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LAND USE REGULATION AND WELFARE

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We evaluate the effect of land use regulation on the value of land and on welfare. Our estimates are based on a decomposition of the effects of regulation into three components: an own-lot effect, which reflects the cost of regulatory constraints to the owner of a parcel; an external effect, which reflects the value of regulatory constraints on one's neighbors; a supply effect, which reflects the effect of regulated scarcity of developable land. Using this decomposition, we arrive at a novel strategy for estimating a plausibly causal effect of land use regulation on land value and welfare. This strategy exploits cross-border changes in development, prices, and regulation in regions near municipal borders. Our estimates suggest large negative effects of regulation on the value of land and welfare in these regions.

KEYWORDS: Land regulation, zoning, urban economics, regulation.

1. INTRODUCTION

WE ESTIMATE THE EFFECT OF LAND USE REGULATION ON THE VALUE OF LAND. To accomplish this estimation we first develop a simple model of the way that land use regulation affects the choice of residential location and the value that people assign to different locations. This model leads us to partition the effect of land use regulation on both land prices and social surplus into three components. First is an own-lot effect, which reflects the cost of regulatory constraints on how land is used. Second is an external effect, which reflects the value of regulatory constraints on the use of nearby land. Third is a supply effect, which measures the effect of regulatory constraints on the supply of developable land. This decomposition leads to an empirical strategy for identifying the causal effect of regulation on land price by separately estimating each of these three components. Our model also shows how these three estimates can be aggregated to evaluate the change in welfare caused by a change in land use regulation.

We estimate the own-lot effect of regulation by looking for a systematic relationship between the change in land prices and the change in regulation across municipal borders. We estimate external effects by considering the way that land prices vary with proximity to a municipal boundary where regulation

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changes. Our econometric technique does not allow us to identify the effect of supply restrictions on land prices. However, our analysis indicates that the effect of supply restrictions on land prices is not relevant to our welfare calculation: such price changes are pure transfers. Rather, welfare depends on the effect that regulation has on the quantity of land developed. To estimate this effect, we examine the relationship between the change in developed share and the change in regulation as we cross municipal borders.

To conduct our investigation, we develop a new data set that combines the Wharton Land Use Regulation data with transaction level land sales from Costar and land cover data from the 2006 National Land Use and Land Cover data. Together, this leads to a national level data set that tracks land regulation, land prices, and development at a very fine spatial scale.

On the basis of point estimates, a 1 standard deviation increase in land use regulation decreases land value by about one-third. Of this, by far the largest loss of welfare is attributable to own-lot and external effects, while a 2 or 3% decrease in developable area contributes the residual. A qualification is important here. We easily distinguish the effect of land use regulation on developed area at conventional levels of significance. Our estimates of the own-lot effect are less precise but also suggest that they are negative at conventional levels of significance. Our estimates do not allow us to determine that the external effects of regulation are negative at conventional levels of significance. This reflects the fact that point estimates of the external effect of regulation are negative but small. However, our estimates are precise enough to place fairly tight bounds on the value of the external effects of regulation. In particular, even at their upper 95% confidence bound, our estimates of the value of the external effects of regulation are not sufficiently large to offset the other costs of regulation. That is, the imprecision of our external-effects estimates appears not to preclude unambiguous welfare statements.

The validity of our estimates rests on the assumption that mean differences in unobserved parcel characteristics across municipal borders are not correlated with differences in regulation across these borders. We adopt three strategies to assure that our data satisfy this condition. First, we develop an algorithm to identify parcels for which the nearest municipal border is a straight line. Almost the entire area of the continental United States outside of the original 13 colonies was surveyed in accordance with the Land Ordinance Act of 1785 (Libecap and Lueck (2011)) and it is likely that many of our straight borders reproduce these survey lines. Such straight municipal borders are deliberately drawn without regard for local physical geography and therefore are unlikely to divide qualitatively different types of land. Second, we are able to locate parcels precisely. Together with the large size of our sample, this allows us to base our estimates on fine bands around municipal boundaries. This ability to compare parcels that are physically close to a boundary increases our confidence that the parcels on either side have similar unobserved characteristics. Finally, we have detailed descriptions of each parcel. By including control variables based on this description, we reduce the scope for unobserved factors to bias our results. Moreover, our data and estimating equations provide a basis for dealing with the possibility that heterogenous residents sort into municipalities on the basis of municipal level unobserved parcel characteristics correlated with land use regulation.

Our results are of interest for at least two reasons. First, land is among the most important assets in the U.S. economy and the market for land is highly regulated. Understanding the impact of land use regulation on land value is an economic problem of the first order. Second, the policy debate surrounding land use regulation attracts many competing interest groups with conflicting agendas. "[T]he Sierra Club urges planning and policies which stimulate… 'Infill' residential and commercial development on unused or under-used land within city boundaries…."[2] On the other hand, the National Association of Home Builders opposes "urban growth boundaries, which restrict the amount of developable land and contribute to increased housing prices…."[3] Our research provides a foundation for land use policy based on the analysis of high quality data rather than interest group politics.

This paper is part of a large literature that looks at the relationship between land use regulation and the land market. However, only a small subset of these papers correct for the endogenous determination of regulation. Thus, only this handful of papers can claim to find a causal effect of land use regulation on land markets. Mayer and Sommerville (2000) instrument for regulation using historical demographic characteristics and find that housing starts respond more slowly to price changes when regulation is more stringent. Ihlanfeldt (2007) also uses historical demographic variables as instruments for regulation and finds that regulation increases house prices and decreases land prices. Saiz (2010) estimates a system of equations for housing demand and supply, and concludes that regulation increases housing prices. Zhou, McMillen, and McDonald (2008) consider a 1957 change to Chicago zoning that appears to increase the value of land. Libecap and Lueck (2011) consider the effect of two different parcel demarcation schemes, rectangular versus metes and bounds, and find higher land prices in areas with rectangular parcels. A large complementary literature looks at the effects of nearby open space and amenities on land prices (see McConnell and Walls (2005) for a survey), and a related literature exploits changes in land prices to value proximity to disamenities such as hazardous waste sites (Greenstone and Gallagher (2008), Gamper-Rabindran and Timmins (2011)). Rossi-Hansberg, Sarte, and Owens (2010) exploit a natural experiment to examine the value of amenities derived from land use regulation, while Walsh (2007) considers the same problem in the context of a

[2]Sierra Club conservation policies, adopted by the Board of Directors, February 1, 1986, http://www.sierraclub.org/policy/conservation/urban.aspx.

[3]National Association of Homebuilders, November 19, 2007, http://www.nahb.org/page.aspx/category/sectionID=633.

structural model of location choice. Loosely, the literature on the effects of regulation is interested in our own-lot effect, while the literature on open space and amenities is interested in our external effect. There is also a small literature that focuses on the way that regulation causes changes in the supply and price of housing, for example, Glaeser and Gyourko (2003), Glaeser, Gyourko, and Saks (2005), Gyourko, Mayer, and Sinai (2006), and Quigley and Rafael (2005).

We improve on the existing literature in four ways. First, by examining a national data set, we are able to consider a more nearly representative sample of U.S. municipalities. In contrast, much (though not all) of the existing literature is based on data describing particular municipalities or small regions. Second, we provide a basis for reconciling papers claiming that land price increases caused by regulation are harmful, for example, Gyourko, Mayer, and Sinai (2006), with those that conclude that such increases are beneficial, for example, Libecap and Lueck (2011). Third, since we exploit a rich description of municipal regulations, we are able to investigate exactly which types of regulation are harmful and which are beneficial. Our data provide weak evidence that minimum lot size regulation is less harmful and that red tape is more harmful. Finally, as the only paper to attempt to estimate the costs (our own-lot effect), benefits (our external effect), and supply effects of land use regulation, we provide the foundations for a more thorough understanding of the effects of land use regulation and of hedonic regressions more generally.[4]

Two of our three main econometric exercises are based on a regression discontinuity and the third is based on a spatial regression. The regression discontinuity design is increasingly popular and is used to investigate the effect of class sizes on educational attainment (Angrist and Lavy (1999)), the effect of changes in social assistance programs on employment (Lemieux and Milligan (2008)), and the effect of mayoral party affiliation on municipal policies (Ferreira and Gyourko (2009)), among others. Theory and best practice are described in Hahn, Todd, and van der Klaauw (2001) and Imbens and Lemieux (2008).

The regression discontinuity method has also been used to investigate the effect of policies that vary over physical space as one crosses from one administrative unit to another. In this case, the cutoff of interest is an administrative boundary. Holmes (1998) looks at the impact of changes in right-to-work laws on manufacturing employment near state borders. Black (1999) and Bayer, Ferreira, and McMillan (2007) look at the effect of changes in property values near school attendance zone boundaries. Duranton, Gobillon, and Overman (2008) look at the effect of changes in municipal taxation across municipal boundaries on the behavior of firms near these boundaries. One way to interpret this approach to identification and estimation is as a two step process: In

[4] Cheshire and Sheppard (2002) deal with all three possible effects in the context of a calibration exercise.

the first, these authors estimate the discontinuity in the outcome variable of interest; in the second, they examine the correlation between the magnitude of these cross-border discontinuities and the corresponding cross-border change in the policy variables of interest. This is also the intuition behind our own-lot and supply effect regressions.

To estimate the external-effect regression, we exploit fine scale spatial variation to estimate cross-border changes in regulation on bordering and more distant interior locations in neighboring municipalities. To our knowledge, the use of this sort of external effect to estimate the value of land use regulation is novel.

2. AN ECONOMETRIC MODEL OF LAND USE REGULATION AND LAND RENT

To develop an econometric model of land use regulation, land rent, and welfare, we proceed in several steps. In Section 2.1, we describe the effects of land use regulation near the border between a single pair of municipalities when land and residents are homogenous. This model suggests that we partition the effects of land use regulation into two components: an own-lot effect, which describes the cost to a landowner of regulatory constraints on the use of his parcel, and an external effect, which describes the benefit (or cost) that accrues to a land owner from regulatory constraints on his neighbors. In the model of Section 2.1, the demand for land is perfectly elastic, so land use regulation cannot affect land prices by affecting the supply of residential land.

In Section 2.2, we generalize to the case when residents are no longer identical in their preferences over municipality pairs. This heterogeneity in location preferences leads to a downward sloping demand for residential land in any given municipality, and if land use regulation affects the amount of land available for residential use, allows us to consider the relationship between land supply and price.

In Section 2.3, we consider the welfare implications of land use regulation and, in particular, analyze the welfare implications of changes in the supply of residential land caused by land use regulation. This analysis shows how to use the own-lot effect, the external effect, and the supply effect to calculate the marginal effect on welfare of a change in land use regulation.

In Section 2.4, we generalize our initial description of the process generating land prices to allow a description of agents and land that is general enough to form a basis for plausible empirical specifications. Finally, in Section 2.5, we extend our model from one to many pairs of municipalities and develop estimating equations.

2.1. *Land Use Regulation and Land Rent Around a Single Border With Homogenous Land and Residents*

To understand the effects of land use regulation on land rent near a municipal border, imagine two municipalities, L and R, that occupy homogenous residential land between $-\overline{\overline{x}}$ and $\overline{\overline{x}}$. At each location x, there is a parcel of measure 1 available for residential use so that the total measure (area) of land in the two municipalities is $2\overline{\overline{x}}$. The two municipalities share a border at the origin. The left municipality consists of land to the left of zero and the right municipality occupies all land to the right. Let $m \in \{L, R\}$ index the municipalities.

The two municipalities are populated from a pool of agents who all earn wage w, pay $p(x)$ for their residential location (the price, rent, or value of their parcel), and derive utility $V(x; \cdot)$ from their location. The utility of each resident is $u(x) = U(w - p(x))V(x; \cdot)$. We discuss V in more detail below. In this section, we suppose that locations differ only in their distance to the border and that the opportunity cost of land in both municipalities is zero.

The set of agents is given by Θ and agents are distinguished by their type θ. All agents choose between locations in the two municipalities and an alternative city where they receive a reservation utility, e^{θ}. Measure 1 of agents inhabit measure 1 of land and the measure of Θ is large relative to the size of the two municipalities. To begin, we assume that there is just one type of agent.

In any equilibrium with freely mobile agents, all residents are indifferent between all locations in either municipality and the alternative city. Thus $\ln(u(x)) = \theta$ for all x. If we assume that $U(x) = e^{w-p(x)}$, then this implies that land rent is $p(x) = w - \theta + \ln(V(x; \cdot))$ for all x. We adopt this particular form of U in the interest of clarity. In our empirical work, we experiment with nonlinear terms in w as a way of testing the robustness of our results to this assumption.[5]

Throughout our analysis, we assume that mobility between the two municipalities and the alternative city is costless. While moving is clearly costly, conditional on the decision to move, the cost of choosing one location as opposed to some other nearby location is essentially zero. Since it is these freely mobile agents who set the price in the land market, our assumption of free mobility, in addition to being standard in the literature (e.g., Brueckner (1987), Henderson (1985)), is a defensible stylization. Given homogenous outside alternatives, an immediate consequence of free mobility is that the demand for residential land must be perfectly elastic.

Let $z^m \geq 0$ denote regulation in municipality m and let increasing values of z^m reflect increasingly stringent regulation. Every location in each municipality is subject to development, but development in both municipalities is subject to

[5]This derivation of the land rent gradient parallels the derivation of land rent gradients in more conventional models based on freely mobile agents. For an example, see Brueckner (1987) for the case of the monocentric city. In particular, in models with freely mobile agents, land rent is determined by the gap between an agent's willingness to pay to live at a particular location and the potential utility at other possible locations, so that the utility value of residence in the reservation location, here denoted θ, always appears as a component of the land rent gradient (e.g., Brueckner (1987, equation 18)).

regulation. We adopt the convention that the left municipality is always regulated at least as intensively as the right, so that $z^L \geq z^R$.

We would like to know how $p(x)$ varies with location and regulation. One possible effect of land use regulation is to decrease land values by constraining how a landowner develops his land. Call this effect of regulation an *own-lot effect*. This effect might operate in many ways: minimum lot size constraints may lead to houses and lots that are "too large," waiting times for permits may increase financing or design costs, or building codes may increase construction costs. For our purposes, the mechanics of how the own-lot effect impacts the value of land are not important, only that regulation z^m is binding and affects land values. Formally, let $v_{\text{OWN}}(z^m) \in R$ denote the component of land value due to the own-lot effect from regulation z^m. Consistent with the discussion above, we expect $v'_{\text{OWN}} < 0$. Since regulation may vary at the municipal boundary, we define an own-lot effect function for the entire area of the two municipalities as

$$V_{\text{OWN}}(x, z^L, z^R) = \begin{cases} v_{\text{OWN}}(z^L) & \text{if } x \leq 0, \\ v_{\text{OWN}}(z^R) & \text{if } x > 0. \end{cases}$$

Unless regulation is the same in both municipalities V_{OWN} is a step function with a discontinuity at zero.

It may also happen that land use regulation has an external effect whereby the value of any given location is affected by regulation at nearby locations. For example, the value of a parcel may vary with the density permitted at nearby locations if residents have a taste for low (or high) density or minimum setback requirements may decrease the risk of fire spreading from one house to another. Alternatively, if regulation discourages neighbors from improving blighted properties or compels them to build unattractive structures, then the external effect can be negative (in fact, our data suggest that the external effect from land use regulation is negative).[6] As for the own-lot effect, the exact mechanics of how regulation causes an external effect are not important to our analysis. Formally, let $v_{\text{EXT}}(z^m) \in R$ denote the component of land value due to the external effect of regulation z^m.

The external effect of regulation affects locations near regulated locations. Thus, locations near $x = 0$ are exposed to the regulations of both municipalities. In particular, parcels in the right municipality but very close to zero are equally exposed to locations subject to z^L and to locations subject to z^R. The same statement is true for locations close to zero in the left municipality. Locations progressively further from the border are progressively more affected by the regulation of their own municipality.

[6]One can easily imagine other explanations for negative external effects. For example, regulation mandating minimum lot size may decrease the value of the landscape by decreasing access to public open space. Alternatively, regulation might mandate single family residential use when residents prefer mixed-use development.

To formalize this intuition, define a continuous weakly increasing function $\delta(x)$ that satisfies $\delta(x) = -1$ if $x \leq -\bar{x}$, $\delta(0) = 0$, and $\delta(x) = 1$ if $x \geq \bar{x}$. We then write the utility derived from the external effect of regulation as

$$V_{\mathrm{EXT}}(x, z^L, z^R) = \frac{1-\delta(x)}{2} v_{\mathrm{EXT}}(z^L) + \frac{1+\delta(x)}{2} v_{\mathrm{EXT}}(z^R).$$

For $x \leq -\bar{x}$, the external effect of regulation is entirely due to regulation in the left municipality and equals $v_{\mathrm{EXT}}(z^L)$. For $x \geq \bar{x}$, the external effect of regulation is entirely due to regulation in the right municipality. As we move closer to the municipal boundary, the utility derived from the external effect of regulation is a weighted sum of exposure to regulation in both municipalities. When we are precisely at the municipal border, the regulations of each municipality are equally weighted. Since δ is continuous, V_{EXT} is continuous in x.

That the external costs or benefits of proximity to a regulated landscape decay with distance is one of the principal assumptions that we must make to identify the external effect of regulation. We assume that the decay function operates over spatial scales that are small relative to the size of municipalities. This is consistent with the literature that estimates the effects of open space on residential housing prices and typically finds that the effects of open space attenuate over distances of less than 1 mile, for example, Irwin and Bockstael (2002) or, for a survey, McConnell and Walls (2005).

We now define the $V(x; \cdot)$ term in our original formulation of utility as $V(x; \cdot) = V_{\mathrm{OWN}}(x, z^L, z^R) V_{\mathrm{EXT}}(x, z^L, z^R)$ and write utility as[7]

$$u(x) = e^{w-p(x)} V_{\mathrm{OWN}}(x, z^L, z^R) V_{\mathrm{EXT}}(x, z^L, z^R). \tag{1}$$

[7]Here, we define preferences over regulation. To see how these preferences may be derived from preferences over landscape characteristics, let $h(x)$ denote a characteristic of housing at location x (e.g., lot size) and suppose that we have

$$V(x, h(x), h) = V_{\mathrm{OWN}}(h(x)) \left[\frac{1}{2\bar{x}} \int_{x-\bar{x}}^{x+\bar{x}} \tilde{v}_{\mathrm{EXT}}(h(x'))\, dx' \right]$$

for $x \in (-\bar{x}, \bar{x})$, $V(x, h(x), h) = V_{\mathrm{OWN}}(h(x)) \tilde{v}_{\mathrm{EXT}}(z^L)$ for $x < -\bar{x}$, and $V(x, h(x), h) = V_{\mathrm{OWN}}(h(x)) \tilde{v}_{\mathrm{EXT}}(z^R)$ for $x > \bar{x}$.

If z is binding everywhere, then $h = z^L$ for $x < 0$ and z^R otherwise. If we let $\delta(x) = x/\bar{x}$, then the expression above evaluates to

$$\begin{aligned} &V_{\mathrm{OWN}}(x, z^L, z^R) \left[\frac{1-\delta(x)}{2} v_{\mathrm{EXT}}(z^L) + \frac{1+\delta(x)}{2} v_{\mathrm{EXT}}(z^R) \right] \\ &\quad = V_{\mathrm{OWN}}(x, z^L, z^R) V_{\mathrm{EXT}}(x, z^L, z^R), \end{aligned}$$

exactly as in the main text.

In this example, landscape preferences take a form commonly used to study agglomeration effects (e.g., Lucas and Rossi-Hansberg (2002)), but omit distance discounting to ease exposition.

