7-1-2018

"But For" Percentages for Economic Development Incentives: What Percentage Estimates are Plausible Based on the Research Literature?

Timothy J. Bartik
W.E. Upjohn Institute for Employment Research, bartik@upjohn.org

Upjohn Institute working paper ; 18-289

Timothy J. Bartik, *W.E. Upjohn Institute for Employment Research*

Upjohn Author(s) ORCID Identifier

https://orcid.org/0000-0002-6238-8181

W.E. UPJOHN INSTITUTE
FOR EMPLOYMENT RESEARCH
Researching the causes and consequences of unemployment

"But For" Percentages for Economic Development Incentives: What percentage estimates are plausible based on the research literature?

Upjohn Institute Working Paper 18-289

Timothy J. Bartik
W.E. Upjohn Institute for Employment Research
email: bartik@upjohn.org

July 2018

ABSTRACT

This paper reviews the research literature in the United States on effects of state and local "economic development incentives." Such incentives are tax breaks or grants, provided by state or local governments to individual firms, that are intended to affect firms' decisions about business location, expansion, or job retention. Incentives' benefits versus costs depend greatly on what percentage of incented firms would not have made a particular location/expansion/retention decision "but for" the incentive. Based on a review of 34 estimates of "but for" percentages, from 30 different studies, this paper 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. In other words, for at least 75 percent of incented firms, the firm would have made a similar decision location/expansion/retention decision without the incentive. Many of the current incentive studies are positively biased toward overestimating the "but for" percentage. Better estimates of "but for" percentages depend on developing data that quantitatively measure diverse changes in incentive policies across comparable areas.

JEL Classification Codes: R30, R12, H71

Key Words: Tax incentives, business location decisions, local economic development

Acknowledgments:
I thank Evan Mast, Ellen Harpel, Ken Poole, and Jim Robey for helpful discussions. I thank Nathan Sotherland for research assistance and Claire Black for general assistance. This research was in part funded by the Michigan Economic Development Corporation (MEDC); however, neither the design of this research review nor the findings had any involvement by MEDC. The findings of this research review are those of the author and should not be construed as reflecting views of MEDC or the Upjohn Institute.

This paper reviews the empirical research literature in the United States on the effects of economic development incentives.[1] Such incentives are provided by state and local governments to business firms, to influence these firms' location decisions, expansion decisions, or job retention decisions. Incentives are mostly business tax breaks, but sometimes are grants.

The paper focuses on the following question: what does empirical research suggest about these incentives' effects on the probability of firms making a location, expansion, or retention decision in favor of a particular state or local area?[2] If incentives are provided by a state or local government to a firm, and the firm ends up choosing that state or local area, in what percentage of those cases would that decision not have been made "but for" the incentive?

This review of the research literature can derive 34 estimates of "but for" percentages from 30 studies. The most common type of study compares incented firms and unincented firms within a single state. Other studies compare counties that differ in incentive usage within a single state. Still other studies are based on surveys that ask firms or economic development experts to estimate "but for" percentages. A final group of studies uses data from multiple states and compares the economic performance of states with different incentives.

The paper discusses various studies' likely biases. As explored further below, many incentive studies are likely to be positively biased: they probably overstate incentives' "but for" effect. Other studies are negatively biased. Even these biased studies help bound the plausible

[1] This review focuses on incentives that are not strongly targeted on economically distressed areas. Thus, I exclude most enterprise zone studies. (Some supposed enterprise zone programs, such as JobZ in Minnesota, really are more general incentives.) Such enterprise zone studies have found little if any effect of enterprise zone incentives (Bartik 2010). This might be attributable to the incentive not outweighing these distressed areas' problems, and so does not yield generalizable conclusions for all incentives. In addition, enterprise zones are often too small to be local economies. Effects for such small geographic areas may not be generalizable to larger areas such as states.

[2] Buss's (2001) prior incentive review mainly focused on the tax literature.

range of incentive effects. However, a minority of studies are less likely to be biased and provide more plausible estimates of incentive effects.

