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## The Effectiveness of Enterprise Zones in Missouri

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### EXECUTIVE SUMMARY

Missouri made extensive use of the Enterprise Zone (EZ) tax credit program between 1982 and 2005. Businesses that located, expanded, or hired within certain areas designated as enterprise zones were eligible for state and local tax credits and abatements. Like many similar government economic development programs, EZs grew rapidly but lacked any measurement of their success or failure. The program was phased out and replaced in 2005, but measuring its success or failure is still a worthy goal because the lessons learned can be applied to current tax credit programs.

The purpose of this case study is to compare eight Missouri counties that employed extensive enterprise zones to 12 neighboring — and very similar — counties that did not establish any zones. The economic growth rates of five established economic measurements have been compared for the 20 counties over the period from 1980 to 2005. A review of the leading economic literature on the subject of enterprise zones is also included as part of the paper.

The goal of the paper is to attempt to find evidence that the adoption of an enterprise zone was associated with greater economic growth in the EZ counties. A regression analysis of the five economic

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measurements over 25 years in the 20 counties found no statistical evidence that adopting an EZ correlated with higher economic growth rates.

However, other evidence demonstrated limited success for the EZs. So, did EZs work or not? Did they increase economic growth within the zone? If so, did it lead to regional growth or was it at the expense of neighboring areas?

The evidence in this study is that these EZs did not succeed. This is not surprising. It simply follows the bulk of the economic literature on the subject of EZs and is further evidence that governments cannot see the future and are not gifted at planning an economy.

Those findings are highly relevant to current policy discussions in Missouri, despite the fact that the EZ program is being phased out and will no longer exist by 2015. Elected officials of both parties and government economic development officials throughout Missouri continue to believe that government involvement in the economy via tax credits or other subsidies is a good idea. Hopefully those officials and the public will consider the evidence that these types of programs do not work as advertised as new programs are considered or current ones are expanded. In particular, the creation of new Enhanced Enterprise Zones in Missouri — now more than 120 different zones — continues unabated despite a lack of evidence they are benefitting the state.<sup>1</sup>

## **INTRODUCTION**

There are a substantial number of government programs to stimulate economic investment in Missouri. There are 36 different state economic development tax credit programs, each with their own requirements and rules.

They range from large programs, such as the historic preservation tax credit and the Quality Jobs program, to the small, such as the state's film tax credit. There are at least half a dozen more state-authorized local tax incentive programs, such as Tax Increment Financing (TIF). Missouri, like many states, aggressively uses these programs to encourage investments the government deems desirable.

But do these programs work? Do they accomplish their various goals, which have many different angles but all fall eventually into the categories of economic growth and job creation? These programs may not be as intense as a Soviet Five-Year Plan, but they are centralized economic planning nonetheless. Any time the government takes tax dollars and directs them to other areas of a market economy, it is engaged in central planning. Some planning is essential, but has this type of economic planning benefitted our state or our local communities?

Measuring these programs can be difficult.<sup>2</sup> Many of the individual program applications are small, and properly collecting and comparing data is complicated (and sometimes impossible). Many of the goals that these programs attempt to achieve are hard to define. It is one thing to measure jobs, but another to measure the importance of preserving historic architecture. Even when economic development officials or urban planners do attempt to measure results, or, alternatively, predict results as a part of the application process, the quality of the analysis is usually atrocious.<sup>3</sup>

This paper is an attempt to add to the conversation on tax credits and incentives by measuring the results in Missouri of one specific program: Enterprise Zones. We are comparing the economic growth, as measured by a variety of factors, in



Missouri counties that adopted large enterprise zones and in bordering counties that adopted zero enterprise zones. We will attempt to find if there is any association between adopting an enterprise zone and improving economic growth as measured by reliable and commonly used statistics. We will attempt to avoid discussion of causation; we are simply trying to see if there is any correlation. It is our hypothesis that there will be no demonstrated and measureable improvements in economic statistics for the counties that implemented enterprise zones compared to neighboring counties that did not. If that is true — that counties that used enterprise zones did no better than counties that did not — we hope policymakers will take that into consideration as they consider future tax incentives and new programs.

Too often in debates about tax subsidies, the shoe is on the wrong foot. It should not be the job of opponents to prove something does not work. It should be the supporters' job to prove that it does (or, at least, is likely to) before tax dollars are committed.

## WHAT IS AN ENTERPRISE ZONE?

The concept of the Urban Enterprise Zone was promoted in the United States by Stuart Butler of the Heritage Foundation in the late 1970s. Urban Enterprise Zones (a.k.a. Enterprise Zones, or EZs) were proposed as a method of granting special tax incentives and deregulation for businesses that located in impoverished areas, especially inner cities. The hope was that this would improve the economic climate of distressed communities. While the federal government did not adopt EZs until 1993, many states, including Missouri, adopted them earlier. Missouri adopted state enterprise zone legislation

in 1982. From that point, the use of EZs in Missouri expanded until the Enhanced Enterprise Zone (EEZ) program replaced the EZ program in 2004.<sup>4</sup> The peak year for the program in fiscal terms was 1999, when it authorized and issued more than \$73 million in state tax credits.

The EZ program was phased out beginning in 2005, with the rule that only businesses which began operations and enrolled in the program prior to 2005 are eligible for the incentives. Although businesses enrolled in the incentive program before 2005 are still participating in it today, the number of participating businesses has declined sharply. For example, Enterprise Zone tax credit authorizations went from \$41.5 million in 2004 to \$10.3 million in 2009.<sup>5</sup> They declined further, to \$2.5 million, in 2011.<sup>6</sup> The last Enterprise Zone tax credits or abatements are scheduled to end in 2015, when the 10-year authorization for credits expires for the last of the businesses that enrolled in the program before 2005.<sup>7</sup>

The Enterprise Zone program allowed for businesses located within a designated zone that hired new workers or made significant investments to receive state and local tax credits. In order to be designated as an Enterprise Zone, a district would be drawn with a boundary that fit certain requirements for poverty rates, unemployment rates, etc. Once so designated, if a firm within the EZ hired at least two new people or made at least \$100,000 in investments, it could (and still may for some businesses enrolled prior to the phase-out) receive up to a 50 percent state income tax exemption, a 50 percent local property tax abatement,<sup>8</sup> and various tax credits depending on the type of person they hired. (Tax credits depended on if the person hired lived within the zone, if they had been

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unemployed, and more.)<sup>9</sup> Generally, businesses that enrolled in the program and were eligible for credits or abatements could receive them for a 10-year period.

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Missouri statutes limited the number of enterprise zones to 50.<sup>10</sup> However, at the program’s peak, there were actually 72 different EZs. Most of the EZs were smaller zones within rural areas. (This was in contrast to the original theory behind EZs, which focused on impoverished inner cities.)<sup>11</sup> Some were in large cities, and some took up almost entire counties. Missouri law prevented an entire county from being declared an EZ.

