

Zoning effects on housing change vary with income, based on a four-decade panel model after propensity score matching



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ABSTRACT

Zoning is one of the oldest policy tools for regulating private land use, but its effect on housing change is disputed. We define housing change as the difference over time in the number of housing units in a given jurisdiction. We conducted a cross-jurisdictional study of zoning effects on housing change, comparing zoned and unzoned townships in Michigan, USA ($n = 709$) after propensity score matching. Four-decade (1970–2010) panel models predicted zoned townships to have on average 2.2% fewer housing units a decade after zoning adoption compared to similar unzoned townships, but this effect was tempered by other variables. Higher-income townships had more housing units a decade after zoning adoption compared to higher-income unzoned townships, whereas lower-income townships with zoning had fewer housing units than similar unzoned townships. This study highlights the heterogeneous effects of zoning on rural and exurban housing change.

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1. Introduction

Zoning is an important policy tool for managing land use, but its effects on housing change are unclear and inconsistent (Wallace, 1988; Esparza and Carruthers, 2000; Croissant and Munroe, 2002). We define “housing change” as the positive or negative difference over time in the number of housing units in a given jurisdiction, and refer to this measurement as “housing growth” when the difference is positive. Ninety-seven percent of the developable land in the United States is considered rural (<6 housing units/km²) or exurban (6–147 housing units/km²), and development in these areas is a major driver of land-use change (Theobald, 2005). However, many studies on the impact of zoning on housing change do not consider rural and exurban areas and are constrained to single jurisdictions (Pogodzinski and Sass, 1990). We develop a unique dataset that tracks zoning implementation across the U.S. state of Michigan, allowing us to compare housing change across zoned and unzoned jurisdictions in a wide variety of settings and to provide a more robust estimate of the impacts of zoning on housing change.

Building and road development outside of urban centers is expanding rapidly in many countries (Nilsson et al., 2013), and has been called the dominant “spatial planning challenge of the twenty-

first century” (Ravetz et al., 2013). In the U.S., 39% of houses are located within the 9% of the land base considered “wildland-urban interface,” an intermix zone of housing and forests or grasslands (Radeloff et al., 2005). In Europe, the area of land covered by non-urban settlements is growing four times faster than urban land area, and is projected to double in 30–50 years (Nilsson et al., 2013). Exurban and rural development can have significant human and environmental impacts. Land converted to residential, commercial or industrial uses tends to persist as such (Nusser and Goebel, 1997) in contrast with more fluid and reversible land-use transitions between forest and cropland (Hamilton et al., 2013). Urban sprawl can have significant impacts on vehicle miles travelled (Bento et al., 2005) and associated air pollution (Ewing et al., 2003), water quality and quantity (Allan, 2004; Saunders et al., 1991), species extinction (Dirzo and Raven, 2003), farmland loss (Egan and Luloff, 2000; Heimlich, 2001; Theobald, 2001) and wildfire management (Cohen, 2000).

In developed nations with highly mobile populations, housing growth outside of cities is due both to “spillover” as people seek low-cost housing near urban centers, and to “counterurbanization” as people seek out areas far from urban centers for recreation opportunities, access to natural resources, and privacy (Esparza, 2011). Suburbanization at the urban fringe can be described as a succession from agriculture-dominated to commuter-driven landscapes, the former reliant on neighborly relations to reconcile land-use and the latter formal land-use controls to manage growth

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(Rudel, 1989). In contrast, growth outside of urban commuting distance often occurs in areas embodying picturesque pastoral and forested landscapes, with space between neighbors and opportunities for relaxation and recreation (Preissing et al., 1996; Stedman, 2003; Tilt et al., 2007). One example of counterurbanization is the American “rural renaissance” of the 1970s in which seasonal and recreational home development outpaced population growth in many high-amenity rural areas (Radeloff et al., 2010). In forested northern Wisconsin, USA, housing units increased 113% between 1940 and 1990 even though population increased only 6% (U.S. Census Bureau, 1990).

Tools available to governments for shaping and managing growth include zoning and other regulations, development rights transfer programs, taxation, and acquisition of land or conservation easements (Bengston et al., 2004). Zoning is one of the most widely used public policy tools for regulating private land development in North America and Europe (Hirt, 2012; Pendall et al., 2006). In its standard (Euclidean) form, zoning ordinances define districts separated by land uses (e.g., residential, commercial, industrial, agricultural, open space) to prevent a comingling of so-called incompatible uses. Enabling laws allowing local zoning have been enacted in all 50 U.S. states, and over 90% of jurisdictions in major U.S. metropolitan areas have adopted zoning (Pendall et al., 2006). However, many non-metropolitan municipalities remain unzoned (Locke and Rissman, 2015). Land-use policies reflect governmental and community goals that are often at odds with each other, including increased tax revenue generation, job growth, environmental and open space protection, and timber and agricultural production.