I conclude that a plausible range for incentives' "but for" percentage is 2 percent to 25 percent. For a typical state and local incentive package, in only 2 percent to 25 percent of the incented projects is the incentive decisive in tipping a location, expansion, or job retention decision towards that state or local area. In the other 75 percent to 98 percent of the time, the same decision would have been made without the incentive.

Based on prior studies of incentives' benefits and costs, this range of "but for" percentages will often lead to benefits and costs that are closely balanced (Bartik 2018). Whether incentives have benefits greater than costs will depend on many details about the incented project, the state economy, and the incentive package's design. For example, with a "but for" percentage of 12 percent, a job multiplier of 6 for the incented jobs may imply a ratio of local incentive benefits to costs of 4.0, but a multiplier of 1.5 may imply a benefit-cost ratio of 0.4 (Bartik 2018, Table 8, p. 58).

The plan of the paper is as follows: I first review the biases in incentive studies from different methodologies. I then present a summary of what these 30 studies show about incentive effects. I highlight the results from a few of the better studies, while more details on other studies are in an appendix. I discuss how these results compare with likely incentive effects based on other research, such as research on how state and local business taxes affect state and local economic development. Finally, I discuss why estimated effects of incentives are so modest. Why do incentives tip far less than half of all decisions about location/expansion/retention?

METHODOLOGIES OF VARIOUS INCENTIVE STUDIES, AND POSSIBLE BIASES[3]

What methodologies do various incentive studies use, and how might that bias their resulting estimates of the "but for" percentage? Among the 34 estimates reviewed in this paper, I reach the following conclusions about their methodologies' likely biases: 23 estimates are likely positively biased—these estimates tend to overstate the "but for" percentage; 4 estimates are likely negatively biased—these estimates tend to understate the "but for" percentage; and 7 estimates are based on methodologies that do not imply an obvious bias in either direction.

As mentioned, the most common study compares the economic performance of incented firms, versus similar unincented firms, within the same state. This methodology is likely to lead to positive biases. Both incented and unincented firms are typically eligible for the same incentives. Because incentives are often tied to a firm making a location or expansion decision, the firms selected for incentives are more likely growing firms. This bias can be described as a "selection bias" or "reverse causation bias." Firms that have a greater propensity to expand are more likely to succeed in being selected for an incentive. Without further analysis, it is impossible to tell whether any positive correlation between firm growth and incentive usage is because the incentive usage caused the growth, or because firm growth caused incentive usage.

Another common methodology compares, within the same state, counties that differ in incentive usage. In most of these studies, all counties have similar eligibility for incentives. This methodology is also likely to lead to a positive bias, overstating the positive effects of incentives on county economic performance. Because incentives are disproportionately awarded to growing

[3] In addition to the incentive studies used here, which provide estimates of incentive effects that can be used to derive "but for" percentage estimates, there are numerous studies that assume various "but for" percentages and use those estimates to derive estimates of incentives' effects. These include Bartik (2018); Minnesota Legislative Auditor (2017); Washington JLARC (2017); Maryland Legislative Services (2016); Mississippi Institutions of Higher Learning (2016); Bartik and Erickcek (2014); Connecticut DECD (2014); Sallee et al. (2012); Luger and Bae (2005).

and locating firms, counties that would be growing fast for any reason would be more likely to receive more incentives, even if incentives had no true effect.

Some studies rely on surveys of either incented firms or economic developers. Such surveys typically ask the incented firms or the developers to provide an opinion on what would have happened without the incentive. Such surveys are likely to overstate incentives' positive effects. Firms have some reason to claim that incentives were decisive, even if the incentive program does not legally require such a claim. A firm could face political criticism if it was revealed that an incentive it received was unnecessary. Furthermore, a firm that once received benefits from an incentive program may want the program to continue. The firm knows that program continuation is more likely if the firm claims the incentive was decisive in encouraging a location or expansion decision. For surveys of economic developers, the developers may also want the program to continue. In addition, any professional wants to believe that their work is effective.

