ECONOMIC INNOVATION GROUP

Are Opportunity Zones Working? What the Literature Tells Us

ANALYSIS, POLICY | OCTOBER 12, 2023

Download the Research Brief

by Kenan Fikri and Benjamin Glasner, PhD

DOWNLOAD

Introduction

With the passage of the *Tax Cuts and Jobs Act* in 2017, Opportunity Zones (OZs) became the most consequential place-based policy initiative in a generation. Designed with decentralization, flexibility, and scalability in mind, OZs were the first federal development program to use capital gains tax incentives as the mechanism to drive behavior.[1] This structure was one important departure among many from the more traditional tax credit model, which together help explain how OZs unlocked $48 billion worth of direct equity capital for investing in targeted low-income communities by the end of 2020 alone.[2]

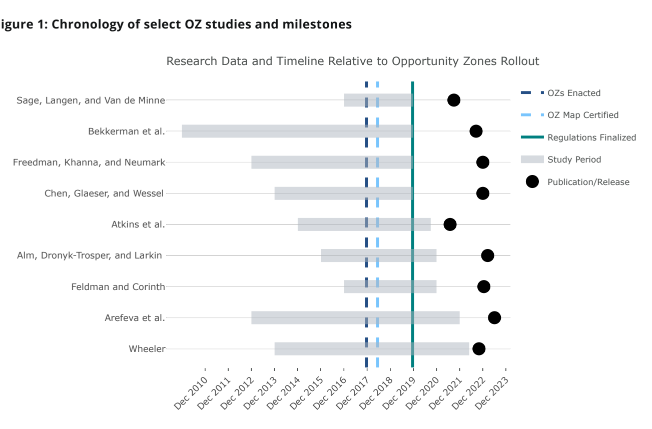
The novelty of the OZ mechanism has ushered in a new generation of research as scholars race to grade the policy. Historically, researchers have found little lasting economic impact from legacy "zone" programs such as Enterprise Zones, prompting many to ask whether OZs would be any different.[3] In the rush to declare success or failure, however, even prominent scholars have succumbed to various analytical pitfalls searching for effects of the policy in the wrong places at the wrong times. The structure of the incentive, pace of regulatory rollout, and nature of community development all caution for patience in allowing the impacts of the policy to register. In the meantime, the studies that most clearly incorporate the structure of the incentive and its most common use-cases into their design show strongly positive initial results that suggest that the OZ model has truly broken new ground. The following brief will critically evaluate the emerging body of academic work on the economic impacts of OZs to level-set on what can credibly be determined at this point in time and, we hope, constructively inform scholars' research agendas going forward.

Taking Stock of the Emerging Literature

Scholars have endeavored to evaluate the performance of OZs across a number of different factors since the policy was enacted. They have explored the relationship between OZ status and home prices,[4] commercial property sale prices and transaction volumes,[5] building permits activity,[6] establishment openings,[7] job openings,[8] employment rates, incomes, and poverty rates.[9] Effect estimates on the immediate and short-run impacts of OZs have ranged from positive[10] to null.[11] Appendix Table 1 compares and contrasts a number of different studies in depth.

Not all of these studies are created equal, however. Some have consequential shortcomings in their designs or methods. For example, most inquiries only examine a short window immediately following the policy's passage—an implausible time horizon for observing meaningful impacts. The analysis window in four of the nine most widely-referenced papers does not extend beyond 2019, meaning their study periods ended just as OZ regulations were finalized (see Figure 1).[12] Results from this pre-regulatory period cannot tell us anything about the direct short-term impact of OZ investment on targeted communities, not to mention anything of the long-term impact of a fully-implemented policy. As it happens, the two studies observing the strongest positive impacts (Arefeva, et al., 2023 and Wheeler, 2022) incorporate at least two full years of post-regulatory observations.

