Vertical Fiscal Interactions and Place-Based Policy

The Case of Local Permit Fees in Opportunity Zones *

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Abstract

I evaluate the effect of Opportunity Zones on localities' permit fees. Targeted localities increase their permit fees by nearly 30%, enough to offset the short-term tax benefits of an Opportunity Zone development. I utilize data from two federal surveys to construct a broad panel of locality-level permit fees. This analysis provides empirical backing to theoretical work on vertical fiscal externalities in federal systems. Furthermore, it substantiates a mechanism that explains the lackluster performance of place-based policies. There are also implications for housing supply: permit fees that rise in response to shocks to developer demand contribute to housing supply inelasticity.

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1 Introduction

There has been a great deal of work examining the effects of place-based policies on their targeted areas, with mixed results. Prior work uniformly assumed local policy to be invariant to treatment. However, the consensus in public finance is that policy is set strategically, with governments responding to one another's policy changes. Theory predicts vertical fiscal interactions: changes in policy from the federal government will prompt strategic local governments to change their own (Agrawal et al., 2024). A reduction in federal taxes increases the size of the local government's tax base through an increase in demand for the taxed activity, altering optimal local policy. I assess how localities (cities, towns) adjust their permit fees in response to a federal tax break and the higher developer interest that comes with it.

I focus on fiscal interactions related to housing development. This is an area where local and state/federal policymakers are often at odds. State and national governments may want to promote housing development to promote equity or economic growth, but localities may want to restrict development for fiscal or exclusionary reasons. Housing development is an area in which localities have strong policy making power. Localities typically control property tax and zoning; they exercise discretion in the approval of permits.

Acquiring permits typically involves concessions and payments from the developer to the locality. Impact fees are intended to charge developers for the effect additional residences will have on public services. Development fees are intended to cover the administration costs of examining and approving development. I refer to these fees collectively as "permit fees". Though the cost of permits is meant to be tied to the cost of public services, recent legal scrutiny has cast doubt on this association (Sheetz v. El Dorado County, 2024). I select permit fees as the local policy response of interest because they are targeted at new development and can be adjusted by local authorities quickly. One would expect localities wishing to capture a recent uptick in developer interest without affecting current residents to reach for this tool.

I measure permit fees using a novel combination of the Census of Governments, the Annual Survey of State and Local Finances, and the Building Permits Survey. I construct a panel of about ten thousand localities' permit fees from 2005-2022. I am able to provide novel descriptive statistics on the distribution and evolution of permit fees over the past two decades.

I estimate local permit fee responses to the Opportunity Zones program. The program incentivizes development in distressed areas. It is perhaps the largest place-based policy implemented in the U.S. to date. Opportunity Zones (OZs) were selected by state policy makers from a list of eligible tracts known as Low Income Communities (LICs). Investments made in OZs receive preferential tax treatment, particularly if investments are made by 2021.[1] The policy was passed as part of the Tax Cuts and Jobs Act of 2017, and the map of zones was finalized mid-2018. Discourse around the program's passage pertained mostly to generating local employment. However, early evidence suggests that two-thirds of the investment has gone into real estate (Kennedy and Wheeler, 2021).[2] I estimate a difference-in-differences model around the implementation of the Opportunity Zones program, comparing localities which gain Opportunity Zones to localities with eligible areas that did not gain OZ status. I find localities increase permit fees significantly. For every 1 percentage point increase in the share of a locality inside an opportunity zone, the locality raises its permit fees about 0.3%.

As a supplemental analysis, I examine locality responses to a 2016 reform to the Low Income Housing Tax Credit's (LIHTC) Difficult Development Areas (DDA) program. LIHTC provides tax credits to developers of multi-family housing in exchange for setting aside a share of units for low income residents. It has subsidized one-third of all recently developed multi-family construction. The DDA program boosts incentives for projects in high rent-to-income areas. In 2016, the geographic units at which DDAs were designated changed from metros to zip codes, shifting some of the boosted incentives across space. I estimate a difference-in-differences model around this reform. I focus on localities which gain DDAs, utilizing inverse propensity weighting to identify comparable localities. The results are consistent with the findings for OZs.[3]

This paper advances three distinct lines of literature. First, this analysis provides well-identified evidence of federal-local fiscal interactions, contributing to the literature on (vertical) fiscal externalities. The theory of vertical fiscal competition is well developed (Keen, 1998; Keen and Kotso-

[1] Investment in an OZ entails selling existing capital and reinvesting the proceeds into a qualifying fund or property. Preferential tax treatment extends to gains from the sale of initial capital, and the gains from the OZ investment. Specifically, investors may claim: a) deferral of capital gains tax on the sale of initial capital through 2026 b) Step-ups in basis of 10% and 15% on their initial capital after five and seven years respectively. (Given the end date of the current program, investments must be made by the end of 2021 to be eligible for the step up.) c) Gains from investment in the OZ fund or property are exempt from capital gains tax if held for 10 years. (Wells Fargo, 2024)

[2] Opportunity Zones lacked mechanisms present in other place-based policies which successfully steer investment towards generating local employment (Freedman et al., 2023a; Freedman and Neumark, 2024)

[3] Appendix B contains all further discussion of the secondary LIHTC DDA analysis.

giannis, 2003). Prior work has provided some empirical evidence for tax interactions (Besley and Rosen, 1998; Brülhart and Jametti, 2006; Devereux et al., 2007; Esteller-Moré and Solé-Ollé, 2001; Hayashi and Boadway, 2001). However, as a recent review by (Agrawal et al., 2024) notes, "... as there is only a single federal government that changes its tax rate periodically, this literature faces challenges because identification comes solely from the time series." In the case of place-based policy, it is as if I can observe two federal governments: one for targeted locations, and one for the rest of the country.[4] Each is exactly like the other, except for preferential tax treatment in Opportunity Zones. This makes place-based policies a powerful tool for studying vertical fiscal interactions. Standard difference-in-differences methods can be used to absorb common shocks which would confound time series analysis. The design of the Opportunity Zones program provides a natural group of control areas. This analysis documents a novel fiscal interaction in terms of the layers of government involved. While national-state, state-county, and even county-locality interactions have been shown, this is the first evidence of national-local interactions. Another novel feature of this analysis is the tax instruments involved. Prior literature focuses on "concurrent" taxation, when two levels of government tax a shared base using similar instruments. Here, I consider federal capital taxation and local permitting fees. These instruments are related in that they are both paid by those investing in housing capital, though at different stages and on different bases. Hoyt (2017) shows this feature does not preclude fiscal interaction, so long as the tax bases are interrelated.

Second, by showing permitting fees are endogenous to developer demand, I contribute to the literature on permit fees and housing supply. Prior empirical work has found a great deal of intra-metro variation in housing supply elasticity (Baum-Snow and Han, 2024). Fees are one such local policy driving this variation. Prior empirical work has found striking cross-sectional correlations between high local permitting fees and low housing supply elasticities (Horton et al., 2024; Ward and Schlake, 2025). I provide the first causal evidence of endogenous permitting fees. Targeted federal tax breaks shock developers' willingness to pay for localities' permits. The tax breaks do not directly affect the cost of public good provision. The fact that permit fees respond to these shocks shows that fees are not anchored to localities' costs, and are increasing in developer demand. This connection will suppress the elasticity of housing supply. The well-developed theoretical literature

[4] Agrawal (2016) overcomes the issue of single federal government by considering local federations, comparing many county-city federal relationships, while Agrawal (2015) examines counties near state borders.

on impact fees is summarized in Burge (2008) . While prior work shows that impact fees may be an efficient alternative to the property tax (Brueckner, 1997), such theory relies on the notion that permit fees are tied to the cost of public goods. In that case, permit fees are offsetting future income taxes. By showing permit fees vary with exogenous changes in developer interest, I suggest the fees are not behaving as the theoretical literature had assumed.

Finally, I complement the literature evaluating place-based policy by identifying a mechanism which explains prior findings. Work which evaluates the efficacy of place-based policy, best summarized in Neumark and Simpson (2015), finds underwhelming results. Early work on Opportunity Zones specifically finds zero effect on resident employment and wages (Freedman et al., 2023b), with conflicting evidence on job growth (Arefeva et al., 2024; Atkins et al., 2023; Shen, 2024). Evidence for effects on the real estate market has been mixed. While some evidence has been found for an increase in permitting (Wheeler, 2023) and construction (Glasner et al., 2025), property values do not appear to increase upon OZ designation (Alm et al., 2021; Chen et al., 2023; Sage et al., 2023). Localities responding to place-based policies with their own counteracting measures could explain (in part) Opportunity Zones' lackluster performance. If localities respond to OZs with higher permitting costs, the benefits of Opportunity Zones are partially exacted by localities before they can generate the intended downstream economic activity.

2 Data

2.1 Measuring Permit Fees

Measuring permit fees has proven challenging, impeding research in the area. The fee schedules of localities are complicated, and depend on the specific attributes of each project. Perhaps for this reason the largest surveys on land policy, the Wharton Residential Land Use Regulation Index and the National Longitudinal Land Use Survey, record limited and infrequent information on impact fees and no information on planning fees (Gyourko et al., 2019; Lo et al., 2019). The National Impact Fee Survey takes a novel approach to capturing impact fee pricing. In order to abstract away from the specifics of each jurisdiction's pricing function, it asks localities to price the impact fees of a few hypothetical developments. However, the sample is small and nonrandom, covering a few hundred localities selected based on consulting relationships (Mullen, 2017a). And, like the more general

surveys mentioned above, the National Impact Fee Survey contains no information on planning fees. Causing further measurement trouble, official permit fees may differ from those actually charged. Fees are often waived for affordable housing development, or during times of decreased developer interest. While localities may post a schedule of impact and planning fees, developers and localities generally negotiate over final payment amounts. Finally, prior work (Lewis and Marantz, 2019) has cast doubt on the reliability of surveying city officials on specific policy parameters, finding inconsistent responses. Without systematic collection of permit fees, the economics, policy, and legal literatures have used case studies focusing on a small number of municipalities (Raetz et al., 2018). Other recent approaches list fees from administrative documents. (Horton et al., 2024) impute impact fees from permitting records in an MLS database at the county level. Reid et al. (2026) extract permitting fees from Low Income Housing Tax Credit applications.