The unusual thing about EZs (and now EEZs) was their combination of state and local incentives. Most tax credits or subsidies are one or the other, but EZs can apply to both state income taxes and local property taxes. For this reason (among others) it would be difficult to directly compare EZs to other incentive programs.<sup>12</sup>

## **ECONOMIC LITERATURE REVIEW**

### **1. Analysis**

The economic literature examining enterprise zones has two main issues. The first focuses on the economic effects and the results are varied. The second issue is the difficulty with data collection, and as a result, many of the conclusions are cautious. Cross-state reviews of EZ are difficult because the rules, requirements, and incentives vary within states. Comparing EZs within the same state can be difficult because many of them

are small and data is difficult to assemble. The literature thus tends to break into two forms. First, there are very narrow micro analyses of specific EZs where the necessary data are available. Second, there are large macro reviews of EZs where cross-state differences are not considered. Our attempts to bridge these data and comparison problems in this study are explained in the next chapter.

One of the most well-known studies of EZs was a case study of Evansville, Ind. Rubin and Wilder concluded that the EZ established in Evansville in 1983 was an effective tool for job growth in that city.<sup>13</sup> The authors used shift-share economic analysis as the basis of this study. Shift-share analysis “is an analytic tool used to assess industry level data in a region and determine which areas are growing or declining relative to national averages.”<sup>14</sup> Shift-share is a good tool for determining what happened, but is a blunt instrument, at best, for trying to determine why something happened. Rubin and Wilder use shift-share analysis to conclude that the Evansville EZ was primarily responsible for a net increase of 1,430 jobs within the zone from 1983 to 1987. That number of jobs, 1,430, is substantial for a city such as Evansville, with a total metropolitan area employment of 123,000 in 1986.

Sheilah Watson used the same shift-share analysis to measure the results of two Missouri EZs.<sup>15</sup> Watson compared the Kansas City and Cuba EZs for job growth, relying on census data and other unbiased economic data. Watson chose the Kansas City and Cuba EZs precisely because they were among the few Missouri EZs with appropriate local data to conduct a study. Watson’s shift-share analysis concludes that the EZ in Kansas City led to a general estimated decline of 8,814 jobs, while the EZ in Cuba led to a net estimated increase of 451 jobs between 1980 and 1990.

Watson concluded that, overall, EZs had a positive impact on these communities.

Studies that have looked at the bigger picture of EZs have been far less favorable than those small case studies. Greenbaum used ZIP code-based business data from multiple states to review economic growth in enterprise zones.<sup>16</sup> He concluded that:

... zone incentives are generally not successful in raising levels of economic activity in zones above that which would have been expected had the zone policies not been implemented.

Papke, like Rubin and Wilder, reviewed enterprise zones in Indiana.<sup>17</sup> She found both positive and negative results in her analysis. She did conclude that the EZs reduced unemployment claims in the cities which implemented them to a statistically significant degree. She also stated that the incentives were “likely to increase zone investment.” However, she found no evidence that the benefits went to the residents of the EZ. Nor could she determine if the increased investment was new business to the zone or simply relocation of existing businesses to the zone with no net benefits to the larger region. Papke states in her conclusion:

Capital incentives may well revitalize economic activity in depressed areas, but it may well be at the expense of neighboring areas.

Gunn evaluated reasons for the substantial growth in state EZs despite “... scant evidence to support the ideologically-oriented enterprise zone policies.”<sup>18</sup> Her analysis attributes their growth to political and institutional factors because:

... the economic literature has been unable to provide support for the theories underlying enterprise zones.

Hoyt and Garen found that enterprise zones specifically “do not have a clear positive (or negative) impact on economic growth”<sup>19</sup> and state that “there is little evidence that EZ designation has any significant impact on employment in depressed regions.”<sup>20</sup>

However, Hoyt, Jepsen, and Troske, in an analysis of Kentucky counties, concluded that the use of state tax incentives in general did have a positive effect on employment within those counties. Interestingly, they found those benefits were higher in counties that bordered other states. The tax incentives had insignificant impacts within interior counties. The lesson is that a tax incentive may benefit a state or region if it causes a business to locate there instead of another state. However, those benefits are lost if one has only enticed a business to move one county over. One county’s gain is another county’s loss, and the net effect is zero.<sup>21</sup> This research, though, was on state tax incentives and did not include local enterprise zone measures in the methodology.

Landers questioned if, and how, EZ incentives were capitalized into property values, and if capitalization aided or impeded the success of the zones in Ohio.<sup>22</sup> He found significant property value capitalization in some of the zones he studied, but not in the majority of them. Landers also stated that creating too many EZs “will simply dilute that effectiveness of incentive packages offered in distressed EZs.”<sup>23</sup> He also determined that, while not statistically significant in many cases, property values in some EZs experienced a brief increase in value at the start of the EZ and then tailed off over time.

Kolko and Neumark studied California Enterprise Zones explicitly looking for evidence of employment growth within

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the zones. They determined that there was no statistically significant effect on employment and job creation within the zones. They further determined that there is no evidence that EZs shift employment toward low-wage industries that would presumably benefit low-wage workers. Finally, they present weak evidence that EZs may shift employment within a zone toward manufacturing, which is a stated goal of most EZ programs. In light of these findings that EZs have not led to job and employment growth in California, Kolko and Neumark recommended that California restructure or terminate the enterprise zone program.<sup>24</sup>

A Minnesota legislative review of enterprise zones summarized the literature on the issue the same way: “... most studies suggest no significant and prolonged increases in employment from enterprise zones.” That review found that several major studies had found some short-term employment increases, but that the bulk of the studies found no economic gains.<sup>25</sup>

Some of the most detailed work on this subject comes from Fisher and Peters (and some of the most enjoyable work comes from Netzer’s discussion of Fisher’s and Peters’ work).<sup>26</sup> Fisher and Peters write that tax incentives in general fail to live up to expectations. They note that economic literature has come to a general agreement that state and local taxes, and thereby incentives, do have some effect on economic growth. However, that effect is far smaller than people realize or admit, particularly those with an active interest in promoting programs. Fisher and Peters quote a consensus estimate taken from Bartik (without necessarily agreeing with the consensus) that state and local taxes and, therefore, economic incentives, have an elasticity between -0.2 and -0.3. That means that a 10 percent reduction

in business taxes (via cuts or incentives) “will increase the long-run level of local business activity by 2 or 3 percent.”<sup>27</sup> With that consensus estimate in mind, Fisher and Peters write:

But given a typical incentive package that represents about a 30 percent cut in state and local taxes, the new “consensus” elasticity implies that only about 1 in 10 new jobs in the average community will actually be attributable to the incentives, even if incentives are provided for all new jobs. Thus, the best case is that incentives work about 10 percent of the time and are simply a waste of money the other 90 percent.<sup>28</sup>

The authors then relate that, in their experience, “it is not unusual for public officials to attribute all new employment to incentive programs.”<sup>29</sup>

In specific relation to enterprise zones, Fisher and Peters note that despite an advantage, “in theory,” over other types of incentives (being specifically targeted to certain areas), EZs have “failed to promote economic growth.”<sup>30</sup>

Prior work by the same authors was slightly more neutral, while still stressing that tax incentives have a limited effect. To that point, they stress that the research indicates incentives are not generally enough to offset the basic systems local governments already have in place.

Locations that offer the highest returns without incentives are pretty much the locations with the highest returns after incentives are included.<sup>31</sup>

In short, having a quality overall environment for economic growth is more important than any incentives. Fisher and Peters nonetheless concluded in 1997 that



incentives offered in some places might “produce measureable gains in investment or employment.”<sup>32</sup> Their later work stepped back from this limited support for EZs. They later wrote that even if local areas saw economic growth from local tax incentives (including EZs), it would be likely that the new investment simply displaced other investments in the state or larger region and led to no net growth.

