ISERP

Working Paper 07-10

Fiscal Spillovers between Local Governments

Keeping up with the Joneses' School District

RANDALL REBACK

Barnard Economics

Columbia University

Columbia University in the City of New York

October 2007

INSTITUTE FOR SOCIAL AND ECONOMIC RESEARCH AND POLICY

PIONEERING SOCIAL SCIENCE RESEARCH AND SHAPING PUBLIC POLICY

FISCAL SPILLOVERS BETWEEN LOCAL GOVERNMENTS: KEEPING UP WITH THE JONESES' SCHOOL DISTRICT

Randall Reback*
Barnard Economics
Columbia University

ISERP Working Paper 07-10

October 2007

* Department of Economics, Barnard College, Columbia University and Institute for Social and Economic Research and Policy, Columbia University. Email: rr2165@columbia.edu. Phone: 212-854-5005. Fax: 212-854-8947. http://www.columbia.edu/~rr2165/
Julia Xu, Rachel Kessler, and Ryan Tan provided excellent assistance with the collection of data concerning school districts' forms of local democracy. We are grateful to the Center for Tax Policy Research at the University of Michigan, the Barnard College Economics Department, and the Columbia University Quantitative Methods in the Social Sciences Program respectively for funding their work. Thanks also to Larry Kenny for his helpful suggestions concerning these data. I am grateful for helpful suggestions from Eric Brunner, Damon Clark, Tim Conley, Rick Hanushek, Jim Hines, Larry Kenny, Dan O'Flaherty, Jonah Rockoff, and Miguel Urquiola, as well as seminar participants at Columbia University, N.Y.U., the University of Connecticut, the University of Florida, the Stanford University School of Education, Teachers College, and the American Education Finance Association conference. I also thank Sean Corcoran for supplying data concerning school district reorganizations. The views expressed in this paper and any errors are solely my own.

Abstract

Although there is a large theoretical literature concerning tax and expenditure competition between local governments, there is relatively little empirical evidence concerning whether such competition actually occurs. In the context of U.S. public school districts, the fiscal behavior of one district could affect the revenue decisions of other, nearby districts. Using financial and geographic data for every school district in the U.S. from 1972 to 2002, this paper estimates the magnitude of fiscal spillovers between districts. The results confirm that districts' revenues are influenced by exogenous shocks in their neighbors' revenues, especially for districts that are already outspending their neighbors. A one dollar increase in the mean revenues per pupil of nearby districts leads to about a 20 cent increase in a district's own revenues per pupil. These results have important implications for the optimal design of school finance programs.

Keywords: tax competition, fiscal competition, expenditure competition, spillovers, education finance, political economy, local property taxes

JEL Classifications: H72, H75, H77, I22

| 1 | INTRODUCTION

In order to fully understand the political economy of local government spending and taxation decisions, one must understand whether one locality's decision will influence other localities' decisions. Fiscal spillovers could result from fiscal competition between localities and from households' residential location decisions. Considering the potential importance of fiscal spillovers between local governments, there is relatively little prior evidence concerning whether they actually occur. Empirically, it is difficult to determine how much of the correlation in the tax and expenditure decisions of localities is due to spillovers as opposed to common underlying trends influencing these decisions, (e.g., unobserved demographic changes, common responses to changes in the local fiscal environment, etc.). If some portion of the changes in nearby localities' fiscal changes is exogenous, then one can use an instrumental variables approach to identify the impact of spillovers. Yet most factors influencing nearby districts' expenditures are also directly related to a district's own expenditures, so it is difficult to find a valid instrumental variable.

This paper empirically tests for fiscal spillovers between U.S. public school districts using two types of instrumental variable strategies. The first strategy predicts neighboring districts' fiscal behavior using observationally similar but geographically distant districts located in the same state. The second strategy examines whether districts located near state borders respond to the predicted fiscal behavior of neighboring districts located in a different state. The fiscal behavior of the out-of-state neighboring districts will often be driven by changes in their states' finance formulas or other state-specific trends that are plausibly unrelated to factors affecting the in-state, border district. The instruments in each of these strategies are very powerful predictors of the actual fiscal behavior of neighboring districts, and the empirical analyses include several robustness checks from which one can infer both the internal validity and external validity of the estimates derived from each approach.

The results suggest that a one dollar increase in the mean per pupil operating revenues of nearby districts causes a district to increase its own per pupil operating revenues by about 20 cents. While this paper cannot rule out several theoretical explanations for this mean response, exploring heterogeneous responses helps to reveal which mechanisms are most consistent with observed behavior. The response is much stronger if a district had previously been outspending its neighbors. The responses also vary based on the form of local democracy used to determine local tax rates and based on whether the district is located in a metropolitan area. As expected, one does not observe fiscal spillovers in the few places where districts lack local discretion over their operating revenues. Spillovers are smaller across districts in the same metropolitan area and the same state, because positive effects related to fiscal competition are reduced by negative effects related to households engaging in Tiebout (1956) resorting across districts. In metropolitan areas, an increase in the revenues of nearby, in-state districts leads to an influx of residents with relatively low household incomes.

The next section briefly summarizes the theoretical reasons why school districts' operating revenues might be influenced by the operating revenues of nearby districts. Sections 3 and 4 describe the empirical methodologies and data used to test for spillovers. Section 5 presents the main results, Section 6 discusses these results and presents additional analyses which shed light on the mechanisms for fiscal spillovers, and Section 7 briefly concludes with a discussion of the implications of these findings.

| 2 | THEORETICAL BACKGROUND AND PRIOR EMPIRICAL RESEARCH

There are several mechanisms by which school districts' fiscal decisions may affect the fiscal decisions of nearby districts. There are three mechanisms which would cause a positive correlation between nearby districts' revenue levels. First, there may be traditional *tax competition*, school districts restraining tax rates in order to compete for residents and/or businesses who might locate in one of the districts (see, for example, Wilson, 1999; Brueckner, 2000; Brueckner & Saavedra, 2001). Maintaining a relatively low tax rate may benefit some of a district's residents if it leads to higher property values and a higher local tax base, or if it helps to attract businesses and commerce that improve employment and private revenues.[1] Second, there may be *service competition*, school districts increasing revenues in order to attract students to the local public schools or to gain popularity among households with children. For example, increasing the number of advanced placement classes at the local high school to compete with a nearby high school might improve a district's popularity among households with children in the region; school district officials might therefore decide to spend more on advanced placement classes, even if this leads to higher local property tax rates and does not lead to positive capitalization effects. A third mechanism could occur regardless of whether student mobility is a concern—there may be *yardstick competition* (Besley & Case, 1995), whereby it is politically popular for districts to take similar actions as neighboring districts. Given the difficulty of measuring public school quality, residents may simply feel that they need to spend money on local schools to "keep up with the Joneses" who live in a nearby district.

In addition to these three mechanisms, there are two other mechanisms which could cause either a positive or a negative relationship between nearby districts' expenditures. There may be Tiebout (1956) re-sorting after one district, for some exogenous reason, changes its expenditure-tax bundle. This change might induce relocation decisions of people or businesses into nearby districts, and this in turn could alter the aggregated social preferences in these nearby districts. Tiebout re-sorting is distinct from tax competition, because it is unrelated to a district's strategic concern about its tax base. A district simply experiences a shift in aggregated social preferences as people relocate there to form better matches between their own preferences and the district's expenditure-tax bundle.[2] Using a computable general equilibrium model, Nechyba (2003) finds that changes in the amount of state aid targeted to one district influence the spending levels of nearby districts, as some households move across

---

[1] Using a model in which local officials can extract rents by using public revenues for private benefits, Wilson and Gordon (2003) show that local officials' concern about the size of local property tax bases can also decrease the share of local revenues devoted to socially wasteful spending.

[2] Given imperfect Tiebout sorting, there is within-district variation in citizens' preferences for local public school expenditures. Changes in expenditures could induce the relocation of citizens with relatively high or relatively low demand for local public school expenditures.

districts and some shift consumption between the private and public schooling sectors.[3] Finally, there may be *externalities*, whereby greater levels of services provided by neighboring districts create an incentive to either expand or cut back on a district's own services. For example, perhaps the presence of field hockey teams in neighboring school districts increases the benefit of adding a team. Alternatively, perhaps the presence of a high revenue district nearby enables a district to maintain relatively low revenues and still attract businesses that employ adults with school-aged children.

A few studies have empirically investigated the topic of fiscal spillovers at the state or county level (e.g., Case, Hines, & Rosen, 1993; Besley & Case, 1995; Figlio et. al. 1999; Baicker, 2004; Baicker, 2005). To identify spillovers, these studies argue that certain variables used in their analyses are only correlated with a government's expenditures through their effects on neighboring governments' expenditures.

In their seminal study, Case, Hines, and Rosen (1993) identify the fiscal interdependence of state expenditures by assuming that demographic trends in a neighboring state are only correlated with a state's own public expenditures via changes in the neighboring state's expenditures. Their study reveals that a state's own expenditures are *not* positively influenced by the spending of contiguous states, those that are geographic neighbors. (A more frequently cited result from their paper is evidence of fiscal interdependence among similar states that are not necessarily geographically proximate.) Figlio et. al. (1999) and Baicker (2005) use policy variables to predict changes in states' welfare expenditures and Medicaid costs respectively. Defining "neighbors" as states with high rates of cross-migration, Figlio et. al. (1999) find that states respond to their neighbors' welfare programs, especially when these programs become less generous. Baicker (2005) finds that a 10% increase in state expenditures causes neighboring states to increase expenditures by between 3.7% and 8.8%. In another study, Baicker (2004) cleverly uses data concerning capital punishment trials to show that counties are likely to increase both expenditures and revenues when a neighboring county experiences an unanticipated increase in taxes.