In the past century, the question of whether and how zoning shapes housing change has been widely debated (Pogodzinski and Sass, 1990). In rural areas, large-lot zoning (requiring lots of over 2 ha) may preserve “rural character” (Tilt et al., 2007) by restricting housing growth in agricultural and forested areas (Croissant and Munroe, 2002; Robinson and Brown, 2009; Pogodzinski and Sass, 1990). However, resulting dispersed housing patterns can lead to forest and farmland fragmentation (Egan and Luloff, 2000; Heimlich, 2001; Theobald, 2001) and drive up per-capita public service costs (Carruthers and Ulfarsson, 2003; Haight et al., 2004). Some argue that the U.S. focus on strict separation of land uses (Hirt, 2012) has caused the sprawling, car-reliant suburbs in which 60% of U.S. residents live (Pendall, 1999; Warren, 2009) and a displacement of development from regulated urban areas to rural, less regulated areas (Carruthers and Ulfarsson, 2003; Esparza and Carruthers, 2000). Others note that higher valued lands tend to be zoned for residential development while marginal lands are zoned for other uses (Wallace, 1988). These scholars argue that the delineation of zoning districts and the allowance of variances “follows the market” and thus zoning has no effect on development beyond that of a free land market (Wallace, 1988).

A limitation of many zoning studies is that they examine a single jurisdiction such as a county (Pogodzinski and Sass, 1994; Thorson, 1994; Wallace, 1988), and thus give limited insight into how development patterns and political processes interact in various contexts. Cross-jurisdictional studies present the difficulty of establishing meaningful control groups to overcome issues of endogeneity. Zoned jurisdictions tend to be wealthier, more urban, and less conservative than unzoned jurisdictions (Locke and Rissman, 2015; Rolleston, 1987), and studies of zoning effects on housing change must account for the strong possibility that characteristics that lead communities to zone also impact their housing change outcomes (Butsic et al., 2011). Zoning studies have dealt with endogeneity using computer modeled counterfactual scenarios (Robinson and Brown, 2009), maximum likelihood estimation without matching (York and Munroe, 2010), hedonic methods to model development decisions (Lewis and Plantinga, 2007; Lewis et al., 2009), and quasi-experimental designs resulting from policy

changes (Carrión-Flores and Irwin, 2016). Few have used matching methods or discontinuity analysis to take full advantage of variation in the data (Butsic et al., 2011). Propensity score matching allows for causal inferences with few assumptions (Abadie et al., 2004; Ho et al., 2007; Wooldridge, 2011), and has been used to show the effect of moratoria on residential development (Bento et al., 2007), and the effect of purchase of development rights programs in preventing farmland conversion (Liu and Lynch, 2011).

Here we present the first large-*n* longitudinal study of zoning effects on housing change in non-urban areas, using propensity score matching as a preprocessing step to minimize selection bias. Our primary objectives are to quantify the effect of zoning adoption on housing change, and determine what factors influence this effect. Focusing on a large sample of Michigan townships (*n* = 709), we compared changes in housing unit counts in zoned and unzoned townships at four decadal timesteps (1970–2010). A panel regression analysis allowed us to take into account spillover effects, and to show how the effect of zoning on housing change varies with household income and other covariates.

2. Methods

2.1. Study area

The state of Michigan provides a fitting study area to answer land-use policy questions, specifically those related to zoning. Counties in Michigan are divided into civil townships, municipalities that govern unincorporated land (land outside of city or village boundaries). Townships contain over 96% of Michigan's land base and provide a unit of analysis appropriate for longitudinal analyses—Michigan alone contains 1240 townships (typical size 93 km²), each with authority to make policy decisions affecting land use (Michigan Townships Association, 2014). The state has diverse land uses, including agricultural southern Michigan, populous suburbs outside of Detroit and other municipalities, and the forested Upper Peninsula.

The consequences of housing growth vary across the state. By 2040, urbanization in western lower Michigan is predicted to reduce the area of prime farmland, reduce wildlife habitat in coastal areas along Lake Michigan, and impair small watersheds in southwestern Michigan (Pijanowski et al., 2002). In northern lower Michigan, 278,850 ha are considered high-risk fire areas where fuels and buildings intermix (Haight et al., 2004). Despite having the eighth highest number of housing units and fourth highest number of recreational housing units out of all 50 states in 2010, Michigan's rate of housing growth between 1970 and 2010 was relatively modest (ranking 40th out of 50 states).

State enabling legislation passed in the 1920s and 1930s granted zoning authority to cities, villages, townships, and counties. Zoning was advanced as a New Deal innovation to restore the forests of the Great Lakes cutover and bring rational planning to land use (Kates, 2001). In Michigan, county-level zoning is rare, and the decision to regulate unincorporated areas is generally left to townships. In a survey of local governments collected by Michigan State University's Institute for Public Policy and Social Research in 2003, 64% of Michigan's 1240 townships had adopted a township zoning ordinance, 19% had adopted county-level zoning, 10% had no zoning, and 7% gave no response (Institute for Public Policy and Social Research, 2004). 254 townships were excluded from this study because of missing data on zoning adoption date and 94 townships were excluded because they were zoned before 1970 and were therefore unable to be matched based on pre-treatment characteristics. The remaining 892 townships served as inputs to the matching process. Once we had a sample that fit matching criteria

