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The effects of an "urban village" planning and zoning strategy in San Jose, California

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ABSTRACT

Allowing mixed-use and higher-density development is a common municipal policy response to pressing urban challenges, including housing affordability and climate change. Yet, comparatively little is known about the effects of density-enabling plans and policies. We study how a major 2011 planning initiative, the designation of "urban villages," affected real estate development in San Jose, California. We track several outcome measures – permits for residential and commercial development, large development projects, parcel transactions, and parcel assessed values – before and after the urban village initiative. We find the initiative's effects to be quite limited. The estimated treatment effects are generally not distinguishable from zero across specifications that vary by parcel land use, treatment period, and identification strategy. Potential explanations include a lack of actual zoning changes; urban village requirements that make development more complicated; and a mismatch between the development types envisioned in municipal plans and real estate market conditions.

1. Introduction

Some growing U.S. cities are responding to pressing challenges — including high housing prices and climate change — by "upzoning" commercial and residential parcels. Upzoning may be defined as an increase in allowable residential density or an increase in allowable development intensity more generally. Cities that enable increases in housing supply on the extensive margin are expected to have lower overall housing prices, but less is known about how specific planning and zoning interventions might affect development outcomes on the intensive margin (sometimes referred to as "densification").

We study the effects of the San Jose, California municipal "urban village" planning and zoning strategy on property values and real estate development outcomes. We use several measures of development-related outcomes: transactions, changes in assessed property valuations, and permits for residential and commercial development. Our basic estimation strategy is difference-in-difference, comparing parcels included in urban villages with other, comparable, parcels. We find, as we describe further below, that San Jose's strategy had little or no effect on any of these outcomes.

San Jose is a useful case study because of the city's size, economic growth, and high housing prices. San Jose is the largest city in Silicon Valley — and the 10th largest city in the United States — with a population of over one million. Over the past two decades, San Jose's population has grown at a rate similar to the country as a whole, but its economic growth has been much more rapid. Between the trough of the Great Recession in 2009 and 2017, metro San Jose's GDP grew by 84%, compared with 35% nationally.[1] Housing prices have also escalated dramatically. Between 2000 and 2019, average home prices in San Jose rose by about 185%, compared with 86% nationally, according to Zillow estimates.[2] In mid-2019, these estimates showed the median home value in San Jose to be about $1 million. High — and rising — prices and rental rates have given rise to a widespread perception of a housing affordability crisis in San Jose and the San Francisco Bay Area generally.

The City of San Jose's *Envision San José 2040 General Plan*, approved in 2011, sought to connect housing and employment land uses, better use existing infrastructure, and encourage non-auto transportation. A key component of the general plan was to direct growth into 68 areas by designating them as "urban villages." Many of these areas were commercially zoned and proximate to public transportation corridors

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1 Total Gross Domestic Product for San Jose-Sunnyvale-Santa Clara, CA (MSA) and for the United States, annual, retrieved from FRED, Federal Reserve Bank of St. Louis; https://fred.stlouisfed.org/series/GDPA and https://fred.stlouisfed.org/series/NGMP41940, November 4, 2019.

2 Zillow estimates downloaded from https://www.zillow.com/research/data/ July 18, 2019.

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and hubs. The general plan itself was implemented by more detailed plans for individual urban villages, and these plans were intended to enable higher development densities and a mix of land uses.

We refer to these urban village plans as "upzonings" because although the plans did not themselves automatically change existing zoning, the planning process was predicated on subsequent approval of rezonings or development entitlements. In fact, the number of actual upzonings during our study period in urban villages was quite small.[3] Between 2012 and 2020, there were 44 upzonings spread across the city's urban villages, with no more than three in any urban village. Most were initiated by property owners and were intended to ensure that existing uses and zoning designations were in conformance. An example of a property-owner initiated upzoning is a proposal to redevelop a one-story restaurant into a five-story mixed-use office development in the Valley Fair/Santana Row Urban Village. More common than upzonings in San Jose were increases in allowable density through planned developments (PDs), which are a type of project-specific approval that enables more flexibility (Gabbe, 2019).

The anticipated effects of the urban village designation on development, rents, and property values are ambiguous and thus empirical questions.[4] To the extent that an urban village designation would enable higher densities and perhaps a loosening of other restrictions, it could stimulate an increase in the local supply of housing and thus relieve upward pressure on housing rents and prices. On the other hand, if the urban village plans impose more controls or fees on aspects of development, they could impede housing development.[5] Additionally, anticipated amenities associated with urban villages could increase local demand and drive up prices. With respect to the volume of real estate transactions, urban village effects are also ambiguous: transactions might increase if prior owners sell to developers, but might be muted due to the increased option value of banking undeveloped land (Cunningham, 2006, 2007).

We estimate the effect of urban village upzoning by comparing a variety of groups of parcels in the periods before and after 2011. These strategies include: (1) comparing urban villages slated for approval and development sooner with those slated later (that is, comparing different city-designated urban village "horizons"); (2) comparing urban village parcels to "synthetic villages"– clusters of contiguous parcels zoned for commercial and multi-family use that are grouped in areas similar to the urban villages; (3) comparing urban villages with approved plans to urban villages without approved plans; and (4) comparing urban villages to areas with a type of adopted plan known as a "specific plan." We focus on parcels zoned for multi-family and commercial use. Few single family homes were included in the urban village areas, and the urban village planning strategy was not intended to upzone single-family residential designations.

We estimate the average change in four outcome indicators: (1) construction permits, (2) major projects, (3) transactions, and (4) assessor valuations. We presume that the intent of the urban village strategy was that parcels so-designated would experience more building permits, more major projects, more transactions, and larger increases in assessor valuations (valuations rise due to sales). Our results show that the urban village strategy had no sizable effects over the 2011–2019 period in terms of these four outcomes. That is, the urban village policies did not accelerate development, increase transactions, or lead to increases in assessed values within the designated areas.

Regulatory and market factors may have impeded the city's urban village strategy implementation. Regulatory barriers may have resulted from additional project requirements in urban villages and delays in the city's approval of urban village plans themselves. Planned development types envisioned in many urban village plans may have been unprofitable from a developer's perspective.

Methodologically, much research on urban development ignores the complex changes in composition of urban parcels as developers split and consolidate parcels. Data analysis on development outcomes over time have to consistently track the experience of parcels. Depending on the method used, transactions, construction permits, and changes in assessed value must be attributed to the consistent parcels. We implement three different aggregation methods to generate consistent parcels over time: using 2005 parcels as "base" parcels, using 2019 parcels as "base" parcels, and using the smallest consistent parcel group as the consistent parcel.

The paper proceeds as follows. Section 2 reviews the literature on zoning and housing prices and availability. Section 3 summarizes the important aspects of San Jose's urban village strategy. Section 4 describes the data we use in our quantitative analysis, the consistent parcel aggregation methods, and the difference-in-difference identification approach taken to estimate the impact of the changes in zoning. Section 5 presents the results. Section 6 suggests further areas of research and offers some concluding comments.

2. Planning, zoning, and housing markets

Recent scholarly debates have addressed the effect of restrictive zoning on housing prices and overall economic growth (Angotti, 2017; Erdmann et al., 2019; Hilber and Vermeulen, 2015; Hirt, 2015; Ihlanfeldt, 2007; Jackson, 2016). An influential argument is that housing supply constraints — including restrictive single-family zoning, reluctance by cities to rezone commercial corridors for housing development, fees, and unpredictable development approval processes — are major causes of rising prices for housing, and those rising prices are inhibiting aggregate economic growth by slowing movement of the labor force away from low opportunity cities towards high opportunity cities. High housing prices in dynamic urban centers dampen the growth potential of such centers, lowering aggregate growth and more importantly opportunities for individuals to match talents and investments to places with high demand (Hsieh and Moretti, 2019).

On the other side of the debate are scholars who find that housing development and housing prices are much less sensitive to zoning changes than supposed. Some argue that changes in patterns of demand, for example through growing inequality in the distribution of earnings, have been responsible for rising prices and greater displacement (Rodríguez-Pose and Storper, 2019). Others focus on the tight housing construction sector, and possible oligopoly strategies of developers, that limit housing supply responses (Coiacetto, 2009; Cosman and Quintero, 2019; Wissoker, 2016). Metcalf (2018) suggests that because the demand effects are so large, and the supply response so limited, publicly provided social housing, rather than just changing zoning, is also necessary to improve housing access, especially for low- and moderate-income residents.

"Upzoning" generally refers to zoning changes and planning overlays that allow higher development densities. Some definitions have focused on allowable residential densities (Been et al., 2014; Gabbe, 2018), while other broader definitions include development intensity more generally (Cullingworth, 1993; Fischel, 1987; Whittemore, 2017). Common forms

[3] The City of San Jose provided, upon request, a list of conventional rezonings in urban villages.

[4] Some informed commentators referred to the urban villages as a major component of the General Plan. Leah Toeniskoetter, San Jose director of SPUR, a major public policy think tank, observed: "The big idea in the plan is to create urban villages, specific areas that will provide active, walkable, bicycle-friendly, transit-oriented, mixed-use urban settings for new housing and job growth … In a sense, the new San Jose General Plan follows the current convention of American planning, protecting most of the city from change, while designating a smaller number of sites for intensive development. But you get a sense of the enormous scale of San Jose's ambition from the number — 70! — of designated urban villages." (Toeniskoetter, 2011).

[5] For example, a recent study (Valley Transit Authority, 2019, p. 11) noted for an area near one planned rail transit station that the "… potential for residential development in the station area may be limited by San José's Urban Village policy, which restricts the location and timing of residential development."

of upzoning under the former definition include relaxing commercial zoning to allow residential and mixed-use development and changing single-family home zoning to allow multi-family residences. Upzonings may be initiated by property owners or municipal leaders; in San Jose, upzonings have been rare and mostly initiated by property owners (Gabbe, 2019).

There are several reasons why it is empirically challenging to identify the causal effects of upzonings or other land use policies on housing market outcomes. The immediate obstacle to estimating causal effects is that zoning changes are likely to be endogenous, responding to perceptions by city officials of the likelihood of development in particular neighborhoods of a city (Ihlanfeldt, 2007; Quigley and Rosenthal, 2005). Zoning is determined at local levels, with small numbers of actors having significant influence over local zoning decisions. City officials, developers, and established residents anticipate changes to neighborhoods and influence the zoning decision-making processes according to those expectations (Fischel, 2005; Molotch, 1976). A second estimation problem is that zoning changes may spill over from targeted zones and have effects on other zones (Pollakowski and Wachter, 1990). To the extent that local housing markets are connected rather than segmented, such general equilibrium effects may be significant. Finally, the impacts of zoning changes are likely to be heterogeneous and vary from neighborhood to neighborhood.

Perhaps because of the significant challenges, there have been few well-identified studies of the impacts of upzoning at the parcel scale. Difference-in-difference estimators have been increasingly adopted for evaluation of the impact of policy changes that were likely to have affected real estate markets. Sage, Langen, and Van de Minne (2019) used difference-in-difference to estimate the magnitude of the short-term effects of the Opportunity Zone designation in the 2017 tax reform. Neumark and Kolko (2010) used difference-in-difference to estimate the effects of California's enterprise zone designation. They used two control groups: a 1 km "ring" around the enterprise zone, and areas that were added to enterprise zones at different times. Freemark (2019) estimated the effect of Chicago's upzoning of certain parcels around mass transit stations. In 2013, Chicago decided to upzone certain parcels within 600 or 1200 feet of rail transit stations and in 2015 expanded the range to 0.25 or 0.5 miles. Because the boundary of the upzoning was basically random relative to parcel pre-existing development, and parcels on either side of the boundary were similar, Freemark compared parcels inside the boundaries with those just outside the boundaries. He found that upzoning increased property values, but had no impact on the number of newly permitted dwellings, in the five year period after the zoning change.

Some researchers have used observational data of differences between cities or metropolitan areas and found (1) negative correlations between indices of restrictive land use regulations and measures of housing production and (2) positive correlations between regulatory restrictiveness and housing prices (Glaeser and Gyourko, 2018; Glaeser et al., 2005; Ihlanfeldt, 2007; Jackson, 2016; Pendall, 2000; Quigley and Raphael, 2005). These observational studies do not adequately address causality: for example, it may be that homeowners anticipating higher valuations for their homes seek to lock-in their appreciation by restricting potential new supply. The regulations are then partly a consequence of appreciating home prices. Additionally, geographic features of cities, such as steep slopes and water bodies, may be correlated with restrictive regulations, housing production, and prices (Saiz, 2010).