With free mobility, land rent adjusts so that all agents are indifferent between every location in the two target municipalities and their reservation location. The resulting land rent gradient is

$$p(x) = w - \theta + \ln\bigl(V_{\mathrm{OWN}}\bigl(x, z^L, z^R\bigr)\bigr) + \ln\bigl(V_{\mathrm{EXT}}\bigl(x, z^L, z^R\bigr)\bigr). \tag{2}$$

The solid line in the top panel of Figure 1 illustrates a land rent gradient consistent with (2) in the case where the external effect of regulation is positive. Recalling our convention that the left municipality is more highly regulated, land rent increases as we travel left from the border and exposure to the less regulated right municipality drops, reaching the level associated with full exposure to z^L at distance $\overline{x}$ from the border. We see the opposite pattern in the

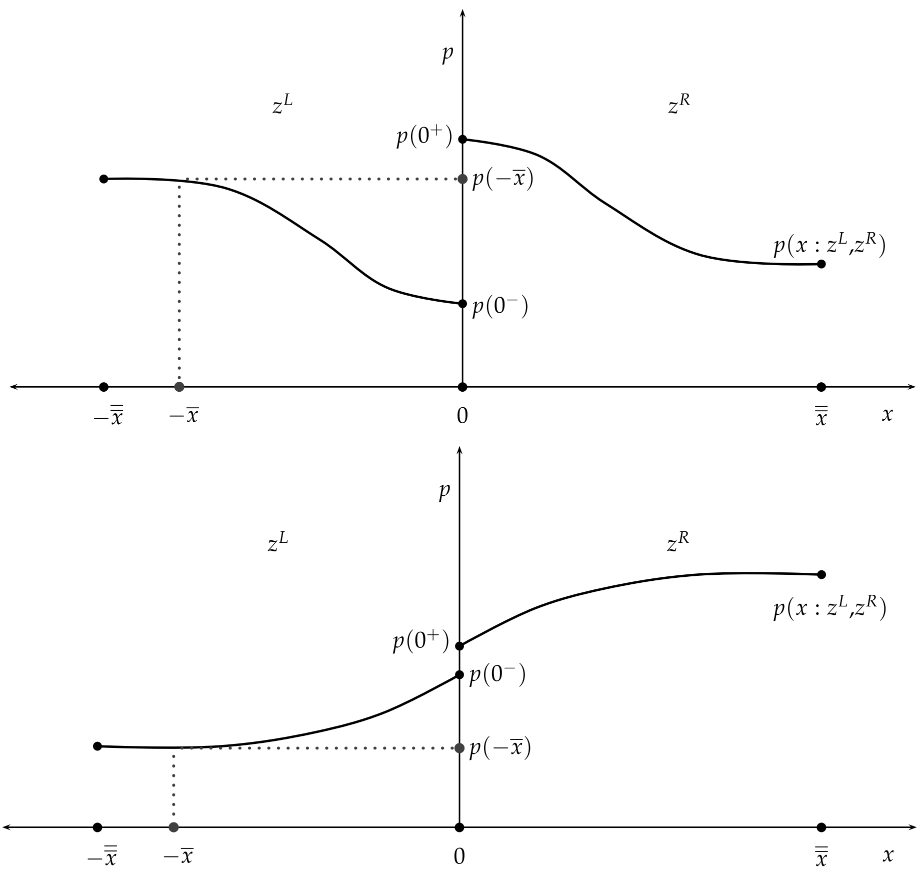

FIGURE 1.—Land rent gradient across the municipal border. The top panel illustrates the case where the external effect is positive. The bottom panel illustrates the case when the external effect is negative, while the own-lot effect remains negative.

right municipality. As we move to the interior of this municipality, land rent drops as exposure to z^L decreases and exposure to z^R increases. Land rent varies discretely at the border. Locations on either side of the border face the same external effect of regulation, since both are equally exposed to regulation of each municipality, but municipalities on the right pay the lower own-lot effect associated with z^R, while those on the left pay the higher own-lot effect of more stringent z^L.

The bottom panel of Figure 1 is analogous to the top panel, but considers the case where the external effects of regulation are harmful, while the own-lot effect remains negative. In this case, land rent decreases discretely when we move from the less constrained right municipality into the left, and declines continuously as we move into the more regulated municipality and increase our exposure to the less desirable regulated landscape.

Figure 1 makes our approach to the problem clear. By comparing parcels on opposite sides of the border, we compare parcels that experience the same external effect of regulation, but different own-lot effects. Using equation (2), we have

$$p(0^+) - p(0^-) = \left[\ln\left(V_{\text{OWN}}\left(0^+, z^L, z^R\right)\right) - \ln\left(V_{\text{OWN}}\left(0^-, z^L, z^R\right)\right)\right]. \tag{3}$$

In words, we can infer the relationship between changes in regulation and changes in the magnitude of the own-lot effect from change in land rent across a municipal border. This intuition will be the basis for our own-lot effect estimation.

Alternatively, if we compare a parcel near the municipal boundary with a parcel far from the boundary, then we compare parcels subject to the same own-lot effect, but different external effects. The boundary parcel is equally exposed to both types of regulation while the interior parcel is wholly exposed to the external effect of its own municipality's regulation. From equation (2), we have

$$p(-\overline{x}) - p(0^-) = \left[\ln\left(V_{\text{EXT}}\left(-\overline{x}, z^L, z^R\right)\right) - \ln\left(V_{\text{EXT}}\left(0^-, z^L, z^R\right)\right)\right]. \tag{4}$$

This suggests that we infer the effect of a change from equal exposure to two levels of regulation to sole exposure to one level of regulation by looking at changes in the land rent gradient as we move from a point very near a municipal border to a point in the interior. This intuition will be the basis for our external effect estimation.

It is useful to note that if we assume that V_{EXT} is symmetric around $x = 0$ for given z^L and z^R, then using (2) and a little bit of algebra, we have

$$p(-\overline{x}) - p(\overline{x}) = \left(p\left(0^-\right) - p\left(0^+\right)\right) + 2\left(p(-\overline{x}) - p\left(0^-\right)\right). \tag{5}$$

Equation (5) allows us to use estimates of equations (3) and (4) together to estimate $p(-\overline{x}) - p(\overline{x})$, the total effect of an increase in regulation from z^R to z^L

on unit land rent. Note that we could also calculate $p(-\overline{x}) - p(\overline{x})$ directly by comparing interior parcels. By construction, however, this involves comparing parcels far from each other that are more unlike in their unobservable characteristics. Thus, such a regression is subject to the identification problem we are trying to overcome: the systematic relationship between unobserved determinants of price and regulation.

Ignoring, for now, the possibility that regulation changes the amount of land available for residential use, we can approximate the total change in land rent that results from a change in regulation by

$$(6)\qquad \big(p(-\overline{x}) - p(\overline{x})\big) \times \text{residential land area.}$$

This approximation requires that we ignore the cross-border spillover effects that the more intensive regulation will have on nearby municipalities and the fact that the full effect of the regulation is not realized until we are some distance inside the municipality. On the other hand, consistent with results reported in Irwin and Bockstael (2002) and McConnell and Walls (2005), we expect the adjustment zones near the boundary of the municipality to be small relative to the size of the whole municipality, so this approximation error is probably not economically important.

For the regulation described by the top panel of Figure 1, the external benefit of regulation is sufficiently large that it offsets the costs of the own-lot effect, and hence we have $p(-\overline{x}) > p(\overline{x})$ and a positive overall effect on land rent. If the own-lot effect is large enough or the external effect is small enough, as in the bottom panel of Figure 1, then we will have $p(-\overline{x}) < p(\overline{x})$, and land use regulation has a negative impact on land rent.

2.2. *Regulation and Land Supply*

Part of the existing literature on the effects of land use regulation is concerned with the possibility that land use regulation affects land or housing prices by affecting the supply of residential land (e.g., Glaeser and Gyourko (2003), Glaeser, Gyourko, and Saks (2005)). In the framework developed above, such supply effects cannot occur: with homogenous agents and free mobility, the demand for land is perfectly elastic and supply changes do not affect price.

To investigate supply effects, we generalize our model in two ways. First, we distinguish between the amount of land in a municipality and the amount of land available for development: up until now, the two concepts have coincided. Suppose that developable land in a municipality is an interval that extends from the border to a point on the interior, $\overline{\overline{x}}(z^m)$. We suppose that $d\overline{\overline{x}}(z^m)/dz^m < 0$ for all $z^m \geq 0$, so that the supply of developable land is decreasing in regulation. In the absence of regulation, all land is subject to development and we have $\overline{\overline{x}}(0) = \overline{\overline{x}}$. In this way, we allow regulation to affect the supply of residential land while keeping the total measure of land unchanged at $2\overline{\overline{x}}(0) = 2\overline{\overline{x}}$. This stylized description of supply effects is tractable and leads us to partition the effects of land use regulation into three independent components: own-lot effects, external effects, and supply effects.

To simplify analysis, this model assumes that regulation removes land from development on the interior of the municipality. This approach has the advantage of simplicity and tractability. In particular, the price of these interior parcels is not subject to the boundary effects described above and so their value is easier to calculate. On the other hand, in reality, regulation probably affects the share of land developed more uniformly across the municipality. Indeed, our empirical work focuses on, changes in developed share at the edges of municipalities. In spite of this divergence, we maintain our simple framework in the interest of clarity. It is straightforward, though mathematically cumbersome, to generalize our description of the effects of regulation on the supply of developable land, and the implications of such a generalized model appear to be qualitatively similar to our more tractable and transparent formulation.

Our second generalization is to allow heterogeneity of outside options. Let the set of possible values for the outside option range over the set of positive real numbers, $\theta \in [0, \infty)$, and let $g(\theta)$ be the measure of agents with type θ. We suppose that g is continuous and differentiable. Let $G(\theta) = \int_0^\theta g(\theta')\,d\theta'$ be the measure of agents with type less than or equal to θ.[8] Define θ^* such that $G(\theta^*) = \overline{\overline{x}}(z^L) + \overline{\overline{x}}(z^R)$. That is, θ^* is the type such that we can fill our two municipalities with immigrants whose outside options are no better than θ^*. In equilibrium, θ^* will also be the type of the marginal agent.[9]

Since each agent is freely mobile, each has a bid–rent curve

$$p^b(x, \theta) = w - \theta + \ln\bigl(V_{\mathrm{OWN}}\bigl(x, z^L, z^R\bigr)\bigr) + \ln\bigl(V_{\mathrm{EXT}}\bigl(x, z^L, z^R\bigr)\bigr) \tag{7}$$

for their particular value of θ, just as in our original formulation. In equilibrium, the price at each location is determined by the marginal buyer, the agent with type θ^*. This leads to an equilibrium where our two municipalities are populated by agents with outside options in the range $[0, \theta^*]$ and the equilibrium price gradient is

$$p(x) = w - \theta^*\bigl(z^L, z^R\bigr) + \ln\bigl(V_{\mathrm{OWN}}\bigl(x, z^L, z^R\bigr)\bigr) + \ln\bigl(V_{\mathrm{EXT}}\bigl(x, z^L, z^R\bigr)\bigr). \tag{8}$$

[8]If we define $G(\infty) = \lim_{\theta\to\infty} G(\theta)$ and assume that this limit exists, then $\mathrm{Prob}(\theta' < \theta) = \frac{G(\theta)}{G(\infty)}$. That is, G is a scalar multiple of a standard probability distribution.

[9]This model of free mobility and heterogeneity of outside options resembles the model developed in Gyourko, Mayer, and Sinai (2006). This model, too, is based on freely mobile agents and taste heterogeneity. Relative to Gyourko, Mayer, and Sinai (2006), we provide a more detailed description of location choice, a simpler description of agent heterogeneity, and a more thorough investigation of the welfare implications of policy changes.

While our framework is nonstandard, the intuition behind this equilibrium is not: in a competitive land market, landowners set prices for the marginal agent.[10]

We imagine that our municipalities are large enough relative to the pool of potential immigrants that the municipalities cannot fill up if they accommodate agents with only a single value of θ. While the own-lot and external effect of regulation influence prices by affecting the utility of immigrants at particular parcels, changes in residential land supply influence land prices by affecting the identity of the marginal price setting agent. Under our assumptions, $\frac{d\theta^*}{dz^m} = \frac{d\overline{\overline{x}}(z^m)}{dz^m}(g(\theta^*))^{-1}$. Alternatively, given a change in the land available for residential use of Δ, the resulting change in θ^* is approximately $\Delta(g(\theta^*))^{-1}$. Thus, G determines both the size of the immigrant pool relative to the available residential land and the rate at which land prices vary with the supply of residential land. Note that the effect on the price gradient is the same whether land is removed from residential use in the left or right municipality and, hence, whether the supply restriction is caused by z^L or z^R.

Figure 2 illustrates the model. If regulation does not restrict the supply of residential land and we have homogenous agents, then we obtain the land rent gradient p, as in our original model. When regulation restricts the supply of residential land but does not affect the outside option of the marginal migrant, then the dashed portions of the gradient p are removed from the market and generate zero rent. With heterogenous agents, regulation that restricts the supply of land also shifts up the land rent gradient. The gradient $\hat{p}$ in Figure 2 illustrates.

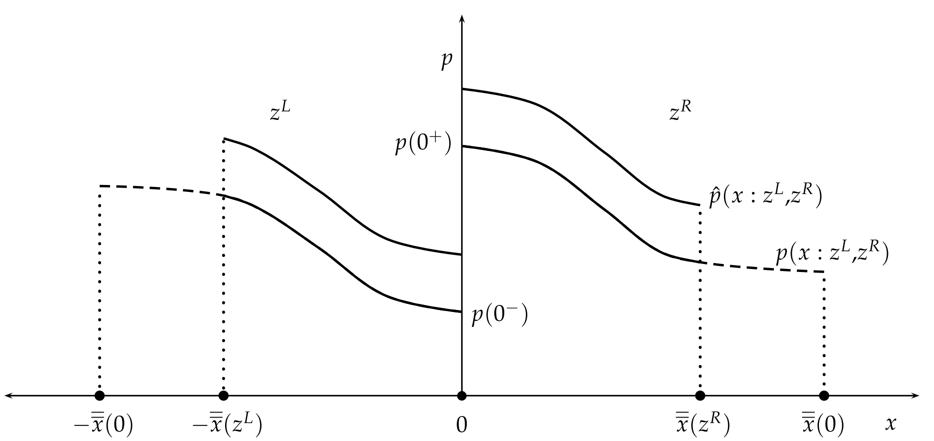

FIGURE 2.—Land rent gradient across the municipal border when regulation affects land supply.

[10]See, for example, pages 63–66 in Henderson (1985) or page 124 in O'Flaherty (2005).

With these generalizations, our model partitions changes in land rent due to regulation into three components: an external effect, an own-lot effect, and a supply effect. The empirical strategies suggested by equations (3) and (4) allow us to identify the external effect and the own-lot effect on the basis of changes in the exposure to regulation near municipal borders. Supply effects, however, operate by changing θ^* in both municipalities and result in a parallel shift in the entire equilibrium rent gradient. Equations (3) and (4) difference out parallel shifts in the rent gradient. This has three implications. First, that we can use the intuition suggested by equations (3) and (4) to identify own-lot and external effects of regulation without regard for supply effects. Second, since changes to land supply affect all locations in the two municipalities equally, we cannot use variation in land rent around a municipal border to identify supply effects on land prices. Third, since the share of land developed can vary across borders with changes in regulation, we can use cross-border changes in the share of land developed to investigate the effect of regulation on developed share.

We have here considered the possibility that immigrants differ in their tastes for living in particular locations. A technical appendix in the Supplementary Material (Turner, Haughwout, and van der Klaauw (2014)) considers the possibility that agents differ in their taste for regulation (as opposed to location) and argues that such preference heterogeneity by itself should not confound our estimates of the value of regulation. We consider other motivations for sorting into municipalities on the basis of demographic characteristics more carefully when we formulate our econometric specifications in Sections 2.4 and 2.5.

2.3. *Welfare*

We would like to evaluate the welfare consequences of a small change in regulatory intensity in a single municipality on the basis of observable changes in developed area and land prices. We consider the effect on welfare of increasing regulation in the left municipality, holding regulation in the right municipality constant. That is, we consider a change from $z_0^L = z^R$ to $z_1^L > z^R$.

To measure welfare, we sum aggregate land rent and a measure of consumers' surplus, defined precisely below. The use of consumers' surplus to measure welfare is standard. The use of land rent to measure welfare is also standard within the urban economics literature and corresponds loosely to producers' surplus in a conventional social surplus measure. That is, land rent is the profit retained by suppliers of land.[11]

Without loss of generality, consider a change to the regulation of the left municipality such that $\overline{\overline{x}}(z_1^L) = \overline{\overline{x}}(z_0^L) - \Delta$. That is, the change in regulation removes a small amount of land from residential use. To clarify the exposition,

[11] Alternatively, if utility were linear in income and shares of land rent were returned to a subset of residents, then a dollar of extra land rent would result in a dollar of extra utility.

we initially suppose that this change in regulation does not change either the own-lot or the external effects of regulation, so that the function $V(x,\cdot)$ is unchanged. Throughout this analysis, we adopt the convention that 0 and 1 subscripts refer to quantities determined by z_0^L and z_1^L, respectively. We let W_1 and W_0 denote final and initial welfare.

$\pi(z^L, z^R)$ denotes the land rent for a pair of municipalities with given land use regulation. Aggregate land rent is the integral of land rent over all residential land in the two municipalities:

$$\pi(z^L, z^R) = \int_{-\bar{\bar{x}}(z^L)}^{\bar{\bar{x}}(z^R)} p(x)\,dx. \tag{9}$$

Any given resident is strictly better off living in our subject municipalities than in their alternative if their bid rent (7) is strictly greater than equilibrium rent (8). Define *consumer's surplus* for a pair of municipalities as the integral over all such agents of this difference. That is,

$$S(z^L, z^R) = \int_0^{\theta^*(z^L,z^R)} \big(p^b(x,\theta) - p(x)\big) g(\theta)\,d\theta \quad \text{for arbitrary } x \in \big[-\bar{\bar{x}}(z^L), \bar{\bar{x}}(z^R)\big], \tag{10}$$

where the value of x is irrelevant because the gap between the bid and the equilibrium rent gradients is the same at all locations. Welfare, W, is the sum of aggregate rent and surplus.

With homogenous agents, regulatory supply restrictions do not affect price, but, by construction, affect the supply of residential land. Moreover, with homogenous agents, surplus is mechanically zero. Therefore, the effect of our hypothetical supply restriction on welfare is

$$W_1 - W_0 = \pi_1 - \pi_0 = -p\big(-\bar{\bar{x}}(z_0^L)\big)\Delta. \tag{11}$$

That is, a supply restriction decreases land rent by exactly the value of the land removed from the market. Since surplus is zero, this is the entire change in welfare from the supply restriction.

If we also allow our change in regulation to affect the shape of V, that is, to affect own-lot and external effects, then the total change in land rent can be approximated by the estimate of total change in rent due to regulation given in (6) plus the quantity in (11). More precisely, if we assume homogeneity of outside options, then we can approximate the total change in welfare resulting from own-lot, external, and supply effects by

$$\begin{aligned} W_1 - W_0 &= \pi_1 - \pi_0 \\ &\approx \Big(\big[p\big(-\bar{\bar{x}}(z_1^L)\big) - p\big(\bar{\bar{x}}(z^R)\big)\big] \times \text{residential land area}\Big) \\ &\quad - p\big(-\bar{\bar{x}}(z_0^L)\big) \times \Delta. \end{aligned} \tag{12}$$

To calculate the welfare implications of supply effects when agents have heterogenous outside options, recall the definition of θ^* and note that $\theta_1^* \approx \theta_0^* - \Delta(g(\theta^*))^{-1}$. By shrinking the area available for residential use, we exclude a small set of marginal agents whose alternatives are just worse than θ_0^*. Using this relationship and our earlier assumption that $V_1 = V_0$, it is straightforward to show that

$$\pi_1 - \pi_0 \approx \frac{\Delta}{g(\theta^*)}\big[\bar{\bar{x}}\big(z_1^L\big) + \bar{\bar{x}}\big(z^R\big)\big] - p\big(-\bar{\bar{x}}\big(z_0^L\big)\big)\Delta. \tag{13}$$

The first term on the right is residential land area times the increase in price caused by the supply restriction. This increase in price reflects the change in value of the outside option for the marginal agent. In Figure 2, this is the area between the rent gradients $\hat{p}$ and p. The second term reflects the loss in land rent resulting from the removal of land area Δ from the market. In Figure 2, this is the area under the dashed portions of the gradient p. This relationship is approximate only because $\Delta(g(\theta^*)^{-1})$ is an approximation of the change in θ^*.