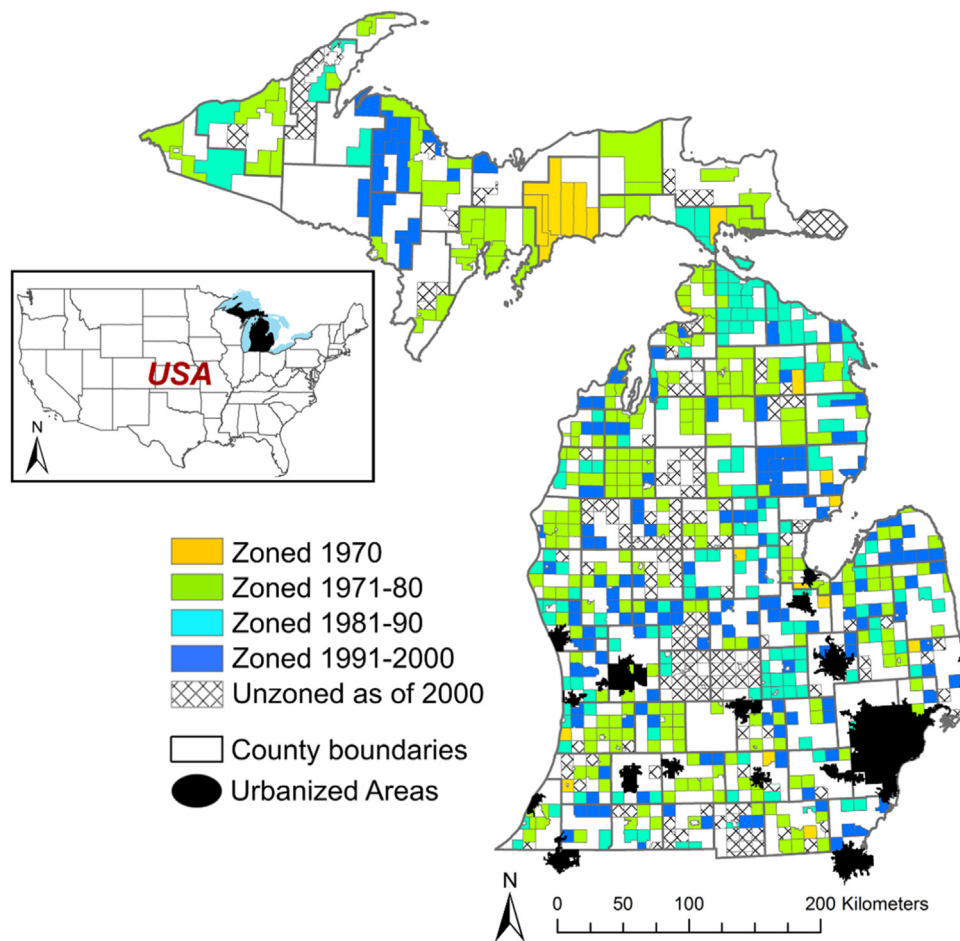


Fig. 1. Year of zoning adoption for study townships in Michigan, USA ($n = 709$). Urbanized Areas are defined as the U.S. Census Bureau as having populations of over 50,000 (U.S. Census Bureau, 2010).

(outlined below), we specified a panel regression model to estimate zoning effects on housing change across four decades (1970–2010).

2.2. Preprocessing: sample selection via propensity score matching

When a treatment group differs from a control group based on inherent, confounding variables, regression techniques alone may not be sufficient to isolate treatment effects (Ho et al., 2007). Matching as a preprocessing step allows researchers to minimize differences between treatment and control groups before regression analyses, to maximize independence between the dependent variable and the covariates and thus minimize model dependence (Ho et al., 2007; Wendland et al., 2015).

For purposes of matching, the zoned (treatment) group contained all townships that adopted zoning anytime between 1970 and 2000, and the unzoned (control) group contained townships unzoned for this entire period. We matched groups of zoned and unzoned townships based on township characteristics at the beginning of our study period, 1970 (Appendix A): mean household income, housing density (# housing units per km^2), percent of housing units classified as recreational or seasonal, distance to urbanized areas of population over 50,000, and percent of township area occupied by lakes or publicly owned land (representing level of natural amenities as well as the percentage of the township that was undevelopable). Township-level data were compiled from the U.S. Census Bureau (Minnesota Population Center, 2011),

the Michigan Department of Natural Resources (Breck, 2004), and the Protected Area Dataset-US (U.S. Geological Survey, 2010).

We calculated propensity scores denoting the probability of adopting zoning using logistic regression in R, and then performed nearest neighbor matching with replacement (because we had fewer control than treated units) using the MatchIt package in R (Ho et al., 2007).

Our matched sample included 709 townships: 561 townships that adopted zoning ordinances between 1970 and 2000, and 148 townships that were unzoned for that time period (Fig. 1). All 709 townships were considered rural ($n = 416$) or exurban ($n = 293$) in 1970, based on housing density (Theobald, 2005). Unmatched townships were either unzoned townships that failed to provide matches for the zoned townships based on the propensity score matching algorithm ($n = 173$), or were zoned townships that did not fall within the range of common support ($n = 10$).

To assure a balanced match based not only on propensity score but also each covariate, we diagnosed post-matching balance by comparing the normalized difference in means (Eq. (2)) between the zoned and control groups for each covariate in the unmatched and matched datasets (Imbens and Wooldridge, 2009). Matching reduced the normalized differences in means between zoned and unzoned townships for all covariates (Table 1, Appendix A). All post-match variables had normalized differences in means below the 0.25 threshold at which regression estimates are sensitive to bias (Imbens and Rubin, 2015); we therefore did not expect any

Table 1

Normalized differences in means and summary statistics of study variables before and after propensity score matching.

