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Taxes and Economic Development: An Update on the State of the Economics Literature

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Abstract

There is a large literature examining the effect of taxes and tax concessions on local economic development, but the last comprehensive review of taxes and economic development was Wasylenko (1997), which mostly examined the location response of firms. After the last major review of the literature, empirical work in this area has sought to address endogeneity concerns that plagued previous studies, resulting in a series of compelling new studies. This paper reviews the empirical literature on tax-based economic development incentives produced since Wasylenko's 1997 review covering the following areas: property tax (including Tax Increment Financing and Business Improvement Districts), spatially targeted and zone-based tax concessions, firm specific incentives and corporate income taxes. The review focuses on academic studies that employ modern program evaluation or quasi-experimental techniques and primarily U.S. based policies.

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Taxes and Economic Development: An Update on the State of the Economics Literature

Introduction

Estimates suggest that the value of state and local economic development incentives in the United States is between \$45 and \$90 billion annually (Bartik 2017).¹ These incentives include a broad range of options— corporate income tax concessions, regulatory concessions, direct grants, targeted infrastructure improvements, employment tax credits, property tax abatements, and refundable tax credits, among others. Taxes and tax concessions play an outsized role in state and local economic development incentives compared to most other policies like customized job training.² There is a large literature examining the effect of taxes and tax concessions on local economic development, but no recent comprehensive review of this literature. The last comprehensive review of taxes and economic development was Wasylenko (1997),³ which mostly examined the location response of firms.⁴

In commentary on Wasylenko (1997), Bartik (1997) called for more studies that make use of natural experiments to study taxes and tax incentive policies, citing endogeneity as a primary concern with the studies available at that time. The primary difficulty in studying how taxes and tax incentives impact local economic development is that they are *a result* of local economic conditions, making variation in the policies directly correlated with outcomes of interest regardless of actual policy effects. Bartik’s critique has become a driving force in the empirical work on taxes and economic development as is lucidly described (in reference to the capitalization of property taxes) by Elinder and Persson (2017, 19):

Most of the earlier studies on property tax capitalization analyze local or regional cross-sectional variation in property tax rates. There are two fundamental identification problems with that approach, and these problems have been known and discussed since the seminal paper by Oates (1969). First, a higher tax rate implies higher tax revenues and consequently higher quality of public goods. Higher quality of public goods puts upward pressure on house prices, making it difficult to isolate the effect of the tax separately. Controlling for public goods quality has been the main concern so as to avoid biased estimates, but this task has proven difficult. Second, when local governments set their tax rate, areas with higher house prices, all else equal, are able to set a lower tax rate to collect a given amount of tax revenues. This creates a simultaneity bias between the property tax rate and house prices.

¹ These estimates are adjusted to 2015 dollars by Bartik (2017) and include estimates from Thomas (2011), Story (2012), and Bartik (2017).

² Bartik (2017) shows that tax concessions (job creation tax credits, property tax abatements, investment tax credits, R&D tax credits) make up 95 percent of incentive value-added, while customized job training is only 5 percent.

³ Also see Buss (2001) for a review of the state tax incentives literature at that time, and Peters and Fisher (2004) for a review of the review papers that existed on economic development incentives at that time.

⁴ Wasylenko’s review was part of a series of papers (and accompanying symposium) examining the economic development effects of several policy options including regulation (Tannenwald 1997), enterprise zones (Fisher and Peters 1997), and public services (Fisher 1997).

Following the general trend in empirical micro-econometric research occurring over the last few decades (Angrist and Pischke 2010), much of the literature on local economic development and taxes has focused on the use of natural experiments to address endogeneity concerns.⁵ The newer literature examining the economic development effects of taxes, especially the capitalization effects of property taxes, relies on examining policy changes that can be categorized as natural experiments to deal with estimation problems. Tax policy can be thought of as a natural experiment if there is a “transparent exogenous source of variation in the explanatory variable” Meyer (1995, 151). Natural experiments break the endogenous relationship between outcomes and policy and allow for unbiased estimation of the effects of tax policy on local economies.

Not all policy changes can be thought of as natural experiments, and within the natural experiment framework there are differing methodologies, each with different assumptions, strengths, and weaknesses.⁶ The most believable natural experiment approaches employ policy changes with two key elements: being unanticipated (or having features that are unanticipated) and varying across an otherwise homogenous population (allowing for treatment and comparison groups). Meyer (1995) makes the point that the movement toward using natural experiments has the benefit of pushing researchers to consider the source of variation in the policies they study, and how it may relate to other factors, improving the quality of overall research.

In the years since Bartik’s commentary, a vast literature on taxes and tax incentives has emerged, much of it designed to confront the problem of endogeneity using natural experiment approaches to evaluation. This paper reviews the modern literature on the effect of tax policy on state and local economic development.⁷ It is rare in the literature to find studies of taxes that examine an outcome not directly related to the type of tax studied—property tax studies tend to examine property values; corporate income tax studies tend to examine corporate investment. This review covers tax-based policies in the following areas: property tax (including Tax Increment Financing and Business Improvement Districts), Spatially Targeted and Zone-Based tax concessions, Firm Specific Incentives and Corporate Income taxes.⁸ The review will cover a host of economic outcomes studied in the literature including business location, employment, income/poverty, and property values. The review focuses on academic studies that employ modern program evaluation or quasi-experimental techniques and examines U.S. based policy,⁹ essentially the work that has answered Bartik’s 1997 call to take endogeneity seriously.

⁵ There are a handful of notable research papers that use a natural experiment approach prior to the time window that this review covers. These studies include Gabriel (1981), Rosen (1982), and Yinger, Bloom, Borsch-Supan and Ladd (1988); see Guilfoyle (2000) for a summary of early studies using a natural experiment approach.

⁶ See Hanson and Rohlin (2018) for a review of natural experiment methods used to study redevelopment incentives.

⁷ I exclude studies that primarily examine direct spending, infrastructure improvement, training programs, extended public services, and regulatory concessions. Tax incentives, especially firm specific and zone-based programs, are often part of a larger package of incentives that includes these other incentives. For examples of excellent studies that examine non-tax economic redevelopment incentives and programs see Holmes (1998), Romero (2009), Patrick (2014), and Kline and Moretti (2014).

⁸ Notably, I do not cover the economic effects of individual income taxes. There is a vast literature on the effects of individual income taxes, and while this literature does investigate some of the traditional economic development outcomes, it primarily focuses on labor supply and mobility.

⁹ In addition to the academic work discussed here, there is a growing number of state sponsored evaluations of specific tax incentives. A 2017 report by the Pew Charitable Trust (Pew, 2017) catalogs and summarizes what each state (and the District of Columbia) is doing to evaluate tax incentives, providing a rating relative to other states. The Pew report offers examples of evaluations and works as a “best practice” guide for evaluators. Notably, while most

Property Taxes and Incentives

The economic development effects of property taxes, or of various forms of property tax relief, are typically measured in the form of capitalization into property values, although some newer studies also examine mobility and construction.¹⁰ A vast pre-2007 literature exists on the capitalization of property taxes and property tax relief beginning with Oates (1969), and aptly summarized by Yinger, Bloom, Borsch-Supan, and Ladd (1988), Guilfoyle (2000), and Sirmans, Gatzlaff, and Macpherson (2008). These reviews conclude that the empirical literature broadly suggests incomplete capitalization of property taxes into property values, although some studies suggest full capitalization.

The endogeneity critique outlined in the introduction of this paper has played a central role in the literature on the economic development (and capitalization) effects of the property taxes. Most of the earlier literature tried to solve this issue by estimating Two-Stage-Least-Squares (2SLS) models, where the first stage explains differences in property tax rates, and the second uses the predicted values of property tax rates to explain property values (or another economic outcome of interest). A well-identified 2SLS model would require that there is a factor driving changes in the property tax rate that is orthogonal to house prices (or any outcome of interest), but there is not a strong case for this important criterion in much of the pre-2000 literature. As discussed in the introduction, this review focuses on the studies that have best dealt with endogeneity concerns.

Post-1997 U.S. Based Studies

The modern literature on the economic effects of property taxes, summarized in chronological order in Table 1, essentially starts with Palmon and Smith (1998), who study capitalization across municipal utility districts in the suburbs of Houston, TX. Palmon and Smith (1998) rely on variation in tax rates within an identical public service district. Tax rate variation comes from differences in when bonds were issued to finance infrastructure, timing of subdivision completion (driving the number of homes per subdivision), presence of commercial property, and size of the district. This study relies on how property tax rates vary among a cross section of 501 homes sold in 1989 and finds that between 56 and 64 percent of tax differences are capitalized into home prices.