To evaluate the effect of our hypothetical change in regulation on surplus, first note that bid rent curves (7) do not depend on land availability, and that bid rent (7) and equilibrium rent (8) differ only in the value of θ on which they are based. These two observations together mean that the change in surplus from our hypothetical supply restriction is

$$S_1 - S_0 = \int_0^{\theta_1^*} g(\theta)\big(\theta_1^* - \theta\big)\, d\theta - \int_0^{\theta_0^*} g(\theta)\big(\theta_0^* - \theta\big)\, d\theta. \tag{14}$$

This evaluates to[12]

$$S_1 - S_0 \approx -\frac{\Delta}{g(\theta_0^*)}\big[\bar{\bar{x}}\big(z_1^L\big) + \bar{\bar{x}}\big(z^R\big)\big]. \tag{15}$$

[12]Recalling that $\theta_0^* > \theta_1^*$ and that $\theta_1^* \approx \theta_0^* - \Delta(g(\theta_0^*))^{-1}$,

$$\begin{aligned}
&\int_0^{\theta_1^*} g(\theta)\big(\theta_1^* - \theta\big)\, d\theta - \int_0^{\theta_0^*} g(\theta)\big(\theta_0^* - \theta\big)\, d\theta \\
&\quad = \int_0^{\theta_0^*} g(\theta)\big[\big(\theta_1^* - \theta\big) - \big(\theta_0^* - \theta\big)\big]\, d\theta - \int_{\theta_0^* - \Delta g(\theta_0^*)^{-1}}^{\theta_0^*} g(\theta)\big(\theta_1^* - \theta\big)\, d\theta \\
&\quad \approx G\big(\theta_0^*\big)\big(\theta_1^* - \theta_0^*\big) - g\big(\theta_0^*\big)\left[\theta_1^*\theta - \frac{1}{2}\theta^2\right]_{\theta_0^* - \Delta(g(\theta_0^*))^{-1}}^{\theta_0^*} \\
&\quad = G\big(\theta_0^*\big)\big(\theta_1^* - \theta_0^*\big) - \frac{1}{2}\Delta^2\big(g\big(\theta_0^*\big)\big)^{-2} \\
&\quad \approx -\frac{\Delta}{g(\theta_0^*)}\big[\bar{\bar{x}}\big(z_1^L\big) + \bar{\bar{x}}\big(z^R\big)\big].
\end{aligned}$$

Unit land rent increases because of the supply reduction and this increase in unit rent times the area of land affected measures residents' loss of surplus from the land price increase. The relationship is approximate both because $\frac{\Delta}{g(\theta_0^*)}$ is a first order approximation of the change in price and because the loss of surplus by excluded agents whose outside options are near the marginal agent is not included. Since these nearly marginal agents lose little surplus, their impact on aggregate surplus is quadratic in Δ.

To calculate the change in welfare resulting from our hypothetical supply restriction, we sum change in rent (13) and change in surplus (15) to get

$$W_1 - W_0 \approx -p\big(-\overline{\overline{x}}\big(z_0^L\big)\big)\Delta. \tag{16}$$

Since the loss of surplus by immigrants equals land rent gain for landowners, the first term of (15) cancels the first term of (13) and the effect of a decrease in the supply of land on welfare is approximately equal to the rent on the lost area.

Comparing equation (16) with equation (11), we see that, to a first order approximation, the welfare implications of supply effects are the same with heterogenous agents and without. With heterogenous agents, losses to residents from supply induced price effects almost exactly offset gains to land owners, and the change in welfare is determined by the rent due to lost residential land area. With homogenous agents, land prices do not change in response to supply effects, and change in welfare is again determined by the rent due to lost residential land area.

If we also allow our change in regulation to affect the shape of V, that is, to affect the own-lot and external effects of regulation, then following the same logic as underlies equation (12), the change in welfare caused by a change in regulation is approximately[13]

$$\begin{aligned}(17)\qquad &\big(\big[p\big(-\overline{\overline{x}}\big(z_1^L\big)\big) - p\big(\overline{\overline{x}}\big(z^R\big)\big)\big] \times \text{residential land area}\big)\\ &\quad - p\big(-\overline{\overline{x}}\big(z_0^L\big)\big) \times \Delta.\end{aligned}$$

That is, change in welfare consists of two components. The first is the change in aggregate land rent resulting from changes in own-lot and external effects. The second is the change in land rent resulting from supply restrictions.

In Sections 2.4 and 2.5, we develop econometric specifications to estimate own-lot, external, and supply effects from observed changes in prices and land

[13]Note that our model determines which agents will occupy the two municipalities of interest. It does not tell us which municipality any particular agent will choose. Thus, it determines consumers' surplus only for the total area of the two municipalities and, consequently, our analysis considers this geography. If we further assume that, conditional on $\theta^* > \theta$, agents are assigned to the left or right municipality at random, then we can make the corresponding calculations for a single municipality.

use near municipal borders. Before we begin this exercise, however, it is useful to discuss the way the framework developed here relates to the existing literature and to comment on our model.

There is a large literature that uses land and housing prices to make inferences about place specific policies. This literature takes contradictory positions about whether increases in land and housing prices indicate welfare increases or decreases. The more common position is that increases in land or housing prices indicate welfare improvements. For example, increases in housing prices indicate better schools in Black (1999) and less disutility from superfund sites in Greenstone and Gallagher (2008). In the context of land use regulation, housing or land price increases are taken to indicate a beneficial change in a property demarcation rule in Libecap and Lueck (2011), to indicate a beneficial change in zoning in Zhou, McMillen, and McDonald (2008), and to positively reflect the value of proximity to open space in papers surveyed in McConnell and Walls (2005). On the other hand, a smaller literature on land use regulation (e.g., Glaeser and Gyourko (2003), Glaeser, Gyourko, and Saks (2005), Quigley and Rafael (2005))[14] takes the opposite position. In these papers, an increase in land or housing prices indicates a decrease in welfare.

Our model provides a basis for reconciling these two literatures. Papers in the first literature are exclusively interested in regulatory effects that resemble the external effect (e.g., Greenstone and Gallagher (2008), Gamper-Rabindran and Timmins (2011), McConnell and Walls (2005)) or the own-lot effect (e.g., Libecap and Lueck (2011), Zhou, McMillen, and McDonald (2008)). As appropriate for their particular applications, these papers implicitly ignore the other effects of regulation articulated here and, in particular, do not consider supply effects. Underlying these papers is a conceptual framework that, implicitly or explicitly, resembles our model with homogenous outside options. When agents have homogenous outside options, an increase in land rent is unambiguously good: with utility levels fixed by the homogenous outside alternatives, land rent is the only margin that can adjust and people pay more to live in places they like better.

Papers in the second literature are exclusively interested in the supply effects of regulation. These papers find that land use regulation decreases the supply of residential land and housing, which in turn drives up prices. While this literature interprets such price increases as harmful, our model suggests that care is required in interpreting such price increases. Land price increases caused by regulation also reflect own-lot and external effects. Moreover, to the extent that price changes do reflect supply restrictions, they are pure transfers from immigrants to land owners. On the other hand, regulated decreases in the *quantity* of land unambiguously decrease welfare and this quantity is probably

[14] Frech III and Rafferty (1984) consider both supply effects and external effects, and also regard price increases resulting from supply effects as harmful.

a more appropriate subject for research on the welfare implications of supply effects.[15]

Our model requires several further comments. First, the presence of supply effects provides a rationale for land use regulation in the model. Supply restrictions that transfer rents from immigrants to incumbent landowners should find a powerful constituency.

Second, since each immigrant has inelastic demand for land, our framework ignores the possibility that regulatory supply restrictions cause agents to reduce their land consumption. Models of cities that endogenize residential land consumption are standard (e.g., Brueckner (1987)) and allowing this possibility would introduce two offsetting effects on surplus. All residents would lose surplus as regulation reduced their land consumption, but the set of residents excluded from a municipality would shrink as individual land consumption fell. This generalization of our model focuses attention on the extent to which regulation changes lot size. Since information on lot size is not available in our data, this generalization would add complexity to our analysis but could not inform our estimation strategy.

Third, by construction, our analysis is limited to external effects of regulation that vary over small regions around municipal boundaries. If the external effects of municipal regulation do not decay over these small distances, as our model requires, these effects will be invisible to our estimations.

Fourth, our description of outside options provides a simple way to generate a downward sloping demand curve for residential land in a given border region. While the model is ad hoc, it is general enough to be consistent with the sorts of demand curves that would arise from a general equilibrium model of location choice (e.g., Bayer, Ferreira, and McMillan (2007)). In such a model, each potential immigrant would draw a value of θ for each border pair in the universe of possible border pairs. In equilibrium, all agents choose a location and prices adjust so that no agent wants to move. Estimation of such a model requires that the econometrician observe the universe of locations and of agents. We observe neither. Our data describe only a fraction of the available residential locations, and since Costar parcels are vacant, none of the agents. Thus, articulating a model of locational choice to endogenize g would involve theoretical constructs that cannot inform our estimations.

With this said, the implications of such a general equilibrium framework for our estimations merit careful consideration. Our model assumes that the distribution of outside options does not respond to changes in a municipality's regulation. Implicitly, each municipality is small relative to the rest of the world. While this assumption seems appropriate to our data, to understand the implications of relaxing it, consider a world consisting of two distinct pairs

[15]This does not mean that urban parks, for example, are welfare decreasing. It means that the welfare value of a park reflects the sum of lost rent on park land and of the external benefits from proximity to the park.

of municipalities, pair A and pair B, facing distributions of outside options g_A and g_B. Regulatory restrictions of the land supply in pair A naturally increases demand for land in B. In this context, a price increase in the only alternative pair of municipalities implies that the distribution of outside option values for potential immigrants to A shifts toward zero. We expect this sort of a shift in g_A to affect the identity of the marginal agent, but, exactly as in our analysis of supply effects above, not to affect our estimates of own-lot and external effects.

More generally, the distribution of people across a system of cities reflects an equilibrium process in which individuals trade off local amenities, congestion, and productivity across many locations. To the extent that agglomeration economies and congestion are externalities, we do not expect that the equilibrium distribution of city sizes will be optimal. In this case, by shifting people from one city to another, regulated land scarcity in one city could affect equilibrium populations and utility levels throughout the whole system. This will clearly complicate how we interpret changes in the price level and local land scarcity.

On the other hand, even embedding our model of land prices around borders in such a system of cities model does not appear to affect the validity of our estimates of own-lot and external effects, with one caveat. Implicit in our analysis is the idea that municipalities are small enough that agents with identical preferences for the regulated landscape can fill them (Appendix A considers an alternative). This seems reasonable if we are interested in the effect of a change in regulation by a single small municipality, all else equal. If we consider a national change in land use regulation then a particular municipality might experience a change in the way that its pool of potential immigrants values regulation. In this case, our estimates of the own-lot and external effect would be incorrect. Our methodology estimates the way that residents value regulation conditional on the current equilibrium distribution of people. If a regulatory intervention is big enough to dramatically rearrange this distribution, then our estimate of the own-lot and external effects of regulation is not based on the relevant set of people.

2.4. *Land Use Regulation and Land Rent Across a Single Municipal Border With Heterogenous Land and Residents*

To estimate own-lot and external effects using the intuition developed in Section 2.1, we require a credible empirical description of land rent gradients near municipal borders. This description of the land rent gradient should reflect the following possibilities: that locations may differ in their intrinsic attractiveness; that members of different demographic groups may have a taste for proximity to others in their own group; that people may sort on the basis of their tastes for local public goods not related to land use; that different demographic groups may value land use regulation differently; that the process that generates municipal land use regulation may be driven by many of the same fundamentals as those that determine land rent.

We maintain the same description of physical space developed earlier: there are two municipalities, L and R, that occupy the intervals $[-\overline{\overline{x}}(z^L), 0)$ and $(0, \overline{\overline{x}}(z^R)]$, respectively. However, we now suppose that each location x has an intrinsic attractiveness, $a(x)$, and that this intrinsic attractiveness can be decomposed into a deterministic component, $f(x)$, and a stochastic component, $\phi(x)$. We suppose that f is positive and, to fix ideas, decreasing in x. We require that f be continuous at 0 and suppose that for all x, $\phi(x)$ is mean zero and that $\mathrm{Cov}(f(x), \phi(x)) = 0$. Intuitively, there is more sunshine or a shorter commute as we move from right to left, with some noise around the trend. On average, the left municipality is nicer than the right. The assumption that f is continuous at zero requires that the municipal border does not divide qualitatively different types of land.

Our main inference problem is that regulation and land rent may both be systematically affected by characteristics of residents and parcels, some of which may be unobserved. With our description of the heterogeneity of land in place, we can now begin to unravel this problem. To begin, it is helpful to have in mind a heuristic model of the settlement and regulation process.

We imagine that the municipalities are populated in two stages. At time zero, measure 0 of immigrants locate in the two municipalities.[16] Each immigrant has a type, $N \in [0, 1]$. N can describe any demographic characteristic, but to ease exposition, we call it education. Types match to locations on the basis of their attractiveness, and we let $N_0(a(x))$ describe this matching. If, for example, $N_0' > 0$, then more highly educated people match to nicer places. The analysis is similar if $N_0' < 0$. What is important for our analysis is that there is a systematic relationship between demographic and parcel characteristics in equilibrium.

Time zero immigrants choose land use regulation for their respective municipalities democratically. Let N_0^L denote the demographic characteristics of the mean resident at $x \leq 0$ and let N_0^R denote mean education for residents located at $x > 0$. As a stylized way to describe the choice of regulation, let $z(N_0^m)$ describe the relationship between the mean voter and the resulting choice of regulation. We suppose that $z > 0$, and that z is continuous and increasing. It follows from our assumptions on f, $z(\cdot)$ and ϕ that if $N_0' > 0$, then $N_0^L > N_0^R$ and hence that $z^L > z^R$. That is, if nicer places attract better educated people, then they should be more intensively regulated. Let $\widehat{z} = [z(N_0^L), z(N_0^R)]$ denote the observed pair of regulatory intensities in our two municipalities.

A second wave of immigrants, of measure $\overline{\overline{x}}(z^L) + \overline{\overline{x}}(z^R)$ subsequently settles the remaining locations. These immigrants match to the location that gives them the highest utility. Denote the resulting distribution of immigrants by $N_1(x)$. By assumption, the initial agents occupied measure 0 of the available land, so that N_0 does not affect the supply of land available to the second wave

[16]This assumption simplifies exposition, but is not essential to the intuition we develop.

of immigrants. The first wave of immigrants, N_0, affects the second wave, N_1, only through its choice of regulation.[17]

As in Section 2.1, we suppose that each immigrant chooses between the left municipality, the right municipality, and the alternative city. In the left or right municipality, an immigrant receives a wage that does not vary with location. We allow wages and outside options to vary with education. Let $w(N_1)$ and $\theta(N_1)$ denote type specific wages and outside options. We suppose that both functions are continuous and increasing: wages and outside options increase smoothly with education.

Land use regulation affects the utility of immigrants in exactly the same way as described in Section 2.1, that is, according to equation (1).

Finally, we allow the possibility that immigrants derive utility from proximity to other immigrants whose types are close to their own. This effect will be determined by the immigrant's own location x, the immigrant's type $N_1(x)$, and the distribution of other agents, N_1. Let $\gamma(x, N_1(x), N_1)$ denote the utility derived from proximity to other people.[18]

We can imagine two basic mechanisms by which proximity to immigrants of particular types can affect utility. In the first, immigrants sort into municipalities on the basis of their tastes for local public goods. In this case, the value of γ is determined in much the same way as is regulation. It is based on the levels of public services and local taxes determined by election. And we expect γ to vary discontinuously with the level of public services at $x = 0$. Note that the level of γ in the two municipalities depends on mean demographic characteristics in the two municipalities. Alternatively, the value of γ may reflect a preference for proximity to people with similar characteristics.[19] In this case, γ is determined by the whole distribution of N_1, but it should vary continuously at the municipal border.

With this notation established, we write the utility of the agent at location x as the product of the different components described above.[20] That is,

$$u(x) = e^{w(N_1(x))-p(x)} V_{\mathrm{OWN}}(x, \widehat{z}) V_{\mathrm{EXT}}(x, \widehat{z}) \gamma\big(x, N_1(x), N_1\big) e^{a(x)}. \tag{18}$$

[17]For regressions containing z and N_1 as regressors not to be identified only on the basis of functional form, we require that z not be a deterministic function of N_1. Given this, since z is determined by N_0, we are implicitly requiring randomness in the relationship between N_0 and N_1. Alternatively, we could explicitly introduce randomness into the relationship between z and N_0.

[18]Since N_0 involves measure 0 of immigrants, provided that γ is constructed by integrating any continuous objective function on a real interval, N_0 does not affect the value of γ.

[19]One such γ is $\gamma(x, N_1(x), N_1) = \int_{-\overline{\overline{x}}}^{\overline{\overline{x}}} e^{-\rho|x-y|} |N_1(x) - N_1(y)|\, dy$ for ρ a positive real "decay rate."

[20]With the multiplicative specification of utility, the marginal utility of z varies with income. This allows us to rationalize our observation that different municipalities choose different regulations. In an additive specification, the marginal utility of regulation does not vary with income, so it is hard to rationalize the heterogeneity of observed regulation.

An equilibrium is an arrangement of types and a land rent gradient such that all agents are indifferent between their own location and their reservation location, and no agent would prefer another agent's location. Thus we have, for all x, that $\ln(u(x)) = \theta(N_1(x))$. Together with equation (18), we have the generalization of the land rent gradient corresponding to equation (2):

$$(19) \qquad p(x) = w\big(N_1(x)\big) - \theta\big(N_1(x)\big) + \ln\big(V_{\mathrm{OWN}}(x, \widehat{z})\big) + \ln\big(V_{\mathrm{EXT}}(x, \widehat{z})\big) + \ln\big(\gamma\big(x, N_1(x), N_1\big)\big) + a(x).$$

2.5. *Land Use Regulation, Own-Lot, External, and Supply Effects Across Many Municipal Borders*

In the model developed in Sections 2.1 and 2.2, only regulation changes when we cross the municipal border. In the more realistic model of prices of Section 2.4, it is at least possible that other determinants of land rent change discretely at the border. This means that we cannot generally identify the effect of regulation by looking at just one border. Instead, we must look for a relationship between land rent and regulation across a set of many municipal borders. An analysis of many borders requires that we generalize our notation. We postpone a discussion of supply effects until the end of this section.

Let $j \in \{1, \ldots, J\}$ index municipal borders and let a j superscript indicate a scalar or function that is particular to a border. We retain our convention that the more stringently regulated municipality is the left municipality and refer to individual municipalities as left or right of border j. Informally, each j refers to a replication of Figure 1 or 2.

