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Can the land tax help curb urban sprawl? Evidence from growth patterns in Pennsylvania

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ABSTRACT

Urban sprawl has become a policy concern of national prominence. One tool that has been suggested for combating sprawl is the land or split-rate tax. In theory, such taxes can raise the ratio of housing capital to land. This in turn can raise the density of housing units where it is applied, if the average size of housing units does not increase enough to offset an effect on the number of housing units. This research explores these issues, looking at a panel of land uses and demographics in Pennsylvania. We confirm the theoretical prediction that the split-rate tax raises the capital/land ratio. We also find that the primary effect is in more housing units, rather than bigger units, suggesting the split-rate tax is potentially a powerful anti-sprawl tool. We find adoption of the split-rate tax increases the number of housing units, and that these units follow a more dense pattern of development.

1. Introduction

In the last third of the 20th century, urban sprawl by one measure was increasing at a rate of about 2.5% per year in the United States, a rate which would double the size of cities every 29 years (Burchfield et al., 2006). As a consequence, the problem of urban sprawl has moved from being a pre-occupation of urban planners and land reformers to the mainstream. By the year 2000, the issue climbed into the national spotlight as a poll showed that 18% of Americans viewed sprawl as the top issues facing their community, tied for the highest response (Burchfield et al., 2006), and as Al Gore highlighted the problem in his campaign for the Presidency. Moreover, recent studies have provided strong evidence that households place a high value on the open space lost to sprawl (see e.g. McConnell and Walls, 2005).

Most economists have linked inefficient levels of sprawl to negative externalities (traffic congestion, pollution, loss of open space), subsidized roads, and distortionary real estate taxes.[2] Communities have a number of tools at their disposal to internalize these externalities, including gasoline taxes, improved mass transit and pedestrian access in the inner city, zoning laws, purchases of land or development rights, and impact fees. Communities have increasingly experimented with these solutions, trying transferable development right programs in states like Maryland (McConnell et al., 2006; McConnell and Walls, 2009) and placing measures to publicly purchase open space on local ballots (Kotchen and Powers, 2006; Banzhaf et al., 2008).

One of oldest proposals for injecting more "smarts" into urban growth is the land tax, which eliminates the portion of property taxes falling on structures. Its cousin, the split-rate tax, is a compromise that applies a lower tax rate to structures than to land. Land or split-rate taxes are in force in Australia, Denmark, and

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2 For excellent overviews on these issues, see Brueckner (2001a) and Nechyba and Walsh (2004). See Bento et al. (2006) for an analytical model of the effects of various anti-sprawl policies. Not all economists are in agreement that sprawl is a sign of market failures. Some have argued that sprawl is simply a socially efficient response to the increasing affordability of the automobile, a superior mode of transportation (Glaeser and Kahn, 2003).

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parts of Indonesia (Youngman and Malme, 1994; McCluskey and Franzsen, 2005). Although not widespread in the US, the split-rate tax has been adopted in about 18 cities and towns in Pennsylvania.[3]

A land tax should not be distortionary, as land is essentially fixed in supply. In contrast, a property tax reduces the equilibrium level of housing capital and, thus, the capital/land ratio. This is known as the "improvement effect" (Brueckner and Kim, 2003). If it lowers the capital/land ratio by reducing the number of housing units per unit land area, the property tax will force a city to sprawl further to house its citizens. We call this the "density effect." However, there is another possibility. If it lowers the capital/land ratio by reducing the amount of housing capital used by each household, the property tax may not cause sprawl, and may even reduce it (Brueckner, 2001b; Brueckner and Kim, 2003; Song and Zenou, 2006). This is known as the "dwelling size effect." Switching from a property tax to a land tax will only be an effective tool against sprawl if the density effect dominates the dwelling size effect.

There have been few empirical tests of these effects of the split-rate tax. Oates and Schwab (1997) and Plassmann and Tideman (2000) show that the split-rate tax increases the capital/land ratio as measured by building permits, but they cannot speak to the important policy issue of whether this improvement effect is due more to the density effect or the dwelling size effect. Song and Zenou (2006) find a *negative* correlation between the size of urbanized areas and average property tax rates, suggesting the dwelling size effect may dominate. However, they do not test the effects of the split-rate tax per se.

This paper is to our knowledge the first not only to test the improvement effect of the split-rate tax but also to decompose it into the density and dwelling size effects. We use the 1970, 1980, 1990, and 2000 US Censuses to construct the evolution of the population and housing stock in Pennsylvanian Census tracts over time. We focus on Pennsylvania because the majority of split-tax jurisdictions are located in that state.

Exploiting the fact that most jurisdictions with the split-rate tax adopted it in the 1980s, we identify the effect of the split-rate tax from changes in pre-existing trends in each area. In each normalized tract (with a fixed land area over time), we first look at the total number of rooms (a proxy for the capital/land ratio or improvement effect). We then consider the decomposition of this effect into two parts: the total number of housing units in each tract (the density effect), and the average number of rooms per housing unit (the dwelling size effect). We also consider alternative measures.

We find evidence that the split-rate tax does raise the capital/land ratio as expected (as proxied by the total number of rooms per square mile). Our central estimates, based on linear fixed effects regression, are that a split-rate tax increases the growth in the total number of rooms by about 3–6% points per decade (in the first two decades after adoption) *relative* to control areas. We also find it is much more likely that this effect comes from more houses in a given land area rather than bigger houses. We estimate a small and statistically insignificant effect on the average number of rooms per housing unit, and a 2–5% point increase in the number of housing units. We also find especially strong growth in the development of multi-unit structures in split-rate jurisdictions, suggesting a high-density pattern of development. Overall, it appears that the split-rate tax is a potentially useful weapon in the anti-sprawl arsenal.

[3] For historical background and an overview of the economics of land and split-rate taxation, see Schwab and Harris (1998). For additional context on politics and the status of the split-rate movement, see Hartzok (1997) and McCluskey and Franzsen (2005).

Table 1
Comparison of single and split-rate tax systems.

	Single rate system	Split-rate system
Land tax rate	5%	7.5%
Improvement tax rate	5%	2.5%
Assessed land value	$50,000	$50,000
Land tax bill	$2500	$3750
Small improvement value	$50,000	$50,000
Big improvement value	$100,000	$100,000
Tax bill on small improvement	$2500	$1250
Tax bill on big improvement	$5000	$2500
Total tax bill with small improvement	$5000	$5000
Total bill with big improvement	$7500	$6250

2. Background on the split-rate tax

Since the physiocratic movement of François Quesnay (1694–1774) in the last years of the *ancien régime*, economists have periodically stressed the virtues of land taxes over other types of taxes. Quesnay and the physiocrats stressed that such taxes were non-distortionary because they captured the material value of economic outputs, as provided by nature, and did not discourage investment. Henry George (1839–1897), in contrast, argued in his *Progress and Poverty* (1879) that such taxes would benefit the poor by increasing the ratio of labor to land in the production process, increasing the returns to labor.