Another approach taken in the literature is to compare the price of housing with an estimated marginal cost of housing, or marginal cost of additional housing units in multi-unit buildings. Many of these studies find large differences across jurisdictions and attribute these differences to the impact of zoning (Kendall and Tulip 2018; Glaeser and Gyourko 2018). More recent analysis has taken into account the general equilibrium spillover effects of variation in regulation in different parts of a city (Kok et al., 2014; Lin and Wachter, 2019).

Beyond zoning changes, a closely related literature points to several factors that may affect housing prices near public transit. Rail stations may be associated with changing housing and condominium prices because of accessibility benefits and local amenities, but proximity may also act as a disamenity – with noise and congestion – dampening prices closest to a station (Atkinson-Palombo, 2010; Bowes and Ihlanfeldt, 2001; Kahn, 2007; Matthews and Turnbull, 2007; Duncan, 2011). Station area design seems to be important, as walkable rail station neighborhoods are associated with higher housing prices than auto-oriented station neighborhoods.

This literature suggests that specificities of local neighborhoods may be quite important in property development decisions. Atkinson-Palombo (2010) is particularly relevant because the author uses hedonic models to parse out the separate effects of transit-related accessibility and overlay zoning on the price of single-family units and condominiums. Transit-oriented development (TOD) overlay zoning, which allowed higher density development, had different effects depending on housing type and neighborhood context. For single-family housing, the TOD overlay zone resulted in no price differences in mixed-use neighborhoods and 12% lower prices in traditional residential neighborhoods. For condominiums, the TOD overlay resulted in a 37% price premium in mixed-use neighborhoods and an 11% price decrease in traditional residential neighborhoods.

Development patterns may affect tax revenue and the cost of public service provision, and municipal land use decisions may be influenced by the fiscal impacts of new development. The direct mechanisms connecting land use and public finance are through revenues (e.g. property taxes, sales taxes) and expenditures (e.g. cost to provide public services, infrastructure costs) (Paulsen, 2014). Planners and policymakers commonly consider the fiscal impacts of new development in decision making. Miscynski (1986) coined the term "fiscalization" of land use to describe the idea that local land use decisions in California were often driven by a desire to increase a municipality's tax base. For example, cities might be driven to compete for sales tax generators such as automobile dealerships or big box stores (Lewis, 2001; Wassmer, 2002).

3. San Jose and its urban villages

In some ways San Jose, and the Bay Area broadly, are extreme cases in planning and housing development. Of the 50 largest cities in the U.S., San Jose had the highest median income and median rents, and the second highest housing values — after San Francisco — in 2018 (U.S. Census Bureau, 2018). Meanwhile, San Jose has suffered from major fiscal problems, experiencing an "acute fiscal strain" during the dot-com bust and the Great Recession, and lacks the resources to address deferred maintenance and invest in the future (Szambelan et al., 2016, p. 11). While the city has engaged in planning, it has not updated its zoning to be consistent with these plans due to the city's financial situation, and relies on its general plan to make development decisions (Karlinsky et al., 2017; Liccardo, 2017).

But, in other aspects, San Jose is like many other American cities. San Jose was developed largely post-World War II and aspires to reduce its associated dependency on the automobile (City of San Jose, 2011). The city favored commercial and industrial development over new housing for fiscal reasons, despite some indications that high density housing has net positive fiscal impacts (City of San Jose, 2011; Svensson, 2015). One of the biggest barriers to new housing construction is high construction costs, which according to some estimates are $81 per square foot higher in the Bay Area, holding project characteristics constant, than the California average (Raetz et al., 2020; Reid, 2020). Other barriers to developing more market-rate and affordable housing in San Jose and the Bay Area possibly include unpredictable and lengthy approval timelines and the complexity of financing affordable housing developments (Karlinsky et al., 2017; Reid, 2020).

San Jose's approach to planning and zoning is guided by local and state requirements, the major components of which are general plans, zoning codes, subdivision regulations, and specific plans. California has long required every city to adopt a comprehensive plan, known in the state as a general plan (State of California, 2017). Each general plan includes a set of required elements, but cities have considerable flexibility with the content of their plans. General plans are implemented by local land use regulations, which are mainly in zoning codes and subdivision regulations. San Jose's land use regulations stipulate building heights, densities, parking, landscaping, and affordable housing requirements, among other things.

The state of California requires consistency between most jurisdictions' general plans and zoning codes; an exception is for charter cities with fewer than 2 million residents, of which San Jose qualifies (State of California, 2017). Beyond state requirements, California cities have considerable latitude to plan and zone. Cities may approve some developments as planned developments, which are site-specific planning and development approvals that supersede the underlying zoning designations (Meck et al., 2000). California also has had a unique type of hybrid long-range plan and land use regulatory document known as a specific plan, of which San Jose has adopted ten (City of San Jose, n.d.; State of California, 2001).

San Jose has embraced the idea of creating "urban villages." San Jose in 2011 identified more than 60 areas to be designated as urban villages, which are described as "active, walkable, bicycle-friendly, transit-oriented, mixed-use urban settings for new housing and job growth […]" (City of San Jose, 2011, p. 18). The urban village strategy was one of the city's 12 major strategies for managing city growth, per the general plan *Envision San José 2040*, adopted in 2011 (City of San Jose, 2011). The goal was to enable more urban-style development in what was a largely a lower density suburban city. Each plan would enable conversion of commercial-only parcels to mixed-use parcels. After community engagement, the city council would approve an urban village plan for each designated area. By mid-2019, eight years after the general plan was adopted, 13 urban village plans had been approved.

Urban villages and transit villages are established concepts in contemporary urban planning, although definitions and implementation have varied by city (Jabareen, 2006). Some western U.S. cities — including San Diego and Seattle — have introduced urban village typologies and enabled urban villages by increasing allowable densities, encouraging mixed-use development, and reducing off-street parking requirements. San Diego's "City of Villages" strategy has been implemented through the city's community plans, and the city has also adopted several zoning overlays, including an urban village overlay (City of San Diego, 1997, 2015). Seattle's approach to creating "urban centers" and "urban villages" has included relatively standard regulations across centers and villages, including minimum density targets and off-street parking standards (City of Seattle, 2017; Gabbe et al., 2020; Krause, 2014). Other cities, such as Portland (OR), have promoted similar concepts using different terms, such as "corridors" and "centers" (City of Portland, 2020). All of these cities' plans have identified priority areas for urban growth based on existing residential and employment densities; proximity to public transit; and future growth capacity (City of Portland, 2020; City of San Diego, 2015; City of Seattle, 2017).

San Jose defined four categories of urban villages. *Commercial Corridor and Center Urban Villages* were typically along major arterials or at intersections of two arterials. *Local Transit Urban Villages* were located near light rail and bus rapid transit (BRT) stations and would include a mix of uses, but lower densities than the regional transit urban villages. *Neighborhood Urban Villages* were small-scale neighborhood-serving commercial developments with limited new housing (City of San Jose, 2011). *Regional Transit Urban Villages* were located near Bay Area Rapid Transit (BART) and Caltrain stations and would include the highest density employment and residential land uses.

Each urban village was also assigned to one of three categories, known as "horizons," that required certain milestones to be reached before an approved plan could be implemented. The most central and highest intensity urban villages were generally in the first and second horizons, while the smaller-scale and outlying urban villages were in the third horizon. One of the appendices in the general plan specifies growth capacities for downtown, employment centers, and urban villages by horizon. Only urban villages in Horizon 1 are permitted to grow at the time of writing; this phasing is reviewed on a four year cycle (City of San Jose, 2011).

Among the urban villages, 10 belong to the Commercial Corridor category (all in Horizon 3); 27 are Local Transit, spread throughout the horizons; 22 are Neighborhood, 21 of which are in Horizon 3; and three are Regional Transit, in Horizons 1 and 2. In addition to the urban villages, we use two other categories of areas for comparisons in our analysis: specific plan areas, of which there are seven, and "synthetic villages," which are agglomerations of parcels designed to be similar to urban villages in their mix of commercial and multi-family residential parcels, of which there are 15.

Urban village plans were not straightforward upzonings. The city's *Urban Village Implementation and Amenities Framework* added two zoning designations that could be applied to parcels within urban villages: an urban village commercial zoning district (UVC) and an urban village mixed use zoning district (UVMU). The urban village designations were a new form of land use regulation in the city, with the designations reducing some requirements for property owners, while adding others. The city council noted that the "intent of this Framework is to ensure that applications for a zoning change from commercial to residential or residential mixed-use in urban villages will only be considered by the City for approval if the developers share the increase in value of their projects from such zoning changes to residential use by constructing, providing, or otherwise funding amenities and public improvements as identified in the applicable Urban Village Plan" (City of San Jose, 2018a, Exhibit A-2). Thus, market rate mixed use development was required to provide urban village amenities equivalent to 2% of the total project value, calculated on a basis of $555 per square foot, and would get "extra credit" for building amenities as part of the project. These amenities could include privately maintained public open spaces (such as plazas), public art, extra commercial space, and off-site improvements.

Although our study is to our knowledge the first econometric analysis of the impact of San Jose's urban village plan, analysis by local advocates has noted the apparent slow pace of development in these areas since the release of the 2011 general plan. A 2019 report by the urban planning advocacy organization SPUR, for example, found that major development projects launched in San Jose since 2011 were largely located outside of urban villages in the city's downtown and commercial and industrial northern district, and suggested some potential reasons for this discrepancy (Wang, 2019).

At the time of writing, the city of San Jose was in the process of modifying its urban village strategy somewhat. City staff recommended removing two urban villages and adding two new urban villages. Meanwhile, members of a task force charged with reviewing the general plan unanimously recommended eliminating horizons altogether, while staff recommended keeping the horizons approach intact and moving several existing urban villages to Horizon 1 because of perceived market demand (City of San Jose, 2020c). Staff also proposed some revised requirements for "signature" projects (City of San Jose, 2019). These deliberations show policymakers' and stakeholders' shared interests in better aligning the urban village strategy with the city's goals, but indicate disagreements about the extent of changes necessary for doing so.

4. Data and estimation strategy

We seek to quantify the impact of urban village designations on urban development outcomes, including indicators of real estate development, such as issuance of construction permits, approvals for major projects, real estate property transactions, and changes in assessor valuations occasioned by sales or improvements.

The endogeneity of initial selection, plan content, and changing of horizons means that naive estimates of the development effects of urban village designation are likely to be biased. The urban village boundaries and plan adoption may have been influenced by staff assessment about the likelihood of mixed-use development to occur and by community opposition to upzoning and higher density. Some of the contemporaneous discourse around the urban village policy suggests that the more likely development was to happen, the more likely the zone was designated as an urban village, given an earlier horizon, and accelerated in the approval process.

An anecdote suggests the likelihood of endogeneity of the timing of implementation or activation of urban village plans. In December 2018, the city council approved a recommendation by the planning staff to immediately shift the Horizons of eight urban villages (4 approved, 4 as-yet unapproved), from Horizons 2 and 3 to Horizon 1 (City of San Jose, 2018b). The planning staff's memo illustrates the endogeneity of decisions about urban village content and timing; related to one set of urban villages, staff noted that "West San José is the primary area of San José where housing is anticipated to be built under current market conditions" (City of San Jose, 2018b).

We estimate the effects of the urban village policy – in essence a change in policy for some geographically defined zones and not for others – with a basic difference-in-difference (DD) approach. Specifically, we compare changes in outcome variables between pre- and post-treatment periods across various definitions of treated and untreated (or control) units, separately for commercial and for multi-family residential properties. Since the urban villages by definition were areas along commercial corridors, we cannot use the boundary between commercial and non-commercial areas itself as a difference. Most urban villages have commercial or multi-family zoned parcels on the urban village side of the boundary and parcels zoned for single-family housing on the other side of the boundary.

We collapse annual data for the pre-treatment years into the period 2005–2010 and post-treatment years into two alternative periods: an immediate 2011–2015 period and a later 2014–2019 period. Comparing two post-treatment periods allows for the possibility of delayed or longer-term impacts.

4.1. Data

We follow the advice of Krause and Lipscomb (2016) in being transparent and replicable in the many data preparation decisions that have to be made for analysis of real estate data. The office of the Santa Clara County Assessor is the source for most of the data. They provided the Secured Assessment Roll for 2005–2015 with Assessor Parcel Number (APN), gross secured value, and tax rate area. The file has 5,196,955 observations (about 500,000 parcels observed for 11 years). We subsequently purchased the 2018 Master file, which contains gross secured value for all APNs for 2018 and 2019. The assessor office also provided a 2015 file of 483,648 APNs with their zip codes and use codes. There are 99 use codes defined by the Santa Clara County assessor. We aggregated the codes into several broad categories, discussed below.