There are two reasons the Palmon and Smith (1998) study begins this review of the literature. First, its publication date places it immediately after the most recent comprehensive literature review. Second, and more importantly, it lays out a case for why the variation in property taxes in the study may be separate from the variation in public services, offering the ability to identify a pure tax capitalization effect. While there are reasons to find the Palmon and Smith (1998) study convincing, there is also good reason that the literature on the economic development effects of property taxes did not end there. First, there are likely to be some differences in public

academic work focuses on the effect of a policy on a particular outcome (such as employment), the state evaluations are meant to be wholesale cost-benefit evaluations of a policy.

¹⁰ See Bruckner 1979, Yinger (1982), and Zodrow and Mieszkowski 1986 for development of the theoretical basis for property tax capitalization and Zodrow (2001) for a summary of the various theoretical views and how they relate.

services across the study area of the Houston suburbs, and moreover Palmon and Smith (1998) do not make much of a case to show that public services are uniform. Second, the sources of variation in property taxes used in the study could easily be correlated directly with house price or correlated with other factors that change house prices. Finally, the study is of a small area within one city in the U.S., and the results may not be externally valid. Perhaps because of these reasons, this literature has continued throughout most of the last 20 years.

The 2000s brought a continued focus on sources of exogenous variation from natural experiments to study the economic effects of property taxes. Many of these modern studies focused on major reforms to state policy to limit property tax payments—Massachusetts’s Proposition 2 ½, California’s Proposition 13, and Michigan’s Proposal A. Other studies employ emerging tools from the program evaluation literature, such as difference-in-difference and border discontinuity methods.

Some of the first papers in the modern literature examined Massachusetts Proposition 2 ½, a measure that reduced effective property tax rates to 2.5 percent and limited the future growth in property tax revenues to 2.5 percent.¹¹ Bradbury, Mayer, and Case (2001) present evidence that suggests Proposition 2 ½ was binding for many municipalities in Massachusetts—they point out that over half of municipalities had effective tax rates higher than 2.5 at the time the limit was voted into law. The fact that Proposition 2 ½ was implemented statewide in Massachusetts (making it exogenous to any individual municipality) and was binding for only a subset of municipalities (creating comparison and treatment areas) make it a viable natural experiment to study the economic effects of property taxes.

Bradbury, Mayer, and Case (2001) and Lang and Jian (2004) both use Massachusetts Proposition 2 ½ to study the effects of property taxes on property values. These papers use different windows of data to study these effects but are otherwise quite similar. Both papers show increases in property values in municipalities that were able to increase tax revenues (expenditures). In the context of the Proposition 2 ½ reform, this indicates that the law was holding the level of public goods/services (and corresponding tax collections) below what residents of municipalities preferred. The Proposition 2 ½ studies represent an earlier use of a natural experiment, but do not fully separate the effects of property taxes from local spending, so therefore do not offer a pure estimate of tax capitalization.

While most of the studies on the economic effect of property taxation examine capitalization, there are several excellent studies that examine the mobility effects of property taxes. Chronologically, most of the mobility studies were published after the Proposition 2 ½ studies, and before a series of excellent studies that also examine property value effects. The modern literature on how property taxes affect mobility begins with Wasi and White (2005), who use California’s Proposition 13 as a natural experiment combined with a difference-in-difference estimation strategy. Proposition 13, voted into law in 1978, mandates a property tax rate of 1 percent plus the cost of paying interest on locally issued bonds. In addition to mandating the property tax rate, Proposition 13 also mandates that properties be assessed at their market value at the time of purchase and that assessment growth cannot exceed 2 percent per year.

¹¹ The limit can be raised in a municipality for new growth, but even so the effective rate cannot exceed 2.5 percent of total assessed value.

Proposition 13 dramatically increased property taxes for new owners of a home, as moving triggers a reassessment of property values on the purchased property. Wasi and White (2005) find that Proposition 13 caused the average tenure for home owners in California to increase by 6 percent, with stronger negative effects on in-migrants to California. Wasi and White (2005) also find that the implicit subsidy for homeowners to stay in their home varied across markets, and that the size of the subsidy was further correlated with declining mobility. Wasi and White (2005) highlight that in areas with an average subsidy of \$250 average tenure increased by less than one year, but in areas with an average subsidy \$1700–\$2600 tenure increased by 2–3 years.

Ferreira (2010) uses amendments to Proposition 13 to further study how property taxes affect mobility. These amendments allow homeowners age 55 or older to transfer the value of their property to a new property, while the provision does not apply to younger homeowners.¹² Using a regression discontinuity design, Ferreira (2010) documents a significant jump in the probability that a California homeowner moves after age 55 relative to younger homeowners. The property tax benefit associated with being able to transfer value (thus reducing property tax burden) at age 55 results in a 25 percent higher mobility rate among 55-year-old homeowners compared to 54-year-old homeowners.

The California property tax preferences for older homeowners are not unique. Many states offer property tax relief programs that are based on age or other characteristics like income. Shan (2010) documents the many state and local property tax preferences that depend on a residents age (as well as income and home value among other characteristics) and studies how these policies affect mobility among the elderly, using a simulated instrumental variables strategy.¹³ Shan (2010) shows that a \$100 increase in annual property taxes is associated with an 8 percent increase (0.73 percentage points on a base of 9 percent) in the mobility rate for homeowners over age 50.

In addition to the statewide property tax reforms in Massachusetts and California, Michigan underwent a major reform in the mid-1990s with Proposal A. The property tax reform in Michigan has also been the subject of several studies that use it in a natural experiment framework to determine the economic effects of the property tax. Proposal A, implemented as a school finance reform, imposed a 5 percent limit (or the inflation rate if it is lower) on the growth of taxable property value and a maximum statutory mill rate for a principal residence.¹⁴ However, values of sold properties are assessed at the acquisition value, creating a disincentive for mobility.

Skidmore, Reese, and Kang (2012) studied how the property tax limits imposed by Proposal A affected the property tax base in the communities of southeast Michigan using the exogenous change in tax rates from the state-wide reform as an instrument for local tax rates. Skidmore,

¹² The original amendment, Proposition 60, only allowed this value transfer for within-county movers. Subsequently, Proposition 90 allowed the value transfer for some cross-county movers for a set of participating counties (Ferreira, 2010).

¹³ Shan (2010) simulates the generosity of property tax relief programs for a state, year, and homeowner age using a national sample of homeowners to avoid endogenous individual homeowner characteristics causing bias in estimation.

¹⁴ The maximum mill rate was not imposed on secondary homes, for example vacation homes or cottages.

Reese, and Kang (2012) find that a community's tax base, as measured by the aggregate property value over a three-year period, is sensitive to the property tax rate. They estimate that for every 10 percent decrease in the property tax rate, the tax base will grow by 1.7 percent. Kang, Skidmore, and Reese (2015) further study Proposal A, examining the potential for differential response to school spending and property taxes by businesses and residents. Using the same methodology as Skidmore, Reese, and Kang (2012), Kang, Skidmore, and Reese (2015) find that residential property values are more sensitive to school spending than property tax rates, but business property values are more responsive to tax rates than school spending. They also point out that business property values respond more to tax rate increases than residential property values.

Johnson and Walsh (2013) also study Michigan's Proposal A but examine how the differential property tax provisions for vacation homes affect vacation home density. By examining vacation home density, Johnson and Walsh (2013) further decouple the link between most public services and the taxes collected, as vacation home owners should not care about things like school quality (although they may care about other local public goods). Their results show that a 3–4 mill decrease in the property tax rate is associated with an increase of 1 vacation home per square kilometer.

In a unique contribution to the literature on property tax capitalization, Bradley (2017) investigates the role that cognitive bias may play in the process. Like other papers in this literature, Bradley (2017) uses Michigan's Proposal A as a natural experiment, but instead of the aggregate data used by other studies, applies microdata on homes sold in Ann Arbor, MI. Bradley points to a unique feature of Proposal A that gives home buyers a temporary tax advantage before their home is assessed at acquisition value for tax purposes—that the taxable value of the home is reset on January 1st of each year. This feature means that a buyer will temporarily pay property taxes based on the prior assessed value for the year they purchase the home (the taxable value will be further below acquisition value for homes the longer it has been since the home sold). Bradley uses this temporary tax savings to show that home buyers are overly-sensitive to the temporary tax savings—for every \$1 increase in temporary tax savings, the sales price increases by \$29. The size of the price increase from the temporary tax savings makes it appear as though buyers are capitalizing a permanent tax savings instead, or that buyers fail to recognize the temporary nature of the tax savings. This result implies that cognitive bias may play a role in how and to what extent property taxes are capitalized into home prices.