More recently, modern economists have shown that in a simple, static setting, land taxes are less distorting than property taxes and are likely to reduce the incentive for cities to sprawl. The reasoning is straightforward. If land supply is fixed, then taxes on land can have no effect on its supply. If capital is determined endogenously, property taxes can and will reduce the resources devoted to development. Thus, by taxing capital, property taxes depress the capital/land ratio (relative to no tax or to a land tax). For formal derivations of this result under various modeling assumptions, see Brueckner (1986), Capozza and Li (1994), England and Ravichandran (2009), Mills (1998), Nechyba (1998), and Oates and Schwab (1997).

However, not taxing capital improvements at all may not be practical or equitable.[4] A compromise is the split-rate tax. The split-rate tax taxes both land and improvements, but does so at differing rates, with more weight put on the land tax. Accordingly, its virtues are qualitatively similar to those of a pure land tax. Table 1 provides an example. Under a single rate property tax, land and improvements are both taxed at 5%. Under the split-rate tax, land is taxed at a rate of 7.5% and improvements are taxed, at a lower rate of 2.5%. In this case, land with a value of $50,000, if given a "small" improvement also worth $50,000, would yield equal revenues of $5000 using either system. However, a "big" improvement worth $100,000 would lead to a tax bill of $7500 under the single rate system but only $6250 under the split-rate system. If a developer were indifferent between the two developments under the single tax, switching to a split-rate system would induce him to prefer the higher-density project.

The claim that land taxes are less distorting than property taxes and reduce sprawl requires three qualifications. The first qualification we have already emphasized, and it motivates our empirical work. As pointed out by Brueckner (2001b), Brueckner and Kim

[4] It may not even be optimal if there are other taxes and distortions in the economy. In the context of second best, the optimal tax would equalize the marginal deadweight loss on all distortionary taxes. The mortgage tax deduction is one particularly important distortion working in the opposite direction, encouraging over-consumption of housing capital.

(2003), and Song and Zenou (2006), relative to land taxes, property taxes also reduce the housing capital consumed by each household. Thus, the higher capital/land ratios associated with the land tax could come from either of two sources: from a greater density of housing units (the density effect) or from larger or nicer housing units (the dwelling size effect) or from a combination. That is:

$$\frac{\text{Capital}}{\text{Land}} = \frac{\text{Capital}}{\text{Housing Units}} \times \frac{\text{Housing Units}}{\text{Land}}. \tag{1}$$

Brueckner and Kim (2003) and Song and Zenou (2006) give examples where either the density effect or the dwelling size effect dominate, and there seems to be no consensus about which effect is likely to dominate in practice.[5] Accordingly the importance of the density effect remains an open question even in theory, and certainly as an empirical question.

The second qualification arises when we consider a dynamic setting. When the future time profile of rents differs between two uses of land—and when land is taxed at its current value, rather than its "highest and best" use—a land tax might also be distorting (Mills, 1981; Capozza and Li, 1994).[6] Oates and Schwab (1997) illustrate this point with the following example. Suppose landowners can earn rents of $1000 forever when they develop their land now for use A, but if they wait one period they can develop it for use B and begin earning rents of $1100. At a 10% discount rate, they would be indifferent between the two options because either would yield a present value of $10,000. If actual rents were taxed at the same rate, or if land were taxed at its highest and best use, the owners would remain indifferent and the land tax would be non-distortionary. But if land is taxed at its current *value*, owners would face a tax on the value of idle land while waiting to develop for purpose B. This "timing effect" consequently favors early development for use A. In this case, the land tax creates a distortion in favor of early development.

To the extent that land values are assessed at current uses, this effect reinforces the likelihood that split-rate communities would have higher capital density (since land is developed sooner). Thus, while it qualifies the normative conclusion that land taxes are non-distortionary, this insight actually strengthens the positive or *empirical* claims associated with the improvement effect.

A third and final qualification pertains to the interpretation of higher-density communities as reducing sprawl or as being part of a "smart growth" strategy. This interpretation seems straightforward when we think of a city as being a single jurisdiction—as in most theoretical models such as Brueckner (1986), Brueckner and Kim (2003), and Song and Zenou (2006). When the only jurisdiction reduces its density, it uses more land and sprawls further. In this case, the split-rate tax would indeed be a reasonable part of a "smart growth" strategy. When there are multiple jurisdictions within a single metropolitan area, however, a further distinction must be made. A jurisdiction on the fringe of the metro area could adopt the split-rate system, increasing its density. From the perspective of the wider metro area, however, this increased density on the fringe might look like more sprawl, particularly if citizens are pulled from the inner city.

The lesson here is that even if it has a large density effect, the split-rate tax would only be an effective weapon in the arsenal against sprawl if it were applied at the appropriate place spatially. We abstract from this issue, looking only at the empirically testable relationship between a jurisdiction's tax system and its housing stock and population density. Accordingly, we do not claim that our empirical work shows that the split-rate tax, as employed in Pennsylvania, has resulted in less sprawl. Nor do we claim that our measurable outcomes of housing and population density in particular locations are a proxy for sprawl.[7] Our more narrow interpretation is simply that the split-rate tax appears to be an effective tool to increase density *in those locations where it is applied*. City officials could then use such a tax to increase density where desirable.

3. Previous empirical work

Given the interest in the split-rate tax among advocates and the attention given to it in theoretical models of urban economics, there has been surprisingly little empirical research on its effects. In part, this is because few jurisdictions have experimented with the tax. Even so, a number of cities in Pennsylvania do provide the opportunity to test the effect of the system. Two major studies have taken advantage of these experiments. Oates and Schwab (1997) focus on Pittsburgh, which in 1979 raised its tax on land to more than five times its rate on structures. Oates and Schwab assess the effect of this reform on building activity, using a difference-in-differences methodology in which the change in building permits around 1979 in Pittsburgh is compared to the change in a set of control cities. As predicted by theory, they find that relative to other cities, Pittsburgh experienced a significant increase in building activity following its adoption of the split-rate tax, relative to other Midwestern cities. Indeed, most cities experienced continued declines in activity while Pittsburgh experienced a rapid increase.