Figure 1: Chronology of select OZ studies and milestones



Timelines Are Critical to Evaluating OZs

All new policies take time to ramp up, but the structure of the OZ incentive makes timing especially crucial to internalize into study designs. Specifically, scholars must take into account key regulatory milestones, grasp how the regulatory timeline shaped the evolution of the market, and align with the on-the-ground realities of investment and development.

- **Regulatory timeline.** OZs were enacted in December 2017 as part of the *Tax Cuts and Jobs Act*, but states first had to identify and propose their OZ selections to the U.S. Treasury, and then those individual census tracts had to be certified by the Secretary. This process was not completed until June 2018. Following designation, regulations essential to investing in OZs were promulgated in three waves, starting in October 2018, then in April 2019, and finally in December 2019—the latter two packages being the largest and most substantive. Thus, 2020 marked the first year a fully-operational OZ investment ecosystem was up and running.
- **Market development timeline.** The regulatory timeline naturally shaped the economic and financial evolution of the market. The amount of equity capital raised by OZ funds grew rapidly as the regulations were finalized and has continued climbing since. The best available estimates report that OZ funds held a cumulative $48 billion in assets by the end of 2020, up from $4 billion in 2018 and $30 billion in 2019.[13] Private data sources suggest that holdings at least doubled again over the subsequent two years, charting a steep climb towards—and likely past—$100 billion in direct OZ equity by 2022.[14] It is important to remember that these figures describe the *supply* side of the OZ market, however, and that they will lead actual capital deployment significantly. In terms of communities, the best available information reports that 48 percent of OZs had already registered investment activity through the incentive by the end of 2020 (translating to approximately 3,800 individual communities, or census tracts), up from 26 percent in 2019.[15] In other words, the number of tracts registering investment was climbing steeply alongside investment dollars in the policy's early years.
- **Investment timeline.** The third critical consideration around timing has to do with the practical mechanics of investing and development. Investors have six months from realizing a capital gain to move the earnings into an OZ fund, and funds then have another six months to begin deploying that money into investments in communities. To date, OZ investments have primarily—although by no means exclusively—taken the form of real estate (often multi-family residential or mixed-use) and by nature (as codified in statute and regulation) must be new builds or substantial rehabilitations. The average multifamily construction project in the United States lasts 17.5 months from authorization to completion; even longer (19.2 months) for the larger buildings often associated with OZ investment.[16] Even before breaking ground, developers and investors have to conduct significant due diligence and project planning. Thus, two years can pass between when a place registers an OZ investment and when that investment becomes an economically active property. Researchers should anticipate these lags in their study designs.

What to Look For, and When

Ultimately, these timeline considerations mean that inquiries need to be grounded in clear theories of cause and effect, or what to expect and when. Timeframes are especially crucial given the upstream nature of an investment incentive, which only indirectly influences many economic indicators researchers care about—and does so at a considerable lag. Enterprise Zones provided direct employment tax credits, for example, the economic impacts of which can register immediately (the credit is only awarded when a person is in a job). OZs are fundamentally different.

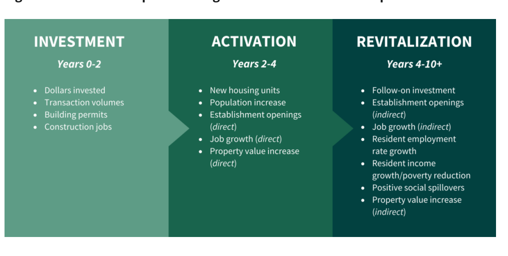
Exploring the impact of designation versus investment

Given the limited data available on OZ investments themselves, many studies focus solely on the impact of designation on outcomes of interest, finding little-to-no immediate or short-term effect, such as in Freedman, Khanna, and Neumark (2023) and Chen, Glaeser, and Wessel (2023). Designation is no guarantee of investment, however; as mentioned above, only around one-quarter of OZs registered any OZ investment activity in 2019, for example. Given that the economic circumstances of tracts did not change with designation, any observed impact stemming from it in the period immediately following designation would mostly be indicative of speculation—that market participants expected OZ designation itself to change the value of property in an area instantaneously. Null effects here may simply tell us that market participants knew too little about what OZ status would entail for an area to immediately change behavior.