## 2. Commentary

Netzer’s work is the appropriate place to begin commentary on these papers. In his comments on Fisher’s and Peters’ work, he considers the literature on tax incentives and enterprise zones more generally. He attacks studies that use the survey method or rely on simplistic counts of incentive programs. He states the simple truth that often the mere creation of a program satisfies the primary purpose for the lobbyists, politicians, and economic development staff, and that the success or failure of the program means little to them. He emphasizes the factor substitution issue with incentives, where common EZ credits used to purchase machinery will likely reduce the need for employees, thus harming the entire goal of job creation.<sup>33</sup> He demolishes absurdly high elasticities for state economic development offices in some studies [not discussed here] by writing, “Who needs oil wells, when a state can be another Kuwait just by increasing the budget of a tiny agency?”<sup>34</sup> Finally, he criticizes the general quality of the economic work on enterprise zones.

Gunn’s statement that political and institutional factors advanced EZs more than actual data will be understood by anyone who has participated in the debate over tax incentives with supporters of their use. Public choice economics explains how political factors favor the

expansion and growth of enterprise zones and other tax subsidies more than their purported economic benefits. Politicians want to be seen as doing something. Enacting “pro-job” legislation such as tax credits or instituting enterprise zones is a low-risk decision. There are political gains and positive claims of growth to be made now. Future results are unlikely to matter, or even to be measured. This paper is being published in 2013 on decisions made in the 1980s. What politician or economic development officials will be harmed *in any way* if we find their choices in 1985 did not pan out as well as they promised? Will “Sorry, I was wrong about the enterprise zone” be a late addition to their tombstone?

While supporters of incentives and EZs widely cite the Rubin and Wilder study, it unfortunately relies on data compiled based on surveys by enterprise zone administrators. The authors state that they confirmed at least a portion of the survey data through later tax credit reports filed starting in 1987. However, if the low quality and high error rate of Missouri’s tax credit reports are any indication, then the confirmation for the Evansville data is hardly sufficient.<sup>35</sup> Data taken by or from active participants in the incentive program is questionable for a number of reasons. Zone managers trying to justify their job, or zone incentive participants hoping to receive additional incentives, have plenty of reasons to exaggerate data, attribute things to the data inaccurately, or claim motives that they know the government will want to hear. Simply put, there is an inherent bias to survey data. As Netzer said in his discussion of a review article on EZs:

I keep hoping that review articles will confine their reference to the survey approach to a dismissive endnote.<sup>36</sup>

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It is intriguing that the authors deemed the Evansville survey data for this 1989 study completely accurate, but when the Indiana Legislative Service Academy attempted to conduct a survey of businesses within Indiana EZs in 2003, the response rate was so low that a portion of the overall project was discontinued.<sup>37</sup> Peters and Fisher also questioned the validity of the shift-share methodology used in the Rubin and Wilder study and others “to identify induced jobs.”<sup>38</sup>

Watson’s results are puzzling. Despite noting the substantial job loss within the Kansas City EZ, she then reaches the implausible conclusion that, “Overall, this research provides evidence that beyond mere image or speculation the Kansas City and Cuba enterprise zones helped revitalize distressed urban and rural communities.” If an estimated decline of 8,000 jobs is a success, we wonder how bad it would have to be for Watson to declare it a failure.

In short, the problem is that Watson is willing to lay the failure of the Kansas City EZ on external factors. That may well be true; the simple creation of an EZ is not going to lead to 8,000 layoffs. But she is perfectly willing to ascribe the job creation in Cuba directly to the EZ, instead of also crediting outside factors in that case. Watson writes about the Kansas City EZ:

Overall, the zone seemingly was less effective in generating a competitive advantage in employment or in offsetting the impact of other internal forces.

And then:

... economic difficulties stem primarily from the enterprise zone area itself, which has suffered from outmigration, loss

of manufacturing business, and a declining economic base.

As we said, that is all perfectly reasonable. However, it is hard to square with her statement about Cuba’s EZ that:

...one can surmise that the activities of the enterprise zone in large part contributed to the economic growth of the area in sum.

Watson next lists the many attributes Cuba had at that time, including a stable employment base, an adequate labor supply, a new industrial park, a wide choice of locations in the EZ, quality infrastructure, and more. These things:

... enabled Cuba officials to develop a fairly successful enterprise zone project.

The question Watson begs here is how the many factors she lists — which are all undoubtedly favorable to economic growth — would have likely led to job creation and growth without any EZs. How Watson can explain away the EZ losses in Kansas City, but ascribe the growth in Cuba directly to the EZ, and conclude that overall the benefits are strong, is difficult to comprehend.<sup>39</sup>

The economic reasons that might explain the short-term gains from some EZ studies are clear enough. The incentives can shift intended investments from outside the zone into it, leading to an appearance of success but no real economic growth. The EZ can have its effectiveness limited after a short period when neighboring jurisdictions implement competing subsidies. Property tax abatements can lead to factor substitution by lowering the cost of capital and leading to an initial investment that is eventually offset by reduced need for labor.<sup>40</sup> Both the location of many zones in struggling areas and the existence of the



subsidy in the first place may encourage riskier investments that are less likely to succeed in the long run.

All these reasons for potential short-term gains pale when confronted with the long-term fact that the substantial majority of the economic research, according to Gunn, "...indicates that the central feature of zones — the use of tax incentives to attract businesses and create jobs — is not effective."<sup>41</sup> Finally, one lesson from the economic literature going forward for Missouri is the finding from some supporters of the enterprise zone concept that the number of zones authorized within a state should be small. The Heritage Foundation was one of the earliest supporters of the EZ concept and they explicitly stated that zone totals should be small and awarded competitively.<sup>42</sup> They recommended no more than 100 federal enterprise zones across the country for this reason. One study found that having a small number of zones correlates with better EZ performance and that having too many zones dilutes the impact.<sup>43</sup> Landers' study of Ohio zones reached a similar conclusion. Unfortunately, because every state's program is different, supporters of EZs have no exact number in mind as to which one is correct. For the sake of comparison, the Evansville, Ind., program that Rubin and Wilder studied and found to be successful was originally created in 1984 as one of just six in the state. By 2003, Indiana had only 28 EZs,<sup>44</sup> by which time Missouri had 72. This point is valuable as Missouri continues to expand its use of Enhanced Enterprise Zones (now more than 120) with no end in sight.<sup>45</sup>

It is fair to state that the bulk of the literature on tax incentives and enterprise zones does not support their effectiveness as tools for economic growth. The studies that find some success in incentives have

far less usefulness than politicians and economic development officials claim. Studies that find truly positive effects for enterprise zones are limited to a few specific case studies. Aside from the fact that any failed program could have a few positive examples, the use of shift-share analysis to determine there has been job growth in an economic sector is valid, but the use of it to attribute the growth to an enterprise zone is highly questionable.

Interestingly, one of Neumark's and Kolko's major recommendations for change was to alter the mapping of enterprise zones to align them with census tracts and other technical advancements to allow for better future measurements of their success or failure. This issue — the layout of the zones and how they align with economic data — is a problem anyone who has attempted to study enterprise zones has encountered, including the current authors. (Note: Missouri Department of Economic Development maps for the EEZ program are better.)