Plassmann and Tideman (2000) similarly test the effect of the split-rate system by looking at building permits, but do so looking at the complete set of Pennsylvania cities that have adopted the split-rate tax, using other Pennsylvania cities as controls. Like Oates and Schwab, they find that the split-rate tax has a statistically significant impact on the number of permits issued. However, they do not find clear evidence that it increases the value of those permits.

Both of these papers are carefully conducted and, taken together, provide strong evidence of the split-rate tax's effect on construction activity. However, neither paper explores the effect of the split-rate tax on the fundamental outcomes of policy interest: population and housing density. Construction permits may reflect additions and tear-downs, representing the dwelling size effect instead of housing unit density.

Taking a different approach, Song and Zenou (2006) regress the geographic size of urbanized areas on property tax rates, controlling for population. They do not look at the split-rate tax directly, but in looking at the property tax do explore the fundamental economic relationships of the improvement, density, and dwelling size effects. They find that urbanized areas with higher property tax rates are actually smaller. They interpret this finding to mean that the dwelling size effect dominates, but they do not test this explanation directly. Moreover, it is not clear whether urbanized areas or metropolitan statistical areas or some other measure is the appropriate spatial scale. It could well happen that lower property taxes shrink the overall metro area, but only by increasing the por-

[5] Brueckner and Kim (2003) suggest that the density effect is more likely to dominate, while Song and Zenou (2006) suggest the opposite. Together, they show that the size effect will dominate under Cobb–Douglas preferences or CES preferences whenever the elasticity of substitution between housing and other consumption is greater than one. It remains possible even for elasticities less than one. The intuition is that when housing and other consumption are highly substitutable, the property tax creates a greater distortion on the housing capital consumed, as households substitute other goods more readily. Song and Zenou (2006) also find this effect in a non-CES preference function in which the elasticity is always greater than one. For an interesting extension, see too the work by Colwell and Turnbull (2003) on area taxes and frontage taxes.

[6] Feldstein (1977) also discusses the effect on savings decisions and the stock of productive capital.

[7] See Burchfield et al. (2006) and Galster (2001) for more sophisticated approaches.

tion that qualifies as "urbanized" in the US Census. Moreover, in their empirical work, Song and Zenou combine multiple jurisdictions into a single metro area observation with the average tax rate across its jurisdictions. Yet any differences between property tax rates within a metro area should be reflected in differences in density across those areas. Lower property tax rates at the fringe of the metro areas might increase density there relative to the core, for example, thereby increasing sprawl by their measure. Thus, their results are consistent with several interpretations.

4. New empirical strategy

We employ a new strategy which directly tests the effect of the split-rate tax on proxies for the improvement, density, and dwelling size effects. In addition, we look at a panel of US Census tracts from 1970 to 2000 rather than averaging different tax policies across a wider area.

Our basic empirical strategy can be summarized with the following regression model. Our first outcome of interest is the total number of rooms per unit land area (TOTRM) as a proxy for the capital/land ratio (i.e. the improvement effect). We then decompose this into two parts: the average number of rooms per dwelling unit (AVGRM) as a proxy for the capital/housing units ratio (i.e. the dwelling size effect), and the number of housing units per land area (HU) as a proxy for the housing units/land ratio (i.e., the density effect). We employ a simple reduced form approach in which the percentage change in these outcomes over decade t in census tract i in jurisdiction j is a function of a tract-specific fixed effect, an average decade effect, a vector of lagged demographic and land use variables (X), and the property tax structure. For example, the total room outcome is modeled as

$$PCT\Delta TOTRM_{ijt,t-1} = \alpha_i + \beta_t + \gamma X_{ijt-1} + \delta SR_{jt} + \varepsilon_{ijt}, \quad (2)$$

where SR is a dummy variable indicating whether the jurisdiction had a split-rate tax as of the midpoint of the period in question and ε_{ijt} is a normally distributed error.

This basic specification has been used recently by Banzhaf and Walsh (2008) and Card et al. (2008) in other applications involving demographic transitions. We highlight four features of this model. First, note that the use of percentage changes allows a simple decomposition in which $\delta^{TOTRM} \approx \delta^{AVGRM} + \delta^{HU}$. That is, taking Eq. (1) and writing it in log form, the total improvement effect is approximately the sum of the density and dwelling size effects.[8]

Second, note that the unit of observation in this model is the census tract. Using the smallest possible unit of observation avoids aggregation errors in the dependent variables and X_{ijt}. Yet the policies of interest are set at the wider jurisdictional level. Moulton (1990) has demonstrated that OLS standard errors can be biased in this case. Following recent suggestions by Bertrand et al. (2004) and Wooldridge (2006), standard errors are clustered at the jurisdiction level. Clustering allows for a jurisdiction level random effect ($\varepsilon_{ijt} = \eta_j + u_{ijt}$). If such effects are important, jurisdictions effectively become a single observation for purposes of computing standard errors. Clustering also allows for non-specified error correlation within the jurisdiction. That is, Cov(ε_{ijt},$\varepsilon_{kj\tau}$) $\neq$ 0 but Cov(ε_{ijt},$\varepsilon_{kl\tau}$) = 0 for $j \neq l$.

Third, note that the policy variable of interest, SR, is a simple indicator for whether or not the jurisdiction has a split-rate tax over the relevant time period. However, a split-rate jurisdiction in which the tax on land is only slightly higher than the rate on structures might not have any perceivable differences from a single-rate jurisdiction, whereas a jurisdiction with a much higher rate on land might. To account for inter-jurisdictional differences in the reliance on land taxes, we also consider a model in which we replace the indicator variable SR with the log of the ratio of the land tax rate to the tax rate on structures (averaged over the course of the decade). This ratio is of course one for all non-split-rate jurisdictions and greater than one for split-rate jurisdictions.[9]

Fourth and finally, note that the presence of the tract fixed effect α_i in the context of a differenced dependent variable implies that the effect of land taxation is identified off of differences from pre-existing trends relative to control communities. This is a difference-in-difference-in-differences model. The effect δ of introducing the split-rate tax in community j at time t is estimated from the difference in the level of the outcome variable from the previous period, relative to the community-specific time path of such differences, and relative to other communities. For the SR model, this effect is only identified from communities that adopt the split-rate tax sometime between 1980 and 2000. For the tax-ratio model, it is identified from communities that change their land-to-structure tax ratio during this period. (The 1970–1980 period is required to establish the pre-existing trend with the fixed effect.)