Realized local permit fees are measured by dividing permit revenue by the total number of units constructed in that locality. For my purposes, a locality is a general purpose sub-county government which issues building permits. These include municipalities and townships. They are the smallest unit of government at which planning decisions are made.

Localities' permitting revenue is measured in the Census of Government's (CoG) Annual Survey of State and Local Government Finances (ALFIN) (U.S. Census Bureau, 2022a). Since 2005, the survey has collected revenue and expenditure amounts by detailed category for local governments. The ALFIN line item of interest for this study is "T29" revenue. This captures non-business license fees not classified in the preceding 28 categories. For local governments, this category will consist almost entirely of housing permit fees. The category explicitly includes planning fees and impact fees associated with new development. Helpfully, it explicitly excludes impact fees assessed on existing structures for infrastructure improvement and commercial permits.[5] While this revenue line explicitly includes permitting fees, it also includes other non-business licensing revenue. However, this additional licensing revenue is either uncommonly collected at the locality level (e.g., marriage licenses and registration of aircraft), or minuscule (pet registration fees). In

[5]The exact definition of the T29 revenue categories reads "Licenses not listed separately above (e.g., animal licenses including "per head" levies on livestock, health permits, marriage licenses, building and equipment permits, individual permits to purchase liquor, registration fees on aircraft and pleasure boats, impact fees paid in connection with issuance of building permits, and other non-business privileges)." The categories listed separately above include occupation and business License (T28), and any ad valorem property Tax (T01). Note that the same categorization scheme is used for categorizing state, county, locality, and special district revenue, so some components may not be applicable at all levels of government.(U.S. Bureau of the Census, 2006)

order to confirm that T29 revenue is indeed measuring permitting revenue, I compare it to more detailed revenue data available for select state and years. In Figure A1, I compare the T29 revenue reported in the Census of Governments with the sum of development, impact, and planning fees recorded for each locality in Minnesota, Massachusetts,[6] California, and Florida in 2022. Strong correlation between these two separate measures of revenue is reassuring.

Permitting revenue was collected from all localities in 2007, 2012, 2017 and 2022. In the interceding years, an approximately 15% sample is taken. The sample is non-random. The largest localities by population are guaranteed to be included. Other localities are sampled with a probability inversely proportional to the "size" of the localities' government, measured in either revenues or expenditures depending on the year. Prior work has used the ALFIN without correcting for this sampling procedure, arguing that it implicitly weights results by population (Pierson et al., 2015). However, I am interested in the behavior of localities, not individuals, so implicit population weighting is inappropriate. Instead, I weight observations by the inverse probability of being sampled. In practice, this means up-weighting smaller governments. For the inter-census sample following the 2002 Census of Governments, the ALFIN micro-data contains the appropriate sampling weights. For the inter-census samples following the 2007, 2012, and 2017 Censuses of Governments, the sampling probabilities are not reported in the data. Instead, I estimate a sampling model using logistic regression on the appropriate locality "size" measure.[7] Unless noted otherwise, all results in this paper incorporate this weighting scheme.

The second survey of localities used is the Building Permits Survey (BPS) (U.S. Census Bureau, 2022b). It aims to survey all permitting offices which issue at least six residential construction permits a year. It collects a count of buildings, units, and assessed value of residences constructed. For large localities, data are collected monthly; otherwise data are collected annually by calendar year. Note that the ALFIN collects data for each locality's fiscal year, which varies. About 40% of cities end their fiscal year in June, 40% in December, with the rest scattered throughout the first and third quarters. To match city permitting revenue with the number of permits issued in a fiscal

[6] In Massachusetts, detailed local receipts for development fees are only available from 2023 onward, after current Census of Government data ends. 2022 T29 revenue data are compared to the 2023 Massachusetts DOR state data, with both series being normalized by the number of building permits issued by each locality in the appropriate year.

[7] Summaries of these weights, as well as a breakdown of the structure of the ALFIN's samples is shown in Table A1. Unweighted summary statistics are displayed in Table A2. For a comparison between weighted and unweighted means in key variables, see Table A3.

year, I use the appropriate monthly permit counts, which are available for about half of localities in the sample. For remaining localities whose fiscal year is the calendar year annual BPS counts can be used. For the remaining localities, I take a weighted average of the two calendar years with which their fiscal year overlaps. To define year fixed effects for regressions and event study diagrams, I assign observations to the calendar year with which their fiscal year overlaps most by month, breaking ties in the direction of the later year.

There are advantages and limitations to this method of measuring permit fees. First, it measures the cost of permitting units "as built". This is advantageous in that it measures permit fees post negotiation between developers and local planners. It is disadvantageous in that the attributes of the units being permitted likely depend on the permit cost schedule. For example, if a locality has high permitting fees on multi-family units such that developers only choose to build single family units, I will observe the permitting cost of those single family units, and fail to measure the high latent cost of developing multi-family units. This will bias my empirical exercise if the composition of units permitted changes in response to OZs. There is also the issue of timing mismatch. Recall that the ALFIN measures revenue by fiscal year. Using monthly permit data or imputation across annual (calendar year) data, I try to measure permits issued in the same time period. However, it is possible that permit issuance and fee collection do not happen simultaneously. This will cause measurement error, especially if development in a locality is volatile over time. For example, if a large number of permits are issued in one period, but fees for such permits are collected in the next period, permit fees will appear to plummet then spike. Reassuringly, there is a much stronger correlation between permit revenue and permits issued than a lag or lead of permits issued.[8] This shows issuance and payment generally fall on the same year, though it is still possible that this mismeasurement is occurring for some localities. Finally, there are the disadvantages inherited from each data set. For example, while ALFIN response rates for revenue items are generally high (around 93%), when data is missing the imputation procedure is poor: lifting a value from a similar government. Such observations are dropped. Despite its limitations, this method of measuring permit fees is valuable because it has far broader coverage than any prior approach.

[8] See Table A4.

2.2 Permit Fee Descriptive Statistics

The mean permit cost across all localities in 2022 is $30,000. This is on the higher side of the range Horton et al. (2024) find using involuntary liens for a subset of US counties. It is also higher than the mean of $13,627 (per single family home of $200k in value) found in the most recent survey by Mullen (2017a), though their sample is non-random and they consider only impact fees. My estimate of $56,719 per unit in California falls squarely in the middle of case studies for California cities conducted by Raetz et al. (2018).

Fees have increased substantially since the start of the data series in 2005, where the average cost hovered just over $10,000. The distribution of permit fees is skewed, and has become increasingly so over the past twenty years. The median permit cost is $10,000, and has merely doubled since 2005. See Figure 1(a). Overall, the distribution of permit fees appears to be distributed log-normal, though with a larger share of values near its truncation at zero than would otherwise be expected. See Figure 1(b). Permit fees vary by geography, with fees generally higher in California, New York, Southern New England, Nebraska, Illinois and to a lesser extent Florida. See Figure 1(c).

I regress the log permit cost on a set of locality characteristics including indicators for the locality type, log population, the value and type of housing being constructed, as well as regional indicators. Results of this descriptive model are displayed in column (1) of Table 1. Permit fees tend to be higher in urban areas and high population localities, but less so in central cities. Fees are much lower in township type localities. Taken together, these imply fees are highest in major suburbs. Higher assessed housing unit values are associated with higher fees, and a higher share of multi-family units are associated with lower fees. Fees are highest in the Northeast and lowest in the South. The Midwest has slightly higher fees than the West. In Appendix C, alternative descriptive specifications are explored, including those utilizing state policy indices.

2.3 Other Data

In addition to data used to measure permit fees, I rely on data describing the Opportunity Zones program. This is the primary place-based policy to which permit cost reactions are measured. The locations of Opportunity Zones are available from IRS guidance (Internal Revenue Service, 2018). Tracts eligible to become Opportunity Zones are taken from Community Development Financial

Institutions Fund (CDFIF, 2018). Contextual data on localities are aggregated from tract level data in the NHGIS (Manson et al., 2024).

3 Empirical Strategy

My main identification strategy is a difference-in-differences around the implementation of Opportunity Zones. Areas which are not Opportunity Zones, but were eligible to become OZs are used as controls, following (Freedman et al., 2023b).

A complication of my analysis is that Opportunity Zones are designated at the tract level, while my outcome of interest is the policy response of localities. Localities span multiple census tracts. It is possible that a single locality contains some treated tracts (Opportunity Zones), some control tracts (Low Income Communities not designated as Opportunity Zones), and some tracts meeting neither criteria. In order to aggregate Opportunity Zone status up to the locality level, I measure the share of a locality's population which resides inside an Opportunity Zone and the share of a locality's population which resides in an eligible LIC. The share of the population in an Opportunity Zone ought to be at least as large as the share of the population in an LIC.[9]

Figure 2(a) displays the national map of eligible Low Income Communities and Opportunity Zones, while Figure 2(b) shows the joint distribution of the share of a locality's population living in an OZ and LIC. Due to spatial correlation in the underlying economic phenomena that determine OZ eligibility, it is generally the case that a locality is either near completely covered in eligible Low Income Communities, or near completely lacking them.

Furthermore, when designating OZs, state policymakers tended to select sets of contiguous eligible tracts. The result is that OZ coverage is not evenly distributed conditional on eligibility, but concentrated in a subset of localities . Figure 2(c) displays the distribution of OZ coverage on the sub-sample of localities which had at least 50% of their area covered by eligible LICs. Note the bimodal distribution, with most localities having near 0% OZ coverage, while a sizable minority had near 100% coverage.

A simple first approach splits localities into treatment and control groups by LIC and OZ

[9]There are a small number of exceptions to this. See Figure 2(b). State policy makers were allowed to select a small number of non-LIC tracts as Opportunity Zones if they were adjacent to a LIC designated as an Opportunity Zone. 3% of Opportunity Zones were designated in this fashion.

coverage. I wish to compare localities largely covered by OZs to localities which were eligible to be largely covered by OZs, but were not. Localities with 50% or more of their population in an Opportunity Zone are considered treated. Localities with less than 50% of their population in an OZ, but at least 50% of their population in an LIC are the control group. Localities with less than 50% LIC coverage are discarded for the binary analysis.

Table 2 assesses the balance between treated and control localities. It identifies some statistically significant differences in levels. Treated localities are smaller than control localities, have a larger Black share of population, a slightly higher share of multi-family housing, are more likely to be townships, and less likely to be central cities. While balance on observables is ideal, only parallel trends are needed for valid inference. When looking at trends in covariates ahead of treatment, only different trends in unemployment predict treatment. Despite some imbalance across covariates, no significant pre-trends are detected in the event study, see Figure 3(a).