In a unique and compelling study, Lutz (2015) examines a 1999 school finance reform in New Hampshire to estimate the effect of property taxes on both residential home construction and capitalization. This is the first modern empirical paper to consider both the price and quantity response to property taxation simultaneously, pointing out that the supply of housing should drive expected capitalization effects of a change in property taxes. To address the primary concern of endogeneity between property taxes, prices, and building, Lutz relies on the nature of the school finance reform in New Hampshire. The reform came as a series of grants to municipalities based on the per-pupil property wealth; the grants were then used to fund property tax reductions, creating an exogenous change in the property tax burden across municipalities but holding public services constant. Lutz finds that a 15 percent reduction in local property taxes (induced by the grant allocation) causes an increase of 11–22 percent in residential construction,

implying an elasticity between -0.73 and -1.46. Perhaps most interestingly, Lutz finds that the building response is not evident within 50 miles of Boston (covering the major suburban areas of the state), but that the property tax cuts in that area were capitalized into home prices. As predicted by the relative supply elasticities between denser suburban areas and the more sparsely populated areas of the state, only limited capitalization takes place outside of the 50-mile ring around Boston.

Outside of the studies that rely on law changes as direct natural experiments, there are two studies that use variation created by property tax differences that occur at municipal borders to identify the effect of property taxes on property values. This method is referred to as the border discontinuity method,¹⁵ and the idea is that areas close to, but on opposite sides of, a municipal boundary are identical (or at least similar) except for a difference in property taxes. If this is true, then comparing the sales prices of similar homes close to the municipal border but on opposite sides allows an unbiased estimate of property tax capitalization.

Gallagher, Kurban, and Persky (2013) apply the border discontinuity method to school district boundaries in Cook County, IL (excluding Chicago). The design of the study goes further than applying the border method to all district boundaries, instead using only boundaries within a city that divides by school district. In addition to applying the border method at specific boundaries, the authors highlight that public services can be better controlled for using the sale of small homes, as these homes are less likely to be occupied by families with children and thus the buyers will not be interested in school quality. After taking all these precautions to isolate the effect of school district property taxes, Gallagher, Kurban, and Persky (2013) find that these taxes are nearly fully capitalized into home values (97 percent).

In another application of the border method, Livy (2018) examines capitalization in Franklin County, OH. This study is unique in that the environment allows the researcher to use properties sold within the same school district, but with different applicable tax rates. The research also employs a large dataset of home sales across a long time period where tax changes occur, allowing the use of property fixed effects and local neighborhood effects. Livy's results suggest that for standard discount rates, property tax differences are fully capitalized into a home's sales price.

Although taxes are the focus of this review, and the modern literature on the economic development effects of property taxes satisfyingly disentangles these from the effect of public services, most policy inextricably links the two. There is an enormous literature examining the capitalization effects of local school quality, which in many ways mirror the tax capitalization literature. Nguyen-Hoang and Yinger (2011, 46) review the literature on school quality capitalization that came out between 1999 and 2011 summarizing the results as "house values rise by 1–4 percent for a one-standard deviation increase in student test scores." The literature on both general local public goods (such as mass transit options) and school quality continues to grow, with most newer studies following a natural experiment or quasi-experimental approach. These results should be considered in tandem with empirical work that isolates the effect of property taxes.

¹⁵ This method was introduced by Holmes (1998) and Black (1999).

Limiting property taxes that pay for valuable services causes property value declines; it is not just that people want lower property taxes, they want public services provided in a cost-effective way. Property taxes also cause some degree of mobility, and these estimates are likely reflective of an “all else equal” approach suggesting that if property taxes and public services are not in line, then residents may relocate to find a better tax/service bundle. The literature that best identifies a pure tax effect (holding public services and other characteristics constant) on property values shows that property tax differences become fully capitalized into property values. To understand how initial characteristics may influence this result (especially local demand and supply elasticities), and if there is any room for property taxes to directly affect other economic outcomes of interest, more studies that follow natural experiments would be useful.

Employment Effects: Results from Traditional Studies

There is only a sparse post-1997 literature on the effects of property taxes on employment; noticeably little modern work has even attempted to make this link. Considering the findings from the capitalization literature this may not be surprising; if property taxes are fully realized in property values there may be no room for other economic effects. It is also worth noting that the studies examining the employment effects of property taxes use more traditional methods of estimation—attempting to control for other factors and estimating a regression model, but do not make use of exogenous variation in tax rates created by a natural experiment. All these studies also examine a single metropolitan area and would be subject to the criticism that their results would differ if a similar policy were implemented in a different area (if the policy interacts with the local economy to produce a differential effect on the population of interest).

Mark, McGuire, and Papke (2000) examine the effect of personal and business property taxes (as well as sales taxes) on employment growth in the Washington, DC metro area. This study estimates the employment effects of local taxes using a municipality fixed effects model and the changes in tax rates occurring over the 1969–1994 period. The results in Mark, McGuire, and Papke (2000) show that a 1 percentage point increase in the personal property tax rate reduces annual employment growth by 2.44 percentage points, or that the elasticity is -2.12. The study also finds that corporate income tax rates and commercial property tax rates are not related to employment growth. The authors note that outside of the fixed effects, the model does not control for public services like education, which may cause biased estimates of tax effects.

In a study of the Chicago metropolitan area, Dye, McGuire, and Merriman (2001) examine how the property tax rate and property tax classification¹⁶ contribute to employment growth. Dye, McGuire, and Merriman (2001) use data from 109 municipalities across the 1991–1996 time period. They use a model with county level fixed effects and also control for a host of other factors, but notably not the level of local public services. Their results show that raising the tax rate on commercial property by 1 percentage point results in a 1.1–1.8 percentage point decrease

¹⁶ Classification refers to the ability of a municipality to impose different effective tax rates on specified classes of property (residential, industrial, commercial). This can be accomplished by varying the property tax rates or by applying different assessment ratios to each property classification, as is the case in the Chicago metro area (Cook County).

in employment. Dye, McGuire, and Merriman (2001) find no independent effect of classification on employment outside of the tax rate effect.

Bollinger and Ihlanfeldt (2003) study the mix of tax incentives in Atlanta, including commercial/industrial property tax abatements, residential construction/rehabilitation abatements, and job tax credits. They are interested in the effect of these policies on the share of regional employment within the Atlanta area. They use census tract level data, and control for other demographic and economic factors of areas outside of taxes but do not employ a natural experiment approach. They find that having a commercial/industrial policy in a census tract is associated with an 80 job increase over a decade. Notably, they use areas within the same metropolitan area as the basis for comparison for policy-treated tracts, and these areas may be a poor comparison area as they could be subject to spillover effects from the policies.

Non-U.S. Studies

Outside of studies that examine U.S. policy, there are a host of studies that examine the economic redevelopment effects of property taxes and tax abatements in other countries. There are two studies that stand out in particular for employing modern methods of identification: Duranton, Gobillon, and Overman (2011) and Elinder and Persson (2017). Although the focus of this review is on U.S. based studies, these papers offer a basis for comparison to the U.S. case and a look into how a different economic environment may interact with property tax policies.

Duranton, Gobillon, and Overman (2011) examine the effects of a non-residential property tax in the U.K. This tax is described as being imposed on all property uniformly, but with differing rates, with the revenues not used for local services. Although empirical work does not rely on a natural experiment that reforms the tax, the authors use variation through time and across jurisdictional borders (in combination with instrumental variables) to create plausibly exogenous variation in the policy to identify the economic effects. Duranton, Gobillon, and Overman (2011) estimate the employment elasticity with respect to the non-residential property tax rate to be approximately -1, but find essentially no effects on firm entry/exit from the property tax. The primary result is statistically significant in the authors' preferred specification, and is robust to some, but not all of the specification checks in the paper. The results of the Duranton, Gobillon, and Overman (2011) are in line with the Chicago and Atlanta U.S. studies that suggest a large effect of business property taxes on employment.

In another European study, Elinder and Persson (2017) examine the capitalization effects of a cut to the national property tax in Sweden. They employ a natural experiment approach, using a reform that lowered the national property tax rate from 1 percent to 0.75 percent of the taxable value and capped the annual tax liability (the annual cap was binding for about half of all properties). The Swedish reform took place in 2008 and created a differential tax benefit for properties that increased rapidly and for properties with tax liability that was previously lower than the annual cap. The authors estimate a difference-in-difference model around the national reform, using the price path of properties below the cap to create a counterfactual for the price path of properties above the cap. Unlike in the U.S. studies that suggest nearly complete capitalization, Elinder and Persson (2017) find evidence to suggest that the property tax cut was capitalized at between 1/3–1/2 of full rate for properties in the top 5 percent of value, but detect

no appreciable capitalization beyond those effects. The study authors suggest that the differential capitalization across the distribution of homes is likely driven by scarcity of land in the top segment of the market, large tax reductions in the top segment being quite salient, and the buyers of these homes being relatively more financially literate.