These fixed effects thus control for any unobservables that affect not only the levels, but also the growth of development in split-rate communities differentially from other communities. Nevertheless, one might be concerned that deviations from long-term time paths are randomly correlated in space with adoption of the split-rate tax. For example, the split-rate tax was adopted in a number of western Pennsylvanian towns in the 1980s, and it may be that in the 1980s western Pennsylvania generally saw an acceleration (or deceleration) in the growth of the capital/land ratio or in the growth of population density, for reasons unexplained by the variables in the model. To control for any such effects that are distributed smoothly in space, the X vector in the model includes an interaction between the decade effects and the communities' locations in terms of degrees latitude, degrees longitude, and the interaction of latitude and longitude. In this way, there is an entire spatial surface estimated for each decade. This surface represents a smooth function in space of decade-specific deviations from communities' long-term time trends. The effect of the split-rate tax is the discontinuous jump at jurisdictional boundaries off of this smooth surface.

The fixed effects also reduce any concern about the endogeneity of the split-rate tax. Although communities might adopt the tax in response to a shrinking tax base, any endogeneity in the model would have to be conditional on pre-existing trends. Moreover, we restrict the effect of SR to those jurisdictions that had adopted it in the first half of the decade, and consider other lags in sensitivity analyses. Thus, if jurisdictions adopt the split-rate tax because of lagging development, it would not pose a problem for our model. More problematic would be a situation where jurisdictions adopt the split-rate tax in response to correct forecasts about *future* development. We think this is unlikely, but acknowledge it as a potential source of concern. But note further that, if anything, it is more likely that jurisdictions would adopt the split-rate tax in response to anticipated slower development. This would bias our results downward, making the positive association we find all the

[8] More precisely (1 + δ^{TOTRM}) = (1 + δ^{AVGRM})(1 + δ^{HU}). The approximation is off by the factor δ^{AVGRM}.δ^{HU}, which can be expected to be small. It is also important to note that this identity is only valid for a given observation. Estimated coefficients may differ from this pattern unless they are consistently weighted. We use the "midpoint formula" for computing the percentage change, using the average of the beginning and ending values in the denominator. In contrast to using the beginning value, this approach allows computation of percentage changes when baseline values are zero. Percentage changes using this formula are necessarily bounded between −2 and +2.

[9] An alternative approach would be to allow the tax rate on land and on structures to enter the model separately. Unfortunately, we do not observe the property tax rate in a number of non-split-rate jurisdictions, so such an approach would require discarding a large portion of the sample.

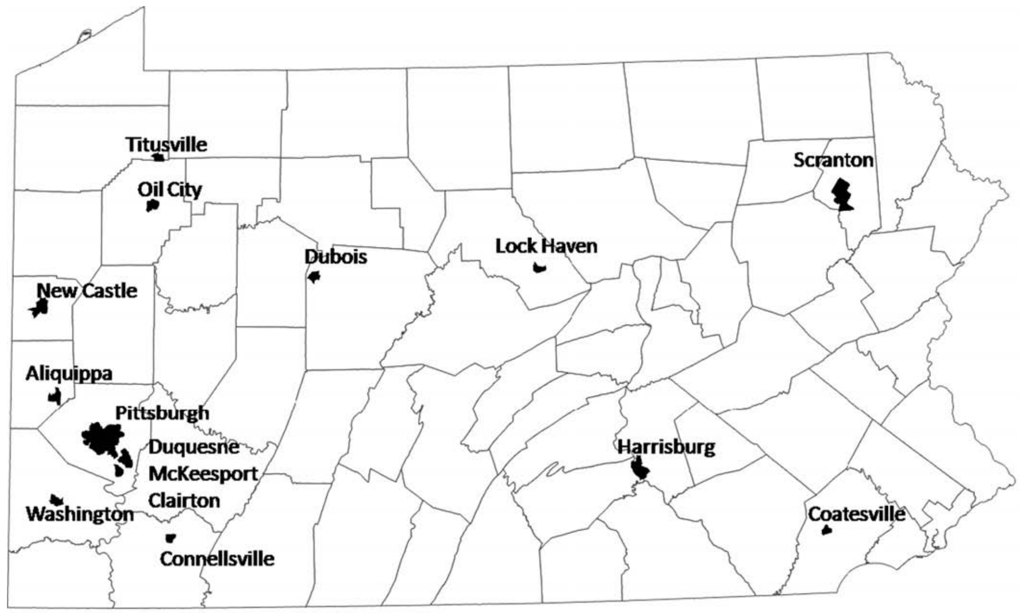

Fig. 1. Split-rate jurisdictions in Pennsylvania (1995).

more strong. For our results to be spurious, it would have to be that jurisdictions adopt the split-rate tax in response to anticipated acceleration in development.

As we discuss in more detail below, we also consider a number of other approaches in sensitivity analyses. These approaches include alternative proxies for the improvement, density, and size effects. They also include using a non-parametric matching estimator.

5. Data

As previously noted, we test for the effects of a split-rate tax among jurisdictions in Pennsylvania, where towns have shown particular interest in the split-rate tax. Fig. 1 shows the split-rate jurisdictions used in the analysis. Table 2 lists the 18 Pennsylvania jurisdictions using the split-rate tax from 1970 to 2000, the year it was adopted, and the range in the tax ratio. These data were collected from Center for the Study of Economics. As shown in the table, most cities adopted the split-rate tax in the 1980s and 1990s. Consequently, census data from 1970 to 2000 is sufficient to document changes in population density before and after the adoption of the split-rate system for these cities, controlling for pre-existing trends. In the basic model in which the split-rate system is treated as a dummy variable, focusing on this period eliminates Harrisburg, Pittsburgh, and Scranton because they had already adopted the split-rate tax by 1970. However, because they did adjust their tax rates over the 1970–2000 period, these three cities still contribute to the estimated effects of land taxes in models that account for the land-to-structure tax ratios. Two cities, Hazleton and Uniontown, abandoned the split-rate tax shortly after adopting it. These two jurisdictions are dropped from the analysis.[10]

One potential concern might be that cities adopting the split-rate tax did not actually lower their tax rates on structures, which is the source of the tax distortion, but merely raised rates on land. However, this does not appear to be the case. According to 1990 and 2000 data from the Tax Foundation, six of seven of those jurisdictions that adopted the tax in that decade lowered their rate on structures, cutting it by an average of 23%. This also appears true when the changes in rates in these six jurisdictions are compared to the 1990–2000 changes in those non-split-rate jurisdictions for which we could find data.[11]

Our analysis is at the Census tract level. To maintain consistency in these geographic boundaries over time, we utilize Geolytics' Neighborhood Change Database, which normalizes all populations to the 2000 census boundaries. Table 3 summarizes the demographic data. The variables in the top panel are the various outcomes of interest. The table shows that, simply looking at the raw data, it appears that split-rate cities are losing population and certainly not growing as fast as jurisdictions with conventional property taxes. The variables in the second panel are the control variables (the X variables of Eq. (2)). It appears that on average split-rate jurisdictions are denser, are poorer, and have an older and less valuable housing stock. A good part, but not all, of these differences are driven by Pittsburgh.