We also generalize our earlier description of intrinsic attractiveness to allow heterogeneity across border pairs. Let $a^j(x) = f(x, \mu^j) + \phi(x)$ be the intrinsic attractiveness of location x of border j. As before, f describes a trend around the border, but we now suppose that it is parameterized by the pair $\mu = (\mu_1, \mu_2) \in R^2$ with $f(x, \mu) = \mu_1 + \mu_2 x$. We suppose that μ is a random variable with density $g_\mu : R^2 \longrightarrow [0, 1]$ and that each border draws a single μ^j. Thus, the μ's are a generalization of a fixed effect and parameterize the gradient of intrinsic attractiveness in a neighborhood of border j. Further suppose that $\phi(x)$ is a real valued random variable with density $g_\phi : R \longrightarrow [0, 1]$. We suppose that $\phi(x)$ is identically distributed for all j and x, that $E(\phi(x)) = 0$, and that $\mathrm{Cov}(\phi(x), \phi(y)) = 0$ for all $x, y \in [-\overline{\overline{x}}, \overline{\overline{x}}]$ and all j. Note that the intuition behind $a^j(x)$ is not changed from our initial discussion: $a^j(x)$ describes the fact that as we move from one municipality to another, locations may become systematically more attractive, with noise around this trend.

Our data describe transactions of particular parcels. To describe these data, as opposed to hypothetical gradients, we let i index parcels in a border pair j. We refer to the sale price of a particular parcel as p_i^j, with other parcel attributes indexed similarly. We refer to the location of parcel i in border pair j

as x_i^j. The magnitude of x_i^j is the distance from border j, with negative distances indicating displacement into the more intensively regulated municipality and positive displacements indicating displacements into the less regulated municipality.

We will sometimes need to distinguish between the municipalities that form a border pair. To do this, we recall that municipalities within a border pair are indexed by $m \in \{L, R\}$ and introduce an extra superscript. Thus, p_i^{mj} refers to the price of parcel i in the m municipality of border pair j. Similarly, p_i^{-mj} refers to a parcel in the other municipality of border pair j.

Naive Regression

If we consider only points far enough from municipal borders, then equation (19) lets us write the price for either municipality (say R) as

$$p^j(x) = w\big(N_1^j(x)\big) - \theta\big(N_1^j(x)\big) + \ln\big(V_{\mathrm{OWN}}\big(z\big(N_0^{Rj}\big)\big)\big) + \ln\big(V_{\mathrm{EXT}}\big(z\big(N_0^{Rj}\big)\big)\big) + \ln\big(\gamma\big(x, N_1^j(x), N_1^j\big)\big) + a^j(x). \tag{20}$$

This is an ordinary hedonic regression and is the basis for much of the extant research on land use regulation.

The problem with this approach is clear. Recalling that $N_0^{Lj} = E(N_0(a^j(x))| -\overline{\overline{x}} < x < 0)$ and that $z^L = z(N_0^{Lj})$, we see that z depends on the distribution of the municipalities' initial attractiveness and, in particular, on μ^j. It follows that if intrinsic attractiveness is not observed by the econometrician, as must surely be at least partly the case, then $z(N_0^R)$ would be correlated with the error term. It follows immediately that to the extent that physical geography partly determines a location's attractiveness, physical geography should not be regarded as a source of exogenous variation in regulation, as is sometimes done. Equation (20) also makes clear the problem with using historical demographic characteristics as instruments for regulation, as is sometimes done. Since these variables are themselves functions of the intrinsic attractiveness of the location, they are not orthogonal to unobserved components of $a(x)$.

Own-Lot Effect Regressions

To overcome the endogeniety problem that affects the cross-municipality regression described by equation (20), we exploit the intuition developed in Section 2.1 to separately estimate the own-lot and external effects of regulation. We first develop our own-lot effect estimating equation, the empirical counterpart of equation (3).

To begin, substitute the more realistic land rent gradient provided in equation (19) into the cross-border land rent differential of equation (3). Recall that $a^j(x) \equiv f^j(x) + \phi(x)$. By construction, V_{EXT} is continuous at zero and we assume f^j is continuous at zero. Thus we have

$$p^j(0^-) - p^j(0^+) = \big[w\big(N_1^j(0^-)\big) - \theta\big(N_1^j(0^-)\big) + \ln\big(V_{\text{OWN}}(0^-, \widehat{z})\big) + \ln\big(\gamma\big(0^-, N_1^j(0^-), N_1^j\big)\big) + \phi(0^-)\big] - \big[w\big(N_1^j(0^+)\big) - \theta\big(N_1^j(0^+)\big) + \ln\big(V_{\text{OWN}}(0^+, \widehat{z})\big) + \ln\big(\gamma\big(0^+, N_1^j(0^+), N_1^j\big)\big) + \phi(0^+)\big]. \tag{21}$$

Given a sample of municipal borders, this expression describes the relationship between the land rent gap at the border and the cross-border difference in regulation, along with several possible confounding factors. Our problem is to develop estimating equations that isolate the relationship between regulation and price.

To begin, we assume that only regulation varies discontinuously at the border. While we will relax this assumption in what follows, note that if γ reflects peoples' preference for being near others in their own demographic group, then we expect it to be described by a potential function of the sort given in footnote 19 and, consequently, to be continuous around zero. With this continuity assumption in place, we are left with

$$p^j(0^-) - p^j(0^+) = \ln\big(V_{\text{OWN}}(0^-, \widehat{z})\big) - \ln\big(V_{\text{OWN}}(0^+, \widehat{z})\big) + \phi(0^-) - \phi(0^+).$$

That is, if w, θ, and γ are all continuous, then any discontinuity in land rent across the municipal border entirely reflects differences in the own-lot effect in the two adjoining municipalities. These own-lot effects, in turn, are functions of regulation. To proceed, parameterize $\ln(V_{\text{OWN}}(0^+, \widehat{z})) - \ln(V_{\text{OWN}}(0^-, \widehat{z}))$ as a linear function of the difference in municipal regulations. That is,

$$\ln\big(V_{\text{OWN}}(0^-, \widehat{z})\big) - \ln\big(V_{\text{OWN}}(0^+, \widehat{z})\big) = B_{\text{OWN}}\big(z^{Lj} - z^{Rj}\big).$$

B_{OWN} measures the relationship between regulation and land rent, and is the parameter of interest.

We have assumed that w, θ, and γ are all continuous at the border. Thus, if we restrict attention to transactions that are close enough to the border, we can treat w, θ, and γ as constant, and the only systematic difference between cross-border parcels is due to regulation. Therefore, defining χ_i^{Lj} to be an indicator variable that is 1 if parcel i lies in the left municipality of border pair j, we have the estimating equation

$$p_i^j = \tilde{A}_0^j + \chi_i^{Lj} B_{\text{OWN}}\big(z^{Lj} - z^{Rj}\big) + \phi_i^j.$$

A little algebra shows that this specification is equivalent to the slightly simpler equation

$$p_i^j = A_0^j + B_{\mathrm{OWN}} z^{mj} + \phi_i^j, \tag{22}$$

where $\tilde{A}_0^j = A_0^j + B_{\mathrm{OWN}} z^{Rj}$.

Conditional on our other assumptions, this estimating equation will give us unbiased estimates of B_{OWN} provided that z^{Lj} and z^{Rj} are orthogonal to ϕ_i^j. This orthogonality follows from the fact that regulation is a function of mean municipal characteristics. Since we are considering only a small section of the municipality and since $\phi(0)$ gives us no information about ϕ at other values of x, it follows that $\phi(0)$ must be orthogonal to any function of municipal mean characteristics, land use regulation in particular.

While equation (22) allows us to estimate how the own-lot effect varies with regulation, this estimation relies on two strong assumptions: first, that we restrict attention to parcels close enough to the border that only own-lot effects and mean zero idiosyncratic error vary across parcels; second, that only regulation varies discontinuously at the border. In particular, we require that the systematic part of the intrinsic attractiveness of the parcels, f, does not vary discontinuously at the border. This is analogous to the continuity assumption required for a standard regression discontinuity design (RDD) estimation (Hahn, Todd, and van der Klaauw (2001)) and requires that municipal borders not divide one "quality" of land from another.

Equation (22) continues to rely on the assumption that w, θ, and γ vary continuously at the border. To relax this assumption, note that any discontinuity in any of these functions depends solely on demographic characteristics. Thus, we can parameterize a border discontinuity as a function of demographic characteristics. To accommodate this, let w^{Lj} denote a vector of municipal mean demographic characteristics for the left municipality and let w^{Rj} denote the corresponding vector for the right municipality. We can then write

$$\begin{aligned} &\big[w\big(N_1^j(0^-)\big) - \theta\big(N_1^j(0^-)\big) + \gamma\big(0^-, N_1^j(0^-), N_1^j\big)\big] \\ &\quad - \big[w\big(N_1^j(0^+)\big) - \theta\big(N_1^j(0^+)\big) + \gamma\big(0^+, N_1^j(0^+), N_1^j\big)\big] \\ &= D_0 + D_1\big(w^{Lj} - w^{Rj}\big). \end{aligned}$$

If we incorporate this parameterization into equation (21), then using the same logic that led to (22), we have

$$p_i^j = \big(D_0 + A_0^j\big) + D_1 w^{mj} + B_{\mathrm{OWN}} z^{mj} + \phi_i^j. \tag{23}$$

Estimating this equation will allow us to assess whether border discontinuities are partly due to changes in demographics across borders.

A comment about this regression is in order. If we estimate equation (22) when equation (23) is correct, since regulation and demographics are correlated (by construction), we will attribute to regulation part of the border gap that equation (23) attributes to demographics. If regulation does not cause the difference in demographics, then this means that equation (22) overstates the effects of regulation. On the other hand, if demographic sorting occurs because of land use regulation, then equation (22) estimates a long run or total effect of regulation, while equation (23) estimates a partial effect.

Note that parameterizing the cross-border gap with municipal *mean* demographics is not strictly correct. w, θ, and γ are all functions of $f(x, \mu)$, the attractiveness of particular locations. In addition to using municipal level demographics, it would be better to parameterize the cross-border gap as a function of very local demographics as well. Since our data describe large unoccupied parcels, this approach is not possible. There are no demographic characteristics for unoccupied land. We can, however, control for parcel specific measures of intrinsic attractiveness—measures of geography and commuting distance in particular. To the extent that these variables measure location specific heterogeneity correlated with demographics and the cross-border gap in land rents, they at least partially resolve this problem. More concretely, if wealthy people choose hilly neighborhoods close to the center of the city *and* wealthy people pay a premium for locations near other wealthy people, then controlling for distance to the center of the city and hilly neighborhoods will at least partly control for the fact that wealthy people like to be near wealthy people.

With this in mind, let y_i^j denote a parcel specific vector describing geography and commuting distance. We then write our final estimating equation based on equation (3) as

$$p_i^j = (D_0 + A_0^j) + D_1 w^{mj} + D_2 y_i^j + B_{\mathrm{OWN}} z^{mj} + \phi_i^j. \tag{24}$$

There is another possible objection to our estimation strategy. First, suppose that municipalities also choose a level of public service, such as the frequency of trash collection, that may vary discretely at the municipal border and that also impacts land prices. Denote this other regulation by z^{*L} and z^{*R}. Allowing for such regulation in equation (3), we have

$$\begin{aligned} & p^j(0^-) - p^j(0^+) \\ & \quad = \ln\big(V_{\mathrm{OWN}}(0^+, \widehat{z})\big) - \ln\big(V_{\mathrm{OWN}}(0^-, \widehat{z})\big) \\ & \qquad + \ln\big(V^*_{\mathrm{OWN}}(0^+, \widehat{z}^*)\big) - \ln\big(V^*_{\mathrm{OWN}}(0^-, \widehat{z}^*)\big) + \phi(0^-) - \phi(0^+), \end{aligned}$$

where V^*_{OWN} describes the contribution to land value of public services, $\widehat{z}^*$. It is clear that if regulation and public services are correlated, our approach will generally confound the effects of the two types of regulation. The exception to this is if z^* is itself a function of z. That is, if zoning for large lots leads to a community with twice weekly trash collection, then we estimate the total effect of zoning, including the effect of the induced high rates of trash collection.

We have two responses to this problem in our empirical work. First, we will control for local services explicitly as suggested by the equation above. Second, we expect that with democratically determined municipal policy, the cross-border change in other non-land-use policy will be systematically related to the cross-border change in observed demographics. Thus, equations (23) and (24) ought to substantially correct for these problems. If there are unobserved changes in other regulation at municipal borders, then we require that, conditional on control variables, these changes be uncorrelated with land use regulation.[21]

External Effect Regressions

We now turn to estimating the external effect of regulation by looking at the price difference between peripheral and interior parcels. To develop this external effect estimation, we substitute the description of the price gradient from equation (19) into the expression describing the price difference between interior and peripheral parcels, equation (4). Recalling that the own-lot effect of regulation must be the same for two parcels in the same municipality, this gives

$$\begin{aligned} p^j(-\bar{x}) - p^j(0^-) = {} & \big[w\big(N_1^j(-\bar{x})\big) - \theta\big(N_1^j(-\bar{x})\big) + \ln\big(V_{\text{EXT}}(-\bar{x}, \widehat{z})\big) \\ & + \gamma\big(\bar{x}, N_1^j(-\bar{x}), N_1^j\big) + f\big(-\bar{x}, \mu^j\big) + \phi(-\bar{x})\big] \\ & - \big[w\big(N_1^j(0^-)\big) - \theta\big(N_1^j(0^-)\big) + \ln\big(V_{\text{EXT}}\big(0^-, \widehat{z}\big)\big) \\ & + \gamma\big(0^-, N_1^j\big(0^-\big), N_1^j\big) + f\big(0^-\big) + \phi\big(0^-\big)\big]. \end{aligned} \tag{25}$$

This equation describes the relationship between cross-border changes in regulation and the price difference between peripheral and interior parcels, together with a detailed description of possible confounding factors.

If γ reflects the value of local public goods that results from a given sorting of population, then these public goods should be provided equally to the whole municipality. In this case, $\gamma(0^-, N_1^j(0^-), N_1^j) = \gamma(\bar{x}, N_1^j(-\bar{x}), N_1^j)$ and the two terms involving γ drop out of equation (25). If we also suppose that w and θ are constant within the left municipality, and recall that $f(x, \mu) = \mu_1 + \mu_2 x$,

[21] A final objection to these estimates of the own-lot effect is that they do not allow the value of regulation to vary systematically with the intrinsic attractiveness of a place. To address this issue, one could allow regulation to interact with measures of landscape, climate, or topography, and to test whether these interaction terms predict changes in land prices across borders. In practice, our sample is not large enough to allow us to estimate such an effect.

then we are left with

$$p^j(-\overline{x}) - p^j(0^-) = \ln(V_{\text{EXT}}(-\overline{x},\widehat{z})) - \ln(V_{\text{EXT}}(0^-,\widehat{z})) - \mu_2^j\overline{x} + [\phi(-\overline{x}) - \phi(0^-)]. \tag{26}$$

Next we parameterize $\ln(V_{\text{EXT}}(-\overline{x},\widehat{z})) - \ln(V_{\text{EXT}}(0^-,\widehat{z}))$ as

$$\ln(V_{\text{EXT}}(-\overline{x},\widehat{z})) - \ln(V_{\text{EXT}}(0^-,\widehat{z})) = B_{\text{EXT}}(z^L - z^R).$$

B_{EXT} is the parameter of interest and describes the impact of the external effect of regulation on land prices. Finally, restrict attention to parcels that are either in a narrow interval near a municipal border or in a narrow interval around $-\overline{x}$, and define χ_i^{Imj} to be an indicator variable that is 1 when a parcel i is an interior parcel lying within a narrow band around $-\overline{x}$ and 0 otherwise. We can now write the estimating equation (26) as

$$p_i^{mj} = A_0^{mj} + \chi_i^{Imj}B_{\text{EXT}}(z^{mj} - z^{-mj}) + \chi_i^{Imj}\mu_2^j\overline{x} + \phi_i^{mj}. \tag{27}$$

Inspection of this equation shows that our strategy of comparing interior and boundary parcels eliminates the level of f, but not its slope. However, since neither $\mu_2^j\overline{x}$ nor $z^{mj} - z^{-mj}$ varies within a municipality, we cannot separate the effects of these two components on p. To resolve this problem, we first introduce controls y_i^{mj} that allow us to estimate $\mu_2^j\overline{x}$ explicitly. These controls will include measures of physical geography and commuting distance. More formally, we parameterize $\mu_2^j\overline{x} = C_1 y_i^{mj} + \varepsilon_i^{mj}$. Substituting in (28) gives

$$p_i^{mj} = A_0^{mj} + \chi_i^{mj}B_{\text{EXT}}(z^{mj} - z^{-mj}) + C_1 y_i^{mj} + \varepsilon_i^{mj} + \phi_i^{mj}. \tag{28}$$

In equation (28), the municipality specific constant measures the level of land rent at the municipal border, and all variation between the border and interior points is attributed either to regulation or to changes in intrinsic attractiveness between interior and boundary locations. We have already established that regulation is orthogonal to ϕ. The additional orthogonality assumption required here is that regulation is orthogonal to ε. In words, this orthogonality condition is that the unobserved component of the slope of f is uncorrelated with regulation. Given our strong controls for parcel level physical geography, this does not seem like a strong assumption.

Changes in demographic characteristics across the border are another possible problem. If the contribution that demographics make to land rent changes discontinuously at the border, then this does not affect the external effect regression. Such a discontinuous shift affects both interior and peripheral parcels equally and, hence, affects only the intercept in equation (28). If the contribution of demographics to land rent varies continuously at the border, as might be the case if the wealthy prefer locations where a higher proportion of their neighbors are wealthy, then a cross-border change in demographics may affect interior and peripheral parcels differently in equation (28). More formally, to allow for the possibility that w, θ, and γ are not constant between our two intervals, let w^{mj} and w^{-mj} denote a vector of demographic characteristics for municipality mj and for its counterpart $-mj$. We parameterize the difference in these quantities as

$$\begin{aligned}(29)\qquad &\big[w\big(N_1^j(-\overline{x})\big) - \theta\big(N_1^j(-\overline{x})\big) + \gamma\big(\overline{x}, N_1^j(-\overline{x}), N_1^j\big)\big] \\ &\quad - \big[w\big(N_1^j(0^-)\big) - \theta\big(N_1^j(0^-)\big) + \gamma\big(0^-, N_1^j(0^-), N_1^j\big)\big] \\ &= C_0\big(w^{Lj} - w^{Rj}\big).\end{aligned}$$

With this notation in place, we write

$$\begin{aligned}(30)\qquad p_i^{mj} &= A_0^{mj} + \chi_i^{Imj} B_{\mathrm{EXT}}\big(z^{mj} - z^{-mj}\big) \\ &\quad + \chi_i^{Imj} C_0\big(w^{mj} - w^{-mj}\big) + C_1 y_i^{mj} + \phi_i^{mj}.\end{aligned}$$

While it would be desirable to control for demographic characteristics exactly at the locations of our transactions, the fact that our data describe large unoccupied parcels prevents this. The specification above approximates this ideal.

Last, as in our own-lot effect regressions, we are concerned that other public policies vary at the municipal border. To control for this problem, we include public policy measures as controls in our estimations. In particular, we treat these variables in the same way as we treat our land use regulations, by interacting the cross-border change in regulation with the interior parcel indicator.

It remains only to determine the widths and locations of the border and interior bins. While theory does not provide any guidance on this issue, the available literature suggests that the scale over which we should expect the external effect to decay is less than a mile. Thus, we will experiment with different sizes and locations for the interior and peripheral bins. By inspection of Figure 1, if increasing the distance of the interior bin from the border affects our estimates, then this bin should be moved further from the border.