Variable	All Townships (N = 1240)	Pre-Match Townships (n = 892)			Matched Study Townships (n = 709)		
	Mean, standard deviations in parentheses	Zoned Mean (n = 637)	Unzoned Mean (n = 255)	[Norm. Diff. in Means]	Zoned Mean (n = 561)	Unzoned Mean (n = 148)	[Norm. Diff. in Means]
Household income ^a (\$1000s, 1970)	9.55 (2.01)	9.87 (2.60)	8.54 (2.11)	0.40	9.15 (1.90)	9.20 (2.03)	0.01
Housing density ^a (# Units/km ² , 1970)	14.1 (270)	17.8 (63.6)	6.17 (8.77)	0.18	8.87 (11.8)	7.70 (12.6)	0.06
% Housing units recreational ^a (1970)	21.8 (24.2)	21.1 (22.6)	25.4 (22.5)	0.16	24.3 (23.0)	21.5 (21.6)	0.12
Distance to Urbanized Area >50,000 pop. ^b (km)	68.0 (56.3)	57.5 (71.3)	97.8 (75.8)	0.39	75.5 (73.6)	66.4 (66.1)	0.09

^aData from the 1970 Census Bureau Summary File 1 (U.S. Census Bureau, 1970; Minnesota Population Center, 2011).^bBased on the Census Bureau definition of Urbanized Areas (U.S. Census Bureau, 2010).

remaining differences between the matched groups to significantly influence regression results.

$$(\text{mean}_{\text{zoned}} - \text{mean}_{\text{unzoned}}) / \sqrt{(\text{variance}_{\text{zoned}} + \text{variance}_{\text{unzoned}})} \quad (2)$$

Differences between control and treatment groups should not be compared with *t*-tests, as the *t*-statistic is a poor metric on which to base matching criteria because it is drastically affected by sample size (Imbens and Wooldridge, 2009). In cases where there are large discrepancies in sample sizes between groups (as in our case), normalized difference in means is a more reliable metric (Imbens and Wooldridge, 2009).

2.3. Panel model

Panel models analyze longitudinal data by controlling for repeated measures for each observation (Bell and Jones, 2015). We obtained township-level data from the U.S. Census Bureau (Minnesota Population Center, 2011), the Michigan Department of Natural Resources (Breck, 2004) and the Protected Area Dataset-US (U.S. Geological Survey, 2010) to estimate zoning effects on housing change in decadal time steps between 1970 and 2010. After matching, we estimated a fixed effects panel regression model to estimate the impact of a host of independent variables, including zoning on the log of the number of housing units in a township:

$$\ln(HU)_{it} = B_0 + B_1 \text{zoned}_{it} + B_2 \text{Year}_t + B_6 \ln(HUlag)_{it} + B_7 \% \text{Rec}_{it} + B_8 \ln(\text{income})_{it} + B_9 \% \text{adjacentzoned}_{it} + m_i + e_{it} \quad (1)$$

Where $\ln(HU)_{it}$ is the natural log of the number of housing units in township *i* in time *t*, *zoned* is equal to 1 if a township is zoned in that time period, *Year* is a dummy variable for each time period, *HUlag* is the average number of housing units in adjacent municipalities (townships, villages, and cities), *%Rec* is the percent of housing units that is second home ownership, $\ln(\text{income})$ is the natural log of the median household income for the township, adjusted to 2009 levels (median household income was not available for 1970 so mean was used for that year), *%adjacentzoned* is the percent of the surrounding townships that were zoned, *m* is a township specific error term, and *e* is the observation specific error term. A municipality was considered adjacent to the focus township if its border touched the township's border. See Appendix A for more information on the study variables.

In Model 1, the zoning variable was assumed to be independent of other variables. To allow the effect of zoning to vary given the values of other covariates, we interacted the zoning variable with other covariates. First, we interacted zoning with the spatial lag term (*HUlag*), to see if the impact of zoning on housing change was affected by the number of housing units in adjacent municipalities (Model 2). Next, we added an additional interaction term between

zoning and *%Rec_{it}*, to test if zoning had a different impact on housing change in areas with high second home ownership (Model 3). Finally, we ran a model where zoning was interacted with all other covariates, allowing the effect of zoning to vary over income levels and time (Model 4). All analyses were completed using the XTREG command in STATA. See Appendix A for more details on the covariates, matching process, and panel model.

3. Results

The results of Model 1 predicted a small but significant effect of zoning on housing change. Model 1 predicted zoned townships to have 1.7% fewer housing units in the decade following zoning than similar townships without zoning (Table 2). This effect remained statistically significant for the models which included interactions (Models 2–4). The magnitude of the effect increased as more interactions were added, increasing to 2.8% for Model 4.

The interactions between zoning and other covariates were statistically significant for all variables except *Year*. The coefficient of the interaction between zoning and the spatial lag variable was always positive and statistically different from zero, indicating that zoning was less effective if townships surrounding it already had high amounts of housing. Likewise, zoning was less effective in areas with higher incomes (Fig. 2) and higher percentages of sec-

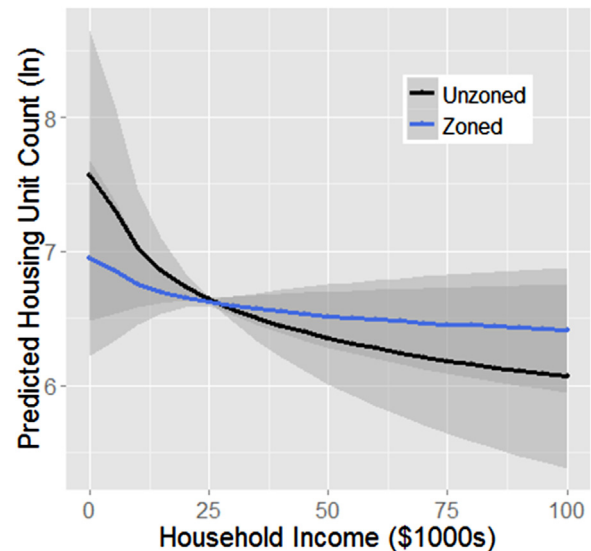


Fig. 2. Panel model predictions of housing units in zoned and unzoned Michigan townships (n = 709), plotted against household income. Shaded areas indicate 95% confidence intervals.