[10] In addition, since 2000 three cities (including Pittsburgh) have abandoned the split-rate tax, while three other jurisdictions have newly adopted it. These moves do not affect our analysis, which ends with 2000 data.

[11] Ideally, we would have used data on property tax rates for all jurisdictions in our analysis. Such data is available from the US Census of Governments and from the Tax Foundation. However, we were only able to find such data for 1990 and 2000, whereas our panel goes back to 1970. Moreover, we were unable to match these data to all jurisdictions. A related concern is that some "control" jurisdictions may actually be split-rate jurisdictions implicitly, if they offer temporary tax abatements on new constructions. We are unable to observe any such cases. We note, however, that the presence of such jurisdictions would bias our results toward the null, since we would be comparing treated observations to a mix of control and treated observations rather than to just controls. Again, our results are more conservative in this sense.

Table 2
Pennsylvania split-rate cities as of 2000.

	Year first adopted	Last year	Land-structure tax ratio		
			Lowest	Highest	1990 Population
Aliquippa	1988	–	11.3	16.2	13,374
Allentown	1997	–	1.5	4.7	105,090
Clairton	1989	–	4.7	4.8	9656
Coatesville	1991	2006	1.6	2.1	11,038
Connellsville	1992	2003	6.5	7.7	9229
Dubois	1991	–	2.3	3.9	8286
Duquesne	1985	–	2.0	2.6	8845
Harrisburg	1975	–	1.4	4.0	52,376
Hazleton	1991	1992	3.2	3.4	24,730
Lock haven	1991	–	2.1	3.8	9230
McKeesport	1980	–	3.7	5.3	26,016
New castle	1982	–	1.8	4.0	28,334
Oil city	1989	–	1.2	3.2	11,949
Pittsburgh	1913	2001	2.0	5.8	369,379
Scranton	1913	–	2.0	5.5	81,805
Titusville	1990	–	3.4	4.1	6434
Uniontown	1992	1992	5.5	5.5	30,472
Washington	1985	–	3.6	17.5	15,791

Source: Center for the study of economics and US Census.

Table 3
Demographic characteristics of Census tracts (mean values).

Variable	Split-rate jurisdictions	Single-rate jurisdictions
Pct change in rooms	−1.9%	26.6%
Pct change in rooms/unit	2.5%	1.7%
Pct change in housing units	−4.3%	25.0%
Pct change in detached houses	−3.2%	23.7%
Pct change in attached houses	3.7%	42.3%
Pct change in multi-unit structures	−11.0%	20.7%
Pct change in 5+ unit structures	−4.4%	29.4%
Pct change in rooms per capita	11.6%	10.4%
Pct change in population	−13.4%	17.1%
Population per sq mi	9524	5002
Households per sq mi	3743	1839
Rooms per sq mi	20,319	10,558
Average # rooms per unit	5.2	5.08
Pct housing units >30 years old	77.6%	47.8%
Pct housing units <10 years old	5.5%	14.0%
Pct age > 65	16.7%	11.6%
Pct age < 18	23.6%	23.8%
Pct black	21.6%	7.0%
Pct hispanic	0.9%	1.3%
Average household income	18,188	21,426
Pct Hholds upper income*	1.4%	1.6%
Pct Hhold in poverty	18.6%	8.7%
Pct unemployed	9.0%	5.0%
Pct housing units vacant	8.1%	5.1%
Average monthly rent	199	195
Average housing value	26,987	38,822
Pct owning home	53.4%	63.8%
Pct no high school diploma	41.2%	31.2%
Pct bachelors degree	12.7%	13.4%
Degrees latitude	40.58	40.46
Degrees longitude	−79.19	−77.07
N	624	8703

Each tract appears three times. Pct changes are 1970–1980, 1980–1990, and 1990–2000. Level variables are for 1970, 1980, and 1990.
* Defined as $50,000 in 1970, $75,000 in 1980, and $125,000 in 1990.

Finally, note that the time period covered, 1970–2000, should be sufficient to capture changes in the housing stock in response to fiscal variables. For example, the average number of rooms in the normalized tracts increased from 5424 in 1970 to 9907 by 2000, representing almost a doubling in the capital stock. To the extent that adjustments to the split-rate tax occurred with a lag, our estimates are then "conservative" in the sense that they are biased toward zero.

6. Results

Our central estimates are summarized in Table 4. The table shows the estimated value of the three parameters of interest, i.e. the parameter δ in Eq. (2). The first panel shows the estimated improvement effect, as proxied by the total number of rooms per land area. The next two panels decompose this effect into, respectively, the size effect (as proxied by rooms per dwelling unit) and the density effect (as proxied by dwelling units per land area). Each column represents a separate model specification. Models 1 and 2 use the split-rate dummy as the variable of interest. Models 3 and 4 use the natural logarithm of the land-to-structure tax ratio (which allows changes in Pittsburgh, Scranton, and Harrisburg to contribute to the estimated effects). Models 1 and 3 are unweighted; models 2 and 4 weight the observations by the number of housing units in the tract (averaged over the beginning and end of each decade). All regressions control for tract-specific fixed effects, the variables listed in the second panel of Table 3 plus squares of those terms, and decade interactions with latitude, longitude, and latitude $*$ longitude.

Each cell represents a separate regression, and reports the coefficient on the split-rate variable, its clustered standard error, and the R^2 of the regression. (Table A1 in the Appendix gives the full results for Model 1.) The first set of results in Table 4 indicates that, as predicted, the split-rate tax has a positive effect on the capital/land ratio, increasing the total number of rooms in the jurisdiction by 5–6% points over pre-existing trends relative to control districts, within the first two decades after adoption. The effect is similar when we account for the tax ratio. Since the average split-rate jurisdiction has a ratio of about 4-to-1, we can roughly multiply the estimates in models 3 and 4 by 1.4 to make them comparable to models 1 and 2. The predicted effects from the "average split-rate" policy in models 3 and four are then 5.5% and 3.9% points respectively, quite close to the effects captured in models 1 and 2. These results are consistent with the improvement effect predicted by economic theory and with previous results on construction permits (Oates and Schwab, 1997; Plassmann and Tideman, 2000).

