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Land-use reforms and housing costs: Does allowing for increased density lead to greater affordability?

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Abstract

We generate the first cross-city panel dataset of land-use reforms that increase or decrease allowed housing density and estimate their association with changes in housing supply and rents. To generate reform data, we use machine-learning algorithms to search US newspaper articles between 2000 and 2019, then manually code them to increase accuracy. We merge these data

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with US Postal Service information on per-city counts of addresses and Census data on demographics, rents, and units affordable to households of different incomes. We then estimate a fixed-effects model with city specific time trends to examine the relationships between land-use reforms and the supply and price of rental housing. We find that reforms that loosen restrictions are associated with a statistically significant 0.8% increase in housing supply within three to nine years of reform passage, accounting for new and existing stock. This increase occurs predominantly for units at the higher end of the rent price distribution; we find no statistically significant evidence that additional lower-cost units became available or moderated in cost in the years following reforms. However, impacts are positive across the affordability spectrum and we cannot rule out that impacts are equivalent across different income segments. Conversely, reforms that increase land-use restrictions and lower allowed densities are associated with increased median rents and a reduction in units affordable to middle-income renters.

Keywords
affordable housing, housing, land use, zoning

摘要
针对提高或降低允许胡住房密度的土地使用改革，我们生成了第一个跨城市面板数据集，并估计了这些土地使用改革与住房供应和租金变化的关系。为了生成改革数据，我们使用机器学习算法搜索了 2000 年至 2019 年间的美国报纸文章，然后对其进行手动编码以提高准确性。我们将这些数据与美国邮政服务信息以及人口普查数据合并，前者有每个城市地址的计数，后者有关于人口统计、租金和不同收入家庭的可负担单元的数据。然后，我们就城市在特定时间的趋势，估计了一个固定效应模型，以考查土地使用改革与出租房屋的供应和价格之间的关系。我们发现，在放宽限制的改革通过后的三到九年内，这些改革与住房供应（包括新房和现有存量房）0.8% 的统计显著性增加相关。这种增长主要发生在租金价格较高的单元；我们没有发现任何具有统计意义的证据表明在改革后的几年中有更多低价格单元可以租到或价格进一步降低。然而，在整个可负担的范围内，影响都是积极的，我们不能排除影响在不同收入阶层之间是相同的。相反，增加土地使用限制和降低允许密度的改革与租金中位数增加和中等收入租房者的可负担单元减少相关。

关键词
可负担的住房、住房、土地使用、分区

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Introduction

The United States is facing a housing affordability crisis that is exacerbating economic and racial inequities (Matlack and Vigdor, 2006; Popov, 2019). Rental prices were at an all-time high before the COVID-19 pandemic (Joint Center for Housing Studies of Harvard University, 2018), and as of March 2021, an estimated 10.7 million adults were delinquent on rent (Center on Budget and Policy Priorities [CBPP], 2021). These challenges disproportionately affect households with low incomes and people of colour since they are more likely to rent than own their homes (Neal et al. 2021).

One explanation for the affordability crisis is that supply has not matched demand.

While demand for rental units rose across all income bands between 2005 and 2015, the supply of rental housing costing less than $800 a month (2016 dollars) dropped as the national rental vacancy rate fell to a 30-year low (Joint Center for Housing Studies of Harvard University, 2018). Many metropolitan areas have experienced increases in housing prices and decreases in new construction over the past 25 years (Freemark, 2022).

The debate over how to increase the supply of affordable housing, however, stands unresolved. Many housing economists posit that inadequate supply stems from overly restrictive land-use regulations. Loosening these restrictions might increase housing production and thus decrease prices (Glaeser et al., 2005; Malpezzi, 1996; Quigley and Raphael, 2005). Research on housing filtering – the process by which properties age and depreciate into affordability – shows that new construction, even if rented or sold at prices above the market average, eventually opens less-expensive housing units for lower-income residents (Liu et al., 2020; Mast, 2021).

But others argue that loosening land-use restrictions (e.g. by increasing height limits) may not increase housing supply because loosened zoning may simply standardise common requests for variations from by-right rules that local zoning commissions already systematically approve for developers (Lo et al., 2020). Moreover, even if a regulatory change yields a supply increase, prices may not fall (or stop rising) accordingly. In rezoned areas, builders might convert existing lower-cost units into higher-cost ones; the amenity effects resulting from these conversions plus associated neighbourhood retail and public safety improvements may, in turn, increase surrounding housing values (Jacobus, 2016). As such, additional research is necessary to identify the effects land-use reforms have on housing supply and price.

To examine these issues, we undertake the first cross-city panel analysis of the impact of land-use reforms on housing supply and rents. While other studies have examined the effect of land-use reforms on supply or housing costs for *individual* cities or *individual* reforms (e.g. Kuhlmann, 2021), we are the first to use a machine-learning approach to identify a diversity of reforms, and then pinpoint their effects in multiple cities simultaneously. We also examine reform impacts on rents rather than sales prices – a unique contribution – and offer insight into how regulations impact unit availability at varying rent levels. We are specifically interested in housing that is affordable, which we define as units that cost no more than 30% of income for low- and moderate-income families, in both subsidised and non-subsidised projects (these units could be newly constructed or filtered down). We limit our investigation to reforms and impacts within individual cities, not across metropolitan areas.

We generate a dataset of a variety of land-use reforms across eight US metropolitan regions encompassing 1136 cities from 2000 to 2019. We also collect data on housing supply and costs. We then develop a random-trend model to estimate outcomes. We find that cities that passed reforms loosening land-use regulations (increasing allowed housing density, or 'upzoning') saw a statistically significant increase in their housing supply compared to cities without reforms. This increase, however, occurred predominantly for rental units affordable to households with higher-than-middle-incomes over the short- and medium-term following reform passage; effects for units affordable to those with extremely low incomes and very low incomes were positive but not significant, perhaps due to the small number of such units at baseline in each city. Cities with reforms that increased regulatory restrictiveness (reducing allowed housing density, or 'downzoning') did not experience a change in housing supply compared to cities without reforms, though downzonings were associated with a significant increase in median rents and a reduction in rental units affordable to middle-income households.

These results suggest that reforms loosening restrictions are, on average, associated with an uptick in new housing supply. But this increase is likely inadequate to expand the availability of housing affordable to low- and middle-income households in the short-term, at least within the jurisdictions that execute reforms, and among the reforms that we studied. Reforms tightening regulations are associated with increased rents, potentially worsening conditions for low- and moderate-income renters. Cities should consider pairing direct investments in housing subsidies, such as immediate investments in housing vouchers and project-based subsidies for publicly assisted housing, with reforms loosening restrictions to address both short-term and long-term housing affordability.