Tax Increment Financing

Much of the literature examining economic development effects of general property taxes focuses on large-scale state reforms; however, economic development is often not the intended outcome of these reforms. A property tax policy that is typically intended to impact economic development is Tax Increment Financing (TIF). The idea of a TIF is to designate a special district where the taxable value of properties is frozen (or potentially reduced) for the purposes of standard property tax collection, while the incremental property value appreciation is taxed to finance an economic development project. TIFs are implemented with the “but for” distinction, indicating that the TIF is only allowed if the economic development project would not have occurred “but for” the special tax treatment. Often, local government bonds will be issued to fund the economic development project and repaid with TIF generated revenues. Merriman (2018) summarizes the use of TIF across U.S. states, reporting at least one active TIF district in all states except Arizona (where they are not allowed) and Delaware. Merriman (2018) also reports that many states have several hundred¹⁷ or even thousands¹⁸ of active TIFs, and that there is \$37 billion of debt associated with TIFs between 2000 and 2014.¹⁹

The idea behind TIFs directly funding economic redevelopment is clear; however, it is challenging to discern the actual economic impact of the policy for several reasons. The first comes directly from the “but for” distinction of TIFs. While TIFs are supposedly not to be designated unless the economic development project would not have happened without the TIF, knowing the counterfactual is not possible (would the developer have made the investment even without the TIF?). Second, because evaluators will not know what would have happened in the TIF designated area in the absence of the policy, they need to estimate this using a comparison area. Finding a comparison area that follows the path that the TIF area would have taken requires an area that is otherwise similar to the TIF district, but that is not subject to spillover effects from the TIF itself, characteristics that may be difficult to measure. Third, it is possible that TIFs are chosen in areas that will be trending differently than comparison areas. Lastly, evaluators must deal with policy overlap from other federal, state, and local policies that can interact with the TIF (zoning policy, other tax policy, Enterprise/Empowerment zones, etc.).

A large literature has emerged on the economic development impact of TIFs. In the most comprehensive review to date, Merriman (2018) offers the most recent review of 31 empirical studies of TIFs occurring between 1994 and 2017. Most of the reviewed studies use data generated pre-2000, although a few of the newer studies use data up to 2013. Merriman (2018, 52) offers the following summary judgement of the empirical TIF literature:

¹⁷ CA, CO, FL, IN, ME, MI, MO, NE, OR, PA, and SD all have at least 100, but less than 1,000 individual TIFs.

¹⁸ IL, IA, MN, OH, TX, WI all have at least 1,000 individual TIFs.

¹⁹ \$25 billion of that debt is in California alone, the largest by far for any individual state. Missouri has the second most TIF debt at \$1.4 billion over the 2000–2014 period (Merriman 2018).

42 percent of the studies—13 total—have positive results. Of the remaining 18 studies, 5 have negative results, 8 have neutral results, and 5 have mixed results. The neutral results suggest that TIF did little or nothing to stimulate economic development, so these studies might be viewed as an argument against the use of TIF.

[...]

[T]he most recent studies, which tend to have the strongest data and best methodologies, are much less positive than earlier studies. Taken together, this review of the rigorous evaluation literature suggests that in most cases, TIF has not accomplished the goal of promoting economic development.

Merriman (2018) goes on to suggest that there is evidence that TIF has positive effects in some cases, although this does not seem to be a function of location as even studies of the same TIF locations produce mixed findings.

Greenbaum and Landers (2014) also offer a review of the economic development effects of TIF. This older review of the empirical work on TIFs does not include studies published after 2014, excluding many of the studies that Merriman (2018) finds to be the most credible and that have the least positive findings. Given this exclusion of the newer empirical work, it is not surprising that Greenbaum and Landers (2014) find the literature on TIFs to be more positive. Greenbaum and Landers (2014) suggest that “the majority of the studies find evidence of some positive associations between TIF districts and growth in property values” (Greenbaum and Landers 2014, 661) but that “papers examining economic development outcomes finds less clear evidence of positive associations with TIF” (Greenbaum and Landers 2014, 662). Most of the studies categorized as studying economic development focus on employment as the outcome.

The vast empirical literature that exists on the economic development effects of TIF broadly points toward a case that it is not an effective economic redevelopment tool; however, it seems reasonable to call for improvements in the existing empirical work before making a final judgement. The continued advance of quasi-experimental methods, especially the synthetic control method, offers a way to improve the construction of a counterfactual for what would have happened in TIF areas in the absence of TIF that would offer unbiased estimates of economic development effects. The end of TIF in California offers a large-scale change to the use of TIF that could also be useful in constructing empirical work using a natural experiment.²⁰

Business Improvement Districts

Designing special property tax districts with the goal of economic development is not unique to TIFs. Another increasingly common policy is Business Improvement Districts or BIDs. A BID is a locally formed collection of business operators and property owners who vote to levy a tax on themselves and use the proceeds for provision of local public goods.²¹ The exact process of

²⁰ See Lefcoe and Swenson (2014) for a description of the end of TIF use in California.

²¹ Examples of BID fund use are street cleaning, maintenance, capital improvements, marketing, and safety (Brooks 2007).

forming a BID differs across areas, but the idea that a BID creates a new tax (and spending) jurisdiction based on geography is the common characteristic.²² BIDs differ from TIFs in that they are typically smaller scale, do not typically take on debt, and do not directly divert revenues that would be collected by the regular property tax.

Empirical work on the economic development effects of BIDs is extremely thin, consisting of only a few studies: two that estimate the effect of BIDs on property values (Ellen, Schwartz, and Voicu 2007; Brooks and Strange 2011), and another on local criminal activity (Brooks 2008). One reason the empirical literature on the economic development effects of BIDs may be sparse is that it began to emerge as the level of sophistication in empirical work was rising rapidly, allowing researchers to predict the shortcomings of using standard methods. Estimating the effects of BIDs on economic outcomes comes with the standard problems encountered in the general property tax and TIF literatures, with the added complication that BIDs are voluntary, so by definition they are endogenously formed (because they are the result of voting by local businesses that demand public service improvements and corresponding taxes).

With the challenges to estimating the effects of BIDs in mind, Ellen, Schwartz, and Voicu (2007) examine the effect of BID adoption in New York City on both commercial and residential property values using a difference-in-difference approach. For their primary analysis, Ellen, Schwartz, and Voicu (2007) use as the comparison group in their study properties that are not in the BID and not directly adjacent to the BID, but still in the same ZIP code as BID properties. They choose this group as they suggest it will not be subject to spillovers from the actual BID, but still share common characteristics with BID areas, reducing the possibility of biased estimates. The authors then see how property values change in BID areas relative to comparison areas after BID adoption. Results of the study show that BID implementation in New York City is associated with a 15.7 percentage point increase in commercial property values,²³ and that the effect on residential property values is likely small, short-lived, and largely driven by the anticipation effects of the BID.

As part of a larger theoretical and empirical investigation into multiple aspects of BIDs, Brooks and Strange (2011) also estimate the effect of BIDs on commercial property values.²⁴ The study controls for neighborhood by year to absorb time varying neighborhood heterogeneity (an improvement over standard difference-in-difference estimation), and compares properties in BIDs with properties in three different comparison areas: places that were almost BIDs, places that are within 1km of a BID, and a sample that is propensity score matched to be the most similar to BIDs. The study also controls for a host of property characteristics and estimates a BID effect by property size. Brooks and Strange (2011) find that, relative to areas that were almost BIDs, commercial properties increased in BIDs by 25 percent on average across all sizes of property, and that this effect is completely driven by properties in the top half of the square

²² See Brooks (2007) and Brooks (2008) for details on BID formation in California, see Ellen, Schwartz, and Voicu (2007) and Meltzer (2012) for details on BID formation in New York City. Also see Brooks and Strange (2011) for a theoretical model of BID formation and accompanying empirical estimation.

²³ In an alternative specification comparing active BIDs to areas that will become BIDs in the future, Ellen, Schwartz, and Voicu (2007) estimate a 31.2 percentage point increase in property values from BID adoption.

²⁴ Brooks and Strange (2011) examine BIDs in a city, but do not divulge the name of the city because they do not want to expose information about individual voting on BID adoption.

footage distribution. The propensity score estimated effect largely confirms this pattern, although the magnitudes of the property value increase are slightly smaller.

The property value increases documented from BID adoption imply they are effectively delivering the local public services they promise at an acceptable cost for participants. Evidence that this is in fact the case comes from Brooks (2008) in her study of BID adoption and criminal activity in Los Angeles. Brooks uses a variety of strategies to control for both neighborhood and time varying effects, as well as matching to an appropriate comparison area to estimate the effects of BID adoption on crime. Her results show that BIDs are quite effective at reducing crime, with a 6–10 percent post adoption reduction in crime, and robust across the various specifications. Brooks also calculates that even in the case BIDs that are only spending on crime reduction (an extreme case), they are reducing violent crimes in a cost-effective way (the cost of reducing crime is lower than the social benefit).

BIDs and the empirical work on them to date are interesting because they both mirror the property tax in important ways. Like the property tax, BIDs are meant to finance valuable local public services. Like the empirical work on the property tax, the empirical work on BIDs shows that the services provided are valuable to those that consume them. When tax collection is in line with service provision, the public is satisfied; when it is out of line, adjustments are made in the form of mobility or price capitalization.

