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Is it clever to be dense? Evaluating the consequences of urban growth boundaries on the cost of housing through a difference-in-difference methodology

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Abstract

With the advent and popularization of faster travel modes in the last centuries, cities started to see more of the phenomenon called "Urban Sprawl." While hard to define empirically, urban sprawl broadly refers to the growth of the urbanized city limit, characterized by low density at the city's edge. However, such a mode of urban development is said to have many negative consequences, as such, in the last three-quarters of a century, several measures were put in place to contain the phenomenon.

One possible effect of such measures is a decrease in housing affordability. As most Western nations seem to struggle with housing affordability, this is a question of supreme social relevance, such an effect would be a relevant decision factor for stakeholders. For this reason, guided partly by the Alonso-Smith-Mills Monocentric model of the city, this paper discusses some possible mechanisms for such an effect. Further, it utilizes a difference-in-difference technique to ascertain whether such an effect exists and approximate its magnitude. For such analysis, the United States Federal Housing Finance Agency House Price Index (HPI) estimated housing affordability.

The HPI is an index used to evaluate changes to the cost of a single-family home in any given geographical area within the United States adjusting for property characteristics. The treated group will be the state of Maryland, where the passage of the ""Smart Growth" and Neighborhood Conservation - "Smart Growth" Areas" Act severely limited the city-level urban planning commission's ability to develop rural land, thus physically constraining the growth of the rural-urban boundary. The analysis does show a significant positive Increase in the House Price Index in the aftermath of the passage of the law. However, some unique characteristics of the dataset do raise internal validity concerns.

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Introduction

Urban Sprawl, the kind of development characterized by rapid, sparse, and car-dependent growth of the geographical boundaries of an urban area, has become the de facto standard mode of growth of urban environments over the past 100 years. This phenomenon is often criticized for its large negative externalities, as well as the additional non-financial cost it imposes on its residents (Sturm & Cohen 2004) (Zhang 2021). As such, a myriad of sprawl containment strategies have emerged as public policy in the last decades. However, as is true for all public policies, there are downsides to such implementations. This paper focuses on the creation of urban growth boundary policies, specifically the application of the Maryland "´ Smart Growth and Neighborhood Conservation - ´Smart Growth´ Areas"(Henceforth sometimes referred to as the Smart Growth Areas Act). Specifically focusing on its consequence in the housing market of the state of Maryland in the United States of America (US).

This paper aims to answer the question: **"What is the impact of the Passage of the Smart Growth Areas Act on the cost of housing in the state of Maryland?"** It further postulates the hypothesis that **the passage of the Smart Growth Areas Act resulted in a higher cost of housing in Maryland.** Lastly, it attempts to gauge the magnitude and the mechanisms behind that effect.

The effect can be theoretically explained by three mechanisms, firstly by an increase in housing demand, secondly by a constraint in total supply, and lastly by a constraint these laws apply to the supply-side elasticity of housing. The first mechanism is perhaps the simplest to explain, as it is a direct byproduct of the ills of sprawl. If it is true that the inhabitants of sprawling metropolitan areas suffer from the externalities of such a mode of housing solution, then it must hold that people are willing to pay a premium to a city not plagued by the issue. The other two mechanisms are more theoretically complex, and somewhat harder to discern, given that they are both supply-side constraints. For the first mechanism, the total possible supply of housing is constrained by the implementation of a boundary, given that a smaller city requires higher density and thus will have a lower quality-adjusted supply of housing competing for the same capital pool. In the last mechanism, the effect generated by the smaller ability of the housing supply to react to demand spikes. The theory behind such an effect is that the housing market in more dense areas is more likely to depend on large time and capital investments, making them riskier and less attractive for builders (Glaser et al 2006).

To ascertain the existence of such an effect, the thesis will use the House Price Index (HPI) published by the United States Federal Housing Finance Agency (FHFA)[1]. This index utilizes a non-hedonic approach to housing quality adjustment and utilizes it to measure changes in the cost of housing within several US-based geographies. For this paper, the non-seasonally adjusted, quarterly publication of the index aggregated at the state level was utilized.

Using such data, a difference-in-difference analysis was performed to evaluate the effect of the passage of the Smart Growth Areas Act when compared to the control the state of Vermont. To evaluate the usefulness of such a control, two separate tests were performed to ensure the pre-intervention trends are parallel, firstly, the direction of the trends is checked visually and they appear to be parallel. Further, the trends are checked empirically by utilizing leads.

This evaluation allows the paper to affirm the alternative hypothesis, implying that if the results are externally valid, the implementation of an urban growth boundary causes a statistically and practically significant increase in the cost of housing.

Relevance

In the aftermath of the popularization of the automobile, distances shortened, and people traveled further, because of this we live in a new paradigm where proximity to the city center is less valued than in ages past, and people commute more(Glaser 2004). This, in turn, leads to the kind of low density that is now sometimes called “Urban Sprawl”. In a 2003 paper, the phenomenon is defined as follows: "Sprawl is low-density, leapfrog development characterized by unlimited outward extension" (Burchell and Galley 2003).

Since the term's popularization, much public and academic discussion has taken place trying to determine the positive and negative effects of this phenomenon. In general, the term was conceived with the intention to carry a negative connotation, and thus, those who believe it to be a net negative for society have dominated most of the academic and social discourse around it. The United Nations, for example, notes critically that “African cities will more than double in population up to 2050, and their spatial extent will likely grow three times, aggravating urban sprawl and mobility” (UN HABITAT). Based on that observation, the

[1] At the time of the first publication of their HPI the Federal Housing Finance Agency was called Office of Federal Housing Enterprise Oversight (OFHEO), however to avoid confusion, it was referred to as FHFA throughout the entire text.

United Nations further calls on nations to change their urban growth patterns as "Cities require an orderly urban expansion that makes the land use more efficient by planning for future population growths" (UN HABITAT). Of course, the United Nations is not in fact the only organization that has a critical view of sprawling urban environments; they are joined by the European Union, the World Economic Forum, as well as a veritable litany of local and national governments.

The reasons for such maligning are varied, both in the logic behind them, as well as in the exact complaints. The Brookings Institute, a politically focused left-of-center US Think Tank, for example, notes that sprawling cities (and specifically the higher amount of transport they require) are an environmental hazard. While adding that "Simply put, the United States cannot reach its [greenhouse gas emissions] reduction targets if our urban areas continue to grow as they have in the past" (Tomer et al 2021). Similarly, a 2021 paper found that excessive sprawl had a significant effect on Environmental Pollution (Zhang 2021).

Separately, a 2004 paper on the relationship between sprawl and health found that "Sprawl significantly predicts chronic medical conditions and health-related quality of life" (Sturm & Cohen 2004). Furthermore, this mode of urban growth is associated with a litany of negative consequences, and while its critics vary in the quality of provided evidence, the sheer number and variety of them are still notable.

As such, it is not surprising that stakeholders have begun to combat the phenomenon. Notably, local governments have implemented a cornucopia of methods to assuage the issue. Amongst the bluntest of these instruments is the outright prohibition or significant obstruction of growth outside the currently developed urban areas. The United States State of Oregon, for example, drew a hard boundary around its cities and prohibited the urban development of land surrounding it in its 1973 Land Conservation and Development Act (Land Conservation and Development Act 1973).

Across the country, in the state of Maryland, the government implemented a slightly less restrictive approach. Instead, combatting growth by limiting the deployment of state funds in areas deemed not to be either already developed or strategically relevant by the legislature ("Smart Growth" and Neighborhood Conservation - "Smart Growth" Areas, 1997). However, one possible consequence of such policies is the decrease in housing affordability. This effect may be achieved by restraining the creation of new housing to more expensive redevelopments of existing structures reducing total supply and supply elasticity of housing, as well as, perhaps, by increasing the desirability of living in the area.

If such an increase in the cost of housing is indeed a consequence of sprawl control policies, these otherwise reasonably popular policies can have quite severe negative consequences and thus should be subject to more thorough public scrutiny. Especially given that, the cost of housing is a reasonable, and growing, public concern. Ironically, some of the same organizations that heavily criticize sprawl-oriented land use are also sensitive and attentive to such housing costs. The World Economic Forum, for example, expects that “By 2025, 1.6 billion people are expected to be affected by the global housing shortage” (World Economic Forum 2021).In short, this question of whether these policies are negatively affecting housing affordability is imminently reasonable.

For this reason, this paper will attempt to ascertain whether such an effect exists. To do this, we will use a house price index indicator provided by the United States Federal Housing Finance Agency .Such a data series will be subjected to difference-in-difference analysis in an attempt to ascertain the impact of the passage of the Smart Growth Areas Act. Further, this paper will propose mechanisms for such an effect, and lastly, it will attempt to ascertain which causal mechanism is most responsible for the observed effect.

Literature Review

While empirical research on this topic is not lacking, it is also not particularly plentiful, further, some of the reference papers on the subject are relatively old, and thus not up to current econometric standards. As such, it is valuable to review not only the literature made with respect to urban growth boundaries but also some of the general economic literature on sprawl control.

A more in-depth review of economic theory will be found in the Theoretical Mechanisms for rising cost of housing section. For the section at hand, the plethora of studies on the subject performed or sponsored by special interest groups were not considered.