As better data on investment itself becomes available, two particularly salient research questions come to the fore: First, what characteristics of tracts are associated with receiving OZ investment, and second, what impact does that investment have on outcomes of interest. Policymakers have a keen interest in knowing which types of places are most responsive to this particular type of investment incentive. This knowledge can help improve spatial targeting and align OZ designation with places where it is most likely to have a meaningful impact. Meanwhile, what effects receiving OZ investment has on a community remains a vitally important subject to explore as a gauge for the long-term effectiveness of the policy.

What do we mean practically by the "upstream nature of the incentive?" We mean that the more indirect the relationship between a capital incentive and an outcome, the longer it will likely take for an effect to be observed. For example, building real estate is the most common use-case for OZ equity, specifically constructing multifamily residential or mixed-use buildings.[17] The pathway from the construction of a new building to neighborhood-scale reduction in poverty is long, and the construction of a building itself has no direct impact on poverty rates. Instead, the poverty reduction effect sets in gradually as new investment spurs new economic activity and the condition of local economic distress—with all of its negative ramifications on lives and livelihoods—begins to fade. Along these lines, Figure 2 charts the likely impacts of a new residential development through three phases: investment itself, the economic activation of that investment (e.g., occupancy), and the longer-term neighborhood revitalization effects that stem from there.

Figure 2: Phases of impact flowing from a mixed-use development



For example, the impacts from a mixed-use multi-family residential construction project might flow like this: First an investor purchases land, likely a vacant parcel. Then a permit must be issued, followed by a lengthy construction period. Eventually, the residential and commercial units will go on the market, increasing the supply of housing and storefronts. As the housing units become occupied, the local population increases. Street-level retail fills in gradually, too, with direct establishment and job creation effects. With new residents and businesses, foot traffic and local spending increase. Property values begin to rise as demand for that location increases. New business opportunities crowd-in additional investment and, with that, more jobs. As the neighborhood improves, the tax intake rises, allowing for reinvestment. At the same time, a variety of positive spillovers and beneficial social impacts take root as the momentum in the community shifts from distress towards opportunity and more people find jobs, stability, and optimism. The incomes and prospects of long-term low-income residents grow, and poverty falls. The cycle of regeneration takes off.

The process of economic development is not always so linear, of course, but the logic behind that sequence should guide researchers and their readers as they think through what impacts to expect to observe from OZs at what point in time—and when the time will be right to evaluate OZs on their ability to deliver the kind of positive change outlined in the paragraph above. Given the design of OZs, resident-level effects on poverty or employment are likely to lag behind indicators of development activity significantly, and we are still years away from being able to credibly estimate them.

Navigating the Pitfalls of OZ Analysis

OZs present researchers with a number of challenges to navigate, from limited data availability to the non-random selection of census tracts and an unfamiliar incentive structure. With no publicly available information on which census tracts have received OZ investment, let alone how much, researchers are left searching for evidence of the policy's impact without knowing exactly where to look. The emerging body of work incorporates several creative approaches to tackling these challenges, but it also highlights several pitfalls that are worth studying for both producers and consumers of OZ research. Precision is crucial—precision in articulating *what* variables are being studied across which geographies (*where)*, *why* those variables are appropriate, and *how* they are expected to influence the outcomes of interest in the timeframe (*when*) under consideration. The merits of any inquiry may quickly come into question if any link in that logic chain is weak or missing. In the case of OZs, a consistent pattern is emerging in which studies with the most inexact specifications or the weakest theoretical linkages between cause and effect find null impacts while those with the tightest linkages and most exacting specifications find significant and positive ones.