Table 2

Coefficients and robust standard errors from four panel models.

VARIABLES	MODEL 1	MODEL 2	MODEL 3	MODEL 4
	ln (housing units)	ln (housing units)	ln (housing units)	ln (housing units)
zoned	-0.0171* (0.00913)	-0.355*** (0.113)	-0.498*** (0.119)	-0.685*** (0.137)
year1980	0.0556** (0.0226)	0.0686*** (0.0223)	0.0743*** (0.0220)	0.1000*** (0.0269)
year1990	0.0481 (0.0385)	0.0629 (0.0379)	0.0713* (0.0374)	0.124** (0.0477)
year2000	0.0597 (0.0541)	0.0741 (0.0535)	0.0858 (0.0527)	0.164** (0.0673)
year2010	0.0733 (0.0602)	0.0847 (0.0598)	0.0971 (0.0590)	0.193** (0.0739)
year1980:zoned				-0.0117 (0.0516)
year1990:zoned				-0.0653 (0.0626)
year2000:zoned				-0.112 (0.0753)
year2010:zoned				-0.137* (0.0779)
ln(HUlag)	0.751*** (0.0576)	0.720*** (0.0607)	0.707*** (0.0599)	0.703*** (0.0605)
ln(HUlag):zoned		0.0482*** (0.0163)	0.0650*** (0.0167)	0.0509*** (0.0188)
%Rec	0.00290*** (0.00100)	0.00296*** (0.00101)	0.00285*** (0.00102)	0.00272*** (0.00102)
%Rec:zoned			0.00137*** (0.000392)	0.00193*** (0.000414)
ln(income)	0.0536 (0.0349)	0.0496 (0.0346)	0.0474 (0.0348)	-0.00179 (0.0449)
ln(income):zoned				0.100** (0.0450)
%adjacent zoned	0.000786*** (0.000260)	0.000902*** (0.000275)	0.000859*** (0.000266)	0.000687* (0.000409)
%adjacent zoned:zoned				0.000257 (0.000400)
Constant	1.162*** (0.386)	1.372*** (0.407)	1.464*** (0.399)	1.610*** (0.421)
Observations	3545	3545	3545	3545
R-squared	0.811	0.813	0.815	0.816
n (Township count)	709	709	709	709
Robust standard errors in parentheses				

*** p < 0.01, ** p < 0.05, * p < 0.1

ond homes, as shown by the positive and significant coefficients for these interaction terms. Areas with more second homes typically had faster growth in housing units. A 1 percentage point change in %Rec lead to a 0.29% (Models 1–2) and 0.36% (Models 3–4) change in housing units.

We also found evidence of spillover effects from surrounding jurisdictions. Townships that were surrounded by townships with more housing units were expected to have more housing units. A 1% increase in the surrounding housing units would lead to a 0.75% increase in that township's housing units. This result was similar across all four models. Areas that were adjacent to townships that were zoned also experienced increased housing units. All other variables held constant, a township with 100% zoned neighbors was predicted to increase its housing units by 8.4% compared to a township adjacent to no zoned townships.

4. Discussion

Despite a body of literature concluding that zoning is ineffective within single jurisdictions (Pogodzinski and Sass, 1994; Thorson, 1994; Wallace, 1988), this panel model showed a small overall negative effect of zoning on housing change in rural and exurban jurisdictions. This zoning effect was tempered by other variables, highlighting the variability of land-use policy effects in different contexts. Zoning was less effective in jurisdictions with higher

income and more recreational homes. Zoning led to higher housing growth in higher-income townships, but to lower housing growth in lower-income areas. Zoning was also less effective if neighboring townships had higher amounts of housing, or if neighboring townships were zoned. Methodologically, the use of pre-regression matching combined with fixed effects panel models provides a robust way to control for endogeneity in models of land-use change. Such methods have been applied widely in evaluating forest protection (Arriagada et al., 2012; Brandt et al., 2015) and proved useful in the context of zoning here. Demonstrating heterogeneous effects of zoning across space helps to clarify the conundrum of why zoning appears to sometimes prevent (Croissant and Munroe, 2002) and sometimes promote (Esparza and Carruthers, 2000) housing growth outside of urban areas.

The panel models predicted higher housing growth in zoned jurisdictions with median household income over \$28,000, but lower housing growth in poorer areas, compared to unzoned jurisdictions. Past research has explored underlying drivers that may explain our findings. Although we do not examine lot size requirements in different jurisdictions, it may be that large-lot zoning in rural areas effectively lowers the supply of land and reduces opportunities for housing growth (Croissant and Munroe, 2002; Robinson and Brown, 2009; Pogodzinski and Sass, 1990), whereas this effect is not seen in wealthier suburban areas where lot sizes are smaller (Magliocca et al., 2012).