Table 4
Effects of split-rate tax on outcomes of interest.

	Policy variable: Weighted?	Model 1 SR dummy No	Model 2 SR dummy Yes	Model 3 ln tax ratio No	Model 4 ln tax ratio Yes
Outcome	Proxy				
Improvement effect (capital/land)	Pct change in total # rooms	0.0633** (0.0311) R^2 = 0.93	0.0470*** (0.0179) R^2 = 0.94	0.0392** (0.0198) R^2 = 0.93	0.0281** (0.0157) R^2 = 0.94
Size effect (capital/units)	Pct change in avg # rooms per unit	0.0010 (0.0104) R^2 = 0.66	−0.0015 (0.0053) R^2 = 0.69	0.0088* (0.0067) R^2 = 0.66	0.0065 (0.0075) R^2 = 0.69
Density effect (units/land)	Pct change in # housing units	0.0540** (0.0329) R^2 = 0.92	0.0510*** (0.0204) R^2 = 0.94	0.0217 (0.0212) R^2 = 0.92	0.0204* (0.0150) R^2 = 0.94

Each cell represents a separate regression. For each outcome/model, the first number listed is the estimate (i.e. δ) from Eq. (2). The second number in parentheses is the robust standard error of the estimate, clustered at the jurisdiction level. The third number is the R^2 of the regression.
All regression control for tract-specific fixed effects, the variables listed in the second panel of Table 3 plus squares of those terms, and decade interactions with latitude, longitude, and latitude $*$ longitude. See Eq. (2) for the specification.
* One-tail test significant at 10%.
** One-tail test significant at 5%.
*** One-tail test significant at 1%.

The second set of results indicates that there is at most a modest "dwelling size effect." According to models 1 and 2, the growth in the size of the average dwelling unit appears no different in split-rate communities than other communities. According to models 3 and 4, there is a small but, in one specification, statistically significant effect (0.9–1.2% points for a 4:1 tax ratio). However, even here the dwelling size effect still accounts for only about one-fifth of the total increase in rooms.

The third and final set of results directly addresses the central policy question. Does the split-rate tax have a density effect, which might make it useful as an anti-sprawl policy tool? Our results suggest it does. Models 1 and 2 estimate an effect for the average split-rate jurisdiction of about five more percentage points in the growth of housing units over pre-existing trends relative to control districts, within the first two decades of adoption. Models 3 and 4 estimate an effect of about 2.8–3.0% points for the typical 4:1 tax ratio.

We subject these estimates to a number of robustness checks. For the improvement effect, our central estimates use the number of rooms in the tract as a measure of the aggregate housing capital/land ratio. In our view, this aggregate measure is the most appropriate, since it captures any of a number of ways in which the capital/land ratio might adjust. For example, housing capital could increase over a given area by adding to existing structures, by building new structures on undeveloped lots, or by subdividing parcels into smaller lots. Any of these margins for adjustment is consistent with the increase in the capital/land ratio predicted by theory. However, a more narrow reading suggests that individual lots should be developed more intensely. In particular, if adopting the split-rate tax merely encourages short-run development of low-density detached housing, it may look like more density, while the *average* density of developed land is actually declining.

Accordingly, our first set of robustness checks uses alternative measures of the outcome variables that differentiate among structure types. In particular, we look at the percentage change in detached houses, attached houses, multi-unit structures (with 2+ units), and 5+ unit structures. The first panel in Table 5 shows the effect on detached houses, representing the lowest-density form of development. Adopting the split-rate tax actually appears to slow construction of such structures, though the effect is not statistically significant. The second panel shows the effect of the split-rate tax on attached houses, representing a middling level of density. The effect is inconsistent and quite sensitive to the specification of the model. The third and fourth panels indicate that the split-rate tax appears to increase construction of high-density structures, with especially strong evidence for a large effect on structures with five or more dwelling units. These patterns suggest that the new housing units constructed following adoption of the split-rate tax are disproportionately high-density structures. In this sense, they support the interpretation that our results imply the split-rate tax appears to increase density, at least in this sample.

Table 5 also includes new estimates of the size and density effects in terms of population, rather than housing units. We find a 1–2% point increase in the number of rooms per capita, representing an alternative perspective on the size effect. For the density effect, we also consider changes in population in the last panel of Table 5, in lieu of the changes in housing units shown in Table 4. Models 1 and 2 show effects similar to Table 4, with the split-rate tax associated with an increase in population of 4–5% points relative to controls. Somewhat surprisingly, models 3 and 4 do not identify any population effects. This may be because in equilibrium, smaller households move into communities with a high ratio of land taxes to structure taxes, or it may be because occupancy rates have not yet caught up with new construction. Or it may simply be because of more noise in the data: notably, the point estimates from models 1 and 2 also cannot be rejected. The question warrants further research.

We considered a number of more minor sensitivity analyses as well. First, rather than confine the split-rate indicator to jurisdictions that had adopted the tax by the midpoint of the decade, we consider any adoption of the split-rate tax over the decade or alternatively only jurisdictions that had adopted the tax by the beginning of the decade. (The analog of the latter model in the tax-ratio model is to use the tax ratio at the beginning of the decade rather than the average tax ratio over the decade.) Second, we consider the effect of dropping Pittsburgh and Philadelphia from the model. Third, we restrict regressions of each outcome to only those Census tracts with available measures for all outcomes.[12] Fourth and finally, we consider differences in logs of the dependent variable rather than percentage changes. Our results are qualitatively un-

[12] When a tract goes from zero rooms and housing units to non-zero, our measure of the percentage change in these variables is still defined, but the measure of the change in the average number of rooms is not defined. The approach taken in this sensitivity analysis drops these observation from all regressions, rather than from just the dwelling size effect regressions.

Table 5
Effects of split-rate tax on alternative outcomes of interest.