Land Supply Regressions

To estimate the effect of land use regulation on land supply, we proceed in much the same way that we did in our own-lot effect regressions, with one important difference. In our own-lot regressions, our unit of observation was a parcel, while in our land supply regressions, our unit of observation is a municipality. To implement this empirically, we consider strips of land along each side of a municipal border and ask whether the share of land developed in such strips varies systematically with the cross-border change in regulation.

Formally, let s^{mj} be the share of land developed in a strip following the municipal border that is contained in municipality m of border pair j. We would like to know how this share changes with regulation. To estimate this relationship, we compare the change in share developed across municipal boundaries where regulation changes. The simplest form of this regression is

$$s^{mj} = A_0^j + B_{\text{SUPPLY}} z^{mj} + \varepsilon^j. \tag{31}$$

B_{SUPPLY} is the coefficient of interest and describes the effect of a 1 unit cross-border change in regulation on the cross-border difference in the share of land developed, while ε^j describes unobserved determinants of developed share. Note that this equation is similar to the own-lot effect regression (22), except that the unit of observation is a municipality rather than a parcel.

An estimation of this equation produces unbiased estimates of B_{SUPPLY} provided that the errors are uncorrelated with the cross-border change in regulation. We can pursue two strategies to assure that this condition holds. The first is to consider narrow strips near the border. In this way, we can be more confident that the unobserved characteristics of strips on either side of the border are similar. Second, we can include additional controls for possible sources of confounding variation. In our empirical work, we pursue both strategies. In addition to equation (31), which corresponds closely to equation (22), our empirical investigation of supply effects uses analogs to our other own-lot effect regressions.

3. DATA

Implementing the regressions described in Section 2.5 requires four principal types of data. To estimate own-lot and external effect regressions, we require a description of land transactions, in particular, the location, price, and other characteristics of parcels that changed hands. To estimate supply effect regressions, we require a description of how land is used in a neighborhood of the relevant borders and, in particular, whether land is developed or not. For all regressions, we require a description of land use regulation by municipalities, and a map that allows the parcel, land use, and regulation data to be integrated and border distances to be calculated.

To measure land prices, we use the proprietary Costar data. These data describe land transactions in 138 metropolitan statistical areas (MSAs) between 1983 and 2009. We note that Costar follows major metropolitan area markets whose boundaries do not strictly follow MSA boundaries. Thus, parcels in some of the 138 MSAs appear to be included as part of Costar's efforts to track transactions in a market that lies primarily in an adjacent MSA. Since Costar does not make their boundary files or market identifiers available, it is not clear how to count the number of markets that Costar covers. From Figure 5, the number of metropolitan areas covered by our sample of Costar data appears to be about 30. In addition to recording the latitude and longitude of each parcel, the Costar data record transaction price and date, parcel size, and many other details about the parcel. Most of our empirical analysis restricts attention to transactions that occurred during 2000–2009, although we check the robustness of our results with samples drawn from other years.

To measure the share of land in residential use, we rely on the 2006 National Land Use and Land Cover data (NLCD) from the United States Geological Survey (U.S. Geological Survey (2011)). These data are based on satellite images and describe land cover in each 30 m square cell of a regular grid covering the entire continental United States.

To measure municipal land use regulation, we use the Wharton Land Use Regulation data (WRLURI) (Gyourko, Saiz, and Summers (2008)). These data result from a 2005 survey of 2,729 U.S. municipalities and describe many different aspects of municipal land use regulation: minimum lot size, permit waiting times, and growth controls, in addition to an index that summarizes overall regulatory intensity. We rely principally on this index as our measure of regulation, although we also experiment with measures of particular regulations.

The WRLURI data describe regulation in both incorporated and unincorporated municipalities. Since the U.S. census does not produce a map that shows the boundaries of both types of units, we overlay a 2000 census map of places on the corresponding map of county subdivisions. We then match each municipality in WRLURI to this map. This allows us to assign regulation to places on the map. Since the Costar data record the latitude and longitude of each transaction, we can locate each Costar parcel in our map. We note that the WRLURI data describe 2,729 municipalities, while there are about 55,000 municipalities on our map. Thus, only a small fraction of U.S. area is covered by the WRLURI.

We must assign each transaction to a border that separates two adjacent municipalities. We do this by calculating the Euclidean distance from each parcel to the nearest municipal boundary and assigning the parcel to this boundary. This process simultaneously selects the neighboring municipality to each parcel. Thus we organize our data around municipal boundaries in conformance with our model.

For a parcel to inform our estimation, we must observe regulation in its home municipality and its neighboring municipality. After restricting attention to the period after the last quarter of 1999 and before the second quarter of 2009, and dropping transactions for which no price data are available, we are left with 136,400 transactions. Of these, 9,730 match to municipal borders for which the WRLURI data record regulation in the own and neighboring municipality.

As a more concrete illustration of our data, the top panel of Figure 3 maps all Costar parcels and municipalities in the Phoenix–Mesa MSA. Light grey indicates the extent of the MSA. Dark grey indicates the extent of WRLURI municipalities. Black dots indicate Costar transactions. White indicates a county or municipality not in WRLURI or in the MSA. The bottom panel provides a detail of the Glendale–Phoenix municipal boundary and of the Costar parcels matched to this boundary.

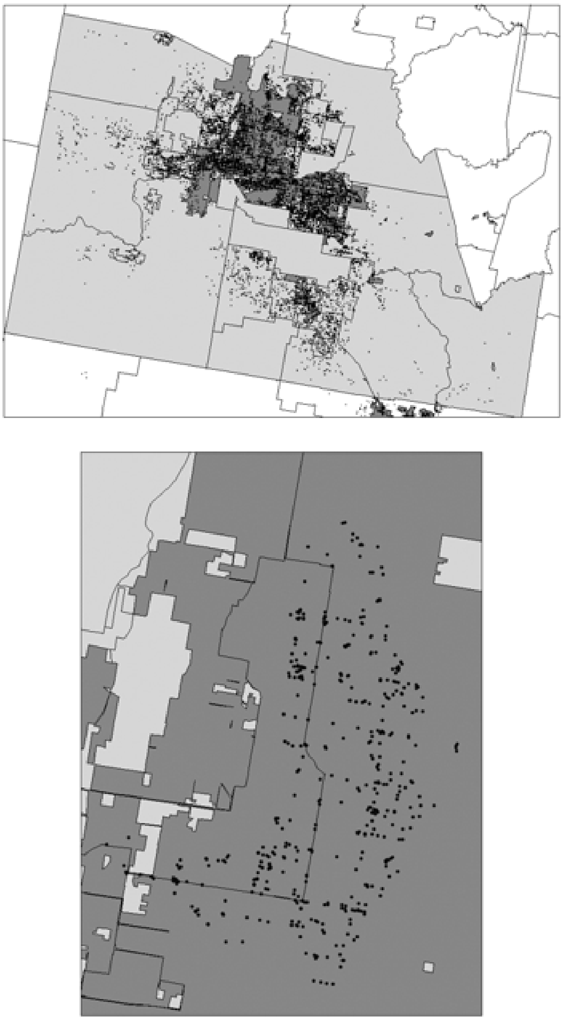

FIGURE 3.—Top: Distribution of parcels in metropolitan Phoenix. Light grey indicates counties in the Phoenix MSA, dark grey indicates municipalities, and black dots indicate parcel transactions. Bottom: Detail of Glendale–Phoenix border and parcels matched to this border. Glendale is on the right; Phoenix on the left.

Our regressions require that municipal borders be exogenous. That is, we require that they not be drawn to systematically separate more attractive land from less attractive land, and, in particular, that municipality boundaries not follow natural features where the attractiveness of land changes discretely.

To identify exogenous borders, we restrict attention to municipal boundaries that are straight lines and, therefore, do not follow features of the landscape.

Almost the entire area of the continental United States, outside of the original 13 colonies, was surveyed in accordance with the Land Ordinance Act of 1785. This act required that nearly all federal lands be surveyed and divided into regular "sections" as a precursor to their eventual settlement. See Libecap and Lueck (2011) for details. Many straight municipal boundaries appear to follow these old survey boundaries.

We rely on an algorithm to identify parcels associated with straight municipal boundaries. For each parcel, we identify the shortest vector that reaches from the parcel to the nearest municipal border. We then calculate the two vectors orthogonal to this vector of length 200 m and originating at the intersection of the first vector and the border. If and only if the ends of both of these orthogonal vectors lie within 15 m of the municipal boundary, we say that the parcel is associated with a straight boundary. Figure 4 illustrates this algorithm. In this figure, point x matches to a straight boundary and point y does not.

Our algorithm identifies points for which the nearest municipal boundary is a straight line. However, some of these points may also lie near a second municipal border. For these parcels, we are concerned that land values will be affected by the regulation in their own municipality and in two neighboring municipalities. This is not consistent with the intuition underlying our econometric methodology. To exclude such parcels, we draw a circle around any intersection of municipal boundary lines and exclude parcels within this circle from our analysis. Parcel z in Figure 4 illustrates such an excluded parcel. We

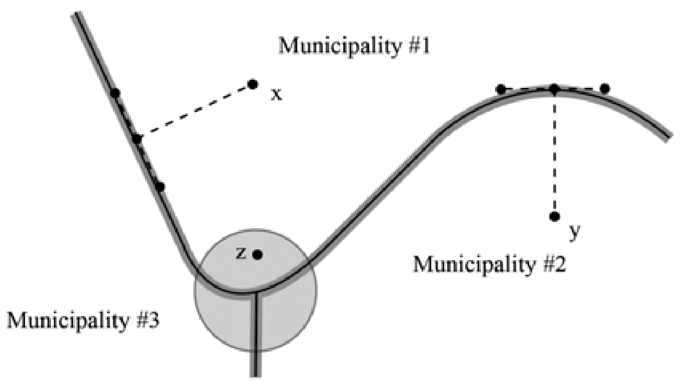

FIGURE 4.—Illustration of our algorithm for identifying straight borders. To determine whether a parcel matches to a straight border, we first calculate the shortest vector that connects the parcel to a municipal border (the length of this vector is our calculated distance to a municipal border). We next calculate the terminal points of the two 200 m long vectors originating on the border and orthogonal to the first vector. If and only if these terminal points both lie within 15 m of the border, then the parcel is determined to "match to a straight border." This figure illustrates the algorithm. The thin black lines represent municipal borders and the wider gray line represents a buffer around this border. Parcel x matches to a straight border; parcel y does not. We exclude parcels, such as z, that are close to two neighboring municipalities.

generate two samples using different versions of our algorithm. First, we exclude parcels that lie within 1 km of the intersection of municipal boundaries, that is, the circle in Figure 4 has a 1 km radius. Most of the results we report are based on this sample. Second, we consider the slightly larger sample that results when we exclude only parcels that lie within 500 m of an intersection of municipal boundaries. We use this sample for robustness checks.

Applying these algorithms to our data, we find that of the 9,730 parcels for which we have data on regulation, 2,933 match to straight borders and are at least 500 m from an intersection of municipal borders, while slightly fewer, 2,763, match to a straight municipal boundary and are at least 1 km from an intersection of municipal boundaries. These two sets of parcels lie in 218 and 197 distinct municipalities, respectively, and provide information about the land price gradient near 233 and 214 municipal border pairs.

Table I describes the parcels in our sample. Column 1 describes the set of all parcel transactions present in the Costar data after we restrict attention to the period from 2000 to 2009 and drop transactions without price data. Column 2 restricts attention to parcels in WRLURI municipalities and adjacent to WRLURI municipalities. Column 3 describes the set of parcels for which price and regulation data are available, that match to a straight border, and that lie at least 500 m from an intersection of municipality boundaries. Column 4 describes the same sample as column 3, but considers only parcels that lie at least 1 km from an intersection of municipality boundaries. Columns 5 and 6 describe the samples on which our estimations will primarily be based. In column 5, we consider all parcels within 100 m of a straight border, at least 1 km from an intersection of municipal boundaries, and for which we record regulation and price data. Column 6 is similar to 5, but considers the larger sample of parcels within 500 m of a border.

The WRLURI data oversample affluent, highly regulated suburban municipalities (Gyourko, Saiz, and Summers (2008)). Consistent with this, in Table I, we see that parcels for which WRLURI data are available are smaller, more expensive per square foot, and slightly closer to the metropolitan region's tallest building than an average Costar parcel. Comparing columns 2 and 3, we see that sample parcels matched to straight boundaries tend to be larger, slightly more remote, and more expensive than an average Costar parcel in a WRLURI municipality, although these differences are tiny compared to standard deviations. To get a sense for the size of the parcels described by the Costar data, note that an acre is 43,560 square feet, so that the average parcel in the sample of column 1 is about 3 acres, in the samples of columns 2–5, just under 1 acre, and in column 6, about 1.3 acres. Thus, the average Costar parcel in columns 2–5 is just over twice as large as the average lot in the three municipalities in suburban Massachusetts analyzed by Black (1999).

Restricting attention to parcels that match to straight borders and are at least 500 m or 1 km from an intersection of municipal boundaries, as we do in columns 3 and 4 of Table I, leaves slightly more expensive parcels than in the whole WRLURI sample, but the change is small relative to the standard er-

TABLE I

MEANS FOR MUNICIPAL CHARACTERISTICS[a]

Variable	All	All and WRLURI	All Straight and WRLURI, 500 m	All Straight and WRLURI, 1 km	Table II Row 3	Table VI Row 2
Price ($/ft^2)	29.75	77.88	111.25	117.16	12.52	11.30
	(625.25)	(288.85)	(390.50)	(401.52)	(19.67)	(16.33)
Size (000 ft^2)	132.46	33.69	38.92	37.13	39.30	68.05
	(1,486.25)	(173.39)	(164.98)	(165.97)	(115.97)	(255.37)
log(km to CBD)	3.43	3.05	3.14	3.11	3.96	3.90
	(0.90)	(1.09)	(1.19)	(1.20)	(0.83)	(0.83)
Share college	0.12	0.12	0.13	0.13	0.12	0.13
	(0.06)	(0.05)	(0.06)	(0.06)	(0.05)	(0.06)
abs(ΔShare college)	0.04	0.05	0.05	0.06	0.04	0.04
	(0.04)	(0.04)	(0.05)	(0.05)	(0.03)	(0.03)
Median income	57,769	55,376	56,288	56,196	60,128	61,202
	(18,038)	(18,037)	(18,555)	(18,428)	(18,667)	(20,764)
abs(ΔMedian income)	13,410	18,402	20,844	21,200	14,561	14,686
	(14,404)	(20,721)	(20,589)	(20,871)	(11,728)	(13,752)
WRLURI		0.47	0.53	0.53	0.76	0.70
		(0.94)	(1.00)	(1.01)	(0.68)	(0.71)
abs(ΔWRLURI)		0.76	0.80	0.79	0.71	0.75
		(0.58)	(0.56)	(0.56)	(0.43)	(0.50)
LLLA		0.04	0.12	0.14	0.54	0.41
		(2.00)	(1.79)	(1.78)	(1.78)	(1.92)
abs(ΔLLLA)		2.14	1.95	1.92	1.78	1.90
		(1.72)	(1.60)	(1.55)	(1.76)	(1.88)
DRI		0.17	0.18	0.19	0.21	0.23
		(0.38)	(0.39)	(0.39)	(0.41)	(0.42)
abs(ΔDRI)		0.26	0.27	0.27	0.33	0.34
		(0.44)	(0.45)	(0.44)	(0.47)	(0.48)
OSI		0.62	0.59	0.58	0.70	0.69
		(0.49)	(0.49)	(0.49)	(0.46)	(0.46)
abs(ΔOSI)		0.36	0.49	0.51	0.34	0.34
		(0.48)	(0.50)	(0.50)	(0.47)	(0.47)
# transactions	136,400	9,730	2,933	2,763	225	505
# WRLURI municipalities		513	218	197	76	118

[a]The means are shown for the absolute value of cross-border changes in municipal characteristics and for parcel transactions between 2000 and 2009. Standard deviations are given in parentheses.

rors and does not involve big changes in parcel or municipal characteristics. In columns 5 and 6 of Table I, we describe the samples of parcels on which most of our regressions are based. In column 5, we restrict the sample of column 4 to parcels within 100 m of a municipal boundary and in column 6, to parcels within 500 m. These are the samples later used in row 3 of Table II and row 2 of Table VI, respectively. These samples describe parcels on the edge of town, and, not surprisingly, these parcels are less expensive per square foot and further from the central business district than more central parcels. These parcels are also slightly more regulated than more central parcels. This is consistent with the tendency for more remote WRLURI municipalities to regulate more intensively.

From Table I, we conclude that the intersection of the Costar and WRLURI samples consists of large parcels of undeveloped land drawn from relatively affluent and relatively highly regulated suburbs. From this sample, we draw parcels near municipal boundaries. These parcels are larger than typical parcels in the WRLURI municipalities, but smaller than typical Costar parcels. Two inference problems may arise as a consequence of this sampling rule.

First, from Burchfield et al. (2006), we know that most development happens at the edges of currently developed areas. By construction, our econometric method restricts attention to boundary parcels and, therefore, to parcels most prone to development. It is natural to suspect that these will also be the parcels where regulation will have the largest effect. Thus, we suspect that our estimates, at least of the own-lot effect, may be larger in our sample than for a representative interior parcel.

Second, our model and estimations implicitly assume that regulation is enforced equally throughout each municipality. In particular, that undeveloped boundary parcels in our sample are not subject to different levels of enforcement than interior parcels. Should this assumption fail, then our own-lot regressions estimate the effect of the cross-border change in municipal regulation as enforced near the boundary. As boundary and interior enforcement diverge, our own-lot effect regressions provide less information about the effect of a regulation on interior parcels, although they continue to provide an accurate estimate of the effect of regulation as enforced at the boundary on boundary parcels. Furthermore, if enforcement effort varies systematically with distance from the border, then so too will the own-lot effect. Our external effect regressions will confound this change in the own-lot effect with the external effect they are intended to identify. The potential of significant bias in our external effect estimates is mitigated by the fact that we consider external effects operating at most a short distance from the border. Moreover, in some external effect regressions, we control for distance to the border (not interacted with the cross-border change in regulation) to account for a systematic change in own-lot effect if regulation enforcement varies systematically with distance to border. We find that this variable does not qualitatively affect our conclusions. Note that if regulation is enforced more stringently in interiors, then we expect our negative own-lot and external effect estimates to underestimate the true negative effects for interior parcels.