In the paper Estimating Effects of an Urban Growth Boundary on Land Development, the authors utilize a linear estimation equation to evaluate the effect of the implementation of an urban growth boundary on the likelihood of development of land parcels. The analysis is made utilizing the county of Knox in the United States state of Tennessee as a case study (Cho et al. 2006). The authors verify that land parcels within the urban growth boundary but outside the city of Knoxville were less likely to be developed; however, those within Knoxville were more likely to be developed. While not directly related to price, the findings are relevant to understanding housing supply within an urban growth boundary. However, the evidence is contradictory, with the direction of the effect seemingly determined by the position of the city limits, which are smaller than the growth boundary. The reason for the divergence postulated by the authors, that it is "related to the fact that the city government had the right to annex land parcels within the UGB boundary without the consent of land owners." (Cho et al. 2006) is unique and thus calls into question the external validity of these findings.

Using a conceptually similar approach to the one that will be employed for the present analysis, in 2014, Michael Ball and his colleagues measured the impact of an implementation of urban growth boundaries in the Australian city of Melbourne. Using regression models for house prices, they measured the price of land inside and outside of the Urban Growth Boundary in Melbourne, authors found while the parcels inside of the boundary were already more expensive in the pre-intervention period, they rose further in the post-intervention period (Ball et al 2014). The estimated average treatment effect in this study is 65% of house prices, which is quite large.

The certainty of the results in the Ball et al paper can be called into question in both directions. For instance, part of this effect may be the permanent devaluing of land outside of the boundary, as that land is now severely restricted, this fact again, raises some validity concerns. Similarly, Woodcock and his colleagues point out that at least some of the effect arises from particularities of the housing market of Melbourne that arise from a combination of sprawl control policies and the politics of the Melbourne populace (Woodcock et al 2011). However, it is also reasonable to assume that the value of developed land outside of the urban growth boundary would rise after its implementation as the difficulties of developing such land create some artificial scarcity, which would move the point estimate toward zero.

A different but fundamentally similar approach was utilized in the paper: *Impact of Urban Growth Boundary on Housing and Land Prices: Evidence from King County, Washington*. In it, the authors reach a similar conclusion about land prices, estimating a 230 percent increase in prices, however, they estimate a 13 percent decrease in the costs of housing using the same method (Mathur, S 2013).

Another approach that can be utilized is comparing different cities in close proximity and measuring their housing market variance in a differences-in-differences approach. Results here are quite mixed, in the US for example, a study of the city of Petaluma, California found an effect of between five and nine percentage point increase in house prices, depending on house sizes (Schwartz et al 1981). While a study of the city of Boulder in Colorado found a much less practically significant, but still statistically significant effect of 3% across all housing(Miller 1986).

In yet a third approach, Michael Elliott used whether there were growth controls at the city or county level as an explanatory variable in a regression for growth in house prices over a 6-year period. The analysis was conducted for cities in California and found that both levels of regulation added to house price raises at a statistically and practically significant level (Elliott 1981).

In yet a different approach, Justin Philips and Eban Goodstein attempt to model house prices across California utilizing linear regressions with demand and supply side explanatory variables (Philips & Goodstein 2000). Here the authors conclude that while there was indeed some effect of implementation of urban growth boundary, most of the movement in that particular city came from "Catching up" to the previously more expensive real estate housing market of the west coast of the United States. However, the variables utilized in this paper are quite limited[2], and the possibility of some form of omitted variable bias is not properly addressed.

In short, the literature on this topic is quite varied in methodology, all methods have their own pros and cons, and nearly all bring in some validity concerns. However, most of the researchers generally agree that the effect of an increase in house prices exists; however, there are exceptions, with a few researchers proposing a negative effect on house pricing.

Theoretical mechanisms for rising cost of housing

There are many plausible mechanisms for an increase in rent after the introduction of a city boundary policy, in this paper; three such mechanisms will be evaluated. Firstly, if it is true that increased density is a net positive, then it must be true that less sprawling cities are more attractive, which could generate a demand-side shock to the price of housing. However, the increase in price could also be a product of a supply-side shock, as the city can physically fit less housing, and thus the housing becomes proportionally smaller while still competing for

[2] Variables included Population, median income, unemployment, median house price, median house price growth, climate mildness, the number of municipalities in the metro area, land availability and an index for regulatory burdens.

the same capital pool. Lastly, another supply-side mechanism for the development of such a trend is that new units could be made more difficult, time-consuming, or capital-intensive to build, thus reducing the supply-side elasticity, and the market responsiveness to increased prices, therefore, it drives up the equilibrium price of housing, especially during times of increased demand.

Firstly, it is reasonable to evaluate the demand side proposition, as it was established above in the paper; research indicates that urban sprawl has negative effects on the residents of such areas, as well as for residents of adjacent urban areas. As such, if living in an area that is more sprawling increases your risk of Arthritis, Chronic Lung Disease, Migraines, and Urinary tract problems (Sturm & Cohen 2004), then it logically follows that people would be willing to pay a premium to live in denser areas. If that is indeed the case, it is not exactly a judgment leap to state that this would cause a rise in the price of housing, given the housing supply is relatively inflexible by nature.

A paper titled “Planning policy, housing density, and consumer preferences” finds some interesting, but contradictory evidence in this respect, as authors find that “in some markets, there is a premium for both lower and higher density options, whereas in other cases (in London) the premium is for medium-high or high density.” (Dunse et al, 2013). In short, while there is some evidence that the effect of valuing density may exist, in some cities, it is also true that, for some reason, people prefer to live in lower-density areas.

For the other two mechanisms, it is relevant to introduce the Alonso-Muth-Mills model of the city. The model is quite restrictive, but nonetheless, it is quite useful for modeling urban development, it assumes a city is an agglomeration of residences around a central business district (CBD); each worker starts his day at his house, and then travels to his job at the central business district. Further, every citizen has the exact same preferences, which can all be precisely explained by a Cobb-Douglas utility function. Lastly, those preferences are as follows: generally, the workers desire to have the least commute time, so they wish to relocate as close to the CBD as possible. However, this causes a demand in the center that leads to increased prices, and thus they slowly move outward adjusting for the cost of commuting. However, the lower cost of housing is not the only benefit of living in the outskirts, as the housing supply is also responsive; the areas near the center are more densely populated, and thus smaller and less desirable. Thus, three factors, his cost of commute, his housing cost, and his density determine any given workers’ utility.

Further, the model provides for an area of rural land around this city, this area’s value is generally held at a constant, not impacted by the distance to the CBD. In effect, this makes it so the city's growth will seize at the geographical spot where the value of land as a city is equal to its value as cropland. This model is obviously a gross simplification of reality, but it does model cities in quite an accurate fashion and thus is still rather useful (Mariano Kulish et al 2012). Importantly, it allows us to model the behavior of cities by limiting the city boundary, as that can be done by simply capping the commute distance parameter.

Lastly, it is relevant to add to the model a factor to account for negative externalities. If the criticism of sprawl holds true, there is some socially shared cost to this mode of growth. This may manifest itself in the loss of public open spaces (Brueckner et al 2001) or in the excess cost to public coffers (Downs 1970). Thus, the welfare maximization policy may place some restrictions on sprawl.

With this model in mind, we can understand the two possible supply-side constraints that could cause an increase in house prices. Brueckner proposes the first in his seminal paper on the economics of urban sprawl, where he notes that “there is a danger that a [urban growth boundary] may be much too stringent, needlessly restricting the size of the city and leading to an inappropriate escalation in housing costs and unwarranted increases in density.” (Brueckner et al 2001).

Brueckner does imply that some of this loss could be regained by a decrease in negative housing sprawl externalities, such as infrastructure cost or loss of open space, and even that a perfectly set urban growth boundary may lead to a net increase in welfare. However, even under those conditions, it would still lead to a net effect of reduced housing-specific utility either from higher prices, from higher densities or from some combination of both.

To understand the argument set forth for this, it is helpful to imagine a city with no sprawl control policies. In it, the urban-rural boundary would be given by a point in which the value of the location as a city block is equal to its marginal agricultural productivity. Ergo, the property values allow for a maximum extraction of housing value. Allowing for unpriced externalities allows for a situation where in effect net total utility may not be at its highest, however, as a pure function of the decision makers' self-interest, it must hold true that a blunt growth boundary results in a net loss of housing specific utils for the decision makers under an equilibrium that promoted sprawl. In short, as Brueckner points out, an urban growth boundary will limit the net housing utility available to the city at the long-term market equilibrium.

Additionally, with respect to the expected rise in house prices, it is notable that the FHFA House Price Index only considers repeat valuations of the same single-family homes. Thus, the only manifestation of the effect will be the increase in prices, as any property can't change size or house more than one family and remain the sample (A more detailed description of this is given in the “model Introduction of FHFA HPI Dataset" section).

Further, in this context, another detail that was not considered by Brueckner shows up, cities grow in population with time, and the cost of transport, likewise, has historically tended to diminish. If that holds true, then wherever the ideal growth boundary is in an idealized model, this location will move with time. Because the ideal placement of urban growth boundaries is in part determined by the population of the city (Brueckner et al 2001), meaning that even a boundary that maximizes welfare when it is made will deviate from such placement with time. This concern may be assuaged by permitting the moving of the boundary placement after the passage of the law, but even in that case, it would be plausible that political pressures may prevent the growth of the boundary at the ideal rate.