## ENTERPRISE ZONES COMPARISONS IN MISSOURI

For this study, we have formed an independent variable group of counties that implemented large Enterprise Zones between 1983 and 1992. We then created a controlled variable group of counties that bordered one or more of the counties in the first group, but which implemented zero EZs within the county. The counties were not selectively or randomly picked. For the first group (EZ counties), we included every county in Missouri that implemented a zone that made up greater than 50 percent of the county.<sup>46</sup> The EZ group ranges from zones that made up between 54 and 94 percent of the county. It is a baseline assumption of this

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study that if a county had greater than 50 percent of the entire county within an EZ, the economic results of that zone will be properly found by countywide economic statistics. In such large zones, the economic effects of the zone itself and the spillover effects of the EZ (if any, in both cases) to the rest of the county are expected to be measurable within countywide economic data.<sup>47</sup>

It is another assumption of this study that county boundary lines are highly permeable. It would not have been difficult for a business to move across county lines within Missouri if it felt incentives were beneficial or the economic environment in a particular county was harmful.

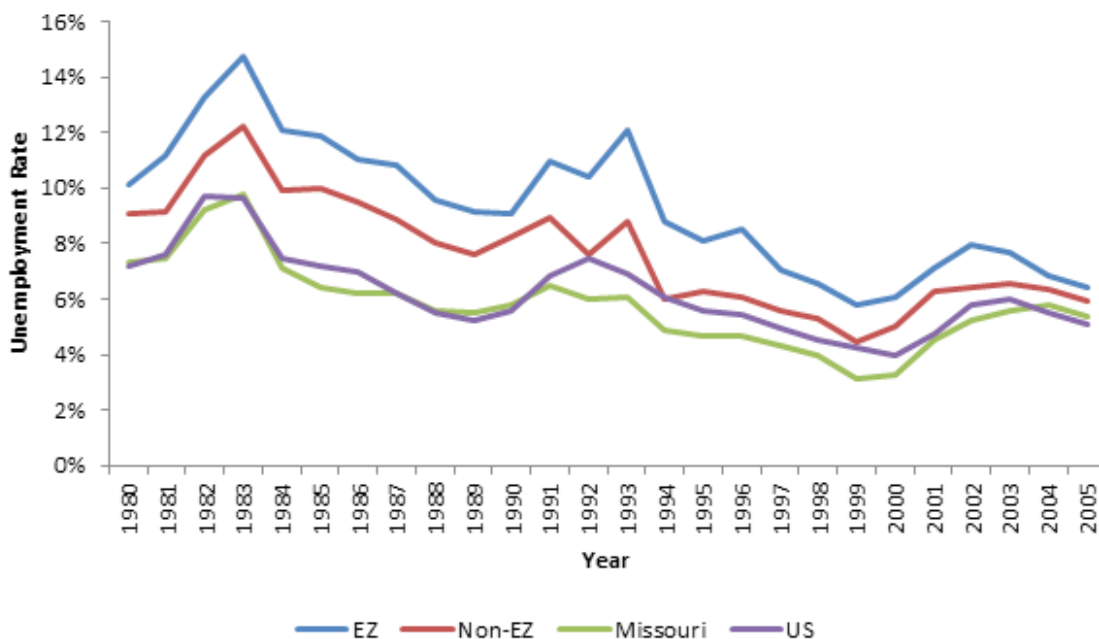
The types of economic statistics used here generally break down to the county level at their lowest baseline. It is not possible

to get this data beyond the county level, and it is not realistic to attempt to gauge how much of an effect a small EZ might have had on a much larger county. Many Missouri counties implemented small EZs during this period. Due to the small size of the zone, the limited data and mapping for them, and the inability to judge what affect a small EZ would have within countywide economic statistics, those counties have been ignored.

The second group of counties (non-EZ counties) includes every county that borders at least one of the EZ counties, never implemented any EZs, and is truly comparable to the eight counties in the EZ group. This group consists of 12 counties.<sup>48</sup> The 20 counties included here make a good fit for comparison. They are all small, rural Missouri counties in the southern half of the state.<sup>49</sup> (Franklin County is a partial exception here. It is

**GRAPH 1**

### Unemployment Rate for Average EZ and Non-EZ County by Year



bigger than the rest, but not so large as to be excluded from the comparison, in the opinion of the authors). Most of them are interior counties, with only Ozark and Howell bordering another state (Arkansas). While the counties in both groups are comparable in many ways (geography, culture, economy), they are, obviously, not exact copies of each other.

For the eight EZ counties, the average 1980 county population was 16,734. The average per-capita income was \$6,576, and the average county poverty rate was 19.23 percent.<sup>50</sup> The average unemployment rate was 10.11 percent.

For the 12 non-EZ counties, the average 1980 county population was 20,076. Their average per-capita income was slightly lower, at \$6,474, while the average poverty rate was also slightly lower, at 18.41 percent. The average unemployment rate was 9.05 percent. In summation, the counties used in this study are accurate comparisons to each other, and neither group was starting out in a significantly worse economic condition in 1980.

The unemployment rates in the counties that instituted enterprise zones were consistently higher than the counties without EZs (*see* Graph 1). This is not a surprise; higher-than-average unemployment rates were a requirement for creating the zones. Some economists have argued that tax incentives are properly aimed at addressing structural unemployment but not cyclical unemployment.<sup>51</sup> As the graph shows, the unemployment rates in both sets of counties tended to follow the typical national patterns, spiking in recessions and shrinking during economic expansions.<sup>52</sup> While the higher-than-state-average rates might indicate some level of structural unemployment problems in both sets of

counties, the close following of state and national unemployment trends indicates that unemployment in these 20 counties was primarily a cyclical issue and not a fundamentally structural one. As such, tax incentives were an inappropriate focus of dealing with unemployment.

The 20 counties and two groups are being compared on five different county-level economic measures. All five are standard, baseline economic measurements. Those measures are labor force, total employment, total personal income, per-capita income, and assessed valuation. All five measurements are compared from 1980 to 2005, except for assessed valuation, which is compared from 1985 to 2005.<sup>53</sup> We chose 1980 as the starting point because it allowed for accurate census data and allowed for several years of pre-zone data for the EZ counties. The data will be arranged and reviewed in a number of ways in the following section. The central purpose is to compare the economic growth rates in the EZ counties to the non-EZ counties, while taking appropriate outside factors into consideration.

## ENTERPRISE ZONE DATA AND ANALYSIS

We have chosen to include a variety of ways to review the economic data. These methods include the standard regression analysis of the data and other statistical tests, but also simpler reviews of average changes over time and the average changes before and after the enterprise zones were implemented. While those latter observations may seem superfluous in light of regression analysis, they are not. Each approach answers a slightly different question.

Economists generally do not make decisions to enact tax subsidies. (There

***[The measures used in the analysis] are labor force, total employment, total personal income, per-capita income, and assessed valuation.***



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would likely be far fewer of them if economists made the decisions.) Political actors usually make those choices and will generally use simpler data as a basis for their decisions. Responding to claims that Project X created 10 jobs with “the regressions don’t prove that” leads to the risk of people talking past each other. Our intention is to review the data in as many ways as possible that others might review that same data.

Some methods may show support for enterprise zones while others do not. If that happens, we relish the opportunity to ask why and attempt to explain it within a framework of data and the economic literature on the subject.