Figure 5 illustrates the geographic distribution of our data. In this figure, light grey indicates the extent of WRLURI municipalities. Medium grey dots

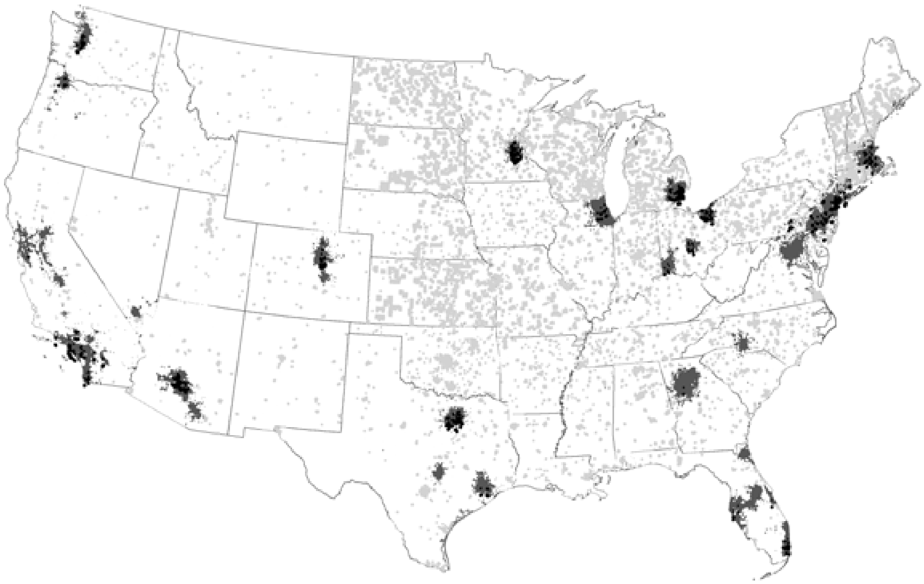

FIGURE 5.—Illustration of the geography of our data. Light grey indicates the extent of WRLURI municipalities. Medium grey dots indicate Costar parcels. Black dots indicate Costar parcels in WRLURI municipalities that match to a straight municipal boundary and are at least 500 m from a junction of municipal boundaries. These are the parcels described in column 3 of Table I.

indicate Costar parcels. This is the same set of parcels described in column 1 of Table I. Black dots indicate Costar parcels that match to a straight border where we observe regulation on both sides of the border. This is the sample described in column 3 of Table I. This map reveals several patterns. First, that WRLURI municipalities are disproportionately in the northeastern quadrant of the country. Second Costar parcels lie predominantly in about 30 major metropolitan areas. Third, the distribution of black dots appears to approximately follow the distribution of medium grey dots. Thus, restricting attention to Costar parcels that match to a straight boundary where we have regulation data does not result in a sample that obviously does not represent the spatial distribution of all Costar parcels. Since the Costar data report the universe of land transactions in the markets that they cover, this means that the spatial distribution of our transactions seems to represent the universe of transactions in these markets. Table AI in the Supplementary Material reports counts of transactions by metropolitan statistical area. Clearly, to the extent that the impact of regulation differs across geographic areas and across MSAs, estimates based on our sample may not generalize outside our sample.

A final sampling issue may also arise in our sample if the likelihood of a transaction depends on the level of regulation.[22] As a trivial example, consider the case where one side of a border is unregulated, while on the other, transactions are prohibited. In this case, we will never observe transactions on the regulated side of the border, and this border pair will be underrepresented in our sample. This type of sampling issue is not problematic for our analysis unless we expect a 1 unit increase in regulation to have a different effect in border areas with a larger cross-border change in regulation. We investigate this possibility that the effect of regulation is nonlinear in Table V. In any case, the data suggest that such exclusion of specific border pairs is actually rare.[23]

The WRLURI land use regulation index is constructed from 11 subindexes. Of these, two vary only at the state level and one is relevant only to New England. Four of the eight remaining indices describe the cost of negotiating the regulatory process. These indices are the *local political pressure index (LPPI)*, which is increasing in the propensity of survey respondents to report that local actors are important in the regulation process; the *local zoning approval index (LZAI)*, which ranges from 0–6 and is the count of the number of entities that must approve a zoning change; the *local project approval index (LPAI)*, which is analogous to the LZAI but gives the count, from 0–6, of entities that must approve a project; and the *approval delay index (ADI)*, which reflects survey respondents' statements about the length of time required to get administrative approval for development. In our empirical work, we aggregate these four measures into a single variable describing the costliness of the regulatory process. To do this, we normalize each of the four cost indices to mean zero and variance one, sum the four components and normalize the resulting sums to have mean 0 and variance 1. We concatenate the first letter of the component index names and name our regulatory cost index LLLA or sometimes, red tape index.

The Wharton data describe four other subindexes. The *density restriction index (DRI)* is 0 if minimum lot size is less than 1 acre and is 1 otherwise. The *exactions index (EI)* is 1 if the municipality mandates exactions to cover infrastructure costs. The *open space index (OSI)* is an indicator variable that is 1 if a municipality has open space set-aside requirements. Finally, the *supply restrictions index (SRI)* describes the extent to which there are explicit caps on the number of building permits issued in a given year. Our regression results

[22]We are grateful to Anna Aizer and an anonymous referee for pointing out this issue.

[23]If we consider Costar parcels within 10 km of a border for which we observe regulation on both sides of the border (a subsample of the sample described by column 2 of Table I), then the correlation of the count of parcels on the more regulated side with the cross-border difference in regulation is only -0.04 and is not different from zero at standard confidence levels. Similar results obtain for samples of parcels within 5 km and 1 km of the border, and if we restrict attention to straight borders. That is, these tests do not allow us to reject the hypothesis that there is no relationship between the cross-border change in regulation and the likelihood of observing a transaction.

primarily examine the effect of the overall index of regulatory intensity, the WRLURI index. While we experimented with estimates of the effects of the subindexes, we report few of them. It turned out that our sample was not always sufficiently large enough to precisely estimate the effect of the component indices, with estimates not tending to be robust across plausible alternative regression specifications.

In Table I, we see that the WRLURI index is marginally higher in the more restricted samples described by columns 5 and 6, although the difference is small compared to standard deviations. More importantly for our estimation strategy, we see that the mean cross-border change in the WRLURI index is about equal to the standard deviation of the index in the whole sample. Thus, our identification strategy relies on changes in regulation of similar magnitude to cross-sectional changes in the level of regulation.

To implement our developed land share regressions, we require data describing the share of developed land in each municipality at a spatial scale that is fine enough to allow us to implement the border study methodology described in Section 2. To do this, we rely on the 2006 NLCD (U.S. Geological Survey (2011)). These are remote sensing data that assign 1 of 20 land use codes to every 30 m square cell in a regular grid covering the entire continental United States. Four codes describe developed land: "developed, open space," "developed, low intensity," "developed, medium intensity," and "developed, high intensity."[24] The NLCD also indicates whether a pixel is principally water. Using these data, we can construct two measures of developed land for any region. The first is of developed area, which is simply the sum of the number of pixels in the four developed classes normalized by the number of pixels in the area. The second is developed nonwater, which is the sum of the number of developed pixels normalized by the number of nonwater pixels in the area.

Using our map of municipal boundaries and the WRLURI data, we identify all municipal borders that separate pairs of municipalities for which we have regulation data. We next create both 100 m and 250 m buffers on each side of these boundaries, excluding all land within 1 km of a third boundary. We then use these buffers to assign cells in the NLCD to a municipal border. With this

[24]These classifications correspond to classification codes 21–24. "Developed open space" describes cells where impervious surfaces account for less than 20% of total cover. Such cells contain some constructed materials, but are mostly vegetation in the form of lawn grasses and most commonly include large-lot single-family housing units, parks, golf courses, and vegetation planted in developed settings for recreation, erosion control, or aesthetic purposes. "Low intensity development" describes cells where impervious surfaces account for 20–49% of total cover. These are cells with a mixture of constructed materials and vegetation, and most commonly include single-family housing units. "Medium intensity development" describes cells where impervious surfaces account for 50–79% of the total cover. These are cells with a mixture of constructed materials and vegetation, and most commonly include single-family housing units. Finally, "high intensity development" describes cells where impervious surfaces account for 80–100% of the total cover. These are cells where people reside or work in high numbers. Examples include apartment complexes, row houses, and commercial/industrial areas (U.S. Geological Survey (2011)).

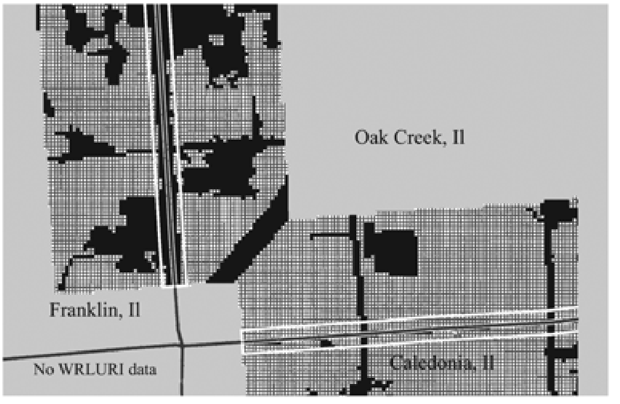

FIGURE 6.—Illustration of NLCD data and 100 m buffers in two municipal border pairs. Cross-hatching indicates undeveloped land. Black indicates developed land. Dark grey lines indicate municipal borders and white lines indicate the extent of 100 m buffers.

done, we are able to calculate the developed share and the developed nonwater share for strips of 100 m and 250 m width on either side of every municipal border where we record regulation data.

Figure 6 illustrates two such municipal border pairs about 70 miles northwest of Chicago. In this figure, hatching indicates pixels classified as undeveloped in the NLCD, while black indicates a pixel in one of the four developed classifications. Dark grey lines indicate municipal borders and white lines outline 100 m strips on either side of borders for which we record WRLURI data on both sides of the border. In the top part of the map, the North–South municipal border divides Oak Creek and Franklin, Illinois, while to the right, the East–West border divides Oak Creek from Caledonia Illinois. We do not record WRLURI data for the municipality in the bottom left of the map, and so these borders are not part of our analysis.

In our supply effect regressions, we are able to treat our regulation measures in exactly the same way as in our other regressions. To calculate most of our other controls, however, we calculate parcel weighted means on the basis of the parcels that lie in the relevant buffer strips. For example, for each strip we calculate parcel weighted distance to the tallest building or terrain roughness from the set of parcels lying in the strip. This is a simple way to insure that the samples we use for the supply regression describe the same municipalities as we use in our other regressions. Appendix B provides a more detailed description of these variables.

It remains to identify straight line boundaries for our municipality level supply effect regressions. This is a different problem than identifying parcels that match to straight segments of boundaries, as we have done for our external and own-lot effect regressions. In one case we are picking out straight segments of borders. In the other, we want lines that are, somehow, globally straight. Such a globally straight border might well be comprised of connected straight segments and there is, as yet, no algorithm for identifying such lines in maps.[25] To overcome this problem, we treat borders as "straight" if and only if at least 50% of the parcels contained within their boundaries match to a straight boundary.

4. RESULTS

4.1. *Own-Lot Effect Regressions*

Table II reports the results of our estimates of own-lot effect regressions using the sample of parcels that match to any straight municipal border for which we have WRLURI data. The table presents five panels of results. In all but the second panel, we consider parcels at least 1 km from any intersection of municipal borders, while the second panel considers the slightly larger sample of parcels at least 500 m from any intersection. All columns consider transactions that occurred between 2000 and 2009.

The dependent variable in every case is parcel price in dollars per square foot. Each cell in this table reports an estimate of our own-lot effect estimating equations. Column 1 estimates equation (22), while columns 2–6 estimate variants of equations (23) or (24). For a particular specification, each cell of the table reports the coefficient of a regulatory index, B_{OWN}, its standard error in parentheses, and the number of parcels and borders on which the estimate is based. Standard errors are clustered by border pair. The top two panels investigate the effects of the WRLURI index. The lower three consider the red tape index, LLLA, the density restriction index, DRI, and the open space restriction index, OSI. We do not report results for other WRLURI subindexes because our investigations did not uncover robust estimates of their effects.

The bottom panel of the table indicates the control variables used for all regressions in each column. Every regression includes a dummy variable for each border pair. All regressions include a dummy for each of the 37 quarters covered by our sample. Parcel controls I include the area of the parcel in square feet and the square of this variable, the log of distance to the tallest building in the metropolitan area and the square of this variable, and a measure of the roughness of the terrain in disks of 500 m, 5 km, and 10 km centered on the parcel. The control variables in Demographics are municipal share of black population, municipal share of Asian population, municipal population share with at least a high school degree, the municipal population share with at least a college degree, and municipal median household income. Parcel controls II includes total 1994 employment in rings of 500 m, 5 km, and 10 km radius centered on the parcel, and total developed area (from 1992 remote sensing

[25] One seemingly promising candidate is sinuosity. *Sinuosity* is the ratio of the path length to the linear distance between endpoints. A large value of this ratio indicates an indirect path from one endpoint to another. We have not attempted to implement this because it does not distinguish between a linear spline, which we would like to count as straight, and a truly curvy boundary.

TABLE II
OWN-LOT EFFECT REGRESSIONS[a]

	(1) [2000–2009]	(2) [2000–2009]	(3) [2000–2009]	(4) [2000–2009]	(5) [2000–2009]	(6) [2000–2009]
WRLURI 1 km						
All	2.62	5.72	7.97	5.25	4.36	7.19
	(7.98)	(10.26)	(11.52)	(13.95)	(11.35)	(11.15)
	2,763/214	2,648/206	2,648/206	2,648/206	2,637/206	2,637/206
Dist. < 0.5	0.99	0.89	−0.07	−1.14	−0.72	−1.34
	(1.02)	(0.98)	(0.99)	(1.40)	(1.05)	(1.33)
	517/116	495/111	495/111	495/111	495/111	495/111
Dist. < 0.1	1.51	−0.99	−8.37	−8.84	−5.45	−5.89
	(1.46)	(1.76)	(2.85)***	(2.75)***	(2.84)*	(2.16)***
	225/69	217/67	217/67	217/67	217/67	217/67
WRLURI 500 m						
Dist. < 0.1	0.49	−1.34	−5.07	−6.73	−1.52	−3.90
	(1.29)	(1.25)	(2.46)**	(2.15)***	(2.35)	(2.00)*
	275/90	263/86	263/86	263/86	263/86	263/86
LLLA 1 km						
Dist. < 0.1	0.94	0.07	−2.47	−2.41	−1.48	−1.41
	(0.67)	(0.73)	(1.20)**	(1.35)*	(1.13)	(1.13)
	225/69	217/67	217/67	217/67	217/67	217/67
DRI 1 km						
Dist. < 0.1	−5.36	−2.54	−3.42	−2.59	−2.49	−3.31
	(2.32)**	(1.89)	(2.24)	(1.82)	(1.50)	(1.76)*
	226/70	218/68	218/68	218/68	218/68	218/68
OSI 1 km						
Dist. < 0.1	1.80	−0.81	−5.49	−4.91	−4.82	−3.06
	(1.10)	(1.89)	(3.76)	(4.17)	(3.42)	(3.10)
	225/69	217/67	217/67	217/67	217/67	217/67
Border pair FE	Y	Y	Y	Y	Y	Y
Quarter dummies	Y	Y	Y	Y	Y	Y
Per pupil expenditures		Y	Y	Y	Y	Y
Property taxes per acre		Y	Y	Y	Y	Y
Demographics			Y	Y	Y	Y
Parcel controls I				Y		Y
Parcel controls II					Y	Y

[a]Each cell describes the results of a different estimate of B_{OWN}. The top number in each cell is the coefficient estimate, the middle number is the standard error of this estimate, and the third line gives the count of parcels and borders on which the estimate is based. Standard errors are clustered by municipal border pair. *, **, and *** denote estimates different from 0 at 10%, 5%, and 1% significance levels.

data) in each of the same three disks centered on each parcel. Finally, in all columns but the first, we control for municipal property taxes per acre and school funding per pupil. A more detailed description of these control variables is available in Appendix B. In subsequent extensions of the results in Table II, we also use these sets of control variables.

As we move between rows in the top panel, we change the sample of parcels used to estimate all regressions in the row. In the top row, we use all parcels that match to each straight border and are more than 1 km from an intersection of municipal borders. In the second row, we restrict attention to parcels within 500 m of a municipal border and in the third we restrict attention to parcels within 100 m of a municipal border. Hence, the number of parcels and borders declines as we move from row 1 to row 3.

As an example, in row 3, column 5 of the top panel of Table II, our estimate of B_{OWN} is -5.45. The standard error of this estimate is 2.84 and the estimate is based on 217 parcel transactions within 100 m of one of 67 municipal borders. In addition to border pair dummies, the other controls included in this regression are quarterly dummies, our second set of parcel controls, demographic controls, school funding per pupil, and property taxes per acre.

The identification strategy developed in Section 2 is most reliable as we restrict attention to parcels closer to the border. Thus, the first two rows of Table II should be regarded as descriptive. When we consider parcels far from borders, we find a statistically insignificant own-lot effect of regulation and point estimates are sometimes positive. As we consider parcels closer to a municipal border, point estimates are consistently negative, and in specifications 3–6, different from zero at conventional levels of significance. Excluding the first column, which does not control for any possible confounding variation, the range of estimates for B_{OWN} is $[-8.84, -0.99]$. Table AII in the Supplementary Material duplicates the results of the third row of the first panel of Table II, but reports coefficient estimates for all control variables as well.

The second panel of Table II duplicates the third row of the first panel, but uses the slightly larger sample of parcels at least 500 m from the intersection of municipal borders. Excluding the first column, which does not control for many possible sources of confounding variation, the resulting coefficient estimates are negative in each column, and are different from zero at conventional levels of significance in 3, 4, and 6. Excluding the first column, the range of estimates for B_{OWN} is $[-6.73, -1.34]$.

We prefer the estimates of B_{OWN} that result from the larger sample used in the second panel of Table II. In these results, we consider parcels within 100 m of a border and at least 500 m from the second closest municipal border. It does not seem reasonable to worry about these estimates being polluted by regulation in some third municipality at least five times as far away as the closest border, so using the more restrictive and smaller sample of the first panel seems unwarranted.

Except for the second panel of Table II, all results of the estimates of B_{OWN} are based on the *less* preferred smaller sample. When we estimate our external effects regressions, there is a clear rationale for using the more restrictive sample, and for consistency, we adopt it for our exposition of the own-lot effects as well. With this said, the pattern suggested by the first two panels of Table II appears general. Using the sample of parcels at least 500 m from an intersection of municipal borders gives similar estimates of B_{OWN} to those obtained from parcels at least 1000 m from such an intersection.

To get a sense for the magnitude of own-lot effect, note that from Table I a 1 standard deviation in the WRLURI index is about 0.7 in the samples on which Table II is based. If B_{OWN} is about -7, then a 1 standard deviation increase in the WRLURI index causes about a 5 dollar decrease in the price per square foot of land. From Table I, for parcels near straight municipal boundaries, the average price per square foot is about 11 dollars, while for a typical Costar parcel in a WRLURI municipality, it is about 77 dollars per square foot. Thus, our estimates suggest that the own-lot effect is an important determinant of the price of land in our sample and is economically important even for more valuable parcels further from municipal boundaries.

The third, fourth, and fifth panels of Table II report estimates of B_{OWN} for the subindexes LLLA, DRI, and OSI. The other indices are not generally statistically significant, although most point estimates are negative. Overall, the estimates of the impact of a 1 standard deviation increase in each subindex are comparable or slightly weaker to that of the impact of a 1 standard deviation increase in the overall index, and it appears that all subindices contribute about equally to the overall effect. Our findings also seem consistent with the claim by Pollakowski and Wachter (1990) that the aggregate effect of regulation is greater than the sum of the effect of individual regulations.

We now consider various robustness tests. The sample we most often use describes transactions occurring from 2000 to 2009. We observe regulation only in 2005. It is possible that the effect of regulation on land prices varied during our sample period, which overlaps with the financial crisis. While we are unable to estimate effects for each year separately, given our sample sizes and large number of controls (leaving us limited degrees of freedom), we can look for broader evidence of time variation in our estimates. To investigate this possibility, Table III replicates estimations from Table II on samples drawn from different time periods, with and without quarterly dummies. The dependent variable in all regressions is parcel price per area, as in Table II.