Yet another mechanism for this possible increase comes from the elasticity of the supply of housing. Glaser and his colleagues, who state that “There is little argument that the less available land is, the more difficult it is to build” (Glaser et al 2006), lay the argument bare. The logic behind this argument is multifaceted. Firstly, they point out that it holds true that the per square meter construction cost is higher in multi-floor apartments when compared to ground-level construction (Glaser et al 2006). Such a price differential makes sense, as the first floor's structure in a one-story building must only sustain its roof. Whereas in a multi-story building, all floors must sustain the floors above it. Likewise, the utilities need more capacity.

While one may believe that some money is saved on land, this is only true if you ignore that economically, it is precisely an increased value of land that drives land density. Further, building is made difficult or more resource-consuming by the congestion that is inherent to densely packed locales (Glaeser et al 2006).

This increased cost of construction is not only passed on to consumers as a higher cost, but also, given the long-term nature of residential buildings as an asset, manifests itself in the ease of creation of new units, and the pace with which the market reacts to changes in demand. This manifests as a decrease in elasticity because more complex, capital intensive and time-consuming projects are riskier and thus at similar margins, are less likely to be pursued than simpler and more expedient ones, as such, housing supply in density constrained cities tends to react slower.

To measure and ascertain such an effect, Glaser and his colleagues use the time to permit, which is somewhat different from the motivator of increased prices studied with Urban Growth Boundaries. However, in case the basic concept and mechanisms proposed hold true, they must also have some effect on house prices for cities constrained by an urban boundary. If this effect is the dominant reason for house price increases, one would expect that the highest effect is seen in times where in unconstrained housing markets, the housing supply increased to compensate for increased housing demand.

Analysis

With these three theoretical mechanisms at hand, it becomes relevant to enquire about the house pricing consequences of urban growth boundary policies. While there are several options to pick, not all are perfect candidates for analysis. For example, the Oregonian version of the law was often criticized for having the boundaries be too large at the time of passage of the law (Brueckner 2001), as such, any effect would be perceived only as the city gradually reached the limits that were set for it.

For this reason, the state of Maryland, and its smart growth policies will be used as a case study. The implementation of the Smart Growth Areas Act was lauded as a successful implementation of sprawl control (Shen & Zheng 2007). It is precisely this effectiveness that allows this paper to perform an econometric analysis of the intervention to answer: **What is the impact of the Passage of the Smart Growth Areas Act on the cost of housing in the state of Maryland?** Based on the mechanisms outlined above, we can hypothesize the answer to that question is whether the **passage of the Smart Growth Areas Act resulted in a higher cost of housing in Maryland.**

The location of the effects felt by these mechanisms is also worthy of consideration. While one may naively expect that the best place to look for such an effect is the areas affected by the policy, this is not the case. Given that these areas are restrained from being developed as urban space, the studied question demands by construction that such areas will continue to be farmland, and thus will not be subject to urban price increases. Instead, the increased housing costs will be diffused throughout urbanized areas. These characteristics mean that a general index of house pricing would be the best tool to capture the effect of the intervention. For this paper, the utilized number will be the Federal House Finance Agency's House Price Index (FHFA HPI).

Smart Growth Policies

The analyzed Intervention will be the passage of the "'Smart Growth' and Neighborhood Conservation - 'Smart Growth' Areas"[3] Act by the United States State of Maryland. The act determines strategic areas for growth and then provides that, with certain exceptions, no state funding may be utilized in projects outside of these strategic areas. While it falls short of utilizing the blunt hammer of strict prohibition on the development of urban infrastructure outside of those areas, it in effect heavily restricts such developments by means of making them comparatively substantially more expensive.

Strategic Areas are defined by the act as follows: (1) Any currently incorporated part of a city with a density of no less than 2 units per acre, (2) Any future city development planned by the time of the passage of the law that is to be finalized in 6 years and will have a density of no less than 3.5 units per acre, (3)Areas zoned for industrial use, any "Enterprise Zone"[4] or "Heritage area"[5] and (4) a subset of geographically defined areas for future growth in the proximity of the largest urban environments of the state" ("Smart Growth" and Neighborhood Conservation - "Smart Growth" Areas, 1997). Summarizing, in the context of the act, a "strategic area", for housing is a currently developed area, an area soon to be developed, an outgrowth of their biggest cities, or a special zone. Thus, in effect, the law prohibits outgrowth from a designated urban growth boundary.

Any development outside of these areas will be severely restricted, with the state withdrawing its support. This includes funding support, defined by the act to include Loans, Loan guarantees, Reduction of loan interest on third-party loans, Tax Credits, as well as any assurance or direct grant. Further, such projects are excluded from some forms of material support such as construction, acquisition, or planning and architectural support, as well as the development of interstate infrastructure surrounding the areas of new growth ("Smart Growth" and Neighborhood Conservation - "Smart Growth" Areas, 1997). In effect, this

[3] The usage of quotation marks in the bill title has been kept from the original bill name where the title is written out in full, but stylized for ease of reading for where it appears in shorter forms
[4] Enterprise Zones are specially selected areas earmarked by the governor for economic development of strategic industries, and are generally not residential areas
[5] A heritage area is an area of historical significance, in effect "Developing" a heritage area most often does not mean building a city, but rather building academic and touristic installations.

significantly coagulates the state to the areas it already had developed in 1997. Especially given the restriction, which makes it very difficult to connect new areas of growth to the highway system.

In summary, the Smart Growth Areas Act works to set a soft boundary around the developments in the state of Maryland. However, while the dissuasion mechanism is not as blunt as similar policies, it is far more geographically restrictive than its more legally severe counterparts were. The Law does include a provision for the inclusion of new areas in funding "Under Exceptional Circumstances", that restriction does seem to have been maintained, and very few areas were developed. In effect, this made the act be considered a success amongst those who advocate for sprawl control (Shen & Zheng 2007).

Introduction of FHFA HPI Dataset

Keeping in mind the nature of the studied intervention, and the possible studied effect, it is valuable to describe the nature of the dataset, the House Price Index (HPI) from the Federal Housing Finance Agency (FHFA). The basic purpose of this index is to check raises in house prices within geographical areas in the United States. The specific version used for the analysis, which includes values for all 50 states and for the District of Columbia is published at quarterly intervals (Calhoun, 1996). The index calculation is based on repeat mortgage or sale evaluations of the same property.

The data utilized for this index is valuations provided by two providers of mortgaging services - Federal Home Loan Mortgage Corporation (Commonly, and henceforth referred to as Freddie Mac), and the Federal National Mortgage Association (commonly, and henceforth referred to as Fannie Mae). Included in the dataset are all single-family units financed at least two times by either Freddie Mac or Fannie Mae since the year 1975 (Calhoun, 1996). For this reason, while the dataset begins in 1980, its early years are based on a small number of properties, and are thus quite noisy, with a volatility that seems to be better explained by measurement errors than by variations of the underlying measured variables (See Figure 1). By the 1990s however, the dataset was far smoother and did not contain any visual tells of low sample sizes.

The reasoning for requiring at least two transactions is that "The use of repeat transactions on the same physical property units helps to control for differences in the quality of the houses comprising the sample used for statistical estimation. For this reason, the HPI is described as a "constant quality" house price index."(Calhoun, 1996) In short, the use of exclusively repeated sales as a manner of tracking price controls for the quality of housing without the need for adjustments by estimation.

Given that property characteristics are not recorded in the data and the same property may be refinanced after being extensively modified, data points are made to be given a sampling error that increases for higher time between transactions (Calhoun, 1996). What this means in practice is that a house that was last evaluated a long time ago and is thus likely to have had Construction or deterioration altering its quality, or to have had its location characteristics be substantially altered is assigned a greater uncertainty than that of a recently sold house. This largely solves the issue of home improvements if one keeps in mind that the use of mortgaging as a point of data collection makes it unlikely that the units are being bought and "flipped" in an observed short-term interval.

This evaluation data is then fed through a weighing mechanism, where any given sale is weighed by its uncertainty term and by a term accounting for the price volatility of that same property. The arithmetic mean of these weighted differentials for any given period is calculated to produce a top-line House Price index number (Calhoun, 1996).

Summarizing, the FHFA HPI is a measure of changes in the price of single-family homes in a geographical area. The number is obtained with a non-hedonic approach to quality adjustment. The specific publication used for further analysis is a quarterly publication of data from 51 geographic areas at the provincial level.

Model Specifications

The data is investigated using a difference-in-difference methodology, as such, a model solves two concerns that would show up under a regular linear estimator. Firstly, and most importantly, it solves for omitted variable bias. The price of housing is a complex and hard-to-model phenomenon, and any model that attempts to have a good estimate of it would have to include a near-infinite amount of confounders. Some examples would be the racial demographic makeup, the cost of living in the area, a measure of income, a measure of wealth, and a measure of housing availability. Further, some of these confounders would be near impossible to quantify, for example, the natural beauty of a region, or the pleasantness of its climate, which are likely to influence the willingness of people to pay for living there, however, these are not easy to summarize into a single data point.

Furthermore, the usage of difference in difference methodology solves a heterogeneity concern in the data. Namely, there is some relationship between the growth of housing costs and the growth of a city's boundaries, where “the spatial size of the city grows as population or income increases, and falls as commuting cost or agricultural rent increases”(Brueckner 2001). This growth in turn may trigger a concern for sprawl and thus could cause a city with a trend for higher rent and housing costs to implement sprawl control. However, by observing that the pre-trends are indeed parallel, we can confidently assuage that concern, as any regulation causing shock to house prices would be noted in the control group as well.