Conclusions from Property Tax and Related Literature

Property taxes and tax concessions tend to be fully capitalized into property values. The literature that best divorces pure property tax effects from public service contributions all shows complete, near complete, or in one case over-capitalization. However, limiting property taxes that pay for valuable public services causes property value declines. The literature shows that it is not merely lower taxes that matter; citizens demand value and efficiency in public service provision. Future literature should continue to follow the trend of outstanding contributions of the last 10 years and use natural experiments in property tax changes and related policies to identify the effects of the property tax across areas with different market characteristics and to examine outcomes related to capitalization like mobility and building activity.

The empirical literature that exists on the economic development effects of Tax Increment Financing (TIF) broadly points toward a conclusion that TIF is not an effective economic redevelopment tool. This literature is less developed than the general property tax literature, and it seems reasonable to call for improvements in existing empirical work before making a final judgement on the general idea of TIFs. The literature on Business Improvement Districts (BIDs) is much more positive than the literature on TIFs, although it relies on only a few high-quality studies. Like the empirical work on the property tax, the empirical work on BIDs shows that the public services provided are valuable to those that consume them. When tax collection is in line with service provision, the public is satisfied; when it is out of line, adjustments are made in the form of mobility or price capitalization.

Spatially Targeted and Zone Based Tax Concessions

Zone-based tax concessions are part of a broader set of place-based policies intended for redevelopment of poor or blighted (mostly urban) areas. Much like TIFs, a Zone is typically carved out of an area from an existing jurisdiction to be given preferential treatment.²⁵ Unlike TIFs, Zones are typically paid for with general revenues, and typically this funding comes from a higher level of government (for example, the U.S. federal government funds local Zones in cities). Another distinguishing characteristic of Zones is that while they are geographically distinct areas, these areas are typically chosen because of the characteristics (poverty rates, unemployment, income, etc.) of the people that live in the area (Neumark and Simpson 2015). Zones also typically include other types of area-based assistance outside of tax concessions such as grants, infrastructure spending, and social services for residents.

Peters and Fisher (2002) review the early empirical literature on Zones, focusing on U.S. state programs. In summarizing the state of the literature at that time, Peters and Fisher (2002, 48) conclude:

Given the paucity of enterprise zone studies, it seems highly unlikely that a broad research consensus on the impact of enterprise zones on growth will be possible for some time to come. The conclusions of the extant literature do point in quite contrary directions; however, the vast majority of the recent literature suggest that enterprise zones have little or no positive impact on growth.

The papers discussed by Peters and Fisher (2001) were all published in or before 2000, and none of them examined the larger scale Zone programs (California's Enterprise Zones, or the Federal Empowerment Zone). Since the Peters and Fisher review, there have been a plethora of studies examining the economic development effects of Zone-based policy. Many of these studies employ the rigorous quasi-experimental research methods that have become common in the program evaluation literature.

Neumark and Simpson (2015) provide an extensive review of the modern literature on Zone based incentives, focusing on studies based in the U.S. and Europe, and highlighting several studies of large state programs and the federal program. Neumark and Simpson (2015, 31) have a pointed discussion of the California Enterprise Zone program, focusing on the contribution of Neumark and Kolko (2010),²⁶ where they summarize the results as:

Across a variety of specifications, there is no evidence that enterprise zones affect employment. The estimates are small, statistically insignificant, and negative as often as they are positive. The statistical power of the evidence is modest, as the confidence intervals for the estimated employment effects are rather

²⁵ In addition to the work on zone-based tax incentives there is a literature on the New Markets Tax Credit (NMTC), a policy designed to subsidize private investment in low income neighborhoods. Using a regression discontinuity design, Freedman (2012) finds that the NMTC reduces neighborhood poverty and may have other positive effects. Harger and Ross (2016) find that the NMTC increases employment at retail and manufacturing firms, but decreases employment at wholesale and transportation firms. Freedman (2015) suggests that most of the benefits from the NMTC accrue to residents outside of the targeted area.

²⁶ Elvery (2009) also studies the California Enterprise Zone program and produces similar findings.

large... estimates do not exhibit any evidence of leading or lagged effects, but instead cement the view that enterprise zones in California did not affect employment... in the analysis accounting for the overlap between state enterprise zones and redevelopment areas or federal zones, there is similarly no evidence that enterprise zones have positive employment effects, whether or not they are combined with these other local policies.

The most rigorous evaluations of other state Enterprise Zones comes from studies of Colorado (Billings 2009) and Texas (Freedman 2013) programs. Each of these studies uses a quasi-experimental approach in estimating the impact of a state program, with Billings (2009) using a border-matching approach and Freedman (2013) using a regression discontinuity approach.

Billings (2009) matches business establishments in EZ areas with establishments outside of EZ areas, using a sample where both are close to the border of the zone. This matching is intended to produce a set of control establishments that is the most similar to actual EZ treated establishments, and is subject to the most similar local economy, but does not receive an preferred tax treatment. After matching, Billings has a sample of 55,952 business establishments over the 1990 to 2000 time period to use for analysis. Billings finds that the Colorado EZ tax credits have no influence on business establishment location—meaning that new businesses are not more likely to start in the Zone area than they are in the comparison area. Billings finds that employment at new establishments increases by between 1.5 and 1.75 employees as a result of the EZ tax credits, but most results show no effect at existing establishments.

In an unusually compelling study, Freedman (2013) uses a regression discontinuity design to estimate the effect of the Texas Enterprise Zone program on various economic development outcomes. Freedman's empirical work relies on the rules for how an area qualifies for EZ status to help identify the effect of the program independent of other factors. Census blocks in Texas were automatically assigned to be EZ areas if they had a poverty rate higher than 20 percent. Freedman can use the automatic assignment of blocks that happens at 20 percent, and the fact that blocks within a small bandwidth of 20 percent are observably similar to estimate the effects of the program. Freedman finds that the annual growth rate for residents of EZ areas is 1–2 percent higher than areas that were just short of the poverty qualifying threshold, resulting in 35–42 resident jobs during the time of the program.²⁷ Freedman also documents a slight increase in EZ population, reductions in poverty, and greater house price appreciation using the regression discontinuity design. In concluding what his results imply about EZ programs, Freedman (2013, 342) suggests the following caveats:

[T]he EZ program in Texas is different from those of most other states, where in general localities must apply for EZ status....[I]t would be misguided to assume that if one were to expand EZ coverage to include more affluent communities, it would have similar effects in those areas.

[F]inding positive effects of the program does not immediately imply that it is cost-effective....[M]any of the jobs created or preserved are lower-paying

²⁷ Freedman finds larger (3–5 percent) increases for workplace employment, but these results are not as precisely estimated as the resident effects.

positions. This, combined with cost-of-living increases in EZs, would tend to erode any improvement in overall welfare owing to the program.

Freedman's caveats point out that even for a compelling empirical study the link between results of an existing program and future policy is difficult to make. One potential way to improve on the viability of using empirical results to inform economic development policy is to examine a program that has uniform benefits but reaches a wide range of areas. One such program is the federal Empowerment Zone program.

The federal Empowerment Zone program is based on the zone concept-offering a series of tax incentives to employers locating in designated areas. Unlike state programs, the benefits of the federal program are largely uniform and designated areas exist across state boundaries. This offers the advantage of studying a similar set of incentives across a group of treated areas that is heterogenous. The federal EZ program offered a wage tax credit based on employee and employer location within a designated area in the following cities: Atlanta, Baltimore, Chicago, Detroit, New York, Philadelphia, and Camden. EZs were also given an allotment of Social Service Block Grants, and some smaller tax incentives for capital investment. See Hanson (2009) for full details about EZ designation and incentives.

There are several studies of the federal EZ program including Hanson (2009), Hanson and Rohlin (2011a), Hanson and Rohlin (2011b), Hanson and Rohlin (2013), Busso, Gregory, and Kline (2013), and Reynolds and Rohlin (2015). Hanson (2009) and Busso, Gregory, and Kline (2013) both study the primary effects of the EZ program on several economic development outcomes, while the other studies examine aspects like differential effects across the income distribution and by business type, and spillover effects. There is also work by Ham et al. (2011) that examines both the federal EZ and state EZ programs simultaneously.

The Hanson (2009) and Busso, Gregory, and Kline (2013) studies use different methodology to study the federal EZ and come to different conclusions about the effects of the program. Busso, Gregory, and Kline (2013) compare the outcomes for EZ designated areas with outcomes in places that applied for EZ status but were rejected and places that later became EZ areas using a difference-in-difference design. Busso, Gregory, and Kline (2013) use a procedure to reweight the comparison area sample to more closely resemble the characteristics of the EZ designated area and provide a more accurate counterfactual for zone areas. The primary results from Busso, Gregory, and Kline (2013) suggest a substantial positive effect of the EZ program—a statistically significant 21 percent increase in jobs in EZ neighborhoods. Using an alternative data set that allows the researchers to distinguish between place of residence and place of work, Busso, Gregory, and Kline (2013) find that most of the job growth was concentrated among residents of EZ areas (although the effect is not precise). In addition to employment effects Busso, Gregory, and Kline (2013) also estimate a 12 percent increase in wages for residents living in the EZ, and a 30 percent increase in residential housing values (although this effect is not precise in the preferred specification). In concluding about the results of their work, Busso, Gregory, and Kline (2013, 931) suggest:

The conclusion of our welfare analysis is that the EZ program appears to have successfully transferred income to a small spatially concentrated labor force with

modest deadweight losses aside from the usual cost of raising the funds for the subsidy itself. We caution however that our study provides only a short run evaluation of the EZ program....The responses of firms, population, and prices may well differ substantially over longer periods of time, if EZ subsidies in fact persist over such horizons.