Studies can be negatively biased if their procedure for comparing counties or firms with greater incentive usage, versus other counties or firms, tends to result in "negative selection" of counties or firms with greater incentives from among all counties or firms that are considered. By negatively selected, I mean that the counties or firms that receive more incentives, compared to the overall sample of counties or firms that are considered, tend to select counties or firms that would be slower growing for reasons unrelated to incentives. Four studies are identified that may have such a negative selection bias. Three studies appear to suffer from such a negative bias in selecting counties: LaFaive and Hicks (2018); Whitacre, Shidelar, and Williams (2016); Hicks and LaFaive (2011). One study appears to suffer from a negative bias due to its process of selecting firms: Gabe and Kraybill (2002).

LaFaive and Hicks (2018) estimate the effect on a Michigan county's job growth of past use of Michigan Business Development Program (MBDP) incentives, controlling for past county job growth. But counties in which a greater proportion of past job growth is associated with MBDP incentives, and is not occurring on its own, would be expected to grow more slowly in the future, even if the incentives had a positive effect.

Whitacre, Shidelar, and Williams (2016) compare manufacturing job growth in counties with more participation in Oklahoma's Quality Jobs incentive program, versus other similar counties in Oklahoma and Kansas. However, their data suggest that the "treatment counties" tended to have lower prior manufacturing job growth, so it appears that the program for some reason tended to select counties with weaker manufacturing growth.

Hicks and LaFaive (2011) compare counties with different usage of Michigan's MEGA job creation tax credit. However, they do not control for year effects, so the MEGA variable in part may reflect the state's greater use of MEGA when the state's economy worsened. In addition, the MEGA program tended to target auto-related jobs (Bartik and Erickcek 2014). Therefore, counties that were more auto-dependent would be more likely to receive more MEGA incentives. These counties could tend to have weaker job growth because of their auto dependence, even if the MEGA program worked in inducing location, expansion, and retention decisions.[4]

One study, by Gabe and Kraybill (2002), is likely negatively biased because of how it selects incented and unincented firms. Gabe and Kraybill's overall sample is of all firms announcing expansions in Ohio. Their treatment group is the firms within this sample of

[4] Hicks and LaFaive (2011) do use instrumental variables to control for the endogeneity of MEGA credit usage, but the instruments are not convincingly exogenous (Bartik and Erickcek 2014). For example, the instruments include whether any MEGA credits were approved in that year for the county and county population. Both instruments will be correlated with unobserved variables driving county growth.

expanding firms who received assistance from one or more state incentive programs. Therefore, by design their sample of unassisted firms will include many firms that were doing great and had neither the time nor the need to apply for any incentive assistance. It seems plausible that among these growing firms, the smaller group that applied for incentives would be self-selected to be firms with somewhat greater challenges to expansion. Therefore, we might expect the incented firms to on average experience less growth than nonincented growing firms.

To summarize and restate this critique of most existing studies of incentive effects: many studies are biased because their data do not include any independent policy variation in incentive availability. These studies are typically of a single state. Within that state, which firms or counties are assisted depends on how firms or counties are selected by policy for assistance. This selection will rarely be random. Depending on the design of the incentive program, and the comparison group used in the study, the selection of some firms or counties for incentives may tend to favor firms or counties that would be growing faster anyway, or may favor firms or counties that would be growing slower anyway. This selection bias makes it difficult to confidently attribute the observed correlation between firm or county incentive usage, and firm or county economic performance, to the causal effect of incentives on location, expansion, or retention decisions.

Seven studies are identified that do not have any obvious selection bias. Six of these studies accomplish this by using multistate data, in which incentive usage varies due to state policy. In this case, one can at least know that there is some genuine difference in eligibility for incentive use among the different states, which is not true among different counties or firms in most single-state studies. There may still be a bias because of political factors that lead to a state's incentive policy adoption. However, it is not clear which direction this bias goes, as the evidence suggests that incentives are not strongly correlated with state unemployment rates (Bartik 2017, pp. 61–63). A state's incentive usage often shifts abruptly with the state's politics (Bartik 2017, Appendix G). In comparing different states, unlike comparing counties, at least different incentive usage is not automatically correlated with the geographic area's growth because incentives target growing firms.