The Model will utilize as variables the FHFA HPI of different US States measured in quarterly intervals, as a dependent variable. The states at hand, as a categorical variable, the periods in which the HPI is measured as a series of dummy variables, and a dummy indicating whether the observation is in the state of Maryland after the passage of the Smart Growth Areas Act.

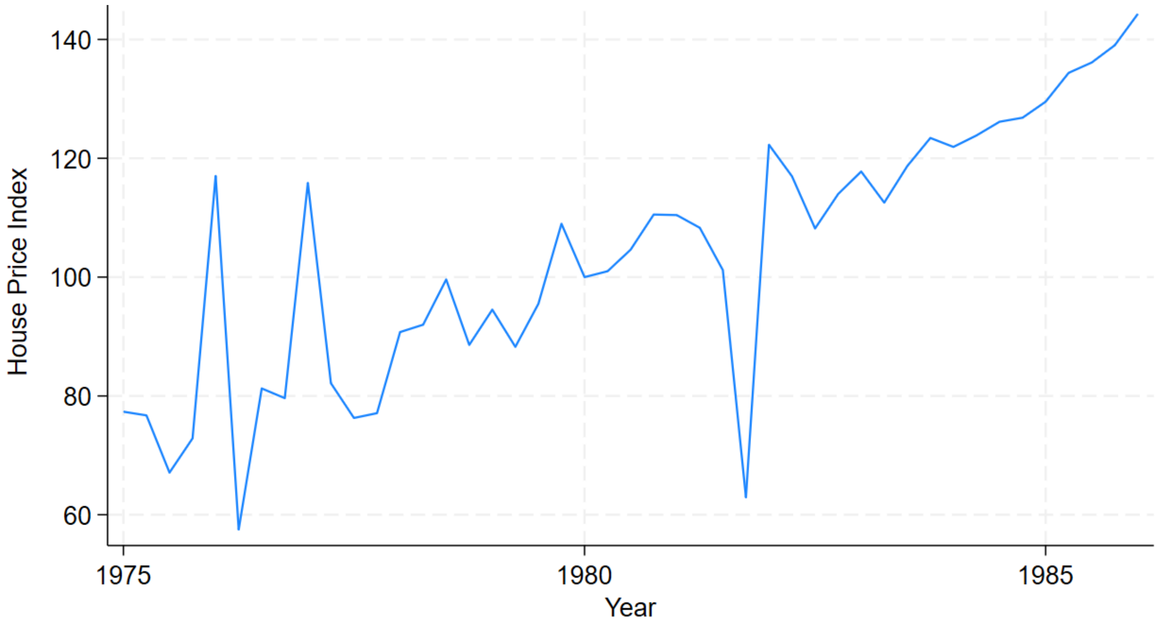
Mechanically, the estimation difference in difference approach can be given by the following regression equation:

$$HPI = \beta_s State + \beta_{pi} Period_i + \beta_T\, Treated + \varepsilon$$

Where HPI is the model estimate of house price index, State is a categorical variable containing the state and β_s refers to the initial difference between states. $Period_i$ is a dummy variable for all periods in the sample and β_{pi} is the size of the effect of any given year in HPI and Treated is a dummy variable that is set to true when the state is Maryland (our treated state) and the period is greater than 89 (the intervention period). In effect, this means modeled effect magnitude and direction are given byβ_T. Lastly, ε is the error term.

Before performing any further analysis, the dataset was trimmed. For the pre-trends analysis, the years preceding 1985 were removed; the reason for such a measure is that the dataset is limited to houses financed twice by either Freddie Mac or Fannie Mae after the year 1975. This leaves only 10 years, and a relatively small sample of properties in the dataset. As a consequence of this, the data is noisy for some geographical areas, one example of such erratic behavior can be seen for the state of Vermont before 1985 in Figure 1 below:

Figure 1: House Price Index in Vermont between 1975 and 1985



Further, it would be difficult to assume that trends would have continued to be parallel after the COVID-19 Pandemic, as such an event caused significant changes to the underlying preferences of people, one relevant example is that people now have a significantly more acute preference for lower density regions (Su & Liu 2021). To assuage this concern, the years after 2019 were also excluded. A similar argument could be made about the 2002 to 2008 housing bubble in the United States, however, given the proximity to the chosen intervention date this would be a difficult event to exclude without raising serious validity questions.

The Studied Event is the passage of the Smart Growth Areas Act. While the act did not come into effect immediately, the efficient market hypothesis would dictate that at least some of its impact was priced in before the actual implementation of the law. This is indeed something that is visible in the data by running the model with the implementation of the act as the intervention, as well as with “lead” variables for the periods preceding it. The results show that the preceding periods have a statistically significant power of prediction when evaluated jointly, with a reported p-value of 0.0042, and thus that the trends are not parallel in the aftermath of the passage of the law with a 95% confidence level. In particular , the period that would correspond for the passage of the law has the largest coefficient at 3.2 HPI units and is significant at the 95% confidence level

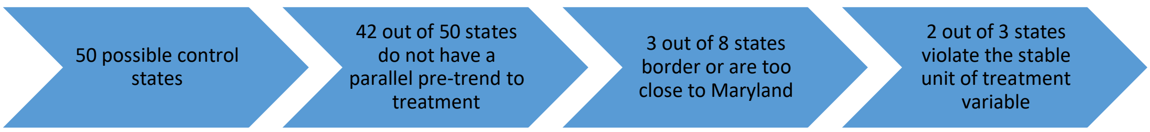
This combination of choices still gives ample pre-intervention data to establish trends, with a total of 48 studied periods over 12 different calendar years (HPI measures were taken every quarter year).

With all of this in mind, the selection of the control group began by performing a visual check for parallel trends for all fifty states and for the District of Columbia. A subset of nine units (including eight states and the District of Columbia) for which the pre-intervention trends could be plausibly said to be parallel were selected. From this sample, all states neighboring Maryland were dropped, ,likewise, the state of New Jersey was removed from the control because, while technically not bordering Maryland, the closest cities between both states are only a 15-minute car trip away (Google Maps).

These choices were made due to concerns over a spillover effect, as a rise in housing costs in Maryland could cause cities in those states to see increased housing demand, as they become commute options for Maryland workers or as Maryland becomes a less desirable commute origin point for their workers. This will especially be the case for those that share at least one urban agglomeration with Maryland.

Further, to ensure a stable unit of the treatment variable, states that implemented similar laws near or after the time of passage of the Smart Growth Act in the studied period have to be removed from the analysis. This step further removes 2 states, Maine with the passage of the Comprehensive Planning and Land Use Act of 1998, (Comprehensive Planning and Land Use Act, 1998), and Washington, due to the passage of the Washington State Growth Management Act (Growth Management Act 1990). The complete taxonomy of exclusion can be seen below in Figure 2:

Figure 2: Process to filter control states

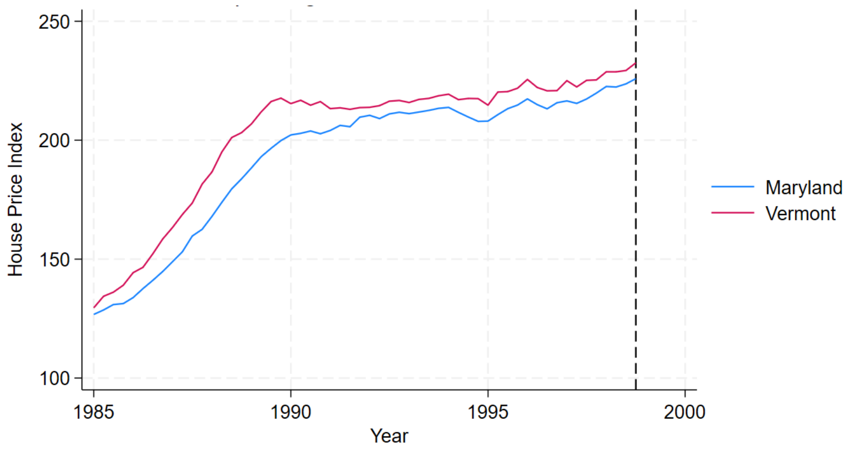


Consequently, the sample included only the state of Vermont as a control. To verify its validity as a control, one important question is whether they represent a stable treatment unit. The state of Vermont does have Act 250, a law intended to assess new construction for environmental and societal impact that establishes a board for reviewing property developments. However, such a law was passed in 1970, well before this paper's analysis, and as such, any effect that its passage had on containment of sprawl is baked into the parallel trends assumption, further, it is not actually intended to control sprawl, and any such effect would have been incidental.

Yet, given that the mechanism for determining the permissibility of new development is not objective, but rather dependent on standards set by an independent branch of government, it is possible that the passage of the smart growth act and the subsequent positive attention it received caused some sprawl control in the state of Vermont. While it is impossible to precisely gauge what such an impact would do to model estimates if it does indeed exist, one could reasonably expect that it would reduce the point estimate of the difference and thus also the certainty values, increasing the p-value and decreasing the power of the experiment.

The last unique requirement of difference in difference analysis is that the pre intervention trends be parallel, this assumption was validated both visually and empirically. Visually, as seen in figure 3, the trends appear parallel. To verify that empirically, we utilize leads on our intervention variable and check for their joint significance using a similar method to the one outlined above. For this model, 10 leads were utilized, and not only were they not jointly significant at a 95% confidence level, with a joint reported p-value of 0.1956. The direction of the trends could not be checked, as there was no transformation of the data that allowed a linear regression to satisfactorily fit the source data.