In the binary specification, I regress log permit cost, $\log\left(T29\ Revenue_{it}/Total\ Units\ Permitted_{it}\right)$, on the interaction between an indicator for high OZ coverage, $\mathbb{1}(OZ\ Share_i \geq 50\%)$, and an indicator for post 2017 observations. Included in the model are fixed effects for localities, γ_i, and year, ψ_t. This specification estimates a simple binary difference-in-differences by way of two-way fixed effects. The binary specification is displayed in equation (1).

$$\log\left(\frac{T29\ Revenue_{it}}{Total\ Units\ Permitted_{it}}\right) = \beta\left(Post_t \times \mathbb{1}\left(OZ\ Share_i \geq 50\%\right)\right) + \gamma_i + \psi_t + \epsilon_{it} \quad \text{if} \quad LIC\ Share_i \geq 50\% \tag{1}$$

While conceptually simple, the binary specification discards many observations and requires researcher discretion splitting localities into treatment and control bins. A continuous specification utilizes all the variation available in my setting. Treatment is simply the share of a locality's population in an Opportunity Zone, which varies between zero and one, $OZ\ Share_i$. As in the binary model, this variable enters the regression interacted with $Post_t$. The coefficient on this interaction, β, can be interpreted as the percent increase in permit costs from a 1 percentage point increase in the share of a locality's residents in an OZ. My estimation of this effect is made conditional on locality eligibility to receive OZ designation. In practice, this is achieved by including the share of

a locality's population residing in an LIC interacted with year indicators, ($LIC\ Share_i \times Year_t$). This controls for eligibility for OZ designation even if its effect on permit fees is time varying. As in the binary specification, locality, γ_i, and year, ψ_t, fixed effects are included. Note that unlike the binary specification, no sub-setting of the data is required, and all complete observations are included in the model. The continuous specification is displayed in equation (2).

$$\log\left(\frac{T29\ Revenue_{it}}{Total\ Units\ Permitted_{it}}\right) = \beta\left(Post_t \times OZ\ Share_i\right) + \gamma_i + \psi_t + \theta_t\left(LIC\ Share_i \times Year_t\right) + \epsilon_{it} \tag{2}$$

Note that because treatment occurs at the same time for all units, I need not contend with confounding dynamic effects in previously treated controls documented by (Goodman-Bacon, 2018). Dynamic effects of treatment are explored with an event study version of this specification, where the $Post_t$ indicator is swapped for a series of year indicators. Further note that the outcome is log transformed to deal with its skewed distribution. While this means there are many zero values that will be dropped, a separate analysis in the robustness section will check for extensive margin effects, an approach recommended by (Aihounton and Henningsen, 2021). Given the panel setting, standard errors will be clustered at the locality level.

4 Results

Headline results are presented in Table 1. The binary specification, presented in column (2), finds localities with high OZ coverage see permit costs increase 27% relative to control localities. Effects of a similar magnitude are detected in the continuous treatment specification, displayed in column (3). Note the interpretation is slightly different: a 1 percentage point increase in locality coverage with OZs causes a 0.29% increase in permit fees. In other words, complete coverage of a locality with Opportunity Zones causes a 29% increase in permit fees.

Headline results survive the inclusion of state by year fixed effects. See column (4). This mitigates concerns that results are driven by changes in state policy concurrent with treatment, for example differential severity in pandemic restrictions. One might also fear that higher observed

permit fees simply reflect changing composition in the units being constructed. Supplemental results show this not to be the case; Opportunity Zones change neither the value of units built, nor the share of units which are multifamily.[10] Furthermore, results are similar, if slightly more precise, when permit fees are measured relative to the assessed value of construction in a locality, rather than the number of units, as is done in column (5). This implicitly controls for the value of units being produced.

The event study equivalent of the binary specification is presented in Figure 3(a). It shows no significant pre-trends or anticipatory effects. The treatment effect in the post period builds in 2019-2021, but dissipates by 2022. The event study equivalent of the continuous treatment specification is presented in Figure 3(b). It shows a similar pattern.

I check for heterogeneous effects across the distribution of permit fees using modified versions of the continuous specification. First, I check for an extensive margin effect on permit fees. Recall that many localities report no permitting revenue despite local construction occurring. My measure of permit fees for such localities is zero. Localities with zero permit cost are excluded from the headline results to accommodate the use of a log transformation. To check for effects on zero permit cost localities, I construct an indicator for positive permit fees as an alternative outcome variable. I also perform quantile regression checking for heterogeneous effects across the distribution of permit fees conditional on being positive. Quantile regression with fixed effects is performed using methods developed in (Machado and Santos Silva, 2019). Note that state by year fixed effects are not included for computational feasibility. These results are presented in columns (6)-(9) of Table 1. There appears to be no extensive margin effect of Opportunity Zone designation on permit fees. Quantile regression results show a clear effect for the bottom half of the permit cost distribution, and smaller effects for the top of the distribution.[11] This pattern implies that the increases in mean permit fees detected in the headline results arise primarily because Opportunity Zone designation prompts low permit cost localities to raise their permit fees.

Due to the structure of the ALFIN, previously discussed results and event studies are based on an unbalanced panel of localities. For robustness, I calculate results using two balanced panels. The first uses all localities which are present in both the 2014-2018 and 2018-2023 inter-census samples of

[10] These contextual results are presented in Table A5.

[11] I estimate results for additional quantiles, which confirm this pattern. See Table A6.

the ALFIN. Being doubly selected into both of these samples compounds the issue of non-random sampling and the corrective weighting is more extreme. This sample yields a result of similar magnitude to the headline result, though with far less precision. The second balanced panel relies only on data from Census of Government years. Only data from the 2022 Census of Governments is post treatment. Based on the headline event study, this is also after treatment effects dissipate. Perhaps not surprisingly, the treatment effect is estimated to be zero. See Table A7.

It may seem concerning that the treatment effects dissipate before the next Census of Governments year. There are reasons to expect the policy's effects to be short-lived. The tax benefits of Opportunity Zones are temporary, expiring in 2027.[12] Some advantageous treatment requires capital to be held for at least 5 years. It is unclear that a property development project with permits granted as late as 2022 would allow the investor to take advantage of the Opportunity Zones program before this iteration of the program ends. Another caveat of this analysis is that the main results are not robust to omission of the sampling weighting scheme.[13] This could be explained by a great deal of the identifying variation arising from small localities, which need to be up-weighted as they are under-sampled. However, it also means the results are dependent on getting the sampling weighting scheme right.

To contextualize results, let us consider a typical investment in a typical OZ. In localities with at least 50% of the population residing in an OZ, the mean 2017 permit fee per unit was about $11,000. Applying the coefficient from my binary difference-in-differences model, this implies an average increase of $3,000 per unit. The value of the favorable tax treatment depends on several project specific attributes. However, we can consider a benchmark per-unit investment. Let us assume a developer invests $200,000 per housing unit, sees a 7.2% rate of return [14], funds their investment by selling a non-OZ asset that they held for ten years, that they will hold their OZ investment for ten years, and that the investment was made prior to 2022. Finally, assume that the investor faces the maximum 20% capital gains rate. [15] Such an investor received $3,000 in short term tax relief due to the step-up in basis on their pre-OZ investment capital gains. After holding the OZ investment for 10 years, they will see $40,000 in tax relief on the capital gains of their OZ

[12]The program was been reformed and extended indefinitely in 2025 (Wessel, 2025). New zones will be selected in 2026.

[13]Unweighted results are presented in Table A8. Also displayed are results weighted by population and construction.

[14]This is chosen for ease of exposition. Investments double every ten years.

[15]This is reasonable, Kennedy and Wheeler (2021) finds OZ investors are extremely high income.

investment. In this illustrative example, the increase in permitting costs is enough to offset the short term tax benefits offered by OZs, but not enough to offset the relief to gains made from OZ investments.

5 Conclusion

This analysis finds that local policy is sensitive to federal policy. Place-based policies which aim to spur development in specific areas may be frustrated by local policy responses. This finding suggests that the fees localities charge developers are set with developer willingness to pay in mind, i.e. they are not firmly anchored to the costs development imposes on a locality.

> "[...] we're constantly monitoring what the impact is of our fees on the overall development. We actually take a look yearly [...] [at] the economic feasibility of projects in the city, understand how our fee stack lines up in the total development stack, and then, our City Council can make adjustments to those fee programs if they feel it's necessary."
>
> – *Jerad Ferguson, Principal Planner for the City of San Jose*
> *During a Terner Center Seminar on Development Impact Fees*

This analysis measures permit fees in a novel way, allowing annual measurement since 2005 with broad coverage across localities. I provide new descriptive statistics on permit fees, tracing their growth and increasing dispersion over time. Using this data, in conjunction with differential tax treatment across space from place-based policy, I perform a difference-in-differences analysis. I estimate novel vertical fiscal interactions between the federal government and localities.

There are also important implications of these results for those who study place-based policies. First, theoretical predictions of place-based policy will miss their mark if fiscal externalities are not accounted for. There are also implications for empirical evaluation of place-based policy. Local policy responses provide a channel by which the effects of place-based policies may spill outside their boundaries into the rest of their locality. This channel for spillovers will bias "close-control" methods, where treated areas are compared to nearby untreated areas.