We also obtained the Condensed Sales files for the period from 2014 through June 2019. These files contain a record of property transfers in Santa Clara County with APN, recording date and transfer dates, indicated price, and names of prior assessee and current assessee.

Building permit data is accessible from the Permit Center website of the City of San Jose. All permits from 2005 through 2019 are available in publicly available tab-delimited text files, and include address and APN for the permits. Unfortunately, the format of the permit files are such that only a few broad categories of permits are identified. There is no field for the approximate value of the work to be undertaken, so from the files it is hard to distinguish, for example, a complete interior remodel of a building and a small alteration to one part of a building. We privilege then a category included in the field "work description" in the permit files, category New Construction (other major categories include Additions/Alterations, Demolition, Finish Interior, ReRoof, Sub-Trades Only, and Tenant Improvement).

We also examine major projects. The City of San Jose every year publishes a report of major building projects in the city, which it defines as residential projects with at least 50 housing units and commercial projects with at least 25,000 square feet (City of San Jose, 2020a). The reports list all the major projects permitted or in the approval process. These projects are listed with address and APN number.

For both the permit and major project data, the APN for and year of issuance of the permit, or the APN for and year a major project appears on the list, may not correspond to an APN and year present in the Assessor Roll. As noted below, development of real estate often leads to a change in the APN numbers as parcels are split or merged. The APN listed at the time a project is launched may not correspond to an APN for that Roll year. We adopt a "fuzzy" merge for the permits and major projects. The vast majority merge with their listed APN and year, but for the fraction that do not we adjust the year forward by a year, and then merge. For the remaining fraction we adjust the year to be one year prior to the listed year, and merge. For those that remain unmerged, we adjust by two years, and merge, and then repeat by adjusting the year variable by three years. We thus are able to link more of the permits and major projects. The fraction of entries that merge through this process is small, and might introduce a small measurement error in the sense that the timing of the permit or major project might be incorrectly assigned to the "post 2011" period rather than to the "pre 2011" period.

Our fourth outcome variable is the parcel secured assessed value. The fiscal implications of this value are substantial for county and city decision-makers as the assessed value is the basis for property tax assessment. The state's 1978 Proposition 13 generally limits allowable increases in assessed value to the lesser of 2% per year or the California Consumer Price Index (California LAO, 2012). This value is known as the "factored base year value" (County of Santa Clara, n.d.) There are several exceptions. First, if a property is sold, its assessed value resets to its acquisition price. Second, if a property owner makes substantial improvements, the assessor will reassess the improvements only. Third, if the market declines and a property's assessed value exceeds its market value, the assessor can temporarily reduce assessed values under the provisions of the state's 1978 Proposition 8, and later return the assessed value to its factored base year value (California LAO, 2012). Observed assessed values thus are not current market valuations but a mix of different vintages of market values depending on the most recent such sale.

The City of San Jose made available spatial data files for the general plan, zoning, and other special designations (City of San Jose, 2020b). We use these to identify whether parcels fall within the boundaries of urban villages and other planning areas.

4.2. Constructing consistent parcels

Our unit of analysis is the parcel, defined by the Santa Clara County Assessor office. Parcels are assigned APN numbers. When parcels are split or combined through real estate transactions or development, new APN numbers are assigned by the assessor. Development projects also often lead to redrawing of assessor maps, and new APN are assigned because the APN numbers refer to pages in the map "book." An important data preparation problem in real estate data analysis is record linkage to track parcels over time, and create a panel of consistent parcels. Two methods may be used: using shapefiles from each year to infer patterns of splits and combinations, or using crosswalks or key files to track changes and reconstruct parcel "parents" and "children." We use the Assessor crosswalks and three different methods for creating a panel of consistent parcels from 2005 to 2019: using 2019 parcels as the "base" set of parcels ("Consistent 2019"); using 2005 parcels as the "base" set of parcels ("Consistent 2005"); and using a minimum consistent parcel group as the unit of analysis ("Consistent grouped parcels"). Appendix A discusses the methods used in the paper to link parcels over time and so create a consistent panel of outcomes at the parcel level. For the main results presented in the paper, we use the Consistent grouped parcels, with results for the alternative Consistent 2005 and Consistent 2019 parcels presented in appendices. Results are generally robust to changing the consistent parcel method.

4.3. Descriptive statistics

Fig. 1 maps the urban villages (distinguishing Horizon 1 from Horizons 2 and 3), and also the synthetic villages. To be noticed is that many urban villages align along commercial corridor streets, and that the synthetic villages appear to be similarly sized although somewhat more compact. Table 1 shows the number of parcels in urban villages (by Horizon) and in other areas, by broad use category, in 2019, along with selected median characteristics based on Census block group data circa 2010. In addition to urban villages, the table includes the following categories of parcel groupings: synthetic villages; specific plan areas; and a residual of all other parcels, which includes downtown as well as largely industrial areas and residential neighborhoods. See Appendix B for further information.

As indicated in Table 1, the urban village parcels together comprised about 6300 parcels, or 2.7% of the approximately 230,000 parcels in the city of San Jose with residential or commercial land uses in 2019. (We exclude public, industrial, and other more specialized and restrictive types of parcels in this table.) More importantly, urban villages included about 1900 parcels with commercial uses, out of about 5000 commercial parcels in the city; thus about 38% of the city's commercial parcels were potentially open to mixed-use and higher development intensities in urban villages. Additionally, about 7% of the city's 49,000 multifamily apartment parcels, condominiums, and townhouses were included in the urban village boundaries. Fewer than 1% of the 175,000 parcels with single family home uses were included in the urban village boundaries.

Urban villages and other parcel groupings may overlap with portions of multiple census block groups. We calculated median characteristics for such areas by first assigning each parcel to its underlying block group. We then found the median block group characteristic for parcels in each urban village horizon or other parcel grouping. Thus, for example, in the first row of Table 1, the median parcel among all UV Horizon 1 parcels was in a block group with residential density of 5.53 housing units per acre and median household income of $71,410. In this table, proportion nonwhite is the proportion of the block group population that did *not* identify as non-Hispanic white race alone. We use block group characteristics based on the 2010 Census or the 2006–2010 five-year sample of the ACS in order to capture the period just prior to the establishment of the urban villages in 2011.

The table suggests that there are some differences across the groups. In particular, the retail jobs index is substantially greater for Horizon 3 urban villages. We note that the City Council reassigned several urban villages from Horizon 3 to Horizon 1 in December 2018; that reassignment is not reflected in the table, which uses the original Horizon designations of 2011. These reassigned urban villages included Santana Row, a major commercial retail development, and several other thriving retail zones. Also noteworthy is the much lower retail jobs index for the "All others" category, which includes the single-family homes zoned areas which are a large part of the city. In terms of ethnic distribution of the population, the overall proportion white (alone, non-Hispanic) for the City of San Jose according to the U.S. Census is about 26%. The block group proportions for nonwhite populations roughly correspond to this. Urban villages in Horizons 1 and 2 are somewhat higher proportion nonwhite, while the Horizon 3 and synthetic villages are similar. As might be expected, the "All others" category, comprising the single-family zones areas of the city, has higher median incomes than the urban villages.

Table 2 presents descriptive statistics for the outcome variables used in the analysis, over the 2014–19 period. These include the average assessed values of parcels, the average number of transactions per parcel, the average number of permits pulled per parcel, of any sort of permit, and the mean number of major projects (per 1000 parcels), according to use categories and horizons. The parcels are grouped according to some of the comparison categories: in urban villages; in algorithm-generated synthetic villages; in specific plan areas; and other parcels. As shown, commercial properties outside of comparison areas had average assessment valuations of about $4.1 million, while in urban villages assessment valuations averaged $2.7 million. Parcels zoned for multi-family use ranged in value from $700,000 to $1.3 million. Finally, for single-family

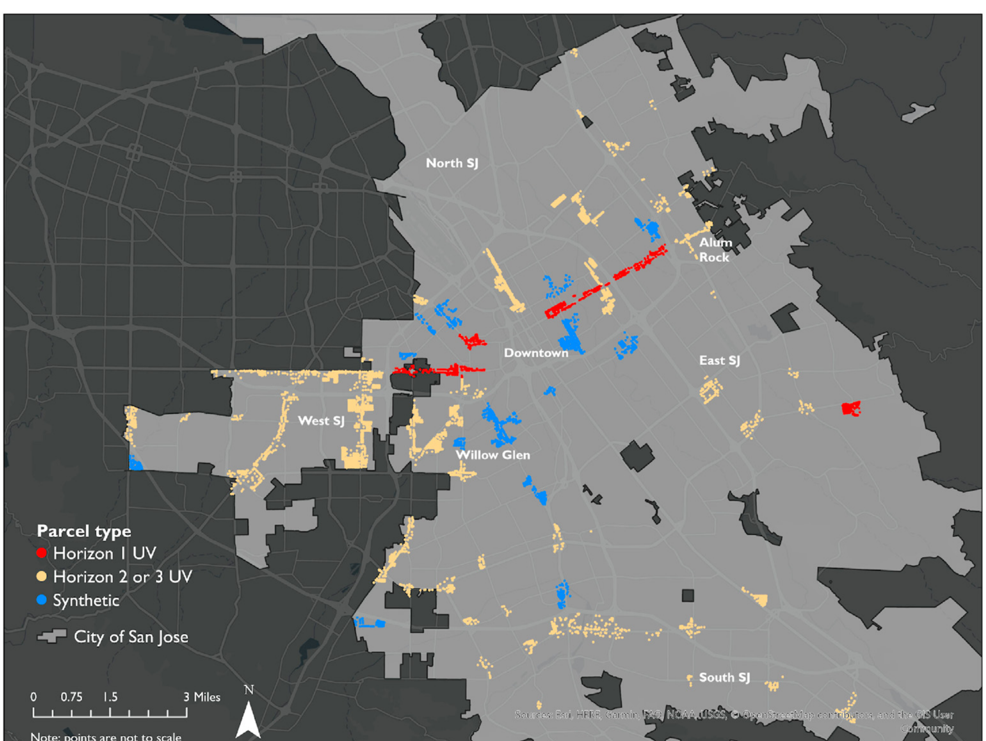

Fig. 1. Map of parcels in urban villages (by horizon) or synthetic villages in San Jose.

Table 1
San Jose parcels and their characteristics by urban village status, 2019.

	Number of parcels			Block group characteristics of median parcel, c. 2010			
	Single-family/ Duplex	Townhouse/Multi-family	Commercial	Residential density	Retail jobs index	Median household income	Proportion non-white
UV Horizon 1	223	526	410	5.53	20	71,410	0.82
UV Horizon 2	573	793	349	4.85	27	69,334	0.87
UV Horizon 3	411	1914	1124	6.43	64	69,265	0.57
Synthetic villages	0	1068	604	5.44	24	62,526	0.60
Specific Plan areas	3142	1971	150	2.71	44	96,131	0.85
All others	170,980	42,663	2429	4.55	3	89,259	0.68
Total	175,329	48,935	5066				

Notes to Table 1: Industrial, manufacturing, public use, and a variety of other use categories are not included in the table. Data sources and definitions for Census block group variables: Residential density– EPA Smart Location Database (EPA 2013) variable D1a, Gross residential density (HU/acre) on unprotected land; Retail jobs index– EPA Smart Location Database variable E5_Ret10, Retail jobs within a 5-tier employment classification scheme; Median household income– ACS tables for 2006–2010 5-year sample, downloaded from NHGIS (Manson et al., 2020); Proportion nonwhite: ACS tables for 2006–2010, 5-year sample, Proportion of block group population that is not in the category *Not Hispanic or Latino: White alone*.

Table 2
Characteristics of San Jose parcels included in analysis, over period 2014-19.