Conceptual framework

Land-use regulations like zoning are generally implemented in the United States by local governments under rules set by states. For years urban economists and housing scholars have sought to understand how they affect housing supply and prices. Though research has identified how regulation restricts construction and raises prices, researchers have yet to come to consensus on the degree to which loosening regulations reverses those effects. Nor have they specified impacts on the supply of rental housing affordable to low- and moderate-income households.

One common approach to evaluating regulations begins by breaking down home price into the sum of its components: labour, materials, neighbourhood attributes, land, and process costs. Higher labour and materials do not appear to dramatically increase housing costs (Gyourko and Saiz, 2006); inflation-adjusted construction costs have remained essentially flat since the 1990s while housing prices have trended upward (Gyourko and Molloy, 2015). Land-use regulations, however, may be partially to blame for high costs. Zoning, impact fees, building codes, review processes, and other regulations have proliferated since the 1970s. These restrictions act as a component part of housing costs and dampen supply (Glaeser, 2017; Kok et al., 2014). Glaeser et al. (2006a) explore the wedge between marginal construction costs and market price, labelling the gap a ‘regulatory tax’ ranging from zero in a few low-demand and low-regulation cities to upwards of 50% of home values in the Bay Area and Manhattan. These studies suggest that regulation reduces supply elasticity, resulting in larger price increases and slower growth in quantity as demand increases, as well as lower responsiveness to demand shocks (Saiz, 2010).

Several cross-sectional, point-in-time studies further explore these phenomena. Using a dataset on lot size, environmental, and subdivision laws in Boston-region jurisdictions, Glaeser et al. (2006b) find that stricter regulations have a negative effect on construction and lead to higher prices. Other cross-sectional studies of specific land-use policies’ effects on construction capture effects on a state-wide or national level. Schuetz and Murray (2019) find that cities with less restrictive zoning issued more permits for multifamily development. Similarly, Mawhorter et al. (2018) finds that a higher proportion of single-family zoning and higher parking requirements in a jurisdiction were negatively associated with multifamily housing production. Using a national survey, Gyourko et al. (2008) identify positive correlations between component measures of regulatory restrictiveness (i.e. strict regulations in multiple dimensions, like parking requirements and height limits, are related), as well as higher housing prices.

Several of these studies use surveys to assess regulatory stringency, but these surveys raise concerns because planners assessing their own land-use rules may not offer an objective view of regulatory stringency (Lewis and Marantz, 2019). Moreover, despite the comparative lessons the above studies offer, they are limited because static cross-sectional data cannot confirm the effects a policy reform would have over time, nor can their averaged housing-price data discern variations in housing production at different levels of affordability.

Other researchers have focused on changes in regulations. Zabel and Dalton (2011) find that larger minimum lot size requirements significantly increased prices over time. Glaeser and Ward (2006) find that those lot size increases decreased housing permits issued. Similarly, Kahn et al. (2010) examine a reform that increased restrictiveness on construction, finding that it increased prices.

Broad-scale regulatory changes that increase allowed housing-unit density are rare, thus few studies have captured their effects on prices – and those that do typically examine jurisdictions one by one. Dong (2021) finds that increased allowed density in Portland was associated with a greater probability of long-term development – though the number of new units developed was small. Freemark (2020) finds that a Chicago reform allowing for higher densities and reduced parking requirements raised prices without affecting supply. Kuhlmann (2021) and Zhou et al. (2008) report similar findings in other cities. Greenaway-McGrevy et al. (2021) show significant increases in parcel costs for underdeveloped land in Auckland, New Zealand after allowed densities were increased. While such studies are suggestive of the impacts of loosened regulations on land values and sales prices, none looks at impacts on rents, nor do they estimate average treatment effects across multiple jurisdictions.

Given these findings, telling a consistent story about zoning reform impacts is difficult – especially when it comes to affordable housing. New construction creates positive spillovers for existing neighbourhoods by improving aesthetics, removing eyesores, and adding neighbourhood vibrancy (Zahirovich-Herbert and Gibler, 2014). These amenity effects exert upward pressure on housing prices (Damiano and Frenier, 2020; Rossi-Hansberg et al., 2010), and newly constructed homes tend to cost more than the older buildings they replace or abut (Zillow, 2020). We may thus expect reforms reducing restrictiveness to decrease affordable housing supply.

Alternatively, the economic principles of supply and demand indicate that an increase in housing availability should reduce scarcity and increase competition among sellers, reducing prices. Additionally, scholarship on housing filtering tells us that supply increases create a chain of out-migration into newer units, creating newly affordable residences (Mast, 2021). Thus, we may expect supply allowances to add units to the market that are affordable for low- and moderate-income families. A recent series of working papers examining the impacts of new housing largely find that such construction reduces rents in the surrounding area, potentially limiting displacement (Asquith et al., 2019; Li, 2022; Pennington, 2021; Phillips et al., 2021). That said, it is possible that outcomes vary by market segment, with more of an effect on moderating the costs of higher-end housing (Damiano and Frenier, 2020). This latter phenomenon could reduce affordable housing in neighbourhoods where amenity affects outweigh supply effects in the context of upzoning, but an increase in affordable housing in the region overall.

Unlike studies leveraging surveys to identify regulatory stringency, we focus on the parameters of actual reforms in individual municipalities, where land-use regulations are written. This allows us to avoid potential biases inherent in survey-based analysis. Unlike studies that examine zoning reforms in individual cities, we use machine-learning approaches to develop a cohort of changes in *multiple cities*. We also provide insight into unit availability by rent level, unlike most studies that examine home sales.

Our research links reforms passed within an individual jurisdiction with outcomes within that same jurisdiction. We acknowledge that reform impacts vary based on scale; it is possible that reforms passed in one jurisdiction have impacts across an entire metropolitan area. A reform increasing housing production in one city could have limited effects therein because of amenity effects surrounding construction – yet at the same time reduce prices in the region overall due to increased supply. This is a key finding in Buechler and Lutz's (2021) examination of zoning changes in Zurich, Switzerland. The latter effect may ultimately be more important for residents seeking housing, since households have the ability to choose between multiple jurisdictions to live in a metropolitan real-estate market, but we do not have adequate data to measure such outcomes in this paper.

By focusing on housing supply variations in multiple cities in the years following reform passage, our research adds evidence on how housing markets change in jurisdictions overall. We do not specifically investigate the number of new units built or their sales costs (as most aforementioned studies emphasise), but rather evaluate the total units available and their rents, with a focus on those units affordable to low- and moderate-income families.