Additional studies have empirically investigated fiscal spillovers between municipalities or school districts. Brueckner and Saavedra (2001) investigate fiscal spillovers between 70 municipalities in the Boston area. They empirically test for spatial endogeneity in their models, and these tests fail to reject the null hypothesis that their independent variables are exogenously determined. Brueckner and Saavedra find evidence of positive spillovers between the municipalities, and they also find evidence that these spillovers disappeared after Proposition 2 ½ limited most Massachusetts' cities ability to increase local property taxes. While there is not strong empirical evidence of spatial endogeneity in these Boston-area data, one would expect spatially-correlated, unobserved variables to become increasingly important as one expands the sample to include a greater number of localities spread across a wider geographic area. Millimet & Rangasprad (2007) find evidence of fiscal spillovers between school districts in Illinois. While their study thoughtfully addresses the difficulties of

[3] In Nechyba's (2003) analysis, a block grant targeted towards the lowest income district leads to increases in the equilibrium spending per pupil in both a medium-income district and a high-income district, whereas an equivalent-sized matching grant targeted towards the lowest income district leads to an increase in the equilibrium spending of the medium-income district and a decrease in the equilibrium spending of the high-income district. These changes are much larger than those implied by the estimates of average fiscal spillovers below, as grants lead to substantial capitalization effects in Nechbya's analysis.

separating spillovers from unobserved trends, their empirical approaches might not fully address this problem.[4] Babcock, Engberg, & Greenbaum (2005) find evidence of fiscal competition specifically related to public school teacher salaries in Pennsylvania districts. They find that a district's salaries are highly influenced by previously established salaries in a comparison group of districts, defined by the contract negotiators. This provides an example of one of the many mechanisms through which a school district's operating revenues may respond to changes in other districts' operating revenues, though, in the case of teacher contracts, the interdependence of revenues does not necessarily correspond with geographic proximity.

| 3 | METHODOLOGY

Define R_{ijt} as the real operating revenue per pupil in district *i* located in state *j* during year *t*. Suppose that changes in district *i*'s revenues per pupil are influenced by recent changes in the mean revenues among neighboring districts, as well as trends related to state finance policies and district *i*'s demographics. Define W_1 and W_2 as weighting matrices based on geographic proximity. For example, when W_1 is based on contiguous districts it is constructed so that multiplying W_1 by a vector of values yields a vector with mean values among the contiguous neighbors of each district.

The relationship between changes in a district's per pupil revenues and changes in neighboring districts' revenues may be expressed as:[5]

$$R_{ijt} - R_{ijt-5} = \beta_1 W_1(R_{ijt} - R_{ijt-5}) + \gamma_{jt}\beta_2 + \gamma_{jt} X_{ijt-5}\beta_{3jt} + e_{ijt}$$
$$e_{ijt} = \lambda W_2 e_{ijt} + \varepsilon_{ijt} \qquad (1)$$
$$\text{with } |\beta_1|<1 \text{ and } |\lambda|<1 \text{ and } \varepsilon_{ijt}\sim N(0,\sigma^2),$$

where γ_{jt} represents a vector of state-year indicator variables and $\gamma_{jt} X_{ijt-5}$ represents these indicator variables interacted with lagged control variables capturing district *i*'s characteristics. The major challenge for estimating Equation 1 is that revenue changes, $R_{ijt} - R_{ijt-5}$, are endogenous. Unobserved shocks should have similar influences on a district's own revenues and the revenues of neighboring districts. The empirical analyses in this paper address the endogeneity of neighboring districts' expenditure changes by using plausibly exogenous predictors of their revenue changes as instrumental variables. The general form of the resulting model is:

---

[4] Millimet and Rangaprasad (2007) use a variety of specifications. One approach is an instrumental variables model similar in spirit to that of Case, Hines, and Rosen (1993). In the local setting, these types of instrumental variables are likely to be endogenous due to residential mobility. Another approach is to use lagged neighbor spending decisions, which is problematic if unobserved, common factors take different amounts of time to influence neighboring districts' expenditures.

[5] This specification can be derived from a framework in which the level of revenues in district *i* during year *t* is a function of the levels of revenues in other districts and of district *i*'s current demographics, i.e., $R_{ijt}=f(W_1R_{ijt}, X_{ijt})$. First-differencing these variables facilitates the use of the instrumental variables described below, which exploit exogenous variation in *changes* in revenues due to school finance reforms or other statewide trends. While the empirical models first-difference the revenues variables, changes in district *i*'s observed characteristics are endogenous; instead of first-differencing the demographic characteristics, the empirical models thus control for the state-by-year effects of detailed lagged demographic variables, which will be correlated with the exogenous component of districts' demographic changes.

$$R_{ijt} - R_{ijt-5} = \beta_1 W_1 \left(R_{ijt} - \hat{R}_{ijt-5}\right) + \gamma_{jt}\beta_2 + \gamma_{jt} X_{ijt-5}\beta_{3jt} + e_{ijt}$$

(2) $$e_{ijt} = \lambda W_2 e_{ijt} + \varepsilon_{ijt}$$

$$\text{with } |\beta_1|<1 \text{ and } |\lambda|<1 \text{ and } \varepsilon_{ijt} \sim N(0,\sigma^2),$$

The control variables in the X_{ijt-5} vector used in the models below include detailed, lagged demographic characteristics: state-year specific quartile spline terms for mean house value and for median income of residents, as well as variables measuring population density, the fraction of the district's population composed of school-aged children (ages 5-17), and the fraction of the district's population composed of people who are at least 65 years old. Given that the demographic effects are allowed to vary by state and by year, these variables will pick up the impact of state-specific factors and policies. The analyses consist of two general instrumental variable strategies, an all-neighbor instrumental variable and an out-of-state neighbor instrumental variable.

3.1 All-Neighbor IV

The all-neighbor instrumental variable strategy assumes that the impact of any spatially-correlated unobserved variable dies out beyond a certain distance. Districts within the same state during the same year are compared based on five lagged characteristics: operating revenues per pupil, mean income, median house value, population density, and the fraction of the population composed of school-aged children (ages 5 to 17).[6] I identify which four districts in the same state as district X and located at least some minimum distance from district X are most similar to district X based on these lagged characteristics.[7] The average actual change in revenues among these four comparison districts is used as an instrumental variable for the change in district X's revenues. For the main analyses, I use a minimum distance of 100 miles for the comparison districts. Using a shorter minimum distance would inflate estimates to the point where they are almost as large as ordinary least squares estimates, ostensibly because spatially-correlated, unobserved factors bias the results, but using a further minimum distance would not reduce the estimates by substantial or statistically significant amounts.[8]

---

[6] Observations for 1977, 1982, and 1987 are matched based on only four characteristics because house value information is not available from the 1970 Census.

[7] To determine similarity, I compute the Z-score for each of these five variables among observations in the same state and year, and then compute an index of dissimilarity equal to the sum of the squared differences between district X's Z-score and the Z-score of the comparison district.

[8] Results using shorter distance cutoffs support the idea that standard instrumental variables models, with predicted values that are partially based on the behavior of geographically proximate districts, are biased upwards. Using a minimum distance of 50 miles for this paper's main all-neighbor IV model, (row 4 of Table 2), would produce an estimate of fiscal spillovers of .322, nearly as large as the corresponding ordinary least squares estimate of .366. In contrast, using a minimum distance of 150 miles for this paper's main all-neighbor IV model yields an estimate of fiscal spillovers of .212, which is not statistically different from the .233 estimate in the main analyses which use a minimum distance of 100 miles.

3.2 Out-of-State Neighbor IV

3.2.1 Baseline Out-of-State Neighbor IV Specification

The out-of-state neighbor instrumental variables models identify fiscal spillovers based on instruments that are even more likely to be exogenous, because their validity lies only on the assumption that unobserved shocks do not spread more than 100 miles across state borders. Spillovers are identified based only on the predicted values for neighboring districts in other states. Furthermore, these out-of-state neighbors' predicted values are based on matches with districts that are not only located at least 100 miles away but are also at least 100 miles away from the relevant state border.[9] The identification thus comes from trends in the neighboring state among districts that are similar to the border districts but are located far away from a district's own state.

For the N school districts in the sample, define S as an N×N matrix with element S_{ij} equal to one if districts *i* and *j* are in the same state and equal to zero otherwise. Define A as an N×N matrix of ones. To identify districts' responses to their mean neighbors' change solely from responses to out-of-state neighbors, one may use S to partition the mean change in neighbors' revenues on the right hand side of equation 2, using instrumented values for out-of-state neighbors' changes and state-by-year means for in-state neighbors' changes:

(3)
$$R_{ijt} - R_{ijt-5} = \beta_1 W_1 (A-S)\widehat{\left(R_{ijt} - R_{ijt-5}\right)} + (\beta_1 W_1 S + \beta_2)\,\gamma_{jt} + \gamma_{jt}\, X_{ijt-5}\beta_{3jt} + e_{ijt}$$
$$e_{ijt} = \lambda W_2\, e_{ijt} + \varepsilon_{ijt}$$
$$\text{with } |\beta_1|<1 \text{ and } |\lambda|<1 \text{ and } \varepsilon_{ijt} \sim N(0,\sigma^2),$$

While the model continues to include all districts in the continental United States, the estimates of fiscal spillovers are identified from β_1 on the first right-hand-side term, the response to predicted changes in out-of-state neighbors' revenues. This estimate of spillovers is based on districts' responses to predicted changes in the mean revenues of their neighbors based on the predicted changes in the revenues of districts' out-of-state neighbors. The next three subsections describe additional out-of-state IV specifications which provide robustness checks for the validity of the instrument and the applicability of the estimates to spillovers between in-state districts.