Figure 3: pre-intervention trends for house price index in selected states



Lastly, the difference-in-difference utilizes regressions, as such, as with any other Ordinary Least Square (OLS) regression some assumptions must be met to ensure proper estimation. Normality of residuals was tested using a Shapiro-Wilk test, which reported a p value of 0.0000 rejecting the null hypothesis that residuals are not normally distributed at a 95% confidence level. And allowing us to affirm that the OLS condition of normally distributed error terms is met with a 95% level of confidence.

Lastly, it is worth noticing that the error terms are correlated by period clusters, and as such, to provide an accurate estimate of the standard errors and p values, and thus permit an accurate assessment of confidence level, all analysis in this paper[6] are done utilizing clustered standard errors for years. A similar argument could be made for states; however, given that unfortunately only one control state was available, this is not possible. Similarly, a clustering by state-period would make no sense, as those clusters would draw erroneously narrow confidence intervals.

In summary, the method used is a difference-in-difference analysis with robust standard errors. The control group utilizes the state of Vermont, While the Treatment group, on the other hand contains the state of Maryland, which passed the Smart Growth Areas Act in the first quarter of 1997. The Variable of interest is The House Price Index of the United States Federal Housing Finance Agency.

Main Results

A summary of relevant results can be seen in Table 1 with an increase of 11.75 House Price Index points in the treated region. Not only can we reject the null hypothesis at a confidence level of 95%, this effect is also practically significant. For example, 5 years after the intervention this would account for 5% of the value of a house. Given that the FHFA HPI is a generally increasing time series, and that the effect is nominal by construction of the model, its proportional impact will be reduced over time. The other variables are not of particular significance, as they do not refer to an absolute housing affordability value, but rather to the absolute position of both curves, in this case, they also refer to the difference between the studied period and the index point of the time series in 1980 at 100 points for every state. This difference can be at least partially attributed to measuring errors, given that the earliest year was calculated using a small sample of houses refinanced twice in the previous 5 years. As discussed above, the earliest points of the time series are less than reliable.

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[6] This includes previously discussed numbers

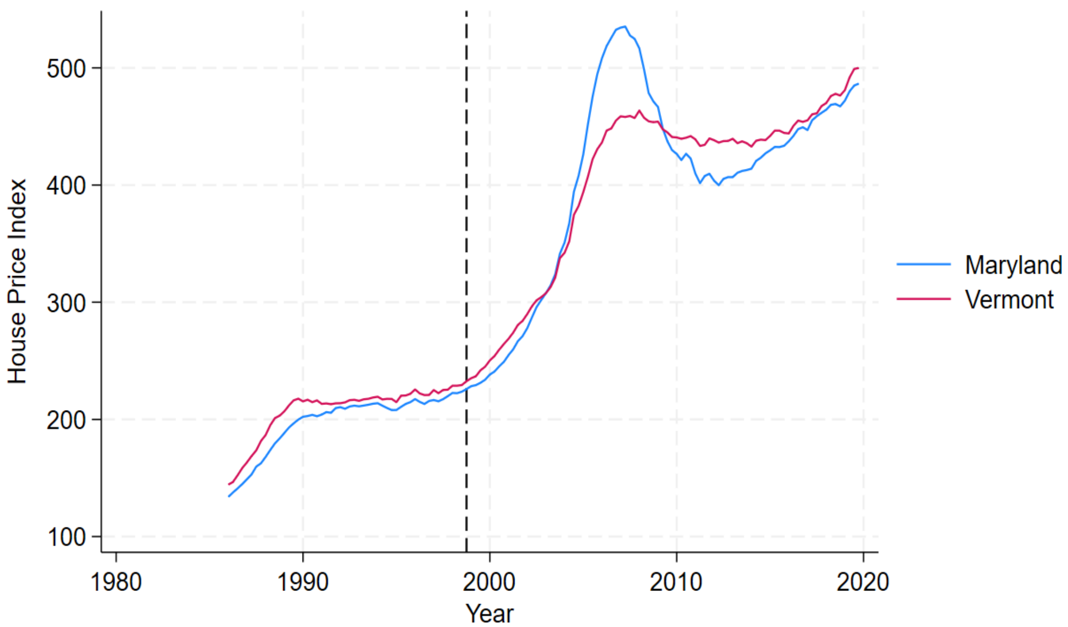
Table 1: Summary of regression results: Effect of treatment on HPI

Effect	Estimate	*SE*	95% CI		*p*
			LL	*UL*	
Average treatment effect	11.76	4.51	2.83	20.68	.010
Vermont	10.60	1.14	8.35	12.85	.000
Constant	133.77	.570	132.6	134.8	.000

Note. Total N = 272. CI = confidence interval; LL = lower limit; UL = upper limit, All Values are statistically significant at the 95% CI Full regression results are available in appendix A

Turning to Figure 4. We can see the price trends across the three states before and after the implementation of the policy (denoted by the dotted line). A few notable aspects of the trend line are that the effect is indeed visible, as the trend line for Maryland approaches Vermont after the intervention. Secondly, it is interesting to observe that the impact of the housing crisis is far more aggressive in the state of Maryland. This difference manifests itself both before peak prices, at which point Maryland residences were less affordable than they were at the end of the series, as well as after the crash, point when Maryland residences plummeted to a deeper trough than would be expected based on the control. This lends credence to the theory that the chief driver of the impact is a reduction in supply-side elasticity.

Figure 4: House Price Index over time in selected cities before and after passage of smart growth areas act



Discussions of Mechanism Plausibility

Having established the magnitude and direction of the effect, it is a reasonable further step to perform preliminary checks of the proposed mechanisms. These are not intended to establish or dispel any mechanism, but simply to serve as a guiding light for future research, thus the methods used are less econometrically sound than those used in the main findings.

For the mechanisms of increased desirability of the housing market and decrease in new housing supply, a chow break test is performed in time series thought to proxy these two variables. As there are no reasonable proxies for supply side elasticity, the theory of an increase in prices coming from a decrease in supply elasticity is tested by rerunning a similar model to the one in the main results while adding a dummy variable to years of increased housing demand in the dataset during the 2002-2008 housing bubble. Importantly, these tests are not enough to provide evidence of causality, nor are they enough to establish the proportionality of an effect when compared to another, as the tests diverge substantially on statistical power.

One way to Proxy the desirability of a city is its population, if a city is more desirable, then more people will move into it causing an increase in population. Thus, to evaluate an increase in desirability a population time series for the state of Maryland was utilized. There are several issues with this approach, for one, it does not account for birth and mortality, which are the main drivers of population change. However, the available datasets for migration are more complex and have data available at the county, and not state level, making it difficult to parse out data for this analysis. As such, the possible effect mechanism was evaluated using the Census Bureau's Population Estimates Program, which provides yearly estimates for the population of several geographical units. For our use, we utilized state level data for the state of Maryland.

To evaluate a possible series break, a chow break test was used to evaluate the hypothesis that there is a structural break in the time series in the year of 1997(the passage of the smart growth areas act). The logic here being that if the act made the city immediately more desirable then a break in the data would appear, as population would significantly rise after the passage of the pow. After performing said test, the reported p-value was 0.112, at a 95% confidence level, this p-value is too high to reject the null that there is no break in the data. Therefore, it is not statistical evidence that Maryland has suddenly become more desirable.

However, this negative should not be taken to be conclusive, not only do the issues previously described make this estimation less than ideal, but it is also possible that the effect of increase in desirability was not felt immediately. The reason for this is that the housing market is relatively inflexible, and it is possible that any changes to housing amenities because of the Smart Growth Areas act of it would only be felt in the medium to long term. Lastly, not being able to prove a difference is epistemologically distinct to proving no difference exists.

A similar approach was performed with respect to the creation of new dwellings, however, this time the Building Permits Survey, also from the US Census bureau was used as a dataset for total approved permits. If supply in Maryland became more constrained one would expect that around the time of the passage of the law, there would be a systematic reduction in the production of new permits, which would show up as a break in the historical data. In this case, the dataset was trimmed to avoid the 2007 housing bubble crash, which caused a complete collapse in the number of issued permits in Maryland and in many other geographies in the dataset. In this case, the reported p-value of the chow break test is 0.504. Again, it is valuable to highlight that the absence of an observed effect does not mean that effect does not exist. And further, there can still be unobserved confounding time varying variables, and the data used was quite limited.

The last theoretical pathway had to be analyzed differently. As elasticity is the reaction to demand, and given demand is hard to directly observe, there is no time series for elasticity. Therefore, The effect was evaluated by modifying the main model and making use of the unnaturally high demand during the housing bubble. Theoretically, if supply was perfectly elastic the increase in demand would have been reacted with housing construction thus keeping pricing more stable. Thus, the differences in price increase between control and treatment groups could be an indication of the fact the effect is driven by a lack of elasticity. To check for this, dummies were created for years in the housing bubble, and the interaction variable between this dummy and the treated dummy was stored and regressed alongside the previous difference in difference model; the results can be seen in table 2 below.