The remainder of this section examines several recent studies to demonstrate how scholars have navigated pitfalls around three core elements of any research inquiry: **variable specification**, **model selection**, and **window of analysis**. The appendix table breaks down each study in detail for further discussion.

- **Pitfall 1: Variable specification**

  The very specific nature of what constitutes an OZ investment can complicate research designs, from the data collected to the econometric model used. All OZ investments must meet either "original use" or "substantial improvement" tests, which are intended to ensure that OZ investments are economically additive to a community. Investments must also be held for at least 10 years to qualify for the full range of tax benefits. OZ investment activity therefore only represents a fraction of the overall investment activity in a designated area. By definition, qualifying OZ investment cannot be purely speculative (i.e., "buy and hold"), as investors cannot simply purchase an asset in a community (an office building, a home, or a piece of land) and hold it to be eligible for any tax benefits. In the real estate context, that means that OZs are better understood as a supply-oriented development or redevelopment incentive than a generalized investment incentive.

  Indeed, misconstruing OZs as a generalized investment incentive open to all-comers for all transactions seems to be the biggest pitfall that scholars have fallen into, prompting them to search for market-level effects before what is in reality a much more bespoke ecosystem has had a chance to emerge. Another example: since investors must use the proceeds from the sale of an appreciated asset to fund their OZ investments and receive the tax benefits, the pool of qualifying investors is relatively small and excludes most retail investors—meaning large portions of the residential and commercial markets are not directly relevant to studying the near-term impacts of OZs.

  For researchers, these caveats mean that price or transaction volume data for commercial or residential real estate will be poor estimators for the near-term activity induced by OZs, since only a fraction of parcels or exchanges will be OZ-eligible. For example, one of the primary sources of data on real estate transactions used by OZ researchers thus far has been the Real Capital Analytics (RCA) commercial investment database.[18] Both Feldman and Corinth (2022) and Sage, et al., (2021) use this data to examine the impact of OZ designation on commercial property sale prices and volumes. Problematically, this dataset is composed mostly of "investment transactions," which RCA defines as traditional sales of buildings that are simply trading hands and decidedly *not* the sorts of transactions that are eligible to benefit from OZ tax incentives. Only about 7 percent of the RCA dataset is dedicated towards redevelopment or renovation[19]—meaning only a small fraction of the dataset includes observations relevant to an inquiry aiming to estimate any direct effects of OZ designation. And indeed, while Sage, et al., find a null effect of OZs on commercial property prices in aggregate, they do find a significant positive one on redevelopment properties.

  Chen, Glaeser, and Wessel (2020) explore the effect of OZs on single-family home price growth rates from 2014 to 2019. Their inquiry is based on designation itself, and they find little evidence that home price growth rates accelerated in the subset of designated communities for which repeat-sale information is available. This neutral impact in the year immediately following designation could reflect a lack of information and awareness; it could also suggest that sellers and buyers did not expect OZ status to lead to disproportionately faster home price growth in designated communities. Wheeler (2022), for his part, finds the null result to be an artifact of the authors having used price growth rates rather than levels or log levels as the dependent variable.[20] But most fundamentally, the near-term connection between the OZ tax incentive and single-family home prices is by nature tenuous. The structure of the incentive makes it much more directly relevant to new construction and substantial rehabilitations in the multi-family (often rental) market. Thus, Chen, et al's findings are best understood as signaling that OZs designation did not immediately trigger speculative activity in the residential real estate market, with nothing to say about the success or failure of the policy in raising property values over time.

  Finally, Atkins, et al. (2021), look at job postings data through March 2020 for early estimates of the new economic activity induced by OZs, finding a modestly positive impact in urban areas with large resident Black populations but no clear relationship nationally. However, data limitations force the analysis to be run at the zip code level, which is a higher unit of aggregation than the operative geography of OZs (census tracts) and may obscure more localized economic impacts. Job postings data itself has its own biases across industries and locations and does not always have a one-to-one relationship to jobs, making it a novel place to look for signs of OZ impact but not one that can provide definitive insights on the policy's immediate and short-run local economic impacts.