Data

Land-use reform data

To generate a novel dataset of land-use reforms, we used machine-learning algorithms to analyse newspaper articles from Access World News, a comprehensive database of major newspapers. This approach to data generation builds on other methods for textual data proxies from newspapers for urban phenomena (Ginsberg et al., 2009; Saiz and Simonsohn, 2013). We first assigned newspapers to their 40 respective US metropolitan regions. We prioritised regions with relatively better news coverage and higher population growth, since we hypothesise that those growing regions are more likely to experience affordability challenges, while also having cities that implement reforms that reduce land-use restrictions (to ease affordability) or reforms that increase restrictions (in response to resident concerns about growth).

Next, we identified 21 types of regulations that we hypothesise could affect housing production and availability, a list developed based on our prior research, and constructed a string of search terms relevant to those 21 policies to use in the machine-learning and article identification process. We then used the search string to narrow down the articles, only including those from between 1 January 2000 and 13 January 2019, producing 76,410 articles.

We then relied on a machine-learning algorithm to tag articles. Articles were identified as describing reforms that are more or less restrictive (i.e. producing lower or higher allowed housing density, respectively), tied to a specific neighbourhood or the whole city, occurring in a particular month and year (i.e. when policies were passed by respective city councils), and tied to a specific land-use reform type. We trained a team of four manual taggers with a background in housing and land-use policy to tag 568 randomly selected articles. The machine used this 'training set' to 'learn' to tag the full article set.

While the machine-learning procedure successfully tagged many articles, the algorithm identified many articles merely discussing zoning reforms that did not indicate reform passage. Though we optimised the machine-learning algorithm to eliminate false negatives, this continued lack of reliability necessitated that the team verify all variables within the dataset for each article. A team of land-use experts including the authors of this study and analysts from the Urban Institute with experience researching land-use reforms then hand-coded the data by reading each article and correcting machine-coded data.[1]

Since the dataset was large, we reduced the sample for hand coding, selecting eight metropolitan regions with a higher number of cities tagged as having implemented reforms loosening restrictions. We selected the most frequent reform types to hand code, including those related to accessory dwelling units (ADUs); floor-area ratio (FAR) or housing density; general rezonings (city-initiated zoning map amendments); height limits; lot sizes; minimum setbacks; and mixed residential and non-residential development. Some articles lacked sufficient information to identify the exact reform type; we coded such reforms as general rezonings. We then randomly selected a set of reforms for manual analysis and independently identified other sources confirming 90% of regulations, suggesting that our approach effectively identified reforms. Table 1 lists the various reform types, noting examples of more or less restrictive versions of each.

We identified 180 major reforms during the study period in the eight regions' 1136 cities (Table 2). We intentionally excluded small-scale reforms, since our interest is in municipality-wide impact. We did not include reforms affecting only one or two neighbourhoods, but we did include reforms that, for example, reduced minimum lot sizes on all parcels. Of the reforms identified, 84 increased development restrictions and 96 loosened them. Most reforms related to ADUs and minimum lot sizes, though many also related to height limits and floor-area-ratio requirements.

About one-third of reforms were in the Los Angeles–Long Beach–Anaheim, CA Metropolitan Statistical Area. Because of our choice to specifically examine regions with many reforms loosening restrictions, the ratio of less restrictive reforms to more restrictive reforms should not be interpreted as nationally representative. Our dataset contained reforms passed by local governments between 2005 and 2019 (Figure 1).

Census and address data

We merged reform data with rent levels and population from the 2000 Decennial Census and each five-year American Community Survey (ACS) available at the time of writing (2005–2009 to 2015–2019) from the IPUMS National Historic Geographic Information System (Manson et al., 2019). Because census-designated places (generally equivalent to municipalities) and tracts change geographies over time, we created city-level information from census-tract data with consistent boundaries based on 2010 tracts. We used Brown University's Longitudinal Tract Database to standardise tracts (Logan et al., 2014). Our findings incorporate error due to ACS estimates, but that error is distributed across all studied communities, no matter whether they undertook a reform.

The ACS publishes data on the number of rented housing units by the gross rent paid. These data are in bucket form; each bucket has a range of gross rent for units. To create our measures of affordable housing units, we calculate the rent that would be affordable to people in the relevant

Table 1. Reform types.

Reform type	Description	More Restrictive, i.e. allowing lower housing densities	Less restrictive, i.e. allowing higher housing densities
Accessory Dwelling Units (ADUs)	Secondary housing units on single-family residential lots, such as tiny homes in the backyard ('granny flats') or basement apartments	Raise minimum lot sizes for ADUs, ban ADUs	Lower minimum lot sizes for ADUs, allow ADUs
Floor-Area-Ratio (FAR) or density	Ratio of a building's total floor area to the size of the parcel where it is built	Lower allowed density or FAR	Higher allowed density or FAR
General Rezoning	Land-use reforms with a broad purpose, but with implications for housing density	Lower density or FAR	Higher density or FAR
Height limits	Limitations to building heights	Lower height limit	Higher height limit
Minimum lot sizes	Requirement that every parcel be larger than a minimum square footage	Minimum allowed size increased	Minimum allowed size decreased
Minimum setbacks	Minimum distance for a building from its property line	Minimum setback increased	Minimum setback decreased
Mixed residential and non-residential development	Blending of residential, commercial, cultural, or institutional uses into one space	Reduce use types allowed in a zone, increase specificity on types of allowed uses in mixed-use zones	Lower restrictions on allowed use, lower specificity in allowed-use sub-types

Table 2. Land-use reform summary statistics.