	Policy variable: Weighted?	Model 1 SR dummy No	Model 2 SR dummy Yes	Model 3 ln Tax ratio no No	Model 4 ln Tax ratio Yes
Outcome	Proxy				
Improvement effect	Pct change in detached houses	−0.0355 (0.0500) R^2 = 0.76	−0.0340 (0.0467) R^2 = 0.72	−0.0017 (0.0270) R^2 = 0.76	−0.0278 (0.0272) R^2 = 0.72
	Pct change in attached houses	−0.0502 (0.1657) R^2 = 0.50	−0.0920 (0.1634) R^2 = 0.49	0.1578* (0.1144) R^2 = 0.50	0.1213 (0.1046) R^2 = 0.49
	Pct change in multi-unit structures	0.1094** (0.0568) R^2 = 0.71	0.0932** (0.0557) R^2 = 0.69	0.0151 (0.0281) R^2 = 0.71	0.0076 (0.0229) R^2 = 0.69
	Pct change in structures with 5 + units	0.2084*** (0.0875) R^2 = 0.52	0.2217** (0.1003) R^2 = 0.50	0.1289** (0.0659) R^2 = 0.52	0.1210** (0.0583) R^2 = 0.50
Size effect	Pct change in avg # rooms per capita	0.0127 (0.0188) R^2 = 0.62	0.0183* (0.0124) R^2 = 0.73	0.0153 (0.0146) R^2 = 0.62	0.0190* (0.0126) R^2 = 0.73
Density effect	Pct change in population	0.0523** (0.0287) R^2 = 0.93	0.0397** (0.0209) R^2 = 0.94	0.0116 (0.0179) R^2 = 0.93	0.0028 (0.0151) R^2 = 0.94

Each cell represents a separate regression. For each outcome/model, the first number listed is the estimate (i.e. δ) from Eq. (2). The second number in parentheses is the robust standard error of the estimate, clustered at the jurisdiction level. The third number is the R^2 of the regression.
All regression control for tract-specific fixed effects, the variables listed in the second panel of Table 3 plus squares of those terms, and decade interactions with latitude, longitude, and latitude $*$ longitude. See Eq. (2) for the specification.
* One-tail test significant at 10%.
** One-tail test significant at 5%.
*** One-tail test significant at 1%.

changed using any of these alternative approaches. These results are available upon request.

We also consider a very different approach. Rather than use an OLS regression model as represented by Eq. (2), we consider a non-parametric propensity score matching model (Abadie and Imbens, 2006; Dehejia and Wahba, 2002; Heckman et al., 1997, Rosenbaum and Rubin 1983). The model is of the form:

$$PCT\Delta TOTRM_{ijt} = \alpha_i + \beta_t + f(X_{ijt-1}) + \delta SR_{jt} + u_{ijt}. \qquad (3)$$

Rather than use all control jurisdictions, this approach selects only those control jurisdictions that are most similar to each treatment jurisdiction in terms of the X variables. It then directly matches those cases, differencing the dependent variable. The approach is non-parametric because the γ are never estimated: Eq. (3) can allow for an arbitrarily complex function $f(X_{it})$. Because the X's are very similar for treatment and control pairs, they simply cancel out in the comparison.

Following Rosenbaum and Rubin (1983), we match "treatment" and "control" observations in the same year on the predicted probability (or "propensity score"), estimated from a first-stage probit, that a Census tract has the split-rate tax. We restrict these matches to the area of "overlapping support," the range of the data where both treatment and control observations are located, thereby dropping two split-rate tracts as well as a number of dissimilar controls. We match each split-rate tract to four treated controls in an effort to gain efficiency in the estimator, although results are very similar when matching to a single most similar control tract. Finally, we regression-adjust the outcome variables for any small differences in the X variables between the treatments and matched controls.

Table 6 shows the estimated effects, i.e., the estimates of δ from Eq. (3), along with robust standard errors computed as suggested by Abadie and Imbens (2006). The estimates are similar to the fixed effects regression estimates reported in Table 4. The point estimates are slightly larger, with housing unit density effects of about

Table 6
Effects of split-rate tax on outcomes of interest (matching models).

	Policy Variable: Weighted?	Model 5 SR Dummy No	Model 6 SR Dummy Yes
Outcome	Proxy		
Improvement effect (capital/land)	Pct change in total # rooms	0.0661 (0.1405)	0.0915 (0.1078)
Size effect (capital/units)	Pct change in avg # rooms per unit	−0.0138 (0.0178)	0.0205** (0.0106)
Density effect (units/land)	Pct change in # housing units	0.0746 (0.1340)	0.0981 (0.1060)

For each outcome/model, the first number listed is the estimate (i.e. δ) from Eq. (3). The second number in parentheses is the robust standard error of the estimate, following the approach of Abadie and Imbens (2006).
All regression control for tract-specific fixed effects, the variables listed in the second panel of Table 3 plus squares of those terms, and decade interactions with latitude, longitude, and latitude $*$ longitude. See Eq. (3) for the specification.
***One-tail test significant at **5%, *10%.

7% points in the unweighted model and 10% points in the weighted model. However, the standard errors are larger and most of the effects are not significant at conventional levels. The housing unit density effects have one-tail *p*-values of about 0.29 in the unweighted model and 0.18 in the weighted model. This is not surprising given the demands this approach requires from the data, as it only uses matched observations. The results are also qualitatively similar for the outcomes in Table 5 (results available upon request).

7. Conclusions

The split-rate tax is a long-advocated tool that should lead to greater economic efficiency. Our results indicate that it should also lead to "smarter" growth patterns. We find that capital-land ratios increase in those areas with split-rate taxes and higher land-structure tax ratios. Moreover, the dwelling size effect appears to be modest, so that most of this increased capital implies greater density for the city. Adopting the split-rate tax results in a 4–5% point increase per decade in the growth of the density of housing units, for the first two decades.

These findings are roughly consistent with the findings of Oates and Schwab (1997) and Plassman and Tideman (2000) on construction activity. They stand in marked contrast, however, with the recent finding by Song and Zenou that urbanized areas with higher average property tax rates are more compact. We might reconcile these findings in the following way. Consider first a metro area with uniform property tax rates and declining density from the urban center. Now consider a second metro area identical in every respect, except with lower property tax rates at the fringe. This increases density at the fringe, so roughly speaking the second metro area will have a lower density gradient. An analysis at the micro level would reveal that ceteris paribus, density would be higher where property taxes are lower. But if we designate some arbitrary level of density as "urban," the urban area in the metro area with lower average tax rates would be bigger. Further detailed exploration of the spatial structure of actual cities appears to be warranted.

In any case, this explanation underscores the point that any effect of land or split-rate taxes on increasing density is not guaranteed to decrease sprawl. If these fiscal tools are applied in exurbs or rural areas, any resulting increase in density would by most measures represent an increase in sprawl. While our results suggest that these fiscal tools have a place in the urban planner's toolkit, like any tool they would have to be used in the right time and place.

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Appendix A

See Table A1.

