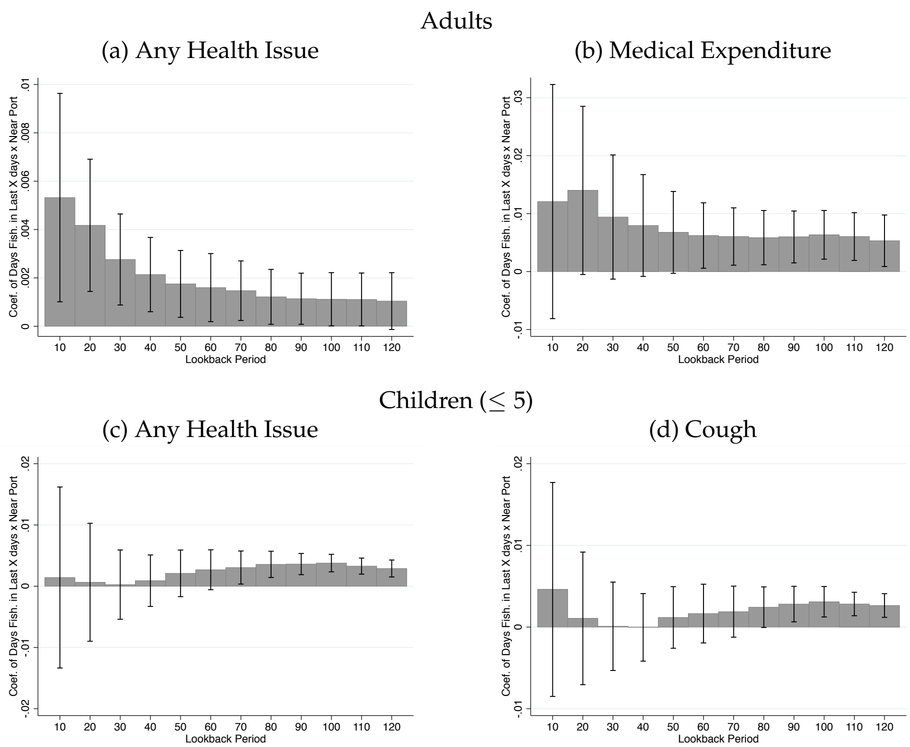
(d) Cough



Children ($\leq$ 1)

We plot the coefficient of "Production Days in the Last 90 Days $\times$ Near Plant", based on regressions similar to those in Table 2.3. We allow the treatment radius that defines "Near Plant" to vary up to 30 kilometers and correspondingly vary the control group, defined as those living outside the treatment radius. 95% confidence intervals based on standard errors clustered as in Table 2.3 are shown.

FIGURE B.3
IMPACT OF FISHMEAL PRODUCTION ON HEALTH: VARYING LOOKBACK WINDOW



We plot the coefficient of "Production Days in the Last x Days × Near Plant", based on regressions similar to those in Table 2.3. We allow the length of the lookback window "x" to vary up to 120 days. 95% confidence intervals based on standard errors clustered as in Table 2.3 are shown. Figures for hospital admissions are not shown as the data only allows for monthly variation in the lookback window.

TABLE B.1
IMPACT OF FISHMEAL PRODUCTION INSTRUMENTED BY FISHING SEASONS ON HEALTH

	Hospitals	Adults		Children: ≤ 5	
	Respiratory Admissions	Any Health Issue	Log Medical Expenditure	Any Health Issue	Cough
	Log Fishmeal Production in Last 30 Days				
Log Fishmeal Prod. in Last 30 Days		0.002 (0.009)	-0.015 (0.042)	0.034 (0.032)	0.013 (0.033)
Log Fishmeal Prod. in Last 30 Days x Near Plant	-6.316 (6.870)	0.068*** (0.021)	0.243** (0.095)	0.010 (0.048)	0.033 (0.055)
	Log Fishmeal Production in Last 90 Days				
Log Fishmeal Prod. in Last 90 Days		-0.002 (0.012)	-0.031 (0.053)	-0.002 (0.024)	-0.032 (0.026)
Log Fishmeal Prod. in Last 90 Days x Near Plant	-3.531 (14.704)	0.147* (0.089)	0.516* (0.304)	0.045 (0.056)	0.103** (0.049)
	Production Days in Last 30 Days				
Production Days in Last 30 Days		0.001 (0.001)	-0.003 (0.007)	0.004 (0.004)	0.002 (0.004)
Production Days in Last 30 Days x Near Plant	-0.566 (0.615)	0.008** (0.003)	0.024* (0.013)	0.001 (0.006)	0.004 (0.007)
	Production Days in Last 90 Days				
Production Days in Last 90 Days		-0.001 (0.001)	-0.001 (0.004)	-0.000 (0.002)	-0.003 (0.002)
Production Days in Last 90 Days x Near Plant	-0.087 (0.362)	0.005*** (0.002)	0.018** (0.007)	0.004 (0.005)	0.009** (0.004)
Hospital/Centro Poblado/District FEs	Yes	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes	Yes
HH Controls	No	Yes	Yes	Yes	Yes