Type of reform	Frequency		
	Total reforms	More restrictive	Less restrictive
ADUs	50	15	35
FAR	34	19	15
General rezoning	22	11	11
Height limits	37	24	13
Minimum lot size	33	14	19
Minimum setbacks	1	1	0
Mixed residential and non–residential development	3	0	3
Metropolitan region	Total reforms	More restrictive	Less restrictive
Boston–Cambridge–Newton, MA–NH	17	11	6
Charlotte–Concord–Gastonia NC–SC	16	7	9
Chicago–Naperville–Elgin, IL–IN–WI	20	11	9
Dallas–Fort Worth–Arlington, TX	15	3	12
Los Angeles–Long Beach–Anaheim, CA	57	28	29
Miami–Fort Lauderdale–West Palm Beach, FL	17	9	8
Philadelphia–Camden–Wilmington, PA–NJ–DE	17	6	11
Portland–Vancouver–Hillsboro, OR–WA	21	9	12
Total	180	84	96

Note: General rezonings refer to land–use reforms that were major, but for which specifics of the type of reform are unclear. We designated regions using Census–defined metropolitan statistical areas (MSAs).
Source: Authors' analysis of land-use reform data.

geography. We then sum the housing units in each rent bucket below the calculated affordable rent. We then approximate the number of units within the bucket that contains the calculated affordable rent by multiplying the number of units in the bucket by the difference between the calculated affordable rent and the minimum value of the bucket over its dollar range. For example, if the bucket between \$80,000 and \$100,000 has 1000 units, and the relevant affordable rent for the calculation is \$90,000, we would count 500 units. Finally, we integrated quarterly US Postal Service (USPS) data on the total number of addresses within each place, provided by the US Department of Housing and Urban Development (HUD) between 2005 and 2018. These data represent the count of units as a housing-supply measure. Our decision to use this as an outcome measure limited our reform universe to cases between 2005 and 2018. USPS address data are available at the tract level, so a geographic transformation from tract to place was necessary to match the datasets. We used a geographic crosswalk to transform the data (Missouri Census Data Center, 2020).

Summary statistics

Cities that institute reforms (more *or* less restrictive) tend to be much more populous than those that never institute reforms (Table 3). We identify similar statistically significant variation with respect to population change, rent levels, and other municipal characteristics. These differences could be exacerbated by bias in our data collection method since smaller cities are less likely to feature robust news coverage.

Cities that institute reforms that increase restrictiveness and those that reduce restrictiveness tend to have an increasing number of

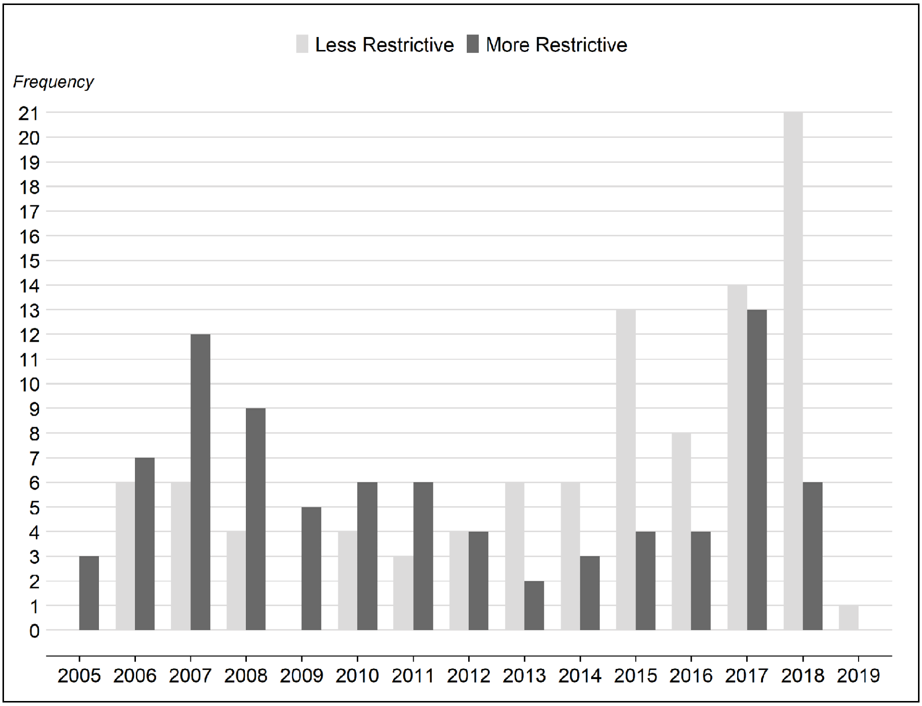

Figure 1. Municipal land-use reforms.
Source: Authors' analysis of land-use reform data.

Table 3. Baseline municipal characteristics by reform status.

Characteristic	Data year	Means		
		Never had a reform	Ever had a more restrictive reform	Ever had a less restrictive reform
Population	2000	19,700	189,585**	212,003**
Population change	2000–2007	1612	8473**	7635**
Addresses	2005	8884	89,385**	96,686**
Change in number of addresses	2005–2006	213	1003**	1230**
Median gross rent	2000	$945	$1122**	$1034**
Change in median gross rent	2000–2007	$67	$100	$89
Aggregate gross rent	2000	$1,846,465	$26,129,489**	$28,536,468**
Change in aggregate gross rent	2000–2007	$739,340	$11,637,144**	$12,151,556**
Per-capita rental units available by income				
Extremely low income (ELI)	2007	0.005	0.008**	0.007
Very low income (VLI)	2007	0.012	0.011	0.012
Low income (LI)	2007	0.038	0.046*	0.043
Median income (MI)	2007	0.015	0.025**	0.023**
Above MI	2007	0.019	0.051**	0.045**
Above 50% AMI	2007	0.033	0.071**	0.084**
Under 50% AMI	2007	0. 056	0.071	0.066

Note: Asterisks indicate significant differences in means between cities that instituted reforms and those that never instituted a reform. $^{**}p \leqslant 0.01$; $^{*}p \leqslant 0.05$.

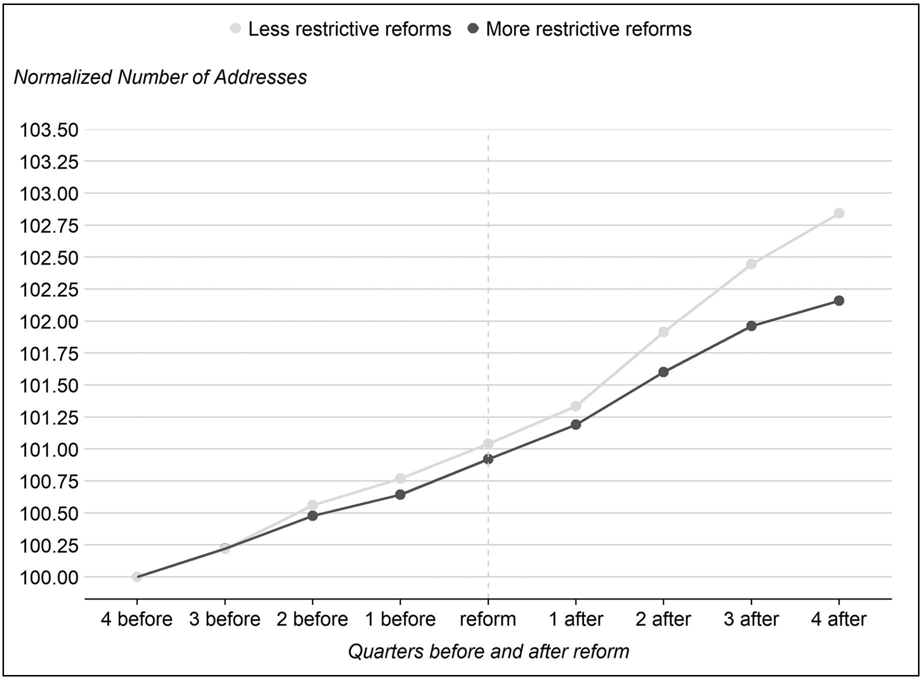

Figure 2. Average number of addresses before and after reforms, normalised.
Source: Authors' analysis of HUD USPS Vacancy Data and land-use reform data.

