The (Non)-effect of Opportunity Zones on Housing Prices

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Motivation: People-based policies or place-based polices?

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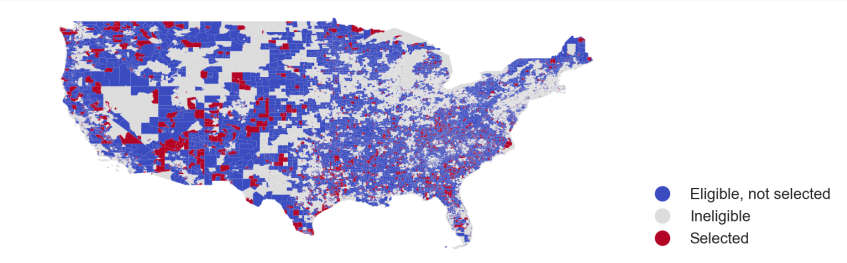
- **Does the Opportunity Zones program do these things?**

Institutional background



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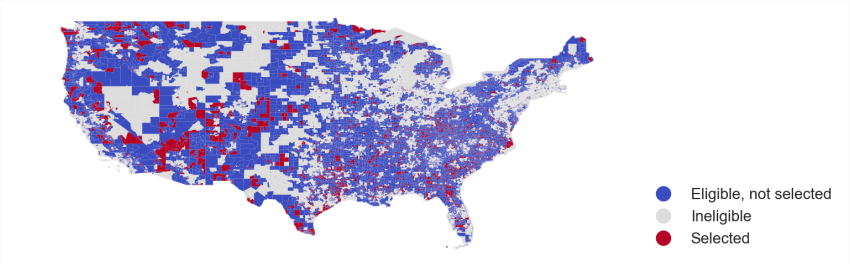

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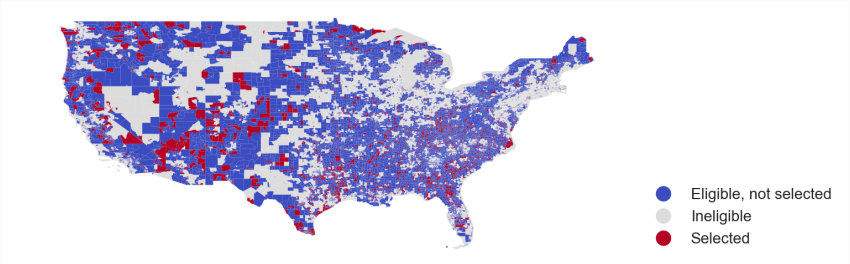

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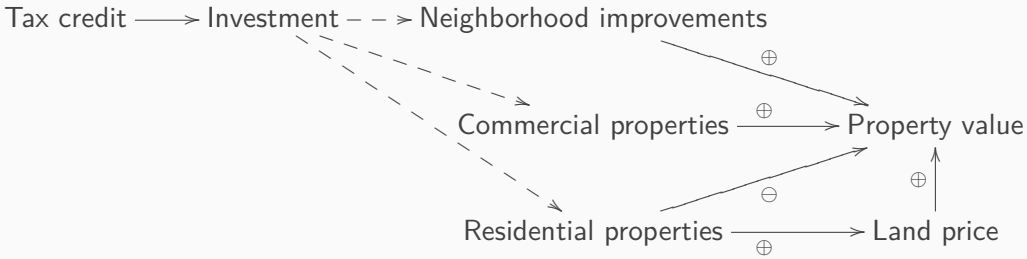
  - To end of 2019, $75B private capital investment (CEA)

Mechanisms of OZ effect on residential housing prices



(Strengths of dashed arrows depend on respective elasticities: e.g. perfectly inelastic residential supply would turn off downward price movements due to increased residential supply.)