IV regressions of health outcomes regressed on measures of production ("Log Fishmeal Production" and "Production Days") and those measures of production interacted with a dummy for living near a plant. We instrument for production and the interaction with the number of days the fishing season was open in last 30 or 90 days and number of days the fishing season was open $\times$ "Near Plant." Hospital admissions measure total monthly admissions at the hospital level. Adult data includes those over 13 years of age living in costal regions sampled in ENAHO (2007-2011), child data includes those under 6 years old living in coastal regions sampled in ENDES (2007-2011). Last 30 or 90 days is calculated as last 1 or 3 months for hospital data. "Near Plant" is defined as 5 kilometers for survey data and 20 kilometers for hospital data. All specifications include a dummy variable for living near a plant. Production not interacted with near plant excluded from hospital regressions due to collinearity with Month $\times$ Year fixed effects. Adult regressions include controls for age, gender, native language and level of education. Child regressions include controls for age gender, household assets and mother's level of education. Hospital, adult and child specifications include hospital, Centro Poblado and district fixed effects, respectively, with standard errors clustered at the same level. A "Production Day" is defined by $>$ 1000 MTs of input at the port level. Fishmeal production is based on daily inputs of fish, measured in 10,000s of MTs. "Respiratory Admissions" is a count, medical expenditure is measured in Peruvian Soles and all other dependent variables are binary. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE B.2
IMPACT OF FISHMEAL PRODUCTION ON LOG HOSPITAL ADMISSIONS

	ln(Hospital Admissions)			
Log Fishmeal Prod. in Last 30 Days	0.021*** (0.005)			
Log Fishmeal Prod. in Last 30 Days x Near Plant	0.016 (0.011)			
Log Fishmeal Prod. in Last 90 Days		0.037*** (0.005)		
Log Fishmeal Prod. in Last 90 Days x Near Plant		0.021* (0.012)		
Production Days in Last 30 Days			0.002** (0.001)	
Production Days in Last 30 Days x Near Plant			0.004*** (0.001)	
Production Days in Last 90 Days				0.001*** (0.000)
Production Days in Last 90 Days x Near Plant				0.003*** (0.001)
Mean of Dep. Var.	4.26	4.26	4.26	4.26
N	141981	141981	141981	141981
Hospital FEs	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes

OLS regressions. Hospital admissions measure total monthly admissions at the hospital level. Last 30 or 90 days is calculated as last 1 or 3 months for hospital data. Near plant is defined as 20 kilometers for hospital data. Hospital fixed effects are included and standard errors are clustered at the hospital level. A "Production Day" is defined by $>$ 1000 MTs of input at the port level. Fishmeal production is based on daily inputs of fish, measured in 10,000s of MTs. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE B.3
IMPACT OF FISHMEAL PRODUCTION ON HEALTH - BEFORE 2009 REFORM

	Hospitals	Adults		Children: ≤ 5	
	Respiratory Admissions	Any Health Issue	Log Medical Expenditure	Any Health Issue	Cough
			Log Fishmeal Production in Last 30 Days		
Log Fishmeal Prod. in Last 30 Days	-7.196^{***}	0.009^{*}	0.001	0.012	-0.001
	(0.864)	(0.005)	(0.027)	(0.032)	(0.032)
Log Fishmeal Prod. in Last 30 Days x Near Plant	8.741^{***}	0.012	0.087	0.220^{***}	0.087^{**}
	(2.985)	(0.011)	(0.076)	(0.038)	(0.034)
			Log Fishmeal Production in Last 90 Days		
Log Fishmeal Prod. in Last 90 Days	-6.905^{***}	-0.002	0.003	0.035^{*}	0.020
	(0.616)	(0.004)	(0.022)	(0.021)	(0.025)
Log Fishmeal Prod. in Last 90 Days x Near Plant	1.701	0.026^{***}	0.012	0.468^{***}	0.192
	(3.159)	(0.009)	(0.097)	(0.119)	(0.152)
			Production Days in Last 30 Days		
Production Days in Last 30 Days	-1.244^{***}	0.001	0.003	0.001	-0.002
	(0.155)	(0.001)	(0.005)	(0.005)	(0.005)
Production Days in Last 30 Days x Near Plant	1.203^{**}	0.003^{**}	0.009	0.014^{**}	-0.004
	(0.581)	(0.002)	(0.009)	(0.006)	(0.005)
			Production Days in Last 90 Days		
Production Days in Last 90 Days	-0.634^{***}	-0.000	0.003	0.002	0.000
	(0.070)	(0.001)	(0.003)	(0.002)	(0.002)
Production Days in Last 90 Days x Near Plant	-0.233	0.002	-0.003	0.022^{***}	0.006
	(0.344)	(0.001)	(0.003)	(0.007)	(0.009)
Mean of Dep. Var.	170.1	0.57	3.60	0.48	0.41
N	56675	63128	63138	3677	3675
Hospital/Centro Poblado/District FEs	Yes	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes	Yes
HH Controls	No	Yes	Yes	Yes	Yes

OLS regressions. Hospital admissions measure total monthly admissions at the hospital level. Adult data includes those over 13 years of age living in costal regions sampled in ENAHO (2007-2009), child data includes those under 6 years old living in coastal regions sampled in ENDES (2007-2009). Last 30 or 90 days is calculated as last 1 or 3 months for hospital data. "Near Plant" is defined as 5 kilometers for survey data and 20 kilometers for hospital data. All specifications include a dummy variable for living near a plant. Adult regressions include controls for age, gender, native language and level of education. Child regressions include controls for age, gender, household assets and mother's level of education. Hospital, adult and child specifications include hospital, Centro Poblado and district fixed effects respectively, with standard errors clustered at the same level. A "Production Day" is defined by >1000 MTs of input at the port level. Fishmeal production is based on daily inputs of fish, measured in 10,000s of MTs. "Respiratory Admissions" is a count, medical expenditure is measured in Peruvian Soles and all other dependent variables are binary. The reform began on April 20th, 2009 in the North/Central region and July 7th, 2009 in the South. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE B.4
IMPACT OF FISHMEAL PROD. ON HOSP. ADMIS. – PLACEBO OUTCOMES