	Use category	Mean gross secured assessment valuation ($)	Mean number of times transacted	Mean number of construction permits pulled per 1000 parcels	Mean number of major projects per 1000 parcels
Urban villages	Commercial	2,688,928	0.15	4.21	6.85
	Townhouse or Multi-family	1,315,006	0.31	0.00	1.24
	Single-family or Duplex	515,165	0.26	4.13	1.65
Synthetic villages	Commercial	1,444,878	0.17	0.00	1.65
	Townhouse or Multi-family	704,435	0.23	0.00	0.00
Specific Plan areas	Commercial	940,389	0.09	0.00	0.00
	Townhouse or Multi-family	869,292	0.36	0.00	0.51
	Single-family or Duplex	810,722	0.17	0.00	0.00
All others	Commercial	4,118,164	0.19	3.45	12.27
Non UV, non control	Townhouse or Multi-family	636,643	0.29	0.41	0.12
	Single-family or Duplex	496,573	0.18	2.13	0.05

Notes to Table 2: Mean values in the table are calculated by taking the mean across years for each parcel over the 2014–2019 period to obtain an annualized mean value for each parcel, and then taking the mean of annualized parcel means across all parcels in the specified area and use category. Thus mean values for transactions, permits, and major projects are average annual "flows." Industrial, manufacturing, public use, and a variety of other use categories are not included in the table.

homes, the average assessment valuation was in the $500,000-$800,000 range. As noted earlier, urban villages by design contained relatively few single-family homes.

Commercial and multifamily parcels saw an average of transactions ranging from 0.09 to 0.36 over the 2014–19, suggesting that roughly 15% of commercial parcels transacted over the five year period while about 30% of multi-family parcels transacted. It appears that about 20% of single-family parcels transacted over the period. Note that the table reports the mean of the sum of transactions over the 2014–19 period for each parcel: some parcels transacted multiple times during the period. So the number is close to but not quite the probability of a parcel transacting.

The table also reports the mean of the number of construction permits pulled, and the mean number of major projects (per 1000 parcels, for convenience). As can be seen, the groups had quite different experiences over the 2014–19 period, but it should be borne in mind that they may also have had different experiences during the 2005–11 period: of interest is the change in the likelihood of transactions, permits and projects, not the level.

As an alternative to the mean per parcel numbers reported in Table 2, one might consider the experience of a typical urban village, synthetic village, or specific plan area. We define as the typical area the one with the median total number of parcels regardless of use category. The median-size urban village had 54 parcels; the median-sized synthetic village had 65 parcels; and the median specific plan area had 398 parcels. Appendix C reports a table of the total number of transactions, construction permits, and major projects for these typical areas for the time periods considered.

4.4. Estimation strategy

We follow a standard difference-in-difference methodology, comparing the change in outcome variables (pre-to post-treatment) between treated and untreated parcels– e.g., parcels in Horizon 1 urban villages (treated) vs. parcels in Horizon 2 and 3 urban villages (untreated). We restrict the sample to parcels in the treatment and control areas being compared and estimate an OLS regression with dummy variables for period, treatment status, and the interaction between them:

$$Y_{it} = \alpha + \beta\, POST_t + \gamma\, TREAT_i + \delta\, POST_t\, TREAT_i + \mu_{it} \quad (1)$$

where i is the consistent parcel or parcel group and t is the time period (0 for the pre-treatment period and 1 for the post-treatment), Y_{it} is the outcome value for parcel i in period t; $POST_t$ is a dummy = 1 for the post-period; $TREAT_i$ is a dummy = 1 if parcel i is in the treatment area; and μ_{it} is the error term. The interaction coefficient δ is the conventional difference-in-difference treatment effect. We collapse annual data for the pre-treatment years into the period 2006–2010 and the post-treatment years into two alternative periods: an immediate 2011–2015 period and a lagged 2014–2019 period.[6] The regressions are run separately for commercial and multifamily parcels.

In practice, although we focus on a couple of particular treatment comparisons, we have a wide range of potential definitions of treatment status to compare, such as parcels in synthetic villages, parcels in specific plan areas, parcels not belonging to any urban village or synthetic village, parcels in Horizon 1 urban villages, in approved urban villages, in any designated urban village, and so forth. To facilitate multiple comparisons across many alternative treatment statuses, we supplement the difference-in-difference estimates by reporting and comparing slope estimates from the following simple pre-post regression for various subsamples of consistent parcels (i) defined by treatment category (j):

$$Y_{it} = \alpha_j + \beta j\ POST_t + \mu_{it} \quad for\ i\ \varepsilon\ area\ j \tag{2}$$

The coefficient β_j is the change in mean outcome between pre- and post-periods for parcels in the subsample for area category j. We estimate β_j for a variety of subsamples defined by treatment category as well as separately for commercial and multi-family use categories.

One identifying assumption for difference-in-difference is common or parallel trends in the treated and untreated units under the counterfactual of no treatment. Endogeneity of the treatment could be associated with a violation of the common trend assumption. For example, suppose as noted earlier that "treated" urban villages were selected for earlier implementation (treatment) because they were in more dynamic parts of the city, with more rapidly rising property values than untreated urban villages. Then the counterfactual trend in property values in the treated urban villages *had they been untreated* would be different from the trend in untreated villages, violating the common trends assumption.

Documents written by city planning staff suggest that urban village approvals and changes to urban village plans responded to perceived interest by developers. The timing of rezoning and the content of the rezoning plans were therefore possibly not "as good as" random. Of course, it is also possible that city staff justifications of changes in zoning were wishful thinking based on casual empiricism and anecdote, and thus basically random. Moreover, it is possible that the areas of the city that were already in processes of redevelopment were viewed by staff as not needing to be included as urban villages. As a check on potential endogeneity of the urban village designation to local growth, we check for pre-trends in the outcome variables (see below), while acknowledging that various endogeneity concerns remain.

4.5. Control groups

The difference-in-difference estimation strategy is only as good as the reasonableness of the control group that is to be compared with the treatment group. Identifying appropriate comparison control groups for urban villages is challenging since the areas were not randomly selected from a population of potential zones that could have been designated as urban villages. There likely was some endogeneity in the selection process.

One common strategy for constructing control groups is to consider treatment borders as basically arbitrary, and compare units inside the border with those outside the border. Urban village impacts in San Jose, however, cannot be estimated by comparing parcels on either side of the urban village boundaries. The boundaries of urban villages were deliberately drawn to include parcels on the commercial avenue or in close proximity, and exclude single-family home areas often just one parcel away from the urban village. Thus urban villages were "strips" along an arterial. The "ends" of the strips of urban villages were often natural boundaries such as bridges, highways, or large existing commercial developments. There is no reason, then, to suppose that parcels on opposite sides of the boundaries would be similar or comparable, either prior to the urban villages or after the urban villages. This precludes a strategy such as that of Freemark's study of upzoning in Chicago (Freemark, 2019).

We consider instead various comparisons that treat the timing and location of treatment as basically arbitrary in a relatively narrow timing window and region. Our two main diff-in-diff specifications compare (1) urban villages in Horizon 1 with urban villages in H2 and H3 and (2) urban villages with synthetic agglomerations that are similar to urban villages in having a mix of commercial and multi-family parcels grouped together. In additional specifications (available upon request), we compare (3) urban villages that have been officially adopted (with approved plans) by the city with as-yet non-adopted urban villages and (4) urban villages with other similar designated areas (specific plan areas). The comparisons are such that reasonable interpretations of the goal of the urban village strategy to densify through upzoning would imply that the first group (the H1 urban villages, or the approved plan urban villages), would have higher proportions of parcels permitted for development and transacting compared with the comparison group, in the period after the urban village strategy was launched, compared with the parcel activity prior to the urban village strategy.

Two of the comparisons (1 and 3) involve the timing aspects of urban villages: we compare parcels in H1 urban villages with those in H2 and H3, and we compare parcels in urban villages that have approved plans with parcels in urban villages that do not have approved plans. As noted earlier, there were two related timing variables associated with the urban villages: the assigned horizon, and the local engagement process and city council approval of an urban village plan. Some urban villages were designated H2 and H3, and planning staff suggested that zoning would likely not change for a decade or more after 2011. Staff recommended, and the city council approved, changes in the horizons several times over the 2011–19 period. The approval process for plans appeared to be somewhat haphazard and apparently partly driven by grant funding to cover the expenses of local engagement (meetings, notifications, reproduction of materials, consultants). By 2019, 19 areas had urban village plans approved, leaving 49 still in process or where the planning and community engagement process had not yet begun. Most of the H1 urban villages had approved plans, while some of the H2 and H3 urban villages did too. In-depth reading of staff memos and other city documents suggests that both the horizon and approval processes had substantial arbitrary components.

The two other comparisons (2 and 4) involve geographic and timing effects. In one we compare parcels in urban villages (however designated) with parcels in other designated plan areas that had been approved (and implemented) in the decades prior to the urban village strategy.[7] These other plans were known as "specific plans," "master plans," or "neighborhood plans." Some potential urban village sites had been evaluated as potential growth sites prior to the urban village strategy, and thus had land use regulatory overlays that predated the General Plan of 2011.

[6] Although our panel starts in 2005, the analysis samples start in 2006, because our measure of inferred transactions or development based on assessor revaluation is based on the one-year change in assessed value, the calculation of which requires dropping the initial year of data.

[7] The other plan areas were: Alviso (plan approved 1998, 2840 acres of urban use land), Communications Hill (1992, 942 acres), Evergreen (1991, 879 acres), Jackson-Taylor a.k.a. Japantown (1992, 109 acres), Martha Gardens (2003, 145 acres), Midtown (1992, 125 acres), and Tamien Station Area (1995,149 acres). Two other zones with more complex histories are the Blossom Hill Rd/Hitachi urban village area and the Berryessa Swap Area (plan approved 1982, 295 acres). Two other Specific Plan areas are not included: Rincon South (1998, subsequently converted to an approved Urban Village plan, 465 acres), and Silver Creek (1989, 134 acres), which appears to be a planned golf community. See Data Appendix B for more information on these other sites.

The city had carved out two zones (downtown and Diridon station area) as also meriting separate plans for zoning purposes. The zones are quite different from the urban village zones and would seem to be poor comparison groups for evaluating the impact of urban village upzoning.

In the other comparison based on geography we compare urban village parcels with commercial and multi-family zoned parcels that are in other local "synthetic villages" identified through an algorithmic approach. We first identify every commercial and multi-family parcel in the city that is not in an urban village, specific plan area, employment area, downtown, downtown core, or within 250 m of the border of those areas. Then we determine whether the parcel is in an "agglomeration," defined as a cluster of at least 25 commercial or multi-family parcels, each within 100 m of another commercial or multi-family parcel in the cluster. To identify these neighboring parcels we search for the four nearest commercial or multi-family neighbors within a search radius of 100 m. These sets of neighboring parcels then form the synthetic village comparison group of parcels that are in a sense similar to urban villages (an agglomeration of parcels that are not single-family homes). Many of the candidate synthetic areas have few or no commercial parcels. To rule out agglomerations that are isolated multifamily housing developments with little mixed-use potential, we adopt the criterion that the control areas must have at least 10 commercial parcels, which leaves 18 synthetic villages containing a total of 1675 parcels. They range in size from 26 to 278 parcels, and thus are fairly representative of an urban village in terms of number of parcels.

As a sensitivity check we also used an alternative definition of synthetic villages that set the search radius to 50 m, required a minimum of 50 neighboring parcels in the cluster, and allowed as few as 2 commercial parcels per cluster, allowing for more residential-type agglomerations. As indicated in Fig. 1 below, these "Synth Alt UVs" were decidedly less commercial in character than the actual urban villages, and thus perhaps a less comparable control group.

These synthetic village control areas include certain neighborhood commercial zones that might have been obvious candidates for urban village designation but were not included: Willow Glen, parts of Meridian Avenue, and parts of Cambrian. It is possible that community opposition to higher density in these neighborhoods led to them not being included in the urban village process.