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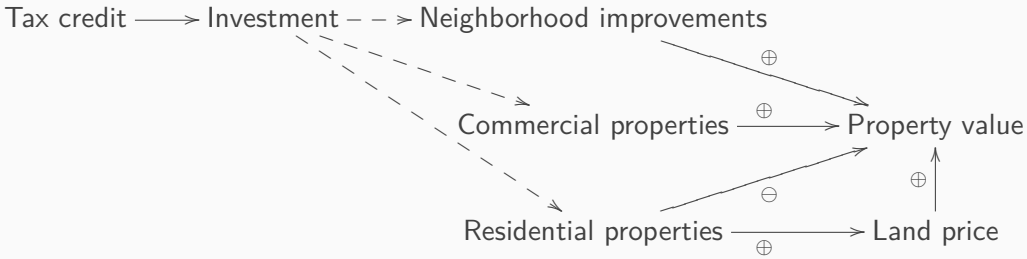

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  Estimates indicate a 0.5 to 1.4pp difference in effects with standard errors of 0.5
- Non-academic work conducted by Zillow and ATTOM Data Solutions using proprietary data
- Both conclude home price increases in OZs, but inferences are dubious since
  - Non-repeated sales price data fail to control for quality
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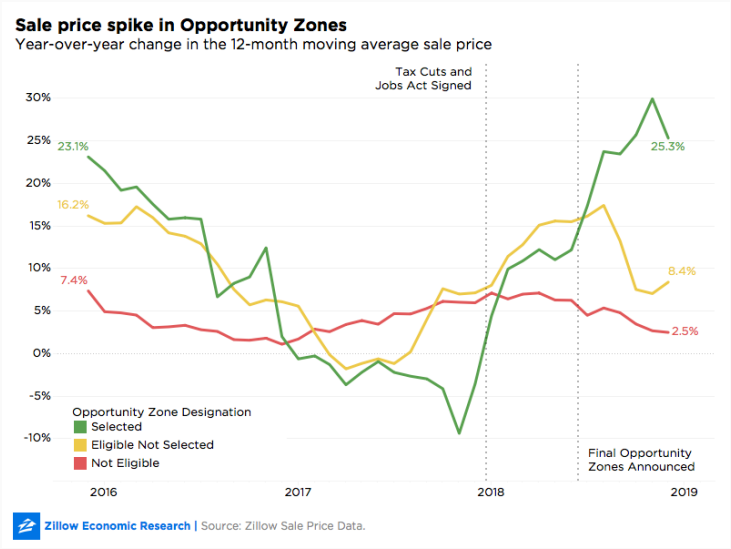
- CEA report replicates our initial work with only 2018 data. Their estimate of +0.5pp (0.2pp) with the 2019 data is in the range of our estimates, though on the higher end


Sale price spike in Opportunity Zones
Year-over-year change in the 12-month moving average sale price
Tax Cuts and Jobs Act Signed
30%
25%
20%
15%
10%
5%
0%
-5%
-10%
23.1%
16.2%
7.4%
25.3%
8.4%
2.5%
Opportunity Zone Designation
Selected
Eligible Not Selected
Not Eligible
Final Opportunity Zones Announced
2016
2017
2018
2019
Zillow Economic Research | Source: Zillow Sale Price Data.

Data

- Outcome variable: Growth in FHFA housing price index (weighted, repeat-sales index of single-family house prices): $Y_{it} \equiv P_{it}/P_{i,t-1}$
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  - Include demographics, economic, and housing related covariates
  - Full list: log median household income, total housing units, percent white, percent with post-secondary education, percent rental units, percent covered by health insurance among native-born individuals, percent below poverty line, percent receiving supplemental income, and percent employed

Methodology

Standard differences in differences setup:

- Let i denote the unit of analysis and let $t = 1, \ldots, t_0, \ldots, T$
- Potential outcomes $Y_{it}(1)$, $Y_{it}(0)$
- Treatment $D_i \in \{0, 1\}$ for being selected as OZ and $D_{it} = 1(t > t_0)D_i$
- Observe outcome $Y_{it} = Y_{it}(D_{it})$
- Assume that $Z_i \equiv (Y_i, D_i, X_i)$ are i.i.d. over units.