	Congenital Disorders	Ext. Factors: Injury/Poisoning	Mental Health
	Log Fishmeal Production in Last 30 Days		
Log Fishmeal Prod. in Last 30 Days	0.016	−0.032	0.063
	(0.018)	(0.052)	(0.070)
Log Fishmeal Prod. in Last 30 Days x Near Plant	0.051	0.060	0.254
	(0.100)	(0.145)	(0.358)
Mean of Dep. Var.	1.30	3.37	9.45
N	141981	141981	141981
	Log Fishmeal Production in Last 90 Days		
Log Fishmeal Prod. in Last 90 Days	0.035*	−0.039	0.071
	(0.020)	(0.059)	(0.073)
Log Fishmeal Prod. in Last 90 Days x Near Plant	0.095	−0.102	0.409
	(0.085)	(0.167)	(0.385)
Mean of Dep. Var.	1.30	3.37	9.45
N	141981	141981	141981
	Production Days in Last 30 Days		
Production Days in Last 30 Days	0.003	−0.003	0.006
	(0.002)	(0.007)	(0.009)
Production Days in Last 30 Days x Near Plant	0.016	0.017	0.097
	(0.011)	(0.024)	(0.063)
Mean of Dep. Var.	1.30	3.37	9.45
N	141981	141981	141981
	Production Days in Last 90 Days		
Production Days in Last 90 Days	0.002	−0.006	−0.001
	(0.002)	(0.006)	(0.006)
Production Days in Last 90 Days x Near Plant	0.009	0.006	0.070
	(0.006)	(0.017)	(0.043)
Mean of Dep. Var.	1.30	3.37	9.45
N	141981	141981	141981
Hospital FEs	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes

OLS regressions. Hospital admissions measure total monthly admissions at the hospital level. Last 30 or 90 days is calculated as last 1 or 3 months for hospital data. "Near Plant" is defined as 20 kilometers for hospital data. Hospital fixed effects are included and standard errors are clustered at the hospital level. A "Production Day" is defined by $>$ 1000 MTs of input at the port level. Fishmeal production is based on daily inputs of fish, measured in 10,000s of MTs. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. Categorizations based upon International Classification of Disease Codes (ICD). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE B.5
IMPACT OF FISHMEAL PRODUCTION ON ADULT HEALTH – BY JOB CATEGORY

	Non-Fishing Workers		Fishing Workers		Non-Fishing Workers		Fishing Workers	
	Any Health Issue	Log. Medical Expenditure	Any Health Issue	Log. Medical Expenditure	Any Health Issue	Log. Medical Expenditure	Any Health Issue	Log. Medical Expenditure
	Production Days in Last 30 Days				Production Days in Last 90 Days			
Production Days in Last 30 Days	0.001***	0.001	−0.002	0.000	0.000**	0.000	−0.001	−0.004
	(0.000)	(0.002)	(0.002)	(0.012)	(0.000)	(0.001)	(0.001)	(0.006)
Production Days in Last 30 Days x Near Plant	0.003***	0.009	0.003	0.040**	0.001**	0.006***	−0.000	0.010
	(0.001)	(0.006)	(0.003)	(0.017)	(0.001)	(0.002)	(0.002)	(0.009)
	Log Fishmeal Production in Last 30 Days				Log Fishmeal Production in Last 90 Days			
Log Fishmeal Prod. in Last 30 Days	0.011***	0.005	−0.019	−0.005	0.006**	0.016	−0.011	−0.014
	(0.003)	(0.014)	(0.018)	(0.102)	(0.003)	(0.014)	(0.017)	(0.097)
Log Fishmeal Prod. in Last 30 Days x Near Plant	0.020***	0.083*	0.017	0.341***	0.013**	0.074**	−0.037	0.052
	(0.006)	(0.047)	(0.031)	(0.128)	(0.005)	(0.033)	(0.038)	(0.156)
Mean of Dep. Var.	0.59	3.72	0.54	3.13	0.59	3.72	0.54	3.13
N	158456	158489	3317	3317	158456	158489	3317	3317
Centro Poblado FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

OLS regressions. Data from ENAHO (2007-2011). Adults older than 13 living in coastal regions are included. “Near Plant” is defined as within 5 kilometers, and all specifications include a “Near Plant” dummy. Also included are controls for age, gender, native language and level of education. Standard errors, clustered at the Centro Poblado level, are included in parentheses. A “Production Day” is defined by $>$ 1000 MTs of input at the port level. Fishmeal production is based on daily inputs of fish, measured in 10,000s of MTs. Medical expenditure is measured in Peruvian Soles, all other dependent variables are binary. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. Labor categories are based on 3 digit job codes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE B.6
IMPACT OF FISHMEAL PRODUCTION ON LABOR MARKET OUTCOMES