### 1. Cumulative Averages

We compare the five economic measurements in these counties from 1980 to 2005. (Assessed valuation is compared from 1985 to 2005.) The results are included in Table 1.

In all five categories, the 12 non-EZ counties outperformed the eight EZ counties. In four out of five categories, both the non-EZ and the EZ groups outperformed the state average, though in two categories the EZ and statewide differences were extremely small. Based

on the 1980 to 2005 averages taken as a whole, the counties that did not adopt EZs outperformed the counties that adopted EZs. This is not proof that EZs failed. It is simply a note that over the entire period of the program, by this measure and for these 20 counties, it does not appear that the counties that enacted EZ subsidies performed better than the counties that did not.

The treatment of establishing EZs within the variable group of counties shows no signs here of having succeeded in the goal of economic growth in comparison to the control group of counties.

### 2. Pre- and Post-Enterprise Zone Analysis

The averages over the entire length of the EZ program may mean little if it can be demonstrated that the counties that enacted EZs did better after the zone was implemented. Our eight enterprise zone counties instituted their EZs in different years between 1983 and 1992. For individual county analysis of economic statistics before and after EZ implementation, please see the county details in the Appendix. It takes time for an EZ to get going once implemented, so we have generally considered the year of implementation in the pre-EZ totals.<sup>54</sup>

**TABLE 1**

Economic Measurement	1980 EZ	1980 Non-EZ	2005 EZ	2005 Non-EZ	Average of EZ County Total Changes**	Average of Non-EZ County Total Changes**	Total Change of EZ County Total***	Total Change of NON-EZ County Total***
Labor Force	51,561	94,490	78,700	153,910	53.54%	58.92%	52.63%	62.88%
Personal Income	\$872,565	\$1,738,583	\$3,935,064	\$8,414,452	345.66%	378.35%	350.98%	383.98%
Per Capita Income	\$52,608	\$77,690	\$183,745	\$282,776	250.10%	267.18%	249.27%	263.98%
Total Employment	53,030	100,147	74,137	151,607	37.80%	45.85%	39.80%	51.38%
Assessed Valuation	\$733,281,248	\$1,201,622,310	\$1,694,895,697	\$3,429,974,722	99.41%	127.22%	131.14%	185.45%

\* 1980 columns reflect 1985 values; no 1980 data available

\*\* Averages all county total changes

\*\*\* Total change in data over the specified time period; same as total change of average of counties

**TABLE 2**

Average Yearly Change Before EZ	EZ Counties	Non-EZ Counties	Difference	Average Yearly Change After EZ	EZ Counties	Non-EZ Counties	Difference
Labor Force	1.47%	1.38%	0.09%	Labor Force	2.01%	1.70%	0.32%
Personal Income	8.52%	9.87%	-1.35%	Personal Income	9.30%	8.92%	0.38%
Per-Capita Income	7.65%	8.77%	-1.11%	Per-Capita Income	6.70%	6.85%	-0.14%
Total Employment	0.50%	1.34%	-0.85%	Total Employment	1.75%	1.30%	0.45%
Assessed Valuation	1.74%	2.31%	-0.57%	Assessed Valuation	7.13%	7.27%	-0.14%

Four counties implemented their zones in 1992. For those four, we have a basically equal number of years pre- and post-zone. For the two others that were implemented in 1985, we have a solid six years of data before the zone, but many more after it. For the two zones that were implemented in 1983 and 1984, we admit the time period pre-zone implementation is limited.

The results are included in Table 2.<sup>55</sup> There is no statistically significant difference in the performance of the counties before and after the implementation of the enterprise zones.

Upon a cursory review, the numbers appear to demonstrate improvements to the economies of enterprise zone counties after EZ implementation compared to non-EZ counties. EZ counties were growing more slowly than non-EZ counties in four out of five measures before implementation. After implementation, EZ counties grew more quickly in three out of five measures. This perhaps is not surprising, as at least some companies already looking to move or expand would have taken advantage of the EZ and moved from a non-EZ county to an EZ county. As Fisher and Peters noted, "...one would expect that enterprise zones would indeed draw jobs from elsewhere in the metro areas."<sup>56</sup>

The differences in growth rates post-EZ are very small, and may have come at the expense of neighboring counties. For example, total employment in Dallas County grew by an average of

0.74 percent per year before its EZ was implemented in 1992 and 3.67 percent after. It borders four counties that did not implement EZs, all of which saw their average rates of total employment growth decline after Dallas County instituted its EZ. It is highly likely (indeed, almost a certainty) that some of the growth that would have gone to those other counties instead went to Dallas County in search of tax subsidies. That may have been good for Dallas County politicians and some Dallas County residents, but it was not likely good for the multi-county regional economy. (In the majority of Missouri counties where the EZs were much smaller than the eight large zones studied here, it would be likely that the "growth" in the enterprise zone came from elsewhere in the same county.)

The idea that counties instituting an enterprise zone showed improvements after the EZ was enacted deserves closer analysis. We ran a statistical test on the pre- and post-enterprise zone differences. The changes in the data before and after enterprise zone implementation were not statistically significant. On all five indicators, the differences were small enough that they could have been caused by random chance.<sup>57</sup> To repeat, there is no measureable, statistically significant improvement in the counties that instituted enterprise zones after the EZ was enacted when compared to counties that did not create EZs.

***[T]here is no measureable, statistically significant improvement in the counties that instituted enterprise zones after the EZ was enacted when compared to counties that did not create EZs.***

TABLE 3

Average Yearly Change Before EZ	EZ Counties	Non-EZ Counties	Difference	Pre/Post Difference-in-Differences	Average Yearly Change After EZ
Labor Force	1.47%	1.38%	0.09%	0.22%	Labor Force
Personal Income	8.52%	9.87%	-1.35%	1.72%	Personal Income
Per-Capita Income	7.65%	8.77%	-1.11%	0.97%	Per-Capita Income
Total Employment	0.50%	1.34%	-0.85%	1.29%	Total Employment
Assessed Valuation	1.74%	2.31%	-0.57%	0.42%	Assessed Valuation

### 3. Difference-In-Differences

We used difference-in-differences estimation to measure what percent of the change in each economic measure might be related to the implementation of the enterprise zones, taking into account the changes within both comparison groups.<sup>58</sup> Please review Table 3 for the difference-in-differences results. The data refers to the difference in average annual growth between EZ and non-EZ counties.

This data provides limited evidence that the implementation of the EZs may have been beneficial to the counties that implemented them. However, the improvements to the EZ counties by this measurement are small and, as Papke admitted in her positive study of Indiana's EZs, gains from EZs in distressed communities may well come from neighboring areas.<sup>59</sup> Landers found short-term gains in property values within some Ohio EZs, but that effect decreased substantially over time. Landers wrote:

In addition, the tailing off of EZ property values over the life of the EZ may imply that once tax abatements and incentives have generated an initial surge of investment, possibly due to the novelty or uniqueness of a new approach...<sup>60</sup>

The results of the difference-in-differences analysis were not found to be statistically significant, with one exception.<sup>61</sup> The exception is assessed valuation.<sup>62</sup> The

result was statistically significant and showed positive gains associated with the enterprise zones. This is consistent with Landers' study that found lower property taxes were sometimes capitalized into enterprise zone commercial and industrial property valuations, partly negating the effects of the EZ. If a firm has to pay higher rents to be within a zone, that offsets part of the gains that may come from the EZ.