housing units leading up to a reform, though their trends diverge in the years after (Figure 2). These overall increases in the number of units do not appear in cities that never instituted land-use reforms. This is likely due to reverse causation; cities may be more likely to institute land-use reforms because of observed changes in their housing supply. Therefore, controlling for these pre-trends helps identify the causal impacts of the reforms, since parallel trends prior to the treatment are necessary for a fixed-effects model.

Methods

We estimate a random-trend, fixed-effects model that compares housing-related changes within cities that implemented land-use reforms to changes within cities that did not. Fixed effects allow us to remove unobserved heterogeneity within places and control for national trends (Wooldridge, 2002). We also include random trends, providing each city with its own linear time trend in addition to the separate level effect. This approach reduces the potentially endogenous relationships between municipal interest in passing land-use reforms in response to changing housing conditions in that community, and helps to fulfil the parallel-trends assumption.

We estimate the following equation using multinomial quasi-conditional maximum likelihood estimation (Wooldridge, 1999):

$$\begin{aligned} Y_{it} = {} & \mathit{RestrictiveReformsImplementation}_{it} \\ & + \mathit{LessRestrictiveReformsImplementation}_{it} \\ & + \mathit{RestrictiveReformsPost}_{it} \\ & + \mathit{LessRestrictiveReformsPost}_{it} \\ & + \lambda_i + \theta_t + \varsigma_i t + \varepsilon_{it} \end{aligned}$$

where Y_{it} is a series of outcome measures related to housing supply and costs, including total address count (2005–2018, quarterly), median rents (2000–2017, annually), aggregate gross rents (2000–2017, annually), and the count of rental units affordable to households at different income levels based on national median incomes (2000–2017 yearly).[2] Note that the latter measure is not quantifying the number of available subsidised units, but rather the number of units affordable to people based on their means: units included could thus be subsidised or not. $RestrictiveReforms_{it}$ and $LessRestrictiveReforms_{it}$ are counts of the number of reforms that altered restrictions in city i in year t or any year before (i.e. it is specified as a stock variable).

The model includes city-level fixed effects that capture time-invariant characteristics, λ_i, and fixed effects for each year or quarter (depending on the outcome) to account for trends in the economy or real-estate market, θ_t. Standard errors are clustered at the city level and are robust to heteroskedasticity and arbitrary forms of error correlation within each city.

Variables are split into an *implementation period* comprising the two years before a reform, the year of the reform, and two years after the reform, plus a *post period* (three or more years after). The implementation period controls for anticipation effects that could cause an Ashenfelter dip or spike (e.g. a reform being discussed but not yet having been passed influencing developer behaviours). The post-period allows time for reforms to take effect; knowledge about reforms may take time to spread. The two-year period after reforms assesses short-term impacts and accounts for the five-year averages in the ACS data to ensure that outcome years do not include pre-periods due to averaging. Because of this averaging, we cannot separate anticipatory effects from construction effects, so we do not split this implementation period into two.

The longer-term post period reform impacts identify outcomes more than two years after reforms versus the three years before the reforms. These estimates represent average treatment effects for all years, three or more years after reform passage. This varies between cities, based on when each reform was passed.

We split the treatment variable into reforms increasing restrictions on land use and those reducing restrictions on land use. This is because we believe that their effects are not symmetric: removing restrictions may not have the opposite effects on housing affordability, at least in the short run, to increasing restrictions. This is because loosening land-use restrictions may simply standardise requests for variations from by-right rules that local zoning commissions already systematically approve (Lo et al., 2020), and because in rezoned areas, builders might convert existing lower-cost units into higher-cost ones. The amenity effects resulting from these conversions plus associated neighbourhood retail and public safety improvements may, in turn, increase surrounding housing values (Jacobus, 2016).[3]

As a robustness check, we estimate the models using area median incomes (AMIs) to calculate affordability (rather than the national median incomes in the standard models) to confirm that our results are not sensitive to this calculation. Although we do not prefer this approach, these findings can help confirm our national-data-based findings related to the lower end of rental affordability. We also run falsification tests to explore whether future hypothetical reforms could predict changes in outcome measures. If significant, these results might suggest that endogeneity exists in the model and that results are not reliable.

We acknowledge several limitations in our approach. Because of our reliance on news articles to identify reforms, we may be undercounting changes occurring in some cities. Our control group (cities without reforms) may include some treated, but unmeasured, cities with reforms. This may attenuate our estimates of reform impact, though we expect that most reforms, especially the largest, were covered in the news. There may also be underlying conditions in cities that pass reforms that *cause* them to pass reforms; cities facing affordability problems, for example, may be more likely to loosen construction regulations. But our models' use of city linear trends and controls for regional context aid us in addressing this concern.

Moreover, though our incorporation of fixed effects and random trends aid efforts to achieve identification, we cannot fully assert a causal relationship between reforms and outcomes as there remain potential endogenous relationships for which we cannot account, plus time-varying, unobserved characteristics. For example, cities passing reforms reducing construction restrictions may have simultaneously invested in increased subsidised housing support. And cities experiencing increasing rents may be more likely to implement reforms loosening restrictions, thus violating the exogeneity assumption of the model.

We also face limitations in our ability to link land-use regulations with outcomes. We do not examine specific parcels experiencing zoning changes at the neighbourhood level; we assume that reforms we identified impacted cities overall. We also do not differentiate between relative impacts of different changes, and we do not have the statistical power to assess the varying impacts of reform types, like ADU or height-limit policy. It is likely that reform effects varied based on neighbourhood, which we do not measure. Nor do we measure effects across entire metropolitan areas, which constitute the broader housing market. And it is possible that our method for identifying reforms was incomplete, particularly in smaller cities with a less active press.

Despite these limitations, by providing the first multi-city, multi-reform dataset, we offer new insight into the short- and medium-term impacts of land-use regulatory changes. Our models provide unique information about how different types of changes may be associated with changes in housing production and affordability.