- **Pitfall 2: Model selection**

  The complexities inherent in OZ timelines make choosing the right model a challenge. Thus far, difference-in-differences (DID) has been the model of choice for most researchers. This method is particularly useful when a treatment and control group (e.g. designated tracts versus eligible but not designated tracts) can be clearly defined and outcomes can be observed both before and after treatment (e.g. before and after designation). DID is designed to estimate just how much the treatment changes the gap on outcomes of interest between the two groups.

  In settings where treatment is not random, researchers need to validate the "parallel trends assumption," ensuring that the treated and control groups were on similar paths before the event of interest, and that any initial difference between the two would likely have persisted had treatment not been introduced. If we believe the parallel trends assumption might not hold (and several scholars[21] have shown that it often does not for OZ tracts), then we cannot be sure that the study is observing the effect of treatment itself. A few solutions exist: one popular one is "matching methods," which are used to improve the quality of comparison units for each treated unit in the sample, building a "valid" control group from the bottom up. In a similar fashion, researchers can create a "synthetic" control group that closely matches the initial characteristics of the observed unit. These specifications matter because the selection of OZs was not random. Governors designated OZs from a predetermined pool of eligible high-poverty and/or low-income census tracts in their states, but from there each state applied qualitative filters to tailor their selections to their own local priorities and circumstances. Not only does this introduce non-random treatment into the sample, it means that there are unobserved characteristics that vary by state and often make selected OZs distinct from non-selected OZs.

  Even some workarounds have their pitfalls, however. Feldman and Corinth (2023) utilize a regression discontinuity (RD) model to examine the impact that OZ eligibility had on commercial investment. By nature, RD models zoom in on either side of a threshold—in this case, a multivariate measure of OZ eligibility cutoffs—to search for observable impacts on the outcomes of interest. However, the model is designed to detect whether OZ eligibility led to a commercial investment jump at the discontinuity—for example, in census tracts with a 20.1 percent poverty rate relative to those with a 19.9 percent poverty rate (i.e. on either side of the 20 percent eligibility cut-off)—and is less well-suited to detecting changes in the rest of the sample, including in the higher-poverty areas where the impact of the incentive may be less marginal/most meaningful. RD models also struggle to control for spillover effects across geographic units, which other studies (Arefeva, et al., 2023; Wheeler, 2022) show are significant for OZs.

  What is more, RD models rely on the comparability of units on either side of the discontinuity, and the fact that the poverty rate is not a continuous variable but rather an aggregate (reporting the share of the population below the poverty threshold, with no information on the depth or severity of poverty within the poor population) suggests it may not provide a reliable axis. For example, a high-income area with a large public housing project or student population may have a high poverty rate but differ significantly from a more lower- or mixed-income area in which the same fraction of the local population falls below the poverty line.

  Combined with an inherently noisy dataset (limited to commercial transactions over $2.5 million and in which most tracts had no observations at all) that is not particularly well-suited to studying OZs (see critique above), the inquiry produces estimates with extremely wide standard errors that encompass negative, neutral, and strongly positive possible outcomes. Even more fundamentally, the study asks whether eligibility itself changed investment trends in qualifying census tracts over the 2018 to 2020 period, even though by mid-2018, the question of eligibility had been resolved and the much smaller pool of actual OZs had been selected.[22] When the authors restrict the sample to census tracts that were selected as OZs, not just eligible, they again cannot rule out "economically significant effects." Similar critiques apply to Alm, et al., who deploy an RD model against real estate transaction price data in Florida and find "little consistent and robust evidence" of an impact of OZs on the measures in question amid very high standard errors. In both cases, the ambiguities likely stem directly from the choice of model and the fitness of the price- and transaction-related variables under scrutiny.