Two other choices went into defining the universe of parcels included in the analysis. One choice was to exclude mobile homes from the analysis. Mobile homes and associated mobile home parks have complex characteristics in terms of their status as parcels. In the past, mobile homes could be considered as unsecured property that could be "rolled away" if the park were sold and developed. But increasingly, mobile home owners are considered by cities to be stakeholders in the development process, and city councils have been under pressure to grant permits for development of mobile home parks only if current mobile home residents are guaranteed affordable housing in the new

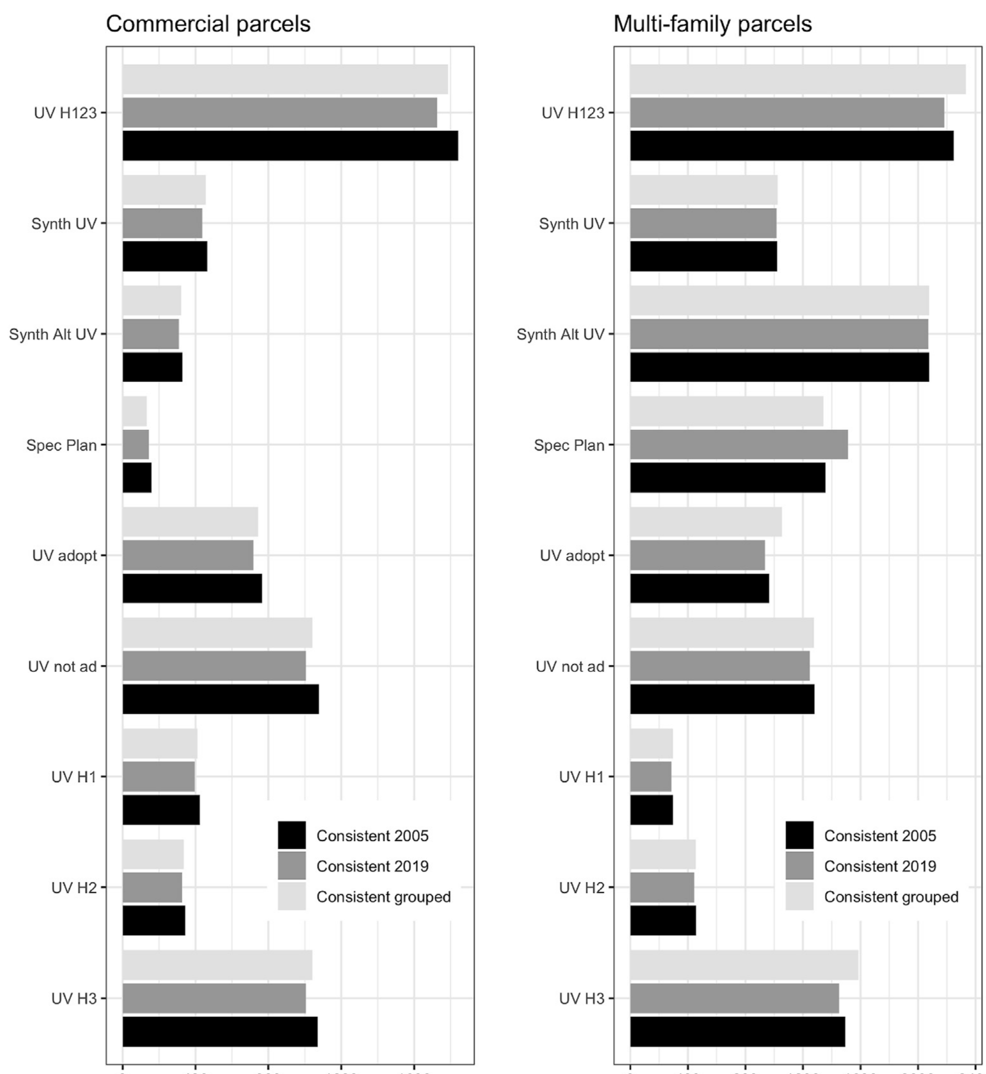

Fig. 2. Numbers of parcels in various treatment categories of areas of San Jose, as defined in text.

development.

Another issue concerns parcels that are not viable for their use code, as commercial or residential. Conversations with staff at the Santa Clara Assessor office suggested that numerous parcels in the city classified as commercial or multi-family might in reality have no economic value or prospects for development. These consist of odd shaped "leftover" or "cutout" parcels created during private development or public urban infrastructure work (streets, utilities). The Assessor Office apparently would give these parcels nominal values. The staff suggested that cutoffs of $50,000 for commercial and $20,000 for multifamily would capture these parcels, and no economically worthwhile parcel would have an assessor valuation lower than those numbers.

Fig. 2 displays the number of parcels in each of the groups outlined above. The urban village areas have about 1700 commercial parcels, and 2400 multi-family parcels, varying with the consistent parcel aggregation method. The two different "synthetic village" algorithms produce about 500 and 300 commercial and about 1000 and 2100 multifamily parcels, respectively. Recall the two have different algorithms, with the first designed to capture more commercial clusters and the second capturing many multifamily parcel clusters. The alternative methods for creating consistent parcel comparisons over time (Consistent 2005, Consistent 2019, and Consistent grouped parcels) are represented by the shading of the bars, and they largely yield similar parcel counts in each category.

5. Results

We present our results in three stages. First, we consider a table of mean outcomes for the pre and post periods. The table makes plain that the reasonably precisely estimated null effects of the difference-in-difference specifications are due to relatively little development across the city over the 2005–19 period. That is, both treatment and control groups saw relatively few transactions, permits, and major projects, both in the period before the urban villages strategy and in the period after. Second, we discuss several difference-in-difference specifications. These suggest there were no large effects on development of the urban village designations, compared to control groups. Third, we display coefficient estimates for simple pre-post comparisons for each aggregation group separately. These are similar in spirit to the difference-in difference estimation but enable us to be more agnostic about privileging certain treatment-control comparisons among a multiplicity of comparison groups. As noted above, there are many reasonable potential treatment-control pairs; in the spirit of transparency we present the underlying pre-post pattern for each group separately.

The four outcome measures are whether the parcel had a new construction permit over the time period, whether the parcel had an assessor revaluation or a sale transaction over the period, whether the parcel was the location of a major project, and the log of maximum assessor valuation over the period. There are 10 categories of areas to be compared with each other (abbreviating urban village as UV and horizon as H): UV H1, UV H2, UV H3, UV H123 combined, UV adopted plans, UV not-yet-adopted plans, specific plan areas, synthetic village control areas (2 versions), and an "area" consisting of all other parcels in the city (called "All other"). The three different ways to track consistent parcels are labelled "Consistent 2019," "Consistent 2005," and "Consistent grouped parcels." The two generalized use-codes for parcels are commercial or multi-family. The pre-period is always defined as the five years 2006–2010, and two alternative post-periods are considered: 2011–2015 and 2014–2019. We limit presentation of results below to the consistent grouped parcels. Results for the two other approaches for creating consistent parcels (using 2005 as base or using 2019 as base) are in Appendices D, E and F.

5.1. Counts and means of outcome variables

Table 3 presents the means of the outcome variables, for the pre and post periods, for two treatment-control pairings (all urban villages compared with synthetic villages, and UV Horizon 1 compared with UV H2 and H3). The table anticipates the regression analysis, showing that there was not much development activity happening in urban villages compared with the synthetic villages, or between the UV H1 and the UV H23. Three of the outcome variables are coded as dummy variables: Was there an assessor revaluation or property transaction? Was there a permit for new construction? Was there a major project? Table 3 displays the counts for the non-zero outcomes for these variables, for the relevant groups of parcels. Separate columns for revaluations and transactions are provided, given the change in data availability over our period. Appendix D provides results for the Consistent 2005 and Consistent 2019 parcel groupings.

As can be seen in the table, the number of positive values is quite sparse for the major projects and new construction permits for both the urban villages and synthetic villages. There was very little development in either of these zones for both the pre and post periods.

5.2. Difference-in-difference estimation

Table 4 displays our estimates of the difference-in-difference treatment effect– δ in equation (1)– focusing on two key comparison pairs: Horizon 1 urban villages compared with Horizon 2 and 3 urban villages, and all urban villages (regardless of horizon) compared with synthetic villages. The regressions are run separately for commercial and multi-family parcels, and separately for alternative definitions of the post-period: 2006–2011 and 2014–2019. We provide two sets of standard errors for the coefficient estimates: conventional White-corrected "robust" standard errors, and clustered standard errors, clustering on

Table 3
Count of positive values for outcomes, by sample.

Land use	Treatment	Period	Number of parcels	Number of parcels with any			
				Revaluation	Transaction	New construction permit	Major project
Commercial	Synth UV	2006–2010	456	128		2	0
		2011–2015	456	100		1	0
		2014–2019	456		55	0	1
	UV H123	2006–2010	1794	455		13	9
		2011–2015	1794	437		13	7
		2014–2019	1786		242	7	12
Multi-family	Synth UV	2006–2010	1022	305		2	0
		2011–2015	1022	404		0	0
		2014–2019	1023		206	0	0
	UV H123	2006–2010	2332	948		4	8
		2011–2015	2332	1135		4	4
		2014–2019	2332		506	1	4

Notes to Table 3: Counts of positive values for outcome variables discussed in text, for commercial and multi-family parcels, synthetic villages and urban villages of any Horizon, and for the pre (2006–2011) and post (2011–15 or 2014–19) periods. Panel is based on the consistent parcel groups method of creating consistent parcels over time.

Table 4
Difference-in-difference estimates of urban village effects.

Outcome	Use code	Treatment	Control	Post	Effect	se_robust	se_cluster
Log value	Commercial	H1	H23	2011–15	0.0057	0.0938	0.0283
				2014–19	0.0239	0.0943	0.0426
		H123	Synthetic	2011–15	0.0298	0.0917	0.0293
				2014–19	0.0153	0.0914	0.0432
	Multi-family	H1	H23	2011–15	0.0596	0.0869	0.0185
				2014–19	0.0657	0.0869	0.044
		H123	Synthetic	2011–15	−0.0119	0.0495	0.0284
				2014–19	−0.058	0.0504	0.0455
Major project	Commercial	H1	H23	2011–15	−0.0049	0.0055	0.006
				2014–19	−0.0022	0.0066	0.0057
		H123	Synthetic	2011–15	−0.0011	0.0022	0.0025
				2014–19	−0.0005	0.0034	0.0034
	Multi-family	H1	H23	2011–15	0.002	0.005	0.0051
				2014–19	0.002	0.005	0.0051
		H123	Synthetic	2011–15	−0.0017	0.0015	0.0015
				2014–19	−0.0017	0.0015	0.0016
Reval/trans	Commercial	H1	H23	2011–15	−0.0248	0.0334	0.0365
				2014–19	0.0689	0.0318	0.0399
		H123	Synthetic	2011–15	0.0514	0.0321	0.0379
				2014–19	0.0414	0.0291	0.0418
	Multi-family	H1	H23	2011–15	0.0445	0.0438	0.1019
				2014–19	0.0223	0.041	0.0357
		H123	Synthetic	2011–15	−0.0167	0.0255	0.0449
				2014–19	−0.0918	0.0232	0.0224
Constr permit	Commercial	H1	H23	2011–15	0.0095	0.0071	0.0066
				2014–19	−0.002	0.0046	0.0043
		H123	Synthetic	2011–15	0.0022	0.0047	0.0045
				2014–19	0.001	0.004	0.0041
	Multi-family	H1	H23	2011–15	0.0078	0.0049	0.0035
				2014–19	0.0015	0.0011	0.0007
		H123	Synthetic	2011–15	0.002	0.0018	0.0019
				2014–19	0.0007	0.0017	0.0016

Notes to Table 4: Estimated coefficients of interest (δ in equation (1)) in difference-in-difference estimation of each of four outcome variables discussed in text, for commercial and multi-family parcels, comparing the pre period (2006–10) to each of two post periods: 2011–15 or 2014–19. Estimation is ordinary least squares. Two sets of standard errors are reported: White-corrected (se_robust) and clustered by zone (se_cluster). Asterisks flag coefficient estimates where the coefficient divided by the standard error is greater than two (corresponding roughly to the conventional p-value level of 0.05), unadjusted for multiple comparisons. Panel is based on the consistent parcel groups method of creating consistent parcels over time.

the specific area (urban village or synthetic cluster) to which the parcel belongs. Clustering may adjust for potential spatial autocorrelation of errors across parcels that are near each other by virtue of being in the same neighborhood. As it turns out, clustered standard errors are in some cases greater and in other cases smaller than the usual robust standard errors, but in almost all cases it does not make a difference for testing the null of zero treatment effect. An asterisk is placed to the right of the standard error estimate for cases in which the point estimate (Effect column) is more than two standard errors away from zero.

The various permutations of specifications result in 32 coefficient estimates, which we estimated for each of our three methods of forming consistent parcel samples across time. Table 4 displays results for the consistent grouped parcels; results for the two other methods for creating consistent parcels, using 2005 as base or using 2019 as base, are in Appendix E.

Table 4 and the tables in Appendix E show that of the estimated coefficients, only a smattering of the coefficients are statistically significant in the conventional sense: in Table 4, 2 of 32 coefficients using robust standard errors, and 4 of 32 using clustered standard errors. In only one case in Table 4 was the coefficient for commercial parcels statistically significant. The coefficient that is “strongly” (rather than marginally) significant by a standard test is that for revaluations/transactions in multi-family parcels, comparing urban villages and synthetic villages. The coefficient is negative, however, suggesting that urban villages saw fewer transactions than the control areas. Taken on their own, these “significant” estimates suggest (1) some possibility that multi-family parcels saw more activity in Horizon 1 urban villages relative to Horizon 2 or 3 urban villages; and (2) a negative treatment effect on 2014–19 transactions for multi-family parcels in urban villages of any horizon relative to synthetic villages.