[...]

Finally, we emphasize that many of our empirical results are imprecise and should not necessarily be expected to generalize to later round and future zones. Additional zones targeting less heavily distressed communities may yield larger distortions as such communities may be closer substitutes with surrounding areas....While we find it plausible that the mix of large block grants and wage credits accompanying EZs would yield different results than their smaller state level predecessors, more work is necessary to disentangle the effectiveness of various combinations of spatial subsidies.

Hanson (2009) uses a different methodology and different data than Busso, Gregory, and Kline (2013) to study the federal EZ program. Noting the potential for an EZ evaluation to be biased due to selection of treated zones from a pool of applicants, Hanson (2009) implements an instrumental variables strategy, using membership and tenure on the House of Representatives Ways and Means committee as a source of variation driving EZ selection. Along with instrumenting, Hanson (2009) also implements a triple-difference estimation strategy, comparing EZ areas and their larger city to the change in applicant areas and their larger city. The primary assumption behind this strategy is that Ways and Means membership and tenure do not directly influence local economic outcomes except through EZ designation. Hanson (2009) presents evidence that this assumption may be valid. The finding in Hanson (2009) suggests no effect of the EZ program on resident employment or poverty rates, but a large effect on residential property values—increasing them by \$100,000 over the course of a decade.

Other research examining the federal EZ suggests that impoverished residents did not benefit from the program and that the areas became more attractive to higher-income residents, with most of the benefits accruing to neighborhoods in EZ that were relatively more attractive prior to designation (Reynolds and Rohlin, 2015). Hanson and Rohlin (2011b) present evidence that the EZ program had a differential effect across firms in different sectors of the economy, with the share of firms in service and retail increasing at the expense of firms in transportation, finance, insurance, and real estate. Hanson and Rohlin (2011a) suggest that new firms enter the EZ area, but that the magnitude of this effect is small; only about 20 new firms enter EZ areas and at a cost of entry of \$19 million per firm. Hanson and Rohlin (2013) examine the potential for the EZ program to induce a spillover effect on surrounding neighborhoods (or economically similar neighborhoods). Hanson and Rohlin (2013) find that areas adjacent to or economically similar to EZ areas experience a loss of both firms and employment, with the magnitude of the loss roughly equivalent to estimated gains in the zone.

Non-U.S. Studies of Zone-Based Policies

Although the focus of this review is on U.S. based studies, there is an unusually high concentration of compelling work on Zones that examines the French Enterprise Zone program (*Zones Franches Urbaines*, or ZFU). The French ZFU program exempts firms from paying the wage tax if they hire 20 percent of their labor force locally from the designated area (Gobillon, Magnac, and Selod 2012). The wage tax represents about 30 percent of all labor costs (Gobillon, Magnac, and Selod 2012), so the ZFU program was quite generous compared to the wage tax credits offered by the federal U.S. EZ program.²⁸

Gobillon, Magnac, and Selod (2012) study the ZFU program using a difference-difference approach, using both a geographically close comparison group and a propensity score method to create a counterfactual. They estimate that the ZFU program had a modest effect on the probability of an unemployed resident obtaining a job. Their preferred specification suggests that the ZFU program was responsible for 10 new transitions between being unemployed and finding work. Briant, Lafourcade, and Schmutz (2015) study how the ZFU program may have interacted with the local geography, examining if there is a differential effect across areas by their degree of spatial isolation. Using a difference-in-difference research design Briant, Lafourcade, and Schmutz (2015) find that the ZFU program increased the inflow of business establishments by 16 percent in less isolated areas, while there was no measurable effect on the most isolated areas.

Contrary to the small findings in Gobillon, Magnac, and Selod (2012), follow up work by Mayer, Mayneris, and Py (2017) finds that ZFUs have a substantial effect on employment, increasing employment by 24 percent. Mayer, Mayneris, and Py (2017) also find that ZFUs are responsible for increases in firm entry, but that this is largely caused by diversion of firms from neighboring un-subsidized areas. While the estimated effects on employment are large, it is unclear what the net effects of the ZFU policy would be considering the diversion of firms from neighboring areas.

In the most recent work on ZFUs, Gobillon and Magnac (2016) implement an evaluation of the ZFU program using the synthetic control method. This method is particularly attractive for the study of zone-based policies as it largely eliminates the concern of finding a comparison area with a parallel trend prior to policy implementation. Synthetic control estimates of the effect of the ZFU program on unemployment are much less precise than the small positive effects estimated in Gobillon, Magnac, and Selod (2012), and are in fact of the opposite sign, casting doubt on the previous findings.

Conclusions from the Spatially Targeted and Zone Based Literature

Overall, the best description of the empirical findings in the EZ literature is “mixed”. It seems that for every positive finding for a particular program, there is an offsetting null or negative finding. These differences come despite the ever-increasing sophistication of empirical methodology to address bias in estimation. In summarizing the literature on EZ-style policies, Neumark and Simpson (2015, 46) come to a similar conclusion, stating the following:

²⁸ The federal U.S. program offers a 20 percent wage tax credit (non-refundable) towards the first \$15,000 in wages paid.

[I]t is very hard to make the case that the research establishes the effectiveness of enterprise zones in terms of job creation or welfare gains, although there clearly are some studies pointing to positive effects. Further progress requires effort to figure out what features of these programs can make them more effective, following on some early efforts in this direction in the existing research. Second, although there has been a slew of new studies in the past few years—and even many studies focusing on the same program—there has not been enough of an attempt to reconcile the disparate evidence. This kind of careful, often painstaking work may well help sharpen the findings from a research literature in which the findings remain rather disparate.

It seems the most appropriate conclusion to draw from the empirical research on EZ policies is that more research needs to be done.

Firm Specific Subsidies and Corporate Taxes

Tax incentives for economic development seem to be increasingly focused on attracting a single large firm. With each passing year, the popular press reports on a new firm (or firms) that localities are attempting to entice to locate (or stay) in their district. Recent examples include Amazon’s HQ2, Foxconn, Boeing, Alcoa, and Intel among many others. Mattera and Tarczynska (2013) estimate that state and local governments have spent \$64 billion on incentive packages over the past 35 years. The academic literature on large scale firm specific subsidies and tax breaks is shockingly thin, despite the attention these incentives receive in the press and the dollars that are spent on them.

In a recent review of literature on state and local economic development incentives, Bartik (2018) categorizes 34 studies to determine the percentage of incentivized firms that were induced to make a location, expansion, or retention decision because of the incentive program. Bartik (2018) examines a wide-range of policies including grants, payroll credits, property tax abatements, sales tax credits, and wage credits. In determining an appropriate response to use for his summary, Bartik carefully classifies each study by the likely bias in the estimation. This is an interesting exercise in its own right as it sheds light on the state of the literature—Bartik (2018) suggests that of the 34 studies, only 7 have no obvious bias (23 are biased toward a positive finding, and 4 are biased toward a negative finding). Across the studies that Bartik (2018, abstract) examines, he concludes that “typical incentives probably tip somewhere between 2 percent and 25 percent of incented firms toward making a decision favoring the location providing the incentive.”

There are two recent studies of large scale firm specific subsidies that use quasi-experimental or natural experiment methods: Greenstone, Hornbeck, and Moretti (2010) and Patrick (2016).²⁹ Both of these studies estimate the effect of a large new manufacturing facility using a sample of plant relocations between 1982 and 1993 from the magazine *Site Selection*. Importantly, this is a select sample, both in terms of the type of business being relocated (manufacturing), the time period chosen, and the fact that the relocation was covered by an international media outlet. Both

²⁹ Also see Freedman (2017) for an excellent study on the labor market effects (and their persistence) of a historical program aimed at directing grant money to attract firms to Mississippi in the 1930s.

studies use a difference-in-difference methodology, with the primary difference between them being how the researchers construct a counterfactual for what would have happened in the area where the subsidized firm relocates. In both cases, this is done through the choice of comparison areas. In the Greenstone, Hornbeck, and Moretti (2010) case, they rely on comparing winning areas to areas that were runner-up in the competition to land the new firm. In the Patrick (2016) case, a comparison group is intentionally constructed by matching to geographically close areas that have similar observable characteristics as the winning area.