3.2.2 Out-of-State Neighbors in States with Major, Unique Finance Reforms

There is prior empirical evidence that one state's spending does not influence the spending of contiguous states (Case, Hines, & Rosen, 1993), but if districts in contiguous states have similar expenditure trends, then even the out-of-state neighbor IV might be endogenous. In the extreme case, contiguous states undergo identical finance trends and the estimates from equation 3 face the same potential pitfalls as estimates from equation 2. To examine the importance of this issue, I estimate additional models which identify fiscal spillovers solely from cases in which one state has a significant finance reform while the neighboring state does not have a similar change. One state's education finance reform has differential effects

[9] As with the all-neighbor IV models, 100 miles appears to be a sufficiently far cutoff for eliminating spatial endogeneity. For the main out-of-state IV model, using a 150 mile cutoff would produce an estimate of .181 (.043 standard error); this estimate is fairly close to and not statistically different from .219, the corresponding out-of-state IV estimate in row 4 of Table 2 based on a cutoff of 100 miles.

on its own border districts, and the lack of a similar policy within the neighboring state allows for a relatively clean case of identifying the impact on various border districts in the neighboring state. The exact timing of responses to official state school finance reforms is difficult to predict, and unofficial policy reforms sometimes have significant effects as states adjust their school finance formulas and tax policies. Therefore, I allow the data to reveal the timing of states' substantial education finance changes. I divide each state's districts into three groups based on income and examine whether the mean change in revenues was large for any of these groups. I define a large change as a group having a mean increase of more than $2,000 per pupil or a mean decrease of more than $1,000 per pupil, (in year 2000 dollars), changes equivalent to the 72nd or 22nd percentile in the distribution of individual district-level revenue changes. This definition of major finance changes matches up well with lists of court-ordered and state legislature-initiated school finance equalization programs compiled by Downes and Shah (2006) and Corcoran and Evans (forthcoming): except for Montana and South Dakota, all states with court-ordered or state legislative reforms experienced this type of major finance change at least once during the ten years following these reforms.[10] Next, I identify fiscal spillovers by using the instrumental variable only in cases in which a district has out-of-state neighbors in a state with large changes among one of the income groups and the district's own state did not experience a "similar change" for its corresponding income group. I define a "similar change" as a change that is at least 50% as large as the other state's change and in the same direction. Identifying spillovers from major, unique changes in states' finances reduces the possibility of a biased estimate due to states engaging in similar fiscal policies, though it decreases the number of observations used to identify spillovers by more than 82%.

3.2.3 Out-of-State Neighbors with High Interstate Residential Mobility

There are theoretical reasons why one might suspect responses to out-of-state neighboring districts to differ from responses to in-state neighboring districts. Tax-payers, students, and school employees may tend to be less mobile across inter-state borders than across within-state borders. Fiscal spillovers between out-of-state neighbors may differ depending on whether residential mobility across state borders is limited due to state-level policies or other factors. To investigate this issue, I re-estimate the out-of-state neighbor IV models above restricting the source of identification to responses among border districts with rates of inter-state residential mobility above the median rate for border districts.

---

[10] South Dakota and Montana experienced major finance changes preceding state legislation in 1986 and a court-ordered reform in 1989 respectively, but neither state experienced major finance changes afterwards. The states that experienced major finance changes due to tax reforms or other changes that were not considered formal school finance equalization reforms, (i.e., not listed by Downes & Shah, 2006 or Corcoran & Evans, forthcoming), were Alabama (major changes over each of the 5 year periods spanning 1982-92), Delaware (1982-1992 and 1997-2002), Indiana (1982-1987), Michigan (1982-1997), Mississippi (1982-1992), Nebraska (1977-1992), North Carolina (1977-1992), North Dakota (1977-1982), Oregon (1977-1992), and Pennsylvania (1977-1992). Removing these unofficial cases of major finance changes would increase the standard error of the fiscal spillover estimates but would barely alter the point estimates. For example, Table 3 below reveals that when neighbors are defined as districts located within a thirty mile radius, the out-of-state IV estimate using unique, major finance changes equals .271 (.062 standard error). The corresponding point estimate equals .273 (.158 standard error) if the identification is further restricted to responses occurring within ten years after a court-ordered or legislature-initiated statewide school finance equalization reform.

3.2.4 Out-of-State Neighbor IV Controlling for Border-by-Year Fixed Effects

The out-of-state neighbor IV estimates identify spillovers by comparing a border district's revenue growth to the revenue growth of similar districts in the same state which did not have any out-of-state neighbors, had out-of-state neighbors with different characteristics, or had out-of-state neighbors located in a different state. Some of this identification is thus related to variation in revenue changes across geographic regions within the same state. The estimate of spillovers may be biased if, controlling for initial characteristics, within-state variation in revenue changes are related to unobserved characteristics. For example, suppose that districts in eastern Pennsylvania experienced high revenue growth compared to observationally similar districts in western Pennsylvania, at a time when New Jersey districts increased their revenues far more than Ohio districts increased their revenues. Differences in revenue growth between districts in eastern versus western Pennsylvania may be due to differences in their neighbors' behavior, coincidental differences in omitted variables across the two regions of Pennsylvania, or some combination of both. If omitted variables across two regions of the same state are systematically correlated with the fiscal behavior of the neighboring states, then the previous out-of-state IV estimates of spillovers may be biased. To address this issue, I repeat all of the out-of-state neighbor IV analyses with the addition of controls for border-year fixed effects. For each five year interval, these border-year fixed effects absorb the average revenue change for districts located in the same state with at least one neighboring district in a specific, other state. Continuing the example above, this model would control for the average difference during each time period between the changes in revenues in Pennsylvania districts bordering Ohio and the changes in revenues in Pennsylvania districts bordering New Jersey. The estimates of spillovers are thus identified from the behavior of districts along the same side of the same state border with out-of-state neighbors that possess different initial characteristics.

| 4 | DATA

The analyses use geographic data for every school district in the United States based on the Census TIGER files, which provide centroid coordinates for the districts and allow the researcher to identify which districts share a border. I combine these data with district-level financial panel data available in five year intervals from 1972 to 2002. The financial data for (spring of) 1992, 1997, and 2002 come from the School District Finance Survey (F-33 files), while earlier years come from the Census of Government Files.

The dependent variable in this paper's main analyses equals changes in a district's real operating revenue per pupil. I examine revenues rather than expenditures because revenue information is available for a longer span of years. Observations with suspicious levels of revenues per pupil are removed from the data prior to analysis.[11] I combine these financial

[11] Less than 1% of all observations in the raw data are dropped due to questionable revenue per pupil values. In particular, I drop observations with real operating revenues per pupil below $400 or above $22,000, (measured in year 2002 $). I also drop observations that would suggest a more than $10,000 change in revenue per pupil in one five-year period followed by a level of revenue per pupil five years later that is within $4000 of the original level from ten years prior. It is highly unlikely that any district would actually undergo such a large, temporary change in revenue per pupil.

data with U.S. Census demographic data aggregated to the school district level for 1970, 1980, and 1990.

Some analyses below also include self-collected data concerning variation in the local political processes for determining school district expenditure levels. Political institutions could influence the magnitude or speed of districts' responses to their neighbors' actions. I obtained information concerning local democratic institutions from 1970 to 2002, using surveys of state finance experts, reviews of school finance documents (e.g., U.S. Department of Education, 2001), data from Saiz's (2005) New England municipality interviews with local school officials, and referenda frequency information for 1970-1972 from Hamilton and Cohen (1974).[12] In the 48 continental states, about 37% of all districts currently determine expenditure levels exclusively through local citizens voting directly, 55% determine their expenditure levels through locally elected representatives, and citizens in the remaining 8% of districts do not have much discretion over local public school operating expenditure levels. Analyzing the magnitude and speed of fiscal responses in various local democratic systems may thus provide insights into the political mechanisms by which fiscal spillovers occur. Except where noted, the analyses below exclude districts lacking local discretion over local public school operating expenditure levels, because these districts would not have the capacity to engage in fiscal competition.[13]

A small share of localities experienced school district re-organizations during the sample period, such as mergers between districts or unifications of elementary-level districts with secondary-level districts. While panel studies of education finance usually ignore these mergers or drop all observations for districts which ever re-organized, it may be important to verify that the ensuing sample selection does not have a large effect on the empirical results. The analyses below incorporate historical data concerning any type of school district re-organization. The main analyses include a full set of observations for districts that merged by combining data from the participating districts for observations predating the merger. These

[12] We first surveyed the contributors to "Public School Finance Programs of the U.S. and Canada: 1998-99" (U.S. Department of Education, 2001) from each state regarding the form of local democracy in that state. If necessary, we also contacted state education officials who were members of the American Education Finance Association. While most of the survey responses alluded only to current practices, the information reported in Hamilton and Cohen (1974) allowed us to detect state-level changes in these policies over time. These changes typically coincided with state education finance equalization reforms. Most states with inter-district variation in the form of local democracy are New England states where the school districts coincide with towns and each district's form of democracy matches the municipal form of democracy coded by Saiz (2005). One exception is New Hampshire, which required us to survey each district individually. Anecdotal evidence for districts in places with intra-state variation suggests that these districts tend to retain the same form of democracy over time, but it is possible that district-level longitudinal changes are a source of measurement error in these data.