Among the many multiple comparisons estimated in Table 4 and Appendix E, we would expect some “significant” effects by chance. Because the samples are not all independent of one another, the multiple comparisons problem is complex.

Overall, the picture is that urban villages had minimal impacts relative to controls.

5.3. Compact visual display of pre-post change

Figs. 3–6 compactly display the mean change in outcome variables between the pre (2006–10) and post (2011–15 or 2014–19) periods. The figures enable rapid comparison across the different designated growth areas and various control areas (such as synthetic villages) to discern potential urban village effects. “Eyeball” econometrics suggests that the confidence intervals overlap in most cases, so that tests of zero difference would not be rejected, but these are not formal pairwise statistical tests. The figures supplement the difference-in-difference estimates presented above.

Each figure corresponds to a different outcome variable. Each panel displays 40 estimated coefficients (10 groups of parcels that may be thought of as either treatment or control groups, 2 use-codes, and 2 outcome periods). The plots are designed to provide a simple comparison of the change in mean outcomes between the pre-treatment and the post-treatment periods, across various categories of parcels. In each case, the dot and whiskers provide the point estimate and 95% confidence interval, respectively, for the slope coefficient β_j in equation (2). Confidence intervals reflect estimated standard errors clustered at the area or zone level. For the three outcome variables that are binary (dummies), the regressions estimate a linear probability model. The results in Figs. 3–6 use the consistent grouped parcels; figures for the two other methods for

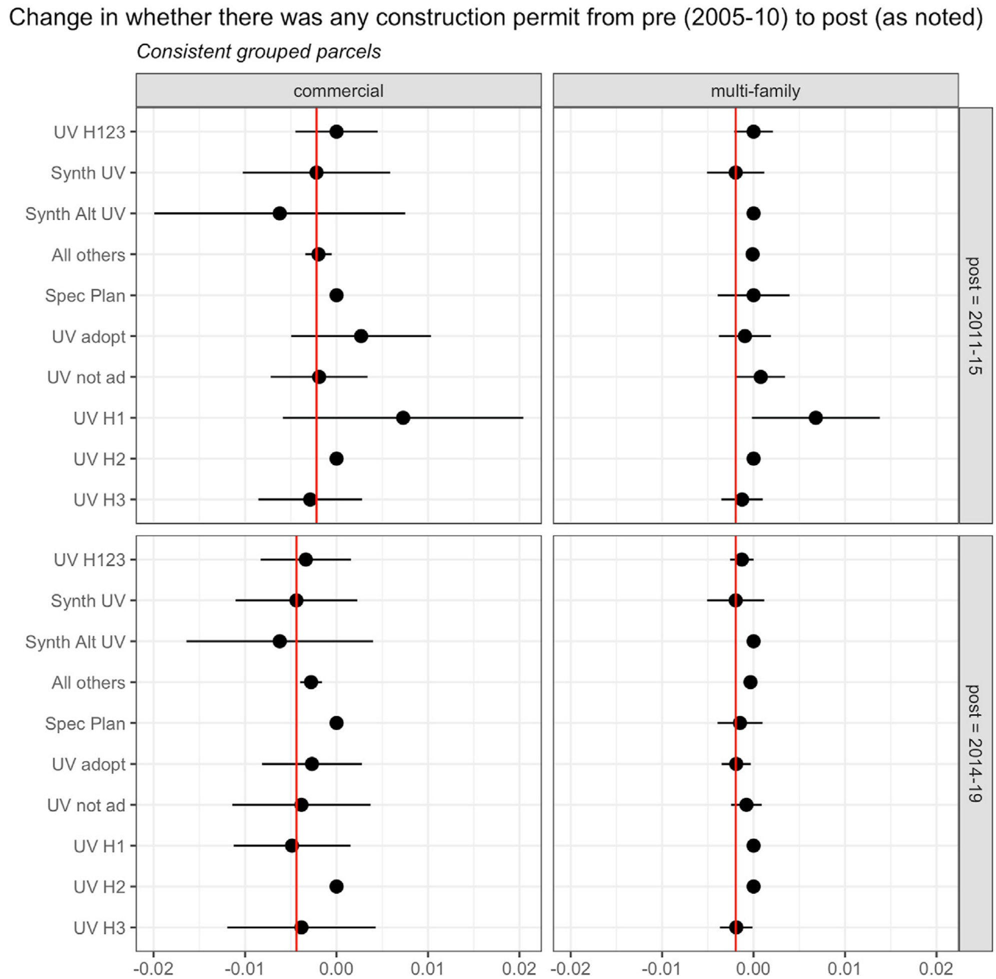

Fig. 3. Coefficients (β_j in equation (2)) and confidence intervals for regressions estimating change in whether parcels had a new construction permit pulled between pre and post periods, for defined areas of San Jose. Confidence intervals are plus or minus two standard errors, where all standard errors are clustered at the zone level. Panel is based on the consistent parcel groups method of creating consistent parcels over time.

creating consistent parcels using 2005 as base or using 2019 as base are in Appendix F.

The data set used in each regression has two observations per parcel: the value for the outcome for the parcel in 2005–10 and the value for the outcome for the parcel in the "post" period. The explanatory variable is a dummy variable for the post-period. We use a balanced panel for each regression; specifically, a parcel or parcel group must be represented in every year of both the pre and post periods. This procedure avoids potential selection problems arising from aggregating over different numbers of years for different parcels. Because we have tried to create consistent parcel or parcel group definitions over time, the balanced panels contain the large majority (typically about 97 percent) of the parcels present in the full unbalanced panel. A positive (negative) coefficient on the "post" dummy variable indicates that on average parcels in the category had higher (lower) values for the outcome in the "post" period.

To interpret Figs. 3–6, consider the upper left panel of Fig. 3 which reports the outcome for whether there was a construction permit pulled for the parcel during the time period. The estimates are for commercial parcels. The first row shows the change in the predicted probability of a permit being pulled for a parcel located in urban villages (regardless of horizon, so including horizons 1, 2, and 3), from the 2006–2010 period to the 2011–2015 period. Such parcels basically saw no change in the probability of a construction permit, on average. The point estimate is centered on zero and the confidence interval includes zero. The red vertical line represents the point estimate for the synthetic village control group of parcels (in the second row). As can be seen, these parcels saw the probability of permits decline by a small amount, about −0.002, and the confidence interval also includes zero. Comparing the first and second rows, the difference between the point estimates of pre-post changes is the diff-in-diff estimator for this comparison (of H123 against synthetic villages), which in Table 4 can be seen to be 0.0022 (sixth row from the bottom), and not significantly different from zero. In the same panel, the fourth row shows the estimated change for parcels not in any of the urban village or urban village-like groups (the "all other" group). These parcels also saw essentially no change in permits.

The confidence intervals suggest that most of the estimates cannot rule out the null hypothesis that there was no difference in the change in permits in urban villages compared with the other comparison group. Within the urban village category, the earlier horizon parcels and the parcels in urban villages with approved plans did not see significantly different changes in permits.

The other panels in Fig. 3 present the estimates for the same groups, but with different time periods and with multi-family instead of commercial parcels. The "whiskers" that define the confidence interval of the point estimate vary because the different groups have different numbers of parcels and because there is different variation within the group. The specific plan group, which has a smaller number of observations than other groups (only 150 commercial parcels), typically had the largest confidence interval. The precisely estimated zero effects– with standard errors of zero as well– for some of the regressions are an artifact of samples in which the dependent variable was 0 for all observations, as indicated in Table 3 above.

We now turn to more discussion of each group of estimates noting some of the estimates of interest. The overall picture is that the urban village area had neither sizable nor significant effects, for the given outcome measures.

5.4. Construction permit activity

The outcome variable is a dummy variable equal to 1 if any new construction permit was issued for a parcel during the specified period. For the entire period 2005–19 this measure comes from the actual permit file provided by the City of San Jose.

The left panel of Fig. 3 was discussed above. Consider then the right panel in the figure, which reports the outcome for the 2014–19 period for multi-family parcels using the consistent parcel groups. Recall that the "large" consistent parcel aggregates outcomes for all parcels that were linked by a split or merge at any time over the 2005–19 period. The first row shows the average change in the probability of a construction permit for parcels located in urban villages (regardless of horizon, so including horizons 1, 2, and 3), from the 2006–2010 period to the 2014–2019 period. Such parcels saw the numbers of permits drop by a very small and statistically insignificant amount (the hypothesis that there was no decline cannot be rejected as the confidence interval includes zero). The red vertical line represents the point estimate for the synthetic control group of parcels (in the second row). As can be seen, these parcels saw permits decline by about −0.002, on average, and the confidence interval again includes zero. In the same panel, the fourth row shows the estimated change for parcels not in any of the urban village or urban village-like groups (the "all other" group). These parcels saw likewise essentially no change in the pace of permitting.

Overall, the point estimates and confidence intervals indicate that the urban village areas were not very different from the control areas, the specific plan areas, and the "all other" areas. The change in permit numbers were small, suggesting that the period after 2011 when the General Plan including the urban village was approved saw little increase in construction activity across the city. Urban villages were not significantly different (neither more nor fewer construction permits) compared to the rest of the city.

5.5. Revaluations/transactions

The outcome variable of transactions is a dummy variable for whether the parcel was revalued or transacted during the specified period. For 2014–19 this measure comes from the actual transaction file provided by the Assessor. For the pre period (2006–10) and the first post period (2011–15), we inferred transactions or development according to whether the annual assessed value increased by more than the typical 2% permissible by law, or the parcel was involved in a split or merger identified in the assessor's crosswalk of parcel numbers (see Appendix G for more details on our process for coding assessor revaluations). Because

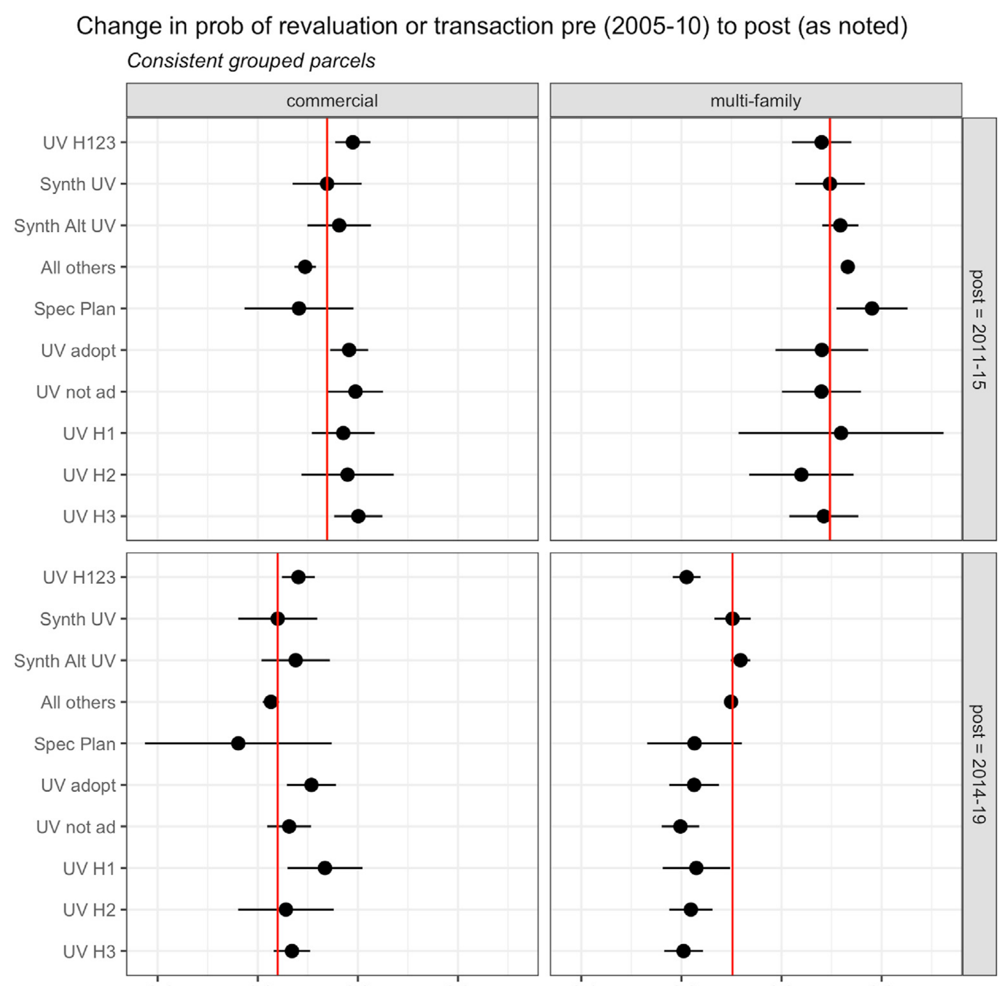

Fig. 4. Coefficients (β_j in equation (2)) and confidence intervals for regressions estimating change in whether parcels underwent an assessor revaluation or transaction between pre and post periods, for defined areas of San Jose. Confidence intervals are plus or minus two standard errors, where all standard errors are clustered at the zone level. Panel is based on the consistent parcel groups method of creating consistent parcels over time.

property revaluations greater than 2% can occur for reasons other than sales (such as improvements that increased a structure's square footage), the variables for revaluations and actual transactions are not strictly comparable. Consequently, the comparison between the 2006–10 pre-period and the 2014–19 post-period must be treated with some caution. There is little reason to think that the change in measurement would affect treated and untreated areas differentially, so we surmise that the comparison across coefficients should still be informative (i.e., the measurement change might likely be "differenced out").