	Panel A: All Adults							
	Has Any Job	Has 2nd Job	Total Labor Hours	Log. Total Income	Has Any Job	Has 2nd Job	Total Labor Hours	Log. Total Income
	Log Fishmeal Production in Last 30 Days				Log Fishmeal Production in Last 90 Days			
Production Days in Last 30(90) Days	0.001*	−0.000	0.000	0.027	−0.000	−0.000**	−0.001	0.004
	(0.000)	(0.000)	(0.002)	(0.018)	(0.000)	(0.000)	(0.001)	(0.008)
Production Days in Last 30(90) Days x Near Plant	−0.000	0.000	−0.002	−0.018	0.000	0.000	−0.000	−0.021
	(0.001)	(0.001)	(0.006)	(0.037)	(0.000)	(0.000)	(0.002)	(0.015)
	Log Fishmeal Production in Last 30 Days				Log Fishmeal Production in Last 90 Days			
Log Fishmeal Prod. in Last 30(90) Days	0.006**	0.001	0.011	0.260	0.002	−0.001	0.003	0.163
	(0.002)	(0.002)	(0.016)	(0.160)	(0.002)	(0.001)	(0.013)	(0.124)
Log Fishmeal Prod. in Last 30(90) Days x Near Plant	−0.007	0.002	−0.028	−0.243	−0.002	0.005	−0.019	0.140
	(0.009)	(0.005)	(0.053)	(0.405)	(0.006)	(0.003)	(0.031)	(0.346)
Mean of Dep. Var.	0.64	0.11	3.46	30.3	0.64	0.11	3.46	30.3
N	161612	161612	161612	161612	161612	161612	161612	161612
	Panel B: Non-Fishing Workers							
	Production Days in Last 30 Days				Production Days in Last 90 Days			
Production Days in Last 30(90) Days	0.001*	−0.000	0.000	0.028	0.000	−0.000**	−0.001	0.005
	(0.000)	(0.000)	(0.002)	(0.018)	(0.000)	(0.000)	(0.001)	(0.008)
Production Days in Last 30(90) Days x Near Plant	−0.000	0.000	−0.004	−0.027	−0.000	0.000	−0.002	−0.029*
	(0.001)	(0.001)	(0.005)	(0.041)	(0.000)	(0.000)	(0.002)	(0.017)
	Log Fishmeal Production in Last 30 Days				Log Fishmeal Production in Last 90 Days			
Log Fishmeal Prod. in Last 30(90) Days	0.006**	0.001	0.011	0.261	0.003	−0.001	0.003	0.158
	(0.003)	(0.002)	(0.016)	(0.163)	(0.002)	(0.001)	(0.013)	(0.127)
Log Fishmeal Prod. in Last 30(90) Days x Near Plant	−0.008	0.002	−0.046	−0.327	−0.001	0.005	−0.024	0.099
	(0.009)	(0.005)	(0.052)	(0.465)	(0.006)	(0.003)	(0.035)	(0.393)
Mean of Dep. Var.	0.63	0.11	3.41	30.1	0.63	0.11	3.41	30.1
N	158295	158295	158295	158295	158295	158295	158295	158295
	Panel C: Fishing Workers							
	Log Fishmeal Production in Last 30 Days				Log Fishmeal Production in Last 90 Days			
Production Days in Last 30(90) Days	−0.002*	−0.001	0.000	−0.066	0.000	0.000	0.004	0.020
	(0.001)	(0.001)	(0.008)	(0.089)	(0.001)	(0.001)	(0.004)	(0.057)
Production Days in Last 30(90) Days x Near Plant	0.003**	0.004**	0.031***	0.142	−0.000	0.002	0.010*	−0.011
	(0.001)	(0.002)	(0.010)	(0.176)	(0.001)	(0.001)	(0.006)	(0.086)
	Log Fishmeal Production in Last 30 Days				Log Fishmeal Production in Last 90 Days			
Log Fishmeal Prod. in Last 30(90) Days	−0.011	−0.001	−0.003	−0.153	0.005	−0.001	0.085*	1.288*
	(0.007)	(0.011)	(0.063)	(0.784)	(0.007)	(0.011)	(0.051)	(0.757)
Log Fishmeal Prod. in Last 30(90) Days x Near Plant	0.012	0.016	0.290***	1.065	−0.011	0.012	0.077	−0.136
	(0.009)	(0.020)	(0.090)	(1.334)	(0.010)	(0.017)	(0.113)	(1.276)
Mean of Dep. Var.	0.93	0.13	5.64	43.0	0.93	0.13	5.64	43.0
N	3317	3317	3317	3317	3317	3317	3317	3317
Centro Poblado FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

OLS regressions. Data from ENAHO (2007-2011). Adults older than 13 living in coastal regions are included. "Near Plant" is defined as within 5 kilometers, and all specifications include a "Near Plant" dummy. Also included are controls for age, gender, native language and level of education. Standard errors, clustered at the Centro Poblado level, are included in parentheses. A "Production Day" is defined by > 1000 MTs of input at the port level. Fishmeal production is based on daily inputs of fish, measured in 10,000s of MTs. Total income is measured in Peruvian Soles. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. Labor categories are based on 3 digit job codes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE B.7
IMPACT OF FISHMEAL PRODUCTION ON SEAWATER QUALITY AND ON ADULT HEALTH BY FISH CONSUMPTION

