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

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

- Should the government help poor people or poor places?
  - “Creating moves to opportunity” vs. subsidizing investment to low-opportunity areas

Arguments against place-based policies:
- Help landowners, might hurt renters
- Distort migration decisions

There are economic arguments for place-based policies as well:
- Internalize local externalities
- Targets aid with less distortion

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Institutional background

- Opportunity Zones (OZ)
  - Governors nominate 25% of eligible tracts
  - Provide deferral, reduction, or elimination of capital gains taxes for investment

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Mechanisms of OZ effect on residential housing prices

Tax credit $\rightarrow$ Investment $\rightarrow$ Neighborhood improvements

Commercial properties $\rightarrow$ Property value

Residential properties $\rightarrow$ Land price

(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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We produce a range of treatment effects estimates which, mostly, do not reject zero and rule out effects of >1.5pp per annum on price growth.

Hypothesis 2: OZ tax credits increase property value more so in commercial areas. 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
- Imperfect control group / parallel trends violations in the pre-periods

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

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

- Strategy 1: Assume eligible, but not selected tracts are sufficiently similar to selected tracts for parallel trends to hold

- Two-way FE estimation, s.e. clustered at state-level

- Without covariates: 0.39 [0.08, 0.7], pretest rejected [CEA estimates 0.53 (0.2)]

- Strategy 2: Strategy 1, but controlling for covariates flexibly
  - Take a propensity-score weighting ([Callaway and Sant’Anna, 2018]) or a doubly-robust estimation method ([Sant’Anna and Zhao, 2018])
  - Valid and efficient when assuming conditional-on-$X$ parallel trends

- Both strategies yield estimates $\approx 0.3$ [-0.1, 0.8]

- Strategy 3: Match each treated unit to its nearest untreated geographical neighbor.
  - Without trend adjustment: 0.65 [0.17, 1.1]
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ZIP-level estimation

- Only about half of selected tracts covered by the FHFA tract level data.
- Define the ZIP level OZ exposure as

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  \[ D_z = \sum_i \left( \text{Proportion of } z \text{ addresses in tract } i \right) \cdot D_i \]
- 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

- Strategy 2:
  - 0.1 [-0.5, 0.7]

All results rule out >1.5pp effects (95%); most specifications rule out 0.9pp effects. The treatment effect is small if at all positive.

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.
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### Heterogeneity

- **Hypothesis 2:** TE for residential areas < TE for commercial areas

- Split on the median of \( \frac{\text{employed population in } z}{\text{residential population in } z} \)

<table>
<thead>
<tr>
<th></th>
<th>No Covariates (1)</th>
<th>Few Covariates (2)</th>
<th>All Covariates (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment × Post</td>
<td>1.680 [1.070, 2.290]</td>
<td>0.066 [−0.571, 0.702]</td>
<td>0.332 [−0.297, 0.961]</td>
</tr>
<tr>
<td></td>
<td>(0.311)</td>
<td>(0.325)</td>
<td>(0.321)</td>
</tr>
<tr>
<td>Treatment × Post × Residential</td>
<td>−1.391 [−2.340, −0.442]</td>
<td>−0.887 [−1.838, 0.065]</td>
<td>−0.584 [−1.526, 0.357]</td>
</tr>
<tr>
<td></td>
<td>(0.484)</td>
<td>(0.486)</td>
<td>(0.480)</td>
</tr>
<tr>
<td>Pretest p-value</td>
<td>0.009</td>
<td>0.439</td>
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</tr>
</tbody>
</table>

- 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.