These results may also be explained by different preferences of residents and developers in lower-income rural areas versus higher-income suburban areas closer to cities (Clawson, 2013; Fitchen, 1991; Rudel, 1989). In our study, distance to urbanized areas with population over 50,000 was negatively correlated with income ($r = -0.51$, $p < 0.0001$ in 1990). In other words, more remote areas had lower household income. It may be that zoned areas are more desirable to home buyers in wealthier suburban communities (Clawson, 2013), but less desirable in lower-income rural communities where restrictive zoning is less popular (Fitchen, 1991; Rudel, 1989).

We expect these trends to be correlated with landcover as well. York and Munroe (2010) found evidence that zoning protected Indiana farmland, while the proportion of land forested was better predicted by economic factors and terrain than zoning. Though we did not incorporate landcover variables in our models directly (consistent landcover datasets were unavailable for the time period of our study), strong correlations between landcover variables and our model covariates indicate that our covariates accounted for much of the variation in the model that would be provided by landcover variables. Using Census data from 2000 (U.S. Census Bureau 2000) and landcover data from 2001 (Homer et al., 2007), the percent of township area in farmland was positively correlated with income ($r = 0.42$, $p < 0.001$) and negatively correlated with recreational homes ($r = -0.68$, $p < 0.001$). In contrast, forestland had a negative relationship with income ($r = -0.44$, $p < 0.001$) and a positive relationship with recreational homes ($r = 0.64$, $p < 0.001$). We can characterize the areas where zoning deterred housing growth as more remote, more forested, lower-income and with more recreational homes. The areas where zoning did not deter housing growth were on average less remote, more agricultural, higher-income and with fewer recreational homes. These results give us different interpretations than in York and Munroe (2010), though similarly point to the importance of economic structure in moderating the effects of zoning on land-use change.

Our results do not contradict the claim that strict land-use regulations push housing growth to less regulated areas (Carruthers and Ulfarsson, 2003; Esparza and Carruthers, 2000). Spillover effects can result from an increase in housing prices and reduced availability of housing units within zoned jurisdictions, which may lead residents to shift to nearby jurisdictions (Pollakowski and Wachter, 1990). In our study a township surrounded by zoned townships was predicted to have an 8.4% higher housing growth than a township surrounded by unzoned townships, all other variables being equal. It is clear that context matters for land-use policy and that local decisions can influence regional patterns of development and land-use change.

In the U.S., zoning ordinance terms vary by municipality (Norton, 2008). We identified general patterns across municipalities, but did not account for differences in zoning terms that may determine the amount of development that may occur and what shape new development takes. Large-lot zoning (requiring lots of over 2 ha) has been shown to push development towards urban centers (Magliocca et al., 2012), but less restrictive measures have little effect on the spatial distribution of development (Arendt, 1997; Magliocca et al., 2012). Large lot sizes may lower the housing potential in a rural jurisdiction, but may also promote dispersed development patterns. Large lots can better accommodate septic systems than small lots, and on-site septic systems allow rural residences to locate outside of public sewer authority boundaries (Newburn and Berck, 2006). This study showed the overall effect of zoning on the quantity of housing units, but further study must be done to determine how policy shapes spatial patterns of development (Warren, 2009) and how policies can be crafted to maximize desirability and efficiency. A useful extension of this study would be to look specifically at variations in zoning codes in order to identify

what types of zoning regulations may increase or decrease housing growth.

Landscape fragmentation due to rural and exurban housing growth can negatively impact ecosystems and ecosystem services (Mcdonnell and Pickett, 1990; Wear et al., 1998). Water quality, ecosystem function, and native species abundance decrease as human land uses fragment open spaces (Hansen et al., 2005; Mcdonnell et al., 1997; Wear et al., 1998). Furthermore, wildfire management is becoming increasingly difficult and expensive in the wildland-urban interface where homes mingle with forests and grasslands (Haight et al., 2004). While human-adapted wildlife and non-native plants may proliferate in exurban residential areas (Odell and Knight, 2001; Maestas et al., 2003), human-sensitive songbirds and medium-sized mammals avoid housing developments with densities as low as 1 house per 13 ha (Odell and Knight, 2001). The median housing density in our full set of Michigan townships ($n = 709$) was already higher than this in 1990, at 1 housing unit per 10 ha. The consequences of rural and exurban housing patterns are outside the scope of this study, but the effects of land-use policy decisions on ecosystems deserve more attention in both scientific and policy realms.

Linking conservation science and land-use policy on private lands is difficult in nations where land-use regulation is decentralized and produces complex and inconsistent effects. Existing literature contains many seemingly idiosyncratic case studies of single jurisdictions (Pogodzinski and Sass, 1994; Thorson, 1994; Wallace, 1988). This frustrating lack of generalizability has contributed to doubts about zoning effectiveness. We show how patterns in this heterogeneity can emerge in a large-n, longitudinal study. In this study, zoning was predicted to reduce housing growth overall, and especially in lower-income areas. The advantages and disadvantages of limiting housing growth depend on local and national visions of sustainable development. There is room for more open dialogue between conservation scientists, planners, and local communities in designing land-use policies that strive to achieve visions for sustainable and livable communities, informed by evidence from historical trends.

5. Conclusions

Crafting effective land-use policy relies on an understanding of the complex drivers of land-use change. This study provides important evidence for heterogeneous but quantifiable effects of zoning on housing change, which informs a long-running debate about zoning effects. In Michigan, zoned townships had on average 2.2% fewer housing units in the decade following zoning than similar unzoned townships, though this effect was tempered by other variables: income, number of recreational homes, and zoning of adjacent townships. Higher-income townships had more housing units a decade after zoning adoption compared to higher-income unzoned townships, whereas lower-income townships with zoning had fewer housing units than similar unzoned townships. This study offers an example of how pre-regression matching can be combined with fixed effects panel models to control for endogeneity in models of land-use change. These methods can be applied to studies in other locations and policy arenas to investigate policy effects on a diverse array of variables.