Greenstone, Hornbeck, and Moretti (2010) generally find large positive effects of the relocating facility on the local economy. Their primary estimates suggest that productivity at surrounding facilities increases by 12 percent after five years, with larger effects at facilities that share labor and technology characteristics. In addition to positive effects on the productivity of surrounding workers, Greenstone, Hornbeck, and Moretti (2010) also estimate a positive effects on other firms entering the area (an increase of 12.5 percent), and on worker wages (an increase of 2.7 percent).

Importantly, although Greenstone, Hornbeck, and Moretti estimate a large average effect on the surrounding facilities, they also point out that for individual plants the effect varies substantially. Showing a series of case studies, Greenstone, Hornbeck, and Moretti find that the average effect at 18 of the 45 facilities was to decrease productivity at surrounding plants, while the average effect at another 12 of the 45 was statistically indistinguishable from zero. Thus, the large positive effect in the aggregate is driven by positive effects at only 25 of the 45 plants, and among those only 13 had positive effects that were statistically significant at conventional levels.

In a compelling counter-weight to the results presented in Greenstone, Hornbeck, and Moretti (2010), Patrick (2016) makes the case that using runner-up areas to construct a counterfactual for areas where plants locate will cause biased results. Patrick (2016) demonstrates that pre-treatment outcomes and covariates differ greatly across the comparison and treatment areas used by Greenstone, Hornbeck, and Moretti, potentially causing bias. Patrick (2016) also points out that the institutional features of the process for reporting runner-up areas in the *Site Selection* publication may induce bias. Finally, Patrick (2016) investigates the possibility that *Site Selection* mis-identified the correct runner-up location and finds evidence that it did so in only two of nine cases.

After documenting that the Greenstone, Hornbeck, and Moretti identification strategy may suffer from severe flaws, Patrick outlines an alternative estimation strategy. Her primary estimates rely on matching places that had similar population, highway access, proximity to a metropolitan area, working age population, earnings, and were in geographic proximity to the actual firm location. Patrick presents evidence that her matched sample of comparison areas closely resembles the sample of locations where firms actually choose to locate. Patrick essentially finds the opposite effects found in Greenstone, Hornbeck, and Moretti, demonstrating that if anything large subsidized firms relocating to an area have a negative effect on the number of other business establishments (most specifications report a finding close to zero). Patrick also finds a much smaller effect on surrounding firms that are operating, showing only a 1–4 percent increase in the manufacturing output of the subsidized area. Despite these negative findings, Patrick does find substantial wage gains (2.6–3.5 percent) and employment gains (3–5 percent) for residents

where large firms relocate, although these estimates include gains from the subsidized firm. Finally, Patrick also finds that places where a heavily subsidized new firm relocates have increased taxes and levels of government debt.

Corporate Income Taxes

In addition to firm-specific incentives and targeting by geography, lowering corporate tax rates, or offering investment incentives to all firms can be used to induce economic activity. There is a fairly large body of evidence on the effect of corporate taxes (and non-targeted investment incentives) and much of it is designed around modern natural or quasi-experimental estimation strategy. I review some of the more recent and most compelling evidence here as a basis to compare broad corporate tax policy with the more targeted approaches already discussed.

Studies on the economic development effects of corporate tax policy can be broken into two broad categories: those that examine rates, and those that examine special incentives (such as bonus depreciation). The most compelling studies that examine corporate tax rates use variation in U.S. state rates (Rohlin, Rosenthal, and Ross 2014; Ljungqvist and Smolyansky 2014). Of the studies that examine special incentives, three examine bonus depreciation (House and Shapiro 2008; Zwick and Mahon 2017; Garrett, Ohn, and Suarez Serrato 2019), and the other examines the domestic production activities deduction (Ohn 2018).

Ljungqvist and Smolyansky (2014) examine the effect of corporate tax rates on employment using the border method and data on resident-based employment and incomes. Ljungqvist and Smolyansky compare counties that straddle state borders where corporate tax policy changes on one side and not the other. They examine 271 corporate tax changes over the 1970–2010 period. The border approach allows the researchers a tight control over other changes to the local economy that may be prompting corporate tax changes, limiting concerns of endogeneity bias in estimation. Ljungqvist and Smolyansky find that a one-percentage point increase in the top marginal corporate tax rate reduces employment by 0.3–0.5 percent. They also point out that corporate tax changes are not symmetric, that is they do not find employment effects from cutting the top marginal corporate tax rate unless that change happens during a recession. Along with employment changes Ljungqvist and Smolyansky also demonstrate that a 1 percentage point increase in the top marginal corporate tax rate results in an income decline of 0.3–0.6 percent (but that cutting corporate taxes does not have a corresponding positive effect unless that policy happens during a recession).

Using data on business location and employment, Rohlin, Rosenthal, and Ross (2014) also use the border method to examine state corporate tax rates. Rohlin, Rosenthal, and Ross examine how corporate tax rates affect the propensity for a business to open on either side of the border when one state changes policy. To bolster the link between policy and the outcome of interest, the researchers examine borders that have a reciprocal tax agreement so that both sides of the border are likely to have a labor market that is common. Using this strategy, Rohlin, Rosenthal, and Ross find that a 1 percentage point increase in the top marginal corporate tax rate reduces the likelihood of a business starting by 34 percent, with larger effects in the manufacturing and service sector.

Most of the studies that examine special corporate incentives focus on their effects on business investment, with Garrett, Ohn, and Suarez Serrato (2019) making the link between business investment and employment. All of these studies examine the U.S. aggregate economy, although some of them look at effects across industry and size of firm. House and Shapiro (2008) use the 2002 and 2003 implementation of bonus depreciation to study the effect of lowering the effective capital tax on investment.³⁰ They find a strong investment response to bonus depreciation that implies an elasticity between 6 and 14. Zwick and Mahon (2017) extend the analysis in House and Shapiro to include later years of policy and to examine the effect by size of firm. Zwick and Mahon also find a significantly large effect of bonus depreciation: it increased investment between 10.4 and 16.9 percent (depending on the year of the policy experiment). They also find that small firms respond more strongly to the policy than large firms. Ohn (2018) examines the investment response from the Domestic Production Activities Deduction (DPAD), a policy that allows firms to deduct some of their manufacturing income from regular taxable income. Ohn uses the implementation of the DPAD in 2005 as a natural experiment in a difference-in-difference framework to study the effects on investment. He finds that a DPAD induced 1 percentage point reduction in the corporate tax rate increases investment by 4.7 percent.

The literature on the effects of the corporate tax clearly shows that reducing it results in increased investment. Garrett, Ohn, and Suarez Serrato (2019) take this work a step further in examining how corporate tax induced investment impacts employment and earnings. Garrett, Ohn, and Suarez Serrato (2019) find that counties with a larger decrease in investment cost from bonus depreciation experience an increase in employment. Moving from the 25th to the 75th percentile in exposure to bonus depreciation increased county employment by 1.9 percent. The magnitude of their results implies a cost per job of \$53,000, but they do not find positive effects on worker earnings. Garrett, Ohn, and Suarez Serrato (2019) also find evidence consistent with capital investment replacing workers after an initial period of five years.

Conclusions from the Firm-Specific and Corporate Tax Literature

There is only a small literature examining firm specific subsidies, more empirical work is clearly needed to fully understand the effects of attracting large-scale employers using tax concessions on local economies. Although the studies that do exist are high quality, they are at odds about findings and appropriate methodology. The empirical work on broader corporate tax policy is also somewhat small, but there are a few recent high-quality studies. Broadly this literature suggests that increasing the corporate tax rate reduces employment and decreases business entry, while more targeted corporate tax policy increases investment.

³⁰ The 2002 bonus depreciation provision allowed firms to deduct 30 percent of the costs of investment from taxable income in the first year, with the remaining 70 percent depreciated over the standard asset life. The 2003 provision increased the first-year allowance to 50 percent. (House and Shapiro 2008)

Conclusion

The literature on the relationship between taxes and economic development has undergone a major transformation since the last comprehensive review in 1997. Following the broad trend toward credibility in empirical micro-econometrics, there have been major strides in identifying the local economic effects of many types of taxes and tax concessions including property taxes, spatially-targeted incentives, and firm-based incentives. With the improvement in methodology apparent in the literature it seems there are some broad conclusions that can safely be drawn from the studies reviewed here.

Property taxes and tax concessions are fully capitalized into property values. The newer empirical literature separates property tax effects from public service contributions and shows complete, near complete, or in one case over-capitalization. Limiting property taxes that pay for valuable public services causes property value declines; the literature shows that it is not merely lower taxes that matter, but that public services should be provided efficiently. This literature can continue to grow by using natural experiments to identify the effects of the property tax across areas with different market characteristics and to examine outcomes related to capitalization like mobility and building activity.