Table 2: Summary of difference-in-difference results: Effect of treatment on HPI during the 2002-2008 housing bubble

Effect	Estimate	*SE*	95% CI		*p*
			LL	*UL*	
Average treatment effect	-0.22	2.85	-5.85	5.41	.939
Treatment during Bubble*	45.40	10.019	25.59	65.22	.000
Vermont*	10.60	1.14	8.34	12.86	.000
Constant*	133.77	.572	132.6	135.0	.000

Note. total N = 272. CI = confidence interval; LL = lower limit; UL = upper limit, All estimates denoted by * are statistically significant at 95% confidence leverFull regression results are available in appendix A

The model provides enough evidence to affirm with 95% confidence that the state of Maryland had a significantly higher price increase in the housing bubble than the control group. Which is quite a strong piece of evidence that in fact the reduction in elasticity from the geographically constrained real estate market of the state of Maryland caused part of the effect. The reduction of the treatment coefficient to statistically insignificant levels appears to be a result of the colder post-recession housing market for Maryland.

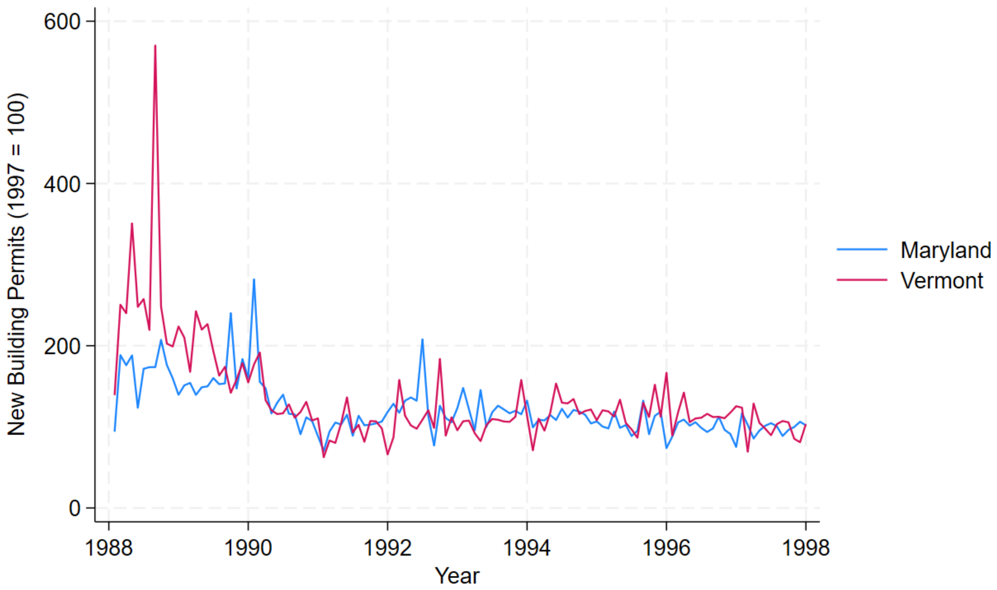
Limitations

The conclusions outlined above, however, should not be taken as some axiomatic truth, but rather, scrutinized under their imperfections. The model is still subject to time varying omitted variable bias. Spillover may have happened in the control state of Vermont, and some indicators of robustness are not perfectly met. Lastly, the state in the control group have significant underlying differences to the state of Maryland, all these factors need to be scrutinized.

Firstly, while the difference-in-difference approach can be said to credibly resolve omitted variable bias for time invariant variables, it is still subject to time varying distinctions between the control and the treatment groups. As an example, it is the case that, as demonstrated, the groups reacted quite differently to the housing bubble. While it is quite likely that the difference there may be explained as a product of the treatment, it is also possible that the state housing markets reacted differently as a byproduct of inherent differences in their house market make up.

Further, the states in the treatment and control group do indeed differ quite a bit in their makeup in ways that are reasonably likely to affect the price of housing, Vermont substantially more rural than the state of Maryland. Despite a lower agricultural land productivity., Moreover, as can be seen in figure 5 below, which shows trends in approved construction permits, the trends were never exactly equal between the two states. While these differences should raise some eyebrows, the parallel trends in the pre-intervention period show us that, for whatever reason, the housing markets in each state reacted similarly to time-varying nationwide conditions.

Figure 5: Issued Construction permits in Vermont and Maryland between 1988 ad 1997 normalized for equal counts in 1997



Note: as the Maryland is a far more populous state, numbers were translated downwards for legibility

Additionally, there is a quite interesting question regarding why the house price indexes were at a different level in the start of the analyzed period. If the difference in 1980 was 0, as they were all indexed at that point in time and the pre intervention trends are indeed parallel, one would expect that both states would be perfectly level before intervention. One partial explanation of this is the previously discussed issue of noise in the data. However it is plausible, if unlikely, that there is some inherent pre-intervention difference that manifests itself only in the 1980-1985 period.

Lastly, there are reasonable pathways for violations of the stable unit of the treatment variable assumption. In Vermont, zoning and building regulation is done through a law called act 250, while this legislation remained unchanged during the studied period, its technical directives are laid not by the law itself but rather by the Vermont Natural Resources Board(Act 250, 1970). As such, it is plausible that after the positive attention received by the implementation of the Smart Growth Areas Act, the board also in effect implemented a similar policy through executive fiat and such policy change was never registered in any official publication. However, even if the violation did happen, it could only possibly reduce the magnitude of the studied effect.

One last detail that causes reasonable internal validity concerns is that the Smart Growth Areas Act was passed within the context of other "Smart Growth" policies, while all of these policies were intended to combat sprawl, and while the Smart Growth Areas Act was the most forceful policy of the set. The legal package also included increases in public transportation funding and a focus on walkability for urban developments. Assuaging the validity concern is the fact that these other policies are broadly considered to not have been implemented in an effective manner (Shen & Zhang 2007).

Discussion and Conclusion

In conclusion, the passage of the Smart Growth Areas Act in the state of Maryland had a statistically and practically significant effect on the house prices inside of the state. On average throughout the studied period, the act resulted in an additional 36.8 House Price Index Points. This research has some reasonable, but not severe internal validity concerns. External Validity is a more mixed bag, one can reasonably expect that the results should replicate for other implementations of urban growth boundary policies, however, the present paper provides no evidence whether the effect is smaller or even nonexistent in case other popular sprawl control techniques are used.

Further, in preliminary fashion, the paper found evidence of the contribution of a reduced elasticity of supply to such an effect. There was not enough evidence to indicate or dispel that increases in demand for housing or reduction in long-term housing supply were significant contributors.

In future research on this topic, it would be interesting to evaluate not only the external validity of the answer, but also to further understand the mechanisms at play. In evaluating validity, research would benefit from confirming that this effect holds in other implementations of urban growth boundaries and other sprawl control policies. Among such policies, those that more closely resemble a Pigouvian tax, such as a gas tax or a congestion-pricing scheme are of particular interest for their economic elegance. Similarly, urban planning choices to limit automobile use, which are currently rising in popularity and infamy, are highly socially relevant.

Further, the studied effect is highly socially relevant, as many nations and cities attempt to fight sprawl and high costs of living simultaneously; the paper elucidates some of the ways in which these two reasonable policy goals may interfere with each other.Moreover, it highlights that there are very real and significant negative impacts to the implementation of urban growth boundaries. Ideally, any locations planning to implement such policies should make clear to all stakeholders that its implementation is likely to cause an increase in the cost of housing. Lastly, while it is perfectly possible that this effect is not felt with some policies intended to control sprawl, it is almost a certainty that it will be felt similarly with policies that are in effect quite mechanically similar to urban growth boundaries. Some popular examples are green rings, conservation areas located proximally to cities, and soft measures intended to minimize commute distances are some examples of such policies.

If one is to take the effects measured by the paper at face value, then urban planners must consider the decreased affordability when proposing sprawl control. While there is reasonably robust evidence that some deleterious effects of urban sprawl exist, whether it be worsened health, lost sense of community, increased emissions, gentrification or increased cost to the taxpayer, these are costs that come with the benefit of cheaper housing. Thus, these interests must be balanced in order to maximize public welfare. One possible way to do this is to couple sprawl control with measures that increase housing affordability, yet another one is to stop restricting sprawl. Regardless, house price effects should be considered.

Appendix A: Full Regression Results for all difference-in-difference models

	Effect On House Price Index When Modelled With					
	Only intervention		Intervention and bubble dummies		Intervention and pre-intervention leads	
Treatment effect	11.756	*	-0.217		10.899	*
	(4.514)		(2.847)		(5.081)	
State:						
Vermont	10.600	**	10.600	**	11.439	**
	(1.140)		(1.144)		(1.452)	
Period:						
1991 Quarter 1	3.015	**	3.015	**	3.015	**
1991 Quarter 2	(0.000)		(0.000)		(0.000)	
1991 Quarter 3	7.610	**	7.610	**	7.610	**
1991 Quarter 4	(0.000)		(0.000)		(0.000)	
1992 Quarter 1	12.560	**	12.560	**	12.560	**
1992 Quarter 2	(0.000)		(0.000)		(0.000)	
1992 Quarter 3	17.070	**	17.070	**	17.070	**
1992 Quarter 4	(0.000)		(0.000)		(0.000)	
1993 Quarter 1	21.880	**	21.880	**	21.880	**
1993 Quarter 2	(0.000)		(0.000)		(0.000)	
1993 Quarter 3	27.590	**	27.590	**	27.590	**
1993 Quarter 4	(0.000)		(0.000)		(0.000)	
1994 Quarter 1	32.965	**	32.965	**	32.965	**
1994 Quarter 2	(0.000)		(0.000)		(0.000)	
1994 Quarter 3	38.260	**	38.260	**	38.260	**
1994 Quarter 4	(0.000)		(0.000)		(0.000)	
1995 Quarter 1	45.395	**	45.395	**	45.395	**
1995 Quarter 2	(0.000)		(0.000)		(0.000)	