Consider the left panel of Fig. 4. The estimates are for commercial parcels. The first row shows the average change in whether a parcel experienced a revaluation for parcels located in urban villages (regardless of horizon, so including horizons 1, 2, and 3), from the 2006–2010 period to the 2011–2015 period. Such parcels saw essentially no change in this variable, on average. The point coefficient is slightly below zero and the confidence interval is relatively small. The red vertical line represents the point estimate for the synthetic village control group of parcels (in the second row). As can be seen, these parcels saw the probability of a revaluation decline by about −0.06. The "all other" group parcels saw a somewhat larger decline in whether a parcel transacted, −0.11. The confidence intervals suggest that most of the estimates (except for the "all other") overlap (and formal hypothesis tests confirm this).

For the bottom right panel in Fig. 4, which reports the outcome for the 2014–19 period for multi-family parcels, the change in the probability of a transaction for parcels located in urban villages (regardless of horizon, so including horizons 1, 2, and 3), from the 2006–2010 period to the 2014–2019 period was estimated to be a decline of about −0.18, on average. The red vertical line represents the point estimate for the synthetic village group of parcels (in the second row). As can be seen, these parcels saw a decline of about −.10 in the probability of a transaction. The urban village parcels were less likely to have transacted. For the other comparison groups, the point estimates and confidence intervals are very similar.

Overall, the confidence intervals indicate that for commercial parcels the urban villages and synthetic village areas were quite similar for the change to the 2011–15 period, but the "all others" and specific plan areas saw fewer transactions in the 2014–19 period. For multi-family parcels the pattern was quite different, with the urban village areas seeing declines in transactions relative to smaller declines for the synthetic village areas. The specific plan areas of the city saw even larger declines. The evidence is mixed, then, as to whether the urban villages saw more real estate transactions: perhaps more commercial transactions and fewer multi-family transactions.

5.6. *Major projects*

Fig. 5 reports the outcome for a dummy variable equal to 1 if the parcel had a major project during the relevant period. Each year, San Jose city staff compile a list of "major projects" that is reported to the City

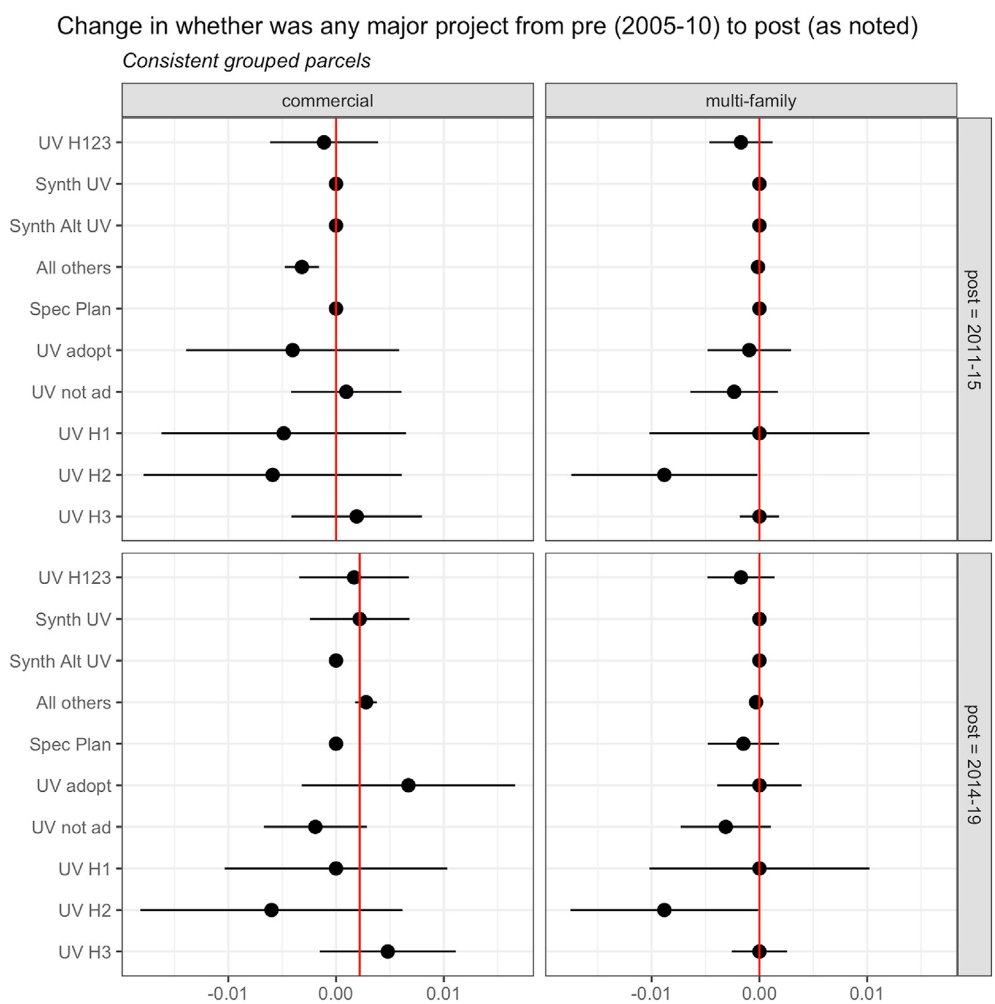

Fig. 5. Coefficients (β_j in equation (2)) and confidence intervals for regressions estimating change in whether parcels had a major project between pre and post periods, for defined areas of San Jose. Confidence intervals are plus or minus two standard errors, where all standard errors are clustered at the zone level. Panel is based on the consistent parcel groups method of creating consistent parcels over time.

Council, as noted above. Occasionally some projects are built in phases, and so a parcel might have two major projects over a five year period. In all cases, we assign the first year the major project appears on the list (usually in the category "under consideration" or "permit pending") as the "date" of the project. The reports will repeat many projects as they go through phases of approved permits, beginning construction, and completed construction. The data from these reports is turned into an outcome variable of whether there was a major project for a parcel for the specified period.

The estimates in Fig. 5 for multi-family parcels all include zero in the confidence intervals (except for H2 in the large consistent parcel aggregation where the confidence interval is large and just below zero). Precise zeroes again occur when none of the parcels in the relevant area had a major project (see Table 3 above). Overall there were few major projects relative to the number of parcels, and major projects were initiated and built over decade-long horizons. So the null hypothesis that there is no change in the number of major projects over various five year periods was unlikely to be rejected. For commercial parcels, the estimates have wider confidence intervals, and again almost all include zero.

There is no evidence, then, that urban villages saw more major projects than other areas in the periods after the 2011 General Plan was adopted.

5.7. Assessor valuation

For property values, we define the value for the five-year span as the maximum assessed value of that parcel across the five years of data; in many cases this will be the value of the parcel in the last year of the period, as overall values were generally rising over time, although the financial recession of 2008–10 meant that many assessed values declined in the period 2010–12. Using the mean value over the five years, rather than the maximum value, does not substantially alter the results. We take the natural log of the value. The estimated coefficients presented in Fig. 6 are then interpreted as approximately the proportional change in the value between the pre and post periods.

Mechanically, because most assessed values rise automatically at 2% per year, we should expect positive coefficients in Fig. 6 for most of the areas. Since the average values of parcels are on the order of $1 million, 2% over five years corresponds to increases of about $100,000 in assessed value, and the default change from the 2005–10 period to the 2014–19 period would be about double that, or $200,000. Indeed, the estimated coefficients for that "post" period are always larger, and sometimes significantly, than zero.

For commercial parcels (the panels on the left), there appears to be no difference in the change in valuation for urban villages compared to the control areas. For multi-family parcels, when comparing the 2014–19 period with the 2005–10 period, the assessed valuation of the urban village parcels increased by less than the synthetic village control areas.

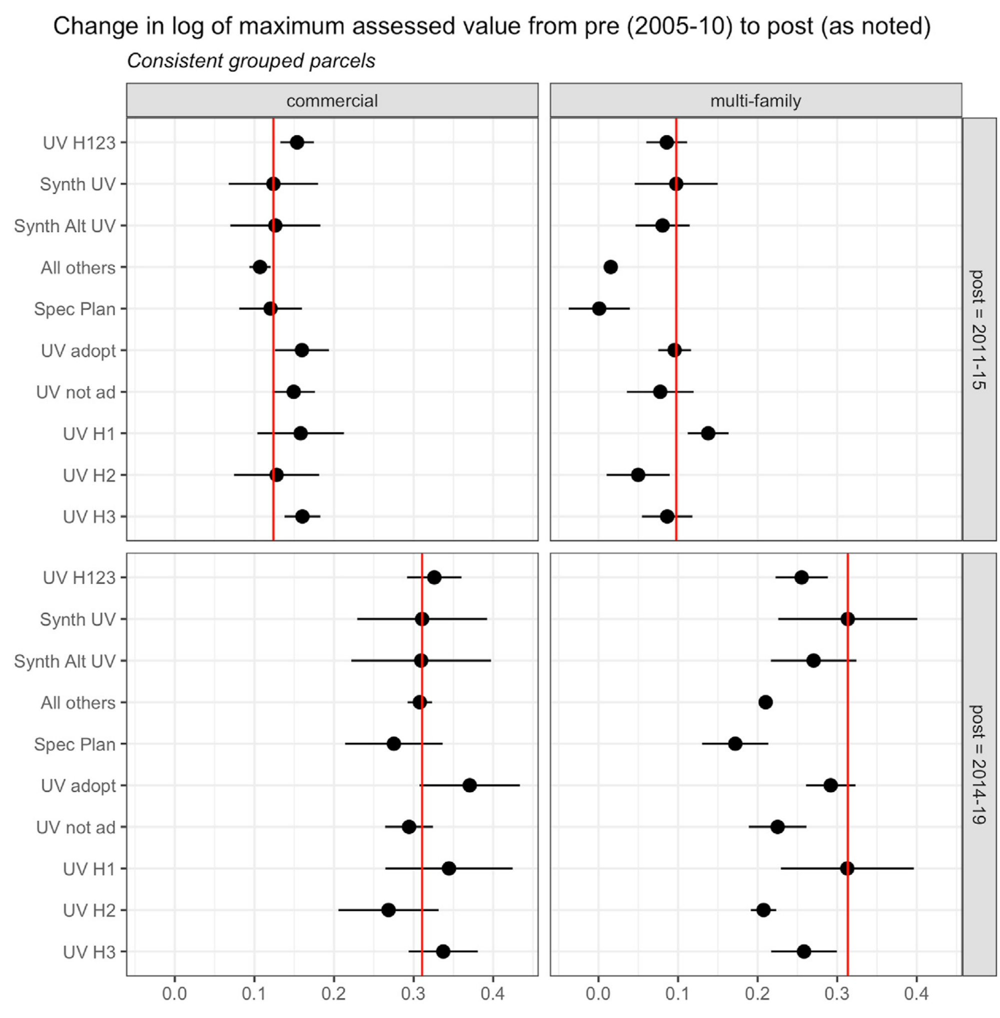

Fig. 6. Coefficients (β_j in equation (2)) and confidence intervals for regressions estimating change in log of maximum assessor valuation of parcels between pre and post periods, for defined areas of San Jose. Confidence intervals are plus or minus two standard errors, where all standard errors are clustered at the zone level. Panel is based on the consistent parcel groups method of creating consistent parcels over time.