	Panel A: Impact of Fishmeal Production on Adult Health by Fish Consumption							
	Production Days				Log Fishmeal Production			
	30 Days		90 Days		30 Days		90 Days	
	Any Health Issue	Log. Medical Expenditure	Any Health Issue	Log. Medical Expenditure	Any Health Issue	Log. Medical Expenditure	Any Health Issue	Log. Medical Expenditure
Consumed Fresh Fish	0.002 (0.004)	0.118*** (0.023)	0.002 (0.004)	0.122*** (0.030)	0.000 (0.004)	0.108*** (0.021)	0.000 (0.004)	0.110*** (0.024)
Consumed Fresh Fish x Near Plant	0.004 (0.019)	0.008 (0.127)	0.016 (0.024)	0.105 (0.141)	0.003 (0.019)	0.022 (0.114)	0.005 (0.020)	0.080 (0.119)
Log Fishmeal Prod. in Last 30 (90) Days	0.013*** (0.004)	0.035** (0.017)	0.007** (0.003)	0.029* (0.018)				
Log Fishmeal Prod. in Last 30 (90) Days x Near Plant	0.019* (0.010)	0.120* (0.066)	0.016 (0.010)	0.139*** (0.051)				
Log Fishmeal Prod. in Last 30 (90) Days x Consumed Fresh Fish	−0.002 (0.003)	−0.035** (0.017)	−0.001 (0.003)	−0.019 (0.016)				
Log Fishmeal Prod. in Last 30 (90) Days x Consumed Fresh Fish x Near Plant	−0.002 (0.010)	−0.042 (0.077)	−0.009 (0.011)	−0.089* (0.053)				
Production Days in Last 30 (90) Days					0.001*** (0.000)	0.004* (0.002)	0.000* (0.000)	0.001 (0.001)
Production Days in Last 30 (90) Days x Near Plant					0.003* (0.001)	0.015* (0.008)	0.001 (0.001)	0.011*** (0.003)
Production Days in Last 30 (90) Days x Consumed Fresh Fish					−0.000 (0.000)	−0.003 (0.002)	0.000 (0.000)	−0.001 (0.001)
Production Days in Last 30 (90) Days x Consumed Fresh Fish x Near Plant					−0.000 (0.001)	−0.008 (0.008)	−0.000 (0.001)	−0.007* (0.004)
Mean of Dep. Var.	0.59	3.74	0.59	3.74	0.59	3.74	0.59	3.74
N	161773	161806	161773	161806	161773	161806	161773	161806
Centro Poblado FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

	Panel B: Impact of Fishmeal Production on Seawater Quality							
	Log Fishmeal Production				Production Days			
	Near Port = Within 5 kilometers		Near Port = Within 20 kilometers		Near Port = Within 5 kilometers		Near Port = Within 20 kilometers	
	30 Days	90 Days	30 Days	90 Days	30 Days	90 Days	30 Days	90 Days
Log Fishmeal Prod. in Last 30 (90) Days	−0.045*** (0.006)	−0.016*** (0.006)	−0.041*** (0.006)	−0.009 (0.005)				
Log Fishmeal Prod. in Last 30 (90) Days x Near Plant	0.028 (0.033)	0.024 (0.023)	−0.002 (0.015)	−0.013 (0.013)				
Production Days in Last 30 (90) Days					−0.006*** (0.001)	−0.002*** (0.001)	−0.005*** (0.001)	−0.001** (0.001)
Production Days in Last 30 (90) Days x Near Plant					0.003 (0.004)	0.003 (0.002)	−0.001 (0.002)	−0.001 (0.001)
Mean of Dep. Var.	0.82	0.82	0.82	0.82	0.82	0.82	0.82	0.82
N	14547	14547	14547	14547	14547	14547	14547	14547
Beach FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel A: OLS regressions. Adult data includes those over 13 years of age living in costal regions sampled in ENAHO (2007-2011). "Near Plant" is defined as 5 kilometers for survey data. All specifications include a "Near Plant" dummy. Adult regressions include controls for age, gender, native language and level of education. Standard errors are clustered at the Centro Poblado level. A "Production Day" is defined by $>$ 1000 MTs of input at the port level. Fishmeal production is based on daily inputs of fish, measured in 10,000s of MTs. Medical expenditure is measured in Peruvian Soles. We define consumption of fresh fish as the purchase of fresh fish at the household level. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. Panel B: OLS regressions. Data collected approximately weekly at the beach level from January 2007-April 2009. Quality is a binary variable equal to 1 for low levels of coliforms ($\leq$1000 NMP/100ml) and 0 for high levels. Note that fishmeal production is correlated with the prevalence of coliforms at public beaches, but the correlation is not greater inside versus outside a five, 20 or 50 kilometer treatment radius around fishmeal ports. Standard errors, clustered at the beach level, are included in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE B.8

IMPACT OF FISHMEAL INDUSTRY ON HEALTH BEFORE AND AFTER 2009 ITQ REFORM – BY JOB CATEGORY

	Reform Effect				Efficient vs. Inefficient Ports				North/Central vs. South			
	Non-Fishing Workers		Fishing Workers		Non-Fishing Workers		Fishing Workers		Non-Fishing Workers		Fishing Workers	
	Any Health Issue	Log Medical Expenditure	Any Health Issue	Log Medical Expenditure	Any Health Issue	Log Medical Expenditure	Any Health Issue	Log Medical Expenditure	Any Health Issue	Log Medical Expenditure	Any Health Issue	Log Medical Expenditure
					North Vs. South							
Post-Reform x Near Plant	0.053**	0.225	0.143	0.679	−0.091	−0.325*	−0.154	0.636	−0.086	−0.359	−0.127	0.118
	(0.027)	(0.145)	(0.124)	(0.531)	(0.057)	(0.180)	(0.315)	(0.971)	(0.053)	(0.342)	(0.282)	(1.376)
North/Central Region x Post-Reform					0.041**	−0.272*	−0.018	−0.177				
					(0.019)	(0.149)	(0.198)	(0.784)				
North/Central Region x Post-Reform x Near Plant					0.142**	0.545**	0.276	0.346				
					(0.056)	(0.220)	(0.281)	(1.058)				
Pre-Reform Max. Efficiency x Post-Reform									−0.021	−1.415***	0.388	4.944
									(0.068)	(0.481)	(0.490)	(3.236)
Pre-Reform Max. Efficiency x Post-Reform x Near Plant									0.388***	1.872**	0.585	1.871
									(0.119)	(0.806)	(0.846)	(3.726)
Mean of Dep. Var.	0.57	3.71	0.52	3.16	0.59	3.75	0.54	3.16	0.59	3.75	0.54	3.16
N	60886	60895	1272	1272	56979	56988	1164	1164	56097	56106	1153	1153
Centro Poblado	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