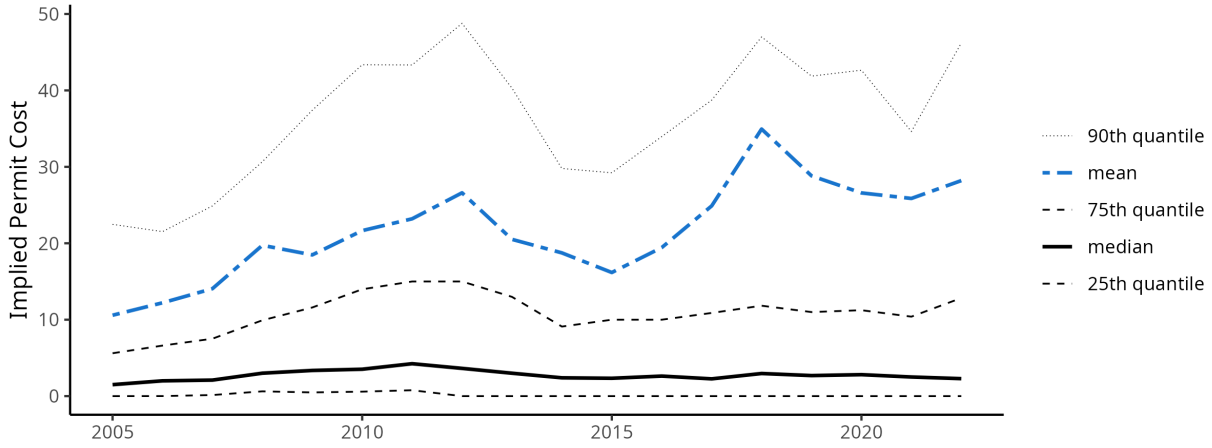
There are implications for housing supply. I show that permit fees are sensitive to developer demand. In this case, increased developer demand was induced by place-based policy. However, one

would expect this pattern to generalize. Housing supply in high demand cities may be constrained as cities raise fees to take advantage of the high level of developer interest. Though if a shock to demand for housing is spread out across many localities, horizontal fiscal competition may help keep permit costs at bay.

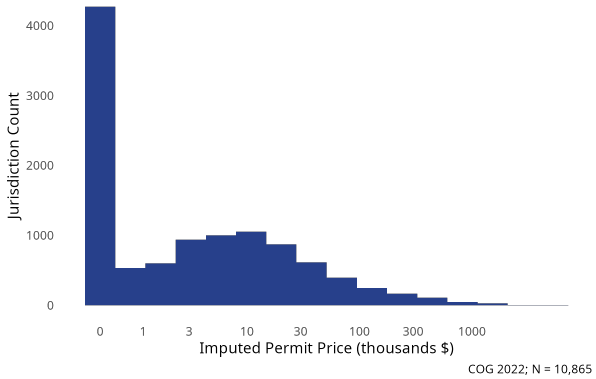
Exhibits

Figure 1: Distribution of Permit Fees

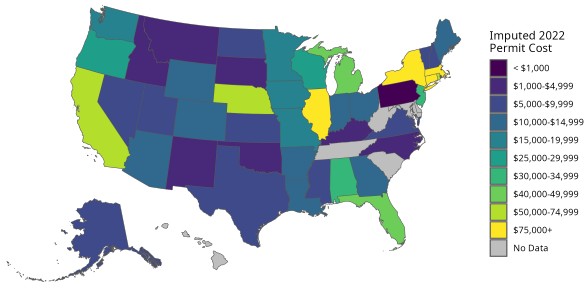
(a) Evolution of Permit Fees



(b) 2022 Permit Fee Histogram



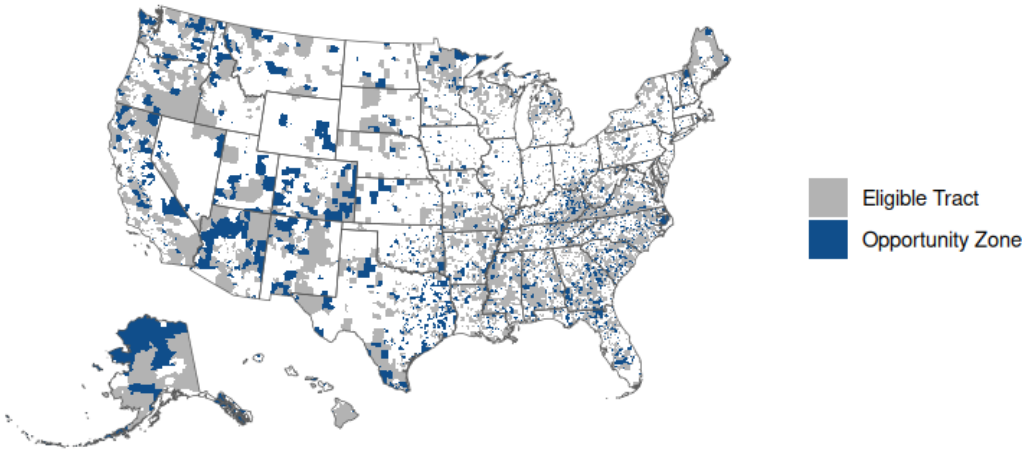
(c) Typical Permit Fees by State



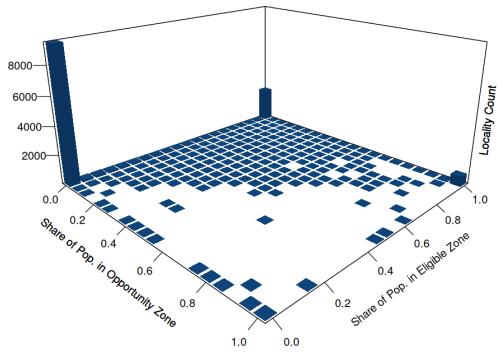
Map displays mean permit fee weighted by locality size. Coverage in greyed out states is poor, and so they are excluded from the map. All states, coverage rates, and some alternative aggregation strategies are displayed in Figure A2.

Figure 2: OZ and LIC Distribution

(a) Map of Qualified Opportunity Zones and Eligible Opportunity Zones



(b) Distribution of EOZs and QOZ in Localities



(c) Distribution of QOZ | >50% EOZ

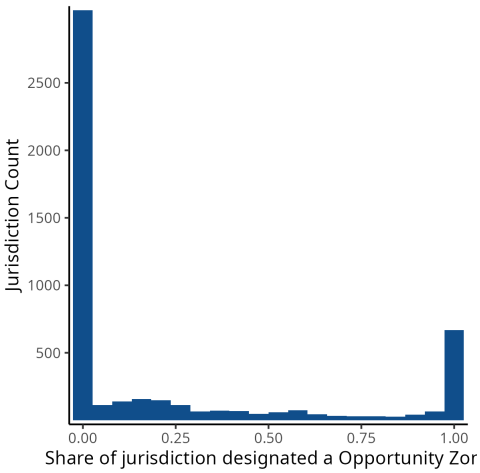
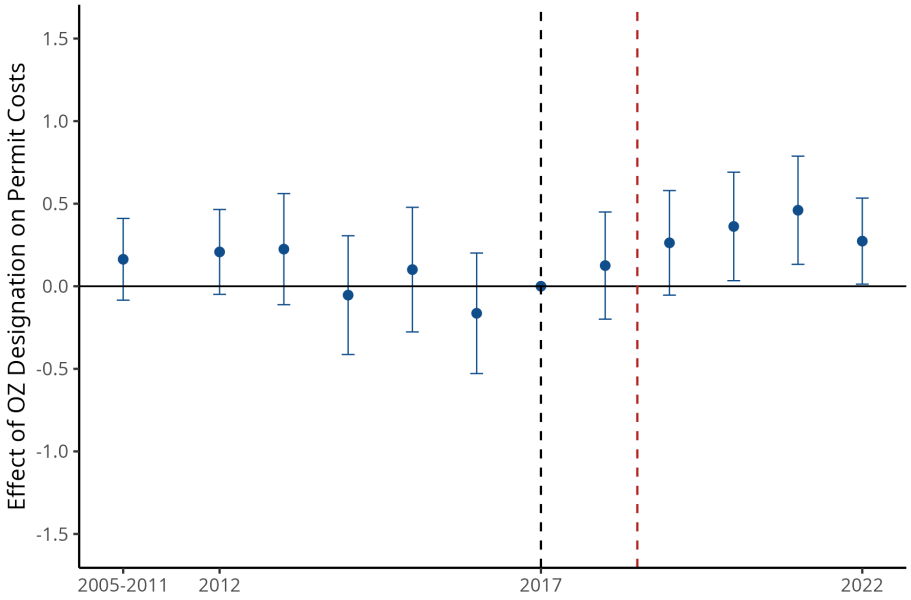
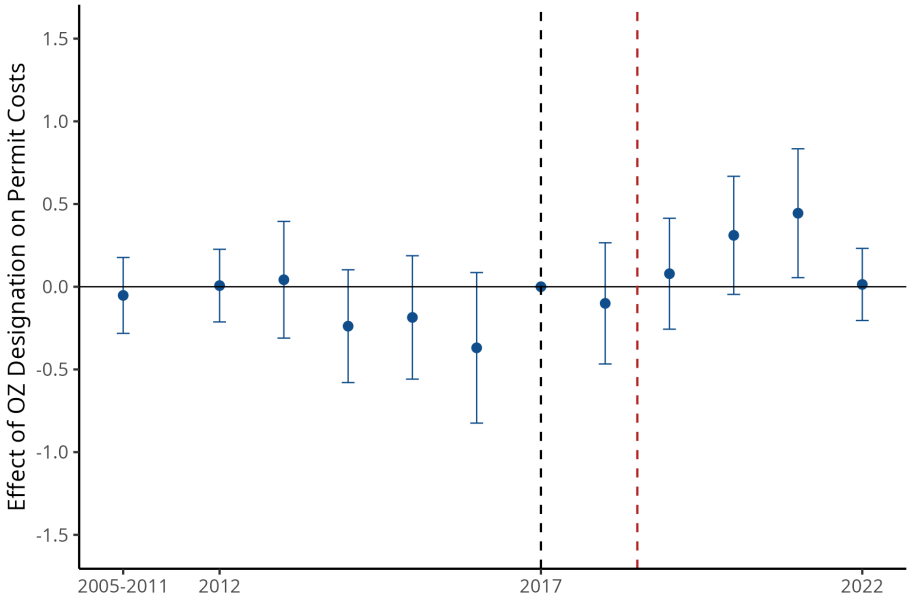


Figure 3: Event Studies

(a) Event Study: Permit Prices at OZ Launch with Binary Treatment



(b) Event Study: Permit Prices at OZ Launch with Continuous Treatment



Blue Dashed line approximates announcement of policy; red dashed line shows policy implementation.