The empirical literature that exists on the economic development effects of Tax Increment Financing (TIF) broadly points toward a conclusion that it is not an effective economic redevelopment tool. This literature is less developed than the general property tax literature, and it seems reasonable to call for improvements in existing empirical work before making a final judgement on the general idea of TIFs. The literature on Business Improvement Districts (BIDs), although only relying on a few high-quality studies, suggests that they are more effective at improving local economies. Like the empirical work on the property tax, the empirical work on BIDs shows that public services are valuable to those that consume them, but when tax collection is out of line with service provision, adjustments are made in the form of mobility and price capitalization.

The most appropriate conclusion to draw from the empirical research on Spatially Targeted or Zone-Based tax concessions is that more research should be done. There are some very compelling studies that show large positive effects of Zone-Based programs on employment; however, there is also a series of fairly compelling studies that find null or negative employment effects. Some of these differences may be attributable to program differences, but some of them come from studies of the same program suggesting that positive results may not be robust. There is only a small literature examining firm specific subsidies, more empirical work is clearly needed to fully understand the effects of attracting large-scale employers using tax concessions on local economies. The work that does exist is at odds about findings, and about appropriate methodology. The empirical work on broader corporate tax policy suggests that increasing the corporate tax rate reduces employment and decreases business entry, while more targeted corporate tax policy increases investment.

Overall, it seems appropriate to suggest that switching between policies like TIFs and EZs that tend to erode the property tax base and into policies like broad corporate tax rate reductions and

investment incentives (but not firm specific targeting) would be a move in the direction toward improving state and local economic development outcomes.

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Appendix

Core Studies of Property Taxes and Economic Development Outcomes

<u>Study</u>	<u>Setting</u>	<u>Estimation Technique</u>	<u>Outcome(s)</u>	<u>Estimated Effect</u>	<u>Components</u>
Palmon and Smith (1998)	Municipal Utility Districts, Houston	Natural Experiment (exogenous variation in tax rates within identical public service district)	Property Value	56–64% Capitalization	
Bradbury, Mayer, and Case (2001)	Massachusetts Proposition 2.5	Natural Experiment	Property Values (through tax limit changes in public spending)	Tax limit induced spending increase of 8.6% leads to a 2% increase in house prices	Effects driven by school spending
Lang and Jian (2004)	Massachusetts Proposition 2.5	Natural Experiment	Property Values	Property tax increases caused property value increases, due to property tax levels being constrained below optimum	
Wasi and White (2005)	California Prop 13	Natural Experiment, Difference-in-Difference	Mobility	Moving triggered property tax increase reduces mobility by 6%. \$\$ of effective subsidy to stay in home correlated with cross-metro mobility rates.	
Ferreira (2010)	Modifications to California Prop 13	Natural Experiment, Regression Discontinuity	Household Mobility	Removing lock-in for 55+ owners increases mobility by 25%	
Shan (2010)	Various State Property Tax Relief Programs	Simulated Instrumental Variables	Elderly Mobility	\$100 increase in annual property tax associated with 8% increase in 2-year mobility rate of owners over 50.	Combined effect of homestead exemption, homestead credit, and circuit-breakers
Skidmore, Reese, Kang (2012)	Michigan Proposal A	Natural Experiment, Instrumental Variables	Tax Base	A 10% reduction in property taxes increases the tax base by 1.7% over three years.	

Johnson and Walsh (2013)	Michigan Proposal A	Natural Experiment	Vacation Home Density	3–4 mil decrease in property tax rates increases the number of vacation homes per square mile by 1	
Gallagher, Kurban, and Persky (2013)	Cook County, IL	Border discontinuity method	Property Value	97% capitalization	
Kang, Skidmore, Reese (2015)	Michigan Proposal A	Natural Experiment, Instrumental Variables	Residential and Business Property Values	A 10% reduction in property taxes increases residential property values by 4–6%. A 10% reduction in property taxes increases business property values by 9.8%	
Lutz (2015)	School Finance Reform, New Hampshire	Difference-in-Difference, intensity of tax reduction	Residential Construction, Property Value	-1 elasticity for residential construction in elastic supply area, 70–97% capitalization in inelastic supply area	Result driven by reduction in property tax burden
Bradley (2017)	Proposal A (capping property tax base increases, with Jan. 1 reset), Michigan	Instrumental Variables	Property Value	Temporary one-year tax savings capitalized as if it was permanent. Over capitalization.	Result driven by inattention or confusion of home buyers
Livy (2018)	Franklin County, OH	Border discontinuity method	Property Value	Full capitalization with a discount rate of 3.5%	

Core Studies of Spatially Targeted Tax Incentives and Economic Development Outcomes

<u>Study</u>	<u>Setting</u>	<u>Estimation Technique</u>	<u>Outcome(s)</u>	<u>Estimated Effect</u>
Billings (2009)	Colorado Enterprise Zones	Border Method	Business Location, Jobs	No Effect on Business Location, small employment effects at newer establishments in zone.
Elvery (2009)	California and Florida Enterprise Zones	Propensity Score	Employment	No effect on employment
Hanson (2009)	Empowerment Zones	Difference-in-Difference and Instrumental Variables	Employment, Poverty, Property Values	No effect on employment or poverty, increase in average property value by \$100K over a decade.
Neumark and Kolko (2010)	California Enterprise Zones	Difference-in-Difference	Employment, Number of Business Establishments	No effect on employment, possible negative effect on number of businesses.
Busso, Gregory, Kline (2012)	Empowerment Zones	Difference-in-Difference, Triple Difference	Employment, Wages	12–21 percent increase in total employment, 8–13 percent increase in wages for residents working in the zone.
Freedman (2013)	Texas Enterprise Zones	Regression Discontinuity	Employment	35–42 resident jobs during the time of the program (higher for workplace employment estimates).
Freedman (2015)	New Markets Tax Credit	Regression Discontinuity, Instrumental Variables	Employment by residence	Residential employment declines, but employment at workplaces in targeted areas increases.
Reynolds and Rohlin (2015)	Federal Empowerment Zone	Propensity Score and Differencing	Distribution of Household Income	Impoverished residents do not benefit, but areas become more attractive to higher income residents.

Core Studies of Firm Specific and Corporate Tax Incentives and Economic Development

<u>Study</u>	<u>Setting</u>	<u>Estimation Technique</u>	<u>Outcome(s)</u>	<u>Estimated Effect</u>	<u>Components</u>
House and Shapiro (2008)	U.S. Federal Corporate Tax, Bonus Depreciation	Natural Experiment	Investment	Elasticity between 6 and 14	Temporary nature of policy and model suggest temporary increase in investment
Greenstone, Moretti, Hornbeck (2010)	Relocation of Large Plants/Facilities	Difference-in-Difference and Triple Difference using "runner-up" comparison	Productivity of workers in near-by plants	New plants increase surrounding plant productivity by 12% five years after opening. Increase employment by 2.7%, number of plants by 5%.	Highly heterogenous effects: average driven by large effect in about 1/3 of sample, 2/3 has no discernable effect or negative effect.
Rosenthal, Rohlin, and Ross (2014)	U.S. State Corporate Tax	Border Method, Difference-in-Difference	Business opening	1 percentage point increase in the top marginal corporate rate reduces likelihood of business opening by 34 percent.	Larger effects for manufacturing and service businesses, smaller for retail.
Ljungqvist and Smolyansky (2014)	U.S. State Corporate Tax	Border Method, Difference-in-Difference	Employment, Income	1 percentage point increase in the top marginal corporate rate reduces employment by 0.3–0.5 percent and reduces income by 0.3–0.6 percent.	Reducing the corporate tax rate only effects employment and income during a recession.
Patrick (2016)	Relocation of Large Plants/Facilities	Difference-in-Difference and Triple Difference using geographic proximate comparison	Number of establishments	New plants increase surrounding plant output by 1–4%. New plants decrease the number of surrounding plants by 5.27%. New plants increase wages by 2.6–3.5%. New plants increase employment by 3–5%.	A comparison study of Greenstone, Moretti, Hornbeck that suggests those results are biased toward positive findings.
Zwick and Mahon (2017)	U.S. Corporate Tax, Bonus Depreciation	Difference-in-Difference	Investment	Investment increases by 10.4 percent between 2001 and 2004, and 16.9 percent between 2008 and 2010. Elasticity of 2.89.	Larger effect for smaller firms

Ohrn (2018)	U.S. Corporations, Domestic Production Activities Deduction	Difference-in- Difference	Investment	1 percentage point reduction in tax rate increases investment by 4.7 percent	\$1 spent on reducing corporate tax rate is nearly equivalent to \$1 spent on accelerated depreciation
Garrett, Ohrn, and Suarez Serrato (2019)	County-level employment resulting from bonus depreciation driven investment changes	Difference-in- Difference	Employment, Earnings	Moving from the 25 th to the 75 th percentile in exposure to bonus depreciation increased county employment by 1.9 percent. Implied cost per job of \$53,000. No effect on worker earnings.	Capital replaces workers after 5-year period