OLS regressions. Data from ENAHO (2007-2011). Adults older than 13 living in coastal regions are included. "Near Plant" is defined as within 5 kilometers, and all specifications include a "Near Plant" dummy. Also included are controls for age, gender, native language and level of education. Standard errors, clustered at the Centro Poblado level, are included in parentheses. The reform began on April 20th, 2009 in the North/Central region and July 7th, 2009 in the South. The port of Ilo is excluded from North vs. South specification due to production outside of designated seasons. Efficiency determined by the maximum 2008 output/input ratio for any plant within the port. Efficiency is included as a continuous variable interacted with both living near a plant and post-reform. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. Labor categories are based on 3 digit job codes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE B.9
IMPACT OF FISHMEAL INDUSTRY ON HEALTH BEFORE AND AFTER 2009 ITQ REFORM – EFFICIENT VS. INEFFICIENT PORTS – NORTH ONLY

	Hospitals	Adults		Children: ≤ 5	
	Respiratory Admissions	Any Health Issue	Log Medical Expenditure	Any Health Issue	Cough
	High Vs. Low Cost Ports				
Post-Reform x Near Plant	2.021	-0.059	0.167	-1.490^{***}	-0.831^{***}
	(26.470)	(0.065)	(0.407)	(0.176)	(0.250)
Pre-Reform Max. Efficiency x Post-Reform	-36.093^{**}	-0.054	0.427	0.115	0.467
	(17.590)	(0.115)	(0.614)	(0.500)	(0.455)
Pre-Reform Max. Efficiency x Post-Reform x Near Plant	38.986	0.328^{**}	0.058	4.170^{***}	2.956^{***}
	(98.722)	(0.162)	(0.887)	(0.504)	(0.592)
Mean of Dep. Var.	174.3	0.56	3.80	0.46	0.38
N	47815	49902	49910	4445	4443
Hospital/Centro Poblado/District FEs	Yes	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes	Yes
HH Controls	No	Yes	Yes	Yes	Yes

OLS regressions. Hospital admissions measure total monthly admissions at the hospital level, limited to 2008/2009. Adult data includes those over 13 years of age living in costal regions sampled in ENAHO (2008-2009). The reform began on April 20th, 2009 in the North/Central region and July 7th, 2009 in the South. All specifications include a dummy variable for living near a plant. Adult regressions include controls for age, gender, native language and level of education. Hospital, adult and child specifications include hospital, Centro Poblado and district fixed effects respectively, with standard errors clustered at the same level. "Respiratory Admissions" is a count, medical expenditure is measured in Peruvian Soles, all other dependent variables are binary. Efficiency is determined by the maximum 2008 output/input ratio for any plant within the port. Efficiency is included as a continuous variable interacted with both living near a plant and post-reform. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE B.10
IMPACT OF FISHMEAL PRODUCTION ON HOSPITAL ADMISSIONS – NON-RESPIRATORY ISSUES

	Total Admissions	Blood Disorders	Nervous System	Circulatory System	Digestive System	Pregnancy Complications	Perinatal Issues
				Log Fishmeal Production in Last 30 Days			
Log Fishmeal Prod. in Last 30 Days	0.570 (1.180)	-0.004 (0.013)	0.075^{**} (0.036)	-0.049 (0.046)	1.161^{***} (0.375)	0.262^{***} (0.085)	0.017 (0.017)
Log Fishmeal Prod. in Last 30 Days x Near Plant	2.277 (5.000)	-0.052 (0.076)	-0.133 (0.237)	-0.142 (0.214)	-1.069 (1.278)	0.934^{***} (0.330)	0.152 (0.139)
				Log Fishmeal Production in Last 90 Days			
Log Fishmeal Prod. in Last 90 Days	4.268^{***} (1.362)	0.000 (0.018)	0.124^{***} (0.047)	-0.047 (0.058)	1.480^{***} (0.358)	0.486^{***} (0.100)	0.030 (0.021)
Log Fishmeal Prod. in Last 90 Days x Near Plant	11.509^{*} (6.075)	-0.005 (0.084)	-0.071 (0.211)	0.322 (0.230)	2.379^{*} (1.295)	0.888^{**} (0.391)	0.071 (0.100)
				Production Days in Last 30 Days			
Production Days in Last 30 Days	0.238 (0.150)	-0.000 (0.002)	0.005 (0.004)	-0.002 (0.005)	0.159^{***} (0.049)	0.021^{*} (0.011)	0.000 (0.003)
Production Days in Last 30 Days x Near Plant	1.438^{**} (0.569)	0.002 (0.013)	-0.010 (0.044)	0.014 (0.025)	0.334^{**} (0.166)	0.186^{***} (0.050)	0.017 (0.017)
				Production Days in Last 90 Days			
Production Days in Last 90 Days	0.182^{*} (0.108)	-0.001 (0.001)	0.006^{*} (0.003)	-0.004 (0.004)	0.084^{***} (0.027)	0.015^{*} (0.008)	0.000 (0.002)
Production Days in Last 90 Days x Near Plant	1.157^{***} (0.407)	-0.001 (0.009)	-0.014 (0.028)	0.011 (0.020)	0.339^{***} (0.107)	0.128^{***} (0.036)	0.001 (0.010)
Mean of Dep. Var.	516.0	1.47	6.00	8.60	71.3	16.5	1.73
N	141981	141981	141981	141981	141981	141981	141981
Hospital FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Near Plant FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes

OLS regressions. Hospital admissions measure total monthly admissions at the hospital level. Last 30 or 90 days is calculated as last 1 or 3 months for hospital data. "Near Plant" is defined as 20 kilometers for hospital data. Hospital fixed effects are included and standard errors are clustered at the hospital level. A "Production Day" is defined by $>$ 1000 MTs of input at the port level. Fishmeal production is based on daily inputs of fish, measured in 10,000s of MTs. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. Categorizations based upon International Classification of Disease Codes (ICD). We found at least one paper associating each of the categories used: (see Medeiros et al., 1983; Dusseldorp et al., 1995; Xu, Ding and Wang, 1995; Gordian et al., 1996; Landgren, 1996; Ponka and Virtanen, 1996; Wang et al., 1997; Dejmek et al., 1999; Pope III et al., 1999; Seaton et al., 1999; Van der Zee et al., 1999; Brook et al., 2002; Bruce, Perez-Padilla and Albalak, 2002; Hoek et al., 2002; Pope III et al., 2004; Riediker et al., 2004; Baccarelli et al., 2007; Kaplan et al., 2010; Moulton and Yang, 2012). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE B.11
IMPACT OF FISHMEAL PRODUCTION ON HEALTH THROUGH AIR POLLUTION IN LIMA
ALTERNATIVE CONSTRUCTION OF POLLUTION MEASUREMENTS

	Port Level Correlation Between Fishmeal Production and Air Pollution			
	PM^{10}	$PM^{2.5}$	NO_2	SO_2
Log Fishmeal Prod. in Last 30 Days	1.210** (0.552)	1.574*** (0.192)	0.742*** (0.159)	1.638*** (0.392)
Mean of Dep. Var.	101.9	46.7	28.7	19.5
N	1231	1414	1416	1416
Month x Year FEs	Yes	Yes	Yes	Yes

	Impact of Air Pollution Instrumented by Fishmeal Production on Health		
	Hospitals	Adults	
	Respiratory Admissions	Any Health Issue	Log Medical Expenditure
		PM10	
Avg. PM^{10} level in last 30 Days x Near Plant	0.205 (0.416)	0.001*** (0.000)	−0.001 (0.000)
		PM2.5	
Avg. $PM^{2.5}$ level in last 30 Days x Near Plant	0.802** (0.392)	0.001*** (0.000)	−0.000 (0.001)
		NO_2	
Avg. NO_2 level in last 30 Days x Near Plant	1.737** (0.849)	0.002*** (0.000)	−0.000 (0.001)
		SO_2	
Avg. SO_2 level in last 30 Days x Near Plant	1.870** (0.914)	0.002*** (0.001)	−0.000 (0.001)
Mean of Dep. Var.	329.2	0.54	4.11
N	19976	33570	33583
Month x Near Plant FEs	Yes	Yes	Yes
Month x Year FEs	Yes	Yes	Yes
HH Controls	Yes	Yes	Yes

Hospital admissions measure total monthly admissions at the hospital level for hospitals whose closest port is Callao. Adult data includes those over 13 years of age whose closest port is Callao sampled in ENAHO (2007-2011). The top panel presents pollutant levels regressed on "Log Fishmeal Production" and month fixed effects. The bottom panel presents IV regressions of health outcomes on average pollutant levels in the last 30 days and average pollutant level in the last 30 days interacted with an indicator for "Near Plant" instrumented by "Log Fish Capture in Last 30 Days" and "Log Fish Capture in Last 30 Days × Near Plant." All pollutants are measured in $\mu g/m^3$. Daily pollutant levels are taken from nearest station to callao with consistent data quality (one station is slightly closer to the port, but has 50% fewer observations for some pollutants). Missing values were imputed using the following technique: (i) construct the empirical distributions for each of the five stations. (ii) On days that data is missing, find the value of the empirical distribution on that day for each of the other stations. (iii) Take the inverse distance weighted average of those values. (iv) Replace the missing data for the station with the concentration corresponding to the point in the empirical distribution found in (iii). Outcomes for children are excluded due to a lack of observations near the port of Callao. Last 30 days refers to the calendar month for hospital data and to the 30 days preceding the survey date for survey data. "Near Plant" is defined as 5 kilometers for survey data and 20 kilometers for hospital data. All specifications include a dummy variable for living near a plant. Adult regressions include controls for age, gender, native language and level of education. Hospital and adult specifications include hospital and Centro Poblado fixed effects respectively, with standard errors clustered at the same level. A "Production Day" is defined by $>$ 1000 MTs of input at the port level. Fishmeal production is based on daily inputs of fish, measured in 10,000s of MTs. Medical expenditure is measured in Peruvian Soles, all other dependent variables are binary. Mean of dep. var. gives unconditional mean for sample included in the corresponding regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix C