Table 1: Permit Costs Regression Results

Dependent Variables:	Log Permit Cost per Unit				Log Permit Cost per Assessed $	Permit Cost >0 Indicator	Log Permit Cost per Unit Quantile		
Model:	(1)	(2)	(3)	(4)	(5)	(6)	0.25th (7)	0.50th (8)	0.75th (9)
$\mathbb{1}(OZ\ Share_i > 50\%) \times Post_t$		0.2730**							
		(0.1274)							
$OZ\ Share_i \times Post_t$			0.2909**	0.2903**	0.3015***	0.0082	0.3934*	0.3248**	0.2584
			(0.1343)	(0.1199)	(0.1151)	(0.1199)	(0.2172)	(0.1592)	(0.2147)
Central City Indicator	-0.3695***								
	(0.0745)								
Urban Indicator	0.2100***								
	(0.0449)								
Township Indicator	-0.9224***								
	(0.0658)								
Log Population	0.3791***								
	(0.0167)								
Log Avg. Assessed $	0.4167***								
	(0.0337)								
Share of Units Multifamily	-0.0120***								
	(0.0007)								
Northeast Region	0.8506***								
	(0.0652)								
Southern Region	-0.6056***								
	(0.0590)								
Western Region	-0.2328***								
	(0.0608)								
Midwest Region	–								
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Locality FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE				Yes	Yes				
Share in LIC × Year FE			Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	61,315	8,598	61,186	61,186	61,221	101,410	61,236	61,236	61,236
Localities	11,884	2,393	11,910	11,910	11,912	18,766	11,916	11,916	11,916

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Locality clustered standard errors in parentheses. All models are weighted by inverse probability of sampling weights (See Table A1). In column (2), the sample is restricted to only those localities with over 50% of their population residing in a LIC eligible to become an OZ. Models include data from 2005-2022.

Table 2: Balance Table

	$< 50\%$ Pop. in OZ	$\geq 50\%$ Pop. in OZ	Coef with Binary OZ	Coef. with Continuous OZ
Levels 2017				
Pop	40500	11000	-0.000 (0.000)*	-0.000 (0.000)
Pop per km^2	491	226	-0.000 (0.000)*	-0.000 (0.000)*
Share Black	0.108	0.124	0.175 (0.082)*	0.126 (0.041)*
Share Asian	0.0544	0.0394	-0.04 (0.155)	-0.065 (0.039)
Share Hispanic	0.135	0.133	-0.026 (0.076)	-0.005 (0.033)
Share with Bachelors	0.19	0.175	-0.071 (0.138)	0.028 (0.024)
Employment/Pop	0.566	0.544	-0.327 (0.187)	-0.024 (0.041)
Unemployment Rate	0.0762	0.0786	-1.03 (0.444)*	-0.33 (0.159)*
Poverty Rate	0.189	0.206	0.409 (0.261)	0.347 (0.093)*
Median HH Income	45000	42700	0.000 (0.000)	0.000 (0.000)
Total Revenue (thou.$)	127000	13500	-0.000 (0.000)	0.000 (0.000)
Permit Revenue (thou.$)	1180	105	-0.000 (0.000)	0.000 (0.000)*
Permit Revenue Share	0.00995	0.00761	-0.13 (0.647)	-0.081 (0.083)
Total Units Permitted	107	15	-0.000 (0.000)	-0.000 (0.000)*
Avg. Unit Value (thou.$)	168	156	-0.000 (0.000)	-0.000 (0.000)
Share of Multi-family Units	0.178	0.169	0.09 (0.048)	0.04 (0.013)*
Township Indicator	0.191	0.0968	-0.121 (0.027)*	-0.031 (0.005)*
Urban Indicator	0.354	0.213	-0.019 (0.024)	-0.004 (0.005)
Central City Indicator	0.0978	0.00568	-0.1 (0.031)*	-0.019 (0.009)*
Changes 2013-2017				
Δ Pop	1430	71.9	0.000 (0.000)	0.000 (0.000)
Δ Pop per km^2	8.96	-1.69	0.000 (0.000)	-0.000 (0.000)
Δ Share Black	0.00166	0.00129	0.28 (0.46)	0.119 (0.16)
Δ Share Asian	0.0019	0.000353	0.105 (0.23)	0.029 (0.101)
Δ Share Hispanic	0.0102	0.00876	-0.406 (0.435)	-0.037 (0.124)
Δ Share with Bachelors	0.0147	0.0128	-0.177 (0.351)	-0.008 (0.069)
Δ Employment/Pop	-0.00906	-0.00765	0.345 (0.292)	0.112 (0.083)
Δ Unemployment Rate	-0.0281	-0.0246	0.879 (0.341)*	0.212 (0.115)
Δ Poverty Rate	-0.000682	-0.00202	-0.262 (0.289)	-0.117 (0.098)
Δ Median HH Income	3230	3010	-0.000 (0.000)	-0.000 (0.000)
Δ Total Revenue (thou.$)	6720	1400	-0.000 (0.000)	0.000 (0.000)
Δ Permit Revenue (thou.$)	500	33.3	0.000 (0.000)	0.000 (0.000)
Δ Permit Revenue Share	0.00142	0.0014	-0.35 (0.71)	-0.046 (0.097)
Δ Total Units Permitted	54.1	5.45	-0.000 (0.000)	0.000 (0.000)
Δ Avg. Unit Value (thou.$)	23.2	18.8	0.000 (0.000)	0.000 (0.000)
Δ Share of Multi-family Units	0.0068	0.0178	-0.021 (0.039)	-0.012 (0.012)
(Intercept)			0.461 (0.145)*	0.009 (0.033)
Share of Pop in QOZ				0.25 (0.013)*
N	751	2,550	1,810	6,410
R^2			0.07	0.29

Columns 1 and 2 show attributes of localities split by whether at least 50% of their population resides inside OZ. The sample is restricted to localities in which and at least 50% of their population resides in an LICs. Column 3 shows coefficients from a linear probability model in which the outcome is a binary indicator of at least 50% of a locality's population will resides in an Opportunity Zone, again conditional on at least half of the population residing in an LIC. Column 4 shows coefficients from a model in which the share of a place covered in Opportunity Zones is the outcome variable. Note that in this model I control for the share of the locality's population which resides in a LIC eligible to be designated an Opportunity Zone. Stars indicate significance at the 5% level. Sample restricted to localities with sufficient permit price information to be used in main analysis. Number of localities in columns 1 and 2 do not sum to number utilized in models in columns 3 due to undefined values in data, particularly those variables which are undefined when the number of units built in is zero. Revenue data pulled from 2012 and 2017 Census of Governments, so no weighting of inter-census observations is needed. Household demographic and economic indicators are sourced from 5-year ACS averages ending in 2012 and 2017.

Acknowledgements

I would like to thank Matthew Freedman, Jan Brueckner, David Agrawal, David Neumark and Miguel Zerecero for their guidance. Thank you to conference participants at the 2024 UCI Urban/Public Summer Conference, the 2025 American Real Estate and Urban Economics Association ASSA Conference, and the 100th Annual Western Economics Association International Conference, especially Rallye Shen. I would also like to thank Olivia Sheppard for her careful proof reading. I am grateful for the financial support of the Lincoln Institute of Land Policy.

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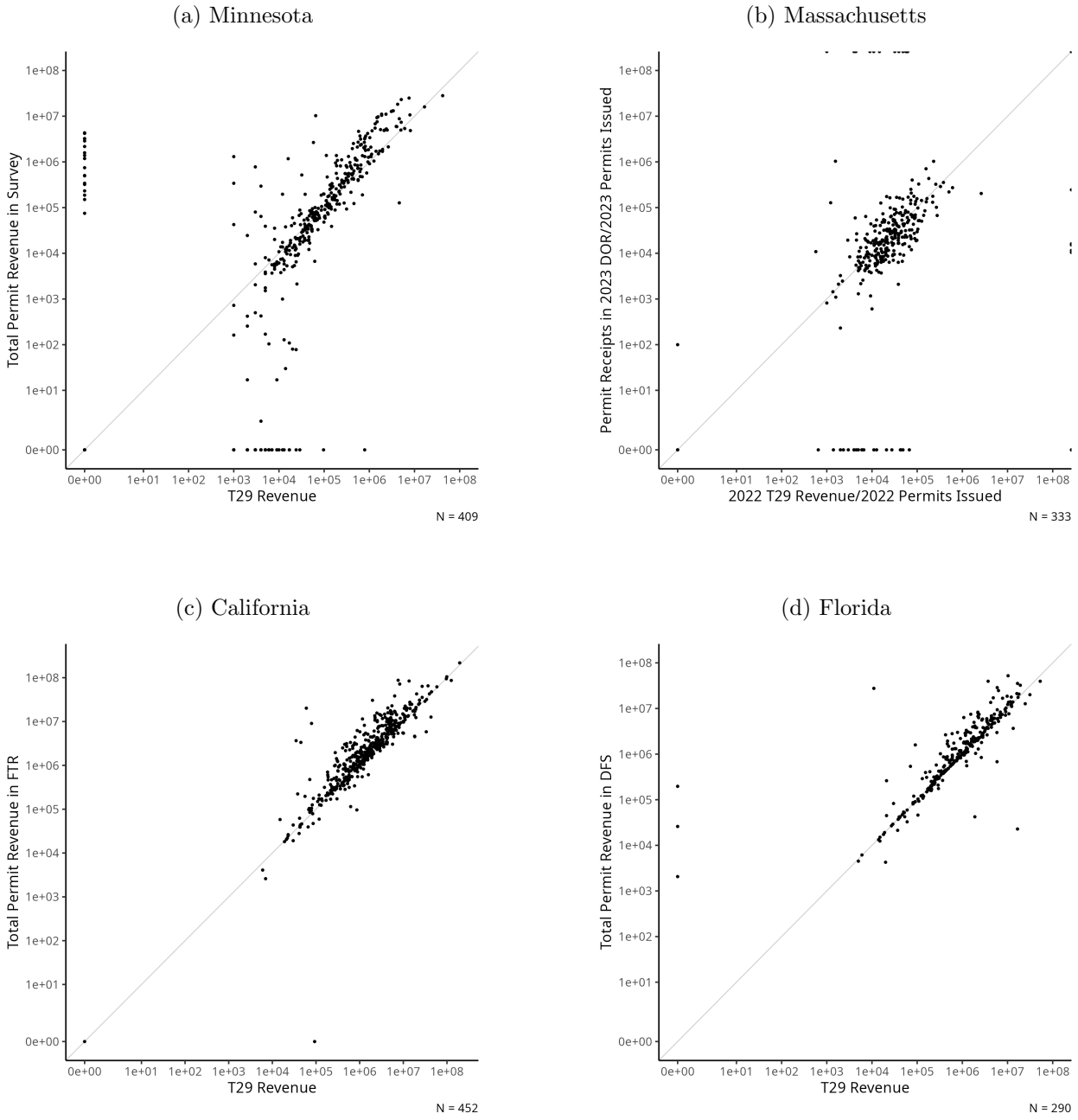
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A Supplemental Exhibits