There is one small but important difference in Missouri rules for prior enterprise zones and the current enhanced enterprise zone program. The current EEZ program requires an official finding and designation of "blight" for EEZs that the EZ program did not have. It is a fair question to wonder how that designation of "blight" would affect property values within an EEZ in comparison to our findings within EZs.<sup>63</sup>

Claims of success by EZ supporters because of short-term gains like those small gains seen in the difference-in-differences measures will not likely stand strict scrutiny. They must be weighed against the lack of any evidence of long-term growth due to the EZ and the likelihood that much of the short-term gains came at the expense of other parts of the region. Unfortunately, many of the people (*i.e.*, politicians) making the final decisions on EZs have a short-term and entirely local focus.

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EZ Counties	Non-EZ Counties	Difference
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9.30%	8.92%	0.38%
6.70%	6.85%	-0.14%
1.75%	1.30%	0.45%
7.13%	7.27%	-0.14%

#### 4. Regression Analysis

The comprehensive review (1980 to 2005) of average growth shows little to no benefit for adopting a large EZ in a county. At the end of the EZ period, non-EZ counties were still doing better economically than EZ counties. Focused reviews of year-by-year comparisons after adopting the zones, such as the difference-in-differences review, show limited support for the idea that an EZ might have helped the county that adopted it. Which is correct?

In order to go further with the information, we ran a regression analysis of data included in this project. The regression measures the marginal impacts of the enterprise zones, while the prior methods focus on average changes. The regression includes 20 counties, five economic measurements, and 25 years of data for each measurement (20 for assessed valuation). The only two independent variables were whether or not a county established an enterprise zone, and what year it was established. Please see Table 4 (page 16) for all the results of the analysis.

The data was run as an ordinary least squares regression both with and without lagged variables. Our findings were consistent that there are no statistical differences in the growth rates between EZ and non-EZ counties. Using these more advanced statistical measurements,

there is no evidence that the presence of an enterprise zone led to economic improvements for EZ counties. In all but one measurement, the increases in economic totals for our five categories for the 20 counties were unrelated to the presence of an enterprise zone.<sup>64</sup>

This is perhaps the most important finding of the study. There is no statistical evidence that supports the idea that enterprise zones are a good investment of tax dollars. That is not to say they have failed, but we can find no real evidence that they succeeded. Simply put, the data does not support the use of enterprise zones.

#### CONCLUSION

Conflicting things emerge from reviewing the growth rates of these 20 comparable counties. First, the overall economic performance of the non-EZ counties was greater, on average, than the EZ counties. Second, there appeared to be a small increase in economic growth after counties adopted an EZ. Neither is surprising, and both are consistent with the literature. In general, enterprise zones and related programs (such as Enhanced Enterprise Zones) do not work, and, to the small extent that some of them appear to work, it is generally by relocating nearby businesses into the zone or by encouraging businesses that may have moved elsewhere in the region to stay within the zone. Those latter small successes may be fine for zone administrators or local political officials, but it has no net benefit to a regional economy.

More advanced analysis of the data finds no statistically significant effect on the economic growth rates of the counties that adopted an EZ compared to counties that did not. There is no hard evidence that EZs work as interpreted through the findings of this data. Not surprisingly, evidence that they work is

***Unfortunately, evidence that they work is too often missing from the discussion about implementing modern tax incentive programs such as TIFs or EEZs.***

***If there was ever a time for EZs or EEZs to have a positive impact, Missouri has long passed it. Cities and counties should cease establishing such zones.***

too often missing from the discussion about implementing modern tax incentive programs such as TIFs or EEZs.

Unfortunately, the effect from EZs on the larger regional economies of these counties from 1980 to 2005 is beyond the capacity of this paper to determine. Every economist who has studied the issue of enterprise zones has discussed the problems of mapping and data. It would be impossible to attempt to include the other counties

within these regions which had small enterprise zones combined with the lack of defined regions for the economic data in the first place.<sup>65</sup> We simply restate that there is no mathematic evidence that enterprise zones are a good use of tax dollars. The data analysis herein supports that fact and is consistent with the bulk of the economic literature on the subject.

The literature also states that, if you are going to enact special zones such as EZs,

**TABLE 4**

**Regression Results of Equation 1: Estimate of the Relationship Between an EZ and 5 Economic Factors**

	Labor Force	Personal Income	Per Capita Income	Total Employment	Assessed Valuation
<b>EZ Existence (1=yes)</b>	-312.507 (857.680)	8,258.063*** (39,843.85)	3,031.781 (510.884)	-623.630 893.003	-1.68e+07 (1.96e+07)
<b>Constant</b>	9,357.917*** (447.063)	329,488.9*** (20,226.09)	13,274.7*** (259.342)	9,618.316*** (453.319)	1.60e+08*** (1.11e+07)
<b>Number of Observations</b>	520	520	520	520	420
<b>R-Squared</b>	.0003	.0001	.06	.0009	.0018

**Regression Results of Equation 2: Estimate of the Relationship Between an EZ and 5 Economic Factors**

	Labor Force	Personal Income	Per Capita Income	Total Employment	Assessed Valuation
<b>1-year lag</b>	1.012*** (.0718)	.982*** (.075)	.895*** (.073)	1.228*** (.070)	1.064*** (.100)
<b>2-year lag</b>	-.002 (.073)	.076 (.080)	.148* (.077)	-.215*** (.072)	.008 (.106)
<b>EZ Existence (1=yes)</b>	2.535 (73.485)	197.630 (1656.381)	-30.196 (84.856)	28.457 (42.362)	2,138,024 (1,860,449)
<b>Constant</b>	67.989 (76.438)	1135.008 (1318.838)	180.700* (94.448)	-37.690 (42.074)	-5,001,137** (,1986,309)
<b>Number of Observations</b>	192	192	192	192	152
<b>R-Squared</b>	.987	.997	.993	.997	.986

it is harmful to enact too many of them. The limited benefits they may have, in theory, for certain areas are reduced further — or, more likely, eliminated completely — if everybody has one. Then, an EZ or EEZ is nothing more than a tax giveaway for any business willing to pay lawyers and accountants to file the required paperwork. There are now more than 120 EEZs in Missouri; more than one per county. As this case study goes to publication, at least nine other zones are in various stages of the implementation process.<sup>66</sup>

If there was ever a time for EZs or EEZs to have a positive impact, Missouri has long passed it. Cities and counties should cease establishing such zones.

Despite the appearance of small improvement in economic measurements in some of the EZ counties studied in this paper, the fact remains that the counties that did not enact EZs still performed just as well as those that implemented them. The cumulative measures from 1980 to 2005 are clear that counties without EZs had growth rates closely comparable or better than the counties with EZs.

For the measurements of pre- and post-EZ measurements and difference-in-differences, the cursory improvements of the EZ counties over the non-EZ counties do not stand up to tests of statistical significance. **Most importantly, the regression analysis demonstrated that there is no correlation between these economic growth measures and the inclusion of an enterprise zone in these 20 counties.** The regression documents that the EZs had no marginal impact on those counties.

In our opinion, there is no evidence enterprise zones grew the economy in

Missouri. They may offer gains to some people, such as economic development officials and politicians, for whom the act of implementing a program — any program — has benefits. But on the whole, and consistent with the wider economic literature, we do not find evidence that counties which implemented enterprise zones had greater economic growth than counties that did not.