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Estimand of interest is the ATT averaged over post periods:

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Identification assumption: (conditional-on-X) parallel trends

For all x, $\mathbb{E}[Y_{it(0)} - Y_{i,t-1}(0) \mid X_i = x, D_i = 1] = \mathbb{E}[Y_{it(0)} - Y_{i,t-1}(0) \mid X_i = x, D_i = 0]$

Tract-level estimation

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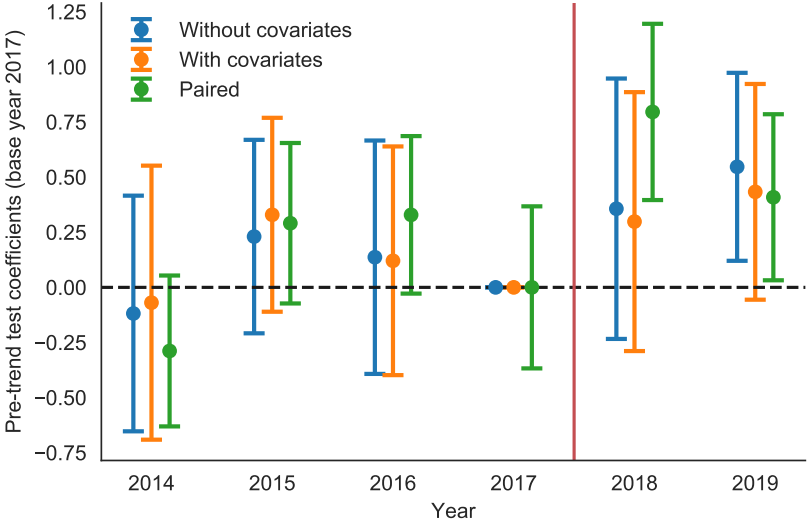
- Strategy 3: Match each treated unit to its nearest untreated geographical neighbor.
  - Without trend adjustment: 0.65 [0.17, 1.1]
  - adjusting for a linear trend: 0.5 [-0.1, 1.1]


Without covariates
With covariates
Paired
Pre-trend test coefficients (base year 2017)
1.25
1.00
0.75
0.50
0.25
0.00
−0.25
−0.50
−0.75
2014
2015
2016
2017
2018
2019
Year

ZIP-level estimation

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- Define the ZIP level OZ exposure as

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- Treatment is now continuous, estimation and interpretation is somewhat more delicate
- Interpretation of the coefficient is now "treatment effect of the OZ designation if the entire ZIP is included in an OZ vs. none of it is included in an OZ."

ZIP-level estimation

- Strategy 1:
  - 0.95 [0.5,1.4] without covariates (rejects pretest)
  - 0.12 [-0.4, 0.6] with covariates
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- The time horizon of the effects should be second half of 2018 and all of 2019
- There seems to be little possibility that home buyers anticipated that inclusion in an OZ would have a dramatic impact on the character of the neighborhood
- This fact does not imply that the OZ program was a mistake, but rather that it is anticipated to have little effect on the neighborhood.

Heterogeneity

- Hypothesis 2: TE for residential areas $<$ TE for commercial areas
- Split on the median of $\frac{\text{employed population in } z}{\text{residential population in } z}$

	No Covariates (1)	Few Covariates (2)	All Covariates (3)
Treatment $\times$ Post	1.680 [1.070, 2.290]	0.066 [−0.571, 0.702]	0.332 [−0.297, 0.961]
	(0.311)	(0.325)	(0.321)
Treatment $\times$ Post $\times$ Residential	−1.391 [−2.340, −0.442]	−0.887 [−1.838, 0.065]	−0.584 [−1.526, 0.357]
	(0.484)	(0.486)	(0.480)
Pretest p-value	0.009	0.439	0.948

- Sign consistent with the hypothesis, but effect size not large enough to be dispositive

Conclusion

- Point estimates for treatment effects are generally positive, but small in magnitude.
- Estimates are insufficiently precise to rule out effects of zero, but sufficiently precise to rule out large positive effects ($>$1pp)
- Point estimates for commercial areas are indeed larger than those for residential areas, but the difference is not large enough to reject zero

References i

Callaway, Brantly and Pedro HC Sant'Anna. 2018. "Difference-in-differences with multiple time periods and an application on the minimum wage and employment." arXiv preprint arXiv:1803.09015 .

Sant'Anna, Pedro HC and Jun B Zhao. 2018. "Doubly Robust Difference-in-Differences Estimators." Available at SSRN 3293315 .