Figure A1: Comparison to State Administrative Data



State data sources: California State Controller's Office (2022), Minnesota Department of Labor and Industry (2022), Massachusetts Department of Revenue (2023), Florida Department of Financial Services (2022)

Figure A2: Typical Permit Fees by State

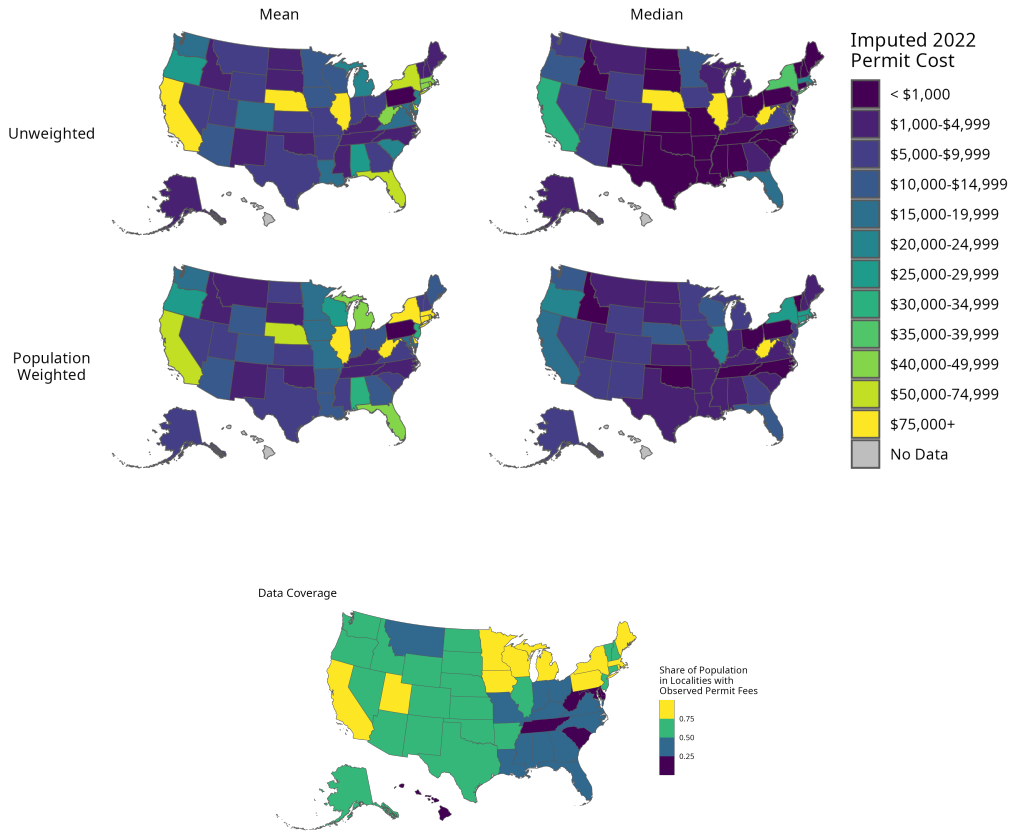


Table A1: Census of Government and Annual Survey of State and Local Government Finances Sample Composition

	Years	Description	Locality Count	Weight Mean (s.d)	Pop. Mean (s.d.)	Revenue Mean (s.d.)
Census	2007, 2012, 2017, 2022	All sub-county general purpose governments	36,115	1 (0)	7,000 (62,000)	$17mil ($581mil)
Annual Sample	2005-2022	Sub-county general purpose governments with populations over 200,000.	120	1 (0)	562,000 (872,000)	$2,415mil ($9760mil)
Inter-census A	2005, 2006, 2008	Sampled with probability inversely proportionate to government size, defined by the maximum of 2002 expenditures and debt.	4,834	7.27 (12.69)	19,000 (29,000)	$29mil ($59mil)
Inter-census B	2009-2011, 2013	Sampled with probability inversely proportionate to government size, defined by the maximum of 2007 revenue, expenditures, debt, and assets.	4,381	6.22 (5.44)	23,000 (31,000)	$46mil ($85mil)
Inter-census C	2014-2016, 2018	Sampled with probability inversely proportionate to government size, defined as 2012 revenue.	4,403	6.16 (5.22)	22,000 (30,000)	$50mil ($91mil)
Inter-census D	2019-2021	Sampled with probability inversely proportionate to government size, defined as 2017 revenue.	4,482	6.31 (4.91)	22,000 (31,000)	$53mil ($98mil)
OZ Balanced Sample	2014-2022	Localities in Inter-census C & D or Annual Sample	2,782	29.65 (50.59)	54,000 (218,000)	$186mil ($2202mil)

Population and revenue figures for each sample is taken from the last year used to determine sample eligibility.

Table A2: Summary Statistics

Statistic	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max	N
Permit Fee	$29,265	$149,356	$0	$55	$3,875	$15,027	$10,344,500	81,189
Permit Revenue	$852,794	$7,103,038	0	0	$20,000	$339,000	$707,234,000	104,566
Total Units Constructed	100	576	0	1	6	40	52,618	104,566
Share of Units Multi-Family	0.17	0.28	0	0	0	0.24	1	81,189
Appraised Value of Units Constructed	$17mil	$86mil	$0	$0.91mil	$1.1mil	$7.4mil	$6,212mil	104,566
Annual BPS Used	0.29	0.46	0	0	0	1	1	104,566
Locality Population	26,084	147,393	0	1,346	5,208	19,383	8,622,698	104,566
Central City Indicator	0.08	0.28	0	0	0	0	1	104,566
Urban Indicator	0.44	0	0	0	0	1	1	104,566
Township Indicator	0.21	0.41	0	0	0	0	1	104,566
Total Locality Revenue	$82,180mil	$1,460mil	$0	$1.4mil	$8.1mil	$37,227mil	$132,619mil	104,566
Permit Revenue Share	0.013	0.027	0	0	0.004	0.014	0.817	104,560
Sampling Weight	2.8	4.3	1	1	1	1.5	50	104,497

Note that no weighting scheme is applied to the summary statistics in this table. For weighted means see Table A3.

Table A3: Census of Government and non-CoG Years Means Comparison

	CoG Years	All non-CoG Years in 2005-2022	
	('07, '12, '17, '22)	Unweighted	Weighted
Permit Fee	$23,960	$34,685	$21,856
Permit Revenue	$442,115	$1,402,395	$364,786
Total Units Constructed	52	164	49
Share of Units Multi-Family	0.12	0.21	0.14
Appraised Value per Unit Constructed	$182,415	$164,993	$172,647
Annual BPS Used	0.31	0.27	0.31
Locality Population	13,010	43,584	12,995
Central City Indicator	0.04	0.15	0.03
Urban Indicator	0.36	0.54	0.36
Township Indicator	0.24	0.18	0.20
Total Locality Revenue	$37mil	$142mil	$35mil
Permit Revenue Share	0.013	0.013	0.011
N (Locality × Years)	59,811	44,686	44,686

Table A4: Permit Revenue and Issuance Correlations

Dependent Variable:	asinh(T29 Revenue$_t$)
Model:	(1)
asinh(Units Permitted$_{t-1}$)	0.0222
	(0.0275)
asinh(Units Permitted$_t$)	0.1090***
	(0.0280)
asinh(Lead Units Permitted$_{t+1}$)	-0.0006
	(0.0308)
Locality FE	Yes
Year FE	Yes
Observations	67,803
R^2	0.81449

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Locality clustered standard errors in parentheses. Because the number of units permitted is skewed but frequently contains zeros, I apply an inverse hyperbolic sine transformation. The same transformation is applied to T29 revenue.

Table A5: Contextual Results

Dependent Variables:	Share Multi-Family	Log Value per Unit	Log Permit Revenue	Log Total Units
Model:	(1)	(2)	(3)	(4)
Share in OZ × post	0.0036	0.0075	0.0818	-0.1417*
	(0.0208)	(0.0375)	(0.0855)	(0.0756)
Year FE	Yes	Yes	Yes	Yes
Locality FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Share in Eligible Tract × Year FE	Yes	Yes	Yes	Yes
Observations	80,989	80,964	71,952	80,989
Localities	15,900	15,889	13,641	15,900

Locality clustered standard errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A6: Additional Quantile Results

Dependent Variable:	Log Permit Cost per Unit Quantile						
	0.1	0.25	0.4	0.50	0.6	0.75	0.90
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share in OZ × Post	0.4339	0.3934*	0.3540**	0.3248**	0.2948*	0.2584	0.2164
	(0.2843)	(0.2172)	(0.1711)	(0.1592)	(0.1721)	(0.2147)	(0.2837)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Locality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	No	No	No	No	No	No	No
Share in Eligible TRact × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quantile Dept. Variable	808	2,005	3,967	5,786	8,584	17,308	52,388
Observations	61,236	61,236	61,236	61,236	61,236	61,236	61,236
Localities	11,916	11,916	11,916	11,916	11,916	11,916	11,916

Locality clustered standard errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A7: Results Restricted to Balanced Panels

Dependent Variable:	Log Permit Cost per Unit	
Model:	(1)	(2)
Share in OZ × Post	0.4000	-0.0137
	(0.2513)	(0.0979)
Year FE	Yes	Yes
Locality FE	Yes	Yes
State × Year	Yes	Yes
Share in Eligible Tract × Year FE	Yes	Yes
Observations	18,050	28,148
Localities	2,424	11,594
Sample Restriction	Balanced 2013-2022	CoG Years Only (2007, 2012, 2017, 2022)

Locality clustered standard errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A8: Alternative Weighting Schemes

Dependent Variable:	Log Permit Cost per Unit			
Model:	(1)	(2)	(3)	(4)
Share in OZ × Post	0.2903**	0.1271	0.1664	0.2252
	(0.1199)	(0.0852)	(0.1481)	(0.1901)
Year FE	Yes	Yes	Yes	Yes
Locality FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Share in Eligible Tract × Year FE	Yes	Yes	Yes	Yes
Observations	61,186	61,236	61,113	61,186
Localities	11,910	11,916	11,880	11,910
Weighting	Inverse Sampling Probability	Population	Units Permitted	Unweighted

Locality clustered standard errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

B Alternative Policy Shock: Difficult Development Areas

As an alternative policy shock, I leverage a reform to Difficult Development Areas (DDAs), a component of the Low Income Housing Tax Credit (LIHTC). The LIHTC offers generous tax credits to developers of multifamily housing in exchange for setting some units aside for low income residents. The tax credits are generally worth the offset, and allocations for LIHTC are exhausted long before developer demand is met. The LIHTC can be thought of as a federal subsidy for multifamily housing development. Difficult Development Areas are geographies within which the generosity of LIHTC is increased by 30%. DDAs are selected by selecting the geographies with the highest rent to income ratios, until 20% of the country resides within a DDA.