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Appendix A.

This Appendix is divided into three parts: 1) data sources and variable explanation, 2) specifics of the matching process, and 3) discussion of panel regression assumptions and possible biases.

A.1. Data sources and variable explanation

With the goal of tracking long-term trends in housing change, we recorded housing unit counts and zoning adoption decisions at the township level from 1970 to 2010. Definitions and data sources of all variables in Table A.1 below.

See [Table A1](#)

A.2. Matching process

Derivation of propensity scores and calculations to assess balance (i.e. normalized differences in means) were done “by hand” to retain a level of control over products into and out of the matching process. Propensity scores were derived from the fitted values resulting from a probit model fit using the `glm` command in R. Townships were then matched on propensity score using the `MatchIt` package in R using the code, `match.output <- matchit(zoned ~ pscore, data = dat, method = “nearest”, caliper = x, replace = TRUE, discard = “both”)`, where *zoned* is a binary variable to denote zoning status and *pscore* is the propensity score representing a township’s probability of adopting zoning. The final matching algorithm was found by moderating the caliper distance *x* to be as small as possible while maintaining an acceptable sample size. The difference between no caliper specification and that of 0.02 was only a difference of 97 townships (*n* = 807 vs. *n* = 709) but a marked improvement in balance. Reducing the caliper to 0.01, however, reduced the sample size significantly to 579 while offering no improvement in balance. The caliper specification used for the final match was therefore set at 0.02. Improvement in balance of propensity scores is visualized in the scatterplots, histograms, and qq-plots below ([Fig. A1](#)), and reported for each covariate in the main text.

In the literature on impact evaluation that focuses on matching methods, normalized difference in means is generally considered the best metric for assessing balance between treated and untreated groups ([Imbens and Wooldridge, 2009](#); [Imbens and Rubin, 2015](#)). Unlike *t*-tests, normalized difference in means is not

sensitive to differences in sample size between groups, and unlike Wilcoxon tests, it directly accounts for variance in the data, which is extremely important in biasing regression results. However, we report Wilcoxon test results here for readers’ information: After matching, Wilcoxon tests showed matched zoned and unzoned townships to have similar income (*p* = 0.88), percent recreational housing (*p* = 0.12), and distance to Urbanized Area (*p* = 0.69), though showed differences in housing density (*p* = 0.002). Pre-analysis matching need not be perfect, as regression methods can account for leftover variation between groups. As the normalized differences in means were below 0.25 for all covariates, we are confident regression results were not biased due to differences in the covariates between treated and control groups.

A.3. Panel model regression diagnostics

Statistical methods chosen in this study are drawn largely from econometrics for several reasons. Econometrics were borne out of a need for analyzing non-experimental data that most often violate common assumptions of parametric models like normality of data and homoscedasticity of variance ([Croissant and Millo, 2008](#)). We chose fixed-effects models over random-effects models because they make less strict assumptions about independence ([Allison, 2009](#)). We added covariates to the models to account for variation in the data not accounted for by the matching procedure (perfect matching is unrealistic with multiple continuous variables). We furthermore employed cluster-robust standard errors to relax the homoscedasticity assumption ([Thompson, 2011](#)) and selected our sample using propensity score matching to remove problematic outliers ([Abadie et al., 2004](#); [Ho et al., 2007](#); [Wooldridge, 2011](#)).

Unlike other panel regression models in medical and social science disciplines, we did not face some common sources of bias like dropout (attrition). Our sample was balanced; all townships present at the beginning of the study period (1970) were present at the end (2010). Because the response rate of local governments was so high (93%) in the Michigan State survey ([Institute for Public Policy and Social Research, 2004](#)), we were able to compile a study sample that we are confident adequately represents the natural variation of the population. However, not all survey data were usable (253 out of 1240 townships were discarded because they did not have dates associated with zoning adoption) and if discarded townships systematically differed from study townships, participant bias could be of concern. Nevertheless, this should not affect our ability to determine zoning effects, because zoned and unzoned townships were matched on several confounding factors.

Table A1
Definitions and data sources for study variables.

Variables	Definition
Dependent	
Housing units (HU) ^{a,b}	Number of US Census-defined housing units at each decadal timestep
Independent	
Year	Dummy variable for decadal timestep (1970, 1980, 1990, 2000, 2010)
Zoned ^a	Dummy variable indicating if townships was zoned or unzoned at each decadal timestep
Housing unit lag variable (HULag) ^{b,c}	Average number of housing units in adjacent municipalities (townships, villages, and cities)
Percent Recreational Housing Units (%Rec) ^{b,c}	Percent of total housing units defined as seasonal or recreational (i.e., second homes)
Income ^{b,c}	Mean (1970) or median (1980, 1990, 2000) household income reported in thousands of US dollars, adjusted to 2009 levels
% adjacent zoned ^a	Percent of surrounding townships zoned at each decadal timestep

^a [Institute for Public Policy and Social Research, 2004](#). To plan or not to plan: Current activity within Michigan’s local governments. *Institute for Public Policy and Social Research. Policy Brief, 8*.

^b U.S. Census Bureau 1970, 1980, 1990, 2000, 2010. *Summary File 1*. United States Census Bureau (Washington, D.C.).