1995 Quarter 3	51.260	**	51.260	**	51.260	**
1995 Quarter 4	(0.000)		(0.000)		(0.000)	
1996 Quarter 1	54.395	**	54.395	**	54.395	**
1996 Quarter 2	(0.000)		(0.000)		(0.000)	
1996 Quarter 3	58.535	**	58.535	**	58.535	**
1996 Quarter 4	(0.000)		(0.000)		(0.000)	
1997 Quarter 1	63.455	**	63.455	**	63.455	**
1997 Quarter 2	(0.000)		(0.000)		(0.000)	
1997 Quarter 3	67.375	**	67.375	**	67.375	**
1997 Quarter 4	(0.000)		(0.000)		(0.000)	
1998 Quarter 1	69.700	**	69.700	**	69.700	**
1998 Quarter 2	(0.000)		(0.000)		(0.000)	
1998 Quarter 3	69.735	**	69.735	**	69.735	**
1998 Quarter 4	(0.000)		(0.000)		(0.000)	
1999 Quarter 1	70.775	**	70.775	**	70.775	**
1999 Quarter 2	(0.000)		(0.000)		(0.000)	
1999 Quarter 3	70.230	**	70.230	**	70.230	**
1999 Quarter 4	(0.000)		(0.000)		(0.000)	
2000 Quarter 1	70.400	**	70.400	**	70.400	**
2000 Quarter 2	(0.000)		(0.000)		(0.000)	
2000 Quarter 3	69.615	**	69.615	**	69.615	**
2000 Quarter 4	(0.000)		(0.000)		(0.000)	
2001 Quarter 1	70.860	**	70.860	**	70.860	**
2001 Quarter 2	(0.000)		(0.000)		(0.000)	
2001 Quarter 3	70.255	**	70.255	**	70.255	**
2001 Quarter 4	(0.000)		(0.000)		(0.000)	
2002 Quarter 1	72.625	**	72.625	**	72.625	**
2002 Quarter 2	(0.000)		(0.000)		(0.000)	
2002 Quarter 3	73.075	**	73.075	**	73.075	**

2002 Quarter 4	(0.000)		(0.000)		(0.000)	
2003 Quarter 1	72.745	**	72.745	**	72.745	**
2003 Quarter 2	(0.000)		(0.000)		(0.000)	
2003 Quarter 3	74.665	**	74.665	**	74.665	**
2003 Quarter 4	(0.000)		(0.000)		(0.000)	
2004 Quarter 1	75.165	**	75.165	**	75.165	**
2004 Quarter 2	(0.000)		(0.000)		(0.000)	
2004 Quarter 3	74.450	**	74.450	**	74.450	**
2004 Quarter 4	(0.000)		(0.000)		(0.000)	
2005 Quarter 1	75.435	**	75.435	**	75.435	**
2005 Quarter 2	(0.000)		(0.000)		(0.000)	
2005 Quarter 3	75.990	**	75.990	**	75.990	**
2005 Quarter 4	(0.000)		(0.000)		(0.000)	
2006 Quarter 1	76.980	**	76.980	**	76.980	**
2006 Quarter 2	(0.000)		(0.000)		(0.000)	
2006 Quarter 3	77.495	**	77.495	**	77.495	**
2006 Quarter 4	(0.000)		(0.000)		(0.000)	
2007 Quarter 1	75.315	**	75.315	**	75.315	**
2007 Quarter 2	(0.000)		(0.000)		(0.000)	
2007 Quarter 3	74.585	**	74.585	**	74.585	**
2007 Quarter 4	(0.000)		(0.000)		(0.000)	
2008 Quarter 1	73.635	**	73.635	**	72.690	**
2008 Quarter 2	(0.000)		(0.000)		(0.726)	
2008 Quarter 3	72.320	**	72.320	**	69.960	**
2008 Quarter 4	(0.000)		(0.000)		(0.726)	
2009 Quarter 1	76.435	**	76.435	**	75.470	**
2009 Quarter 2	(0.000)		(0.000)		(0.726)	
2009 Quarter 3	77.765	**	77.765	**	75.630	**
2009 Quarter 4	(0.000)		(0.000)		(0.726)	

2010 Quarter 1	79.255	**	79.255	**	77.080	**
2010 Quarter 2	(0.000)		(0.000)		(0.726)	
2010 Quarter 3	82.390	**	82.390	**	80.760	**
2010 Quarter 4	(0.000)		(0.000)		(0.726)	
2011 Quarter 1	79.485	**	79.485	**	77.370	**
2011 Quarter 2	(0.000)		(0.000)		(0.726)	
2011 Quarter 3	77.910	**	77.910	**	75.980	**
2011 Quarter 4	(0.000)		(0.000)		(0.726)	
2012 Quarter 1	79.240	**	79.240	**	76.070	**
2012 Quarter 2	(0.000)		(0.000)		(0.726)	
2012 Quarter 3	81.720	**	81.720	**	80.260	**
2012 Quarter 4	(0.000)		(0.000)		(0.726)	
2013 Quarter 1	73.992	**	79.979	**	72.961	**
2013 Quarter 2	(2.257)		(1.424)		(2.642)	
2013 Quarter 3	76.292	**	82.279	**	75.261	**
2013 Quarter 4	(2.257)		(1.424)		(2.642)	
2014 Quarter 1	77.652	**	83.639	**	76.621	**
2014 Quarter 2	(2.257)		(1.424)		(2.642)	
2014 Quarter 3	80.727	**	86.714	**	79.696	**
2014 Quarter 4	(2.257)		(1.424)		(2.642)	
2015 Quarter 1	80.612	**	86.599	**	79.581	**
2015 Quarter 2	(2.257)		(1.424)		(2.642)	
2015 Quarter 3	81.557	**	87.544	**	80.526	**
2015 Quarter 4	(2.257)		(1.424)		(2.642)	
2016 Quarter 1	84.262	**	90.249	**	83.231	**
2016 Quarter 2	(2.257)		(1.424)		(2.642)	
2016 Quarter 3	86.822	**	92.809	**	85.791	**
2016 Quarter 4	(2.257)		(1.424)		(2.642)	
2017 Quarter 1	88.067	**	94.054	**	87.036	**

2017 Quarter 2	(2.257)		(1.424)		(2.642)	
2017 Quarter 3	91.627	**	97.614	**	90.596	**
2017 Quarter 4	(2.257)		(1.424)		(2.642)	
2018 Quarter 1	94.482	**	100.469	**	93.451	**
2018 Quarter 2	(2.257)		(1.424)		(2.642)	
2018 Quarter 3	99.362	**	105.349	**	98.331	**
2018 Quarter 4	(2.257)		(1.424)		(2.642)	
2019 Quarter 1	102.532	**	108.519	**	101.501	**
2019 Quarter 2	(2.257)		(1.424)		(2.642)	
2019 Quarter 3	107.457	**	113.444	**	106.426	**
2019 Quarter 4	(2.257)		(1.424)		(2.642)	
1991 Quarter 1	111.752	**	117.739	**	110.721	**
1991 Quarter 2	(2.257)		(1.424)		(2.642)	
1991 Quarter 3	116.872	**	122.859	**	115.841	**
1991 Quarter 4	(2.257)		(1.424)		(2.642)	
1992 Quarter 1	122.007	**	127.994	**	120.976	**
1992 Quarter 2	(2.257)		(1.424)		(2.642)	
1992 Quarter 3	128.732	**	134.719	**	127.701	**
1992 Quarter 4	(2.257)		(1.424)		(2.642)	
1993 Quarter 1	132.602	**	138.589	**	131.571	**
1993 Quarter 2	(2.257)		(1.424)		(2.642)	
1993 Quarter 3	138.877	**	122.164	**	137.846	**
1993 Quarter 4	(2.257)		(4.870)		(2.642)	
1994 Quarter 1	146.762	**	130.049	**	145.731	**
1994 Quarter 2	(2.257)		(4.870)		(2.642)	
1994 Quarter 3	153.807	**	137.094	**	152.776	**
1994 Quarter 4	(2.257)		(4.870)		(2.642)	
1995 Quarter 1	158.162	**	141.449	**	157.131	**
1995 Quarter 2	(2.257)		(4.870)		(2.642)	