Findings

To identify relationships between land-use reforms and housing supply, we estimate reform impacts on the overall housing supply (measured by address counts), housing costs (median gross rents to represent average rental costs, plus aggregate gross rents to understand how total rents in cities change, which could be affected by both the number of rental units and their individual rents), and the supply of units affordable to households in different income buckets. We include subsidised and non-subsidised housing.

Using a fixed-effects model with city-level random trends, we find that the reforms loosening restrictions were associated with a statistically significant 0.8% increase in the total number of addresses over the medium- to long-term post reform, meaning at least three years after reform (Table 4).[4] These estimates are the average treatment effects for all post-reform years, compared to three years pre-reform, each of which varies by city based on when reforms passed. These results pass a falsification test (section 6). We find no effects on total address counts in the implementation period (meaning two years pre-reform to two years after) for these reforms. Among reforms loosening restrictions, we find no significant effects of reforms during either the implementation period or the post-reform period on rent levels (though the coefficients for median gross rents are negative).

Table 4. Effect of land-use reforms on address count and rents.

		(1) Ln total addresses	(2) Median gross rent	(3) Ln aggregate gross rent
Reforms increasing restrictions	Post Period	0.004 (0.003)	\$49.54* (24.42)	−0.003 (0.026)
	Implementation Period	0.001 (0.002)	\$20.42 (12.87)	0.002 (0.014)
Reforms loosening restrictions	Post Period	0.008** (0.003)	−\$60.52 (36.05)	0.008 (0.031)
	Implementation Period	0.003 (0.002)	−\$23.55 (21.21)	0.008 (0.020)
Quarter/year fixed effects		Yes	Yes	Yes
City-level fixed effects		Yes	Yes	Yes
City specific time trends		Yes	Yes	Yes
Observations		68,634	12,176	12,178
Adjusted *R*-squared		0.785	0.446	0.606

Note: Results for logged outcomes are semi elasticities from a fixed effects model with random trends that includes place and quarter or year fixed effects (quarter for number of addresses, year for rent) and city specific time trends (or random trends). Sample is a quarterly panel of 1136 cities in eight MSAs from 2005 quarter 4 to 2021 quarter 1 for total addresses, and a yearly panel from 2008 to 2017 for median and aggregate gross rent. Robust standard errors in parentheses, clustered at the place level. $^{**}p \leqslant 0.01$; $^{*}p \leqslant 0.05$.

Reforms increasing land-use restrictiveness, such as those increasing minimum lot sizes, were associated with a significant, \$50 increase in median rents in the post-reform period, but not in the implementation period. These results, interestingly, are somewhat symmetrical to those related to the reforms loosening restrictions. This finding also passes a falsification test. We found no effect of increasing land-use restrictions on the address count in either period.

We also estimate the effect of reforms on the price distribution of rental units. We developed a set of cut points for affordability (assuming families are to pay no more than 30% of income to rent), by year, for families at or below 30% of the median income (extremely low income, or ELI); 30–50% of median income (very low income, VLI); 50–80% of median income (low income, LI); 80–100% of median income (middle income, MI); and above.[5]

We find statistically significant increases in housing supply at the top end of the rent distribution (i.e. for rental units affordable to households making more than the national median income) in both the implementation period and the post period for reforms loosening restrictions, meaning those allowing for increased density (Table 5). After such reform passage, we find an increase in units affordable to families with incomes above the national median of 43% in the short run and 63% in the medium to long run. These estimates are large, but note that based on the 95% confidence interval, these estimates could range from 15% to 70% for the short-run implementation period and from 15% to 112% at least three years post-reform. We can rule out that the estimate is zero, however. These results also pass a falsification test.

These results suggest, perhaps unsurprisingly, that allowing additional housing construction compared to the baseline attracts investment in 'market-rate' units, which are generally not affordable to low- or moderate-income households. While we do not find statistically significant evidence that existing units become less expensive in the implementation period or post period, the estimates for every affordability

Table 5. Effect of land-use reforms on address count by affordability level.

		(1) Ln ELI	(2) Ln VLI	(3) Ln LI	(4) Ln MI	(5) Ln above MI
Reforms increasing restrictions	Post Period	−0.365	−1.523	−0.063	−0.241*	−0.335
		(1.263)	(2.188)	(0.053)	(0.111)	(0.216)
	Implementation Period	0.434	−0.527	−0.026	−0.050	0.000
		(0.577)	(0.353)	(0.029)	(0.061)	(0.162)
Reforms loosening restrictions	Post Period	2.047	1.264	0.122	0.051	0.632*
		(3.303)	(1.393)	(0.083)	(0.126)	(0.248)
	Implementation Period	1.393	0.454	0.063	0.078	0.430**
		(1.773)	(0.646)	(0.053)	(0.072)	(0.140)
Quarter/year fixed effects		Yes	Yes	Yes	Yes	Yes
City-level fixed effects		Yes	Yes	Yes	Yes	Yes
City specific time trends		Yes	Yes	Yes	Yes	Yes
Observations		12,177	12,175	12,177	12,172	12,177
Adjusted R-squared		0.276	0.221	0.126	0.132	0.178

Note: Results for logged outcomes are semi elasticities from a fixed-effects model with random trends that includes place and year fixed effects and city-specific time trends. Sample is a yearly panel from 2008 to 2017. Robust standard errors in parentheses, clustered at the place level. $^{**}p \leqslant 0.01$; $^{*}p \leqslant 0.05$.

category for reforms that loosened restrictions are positive, and the effects on the number of rental units that are affordable to extremely-low income and very low-income households are much larger in magnitude than the effects on higher-cost units. The standard errors on the ELI and VLI unit estimates are much larger, likely because the base number of units in those affordability ranges are small to begin with in many of the cities in our study. Therefore, we cannot disprove that the impacts are positive and equivalent across the different affordability categories.

For reforms that tighten land-use restrictions and reduce allowed density, we find a 24% decline in housing units affordable for middle-income families in the post period at least three years after reforms (regressions show negative coefficients for unit counts across all income ranges for these types of reforms, though these changes are not statistically significant). However, this finding fails a falsification test – and has no effect for other income levels – so we cannot rule out that this is a continuation of that pre-trend.

Robustness checks

Using area median income to define affordability

We run additional models to ensure our results are robust to misspecification. Because national median incomes may not capture local income differentials and costs of living, we run the analysis using an outcome measure for affordable units based on AMI. In other words, we consider the impact of reforms on the count of rental units affordable to people in each metropolitan region based on *that region's* median income.