In 2016, there was a major reform to the DDA selection process. Prior to this reform, the geographic unit at which DDA status was assigned was the HUD metropolitan area. See the yellow and green regions in Figure B1(b). From 2016 onward, DDA status was awarded at the zip code level. See the blue and green regions inFigure B1(b). The effect was a sudden spatial redistribution of LIHTC generosity. While previously DDAs had been confined largely to coastal regions in California, Florida, and the New York City Metro, they have been spread out more finely across high cost areas throughout the country.

The decision to change the geography at which DDA status is defined was due to the develop-

ment of HUD's Small Area Fair Market Rents, which were initially calculated in 2010. The policy change was announced in October of 2013, when SAFMRs were deemed reliable enough for policy application. DDAs were reclassified based on the new geography in 2015, for use in 2016. However, localities could view HUDS ranking of zip codes for DDA eligibility when the policy was announced in 2013. Though DDAs are defined for rural areas, only metropolitan areas experienced the SAFMR reform, so this analysis is limited to metropolitan localities. DDA status assignment under the new and old policy are provided by HUD (U.S. Department of Housing and Urban Development, 2023).

This analysis focuses on localities which previously were not covered in DDAs, and were either completely or partially covered by new DDA zip codes. These localities could be compared to those which never received any DDAs. There are also many localities which had been completely within DDAs suddenly lost their more generous subsidy, but they are fewer in number and are not analyzed.

Similar to the Opportunity Zones case, I leverage the SAFMR reform using difference-in-differences with continuous treatment estimated using two way fixed effects. Treatment is defined as the share of a locality's population covered by DDA zip codes. Among localities for which I have complete data, about 75 localities are mostly covered by DDAs, with 26 being completely covered by DDAs. About 150 have the minority of their areas covered by DDAs and 400 never receive any DDA treatment.

DDAs are assigned to zip codes with high rent to income ratios. Therefore, localities containing DDAs are likely to be economically distinct from localities which never gained one. Indeed, looking at Table B1, which shows balance between treated and untreated localities, it is clear that localities which receive more treatment are higher income, lower poverty, and have lower employment. Reassuringly, while there are differences in the levels of these measures across treated and control localities, there is no statistically significant difference in trends. Locality fixed effects should deal with these level differences.

Nevertheless, I attempt to improve the comparability of treatment and control groups through inverse propensity score weighting. The method by which DDAs are assigned to zip codes is known. High rent to income ratio ZCTAs receive DDA status, others do not. The propensity of a locality to receive a DDA should therefore depend only on its zip codes' rent to income ratios. I estimate the propensity for a locality to receive at least one DDA zip code using a) the mean of its zip codes'

rent to income ratio, b) the standard deviation of its zip codes' rent to income ratios, and c) the interaction between the two. The idea is that a locality is likely to gain a DDA zip code if it either generally has a high rent to income ratio, or has a large spread between its highest and lowest rent to income ratio zip codes. I want to allow effects to interact. I estimate propensity using a logistic regression. Estimates of the above model are displayed in Table B1. Using the estimated probabilities of treatment, I calculate inverse propensity weights. Balance between the weighted control and weighted treatment groups is displayed in Table B3. Note some level differences remain.

As with Opportunity Zones, I estimate a difference-in-differences around the reform date. Log permit costs are regressed on the share of a locality's population residing in a difficult development area, interacted with an indicator for the treatment period. Fixed effects for year, ψ_t, and locality, γ_i, are included. The coefficnet β estimates the ATT over the treatment period. The model is displayed in Equation B1.

$$\log\left(\frac{T29\ Revenue_{it}}{Total\ Units\ permitted_{it}}\right) = \beta\left(Post_t \times DDAShare_i\right) + \gamma_i + \psi_t + \epsilon_{it}$$

$$\text{Weighted by } w_i = \begin{cases} 1 & \text{if } DDA\ Share_i > 0 \\ \frac{\widehat{P(DDA\ Share_i)}}{1-\widehat{P(DDA\ Share_i)}} & \text{if } DDA\ Share_i\ = 0 \end{cases} \tag{B1}$$

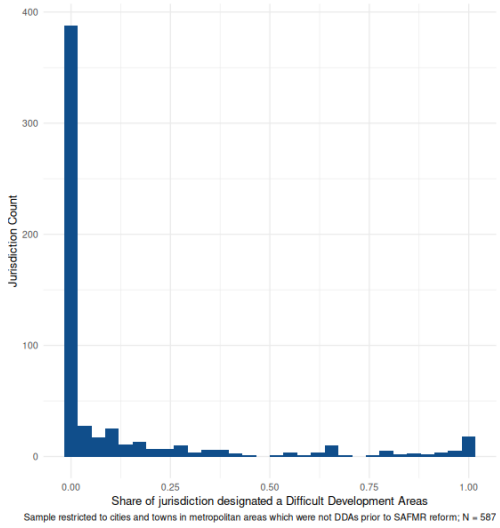
In contrast to the Opportunity Zone analysis, I rely on propensity weighting to get suitable controls, as there is not a natural control group to incorporate into a triple-difference. Because the policy was announced at the end of 2013, I exclude the 2014-2015 period from my main analysis, focusing on the change between the true pre-period (2011-2013) and the post reform period (2016-2021).

Localities which gain DDA zip code coverage see an increase in permit fees. See Table B4. Column (1) displays a model with a binarized outcome. Localities which receive at least some DDA designation see a 32% increase in permitting costs. With continuous treatment, I find that a 1 percentage point increase in DDA coverage of a locality generates a 0.43% increase in permit fees. Estimation of this treatment effect is not precise when state by year fixed effects are included in the model. When permit fees are measured relative to the assessed value of construction rather than the number of units, the estimated effect is of similar magnitude and precision. The event

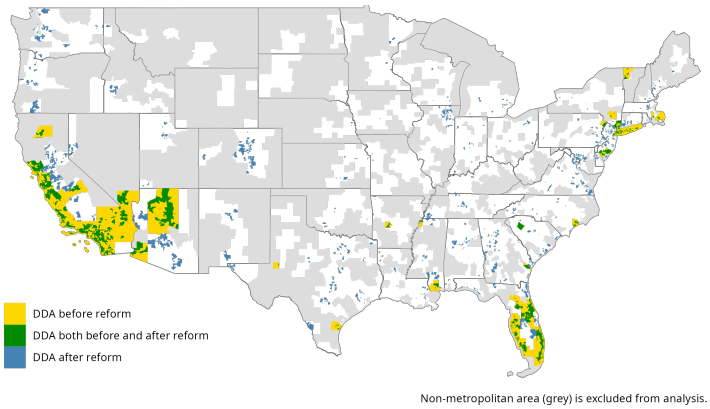
study, see Figure B2, shows a lack of pre-trends, but also an inconsistent treatment effect in the post period, oscillating between large positive effects and zero effects. While this secondary analysis suggests similar conclusions to the main OZ analysis, the certainty surrounding the results is far smaller, likely on account of the smaller number of localities involved in the reform.

In comparing the two cases studies here, it is important to note their differences. DDAs target high rent, high income areas. The localities which they reside in typically already have fairly high permitting costs, with a mean of $53,000 and a median of $10,000. By contrast, Opportunity Zones are meant to target distressed areas, and their pre-treatment permit fees were lower, with a mean of $40,000 and a median of $5,000. Thus, although in both cases a similar percentage increase in fees was detected, the level of cost increase is smaller in the opportunity zone case.

Figure B1: Distribution of DDAs

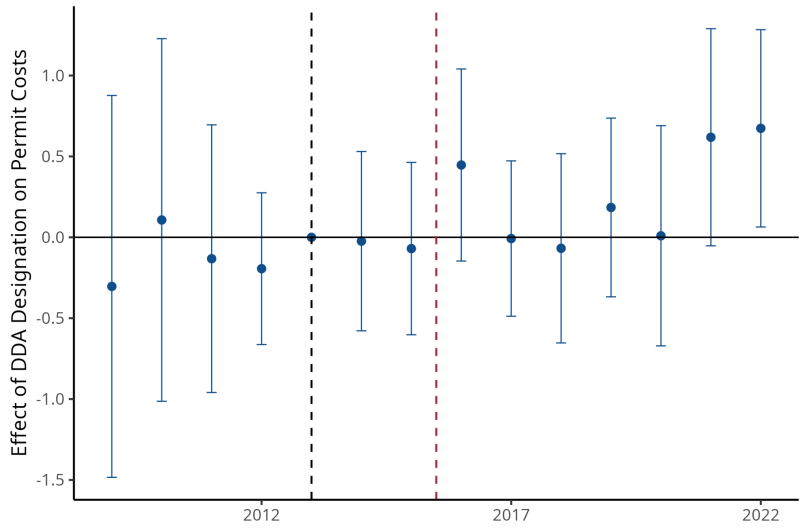


(a) Distribution of DDAs in Localities



(b) Map of DDAs Before and After SAFMR Reform

Figure B2: Event Study: Permit Prices at DDA SAFMR Reform



Blue Dashed line approximates announcement of policy; red dashed line shows policy implementation.