The confidence intervals overlap somewhat however and so the differences are not statistically significant. Multi-family parcels in specific plan areas seemed to not have increased in assessed value as much as in other zones.

5.8. *Overall assessment of impact of urban villages*

The sparseness of many outcome variables as displayed in Table 3, the relative absence of statistically significant coefficient estimates in the difference-in-difference estimates reported in Table 4, and the overlapping confidence intervals for the estimated coefficients in the pre-post regressions, together suggest little evidence of sizable impact of the urban villages compared with other comparable areas of the city. There are a few cases where urban villages have statistically significantly different outcomes from comparison groups. The differences are small however.

The plethora of estimates illustrates the “garden of forking paths” dimension to econometrics: there are numerous ways to create consistent parcels that can be used to estimate impact analysis, numerous ways to define treatment and control groups, and numerous alternative outcomes to examine. Many published papers in this field do not, however, report the exact procedures used to create parcel samples, nor do they examine the robustness of their estimates to alternative aggregation procedures.

5.9. *Pre-trend checks*

The difference-in-difference strategy for estimating treatment effects relies on the assumption that the treated and control groups would have had similar counterfactual experiences (in terms of the outcomes) if there had been no urban village strategy, and thus share a common trend. This assumption is not testable, but an informal approach that has become standard practice is to check the “pre-trend” behavior of the outcome variables. To do this, we estimate and plot the interaction between the treatment group dummy and individual year effects. A visible trend in this treatment-control differential prior to the treatment event horizon (in our case, 2011) would suggest differential pre-trends, violating the common trends assumption.

Fig. 7 presents these plots for two of our treatment variables: the revaluation/transaction dummy and the log of assessed value, respectively. Plots for the construction permit and major project interactions are uninformative at annual frequency, given the sparseness of these events in our samples (see Table 3). Fig. 7 uses the consistent grouped parcels; figures for the two other methods for creating consistent parcels using 2005 as base or using 2019 as base are in Appendix H.

As can be seen in the left column, the treatment effects for revaluation probabilities in multi-family parcels appear to exhibit a downward pre-trend in the years leading up to 2011. This suggests that the significant negative difference-in-difference effect of urban villages on transactions of multi-family parcels in our pre-post comparison should be taken with a grain of salt. It may be that for reasons unrelated to the urban village plan, transactions were already declining more quickly or increasing less quickly in what would become urban village areas relative to similar synthetic villages.

The right column of Fig. 7 repeats the exercise for the log of assessed value. Clearly, no differential trends or significant differences between treated and untreated parcels are evident.

6. Conclusion

Political leaders and ordinary residents of San Jose, as in other cities, believe that the housing crisis in growing metropolitan areas necessitates significant changes in the set of policies that shape the extent and nature of the private and public provision of housing. Upzoning has been a preferred strategy of many cities. The analysis presented in this paper describes and estimates the impact of San Jose’s urban village strategy. The urban village strategy was adopted in 2011, and involved designating 68 areas for upzoning.

The first contribution of this paper is to present empirical estimates of impact that suggest that urban villages did not lead to significantly more development in the eight years following the adoption of the strategy. We used several measures of development-related outcomes: construction permits, transactions, changes in assessor property valuations, and major projects. Since many of the measures have measurement error, we believe that triangulating multiple indicators may be a better approach than focusing on a single indicator. The estimation strategy adopted in this paper was to compare outcomes in zones designated as urban villages

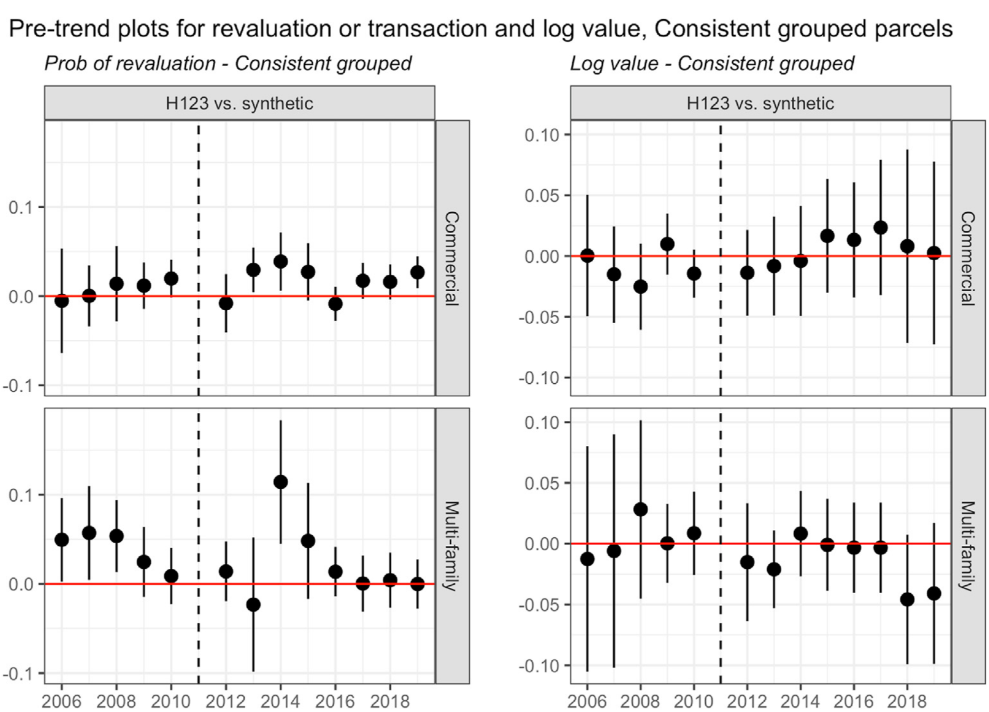

Fig. 7. Pre-trend graphs of estimates of the interaction of year with treatment category using annual data, comparing revaluations or transactions (left column) and log of assessed value (right column) in urban village H123 parcels with synthetic villages. Data restricted to consistent grouped parcels. Confidence intervals are plus or minus two standard errors, where standard errors are clustered at the zone level. Panel is based on the consistent parcel groups method of creating consistent parcels over time.

with comparable zones (with different designations or later designations) that serve as appropriate controls or counterfactuals. Since urban village designation was not random and may have responded to characteristics of the zones themselves, the effects of urban village designation are estimated through a difference-in-difference strategy, where the change in outcomes in urban villages, from the period prior to designation to the recently available time period (including possible changes in trends of outcomes), are compared with the changes (or changes in trends) for the control group (for which there are two possibilities: urban villages whose implementation was delayed, or "synthetic" urban villages created through a simple algorithm).

The urban villages areas were not randomly selected from all possible areas with commercial zoning. The city council and general plan team sought to spread urban villages throughout the city, encourage development near existing transportation infrastructure, and designate at least one urban village within walking distance of most city residents. Because there was considerable endogeneity in the designation of zones as urban villages and the assignment of horizons to the 68 urban village areas, most estimation strategies will generate biased estimates of the impact of urban village designation. This is the significant methodological challenge of estimating impacts in complex urban planning environments. This caution applies to anecdotal and expert evaluations of the impact of urban villages as well as to quantitative evaluations. Anecdotal and expert evaluations may be, however, prone to other sorts of bias in both assessment and interpretation, as imprecision in measurement, unclear sample selection, salience bias, and unstated assumptions permit wide latitude for drawing conclusions.

Our main quantitative findings that the urban villages saw little development, using reasonable comparison areas as the counterfactuals, are largely in agreement with some expert assessments. A report by SPUR, an influential urban planning advocacy group, contributing to the 2019 public process of revising the General Plan, noted that: "SPUR consistently heard in interviews that developers are actively avoiding urban villages and instead seeking out opportunities downtown, in North San Jose or in other cities such as Santa Clara, Sunnyvale, Mountain View and Redwood City. Many have given feedback that it is the urban village framework and process, rather than the locations or the real estate market, that are the biggest deterrents …. Unfortunately, the seven most recent urban village plans took, on average, four to four and a half years to complete, far exceeding the current goal of one year. In addition, the plans themselves do not provide any streamlining or improvements to the approvals or permitting process" (Wang, 2019, p. 6).

Expert analysis also points to other reasons why there was little impact of the urban village strategy. The efficacy of upzoning in leading to residential development and changes in the affordability of housing is likely to vary according to general conditions of the profitability of development. It has not been clear that residential housing development in San José (with high sale prices) has been more or less profitable than in other metropolitan areas (with low sales prices). Keyser Marston & Associates (KMA) conducted an analysis of market conditions for high-density apartments for the San Jose City Council and presented at a council study session on the city's residential housing strategy in April 2018 (City of San Jose, 2018a. The report presented estimates of per-unit project value and project cost for 10 prototypical developments across five San Jose sub-regions: downtown core, North San Jose, West San Jose, Central San Jose, and South/East San Jose. Each region was paired to a project type, ranging from 20 to 25 floor high-rises (downtown), to 7-floor mid-rise (West, North, Central) and 5-floor low-rise buildings (South/East). For each sub-region, projects had their development values estimated based on average rents of real, adjacent properties. For example, average rents ranged from a per-unit value of $2750 in South/East San Jose to $3450 in West San Jose (Stevens Creek). KMA estimated the cost of development including land costs, city fees (affordable housing fees, park fees, permits, etc.), and construction costs. Profit margins were estimated by subtracting per-unit development costs from per-unit values. According to KMA, only the Central San Jose and West San Jose development projects had positive profit margins (3% and 19%, respectively).

The second contribution of the paper is methodological. We have tried to be transparent about two methodological points that are essential elements to analysis of urban planning strategies. The first is that there are several choices for constructing consistent panel data on urban development. Depending on the nature and rapidity of development, different choices may generate different impact estimates. As we have seen, three alternatives could be carried out, at a minimum: using the initial parcel set as the sample; using the final parcel set as the sample; and using the set of smallest consistent parcel groups as the sample. For San Jose, the choices were largely congruent in their estimates, but there is no theoretical reason why they have to generate impact estimates of similar magnitude.

The second methodological point is that there are numerous interesting and relevant outcome variables, and a "garden of forking paths" emerges in the many decisions researchers have to take to construct variables (Gelman and Loken, 2014). The question of permits is instructive: the city's database of permits includes permits of many types, and sorting the permits into broad categories that are useful for analysis involves many choices. Should permits to repair building faults identified by inspectors be treated as "development" or be excluded? Are permits to re-roof to be grouped with permits for new construction? Moreover, multiple outcome measures and definitions raise flags of multiple testing problems: it is well-established that researchers are biased by the professional value of "statistical significance" and decisions about when to "stop" data analysis are asymmetric: null findings prompt more analysis, significant findings dull the incentives for more analysis. Researchers should endeavour to make data available upon request so that other researchers can examine the robustness of findings to alternative definitions and specifications. The appendices to the paper discuss in considerable detail the many choices made, and R code for the analysis is available.

A final contribution of this paper is to discuss in detail what the "urban villages" strategy actually entailed, and suggest some improvements for cities and voters considering similar strategies. The description of the urban village strategy makes clear that it was not a simple upzoning. The process of approving urban village plans, adopting a framework for the upzoning process, and facilitating community engagement throughout the process proved cumbersome. By 2019, only a fifth of the 68 proposed urban villages had approved plans.

We conclude with the caution that while there are significant challenges to data-based econometric analysis of the impact of urban village and similar urban planning strategies, these analyses should be included in public discussion of the success or failure of urban planning strategies, which perhaps too often are guided by selective and salient anecdotes about particular development projects or other outcomes.

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Author statement

All authors contributed equally to the conceptualization, data gathering, analysis, and writing of the paper.

Data statement

The data used for the analysis in the paper is a combination of publicly available data taken from City of San Jose websites and of purchased data from the Santa Clara County Assessor Office. Data from the Assessor Office is restricted. The authors are happy to work with the Assessor Office to determine if the data can be made available for replication studies.

Author contributions

C.J. Gabbe, Michael Kevane, William A. Sundstrom: The authors contributed equally to all aspects of Conceptualization; Data curation; Formal analysis; Methodology; Project administration; Visualization; Roles/Writing - original draft; Writing - review & editing.

Declaration of competing interest

None.

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Appendices A-H. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.regsciurbeco.2021.103648.

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