- **Pitfall 3: Window of analysis**

  A final set of studies underscores the importance of looking for the right thing, in the right places, at the right time. Freedman, Khanna, and Neumark (2023) study some of the most important long-term proof points for the OZ model, namely whether the incentive has an impact on employment, poverty, and incomes in targeted neighborhoods. Using American Community Survey microdata through 2019, the paper aims to explore effects at the resident level. The authors report null effects across the outcomes of interest. However, the outcomes of interest are long-run by nature. It is not plausible to expect poverty rates to fall simply and immediately because a place was designated as an OZ. Thus, the important benefits to residents the authors care about (right thing) are unlikely to appear in the analytic window (wrong time). It is a prime example of a quality study that should be re-run in the future but has no practical utility until the logic of cause and effect comes into line down the road.

  The work of Arefeva, et al., (2023) demonstrates the value of re-running analyses to corroborate and/or refine initial estimates as additional years of data become available. In their initial inquiry, the authors tested for the effect of OZs on business and job creation through 2019 using a DID model. The 2019 window was still early in the life of the policy, but the model was designed to detect direct initial effects of investment (the "investment and activation" phases from Figure 2), as opposed to more indirect revitalization effects (i.e. increases in resident employment rates as in Freedman, et al., (2023)). The authors found that OZs significantly increased the growth rate of employment and establishments at the tract level, with positive spillovers on neighboring tracts, too. They found the largest impacts in the construction industry, which aligns with the lifecycle of most OZ investment activity in the window they analyze. This plausible positive finding was corroborated in an update to the paper published in 2023 with results through the end of 2021. The revised estimates find moderately weaker establishment growth effects but moderately stronger job growth ones. These revisions confirm the directionality of the original study, which was one of the first to register positive effects on the expected indicators and in the expected places, and they also underscore that the effects of OZ will take time to register and will continue to evolve in communities over time.

  Finally, Wheeler (2022) advances a design that naturally reflects the timing and mechanisms of the incentive and clearly looks at the right indicator at the right time. These characteristics make it the most valuable study to date and lends its findings a high weight in the portfolio of accruing evidence. The study explores the effect of OZ designation on new residential and commercial development as measured by building permits across 47 large cities covering 12,000 neighborhoods from January 2014 through June 2022. Given the nature of the OZ incentive and how it is used most widely in the marketplace (the development of new or refurbished structures), building permits are one of the first places one might expect an impact of OZs to register. Wheeler finds that OZ designation significantly increased new development both in OZs and nearby areas within the sample of large cities, consistent with the positive spillover effects found by Arefeva, et al. (2023), too. The effects are largest among neighborhoods with more available land and in-fill opportunities, a more elastic housing supply, and lower home values—all of which would be expected given the structure and predominant use-cases of the incentive. In the end, he finds that designated urban communities experienced a 20 percent increase in the likelihood of seeing development activity in any given month, and that the policy has boosted home values while keeping rents in check thanks to new supply.

Conclusion

Economic development is a long-term process and OZs are still a young policy. At its best, the first wave of research published in the years immediately following

the policy's passage can only possibly yield estimates about the immediate and short-run effects of being designated an OZ. By nature (and due to current data limitations), the work can say little about the effect of actually receiving investment on a community, and it is completely unable to quantify any long-run impacts of the policy.

The research community is rightly impatient to determine whether OZs are having an impact on important economic indicators in targeted areas, including on the livelihoods of low-income residents. The scale of capital being raised underscores the compelling public policy interest in knowing the effectiveness of the model. The practical realities of the incentive and the lags inherent in procuring quality data counsel for patience, however.