Appendix: Chapter 3

TABLE C.1
COMMON OFFENSES

	Count	Any Money Bail	Bail Amount	Non-White	Male
Intentional Possession of a Controlled Substance	22,846	15%	$643	48%	84%
Manufacture, Delivery, or Possession With Intent to Manufacture or Deliver	18,913	87%	$17,511	56%	92%
Aggravated Assault	12,417	97%	$49,645	63%	77%
DUI: 1st Offense	11,436	27%	$2,166	43%	82%
Retail Theft-Take Merchandise	10,424	36%	$1,284	58%	63%
Simple Assault	6,293	84%	$4,449	54%	80%
Possession of Instrument Of Crime W/Intent to Employ	6,081	85%	$10,928	54%	66%
Receiving Stolen Property	5,865	55%	$14,205	59%	85%
Possession Of Marijuana	5,641	10%	$433	72%	92%
Purchase or receipt of Controlled Substance by Unauthorized Person	5,518	11%	$288	35%	76%

TABLE C.2
RANDOMIZATION CHECK IN PITTSBURGH

			Joint Regressions	
	Means	Pairwise	No Controls	Controls
	(1)	(2)	(3)	(4)
Non-White	0.42	0.019***	0.019***	0.015***
		(0.002)	(0.002)	(0.004)
Race Missing	0.027	0.0050	0.015**	-0.013
		(0.007)	(0.007)	(0.011)
Male	0.77	0.014***	0.013***	0.0093***
		(0.003)	(0.003)	(0.003)
Age	33.4	-0.00011	-0.000042	0.000053
		(0.000)	(0.000)	(0.000)
Out of State	0.029	0.015**	0.016**	0.014+
		(0.007)	(0.007)	(0.009)
Prior Cases	0.33	-0.0063***	-0.0060**	0.0036
		(0.002)	(0.002)	(0.003)
N. of cases			38149	38149
F-Statistic			20.0	4.74
Offense FEs		No	Yes	Yes
Month FEs		No	No	Yes

+ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE C.3
IV SPECIFICATION BY BAIL AMOUNTS

	1st Quartile (1)	2nd Quartile (2)	3rd Quartile (3)	4th Quartile (4)
Any Money Bail	0.097***	0.013	0.068	0.040
	(3.05)	(0.07)	(0.57)	(0.30)
Age	0.014***	0.0047	-0.0078***	-0.0097***
	(10.44)	(1.52)	(-4.06)	(-7.25)
Age^2	-0.00016***	-0.000039	0.000084***	0.00011***
	(-10.29)	(-1.12)	(3.37)	(6.03)
Non-White	-0.040***	0.011**	-0.063***	-0.055***
	(-5.82)	(2.11)	(-11.55)	(-8.32)
Race Missing	-0.22***	-0.16***	-0.20***	-0.19***
	(-23.65)	(-8.70)	(-9.96)	(-13.71)
Male	-0.021***	0.036***	0.11***	0.033***
	(-3.95)	(3.08)	(6.86)	(4.21)
Offense Controls	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Office × Month	Yes	Yes	Yes	Yes
N. of cases	43974	47396	46327	46047
Avg. Bail Amount	734.2	3638.5	14974.9	65859.3

Marginal effects; *t* statistics in parentheses
(d) for discrete change of dummy variable from 0 to 1
+ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE C.4
IV SPECIFICATION BY BAIL AMOUNTS, EXTENSIVE MARGIN

	IV Coefficient (1)	N (2)	Avg. Bail (3)
	Panel A: Felonies		
Drug Possession, Distribute	0.016	23652	16940.5
	(0.27)		
Aggravated Assault	-0.33***	12382	49633.7
	(-2.36)		
Burglary	-0.086	4420	20715.6
	(-0.43)		
Retail Theft	0.24***	4323	2075.2
	(2.59)		
Receiving Stolen Property	0.21***	3644	20077.5
	(2.38)		
	Panel B: Misdemeanors		
Possession of Drugs	0.023	22776	542.1
	(0.62)		
1st DUI	0.024	11419	2166.3
	(0.56)		
Simple Assault	0.048	6270	4449.4
	(0.71)		
Gun Possession	0.019	6064	10927.9
	(0.20)		
Retail Theft	0.11**	6059	719.6
	(2.03)		
Offense Controls	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Office $\times$ Month	Yes	Yes	Yes

Marginal effects; t statistics in parentheses
(d) for discrete change of dummy variable from 0 to 1
+ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE C.5
IV SPECIFICATION BY BAIL AMOUNTS, INTENSIVE MARGIN

	IV Coefficient (1)	N (2)	Avg. Bail (3)
	Panel A: Felonies		
Drug Possession, Distribute	-0.016	20336	16940.5
	(-0.98)		
Aggravated Assault	-0.020	12033	49633.7
	(-0.62)		
Burglary	0.046	4185	20715.6
	(1.27)		
Retail Theft	0.059	2318	2075.2
	(1.01)		
Receiving Stolen Property	-0.39	2078	20077.5
	(-0.83)		
	Panel B: Misdemeanors		
Possession of Drugs	-0.100***	3412	542.1
	(-2.90)		
1st DUI	-0.021	3090	2166.3
	(-0.55)		
Simple Assault	-0.022	5239	4449.4
	(-0.67)		
Gun Possession	0.096***	5147	10927.9
	(2.84)		
Retail Theft	-0.027	1442	719.6
	(-0.68)		
Offense Controls	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Office $\times$ Month	Yes	Yes	Yes

Marginal effects; t statistics in parentheses
(d) for discrete change of dummy variable from 0 to 1
+ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$