[13] The data used for the main analyses thus exclude observations for districts in California from 1977 on, Michigan from 1997 on, Nevada from 1977 on, New Mexico from 1977 on, Oregon for 1997, and Wyoming for 2002. California is classified as a no-local-control state, because in 1976 the *Serrano* decision took away virtually all local control of operating expenditure levels. However, California districts have had the option of using a parcel tax to fund some local public school operating expenditures. During the sample period, this parcel tax required approval from two-thirds of district voters and its use was mostly limited to relatively wealthy districts in the northern part of the state. I exclude all California districts from the main analyses because they had relatively little local discretion, but I also account for California's parcel tax option in additional analyses that focus on districts lacking any local discretion.

analyses also control for whether a district re-organized and whether any of a district's neighbors re-organized.[14]

| 5 | RESULTS

5.1 Descriptive Statistics

Table 1 displays how district operating revenues per pupil have changed between 1972 and 2002. Each five year interval was associated with a rise in real mean district revenue per pupil, except for the period between 1972 and 1977 when large growth in student populations outpaced revenue growth. Mean revenues per pupil increased rapidly from the late 1970's through the mid 1980's, as population growth slowed and many states enacted school finance reforms. Changes in a district's own revenues per pupil are highly correlated with changes in the mean neighboring districts' revenues per pupil. This correlation was particularly high for in-state neighbors during the 1980's, when many states enacted school finance equalization policies. The correlation is much higher for in-state neighboring districts than for out-of-state neighboring districts, which is consistent both with common underlying trends for in-state neighbors and with districts being more responsive to changes among their in-state neighbors.

The Appendix displays the means and standard deviations of descriptive variables used to formulate some of the independent variables in the regressions below. Districts with at least one out-of-state neighbor have a higher average population density than other districts, and they also tend to have wealthier residents and slightly higher revenues per pupil than other districts. These differences, which are statistically significant at the .001 level, imply that it may be important to compare the all-neighbor and out-of-state neighbor instrumental variable specifications' estimates specifically among districts with certain characteristics, such as districts that are relatively wealthy or are located in metropolitan areas.

5.2 Power of the Instrumental Variables

Both the out-of-state neighbor IV and all-neighbor IV are powerful predictors of actual mean revenue changes. When actual mean neighbor revenue changes are regressed on the all-neighbor instrument controlling for state-by-year fixed effects, the estimated slope equals .905 with a standard error of .004, and the instrument explains 42% of the within-state-year variation in mean neighbor revenue changes.[15] The high power of this instrument is partly

[14] The models include four independent variables related to re-organizations: an indictor for whether the district re-organized during that time period, an indicator for whether any of the district's neighbor's re-organized during that time period, an indicator for whether the number of neighbors increased from the prior period, and an indicator for whether the number of neighbors decreased from the prior period. Additional analyses, not shown here, test the sensitivity of this specification by instead using a balanced panel containing districts that never underwent any re-organization. Estimates of fiscal competition using this balanced panel are slightly larger than the estimates shown in this paper, with differences of less than 5 cents (.05) for the main specifications below.

[15] These estimates are based on defining neighbors as districts with centroid coordinates that are within a 30 mile radius of each other. Defining neighbors as districts within a twenty mile radius, the estimated coefficient equals .930 with a .004 standard error and explains 40% of the within-state-by-year variation in bordering

due to states frequently changing their education finance formulas, with similar consequences for similar districts within the same state. For the out-of-state neighbor IV, when actual mean revenue changes among out-of-state neighboring districts are regressed on their instrumented values controlling for state-by-year fixed effects, the estimated coefficient equals .897 with a .006 standard error and the instrumental variable explains 46% of the within-state-by-year variation in mean revenue changes of districts' out-of-state neighbors.[16]

5.3 Main Results

Table 2 reveals estimates of fiscal spillovers given various definitions of neighbors and various methodologies. Each estimate represents the impact on a district's own revenues per pupil if the mean neighboring district revenues per pupil increases by one dollar, measured in year 2000 dollars. As discussed previously, these regressions also control for state-year fixed effects, as well as state-year-specific effects of districts' lagged demographic variables. From left to right, each column of Table 2 displays estimates for models which are increasingly likely to only reflect responses to exogenous changes in neighbors' behavior. The first column displays estimates from ordinary least squares regressions. The second column displays estimates from the IV model that uses predicted changes for both in-state and out-of-state neighbors, the third column displays estimates from the IV model that only uses predicted changes for out-of-state neighbors, and the fourth column displays estimates from the IV model that only uses predicted changes for out-of-state neighbors in states with major, unique school finance changes. The samples sizes vary slightly between the rows of Table 2 as the definition of neighboring districts changes, because districts must have at least one neighbor with a valid instrumented value in order to be included in the regression. The sample sizes are the same across each row, because districts need not have an out-of-state neighbor to be included in the out-of-state neighbor IV regressions.

The point estimates in Row 1 of Table 2 suggest that a one dollar increase in the mean operating revenues per pupil of contiguous districts leads to between an 7.3 cent and 26.8 cent increase in a district's own operating revenues per pupil, depending on which instrumental variables specification is used. As expected, the OLS estimates (first column) are greater than the corresponding instrumental variable estimates. The out-of-state neighbor instrumental variables model provides conservative point estimates for overall spillovers, suggesting that districts respond by at least 7.3 cents per pupil for a $1 change in mean neighbors' per pupil revenues. This lower bound point estimate is statistically significant at the .10 level, suggesting that one can be somewhat confident that districts actually respond to changes in their contiguous neighbors' finances. The third instrumental variable specification, which identifies competition solely from unique finance changes in neighboring states, produces an even larger estimate of 14.5 cents, though this estimate is not statistically significant at conventional levels.

---

districts' revenue changes. Defining neighbors as contiguous districts, the estimated coefficient equals .998 with a .004 standard error and explains 43% of the within-state-by-year variation in bordering districts' revenue changes.

[16] These estimates are based on defining neighbors as districts with centroid coordinates that are within a 30 mile radius of each other. Defining neighbors as districts within a twenty mile radius, the estimated coefficient equals .920 with a .008 standard error and explains 47% of the within-state-by-year variation in bordering districts' revenue changes. Defining neighbors as contiguous districts, the estimated coefficient equals .903 with a .009 standard error and explains 44% of the variation in bordering districts' revenue changes.

School district revenues not only respond to revenue changes among contiguous school districts, but they have greater responses to changes among other nearby districts. Rows 2 and 3 of Table 2 respectively display estimates of spillovers among districts located within a twenty mile radius and districts located within a twenty mile radius that have similar median household incomes.[17] A neighboring district has a similar median income if its median income is within 20% of the district's own median income.[18] Estimates of spillovers are between 8 and 22 cents per one dollar change in the mean revenues of districts located within twenty miles, while estimates are between about 14 and 21 cents for districts located within twenty miles and possessing similar median incomes.

Expanding the definition of neighbors to districts within thirty miles, the all-neighbor IV estimates of spillovers are slightly larger and the out-of-state-neighbor IV point estimates of spillovers converge on these all-neighbor IV estimates. Rows 4 and 5 reveal that the point estimates of spillovers are between about 22 and 27 cents when neighbors are defined as districts within thirty miles and between about 17 and 27 cents when neighbors are defined as districts within thirty miles with similar median household incomes. All of these estimates are statistically significant at the .001 level, so one can be very confident of a positive fiscal response to the mean per pupil revenues of districts within thirty miles. These estimates remain statistically significant if one adjusts the standard errors for spatial and serial autocorrelation.[19] Estimates of spillovers do not increase if the definition of neighbors is expanded to include districts beyond thirty miles, nor do they increase if one examines ten year intervals rather than five year intervals. The remaining analyses presented in this paper define neighboring districts as those within a thirty mile radius, because estimates of spillovers are large and similar across the various instrumental variable specifications in row 4 of Table 2.

Table 3 displays estimates of spillovers based on the additional out-of-state neighbor IV specifications described in the previous section. The first two columns of Table 3 are analogous to the estimates in columns 3 and 4 of row 4 of Table 2. For the most inclusive group of out-of-state neighbors, spillovers decrease from about 23 cents to about 14 cents when border-year fixed effects are included. Controlling for border-year fixed effects does not substantially change the estimates based on out-of-state neighbors in states with major, unique finance changes, which is sensible given that there are very few cases in which a state borders two different states that simultaneously undergo substantial finance changes.

To investigate the importance of the permeability across a state border, I re-estimate the out-of-state IV analysis identifying fiscal spillovers solely from the 50% of out-of-state

[17] Throughout this paper, distances between districts are based on distances between their centroid coordinates. Minimum distances between districts' borders are not readily available in these data.

[18] Just over half of all out-of-state districts within a twenty or thirty mile radius meet this income criterion.

[19] The standard errors reported in the tables do not account for potential spatial autocorrelation and serial autocorrelation. Adjusting for autocorrelation increases the standard errors for the 30 mile radius models (row 4 of Table 2) from .01 to .04 for the all-neighbor IV model and from .04 to .05 for the out-of-state neighbors IV model. These adjusted standard errors were found using the GMM estimation proposed by Conley (1999), finding weighted averages of spatial autocovariance terms with weights set to zero if districts are located more than 50 miles away from each other or their observations are more than one time period (5 years) apart. I am grateful to Tim Conley for providing the Matlab program which I adapted for these estimations. Due to the lengthy computational time required, spatially adjusted standard errors were not computed for the other models.

neighboring school districts with the highest rates of inter-state residential mobility.[20] Column 3 of Table 3 displays the estimates for this model, with or without border-year fixed effects. Districts with relatively high rates of inter-state mobility have a greater fiscal response to their out-of-state neighbors, especially if the model does not control for border-year fixed effects. The estimate controlling for border-year fixed effects is arguably more informative, because the potential regional bias described earlier might be particularly strong when residents in multiple regions of a state are highly mobile across different state lines. To the extent that inter-state neighbors across relatively permeable state borders are similar to typical neighboring districts, this estimate suggests the typical level of fiscal spillovers may be about 20 cents per a $1 change in the mean neighbors' revenues; this is fairly close to the all-neighbor IV point estimate of 23 cents.