Finally, one study, by Bartik and Hollenbeck (2012), compares the performance of different firms within a state but does so by relating job growth to whether the firm would receive greater incentive benefits due to preexisting firm characteristics that are plausibly unrelated to firm growth. This reveals the causal effects of incentives on firm growth, as the analysis focuses on variation in incentive usage that is uncorrelated with unobserved variables driving a firm's growth.

Summary of "But For" Percentages from Incentive Studies

In this section, I summarize the "but for" percentage estimates from research studies of incentives. Based on 30 studies, I derive 34 estimates of what proportion of location, expansion, or retention decisions would not have occurred "but for" the incentive.

Why focus on the "but for" estimate as a summary measure of incentive effectiveness? One could argue that a better measure would be how much the "but for" percentage varied as the incentive's magnitude relative to the project's size varied. For example, a reasonable assumption is that if incentive X is twice as large as incentive Y in dollar value relative to a new plant's costs, incentive X will have a greater effect in tipping the location decision for the new plant, perhaps around twice as great an effect.

Despite this argument, this survey focuses on an average "but for" percentage for each study and ignores each study's incentive magnitude. I ignore incentive magnitude for two reasons. First, for most studies, there is no good measure available of incentive magnitude. Second, even if a study focuses on one particular incentive, in many cases that particular incentive would be accompanied by other incentives. A study's estimated effect of one incentive may reflect overall effects of an incentive package.

The "but for" percentages reported in these various studies are probably best interpreted as the effects of a typical incentive package. In the real world, larger incentive packages will tend to have effects toward the high end of the estimates here, and smaller incentive packages will tend to have effects toward the low end.

Table 1 summarizes the 34 estimates. An appendix provides more information on the 30 studies behind these estimates, and how the various estimates are derived.

I look at both the central tendency from all 34 estimates and the central tendency from estimates grouped by whether the estimates are likely to be positively biased, negatively biased, or are not clearly biased in either direction.[5] For each group of estimates, I report the mean "but for" percentage and the standard error of estimation of that mean. I also report the median estimate from that group of studies, and the range from the 25th percentile to the 75th percentile of that group of studies. If a study's estimate implied that incentives had a negative effect on business location/expansion/retention decisions, the "but for" percentage is bottom-coded at zero. If a study's estimate implied that incentive effects exceeded the number of jobs incented, the estimate is top-coded at 100 percent.

[5] In a regression meta-analysis of the 34 estimates, a regression of the "but for" percentage on two dummy variables for positive bias and negative bias, with the omitted category being no obvious bias, yields an estimated coefficient of 0.256 (t-statistic = 4.09) on the positive bias dummy, and a coefficient of −0.067 (t-statistic = −2.48) on the negative bias dummies. I also tried adding variables for whether the state was single state or multistate, whether it used aggregate or micro data, and whether it was based on a survey or on econometric estimation. Controlling for the bias dummies, none of these other characteristics of the estimates were statistically significant or substantively important.

For all 34 estimates, the mean estimated “but for” percentage is 23.2 percent, with a standard error of 4.4 percent. It is unclear exactly what “population” this sample represents. One possible interpretation is the following: if we had a much larger population of studies with a similar range of methodologies, and these studies were considering similar types of incentives to what is observed in this sample, the mean “but for” percentage in that larger population of hypothetical studies would likely range from around 14 percent to 32 percent, which is plus or minus two standard errors around this sample mean. The median estimate is lower at 12.7 percent. The lower median than mean reflects that the distribution of estimates in the various studies is quite skewed, with some of the 34 estimates being quite high. The range from the 25th to the 75th percentile of studies is highly asymmetrical, ranging from 2.3 percent to 35.7 percent.

However, a majority of the 34 estimates are positively biased. Therefore, the mean and median from all 34 estimates would be expected to tend to overestimate the average incentive package’s “but for” percentage.

Across the different “bias groups” of estimates, the pattern of estimated “but for” percentages is what might be expected. For the 23 estimated “but for” percentages whose methodologies suggest a positive bias, the mean estimated “but for” percentage is 32.3 percent (standard error = 5.5 percent). The median “but for” percentage is 23.5 percent, with a range from 11.5 percent at the 25th percentile to 56 percent at the 75th percentile. In these positively biased estimates, some estimates are quite high. A quarter of the estimates have a “but for” percentage exceeding 56 percent. However, even in these positively biased estimates, the central tendency for the “but for” percentage, from either the mean or median of the estimates, is well below tipping half of the location/expansion/retention decisions. For example, the median positively biased study implies that the average incentive package tips less than one-quarter of all business location/expansion/retention decisions.