It is our hope that future attempts to enact new tax subsidies or expand current ones will be required to provide legitimate economic analysis that those programs will succeed before tax dollars are committed.

Hand in hand with that evidence must be new ways to account for the use of the tax subsidies and better ways to measure their effectiveness. Imprecise accounting methods and unwieldy data collection systems (*e.g.*, enterprise zone boundaries that do not match up to any other maps) prevent legitimate measurements of the success or failure of many Missouri tax subsidy programs. It is not beyond the pale for us to wonder if that might be intentional.

Missouri Gov. Jay Nixon recently reconvened a state tax credit review commission that previously recommended positive changes in state tax credit policy. Hopefully, the findings in this paper will be valuable to the Missouri General Assembly as it considers the recommendations of that commission. The much-needed changes the commission has recommended in its two reports would tighten and reduce the use of tax credits in our state and would benefit Missouri. Enterprise Zones did not benefit the counties that enacted them. The same goes for government planning of Missouri's economy in general.

***It is our hope that future attempts to enact new tax subsidies or expand current ones will be required to provide legitimate economic analysis that those programs will succeed before tax dollars are committed.***



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The Appendix is listed at [SMIinfo.org/EZappendix](http://SMIinfo.org/EZappendix).

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## SOURCES

The data source for Personal Income, Per-Capita Income, and Total Employment is the U.S. Bureau of Economic Analysis.

The source for Labor Force is the Economic & Policy Analysis Research Center at the University of Missouri-Columbia.

The source for Assessed Valuation is the Missouri State Tax Commission.

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## NOTES

<sup>1</sup> Two recent EEZs were enacted in Boone County cities: Centralia and Columbia. At the time of publication, Fulton, Mo., in Callaway County, is debating implementing an EEZ, as are seven other Missouri cities and counties. Columbia is already considering removing its new EEZ. EEZ boundary data released in 2012 by the DED mapped out 122 Missouri EEZs but did not include the Columbia EEZ, and may have missed other recent zones.

<sup>2</sup> So difficult, in fact, that the Missouri Department of Economic Development often makes no attempt to measure the various programs.

<sup>3</sup> To give one example, cost-benefit analyses prepared for TIF projects generally consider all sales at a new store to be “new” sales and “new” tax revenue and make no attempt to measure that many of those same sales are lost to nearby stores.

<sup>4</sup> The EZ and EEZ programs are similar. For detailed differences, visit the Missouri DED website: <http://www.ded.mo.gov/Programs.aspx>.

<sup>5</sup> Missouri State Audit Report No. 2010 – 106, “Enterprise Zone and Enhanced Enterprise Zone Tax Credit Programs.” September 2010, p. 14.

<sup>6</sup> Research by Michael Rathbone of the Show-Me Institute.

<sup>7</sup> RSMo 135.230. Of course, like many statutes, there are exemptions upon exemptions to the rules and the author will not be surprised if a few businesses manage to remain in the program after 2015.

<sup>8</sup> A property tax abatement should not affect the assessed valuation (one of our economic growth measures) of the property being abated.

<sup>9</sup> The exact rules for Enterprise Zones can be found in Revised Missouri Statutes Chapter 135, or on the Missouri Department of Economic Development website: <http://ded.mo.gov/BCS%20Programs/BCSProgramDetails.aspx?BCSProgramID=41>.

<sup>10</sup> RSMo 135.210.

<sup>11</sup> Gunn, Elizabeth. “The Growth of Enterprise Zones: A Policy Transformation.” *Policy Studies Journal*, Vol. 21, No. 3, 1993, p. 437.

<sup>12</sup> A few TIF districts involve state funds along with local property taxes, but the large majority of TIFs only involve local taxes. The current EEZ program, like its predecessor, involves state and local tax incentives.

<sup>13</sup> Rubin, Barry, and Margaret Wilder. “Urban Enterprise Zones: Employment Impacts and Fiscal Incentives.” *Journal of the American Planning Association*, 1989, 55:4, pp. 418-431.

<sup>14</sup> Definition from Wayne State University Center for Urban Studies: <http://econdev.cus.wayne.edu/Menu/Calculators.aspx>.

<sup>15</sup> Watson, Sheilah. “Using Public-Private Partnerships to Develop Local Economies: An Analysis of Two Missouri Enterprise Zones.” *Policy Studies Journal*, Vol. 23, No. 4, 1995.

<sup>16</sup> Greenbaum, Robert. “An Evaluation of State Enterprise Zone Policies: Measuring the Impact on Business Decisions and Housing Market Outcomes: Dissertation Summary.” W.E. Upjohn Institute for Employment Research, 1999.

<sup>17</sup> Papke, Leslie. “What Do We Know About Enterprise Zones?” National Bureau of Economic Research, *Tax Policy and the Economy*, Vol. 7, January 1993, pp. 37-72.

<sup>18</sup> Gunn, Elizabeth. “The Growth of Enterprise Zones: A Policy Transformation.” *Policy Studies Journal*, Vol. 21, No. 3, 1993, pp. 432-449.

<sup>19</sup> Hoyt, Jepsen, Troske, p. 6. (see endnote 21).

<sup>20</sup> Hoyt, William H., and John E. Garen. “Fiscal Policy and Economic Development.” National Center for Real Estate Research, State and Local Fiscal Research Institute, November 2006, p. 44.

<sup>21</sup> Hoyt, William, Christopher Jepsen, and Kenneth Troske. “Business Incentives and Employment: What Incentives Work and Where?” IFIR Working Paper No. 2009-02, Institute for Federalism and Intergovernmental Relations, November 2008. The Kentucky Enterprise Zone Program (KEZA) is not listed on page 8 as one of the incentives they studied.

<sup>22</sup> Landers, Jim. "Why Don't Enterprise Zones Work? Estimates of the Extent that EZ Benefits are Capitalized into Property Values." *The Journal of Regional Analysis and Policy*, Vol. 36, No. 1, pages 15-30.

<sup>23</sup> Landers, p. 25.

<sup>24</sup> Kolko, Jed, and David Neumark. "Do California's Enterprise Zones Create Jobs?" Public Policy Institute of California, 2009.

<sup>25</sup> Hirasuna, Don, and Joel Michael. Minnesota House of Representatives Research Department, "Enterprise Zones: A Review of the Economic Theory and Empirical Evidence." January 2005.

<sup>26</sup> Peters, Alan, and Peter Fisher. "Tax and Spending Incentives and Enterprise Zones." *New England Economic Review*, March/April 1997, pp. 109-137.

<sup>27</sup> Bartik, Timothy J. "Solving the Problems of Economic Development Incentives." In *Reining in the Competition for Capital*, Ann Markusen, ed. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research, 2007. p. 106.

<sup>28</sup> Fisher and Peters. "The Failures of Economic Development Incentives." p. 32.

<sup>29</sup> Ibid, p. 32. (The author of this case study concurs with that experience.)

<sup>30</sup> Ibid, p. 31.

<sup>31</sup> Fisher and Peters. "Tax and Spending Incentives and Enterprise Zones," p. 128.

<sup>32</sup> Ibid. p. 129.

<sup>33</sup> Missouri's EZ program included credits for plant investment. To claim the credit, you had to create new jobs and invest in plant or equipment facilities. It was certainly possible for the employment increases to be temporary while the capital investment had a longer term effect of reducing needs for workers.