5.4 Falsification Test

Some U.S. school districts lack local discretion over operating revenues, because the state decides their level of revenues for them. These districts provide a nice falsification test for the instrumental variables models—spillovers should be absent for districts lacking local discretion, because any co-movements in revenues per pupil among neighbors are not due to districts' own fiscal responses. For this falsification test, I re-estimate the models in row 4 of Table 2 using observations from districts lacking control. This sample includes observations from Nevada, New Mexico, and some Californian districts.[21] California removed much local control from its districts after the *Serrano* decision in 1976, but California's districts have also had a parcel tax option for funding local public school operating expenditures. Due to the availability of this parcel tax option, observations from half of all California districts are excluded from the "no local control" sample, specifically districts whose median household income in 1980 was above the median Californian district. These Californian districts had a moderate probability of attempting to adopt a parcel tax to fund local school operating expenditures, while this probability was extremely low for all other Californian districts.[22]

As expected, the out-of-state neighbor IV estimates suggest that districts unable to locally determine their budgets do not respond positively to changes in neighboring districts' revenues. The estimates of spillovers for these districts are -.080 cents (standard error of .165

[20] Estimates suggest that more than 6.4% of the residents living in these districts in the year 2000 had lived in a different state five years earlier. These estimates are available from the 2000 Census Public Use Micro-Sample and unavailable for earlier years, and these mobility rates do not identify the particular states where residents formerly resided. The high mobility group excludes a handful of districts in which more than 50% of residents lived in a different city during the prior 5 years, because inter-state mobility in these districts was generally due to local colleges or military bases rather than typical residential relocation decisions. While high interstate mobility between 1995 and 2000 could possibly be endogenous with respect to fiscal changes during the 1990's, the results remain similar if one focuses on fiscal spillovers between these districts prior to the 1990's.

[21] Observations are excluded if the form of local democracy changed within the past ten years or the next five years, because such a change could lead to residential movement that would influence the estimates.

[22] According to EdSource (2007), between 1983 and 2006, "210 school districts out of nearly 1,000" attempted to pass this type of parcel tax, and "about 90% of the elections were held in districts that were below the state average of 49% low-income students." Given that a non-trivial portion of the wealthier Californian districts exercised local discretion, including them in the "no-local-control" group would increase the estimates of fiscal spillovers among "no-local-control" districts from -.080 to -.075 for the out-of-state neighbor IV model, from -.091 to -.052 for the out-of-state neighbor IV model with border-year fixed effects, and from .084 to .197 for the all-neighbor IV model.

cents) for the out-of-state neighbor IV model and -.091 cents (standard error of .200 cents) for the out-of-state neighbor IV model controlling for border-year fixed effects. While the point estimates for these districts are negative, there are large standard errors due to the small sample size for this group and the estimates are statistically insignificant.

For the all-neighbor IV model, the estimate of fiscal spillovers is positive for districts lacking local control, but is not statistically significant at conventional levels. The estimated coefficient equals .084 cents with a .065 cent standard error and a p-value of .200.

5.5 Discussion of Results and Exploration of Heterogeneous Effects

The results in Tables 2 and 3 suggest that fiscal spillovers between districts are, on average, in the neighborhood of 20 cents per a $1 change in mean neighbors' revenues, where the relevant set of neighbors is districts located within a thirty mile radius. The instrumental variables estimates are considerably smaller than the ordinary least squares estimate of about 38 cents. The most conservative instrumental variables estimate is the 14.2 cent response from the out-of-state neighbor IV model that controls for border-year fixed effects. It is reassuring that the out-of-state IV estimates remain similar when identification is based only on states with unique school finance reforms, and it is also reassuring that these models do not suggest positive spillovers among districts that lack local discretion over operating revenues.

The all-neighbor IV model produces slightly larger estimates than the out-of-state IV neighbor model, unless the out-of-state IV model focuses on districts with relatively high inter-state residential mobility. This begs the question: Are the slightly greater estimates for the all-neighbor IV model due to a small bias or due to heterogeneity in spillovers that causes in-state spillovers to differ from inter-state spillovers? There might be a small upward bias in the all-neighbor IV model, given that this model produced a positive coefficient of .084 cents for the no-local-control sample of districts. On the other hand, this .084 cent estimate was not statistically significant, its magnitude would not necessarily apply to districts in other states, and there may be legitimate reasons why local fiscal spillovers are weaker across state borders.

Additional analyses help reveal which mechanisms are most responsible for fiscal spillovers and provide evidence supporting legitimate explanations for differences between within-state and between-state spillovers. The remainder of this section describes heterogeneity in fiscal spillovers along several dimensions: the form of local democracy used to determine local school revenues, whether the district is located in a Metropolitan Statistical Area, whether the district was initially wealthier than its neighbors, and whether the district was initially outspending its neighbors. The results suggest that differences between the fiscal spillover estimates across the instrumental variable specifications stem from districts that are initially wealthier than all of their neighbors. These regionally wealthy districts engage in fiscal competition with their in-state neighbors but not with their out-of-state neighbors.

Table 4 displays the results of separate regressions which add an indicator for districts' initial status compared to their neighbors, as well as an interaction term between this indicator and the predicted change in neighboring districts' revenues. Panels (i) and (ii) of Table 4 reveal that, for the out-of-state neighbor IV models, spillovers are strongest when a district's initial median household income exceeds the mean of its neighbors but does not exceed the median household income of *every* neighbor. In fact, fiscal spillovers are very small and statistically insignificant in these models if a district's median household income already exceeds that of all of its neighbors. In contrast, fiscal spillovers in the all-neighbor IV model are similar regardless of districts' initial income positions. This result might be due to state policy differences in terms of the progressiveness of taxation and school expenditures across state lines; relatively wealthy households residing near state borders likely choose to reside in the state with fewer redistributive policies, and cross-state differences in progressiveness overshadow marginal changes in the tax-expenditure bundles of nearby, out-of-state districts. Only the districts that are the wealthiest in their region appear to be immune to the behavior of their out-of-state neighbors—for all other districts, fiscal spillovers are very similar across the all neighbor IV model and out-of-state neighbor IV models.

Additional analyses suggest that local fiscal spillovers are asymmetric and that the prior estimates may be very slightly biased downwards due to reversion to the mean. Panel (iii) of Table 4 reveals that districts starting with relatively low revenues increase their revenues by more than $1,000 per pupil, on average, compared to other districts. This difference in intercepts suggests that there is substantial mean reversion in terms of district revenues within local regions. While districts with relatively high revenues per pupil have a smaller intercept due to this mean reversion, the slope associated with responses to changes in neighbors' revenues is much larger for these relatively high spending districts. For the model using the out-of-state neighbor instrumental variable controlling for border-year fixed effects, districts initially possessing greater per pupil revenues than their neighbors raise an additional 37.6 cents per pupil for a one dollar increase in the average per pupil revenues of their neighbors; in contrast, districts initially possessing lower per pupil revenues than their neighbors respond to their neighbor' revenues changes with a slope of 12.6 cents per pupil. For the all-neighbor IV model, these estimated slopes equal 28.9 cents and 18.3 cents respectively. Local spillovers are less often a matter of "keeping up with the Joneses" than a matter of "staying ahead of the Joneses."

Metropolitan area status provides another interesting source of heterogeneity in the size of spillovers. I classify districts as located inside an MSA (Metropolitan Statistical Area) based on their classification in the 2000 Census. One might expect densely populated metropolitan areas to engage in greater degrees of fiscal competition, because tax rates and public school popularity may be more readily capitalized into house prices in these areas and because residents may be relatively well informed of the behavior of surrounding districts. Panel (iv) of Table 4 confirms that responses to the revenue changes of out-of-state neighbors are in fact strongest among districts located in Metropolitan Statistical Areas (MSA's). Fiscal spillovers across state borders are more than 40% greater in MSA's than in non-MSA's. More surprisingly, fiscal spillovers in the all-neighbor IV model are strongest outside of MSA's, with a 23.8 cent response for non-MSA districts versus a 12.9 cent response for districts in MSA's.