^c [Minnesota Population Center \(2011\) National Historical Geographic Information System: Version 2.0](#) (University of Minnesota, Minneapolis, MN) Available at: <http://www.nhgis.org>.

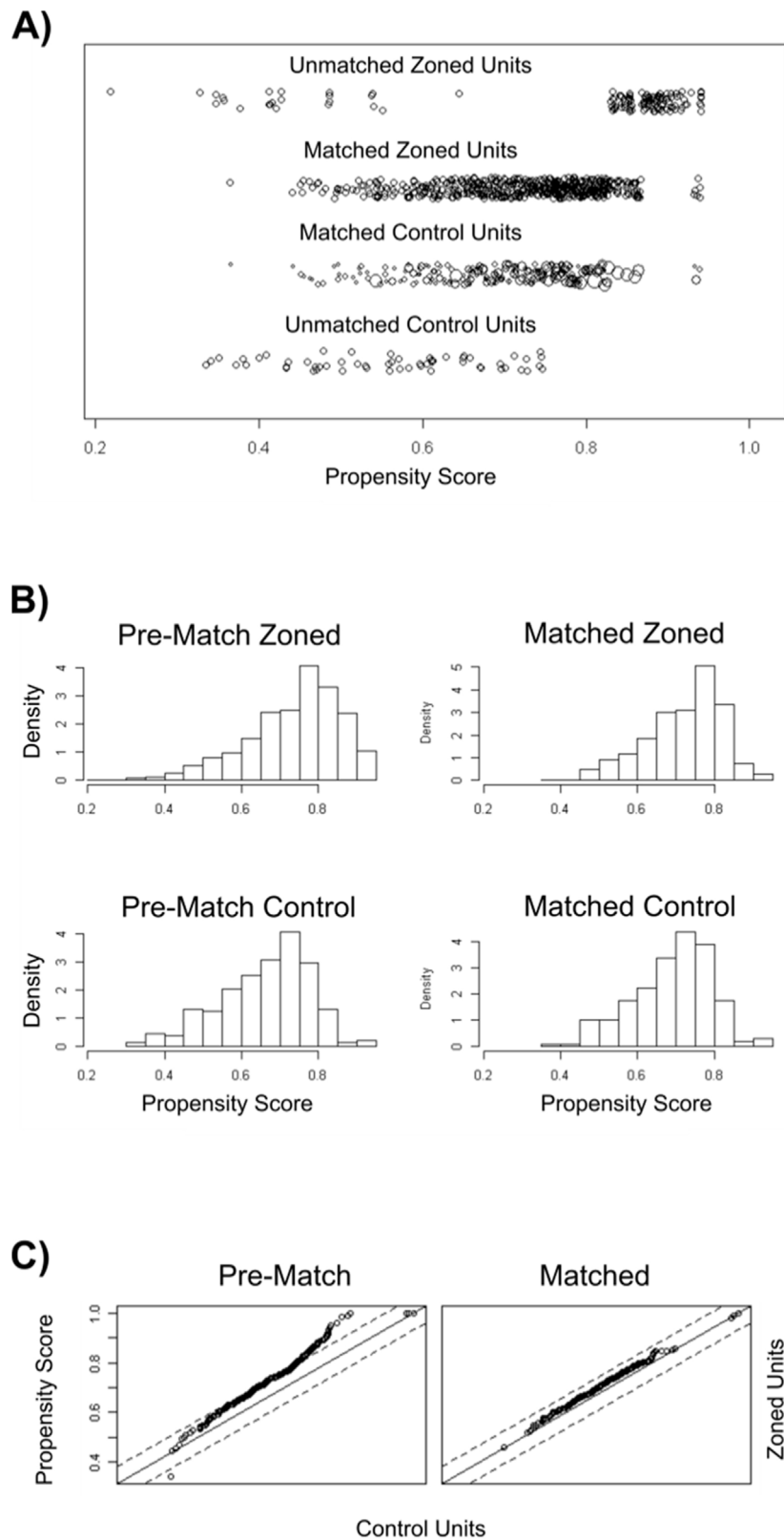


Fig. A1. Propensity score distribution of zoned and unzoned (control) pre-matched townships ($n = 892$) and post-matched study townships ($n = 709$). Improvement in balance following matching is shown via A) scatterplot, B) histogram, and C) qq-plot. See Table 1 in the main text for further diagnostics.

Omitted variable bias is another potential concern. One variable we were unable to quantify was availability of infrastructure. Comprehensive statewide data on municipal sewer service and impact fee policies are lacking in Michigan so were not included

in this analysis, but these variables could be incorporated into future research to address these hypotheses. Construction costs are cheapest for developers where there is existing infrastructure (sewers, roads, etc.) at the building site or when new infrastruc-

ture costs are covered by taxpayer dollars. When construction costs are transferred to developers through impact fees, housing growth rates decrease due to increased development costs (Skidmore and Peddle, 1998). It is possible that construction costs differed in our zoned and unzoned study areas and contributed to the differences in housing growth we saw. The availability of public sewer systems is an important predictor of urban-fringe development, though rural development dependent on individual septic systems is able to leapfrog “well beyond” designated sewer service areas (Newburn and Berck, 2006). If existing infrastructure is more prevalent, or policies like impact fees less prevalent, in higher-income zoned areas compared to unzoned jurisdictions, we would expect these variables to partially account for the higher housing growth rates we saw in these areas.

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