1995 Quarter 3	162.752	**	146.039	**	161.721	**
1995 Quarter 4	(2.257)		(4.870)		(2.642)	
1996 Quarter 1	168.667	**	151.954	**	167.636	**
1996 Quarter 2	(2.257)		(4.870)		(2.642)	
1996 Quarter 3	177.512	**	160.799	**	176.481	**
1996 Quarter 4	(2.257)		(4.870)		(2.642)	
1997 Quarter 1	194.597	**	177.884	**	193.566	**
1997 Quarter 2	(2.257)		(4.870)		(2.642)	
1997 Quarter 3	201.592	**	184.879	**	200.561	**
1997 Quarter 4	(2.257)		(4.870)		(2.642)	
1998 Quarter 1	214.757	**	198.044	**	213.726	**
1998 Quarter 2	(2.257)		(4.870)		(2.642)	
1998 Quarter 3	239.552	**	222.839	**	238.521	**
1998 Quarter 4	(2.257)		(4.870)		(2.642)	
1999 Quarter 1	250.132	**	233.419	**	249.101	**
1999 Quarter 2	(2.257)		(4.870)		(2.642)	
1999 Quarter 3	265.062	**	248.349	**	264.031	**
1999 Quarter 4	(2.257)		(4.870)		(2.642)	
2000 Quarter 1	284.662	**	267.949	**	283.631	**
2000 Quarter 2	(2.257)		(4.870)		(2.642)	
2000 Quarter 3	303.807	**	287.094	**	302.776	**
2000 Quarter 4	(2.257)		(4.870)		(2.642)	
2001 Quarter 1	317.657	**	300.944	**	316.626	**
2001 Quarter 2	(2.257)		(4.870)		(2.642)	
2001 Quarter 3	327.352	**	310.639	**	326.321	**
2001 Quarter 4	(2.257)		(4.870)		(2.642)	
2002 Quarter 1	337.627	**	320.914	**	336.596	**
2002 Quarter 2	(2.257)		(4.870)		(2.642)	
2002 Quarter 3	342.067	**	325.354	**	341.036	**

2002 Quarter 4	(2.257)		(4.870)		(2.642)	
2003 Quarter 1	348.877	**	332.164	**	347.846	**
2003 Quarter 2	(2.257)		(4.870)		(2.642)	
2003 Quarter 3	351.557	**	334.844	**	350.526	**
2003 Quarter 4	(2.257)		(4.870)		(2.642)	
2004 Quarter 1	351.757	**	335.044	**	350.726	**
2004 Quarter 2	(2.257)		(4.870)		(2.642)	
2004 Quarter 3	348.362	**	331.649	**	347.331	**
2004 Quarter 4	(2.257)		(4.870)		(2.642)	
2005 Quarter 1	346.032	**	329.319	**	345.001	**
2005 Quarter 2	(2.257)		(4.870)		(2.642)	
2005 Quarter 3	345.202	**	351.189	**	344.171	**
2005 Quarter 4	(2.257)		(1.424)		(2.642)	
2006 Quarter 1	333.192	**	339.179	**	332.161	**
2006 Quarter 2	(2.257)		(1.424)		(2.642)	
2006 Quarter 3	321.557	**	327.544	**	320.526	**
2006 Quarter 4	(2.257)		(1.424)		(2.642)	
2007 Quarter 1	317.582	**	323.569	**	316.551	**
2007 Quarter 2	(2.257)		(1.424)		(2.642)	
2007 Quarter 3	315.477	**	321.464	**	314.446	**
2007 Quarter 4	(2.257)		(1.424)		(2.642)	
2008 Quarter 1	303.137	**	309.124	**	302.106	**
2008 Quarter 2	(2.257)		(1.424)		(2.642)	
2008 Quarter 3	296.277	**	302.264	**	295.246	**
2008 Quarter 4	(2.257)		(1.424)		(2.642)	
2009 Quarter 1	290.467	**	296.454	**	289.436	**
2009 Quarter 2	(2.257)		(1.424)		(2.642)	
2009 Quarter 3	288.657	**	294.644	**	287.626	**
2009 Quarter 4	(2.257)		(1.424)		(2.642)	

2010 Quarter 1	285.462	**	291.449	**	284.431	**
2010 Quarter 2	(2.257)		(1.424)		(2.642)	
2010 Quarter 3	288.592	**	294.579	**	287.561	**
2010 Quarter 4	(2.257)		(1.424)		(2.642)	
2011 Quarter 1	287.237	**	293.224	**	286.206	**
2011 Quarter 2	(2.257)		(1.424)		(2.642)	
2011 Quarter 3	279.472	**	285.459	**	278.441	**
2011 Quarter 4	(2.257)		(1.424)		(2.642)	
2012 Quarter 1	272.587	**	278.574	**	271.556	**
2012 Quarter 2	(2.257)		(1.424)		(2.642)	
2012 Quarter 3	276.092	**	282.079	**	275.061	**
2012 Quarter 4	(2.257)		(1.424)		(2.642)	
2013 Quarter 1	279.802	**	285.789	**	278.771	**
2013 Quarter 2	(2.257)		(1.424)		(2.642)	
2013 Quarter 3	276.072	**	282.059	**	275.041	**
2013 Quarter 4	(2.257)		(1.424)		(2.642)	
2014 Quarter 1	273.112	**	279.099	**	272.081	**
2014 Quarter 2	(2.257)		(1.424)		(2.642)	
2014 Quarter 3	276.437	**	282.424	**	275.406	**
2014 Quarter 4	(2.257)		(1.424)		(2.642)	
2015 Quarter 1	277.267	**	283.254	**	276.236	**
2015 Quarter 2	(2.257)		(1.424)		(2.642)	
2015 Quarter 3	278.187	**	284.174	**	277.156	**
2015 Quarter 4	(2.257)		(1.424)		(2.642)	
2016 Quarter 1	278.167	**	284.154	**	277.136	**
2016 Quarter 2	(2.257)		(1.424)		(2.642)	
2016 Quarter 3	279.732	**	285.719	**	278.701	**
2016 Quarter 4	(2.257)		(1.424)		(2.642)	
2017 Quarter 1	279.337	**	285.324	**	278.306	**

2017 Quarter 2	(2.257)		(1.424)		(2.642)	
2017 Quarter 3	278.432	**	284.419	**	277.401	**
2017 Quarter 4	(2.257)		(1.424)		(2.642)	
2018 Quarter 1	284.272	**	290.259	**	283.241	**
2018 Quarter 2	(2.257)		(1.424)		(2.642)	
2018 Quarter 3	286.097	**	292.084	**	285.066	**
2018 Quarter 4	(2.257)		(1.424)		(2.642)	
2019 Quarter 1	287.762	**	293.749	**	286.731	**
1991 Quarter 1	(2.257)		(1.424)		(2.642)	
1991 Quarter 2	290.832	**	296.819	**	289.801	**
1991 Quarter 3	(2.257)		(1.424)		(2.642)	
1991 Quarter 4	294.567	**	300.554	**	293.536	**
1992 Quarter 1	(2.257)		(1.424)		(2.642)	
1992 Quarter 2	294.497	**	300.484	**	293.466	**
1992 Quarter 3	(2.257)		(1.424)		(2.642)	
1992 Quarter 4	294.077	**	300.064	**	293.046	**
1993 Quarter 1	(2.257)		(1.424)		(2.642)	
1993 Quarter 2	295.757	**	301.744	**	294.726	**
1993 Quarter 3	(2.257)		(1.424)		(2.642)	
1993 Quarter 4	301.367	**	307.354	**	300.336	**
1994 Quarter 1	(2.257)		(1.424)		(2.642)	
1994 Quarter 2	306.457	**	312.444	**	305.426	**
1994 Quarter 3	(2.257)		(1.424)		(2.642)	
1994 Quarter 4	306.692	**	312.679	**	305.661	**
1995 Quarter 1	(2.257)		(1.424)		(2.642)	
1995 Quarter 2	306.157	**	312.144	**	305.126	**
1995 Quarter 3	(2.257)		(1.424)		(2.642)	
1995 Quarter 4	312.912	**	318.899	**	311.881	**
1996 Quarter 1	(2.257)		(1.424)		(2.642)	

1996 Quarter 2	315.067 **	321.054 **	
1996 Quarter 3	(2.257)	(1.424)	
1996 Quarter 4	319.572 **	325.559 **	
1997 Quarter 1	(2.257)	(1.424)	
1997 Quarter 2	322.162 **	328.149 **	
1997 Quarter 3	(2.257)	(1.424)	
1997 Quarter 4	327.257 **	333.244 **	
1998 Quarter 1	(2.257)	(1.424)	
1998 Quarter 2	328.597 **	334.584 **	
1998 Quarter 3	(2.257)	(1.424)	
1998 Quarter 4	326.837 **	332.824 **	
1999 Quarter 1	(2.257)	(1.424)	
1999 Quarter 2	331.552 **	337.539 **	
1999 Quarter 3	(2.257)	(1.424)	
1999 Quarter 4	340.917 **	346.904 **	
2000 Quarter 1	(2.257)	(1.424)	
2000 Quarter 2	347.052 **	353.039 **	
2000 Quarter 3	(2.257)	(1.424)	
2000 Quarter 4	348.347 **	354.334 **	
	(2.257)	(1.424)	
trbubble		45.400 **	
		(10.019)	
lead1			-3.420
			(0.000)
lead2			2.480
			(0.000)
lead3			-0.370 **
			(0.000)
lead4			0.970 **

					(0.000)	
lead5					-1.090	
					(0.000)	
lead6					0.080	**
					(0.000)	
lead7					2.340	**
					(0.000)	
lead8					-2.790	**
					(0.000)	
lead9					2.830	
					(0.000)	
lead10					1.889	
					(1.452)	
Intercept	133.770	**	133.770	**	133.350	**
	(0.570)		(0.572)		(0.726)	
Number of observations	272		272		252	

** p<.01, * p<.05

Appendix B: Unusual Acronyms utilized

FHFA: The Federal Housing Finance Agency, in 1996, time of publication of their House Price Index they went by OFHEO (Office of federal Housing Enterprise Oversight)

HPI: House Price Index

Smart Growth Areas Act: the "'Smart Growth' and Neighborhood Conservation - 'Smart Growth' Areas"

OLS: Ordinary Least Squares Regression

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