At this stage, a few facts can be established. First, the incentive is unlocking more investment capital and reaching more low-income communities than predecessor programs did at similar stages. The best available evidence strongly suggests that the size, scale, and geographic diversity of OZ capital-raising is registering in *both* a large proportion of targeted communities *and* spilling over positively into neighboring ones. The structure of the incentive itself ensures that OZ investments are economically additive to a community. It is increasingly safe to assume that OZ effects will be detectable when given a chance to play out. The first generation of studies (e.g. Chen, et al., and Corinth and Feldman) demonstrated value in showing that OZs did not trigger speculative activity in targeted communities. The second generation (e.g. Arefeva, et al., and Wheeler) is beginning to confirm that direct OZ investment activity is substantial and widespread. The third generation, which cannot credibly begin for a few more years, will start to answer the important questions about the policy's long-term effects on neighborhoods. For now, a close look at the most comprehensive data *already* makes clear that OZs are breaking new ground and challenging us to reimagine what federal tax policy can achieve in chronically distressed parts of the country.

Appendix Table 1

Notes

1. **Hassett and Bernstein** (2015).[↩]
2. Coyne and Johnson (2023).[↩]
3. **Papke** (1993); **Neumark and Kolko** (2010); **Freedman** (2012), **Busse, Gregory, and Kline** (2013); **Neumark and Simpson** (2015); **Reynolds and Rohlin** (2015); **Freedman** (2015); **Neumark and Young** (2021).[↩]
4. **Chen, Glaeser, and Wessel** (2023).[↩]
5. **Bekkerman, et al.** (2022); **Feldman and Corinth** (2023).[↩]
6. **Wheeler** (2022).[↩]
7. **Arefeva, et al.** (2023).[↩]
8. **Atkins, et al.** (2021).[↩]
9. **Freedman**, **Khanna, and Neumark** (2023).[↩]
10. **Arefeva et al.** (2023); **Wheeler** (2022; **Bekkerman et al.** (2022); **Sage, Langen, and Van de Minne** (2021).[↩]
11. **Chen, Glaeser, and Wessel** (2023); **Freedman**, **Khanna, and Neumark** (2023); **Atkins, et al.** (2021); **Feldman and Corinth** (2022); **Alm, Dronyk-Trosper, and Larkin** (2023).[↩]
12. For the purposes of this analysis, we reference the original working paper by Chen, Glaeser, and Wessel (2019), which contained data up to the end of 2019. Subsequently, the published version released in the Journal of Urban Economics in 2023 extended data coverage until the end of 2020. We've maintained the original date range in this graphic because of the significant impact their initial paper had on public perceptions of OZs.[↩]
13. **Coyne and Johnson** (2023).[↩]
14. **Novogradac** (2023).[↩]
15. **Coyne and Johnson** (2023).[↩]
16. U.S. Census Bureau's **Survey of Construction** (2023).[↩]
17. **Coyne and Johnson** (2023); **Novogradac** (2023).[↩]
18. **Sage, Langen, and Van de Minne** (2021); **Feldman and Corinth** (2023).[↩]
19. As reported by **Feldman and Corinth** (2023).[↩]
20. With another year of data and using log levels of housing values in the American Community Survey instead of house price growth rates from the Federal Housing Finance Agency, **Wheeler** (2022) documents a 3.4 percent increase in median home values in a subset of urban OZs from 2017 to 2020.[↩]
21. **Chen, Glaeser, and Wessel** (2023); **Freedman**, **Khanna, and Neumark** (2023); **Eldar and Garber** (2023).[↩]
22. Over 40,000 census tracts were eligible to be selected as OZs: just over 8,700 were ultimately selected, including the OZs in U.S. territories.[↩]

OPPORTUNITY ZONES | COMMUNITY DEVELOPMENT | PUBLIC POLICY| SPATIAL INEQUALITY

Related Posts

Raising the SALT cap will not help low-income, rural, or pro-Trump places

February 18, 2025

ECONOMIC INNOVATION GROUP

CONTACT
info@eig.org
media@eig.org

CONNECT WITH US

SIGN UP TO RECEIVE THE LATEST UPDATES

Email address

SIGN UP