Additional empirical analyses suggest these differences are related to residential mobility. Changes in the tax-expenditure bundle offered by one district can cause some of its residents to move to other, nearby districts, and can marginally influence the location decisions of people moving to the area. The additional analyses empirically test for the impact of changes in neighboring districts' revenues on the residential composition of a district; they are similar to the main analyses, except the dependent variable is changed to the median household income in the school district.[23] Given that there will be multiple, simultaneous changes occurring in districts' expenditures and demographics, these analyses simply reveal reduced-form relationships associated with districts moving from one equilibrium to another. The results suggest that an increase in the revenues of in-state, neighboring districts is associated with a decrease in a district's own median household income if and only if the district is located in a metropolitan area. In particular, the all-neighbor IV model suggests that a $1000 increase in mean revenues of neighboring districts leads to an $86 *decrease* in a metropolitan districts' own household income but a $58 *increase* in a non-metropolitan districts' own household income, a difference of $144 that is statistically significant at the .01 level. Rising school revenues in nearby metropolitan area districts likely lure away relatively wealthy households, thus lowering the median income of a district's own residents in spite of the district's positive fiscal response to its neighbors. Given that local public education spending is a normal good, this change in residential composition attenuates the district's positive fiscal response. Changes in household income might be symptomatic of more general changes whereby, particularly in metropolitan areas, an increase in neighboring districts' revenues leads to an influx of residents with a relatively low demand for public school services.[24]

The magnitude of fiscal spillovers is also influenced by the type of political institution used to make local school tax revenue decisions. Table 5 displays regression results for models dividing the sample based on the form of local democracy. The sample is divided based on the three categories described in Section 4: (1) districts without any local discretion, (2) districts using representative democracy, and (3) districts exclusively using direct democracy. As discussed earlier, the estimates of fiscal spillovers are statistically insignificant and relatively small for districts without any local discretion. The estimates of fiscal spillovers are slightly larger for districts using direct democracy than for districts using representative democracy. For either of these forms of local democracy, the in-state IV estimates are only slightly larger than the out-of-state IV estimates. Elected school officials, (e.g., superintendents, school board members, etc.) are often agenda-setters in districts with direct democracy, choosing which tax rates or spending levels to submit for voter approval. The similarity of the size of fiscal spillovers across the two forms of democracy suggests that these

---

[23] Demographic variables are only available at ten year intervals corresponding with Census years. Therefore, instead of using five year intervals as in the previous analyses, these models regress median household income on the predicted changes in neighboring districts' operating revenues per pupil over the prior ten years. These models use the same set of control variables as in the previous analyses, so the control variables include lagged median household income and other lagged demographic characteristics. A limitation of these analyses is that the fiscal and demographic data are not available in precisely the same year, so the Census year median household income variable (i.e., 1980, 1990, and 2000) is actually regressed on predicted ten year changes in neighboring district revenues that slightly overlap these Census years (i.e., 1972 to 1982, 1982 to 1992, and 1992 to 2002). These analyses thus fail to capture the impact of any unexpected statewide revenue change occurring in the two years immediately following each Census.

[24] While the differential impact on median household income can partially explain why metropolitan districts have smaller net fiscal spillovers, this income differential alone is not large enough to explain the entire .109 difference in the slopes of fiscal spillovers between metropolitan and non-metropolitan districts— the elasticity of local school operating revenues with respect to median household income would have to be as high as 4 in order to explain this entire difference.

officials may face similar objectives and constraints regardless of whether the district uses direct or representative voting to determine operating revenues.

Some districts with local control might actually be constrained in terms of their local expenditure decisions, because many states limit school district revenues or expenditures. Brueckner & Saavedra (2001), for example, find that tax competition between Boston-area municipalities disappeared after the arrival of Massachusetts' Proposition 2 ½. To explore this issue in the school district context, I also tested for heterogeneous levels of fiscal spillovers based on the timing of states' adoption of tax or expenditure limits. On average, states' adoptions of these policies did not decrease the magnitude of spillovers between school districts.[25] This result may stem from the fact that tax and expenditure limits are typically binding for only a subset of districts in a particular state, and districts might be strongly influenced by their neighbors when deciding whether to raise the maximum allowable revenues.

[25] I defined states as restricting districts' ability to increase expenditures if the state limited district expenditures, limited district revenues, or limited both local property taxes and property value assessments. This classification is similar to one adopted by Figlio (1997) and by Downes (2007). Like Downes, I use information presented by Mullins and Wallin (2004) to identify the timing of states' adoptions and removals of these policies. While the adoption of these policies is certainly non-random, this specification assumes that their adoption is not related to unobserved variables which influence the magnitude of fiscal competition. Controlling for state-specific slopes of fiscal spillovers and controlling for year-specific slopes of fiscal spillovers, one cannot reject the null hypothesis ($p \leq .05$) that states' tax/expenditure limits do not affect fiscal spillovers between districts. The sign of the estimates are actually consistent with state limits increasing spillovers, as the policies are associated with a 11.3 cent increase in spillovers for the all-neighbor IV model (6.4 cent standard error) and a 17.5 cent increase for the out-of-state neighbor IV model (21.2 cent standard error).

| 6 | Conclusion

Districts appear to directly respond to changes in neighboring districts' revenues. The largest responses to mean changes in the revenues of nearby districts are found when the set of "neighboring" districts is based on districts located within a thirty mile radius. Point estimates suggest that, on average, a one dollar increase in the mean revenues per pupil among a district's neighbors will cause the district to increase per pupil operating revenues by about 20 cents. Conservative estimates, which are as low as 14 cents, are based on predicted changes for out-of-state neighboring districts' spending and may understate overall fiscal spillovers because some districts are less responsive to out-of-state neighbors' behavior than other neighbors' behavior. Other estimates are based on predicted changes for all neighboring districts' spending. These results are robust to a variety of specifications, several of which suggest that the mean level of fiscal spillovers could be slightly greater than 20 cents.

There is a great deal of heterogeneity in the size of these fiscal responses depending on the initial relative position of a district's revenues compared to those of its neighbors. The responses are much greater in districts that were already outspending their neighbors. As expected, there is not any evidence of fiscal responses from districts lacking local control over their public school operating revenues. Fiscal spillovers are slightly greater in districts using direct local democracy than in districts where elected representatives determine the size of local taxes and spending. Spillovers between in-state, neighboring districts are smaller in metropolitan areas than in non-metropolitan areas, and additional analyses suggest that this is at least partially due to residential sorting. In metropolitan areas, where households may be especially mobile across district lines, an increase in the mean revenues of nearby, in-state districts leads to a decrease in a district's own median household income. General equilibrium effects of neighboring districts' spending changes are an important topic for further study. Fiscal spillovers may be related to non-residential property wealth, which has been an understudied topic due to the scarcity of district-level data accurately measuring the non-residential portion of the property tax base. Future research might also investigate fiscal spillovers between private schools and public schools.

This paper's findings concerning the size and direction of local fiscal spillovers have important implications for the optimal design of school finance plans. Policies that focus on increasing revenues in relatively low-spending districts could indirectly lead to substantial increases in the revenues of other, nearby districts. Policy makers hoping to narrow expenditure gaps across districts must recognize that narrowing these gaps is akin to hitting a moving target. When policies aim at boosting the expenditures of low spending districts, the higher spending neighboring districts respond by further increasing their own expenditures.

REFERENCES

Babcock, Linda, Engberg, John and Robert Greenbaum. (2005). “Wage spillovers in public sector contact negotiations: the importance of social comparisons.” *Regional Science and Urban Economics 35*, 395-416.

Baicker, Katherine. (2004). “The Budgetary Repercussions of Capital Convictions.” *Advances in Economic Analysis and Policy* 4(1), article 6.

Baicker, Katherine. (2005). “The Spillover Effects of State Spending.” *Journal of Public Economics* 89, pp. 529-544.

Besley, Timothy and Anne Case. “Incumbent Behavior: Vote-Seeking, Tax-Setting, and Yardstick Competition.” *American Economic Review*, (1995).

Brueckner, Jan K. “A Tiebout/Tax-competition Model” *Journal of Public Economics* 77(2), (2000).

Brueckner, Jan K. & Saavedra, Luz Amparo. “Do Local Governments Engage in Strategic Property-Tax Competition?” *National Tax Journal* 54, (2001).

Case, Anne C., James R. Hines Jr., and Harvey S. Rosen. “Budget Spillovers and Fiscal Policy Interdependence: Evidence from the States.” *Journal of Public Economics* 52, (October 1993).

Conley, Timothy G. “GMM Estimation with Cross Sectional Dependence,” *Journal of Econometrics* 92, (1999), pp. 1-45.

Corcoran, Sean P. and Evans, William N. “Equity, Adequacy, and the Evolving State Role in Education Finance,” in *Handbook of Research in Education Finance and Policy,* ed. Helen F. Ladd and Edward B. Fiske, Lawrence Erlbaum Publishers, forthcoming.

Downes, Thomas A. “Do Non-school Resources Substitute for School Resources? A Review of the Evidence.” (2007), mimeo, Stanford University Institute for Research on Education Policy & Practice.

Downes, Thomas A. and Mona P. Shah. “The Effect of School Finance Reforms on the Level and Growth of Per-pupil Expenditures.” *Peabody Journal of Education* 81(3), (2006), pp. 1-38.

EdSource, Inc. “California School Finance: Parcel Taxes.” http://californiaschoolfinance.org/FinanceSystem/Revenues/ParcelTaxes/tabid/121/Default.aspx. accessed on 5/24/07, (2007).

Figlio, David, Van Kolpin, and William Reid. “Do States Play Welfare Games?” *Journal of Urban Economics* 46, (1999).

Fletcher, Deborah and Lawrence W. Kenny. “The Influence of the Elderly on School Spending in a Median Voter Framework.” University of Florida working paper, (2007).

Figlio, David N. (1997). “Did the ‘Tax Revolt’ Reduce School Performance?” *Journal of Public Economics* 65, pp. 245-269.

Hamilton, Howard D. and Cohen, Sylvan H. (1974). *Policy Making by Plebiscite: School Referenda*. D.C. Heath & Company, Lexington, MA, p. 4.

Millimet, Daniel L. and Vasudha Rangaprasad. “Strategic Competition Amongst Public Schools.” *Regional Science and Urban Economics*, forthcoming.

Mullins, Daniel R. and Wallin, Bruce A. (2004). "Tax and Expenditure Limitations: Introduction and Overview." *Public Budgeting & Finance* 24, pp. 2-15.

Nechyba, Thomas. (2003). “Public School Finance and Urban School Policy: General versus Partial Equilibrium Analysis.” *Brookings-Wharton Papers on Urban Affairs*: 2003,

Public School Finance Programs of the United States and Canada: 1998–99. U.S. Department of Education, National Center for Education Statistics. NCES 2001–309; Compiled by Catherine C. Sielke, John Dayton, C. Thomas Holmes, of The University of Georgia and Anne L. Jefferson of the University of Ottawa. William J. Fowler, Jr., Project Officer. Washington, DC, (2001).