Table A1
Results from Table 4, model 1, panels 1–3.

Dependent variable: tract FE?	Pct change rooms Yes	Pct change rooms/unit Yes	Pct change housing units Yes
Split-rate dummy	0.063296**	0.000977	0.053985**
	0.031065	0.010354	0.032826
Population per sq mi	−3.3E−05***	3.79E−06*	−4.9E−05***
	7.96E-06	2.47E-06	4.68E-06
Households per sq mi	−8.4E−05***	−2E−05*	−9.1E−05***
	0.000019	1.57E−05	1.78E−05
Rooms per sq mi	−1.3E−05**	5.33E−07	−5.82E−06
	5.74E−06	3.27E−06	4.97E−06
Average # rooms per unit	−0.20372***	−0.23038***	−0.12782***
	0.020454	0.029916	0.028141
Pct housing units >30 years	−0.01727	−0.05329**	−0.07347
	0.08825	0.028602	0.09754
Pct housing units <10 years	−0.11753*	−0.02539	−0.13859*
	0.079084	0.035569	0.087006
Pct age > 65	−0.98453**	−0.0835	−0.85592
	0.540408	0.236314	0.366627
Pct age < 18	−2.1882***	−0.19117	−2.39623***
	0.51562	0.389028	0.484579
Pct black	−0.17508	−0.12158	0.184196
	0.15258	0.113351	0.185842
Pct hispanic	0.191429	−0.16296***	0.366662**
	0.204752	0.068713	0.222687
Average household Inc.,	−2.91E−06	−2.31E−07	−3.17E−08
	1.25E−06	3.24E−07	1.73E−06
Pct Hholds upper income	−0.16843	0.354562**	−0.40054
	0.483757	0.165853	0.663707

(*continued on next page*)

Table A1 (*continued*)

Dependent variable: tract FE?	Pct change rooms Yes	Pct change rooms/unit Yes	Pct change housing units Yes
Pct Hhold in poverty	−0.11739	−0.10126*	−0.08763
	0.191826	0.068981	0.217628
Pct unemployed	−0.0519	0.01327	−0.37146
	0.224068	0.056796	0.365217
Pct housing units vacant	−0.93155***	−0.15174	−1.28019***
	0.268909	0.129145	0.369986
Average monthly rent	−0.00026**	−5.3E−05	−0.00032**
	0.000155	8.75E−05	0.000175
Average housing value	2.52E−06***	3.46E−07**	2.40E−06***
	6.57E−07	1.71E−07	6.61E−07
Pct owning home	−1.03616***	−0.32442***	−1.55512***
	0.374094	0.090496	0.260846
Pct no HS diploma	0.335323	−0.09728	−0.22872
	0.292518	0.091489	0.226063
Pct bachelors degree	0.261747*	0.096864	−0.33106
	0.188268	0.125184	0.369928
Population/sq mi squared	3.72E−10***	−1.61E−11	4.96E−10***
	6.13E−11	1.67E−11	4.98E−11
Households/sq mi squared	1.44E−09***	2.63E−10	1.31E−09***
	2.95E-10	3.04E-10	2.84E−10
Rooms/sq mi squared	8.68E−11***	3.88E−13	7.54E−11***
	3.19E−11	2.03E−11	3.07E−11
Pct >65 squared	1.27567*	−0.35617**	1.283933*
	0.845764	0.179358	0.929051
Pct <18 squared	3.192298***	0.326	3.400355***
	0.6271	0.573119	0.93334
Pct black squared	0.176235	0.047791	−0.02387
	0.169729	0.067474	0.09297
Pct hispanic squared	−0.07333	−0.03071	0.026089
	0.252398	0.060991	0.277465
Avg HHold income squared	9.19E−12**	4.58E−12***	−4.45E−13
	4.56E-12	1.77E−12	5.79E−12
Pct upper income squared	−0.47602	−1.34549***	0.030501
	0.849271	0.493665	0.970083
Pct poverty squared	−0.14466	0.184451***	−0.12188
	0.344204	0.053105	0.436802
Pct unemployed squared	−0.97691	−0.2308	−0.16526
	0.925003	0.279572	1.488769
Pct units vacant squared	0.589525***	0.252813	0.851991***
	0.244674	0.220017	0.262712
Avg rent squared	−7.61E-08	−3.63E−09	−4.53E−08
	1.49E-07	8.25E−08	1.83E−07
Avg value squared	−3.54E−12***	2.95E−13	−3.87E−12***
	1.46E−12	5.03E−13	1.62E−12
Pct own squared	1.079368***	0.339166	1.455188***
	0.323153	0.094112	0.28119
Pct no high school squared	−0.2286	0.008347	0.167358
	0.287273	0.068122	0.244422
Pct bachelors degree squared	−0.27169	−0.22862***	0.230403
	0.218717	0.047406	0.438531
Pct units <10 years squared	0.084144	−0.01529	0.106376
	0.137847	0.032189	0.14032
Pct units >30 years squared	0.151208**	0.047091**	0.181633***
	0.067201	0.023812	0.069088
1980–1990 Dummy	51.75395***	−2.26434	47.48225***
	18.90967	6.127529	19.48807
1990–2000 Dummy	30.67314**	−19.4982***	21.02069**
	13.30623	3.7264	11.23951
Latitude × 1990	0.537804	−0.46966***	0.672244*
	0.478764	0.091954	0.487531
Latitude × 2000	1.275094***	−0.41597***	1.173807***
	0.465517	0.155761	0.479939

Table A1 (*continued*)

Dependent variable: tract FE?	Pct change rooms Yes	Pct change rooms/unit Yes	Pct change housing units Yes
Longitude × 1990	0.65834***	0.249308***	−0.33542*
	0.244515	0.047759	0.255431
Longitude × 2000	0.384691**	0.219243***	−0.60182***
	0.16699	0.082629	0.252257
Latitude × longitude × 1990	0.006979	−0.00599***	−0.0149***
	0.006213	0.001175	0.006216
Latitude × longitude × 2000	0.016213***	−0.00529***	−0.00636**
	0.006023	0.002	0.003549
Constant	1.311371**	21.02479***	−91.7422***
	0.669754	3.817763	38.88932
N	8891	8247	8896
R^2	0.93	0.66	0.92

The table shows the full results from the first column of Table 4 (i.e. Model 1, for all three outcome variables).
The first number shown is the estimated coefficient. The second number is the standard error.
* One-tail test significant at 10%.
** One-tail test significant at 5%.
*** One-tail test significant at 1%.

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