Using AMIs to calculate affordability is not our preferred specification for two reasons. First, there is value in having a standard nationwide affordability definition, since some areas may have such high median incomes that what is considered affordable there based on AMI may not be affordable to people earning minimum wage, or even for teachers or police officers. For example, since 2019 San Francisco-region median household incomes were \$121,795 in 2019, using that AMI we might claim that a monthly rent of $2436 is affordable for a low-income family. The average salary for a teacher in San Francisco is $62,123, however, meaning that the rent calculated to be 'affordable' there based on AMI is almost twice what a single teacher can afford, or $2436 compared with $1242 (local median incomes may be more appropriate for two-earner households). We therefore prefer to use national medians to calculate affordability since these rents are more universally affordable to low-wage households.

Second, the highest rent bucket in the ACS is lower than the cut-off point for what is affordable to households above middle income in our sample's more expensive cities, so we can only observe the number of rental units affordable to households that are ELI, VLI, and above VLI. For example, in Boston, affordable rent for someone at 100% of AMI would be $2696 per month, but the highest rent bucket in Census data is $2000 or more. We would therefore not be able to ascertain how many units are affordable for that income level.

Nevertheless, we re-run the models using AMI as a robustness check on our primary results. We find evidence that reforms *increasing* restrictions reduce the availability of units affordable for households with low incomes or above on average – but with no statistical significance. We also find no statistically significant impacts for reforms that *loosened* restrictions, though this result may reflect limited data availability.[6]

Long-run effects

We also trace out the full adjustment path for the reforms, as per Wolfers (2006), allowing us to monitor changes year-by-year. This helps to confirm that the inclusion of place-specific time trends created using effects post-reform is not biasing our results. It also helps to identify effects over time of reforms that are more or less restrictive.

Due to our dataset having a relatively short panel, some of the results in our average treatment effects are not significant in this model, but the results generally confirm our main findings in terms of directionality of coefficients (Table 6). The fully lagged model shows that less restrictive reforms reduce median gross rents the first, fourth, and fifth years after reform passage. It also shows that more restrictive reforms reduce the supply of rental units affordable to people at and above middle income the fourth and fifth years after such a reform.

Falsification tests

Finally, we run falsification tests to examine whether impacts are detectable in cities *prior* to actual reforms, which, if demonstrated, might suggest that the results above are the product of endogeneity and selection bias rather than the reforms themselves. To do so, we include a variable for three years (or quarters, for USPS data) prior to each reform in the models to see whether that coefficient is statistically significant (in essence, we are testing for the effects of a hypothetical reform that never occurred). We use three years prior since many of our outcomes are based on five-year ACS estimates, so one and two years prior to the reform could produce significant outcomes due to averaging. Additionally, three years prior to reform is the year before our implementation period control variable.

We find no evidence of endogeneity in terms of housing supply. These results confirm our Table 4 finding showing an increase in address count following reforms loosening housing regulations. But there may be some endogeneity in terms of rent prices leading up to a reform. Specifically, cities that instituted reforms that loosened restrictions experienced

Table 6. Effects of future land-use reforms, year-by-year.

Period post-reform	(1) Ln Total Addresses	(2) Median Gross Rent	(3) Ln Aggregate Gross Rent	(1) Ln ELI	(2) Ln VLI	(3) Ln LI	(4) Ln MI	(5) Ln above MI
Reforms increasing restrictions								
First year	0.001	−25.520	0.009	0.402	1.576	−0.045	0.116	0.077
	(0.001)	(21.050)	(0.012)	(1.872)	(1.549)	(0.033)	(0.087)	(0.136)
Second year	−0.000	−28.440	−0.007	−2.585	0.729	−0.009	0.030	−0.147
	(0.001)	(21.780)	(0.020)	(2.092)	(1.500)	(0.035)	(0.083)	(0.159)
Third year	−0.001	−23.620	0.010	−3.541	0.152	−0.016	−0.006	−0.252
	(0.001)	(22.320)	(0.018)	(2.303)	(1.871)	(0.050)	(0.097)	(0.154)
Fourth year	−0.001	−20.360	0.014	−3.215	0.373	−0.039	−0.073	−0.374*
	(0.002)	(21.010)	(0.015)	(2.250)	(1.668)	(0.051)	(0.101)	(0.169)
Fifth year	−0.001	17.290	0.014	−3.256	0.156	−0.032	−0.136	−0.429*
	(0.002)	(32.756)	(0.018)	(2.233)	(1.899)	(0.064)	(0.101)	(0.188)
Sixth year	0.000	16.360	0.010	−2.199	−1.184	−0.027	−0.127	−0.235
onwards	(0.002)	(20.273)	(0.019)	(1.952)	(1.339)	(0.045)	(0.095)	(0.165)
Reforms loosening restrictions								
First year	0.000	−64.390*	0.001	1.854	2.755	0.076	0.027	0.202
	(0.002)	(27.770)	(0.027)	(3.903)	(2.814)	(0.057)	(0.082)	(0.201)
Second year	0.001	−26.940	0.020	2.853	2.855	0.050	0.130	0.431
	(0.002)	(22.270)	(0.032)	(3.506)	(2.851)	(0.053)	(0.103)	(0.262)
Third year	0.001	−38.440	0.024	2.356	4.703	0.044	0.197	0.323
	(0.002)	(22.747)	(0.043)	(3.802)	(3.814)	(0.058)	(0.121)	(0.275)
Fourth year	0.000	−62.270**	−0.002	4.916	4.079	0.092	0.149	0.170
	(0.002)	(23.862)	(0.035)	(3.683)	(3.860)	(0.062)	(0.139)	(0.251)
Fifth year	0.001	−61.660*	−0.000	2.423	3.146	0.061	0.030	0.241
	(0.002)	(27.878)	(0.045)	(4.680)	(2.906)	(0.055)	(0.114)	(0.273)
Sixth year	0.001	−46.540	−0.012	−1.403	2.319	0.049	0.059	0.240
onwards	(0.001)	(26.975)	(0.043)	(3.223)	(2.268)	(0.040)	(0.132)	(0.274)
Observations	68,630	11,867	11,869	11,869	11,867	11,869	11,865	11,869
Adjusted R^2	0.785	0.442	0.603	0.274	0.221	0.125	0.133	0.166

Note: Results for logged outcomes are semi elasticities from a fixed effects model with random trends that includes place and quarter or year fixed effects (quarter for number of addresses, year for rent) and city specific time trends (or random trends). Sample is a quarterly panel of 1136 cities in eight MSAs from 2005 quarter 4 to 2021 quarter 1 for total addresses, and a yearly panel from 2008 to 2017 for median and aggregate gross rent. Robust standard errors in parentheses, clustered at the place level. $^{**}p \leqslant 0.01$; $^{*}p \leqslant 0.05$. All models include quarter/year fixed effects, city fixed effects, and city-specific trends.

decreases in aggregate gross rents prior to instituting a reform (Table 7, model 3). We also find some evidence for a pre-existing decline in middle and above-middle-income units in cities that increased restrictions (models 7–8). That said, we find no evidence for pre-trends pointing towards increased above-middle-income units in cities that loosened restrictions, confirming Table 5.