Table B1: Balance Between Localities with and without DDAs (pre-weighting)

Covariate	No DDA Mean	Some DDA Mean	Coef. with Binary DDA	Coef. with Cont. DDA
Levels 2009-2013				
Pop	33800	116000	3.53e-07 (1.5e-07) *	1.63e-08 (4.57e-08)
Pop per km^2	1010	1220	-1.01e-05 (2.71e-05)	4.13e-06 (1.49e-05)
Share Black	0.11	0.089	-0.109 (0.163)	0.0456 (0.0748)
Share Asian	0.064	0.105	0.453 (0.434)	0.347 (0.298)
Share Hispanic	0.102	0.167	0.925 (0.238) *	0.213 (0.133)
Share College	0.333	0.378	0.468 (0.216) *	0.151 (0.134)
Employment/pop	0.645	0.661	0.481 (0.388)	-0.697 (0.199) *
Unemployment rate	0.088	0.087	3.49 (1.06) *	1.82 (0.564) *
Poverty rate	0.133	0.114	-1.31 (0.482) *	-0.858 (0.313) *
Median HH Income	60500	71100	2.73e-06 (1.48e-06)	3.99e-06 (1.15e-06) *
Change from 2008-2012				
ΔPop	326	1590	1.07e-05 (1.05e-05)	4.07e-06 (3.77e-06)
ΔPop per km^2	7.88	16	0.001 (0.001)	0.001 (0.001)
ΔShare Black	0.001	0.001	1.93 (2.1)	0.324 (1.13)
ΔShare Asian	0.001	0	-0.761 (1.62)	1.06 (0.856)
ΔShare Hispanic	0.003	0.002	-2.26 (2.1)	0.906 (1.31)
ΔShare College	0.005	0.004	-0.443 (1.55)	1.44 (1.02)
ΔEmployment/pop	-0.004	-0.006	-2.77 (1.61)	-0.749 (1.01)
ΔUnemployment rate	0.003	0.003	-2.79 (2.61)	-0.619 (1.23)
ΔPoverty rate	0.004	0.004	0.421 (1.7)	-0.61 (0.812)
ΔMedian HH Income	-47.9	2.09	1.89e-07 (7.97e-06)	3.83e-07 (5.98e-06)
(Intercept)			-0.478 (0.325)	0.141 (0.164)
N	275	293	548	548
Adj. R^2			0.175	0.220

Sample restricted to localities in metropolitan areas which did not have difficult development area status pre-reform and with sufficient permit price information to be used in main analysis. Columns 1 and 2 show unconditional means of various measures in localities with with no Difficult Development Areas and at least some Difficult Development Areas respectively. Column 3 shows coefficients from a linear model in which the outcome is a binary indicator as to whether a locality receives any Difficult Development Areas. Column 4 shows coefficients from a model in which the share of a locality covered in Difficult Development Areas is the outcome variable. Stars indicate significance at the 5% level. Number of No DDA localities and Some DDA localities do not sum to number utilized in models in columns 3 and 4 due to missing values in covariates.

Table B2: Estimating Propensity for DDA Treatment; Logit Regression

	$\mathbb{1}(\Delta$ DDA Coverage $> 0)$
Mean SAFMR/Income-Limit of ZCTAs in Locality	57.866***
	(17.337)
Standard Deviation of SAFMR/Income-Limit of ZCTAs in Locality	1,554.260***
	(283.262)
Mean SAFMR/Income-Limit of ZCTAs in Locality ×	−42,484.060***
Standard Deviation of SAFMR/Income-Limit of ZCTAs in Locality	(8,699.125)
Constant	−2.206***
	(0.497)
Observations	592
Log Likelihood	−380.660
Akaike Inf. Crit.	769.320
Note:	*p<0.1; **p<0.05; ***p<0.01

Table B3: Balance Between Localities with and without DDAs with Inverse Propensity Weighting

	No DDA Mean	Some DDA Mean	Coef. with Binary DDA	Coeff with Cont. DDA
Levels 2009-2013				
Pop	32100	174000	3.51e-07 (1.45e-07) *	1.55e-08 (4.11e-08)
Pop per km^2	903	1180	-2.99e-06 (3.24e-05)	2.61e-06 (1.55e-05)
Share Black	0.11	0.095	-0.164 (0.175)	-0.025 (0.072)
Share Asian	0.062	0.112	0.928 (0.485)	0.375 (0.295)
Share Hispanic	0.1	0.175	0.652 (0.258) *	0.181 (0.129)
Share College	0.327	0.376	0.717 (0.239) *	0.159 (0.133)
Employment/pop	0.644	0.658	0.433 (0.41)	-0.603 (0.21) *
Unemployment rate	0.089	0.089	4 (1.15) *	2.01 (0.549) *
Poverty rate	0.135	0.124	-1.4 (0.506) *	-0.752 (0.324) *
Median HH Income	60300	68000	2.8e-07 (1.84e-06)	3.32e-06 (1.42e-06) *
Change from 2008-2012				
ΔPop	325	2380	1.13e-05 (1.05e-05)	3.19e-06 (3.07e-06)
ΔPop per km^2	6.89	17.8	0.001 (0.001)	0.001 (0.000)
ΔShare Black	0	0	2.23 (2.61)	0.141 (1.25)
ΔShare Asian	0.001	0	-0.066 (1.84)	1.12 (0.886)
ΔShare Hispanic	0.003	0.002	-2.44 (2.39)	-0.679 (1.26)
ΔShare College	0.005	0.003	-1.11 (1.72)	1.57 (0.977)
ΔEmployment/pop	-0.005	-0.006	-2.29 (1.63)	-0.812 (0.872)
ΔUnemployment rate	0.002	0.004	-2.81 (2.72)	-1.11 (1.24)
ΔPoverty rate	0.004	0.005	0.444 (1.64)	-1.02 (0.783)
ΔMedian HH Income	103	0.285	-9.54e-08 (8.09e-06)	-2.44e-06 (6.43e-06)
(Intercept)			-0.465 (0.336)	0.099 (0.165)
N	275	293	462	462
Adj. R^2			0.205	0.189

Same as table two, but with means and regressions weighted with inverse propensity scores. See notes for table 2.

Table B4: Permit Fee Changes in Response to DDA SAFMR Reform

Dependent Variables:	log(Permit Cost per Unit)			log(Permit Cost per $)
Model:	(1)	(2)	(3)	(4)
$\mathbb{1}$(Share in DDA $> 0\%$) $\times$ post	0.3193**			
	(0.1519)			
Share in DDA $\times$ post		0.4334**	0.3465	0.3473*
		(0.1989)	(0.2276)	(0.1879)
Year	Yes	Yes	Yes	Yes
Localities	Yes	Yes	Yes	Yes
State $\times$ Year			Yes	Yes
Observations	2,933	2,933	2,933	2,933
Localities	491	491	491	491

Locality clustered standard errors in parentheses.
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

C Descriptive Results with State Policy Indicies

This section expands upon the descriptive model displayed in column (1) of Figure 3(b), which is repeated in column (2) of Table C1. Column (1) displays a simpler model which regresses log permi cost per unit on locality types, log population, and regional indicators without conditioning on the type of housing being constructed. The pattern of higher fees in the major suburbs, and the ranking of regions by fees remains the same. In column (3), regional indicators are dropped in favor of state fixed effects. Once state fixed effects are added, urbanity no longer predicts higher permit fees. State fixed effects explain variation in permit fees substantially better than other models with regional/city-structure indicators. This suggests room for state policy in determining permit fees.

In column (4) an Impact Fee Regulation Stringency Index is incorporated with other locality characteristics into a descriptive regression. In most states, the authority to collect permit fees, particularly impact fees, is guided by Impact Fee Enabling Acts. To various degrees, these acts put boundaries on localities' fee exacting ability. I construct an index summarizing states' laws regarding localities' powers (Mullen, 2017b). The index is a unweighted average of four sub-indexes. The credit index (1) is the average of several dummies covering state requirements that localities credit developers for expected local revenue increases or construction. The breadth index (2) is an average of indicators for whether or not a specific public service (e.g., road infrastructure, schools) may be funded with impact fees. The analysis index (3) is an average of indicators for requirements that cities must provide reports, projections, or updates on the use of income generated from fees. The Ceiling Index (4) is an average of indicators for whether exactions can be used to increase service quality, and whether developers can claw back unused permit revenue. One might expect that stricter limits on localities' fee collecting power might result in lower permit fees, but my descriptive model finds no correlation between state policy stringency and permit cost magnitude.

High permit fees, particularly high impact fees, emerged shortly after many states' capped their localities' ability to collect property tax with Tax and Expenditure Limits (TELs). In Table C1 model (5) I use a index constructed by (Wen et al., 2020) to assess the relationship between state TEL stringency and permit fees. One might expect that stricter limits on localities' property tax collection result in higher permitting fees to compensate, but I find no correlation between TEL stringency and permit fees.

Table C1: Descriptive Regressions

Dependent Variable:	Log Permit Cost per Unit				
Model:	(1)	(2)	(3)	(4)	(5)
Central City Indicator	-0.5208***	-0.3695***	-0.3464***	-0.5827***	-0.5847***
	(0.0750)	(0.0745)	(0.0658)	(0.1609)	(0.1609)
Urban Indicator	0.3689***	0.2100***	0.0439	0.4461***	0.4390***
	(0.0462)	(0.0449)	(0.0416)	(0.1062)	(0.1064)
Township Indicator	-0.7213***	-0.9224***	-0.8531***	0.0120	0.0869
	(0.0687)	(0.0658)	(0.0662)	(0.2390)	(0.2408)
Log Population	0.3055***	0.3791***	0.3192***	0.2882***	0.2693***
	(0.0168)	(0.0167)	(0.0161)	(0.0504)	(0.0502)
Log Avg. Assessed Unit Value		0.4167***	0.3678***		
		(0.0337)	(0.0304)		
Share of Units Multifamily		-0.0120***	-0.0119***		
		(0.0007)	(0.0006)		
Impact Fee Regulation Stringency Index z-Score				0.0022	
				(0.0851)	
Property Tax Regulation Stringency Index z-score					0.1492
					(0.0936)
Northeast Region	0.7642***	0.8506***			
	(0.0686)	(0.0652)			
Southern Region	-0.6216***	-0.6056***			
	(0.0586)	(0.0590)			
Western Region	-0.1664***	-0.2328***			
	(0.0637)	(0.0608)			
Midwest Region	–	–			
Year FE	Yes	Yes	Yes	Yes	Yes
State FE			Yes		
Clustering	Locality	Locality	Locality	State	State
Observations	61,323	61,315	61,315	61,323	61,309
R^2	0.13115	0.19627	0.33449	0.09207	0.09962

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*