Saiz, Albert. “The Median Voter Didn’t Show Up: Representative Democracy and Public Employee’s Wages.” Mimeo, Wharton, 2005.

Tiebout, Charles M. “A Pure Theory of Public Expenditures.” *The Journal of Political Economy* 64(5), (1956), pp. 416-424.

U.S. Department of Education, National Center for Education Statistics. (2001). *Public School Finance Programs of the United States and Canada: 1998–99.* NCES 2001–309. Compiled by Catherine C. Sielke, John Dayton, C. Thomas Holmes. William J. Fowler, Jr., Project Officer. Washington, DC: 2001.

Wilson, John D. (1999). “Theories of Tax Competition.” *National Tax Journal* 52(2).

Wilson, John D. and Roger H. Gordon (2003). “Expenditure Competition.” *Journal of Public Economic Theory* 5.

Table 1: Changes in School District Operating Revenues per Pupil, 1972-2002

Time Period	Mean (Year 2000 $)	Standard Deviation	Correlation with Change in Operating Revenues per Pupil Among…		
			All contiguous neighbors	In-state contiguous neighbors	Out-of-state contiguous neighbors
1972-77	-1,876	1,833	.516	.515	.287
1977-82	2,666	2,060	.518	.517	.328
1982-87	1,490	3,418	.646	.656	.138
1987-92	571	3,945	.760	.767	.293
1992-97	594	1,609	.341	.343	.074
1997-2002	1,425	1,748	.269	.275	.051

Table 2: Estimates of Fiscal Spillovers between U.S. School Districts

Definition of Neighbors	OLS	Instrumental Variables Model Identified Using… All neighbors	Out-of-state neighbors	Out-of-state neighbors in states with major, unique finance changes
(1) Contiguous School Districts (*N=65,947)*	.351 (.007)	.268 (.010)	.073 (.043)	.092 (.071)
(2) Districts within a 20 Mile Radius (*N=63,200)*	.331 (.007)	.217 (.010)	.138 (.045)	.084 (.077)
(3) Districts within a 20 Mile Radius and with Similar Median Household Income (*N=60,522)*	.271 (.007)	.211 (.010)	.146 (.044)	.137 (.072)
(4) Districts within a 30 Mile Radius (*N=65,566)*	.384 (.008)	.233 (.011)	.219 (.038)	.271 (.062)
(5) Districts within a 30 Mile Radius and with Similar Median Household Income (*N=64,383)*	.312 (.008)	.230 (.011)	.174 (.037)	.272 (.060)

Notes to Table 2: Each cell represents a separate regression and reveals the estimated change in a district's operating revenue per pupil from a one dollar increase in the average operating expenditures per pupil among neighboring districts during five year intervals from 1972 to 2002. Each regression has a sample size of 65,566, since observations are included even if they do not have any relevant neighbors. All values are in year 2000 dollars. Each regression controls for state-year fixed effects, as well as for the state-year specific effects of demographic variables. The regressions also control for recent district re-organizations. The sample excludes districts which lack much local discretion over operating revenues per pupil.

Table 3: Additional Estimates of Fiscal Spillovers between Districts Located within Thirty Miles of each other Across State Borders

	All out-of-state neighbors	Out-of-state neighbors in states with major, unique finance changes	Out-of-state neighbors with high inter-state residential mobility
Baseline	.219	.271	.370
	(.038)	(.062)	(.074)
Controlling for border-year fixed effects	.142	.274	.200
	(.055)	(.094)	(.098)

Notes to Table 3: See Notes to Table 2. Neighboring districts are defined as having high inter-state residential mobility if they were above the median rate of inter-state moves for border districts based on the 2000 Census, (see footnote 20).

Table 4: Heterogeneous Estimates of Fiscal Spillovers Based on Initial Characteristics Compared to Neighbors or Based on Location in a Metropolitan Area

	Estimated coefficients based on model with: Out-of-state Neighbors IV	Out-of-state Neighbors IV controlling for state-year border fixed effects	All Neighbors IV
(i) D_i =1 if District Initially Had Lower Median Household Income than Average of Neighbors' Median Houshold Income			
Predicted Change in Mean Neighbors' Revenues	.262	.174	.179
	(.044)	(.060)	(.011)
" " " * D_i	-.073	-.058	.001
	(.038)	(.039)	(.009)
D_i	.013	.012	.016
	(.019)	(.019)	(.019)
(ii) D_i = 1 if District Initially Has a Greater Median Household Income Than Every Neighbors' Median Household Income			
Predicted Change in Mean Neighbors' Revenues	.246	.169	.181
	(.039)	(.057)	(.010)
" " " * D_i	-.228	-.167	-.021
	(.076)	(.081)	(.017)
D_i	-.003	-.010	-.007
	(.035)	(.036)	(.037)
(iii) D_i = 1 if District Initially Had Lower Revenues per Pupil than Avg. Neighbors' Revenues per Pupil			
Predicted Change in Mean Neighbors' Revenues	.508	.376	.289
	(.039)	(.054)	(.010)
" " " * D_i	-.278	-.250	-.106
	(.034)	(.035)	(.007)
D_i	1.10	1.11	1.17
	(.014)	(.014)	(.015)
(iv) D_i = 1 if District is Located in a Metropolitan Statistical Area (based on MSA boundaries in 2001)			
Predicted Change in Mean Neighbors' Revenues	.162	.119	.238
	(.042)	(.058)	(.011)
" " " * D_i	.160	.081	-.109
	(.049)	(.057)	(.011)
D_i	-.030	-.020	.068
	(.022)	(.022)	(.023)

Notes to Table 4: Neighboring districts are defined as those located within a 30 mile radius. Each column of each panel provides three estimated coefficients from a single regression.

Table 5: Heterogeneous Estimates of Fiscal Spillovers Based on Local Democracy

Form of Local Democracy for Determining Public School Operating Expenditures	Out-of-state Neighbors IV	Out-of-state Neighbors IV controlling for state-year border fixed effects	All Neighbors IV
(1) No Local Control	-.080	-.091	.084
	(.165)	(.197)	(.065)
(2) Representative Democracy or Mixed Democracy	.153	.122	.167
	(.055)	(.082)	(.012)
(3) Direct Democracy Only	.260	.142	.189
	(.053)	(.076)	(.016)

Notes to Table 5: Point estimates are from regressions analogous to the models in the second and third columns of Table 2, except that the sample is divided based on districts' form of local democracy for determining operating expenditures. The sample also excludes observations for districts that recently changed their form of local democracy; observations are dropped if the form of local democracy changed within the past ten years or next five years. Similar to panel (4) of Table 2, neighboring districts are defined as districts located within a thirty mile radius.

Appendix: Summary Statistics for School District Characteristics,
Means with Standard Deviations in Italics

	Full Sample	Districts in sample with at least one out-of-state neighbor within 30 miles[a]
Number of Districts	65,566	21,776
Operating revenues per pupil	$5,683	$5,783
(at the beginning of each 5 year period)	*$2,922*	*$3,144*
5-year change in operating revenues per pupil	$805	$900
	$2,851	*$2,682*
Variables based on the 1980 Census[b]		
Median Household Income (year 2000 $)	$34,856	$37,299
	$11,126	*$13,079*
Mean House Value (year 2000 $)	$92,758	$101,349
	$41,957	*50,799*
% Ages 5-17	22.1%	21.9%
	3.3%	*3.2%*
% Ages 65 & over	12.6%	12.5%
	4.7%	*4.5%*
Population Density (people per 100 sq ft.)	.027	.044
	.080	*.112*

[a] In this paper's main analyses, neighboring districts are defined as districts with centroid coordinates located within thirty miles of each other.
[b] Census variables are limited to the 1980 year in this table in order to facilitate comparisons of characteristics of districts with or without out-of-state neighbors. The actual regression analyses control for the state-by-year effects of variables based on the immediate prior Census data (1970, 1980, or 1990).

ISERP Working Papers

2007

07-09: "Long-Term Effects of a Recession at Labor Market Entry in Japan and the United States," Yuji Genda, Institute of Social Science, University of Tokyo, Ayako Kondo, ISERP Graduate Fellow, Economics, Columbia University, Souichi Ohta, Economics, Keio University

07-08: "Simultaneous Estimation of Hedonic Equations with Unbalanced Data," Valerie A. Mueller, Earth Institute, Columbia University, Glenn Sheriff, School of International and Public Affairs, Columbia University

07-07: "Poverty Analysis Based on Kernel Density Estimates from Grouped Data," Camelia Minoiu, Economics, Columbia University

07-06: "Does Gaming the System Affect Students' Academic Achievement?" Jennifer Booher-Jennings, Sociology, Columbia University, Andrew A. Beveridge, Sociology, Queens College and CUNY Graduate Center

07-05: "Differential Effects of Graduating During a Recession across Race and Gender," Ayako Kondo, Economics, Graduate Fellow, ISERP, Columbia University

07-04: "PowerPoint Demonstrations: Digital Technologies of Persuasion," David Stark, Sociology, Columbia University, Verena Paravel, Center on Organizational Innovation, ISERP, Columbia University

07-03: "No Entiendo: The Effects of Bilingualism on Hispanic Earnings," Jeronimo Cortina, Political Science, Columbia University, Rodolfo de la Garza, Political Science and International Affairs and Public Affairs, Columbia University, Pablo Pinto, Political Science, Columbia University

07-02: "The Assessment of Poverty and Inequality through Parametric Estimation of Lorenz Curves," Camelia Minoiu, Economics, Columbia University, Sanjay Reddy, Barnard Economics

07-01: "Implementing Second-Best Environmental Policy under Adverse Selection," Glenn Sheriff, School of International and Public Affairs, Columbia University