Conclusion

This analysis is the first cross-city, panel analysis of the effect of land-use reforms on the supply of affordable housing. We offer preliminary evidence of the potential for using machine-learning to identify where zoning changes are occurring. We find that land-use reforms that reduce restrictions to

Table 7. Falsification test: effects of future land-use reforms.

	(1) Ln Total addresses	(2) Median gross rent	(3) Ln Aggregate gross rent	(4) Ln ELI	(5) Ln VLI	(6) Ln LI	(7) Ln MI	(8) Ln above MI
Future reforms that increase restrictions	−0.000	−$29.97	−0.002	0.834	2.300	−0.010	−0.261*	−0.361*
	(0.003)	(15.89)	(0.019)	(1.542)	(1.441)	(0.059)	(0.105)	(0.174)
Future reforms that loosen restrictions	0.000	$15.20	−0.034*	−3.746	−1.990	−0.031	0.003	−0.030
	(0.003)	(17.90)	(0.017)	(1.996)	(1.908)	(0.046)	(0.200)	(0.252)
Observations	68,634	12,176	12,178	12,177	12,175	12,177	12,172	12,177
Adjusted *R*-squared	0.785	0.446	0.606	0.277	0.221	0.126	0.132	0.178

Note: Results for logged outcomes are semi elasticities from a fixed-effects model with random trends that includes place and quarter or year fixed effects (quarter for number of addresses, year for rent) and city specific time trends (or random trends). Sample is a quarterly panel of 1136 cities in eight MSAs from 2005 quarter 4 to 2021 quarter 1 for total addresses, and a yearly panel from 2008 to 2017 for median and aggregate gross rent. Robust standard errors in parentheses, clustered at the place level. *$p \leq 0.05$. All models include quarter/year fixed effects, city fixed effects, and city-specific trends.

increase allowed density lead to a 0.8% increase in housing supply, on average, in the cities we study. However, we find no statistically significant evidence that these reforms lead to an increase in affordable rental units within three to nine years of reform passage. We do find that such reforms are associated with an increase in units affordable for above-middle-income households, and that effects on units affordable to those with extremely low incomes and very low incomes are positive but with large standard errors, likely because of the small number of units affordable at these levels at baseline. Therefore, we do not have enough data to conclude that the impacts are significant.

We theorise that these outcomes may be produced by amenity affects occurring when a reform takes place; new buildings increase housing supply, but not only are new units likely to be more expensive than existing units, they may also bring amenities that improve the attractiveness of a city's housing market overall. This could outweigh the effects of the supply increase on reducing prices for more affordable units – at least in the jurisdiction where zoning reforms occur. In other words, certain zoning reforms may induce more construction, but rather than opening up existing units in the surrounding area for lower-income families, existing housing units maintain relatively stable rents due to increased demand. Even so, at the metropolitan scale and in the longer run, we expect that more construction reduces costs.

These results indicate that policies targeting affordable housing may need to accompany measures designed specifically to increase supply. Direct development or preservation of affordable units through nonprofit housing developers may be more successful at increasing the supply of low-cost units in the short run than regulatory reform alone. If supply grows at pace with household growth, then income or rent supplements could also ease affordability problems for low-income households.

Conversely, we find that reforms that increase restrictions on housing construction are associated with an increase in median rents over the longer term, combined with a decline in units affordable for middle-income households. This indicates that *tightening* restrictions on housing construction is, as predicted by economic theory, associated with less housing supply and less affordability. These results are not without their limitations. It is likely that heterogeneity exists among reforms; some reform types probably work better than others in terms of increasing housing supply and affordability, and the marginal impacts of loosening restrictions may be relatively small in cities surrounded by other municipalities that were also relaxing regulations. We do not have enough power in our datasets to test for heterogeneous effects. Our methods also do not entirely resolve endogeneity concerns that are endemic to all studies on housing regulations, supply, and cost. Moreover, while we have successfully measured associations between reforms and the housing market in the years following reforms, we acknowledge that the effects over a much longer term, such as a decade after, may vary significantly. And it is possible that reform impacts occurred across metropolitan areas as a whole rather than within the jurisdictions we studied, but we did not have the data to measure those outcomes. Finally, since we selected our metropolitan regions to be those that were most likely to include cities that instituted reforms, and because our data collection method does not guarantee that we identified all reforms, it is likely that our estimated effects are a lower bound on the true impact since some of our control cities also likely had reforms that were not reported.

Future studies should expand the types of policies examined to include those that directly require or incentivise affordability and should explore effects over a larger set of cities. The use of additional datasets, such as building permit information, could further inform this research. And detailed investigations of the *metropolitan*-scale effects of the zoning reforms introduced by individual jurisdictions could produce vital new evidence on the impacts of this type of public policy. Continued advancements in machine learning could help researchers examine policies such as these at a larger scale.

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Notes

1. For additional detail on the machine-learning methods used to create this dataset, see: Zheng V (2020) How Urban Piloted Data Science Techniques to Collect Land-Use Reform Data. Data@Urban Blog, available at: https://urban-institute.medium.com/how-urban-piloted-data-science-techniques-to-collect-land-use-reform-data-475409903b88
2. We put dependent variables into natural logged form when both the variables themselves and the residuals from their regressions show a logged distribution. Once we log the variables, the residuals produce a normal distribution. The measure of units affordable to households at different income levels includes subsidised and non-subsidised units.
3. Results are similar when the treatment variable is run as a single reform variable, but

they are attenuated and less informative since we cannot determine whether the effects are symmetric. Authors can provide these results upon request.

4. We identify similar results when we run the regression with the treatment variable run as a single reform variable. This implies that the results are somewhat symmetrical (meaning loosening restrictions may have the opposite effect as increasing them), though more research is necessary to confirm this finding.
5. We also test for effects on number of rental units by affordability at the regional level using Area Median Income (AMI) as a robustness check later. Authors will provide cut-point data on request.
6. The authors can provide tabular results on request.

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