The top row of Table III reports the results of own-lot effect regressions based on a sample of all transactions that occurred from 2000 to 2009, are within 100 m of a municipal border, and are not within 1 km of an intersection of municipal borders. The second row of Table III is identical to the first, but considers transactions that occurred only from 2003 to 2009. The first column of Table III describes regressions with the same control variables as in column 3 of Table II. The second column is identical to the first, but does not include quarterly dummies. The third column is based on column 4 of Table II and the fourth column drops quarterly dummies. Similarly, columns 5 and 6 are based on column 6 of Table II, with and without quarterly dummies. In Table III, the coefficient of the WRLURI index is consistently negative, often statistically

TABLE III

OWN-LOT EFFECT REGRESSIONS, ROBUSTNESS TO SAMPLE TIMING, AND QUARTERLY DUMMIES[a]

WRLURI, 1 km (Dist. < 0.1)	(1)	(2)	(3)	(4)	(5)	(6)
[2000–2009]	−8.37	−9.87	−8.84	−11.29	−5.89	−7.89
	(2.85)***	(1.86)***	(2.75)***	(1.29)***	(2.16)***	(1.36)***
[2003–2009]	−7.11	−4.41	−5.90	−4.62	−2.73	−3.70
	(4.31)	(2.49)*	(4.30)	(2.38)*	(3.67)	(2.43)
Border pair FE	Y	Y	Y	Y	Y	Y
Quarter dummies	Y		Y		Y	
Per pupil expenditures	Y	Y	Y	Y	Y	Y
Property taxes per acre	Y	Y	Y	Y	Y	Y
Demographics	Y	Y	Y	Y	Y	Y
Parcel controls I			Y	Y	Y	Y
Parcel controls II					Y	Y

[a]Each cell describes the results of a different estimate of B_{OWN}. The top number in each cell is the coefficient estimate and the second number is the standard error of this estimate. Standard errors are clustered by municipal border pair. *, **, and *** denote estimates different from 0 at 10%, 5%, and 1% significance levels.

different from 0. The range of point estimates reported is [−11.29, −2.73]. These results are broadly consistent with the results reported in the top two panels of Table II.

Unsurprisingly, standard errors are larger in the smaller 2003–2009 sample than in the 2000–2009 sample. Adding and removing quarterly dummies from the regressions does not have a consistent effect across specifications or samples. Overall, the estimates in Table III provide little evidence of substantial variation over time of the impact of regulation on land prices.

In Table IV, we look for evidence of misspecification by considering a more extensive set of specifications and control variables. As for the two previous regression tables, our dependent variable is parcel price in dollars per square foot. Each cell reports the coefficient estimate B_{OWN} for the WRLURI index along with standard errors. For all regressions, we consider all parcels sold from 2000 to 2009 that are within 100 m of a municipal border and are at least 1 km from the nearest intersection of municipal borders. The regressions in the three columns of Table IV are based on the specifications reported in columns 3, 4, and 6 of Table II. In the first row of Table IV, we modify the specification from Table II by replacing the school quality measure `per pupil expenditure`[26] with `pupil teacher ratio`. In the second row, we add `pupil teacher ratio` to each of the three specifications. In the third row, we control for income with the log of `municipal mean income` rather than its level. In the fourth row, we add a quadratic term in `municipal mean in-`

[26]See Appendix B for a description of these variables.

TABLE IV

OWN-LOT EFFECT REGRESSIONS, ROBUSTNESS TO CHOICE OF CONTROLS, AND FUNCTIONAL FORM[a]

WRLURI, 1 km (Dist. < 0.1)	(1)	(2)	(3)
Sub P/T ratio for PP expenditures	−2.92	−4.11	−1.05
	(3.28)	(3.11)	(2.50)
Add P/T ratio	−10.32	−13.22	−9.23
	(6.31)	(4.83)***	(5.25)*
Sub log(income) for income	−6.96	−7.76	−5.43
	(2.76)**	(2.56)***	(2.03)***
Add income^2	−8.73	−9.34	−5.75
	(3.13)***	(3.04)***	(2.43)**
Add demographic quadratic terms	−7.11	−33.83	−43.10
	(10.58)	(10.14)***	(9.51)***
Initial controls			
Border pair FE	Y	Y	Y
Quarter dummies	Y	Y	Y
Per pupil expenditures	Y	Y	Y
Property taxes per acre	Y	Y	Y
Demographics	Y	Y	Y
Parcel controls I		Y	Y
Parcel controls II			Y

[a]Each cell describes the results of a different estimate of B_{OWN}. The top number in each cell is the coefficient estimate and the second number is the standard error of this estimate. Standard errors are clustered by municipal border pair. *, **, and *** denote estimates different from 0 at 10%, 5%, and 1% significance levels.

come as a control. In the fifth row, we add quadratic terms for all of our demographic control variables.

With the exception of the fifth row, the results in Table IV are consistent with the results of Tables II and III. That is, point estimates of B_{OWN} are consistently negative and often different from zero at standard confidence levels. The range of point estimates in the first four rows of this table is [−13.22, −1.05], about the same as in Tables II and III. While we cannot eliminate the possibility that the negative own-lot effect of the WRLURI index is due to specification error, the first four rows of Table IV do not support this conclusion. In the fifth row of Table IV, our coefficient estimates vary wildly. While this may indicate misspecification, it seems more likely that we simply do not have enough observations to pin down all of the many extra coefficients.

In all of the own-lot effect regressions presented so far, we assume that the own-lot effect is linear in the cross-border regulation gap. Table V relaxes this restriction. In Table V, we use transactions from 2000 to 2009, within 100 m of a municipal border and at least 1 km from an intersection of borders. The dependent variable is the price per unit square foot of land. The three columns are based on specification 3, 4, and 6 from Table II. We adjust these regressions

TABLE V

OWN-LOT EFFECT REGRESSIONS, ROBUSTNESS TO SPECIFICATION, AND FUNCTIONAL FORM OF REGULATION MEASURES[a]

WRLURI, 1 km (Dist. < 0.1)	Coef.	(1)	(2)	(3)
Add WRLURI^2	WRLURI	−13.25 (3.85)***	−5.92 (4.45)	−7.17 (6.30)
	WRLURI^2	2.46 (2.48)	−1.47 (2.97)	0.68 (3.25)
Border pair FE		Y	Y	Y
Quarter dummies		Y	Y	Y
Per pupil expenditures		Y	Y	Y
Property taxes per acre		Y	Y	Y
Demographics		Y	Y	Y
Parcel controls I			Y	Y
Parcel controls II				Y

[a]Each cell describes the results of a different estimate of B_{OWN}. The top number in each cell is the coefficient estimate and the second number is the standard error of this estimate. Standard errors are clustered by municipal border pair. *, **, and *** denote estimates different from 0 at 10%, 5%, and 1% significance levels.

from what is reported in Table II by including a quadratic term in the WRLURI index, and report coefficient and standard errors for both WRLURI terms. In two of the three specifications, adding a quadratic term in the WRLURI index causes both linear and quadratic terms to be indistinguishable from zero. In no case does the point estimate of the linear term move outside the range of estimates reported in Table II, and the quadratic term is always zero. Thus, we fail to reject the hypothesis that the effect of regulation on prices is linear. If there is a nonlinear effect of cross-border differences in regulation on land price, this effect is too small to be measured by our data. We also experimented widely with regressions that include more than one measure of regulation. None of these regressions was robust to plausible changes of specification.

To sum up, we find evidence that the own-lot effect is negative for the WRLURI index. Point estimates are consistently negative with a mode around 6. These estimates are often statistically different from zero. In spite of the large initial sample of transactions, as required by our identification strategy, ultimately our results are based on a sample of several hundred parcels very close to straight municipal borders. Thus, there is reason to be concerned that our own-lot effect estimates may not necessarily be applicable to interior parcels. An analysis of the components of the overall regulation index suggests that they all contribute about equally to the overall effect.

4.2. *External Effect Regressions*

In Table VI, we report estimates of external effects, B_{EXT}, using parcels that match to straight municipal borders for which we have WRLURI data. The

TABLE VI

EXTERNAL-EFFECT REGRESSIONS[a]

	(1) [2000–2009]	(2) [2000–2009]	(3) [2000–2009]	(4) [2000–2009]	(5) [2000–2009]	(6) [2000–2009]	(7) [2000–2009]	(8) [2000–2009]
WRLURI 1 km								
$1 > x > 0.5, 0.1 > x > 0$	1.00	3.36	−5.26	5.14	−1.24	−2.24	−0.98	−1.83
	(10.68)	(12.83)	(15.64)	(15.68)	(17.96)	(18.82)	(16.12)	(15.15)
	545/174	448/137	545/174	545/174	448/137	448/137	448/137	448/137
$0.5 > x > 0.25, 0.25 > x > 0$	−1.91	−0.69	−2.28	−3.00	−0.98	−1.24	−1.27	−1.71
	(1.22)	(1.56)	(1.09)**	(1.32)**	(1.36)	(1.28)	(1.39)	(1.48)
	505/150	440/126	505/150	505/150	440/126	440/126	440/126	440/126
$0.5 > x > 0.25, 0.1 > x > 0$	−1.55	0.05	−2.51	−3.34	−0.11	−0.78	−0.63	−1.34
	(1.60)	(1.69)	(1.18)**	(1.55)**	(1.63)	(1.39)	(1.68)	(1.63)
	381/128	331/109	381/128	381/128	331/109	331/109	331/109	331/109
WRLURI 500 m								
$0.5 > x > 0.25, 0.25 > x > 0$	−2.91	−3.17	−0.61	0.15	−2.94	−3.23	−2.80	−3.09
	(1.29)**	(1.80)*	(2.18)	(2.60)	(1.75)*	(1.76)*	(1.77)	(1.81)*
	629/187	543/151	629/187	629/187	543/151	543/151	543/151	543/151
$0.5 > x > 0.25, 0.1 > x > 0$	−3.37	−1.40	0.25	1.35	−1.23	−1.86	−0.87	−1.41
	(1.67)**	(1.71)	(2.74)	(3.42)	(2.11)	(1.98)	(1.80)	(1.88)
	472/160	405/130	472/160	472/160	405/130	405/130	405/130	405/130
LLLA 1 km								
$0.5 > x > 0.25, 0.25 > x > 0$	−0.62	0.05	−0.43	−0.65	−0.43	−0.39	−0.37	−0.47
	(0.77)	(1.20)	(0.81)	(0.84)	(0.75)	(0.74)	(0.76)	(0.76)
	509/153	444/129	509/153	509/153	444/129	444/129	444/129	444/129
$0.5 > x > 0.25, 0.1 > x > 0$	−0.39	0.98	−0.27	−0.67	0.30	0.31	0.33	0.13
	(0.98)	(1.28)	(0.91)	(0.92)	(0.75)	(0.75)	(0.77)	(0.72)
	383/130	333/111	383/130	383/130	333/111	333/111	333/111	333/111

(*Continues*)

TABLE VI—*Continued*

	(1) [2000–2009]	(2) [2000–2009]	(3) [2000–2009]	(4) [2000–2009]	(5) [2000–2009]	(6) [2000–2009]	(7) [2000–2009]	(8) [2000–2009]
DRI 1 km								
$0.5 > x > 0.25, 0.25 > x > 0$	3.56	2.25	2.61	3.47	1.43	0.32	1.12	0.91
	(1.87)*	(1.86)	(2.71)	(2.76)	(2.34)	(2.11)	(2.21)	(2.20)
	527/165	460/139	527/165	527/165	460/139	460/139	460/139	460/139
$0.5 > x > 0.25, 0.1 > x > 0$	4.92	3.82	4.62	5.96	2.88	−0.44	1.77	0.02
	(2.37)**	(3.10)	(4.07)	(4.11)	(2.94)	(2.40)	(2.80)	(2.74)
	394/139	342/118	394/139	394/139	342/118	342/118	342/118	342/118
OSI 1 km								
$0.5 > x > 0.25, 0.25 > x > 0$	−3.33	−3.59	−4.80	−4.98	−3.54	−3.59	−4.30	−4.68
	(1.33)**	(1.13)***	(1.96)**	(2.13)**	(2.08)*	(1.80)**	(2.20)*	(2.27)**
	511/153	446/129	511/153	511/153	446/129	446/129	446/129	446/129
$0.5 > x > 0.25, 0.1 > x > 0$	−3.13	−2.87	−6.20	−6.09	−0.13	−1.02	−2.04	−2.69
	(1.55)**	(1.64)*	(2.89)**	(3.20)*	(2.85)	(2.51)	(3.01)	(3.15)
	384/131	334/112	384/131	384/131	334/112	334/112	334/112	334/112
Municipality-border FE	Y	Y	Y	Y	Y	Y	Y	Y
Quarter dummies	Y	Y	Y	Y	Y	Y	Y	Y
ΔPer pupil expenditures		Y			Y	Y	Y	Y
ΔProperty taxes per acre		Y			Y	Y	Y	Y
ΔDemographics			Y	Y	Y	Y	Y	Y
Parcel controls I			Y	Y			Y	Y
Parcel controls II				Y	Y		Y	Y
Interior dummy				Y		Y		Y

[a]Each cell describes the results of a different estimate of B_{EXT}. The top number in each cell is the coefficient estimate, the middle number is the standard error of this estimate, and the third line gives the count of parcels and borders on which the estimate is based. Standard errors are clustered by municipal border pair. *, **, and *** denote estimates different from 0 at 10%, 5%, and 1% significance levels.

table presents five panels of results. In all but the second, we restrict attention to parcels at least 1 km from the intersection of municipal borders. The second panel considers the larger sample of parcels at least 500 m from such an intersection. All columns consider transactions that occur between 2000 and 2009. The dependent variable in every case is the price per square foot of the parcel. As for Table II, the top two panels of Table VI investigate the effects of the WRLURI index, while the lower three consider the LLLA, DRI, and OSI indices. We are interested in the change in land rent between the municipal boundary and the interior. The leftmost column describes the location of "interior" and "boundary" bins for regressions in that row. We experiment with different definitions of boundary and interior in the different rows in each panel.

Each cell in this table reports an estimate of a variant of our external-effect estimating equation. Column 1 estimates equation (27), while the other columns estimate variants of equations (28) and (30). For a particular specification each cell of the table reports, B_{EXT}, the coefficient of the regulatory index interacted with an interior dummy defined over the appropriate interior bin, its standard error in parentheses, and the number of parcels and borders on which the estimate is based. Standard errors are clustered by municipality. The bottom panel indicates the control variables used for all regressions in each column. The quarterly dummies and the two sets of parcel controls are the same as for our own-lot regressions, for example, Table II. The demographic controls and the controls for school expenditures and local property taxes are also the same as we used in our own-lot regressions except that, consistent with the analysis of Section 2, we include these variables as cross-border changes interacted with the interior dummy. In columns 4, 6, and 8 of Table VI, we also control for whether or not a parcel is interior or boundary. This controls for the possibility that land prices or regulatory enforcement are systematically different for boundary or interior parcels. Table AIII in the Supplementary Material reports all regression coefficient estimates for the regressions reported in row 2 of Table VI.

As an example, in row 4, column 6 of the second panel of Table VI, an interior parcel is one that lies between 250 m and 500 m from the border, while a boundary parcel lies within 250 m of the boundary: our estimate of B_{EXT} is -3.23; the standard error of this estimate is 1.76 and the estimate is based on 543 parcel transactions each associated with one of 151 straight borders. Both the WRLURI index and the dependent variable are in levels, so this estimate means that a 1 unit increase in the cross-border gap in the WRLURI index causes a 3.23 dollar per square foot decrease in the land price of an interior parcel relative to a boundary parcel.

The top panel shows that there is no measurable external effect of the WRLURI index on land prices if we compare parcels closer than 100 m to parcels that lie between 500 m and 1000 m. On the other hand, if we compare parcels closer than 250 m or closer than 100 m to parcels between 250 m and 500 m, we see a negative external effect of the regulation on land rents. These estimates are statistically significant in some cases, though not when we control for local tax rates and school quality in columns 6 and 7. In the second panel of Table VI, we replicate the bottom two rows of the top panel on the larger sample of transactions that are at least 500 m from an intersection of municipal boundaries. The second panel of Table VI is based on a larger sample than the first panel; however, the extra observations are all between 500 and 1000 m of a third municipal boundary, which introduces error into our measurement of regulation. Given this, we prefer the similar but less precise estimates in the first panel.

Point estimates of B_{EXT} in the top two panels of Table VI are almost all negative, although we cannot usually distinguish coefficients from 0 at standard levels of confidence. With this said, our estimates are reasonably precise. For example, the largest point estimate in rows 2 or 3 of Table VI is 0.05 and occurs in row 3, column 2. The standard error of this estimate is also the largest of any in these two rows at 1.69. Even for this estimate, we reject the possibility that $B_{\mathrm{EXT}} > 2.82$ at the 5% level. While we cannot be confident of the sign of the external effect, our data suggest that a 1 unit increase in the WRLURI index provides at most 2 or 3 dollars per square foot in external benefits.

To conclude that land use regulation is harmful, we require that the external effect not be large enough to offset the harm caused by the own-lot effect. On the basis of our point estimates, reaching this conclusion is trivial. Point estimates of the external effects are consistently negative. This suggests an equilibrium, like the one described by the bottom panel of Figure 1, where regulation constrains land owners from developing their land the way they want and leads to landscapes that residents like less well. On the other hand, our estimates of B_{EXT} are too imprecise to allow confidence in the sign of our estimates. Nevertheless, our data do suggest that external effects are small and, as we calculate in the conclusion, are probably too small to compensate for the cost of own-lot effects.

The next three panels of Table VI revert to the sample of parcels at least 1 km from an intersection of municipal boundaries and repeat the regressions of the second panel using different regulation variables. The results are less robust than for the WRLURI index, but certainly do not support the idea that either the red tape index, LLLA, or open space requirements, OSI, have beneficial external effects. The point estimates for the effect of the density restriction index, however, are usually positive and in two cases are statistically different from 0. On the basis of Table VI, if one were forced to pick a land use regulation with positive external effects, it would be the DRI index, minimum lot size restrictions. Obviously, the evidence for this positive effect is weak.

Table VII explores whether there is evidence of heterogeneity in the impact of regulation over time. More specifically, the top row of Table VII reports estimates of B_{EXT} based on a sample of all transactions that occurred from 2000 to 2009, match to a straight border, and are at least 1 km from the intersection

TABLE VII

EXTERNAL-EFFECT REGRESSIONS, ROBUSTNESS TO SAMPLE TIMING, AND QUARTERLY DUMMIES[a]

WRLURI, 1 km (0.5 > x > 0.25, 0.25 > x > 0)	(1)	(2)	(3)	(4)	(5)	(6)
[2000–2009]	−0.69	−1.05	−2.28	−1.96	−1.24	−1.15
	(1.56)	(1.95)	(1.09)**	(1.20)	(1.28)	(1.48)
[2003–2009]	−0.42	−1.21	−5.06	−4.52	−2.92	−3.39
	(1.77)	(2.13)	(1.52)***	(147)***	(1.61)*	(2.03)*
Municipality-border FE	Y	Y	Y	Y	Y	Y
Quarter dummies	Y		Y		Y	
ΔPer pupil expenditures	Y	Y			Y	Y
ΔProperty taxes per acre	Y	Y			Y	Y
ΔDemographics			Y	Y	Y	Y
Parcel controls I			Y	Y		
Parcel controls II						
Interior dummy					Y	Y

[a]Each cell describes the results of a different estimate of B_{EXT}. The top number in each cell is the coefficient estimate and the second number is the standard error of this estimate. Standard errors are clustered by municipal border pair. *, **, and *** denote estimates different from 0 at 10%, 5%, and 1% significance levels.

of municipal borders. Definitions of interior and boundary parcels are as in row 2 of Table VI. The second row of Table VII is identical to the first, but considers transactions that occurred only from 2003 to 2009. The first, third, and fifth columns report regressions with the same control variables as in columns 2, 3, and 6 of Table VI. The even numbered columns omit quarterly dummies. Table VII is broadly consistent with rows 2 and 3 of Table VI. The coefficient of the WRLURI index is consistently negative and often statistically different from 0. Standard errors are relatively smaller in the second row where we consider samples drawn from 2003 to 2009. These results indicate that our estimates are relatively stable over time and not driven by some peculiarity of our study period.