<sup>34</sup> Netzer, Dick, discussion of Fisher and Peters. "Tax and Spending Incentives and Enterprise Zones" p. 134.

<sup>35</sup> "Auditor Slams Jobs Program's Claims." *Columbia Daily-Tribune*, July 3, 2012. Online here: <http://www.columbiatribune.com/news/2012/jul/03/auditor-slams-job-programs-claims/>.

<sup>36</sup> Netzer, Dick, discussion of Fisher and Peters. "Tax and Spending Incentives and Enterprise Zones." p. 134.

<sup>37</sup> Indiana Enterprise Zone Fiscal Impact

Project, Indiana Legislative Services Agency, 2005, p. 3. Online here: [http://www.in.gov/legislative/pdf/INDIANA\\_ENTERPRISE\\_ZONE\\_FISCAL\\_IMPACT\\_PROJECT.PDF](http://www.in.gov/legislative/pdf/INDIANA_ENTERPRISE_ZONE_FISCAL_IMPACT_PROJECT.PDF).

<sup>38</sup> Peters, Alan, and Peter Fisher. "The Failures of Economic Development Incentives," *Journal of the American Planning Association*, Vol. 70, No. 1, 2004, p. 34.

<sup>39</sup> Watson notes, for example, that Cuba is large and, at the time, the new industrial park was open and being actively promoted before the implementation of the EZ.

<sup>40</sup> By offset, we are referring to offsetting the stated enterprise zone goal of job growth.

<sup>41</sup> Gunn, p. 432.

<sup>42</sup> The Heritage Foundation, "New Life for Federal Enterprise Zone Legislation: Seven Lessons from the States." *Backgrounder* #833, June, 1991, p. 12.

<sup>43</sup> Erickson, Rodney, and Susan Friedman. "Comparative Dimensions of Zone Policies." *Enterprise Zones: New Directions in Economic Development*, ed. Roy Green (Sage Publications, Inc.; Newbury Park, Calif., 1991), pp. 173-174.

<sup>44</sup> Landers, Jim, and Dagny Faulk. "In The Zone: A Look At Indiana's Enterprise Zones." *Indiana Business Review*, Vol. 80, No. 2, Summer 2005. Online here: <http://www.ibrc.indiana.edu/ibr/2005/summer/article3.html>.

<sup>45</sup> There is no limit on the number of Enhanced Enterprise Zones. Even if there were a limit, it would be meaningless as the legislature quickly exceeded the previous limit. There is a cap on the amount of total state tax credits each year — \$24 million. There is no cap on the amount of local tax abatements in the program.

<sup>46</sup> This came to eight counties: Morgan, Howell, Dallas, Texas, Iron, Madison, Crawford (which included the Cuba EZ discussed in section three), and Washington. We were unable to obtain maps for a few of the 72 EZs, so it is possible that we missed a county that should have been included in this group. Shannon County was not included because its EZ only passed 50 percent after a large zone expansion in 1999,

well after the initial zone creation. Other counties (Texas) with EZ expansions were included in the group because the expansion occurred quickly after the initial implementation. The mapping was also an approximation. We applied old EZ maps from the 1980s to GIS software using 2010 census tracts. Other counties that were considered but excluded were Dent (just less than 50 percent), Maries (right at 50 percent), and Gasconade (just more than 50 percent but largest city [Herman] entirely outside of zone). In the case of Gasconade, the assumption that the spillover effects of the EZ can be found in county statistics breaks down if the largest city and economic engine of the county is entirely outside of the zone. Also, the maps for Maries and Gasconade counties were especially unclear and estimating the boundaries and percent of the county was very difficult.

<sup>47</sup> Papke made a similar decision when deciding that she would apply unemployment data from an entire Indiana city to EZs that only made up a portion of the city.

<sup>48</sup> The 12 counties are: Cooper, Benton, Hickory, Polk, Webster, Ozark, Pulaski, Carter, Reynolds, Franklin, Laclede, and Bollinger. One county (Carter) was included even though it does not border any EZ counties. While it may not border any, it is very close to several and directly comparable to many. Only one county was excluded solely on not being comparable. Jefferson County borders an EZ county, and had no EZs itself, but it is too large a county to fairly compare to the others.

<sup>49</sup> The fact that they are in the southern half of the state is important. The geography of the state is different north and south of the Missouri River.

<sup>50</sup> The poverty rates used here are from 1979.

<sup>51</sup> Wall, Howard. "Tax Credits as a Tool of State Economic Development Policy." Show-Me Institute Policy Study No. 30, October 2011, p. 8. It is unknown whether the higher unemployment rates within the EZ counties were used to justify the establishment of the EZs, though it is likely.

<sup>52</sup> Due to the heavily national factors influencing unemployment rates, it was not selected as one of our primary areas of measurement.

<sup>53</sup> Missouri changed the way property

assessment was done in 1985, so data before that is not comparable.

<sup>54</sup> The two exceptions are Iron and Madison, which implemented their EZs in January of 1992. For those two, 1992 is considered post-EZ.

<sup>55</sup> Each EZ county has been assigned at least two directly comparable counties that they border, or, if they do not border, two counties they are very close to. The average non-EZ totals here include the data from that county before and after its comparable EZ county instituted a zone. In cases where one non-EZ county was a directly comparable county to multiple EZ counties, such as Franklin bordering both Crawford and Washington, the different dates of the zone implementation affected which dates were used for Franklin County statistics. Because one county could be a direct comparison to multiple counties, there is some necessary double-counting of non-EZ county data here.

<sup>56</sup> Fisher and Peters. "Tax and Spending Incentives and Enterprise Zones." p. 127.

<sup>57</sup> The test applied on all five indicators was

a "T" test.

<sup>58</sup> Difference-in-difference equation:  $(EZCountyAfterEZ - EZCountyBeforeEZ) - (NonEZCountyAfter - NonEZCountyBefore)$ .

<sup>59</sup> Papke, p. 62.

<sup>60</sup> Landers, p. 25.

<sup>61</sup> Personal income was significant at the 10 percent level, but only assessed valuation was significant below the 5 percent level.

<sup>62</sup> The date for assessed valuation does not include pre-1985, so it is limited for the full group of EZ counties. There are no pre-years for two of the EZ counties, and very limited data for two others. Nonetheless, we used all the information we had at our disposal for these results. We also ran the assessed valuation tests for the four EZ counties that instituted the zones in 1992. For that subgroup, there was no statistical significance for any assessed valuation changes in either pre- or post-data or the difference-in-difference tests.

<sup>63</sup> It is important to note that Landers' study only focused on commercial property, while our study includes all types of property. Residential property is included within Missouri zones. Breaking down the Missouri county data to commercial property alone for this study is impossible due to data and source limitations.

<sup>64</sup> The data result for per-capita income in the regression not using lagged variables was found to be statistically significant in favor of EZ counties. However, the  $r^2$  for this factor is also small, at .06. That means that, while measurable, the presence of an EZ would be responsible for, at most, 6 percent of any improvements in per-capita income within the EZ counties.

<sup>65</sup> Unlike, for example, Tennessee, there is no widely accepted and legal definition of what counties are in what regions in Missouri.

<sup>66</sup> E-mail from Missouri Department of Economic Development to author on Dec. 21, 2012.



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