2006

06-01: "The Impact of Parental Marital Disruption on Children's Performance in School," Christopher Weiss, ISERP, Columbia University, Kathleen Foley, University of Pennsylvania

06-02: "The Choice of Index Number: Part I, Valuation and Evaluation," Sanjay Reddy, Barnard Economics, Benjamin Plener, Yale University

06-03: "Real Income Stagnation of Countries, 1960-2001," Sanjay Reddy, Barnard Economics, Camelia Minoiu, Economics, Columbia University

06-04: "Chinese Poverty: Assessing the Impact of Alternative Assumptions," Sanjay Reddy, Barnard Economics, Camelia Minoiu, Economics, Columbia University

06-05: "Spaghetti Politics," Paolo Parigi, Sociology, Columbia University, Peter Bearman, Sociology, Columbia University

06-06: "Attention Felons: Evaluating Project Safe Neighborhoods in Chicago," Andrew Papachristos, University of Chicago, Tracey Meares, University of Chicago, Jeffrey Fagan, Law, Columbia University

06-07: "Dynamics of Political Polarization," Delia Baladassarri, Columbia University, Peter Bearman, Columbia University

06-08: "Why do Some Countries Produce So Much More Output per Worker than Others?" Emmanuel Pikoulakis, University of Hull Business School, Camelia Minoiu, Economics, Columbia University

06-09: "Trivers-Willard at Birth and One Year: Evidence from U.S. Natality Data 1983-2001," Douglas Almond, Economics, Columbia University, Lena Edlund, Economics, Columbia University

06-10: "Forecasting House Seats from General Congressional Polls," Robert Erikson, Political Science, Columbia University

06-11: "From Drafts to Checks: The Evolution of Correspondent Banking Networks and the Formation of the Modern U.S. Payments System, 1850-1914," John James, Economics, University of Virginia, David Weiman, Economics, Barnard College, and History, Columbia University

2005

05-01: "Social Construction of Flows: Price Profiles Across Producers Gear to Market Context Upstream, Downstream and Cross-Stream," Harrison White, Sociology, Columbia University

05-02: "Temporality and Intervention Effects: Trajectory Analysis of a Homeless Mental Health Program," Mary Clare Lennon, Public Health, Columbia University, William McAllister, ISERP, Li Kuang, Public Health, Columbia University, Daniel Herman, Public Health, Columbia University

05-03: "Do Parents Help More Their Less Well-off Children?: Evidence from a Sample of Migrants to France," François-Charles Wolff, Université de Nantés, Seymour Spilerman, Sociology, Columbia University, and Claudine Attias-Donfut, Caisse Nationale d'Assurance Vieillesse

05-04: "Politics, Public Bads, and Private Information," Glenn Sheriff, International and Public Affairs, Columbia University

05-05: "Determinants of Justification and Indulgence," Ran Kivetz, School of Business, Columbia University, Yuhuang Zheng, School of Business, Columbia University

05-06: "Political Competition and Policy Adoption: Market Reforms in Latin American Public Utilities," Victoria Murillo, International and Public Affairs, Columbia University, Cecilia Martinez-Gallardo, Centro de Investigación y Docéncia Económica

05-07: "In Search of Lost Memories: Domestic Spheres and Identities in Roman Amheida, Egypt," Anna Lucille Boozer, Anthropology, ISERP Graduate Fellow, Columbia University

05-08: "Global Links, Local Roots: Varieties of Transnationalization and Forms of Civic Integration," David Stark, Sociology, Columbia University, Balazs Vedres, Central European University, Laszlo Bruszt, European University Institute

05-09: "Socio-Technologies of Assembly: Sense-Making and Demonstration in Rebuilding Lower Manhattan," Monique Girard, ISERP, Columbia University, David Stark, Sociology, Columbia University

2004

04-01: "Reducing Bias in Treatment Effect Estimation in Observational Studies Suffering from Missing Data," Jennifer Hill, International and Public Affairs, Columbia University

04-02: "Production Markets Broker Upstream to Downstream, balancing their volume and quality sensitivities to firms through an oriented market profile of signals," Harrison C. White, Sociology, Columbia University

04-03: "Measuring Economic Disadvantage During Childhood: A Group-Based Modeling Approach," Robert L. Wagmiller, Jr., SUNY Buffalo, Mary Clare Lennon, Public Health, Columbia University, Philip M. Alberti, Public Health, Columbia University, and J. Lawrence Aber, New York University

04-04: "Policymaking and Caseload Dynamics: Homeless Shelters," William McAllister, ISERP, and Gordon Berlin, Columbia University

04-05: "Fresh Starts: School Form and Student Outcomes," Christopher Weiss, ISERP, Columbia University and Peter S. Bearman, Sociology, ISERP, Columbia University

04-06: "Parental Wealth Effects On Living Standards and Asset Holdings: Results From Chile," Florencia Torche, Sociology, Queens College, Center for the Study of Wealth and Inequality, Columbia University and Seymour Spilerman, Sociology, Center for the Study of Wealth and Inequality, Columbia University

04-07: "Routes into Networks: The Structure of English Trade in the East Indies, 1601-1833," Emily Erikson, Sociology, ISERP, Columbia University and Peter Bearman, Sociology, ISERP, Columbia University

2003

03-01: "The Plasticity of Participation: Evidence From a Participatory Governance Experiment," Shubham Chaudhuri, Economics, Columbia University, and Patrick Heller, Sociology, Brown University

03-02: "Factional Politics and Credit Networks in Revolutionary Vermont," Henning Hillmann, Sociology, Columbia University

03-03: " 'Active Patients' in Rural African Health Care: Implications for Welfare, Policy and Privatization," Kenneth L. Leonard, Economics, Columbia University

03-04: "Living at the Edge: America's Low-Income Children and Families," Hsien-Hen Lu, Public Health, Columbia University, Julian Palmer, Younghwan Song, Economics, Union College, Mary Clare Lennon, Public Health, Columbia University, Lawrence Aber, Public Health, Columbia University

2002

02-01: “Alternative Models of Dynamics in Binary Time-Series-Cross-Section Models: The Example of State Failure,” Nathaniel Beck, Political Science, UC San Diego, David Epstein, Political Science, Columbia, Simon Jackman, Political Science, Stanford and Sharyn O’Halloran, Political Science, Columbia

02-03: “Link, Search, Interact: The Co-Evolution of NGOs and Interactive Technology,” Jonathan Bach, Center on Organizational Innovation, Columbia University and David Stark, Center on Organizational Innovation, Columbia University

02-04: “Chains of Affection: The Structure of Adolescent Romantic and Sexual Networks,” Peter Bearman, Institute for Social and Economic Research and Policy, Columbia University, James Moody, Sociology, Ohio State, Katherine Stovel, Sociology, University of Washington

02-05: “Permanently Beta: Responsive Organization in the Internet Era,” Gina Neff, Center on Organizational Innovation (COI), Columbia University, and David Stark, Center on Organizational Innovation (COI), Columbia University

02-06: “Negotiating the End of Transition: A Network Approach to Political Discourse Dynamics, Hungary 1997,” Balázs Vedres, Columbia University, Péter Csigó, Ecole des Hautes Etudes en Sciences Sociales

02-07: “The Influence of Women and Racial Minorities Under Panel Decision-Making in the U.S. Court of Appeals,” Sean Farhang, Political Science, Columbia University, Gregory Wawro, Political Science, Columbia University

02-08: “The Role of Effort Advantage in Consumer Response to Loyalty Programs: The Idiosyncratic Fit Heuristic” Ran Kivetz, Business, Columbia University, Itamar Simonson, Business, Stanford University

2001

01-01: “Pathways of Property Transformation: Enterprise Network Careers in Hungary, 1988-2000 Outline of an Analytic Strategy,” David Stark, Sociology, Columbia and Balázs Vedres, Sociology, Columbia

01-02: “Policy Space and Voting Coalitions in Congress: the Bearing of Policy on Politics, 1930-1954,” Ira Katznelson, John Lapinski, and Rose Razaghian, Political Science, Columbia

01-03: “Doing Fractions: An Analysis of Partisan ship in Post-Socialist Russia,” Andrew D. Buck, Sociology, Columbia

01-04: “Opposite-Sex Twins and Adolescent Same-Sex Attraction,” Peter Bearman, Sociology/ISERP and Hannah Brückner, Sociology, Yale

01-05: “On the Uneven Evolution of Human Know-How,” Richard R. Nelson, Business/SIPA, Columbia

01-06: “Self-Control for the Righteous: Toward a Theory of Luxury Pre-Commitment,” Ran Kivetz, Business, Columbia and Itamar Simonson, Business, Stanford

01-07: “Distributing Intelligence and Organizing Diversity in New Media Projects,” Monique Girard, ISERP, Columbia and David Stark, Sociology, Columbia

01-08: “Agricultural Biotechnology’s Complementary Intellectual Assets,” Gregory D. Graff, Agricultural and Resource Economics, Berkeley, Gordon C. Rausser, Agricultural Economics, Berkeley and Arthur A. Small, SIPA/Earth Institute, Columbia

For copies of ISERP Working Papers
Visit http://www.iserp.columbia.edu/research/working_papers/
write to iserp@columbia.edu or call 212-854-3081

ISERP
INSTITUTE FOR SOCIAL AND ECONOMIC RESEARCH AND POLICY

Institute for Social and Economic Research and Policy
Columbia University in the City of New York
420 West 118th Street
8th Floor, Mail Code 3355
New York, NY 10027
Tel: 212-854-3081
Fax: 212-854-8925
Email: iserp@columbia.edu
www.iserp.columbia.edu

OCTOBER 2007