In Table VIII, we investigate the role of functional form and consider more extensive sets of control variables. Each cell reports B_{EXT} for the WRLURI index, along with standard errors. For all regressions, boundary parcels are within 250 m of a straight boundary, while interior parcels are between 250 m and 500 m from a straight boundary. All parcels less than 1 km from an intersection of municipal borders are excluded. The regressions in the three columns of Table VIII are based on the specifications reported in columns 2, 3, and 6 of Table VI. In the first row of Table VIII, we modify the specification from Table VI by replacing `per pupil expenditure`[27] with `pupil teacher ratio`. In the second row, we add `pupil teacher ratio` to

[27]See Appendix B for a description of variables.

TABLE VIII

EXTERNAL-EFFECT REGRESSIONS, ROBUSTNESS TO CHOICE OF CONTROLS, AND FUNCTIONAL FORM[a]

WRLURI, 1 km ($0.5 > x > 0.25$, $0.25 > x > 0$)	(1)	(2)	(3)
Sub P/T ratio for PP expenditures	0.09		−0.90
	(1.84)		(1.37)
Add P/T ratio	0.11		−0.92
	(1.84)		(1.40)
Sub log(income) for income		−2.73	−1.57
		(1.10)**	(1.30)
Add income^2		−2.35	−1.11
		(1.10)**	(1.28)
Add demographic quadratic terms		−2.54	−1.22
		(1.02)**	(1.08)
Initial controls			
Municipality-border FE	Y	Y	Y
Quarter dummies	Y	Y	Y
ΔPer pupil expenditures	Y		Y
ΔProperty taxes per acre	Y		Y
ΔDemographics		Y	Y
Parcel controls I		Y	
Parcel controls II			
Interior dummy			Y

[a]Each cell describes the results of a different estimate of B_{EXT}. The top number in each cell is the coefficient estimate and the second number is the standard error of this estimate. Standard errors are clustered by municipal border pair. *, **, and *** denote estimates different from 0 at 10%, 5%, and 1% significance levels.

each of the three specifications. In the third, we control for income with the log of `municipal mean income` rather than its level. In the fourth row, we add a quadratic term in `municipal mean income`. In the fifth row, we add quadratic terms for all of our demographic control variables. The robustness of our results in Table VIII provides little evidence of biases in our estimates due to specification error.

In all of the external-effect regressions presented so far, we assume that the external effect is linear in the cross-border regulation gap. Table IX relaxes this restriction. For all of the results reported in Table IX, we use transactions from 2000 to 2009 that match to a straight border and are at least 1 km from an intersection of borders. In every case, a boundary parcel is less than 250 m from the nearest border, and an interior parcel is between 250 and 500 m from the nearest border. The three columns of Table IX are based on columns 2, 3, and 6 from Table VI. We adjust these regressions by including a quadratic term in the WRLURI index, and report coefficient and standard errors for both WRLURI terms. Adding a quadratic term in the WRLURI index leaves our estimates of the linear term qualitatively unchanged and is itself indistin-

TABLE IX

EXTERNAL-EFFECT REGRESSIONS, ROBUSTNESS TO SPECIFICATION, AND FUNCTIONAL FORM OF REGULATION MEASURES[a]

WRLURI, 1 km ($0.5 > x > 0.25$, $0.25 > x > 0$)	Coef.	(1)	(2)	(3)
Add WRLURI^2	ΔWRLURI	−0.80	−2.55	−1.25
		(1.51)	(1.04)**	(1.23)
	(ΔWRLURI)^2	−0.57	−0.64	−0.04
		(1.00)	(0.96)	(1.28)
Municipality-border FE		Y	Y	Y
Quarter dummies		Y	Y	Y
ΔPer pupil expenditures		Y		Y
ΔProperty taxes per acre		Y		Y
ΔDemographics			Y	Y
Parcel controls I			Y	
Parcel controls II				
Interior dummy				Y

[a]Each cell describes the results of a different estimate of B_{EXT}. The top number in each cell is the coefficient estimate and the second number is the standard error of this estimate. Standard errors are clustered by municipal border pair. *, **, and *** denote estimates different from 0 at 10%, 5%, and 1% significance levels.

guishable from 0. There is, therefore, little evidence for a nonlinear effect of regulation. We also experimented extensively with regressions that control for more than one measure of regulation. These regressions did not yield robust results and we do not report them.

4.3. *Supply Effect Regressions*

In Table X, we report estimates of supply effects, B_{SUPPLY}. The dependent variable in each regression is the share of developed land, as measured by the NLCD, in a strip along one side of a municipal border. As discussed in Section 3, we restrict attention to municipal borders where we record WRLURI data for both adjoining municipalities and where at least half of all Costar parcels in the municipality match to a straight border. As for our own-lot effect regressions, we base our estimates on regions close to the border to assure that we are comparing places with similar unobserved characteristics.

In each cell of Table X, we report the coefficient of the WRLURI index from a variant of equation (31), along with its standard error. The third row in each cell reports the number of municipal border pairs on which the estimate is based. The first row presents the results of regressions where the dependent variable is the share of land in a municipality in a strip extending 100 m from the municipal border. In the second row, the dependent variable measures the same quantity in a 250 m strip. The third and fourth rows are similar, but consider only the area of a strip that is not water. As we move across columns of Table X, we vary control variables. The control variables are similar to those

TABLE X

SUPPLY EFFECT REGRESSIONS[a]

WRLURI, 1 km (Share Straight ≥ 0.5)	(1)	(2)	(3)	(4)	(5)	(6)
Share urban (100 m)	−0.02	−0.03	−0.03	−0.03	−0.03	−0.03
	(0.01)*	(0.01)**	(0.01)***	(0.01)**	(0.01)**	(0.01)**
	90	76	76	64	76	64
Share urban (250 m)	−0.03	−0.03	−0.04	−0.04	−0.05	−0.04
	(0.01)**	(0.01)**	(0.01)***	(0.01)**	(0.02)***	(0.01)**
	90	76	76	64	76	64
Share urban/share dry land (100 m)	−0.03	−0.04	−0.05	−0.04	−0.04	−0.04
	(0.01)***	(0.02)***	(0.02)***	(0.01)***	(0.01)***	(0.01)***
	90	76	76	64	76	64
Share urban/share dry land (250 m)	−0.04	−0.06	−0.07	−0.04	−0.05	−0.04
	(0.02)**	(0.02)**	(0.02)***	(0.01)***	(0.02)***	(0.01)***
	90	76	76	64	76	64
Border pair FE	Y	Y	Y	Y	Y	Y
Per pupil expenditures		Y	Y	Y	Y	Y
Property taxes per acre		Y	Y	Y	Y	Y
Demographics			Y	Y	Y	Y
Municipal controls I				Y		Y
Municipal controls II					Y	Y

[a]Each cell describes the results of a different estimate of B_{SUPPLY}. The top number in each cell is the coefficient estimate, the middle number is the standard error of this estimate, and the bottom number is the count of borders on which the estimate is based. Standard errors are clustered by municipal border pair. *, **, and *** denote estimates different from 0 at 10%, 5%, and 1% significance levels.

we have used in our own-lot effect regressions, except that, where appropriate, we construct municipal level variables by averaging over parcels in the relevant part of the municipality.

As an example, in row 2, column 1, our estimate of B_{SUPPLY} is −0.03. The standard error of this estimate is 0.01 and the estimate is based on 90 municipal borders. This point estimate is about −3%, so a 1 unit increase in the WRLURI index decreases the share developed by 3%. This coefficient is precisely estimated. This same description is broadly true of the estimates of B_{SUPPLY} presented in Table X. They range between −2% and −5%, and are precisely estimated. These results strongly suggest that regulation has a modest negative effect on the supply of land in a municipality.

5. CONCLUSION

In Section 2, we develop a simple framework to address two problems that arise in evaluating land use regulation. First, our framework illuminates the inference problems that confront an effort to estimate the effects of land use regulation on land prices. Second, it provides a basis for using observable transaction data to calculate municipal level changes in welfare. For both problems, our framework suggests that the effects of a marginal change in land use regulation on municipal level land prices and welfare can be decomposed into three parts: an own-lot effect, an external effect, and a supply effect.

In Section 4, we use the WRLURI data on land regulation and Costar data to estimate own-lot and external effects. Our estimates of both own-lot and external effects are consistently negative and are robust to changes in specification, controls, and sample. Our estimates of the own-lot effect are generally different from 0 at standard levels of significance. Our estimates of the external effect, while reasonably precise, are not precise enough that we can be confident in the sign of the external effect. We can, however, be confident that this effect is at most a small positive number.

We also experiment with WRLURI regulatory subindexes. While these estimations are less robust than those that involve the overall measure of regulation, we see little evidence of statistically significant differences in the extent to which regulatory subindices affect land prices, suggesting that they contribute about equally to its overall impact.

Finally, in Section 4, we use the WRLURI and NLCD to estimate the effect of a change in regulation on the share of land developed in a municipality. These estimates strongly suggest that a 1 unit increase in the WRLURI index causes about a 3% decrease in the share of land available for development. Alternatively, in the sample used for our supply regressions, the standard deviation of the WRLURI index is about 0.7. Thus, a 1 standard deviation increase in this index causes about a 2% decrease in the share of land available for development. These results are consistent with those of Mayer and Sommerville (2000) and Quigley and Rafael (2005), who find that regulation tends to decrease the supply of housing.

Recalling equation (17), we can approximate the change in welfare that results from increasing z_0^L to z_1^L while holding regulation in the right municipality constant by

$$\begin{aligned}&\bigl(\bigl[p\bigl(-\overline{\overline{x}}\bigl(z_1^L\bigr)\bigr)-p\bigl(\overline{\overline{x}}\bigl(z^R\bigr)\bigr)\bigr]\times \text{residential land area}\bigr)\\&\quad-p\bigl(-\overline{\overline{x}}\bigl(z_0^L\bigr)\bigr)\times\Delta.\end{aligned}$$

To evaluate the leftmost term, recall that $p(\overline{x})$ and $p(-\overline{x})$ are land prices for interior parcels far enough from the border that they are wholly exposed to their own municipality's regulation. We can calculate the difference between interior prices using equation (5). Inspection of this equation and of Figure 1 shows that the first parenthetical quantity on the right hand side is the own-lot effect, while the second is the external effect. Thus, we calculate $p(-\overline{x})-p(\overline{x})$ as the own-lot effect plus twice the external effect. That is,

$$\begin{aligned}(32)\qquad p(-\overline{x})-p(\overline{x})&=\bigl(p\bigl(0^-\bigr)-p\bigl(0^+\bigr)\bigr)+2\bigl(p(-\overline{x})-p\bigl(0^-\bigr)\bigr)\\&=B_{\text{OWN}}+2B_{\text{EXT}}.\end{aligned}$$

Although our estimates of the external effect are not precise enough to allow us to sign this effect at conventional levels of significance, given our other estimates, they appear to be more than precise enough to allow us to sign the total of external and own-lot effects, $B_{\text{OWN}} + 2B_{\text{EXT}}$. In particular, from Table II, row 3, column 6, we have the own-lot effect is -5.89 with standard error 2.16 and from Table VI, row 3, column 8, we have the external effect is -1.34 with standard error 1.63. If we assume that these coefficients do not covary, then we calculate the mean and standard error of $B_{\text{OWN}} + 2B_{\text{EXT}}$ as $(-8.6, 3.9)$. That is, the mean of total effect of own-lot and external effects is about 8.6 dollars per square foot per unit of change in the WRLURI index, and this value is more than twice as large as its standard error. Even if we base our estimate of the total effect of regulation on the largest and most variable estimate of the external effect (from row 3, column 2 of Table VI), the corresponding mean and standard error of the total effect is $(-6.0, 4.0)$, so the estimated cost of regulation is still 1.5 times as large as its standard error.

Using this last estimate, each unit increase in regulation causes about a 6 dollar per square foot decrease in the price of an average interior parcel. From Table I, for the parcels on which our regressions are based, a 1 standard deviation change in the WRLURI index is about 0.7 and the price of an average parcel is about 12 dollars per square foot. Thus, a 1 standard deviation increase in the WRLURI index causes about a 4 dollar per square foot, or 36%, decrease in the price of land in our sample. Together with a 2% decrease in the share of land available for development because of such regulation, this means that a 1 standard deviation increase in regulatory intensity causes about a 38% decrease in the value of our border parcels.

This conclusion requires three caveats. First, to implement our identification strategy, we consider parcels and development near municipal borders. Burchfield et al. (2006) find that most development occurs at the edges of existing developed areas. Thus, in addition to resolving inference problems, our method focuses attention on regions where regulation is most likely to have an effect. While the advantages of this approach are clear, we may overstate the effect of regulation on parcels located far from municipal borders. Second, by construction, our econometric technique only measures external effects of municipal regulation that operate over spatial scales of a few hundred meters. That is, they tell us if the regulation measured by the WRLURI index leads to more pleasant neighborhoods, not if they lead to more productive or nicer regions. The possibility remains that municipal land use regulation has beneficial effects on a geographic scale larger than the one we consider. Third, while we consider the possibility that regulation has partial equilibrium effects on the supply of land, our data do not permit us to consider general equilibrium supply effects. That is, the possibility that land supply in one municipality affects prices in another. Our prior is that such effects will generally be small.

While these caveats point to areas of further research, we note the progress that this paper makes on the difficult problem of evaluating land use regulation. We develop a novel strategy for estimating the causal effects of regulation on land prices and implement this strategy with data as good as any currently available to researchers. In addition, we develop a logically consistent framework in which to interpret the various effects of regulation, that is, own-lot, external, and supply effects. Until now, the lack of such a framework allowed contradictory interpretations of empirical findings. This framework also allows a precise welfare interpretation of our estimates. We find evidence that marginal reductions in land use regulation are likely to have substantial welfare benefits to areas on the less developed edges of towns and probably somewhat smaller benefits for areas near the centers of towns.

APPENDIX A: LAND USE REGULATION AND RENT WITH HETEROGENOUS TASTES FOR REGULATION AND HOMOGENOUS OUTSIDE OPTIONS

Suppose that there are two types of agents, A and B, and the two types have different tastes for regulation. Let v^A_{EXT} and v^B_{EXT} denote the value that type A and type B agents assign to the external benefits of the more regulated left municipality, and assume that $v^A_{\text{EXT}}(z) > v^B_{\text{EXT}}(z)$ for all $z > 0$. We suppose that types A and B are otherwise alike. From the definition of V_{EXT}, we have that $V^A_{\text{EXT}}(x, z^L, z^R) > V^B_{\text{EXT}}(x, z^L, z^R)$ for all $x < \overline{x}$ and are otherwise equal. Analogous to equation (2), with free mobility, the bid rent functions for type A and B agents are

$$p^A(x) = w - \theta + \ln\bigl(V_{\text{OWN}}\bigl(x, z^L, z^R\bigr)\bigr) + \ln\bigl(V^A_{\text{EXT}}\bigl(x, z^L, z^R\bigr)\bigr),$$
$$p^B(x) = w - \theta + \ln\bigl(V_{\text{OWN}}\bigl(x, z^L, z^R\bigr)\bigr) + \ln\bigl(V^B_{\text{EXT}}\bigl(x, z^L, z^R\bigr)\bigr).$$

Since $V^A_{\text{EXT}} > V^B_{\text{EXT}}$, it follows immediately that the type A agents outbid type B at every location where the external effect of the more stringent regulation of the left municipality is felt; that is, all locations to the left of $\overline{x}$. For locations where the external effect of the stringent left regulation is not felt, we have $p^A(x) = p^B(x)$, and types A and B are assigned to these locations at random. Our econometric strategy will rely on this result: for type specific preferences for regulation, the region $[-\overline{x}, \overline{x}]$ is entirely populated by agents of a single type.

This leads us to the following unsurprising conclusion. If sorting on the basis of preference for regulation occurs in the neighborhood of a municipal border, then cross-border variation in land rents reflects the value placed on regulation by the type who live near the border. Our econometric estimates of the value of land use regulation should be understood in this light.

APPENDIX B: DATA DESCRIPTION

In our results, we commonly refer to sets of control variables under the names Parcel controls I, Demographics, and Parcel controls II. We also refer to two particular control variables: per pupil expenditures and property taxes per acre. In this appendix, we enumerate the groups of controls and describe the construction of all variables.

Parcel controls I includes a quadratic function of parcel area (in square feet), the log of distance to the central business district (CBD), and the log of the square of this distance. Both of these variables are taken from the Costar data. The distance to CBD is calculated by measuring the distance to the tallest building in the metropolitan statistical area (MSA) containing the parcel.

In addition to these variables from Costar, the first set of parcel controls contains three terrain measures. We begin with a digital elevation map with a resolution of 3 arc sec (90 m). Let v be the elevation of the subject pixel and let s_1–s_8 be the elevations for the eight adjacent pixels. For each v in a given jurisdiction, we calculate $g(v) = (\sum_{i=1}^{8}(v - s_i)^2)^{1/2}$. This index, which is similar to the mean standard deviation in elevation between each pixel and its neighbors is used in Burchfield et al. (2006). This index provides an intuitive measure of roughness and is particularly simple to calculate with GIS software.

Our terrain measures are the mean of $g(v)$ over all pixels in the area surrounding each Costar parcel. The three terrain roughness measures we use are TRI 500 m, TRI 5 km, and TRI 10 km. These represent mean roughness, $g(v)$, calculated for pixels with 500 m of each Costar parcel, within 5 km of each Costar parcel, and within 10 km of each Costar parcel. Given the expected relationship between the ruggedness of the terrain and the cost of buildings and transportation, we expect these variables to have an important impact on land prices.

Municipal controls I used in the supply effect regressions are averages over all parcels in the relevant parts of each municipality of the variables in Parcel controls I.

Demographics contains municipal level demographics variables taken from the 2000 census. These variables are the shares of municipal population, that is, black, Asian, under 17, has at least a high school degree, and has at least a college degree. The set of demographic controls also includes median municipal household income. In our own-lot regression, these variables are included as levels. Consistent with the econometric framework developed in Section 2.5, in the external effect regressions, these variables are included as cross-border changes interacted with an interior dummy.

Parcel controls II. The construction of these controls follows the construction of similar variables in Eid, Overman, Puga, and Turner (2008). For each Costar parcel in our data, we begin by creating disks around these parcels of radius 500 m, 5 km, and 10 km (as we did to construct terrain roughness measures). With this done, we use 1994 zip code business pattern data to impute employment to each such disk under the assumption that employment is uniformly distributed across each zip code. This leads us to calculate Emp 500 m, Emp 5 km, and Emp 10 km, the employment density, in jobs per square kilometer, in each of three concentric rings around each parcel. These variables measure proximity to employment and thus ought to help to predict land prices. Finally, we use the 1992 version of the NLCD data to calculate developed share in the same set of concentric rings around each parcel. This leads us to calculate Dev. 92 500 m, Dev. 92 5 km, and Dev. 92 10 km. To the extent that the availability of land suitable for building affects land prices, we should expect these variables to help to determine land prices.

Municipal controls II used in the supply effect regressions are averages over all parcels in the relevant parts of each municipality of the variables in Parcel controls II.

We note that the calculation of the three Dev. 90 variables is similar to our calculation of the share of nonwater area developed for our supply effect regressions. There is one important difference, however: the 1992 data do not include the very low density development class that appears in the 2006 data.

For both the employment and the development variables that make up our second set of parcel level controls, we rely on old data to reduce concerns about endogeneity.

Other municipal policy variables. In addition to our measures of land use regulation, our estimations rely on three other measures of local public policy: property tax per square foot of municipality area, school district funding per student, and pupils per teacher. Local property tax revenue data are from the 2002 Census of Governments. We rely on Common Core data provided by the National Center for Education Statistics to develop our measures of 2005–2006 per pupil expenditures and pupil/teacher ratios for unified and secondary school districts.

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