For the four negatively biased estimates, all show negative effects of incentives. All four estimates are bottom-coded at zero.

Finally, the seven estimates that are not clearly either positively or negatively biased tend to show positive effects of incentives, but lower than in the positively biased studies. For these seven estimates, the mean "but for" percentage is 6.7 percent (s.e. = 2.8 percent). The median "but for" estimate is 3.4 percent, with a range from zero at the 25th percentile to 11.4 percent at the 75th percentile.

Overall, these estimates suggest that incentives have some effect on location decisions, but considerably less than 50 percent. An average incentive package might tip somewhere between 2 and 25 percent of business location/expansion/retention decisions. The lower end is suggested by the clear finding in most studies of some effect, and by considering the mean, median, and standard error for the seven studies without obvious bias. The higher end is suggested because incentive effects should be somewhat less than in the positively biased studies, which have a median "but for" percentage of 23.5 percent.

Within this 2–25 percent range, the actual incentive effect no doubt varies greatly in different circumstances. Incentive effects might tend toward the higher end of this range for larger incentives, better-designed incentives,[6] or incentives in cases where the chosen location has close substitutes in other states; for example, if the chosen location is on a state border. The lower end of the range is more plausible for smaller incentives, more poorly designed incentives, or cases in which the firm has very strong ties to the state.

[6] For example, as argued in Bartik (2018, pp. 66–70), incentives will have a higher bang for the buck if they are more upfront, as firms in making investment decisions tend to heavily discount future cash flows.

DISCUSSION OF SELECTED ESTIMATES

To provide a fuller picture of the estimates from these studies, I will summarize results from a few studies with stronger methodologies. Among the positively biased estimates, three studies with relatively strong methodologies include, among the survey-based methodologies, Jensen (2017c); among the methodologies using data on county growth, Hoyt, Jepsen, and Troske (2008); among the methodologies using data on individual firms, Faulk (2002). Among the studies with no obvious positive or negative bias, studies with relatively strong methodologies include Chirinko and Wilson (2008) among the studies of relative state performance with different incentive regimes, and Bartik and Hollenbeck (2012) as the only study in this group that compares firms that face different cost reductions due to incentive design. The four negatively biased studies were already summarized above.

Jensen's (2017c) study provides an interesting combination of survey results with some evidence that the survey results might be valid. The "chapter 313" program considered is property tax breaks offered by school districts in Texas, for which they are reimbursed by state education funding. School districts negotiate side-payments with incented firms in which the incented firms pay the school districts some percentage of the property tax break. This side-payment averages around 31 percent of the property tax break but varies across projects from less than 1 percent of the property tax break to 62 percent of the property tax break.

Jensen (2017c) combines data on these side-payments for 257 projects with expert opinion on 106 projects. This expert opinion was from economic development professionals or consultants who were familiar with Texas incentives but were not involved with the particular project. Jensen finds that the expert opinion on whether the incentive was necessary for the project to proceed was highly correlated with the magnitude of observed side-payments. If the incentive was deemed unnecessary, the school district was more likely to negotiate a higher side-payment, presumably because its bargaining position was stronger, and vice versa. He then projects the observed relationship for these 106 projects between whether experts judged the incentive was necessary, and the side payment, to all 257 projects. His result is that the mean predicted "but for" probability for the 257 projects was between 10 and 13 percent.

Although this methodology is intriguing, the resulting estimates of "but for" percentages are still likely to be positively biased. The survey results are from economic development professionals who have been involved with either providing Texas incentives or advocating for their use. Such experts would probably have some tendency to exaggerate incentive effectiveness.

Hoyt, Jepsen, and Troske (2008) relate a Kentucky county's employment in one year to Kentucky incentives provided two years previously. This regression is likely to be positively biased because counties that are growing will have more firms that are eligible for incentives. The authors do try to minimize endogeneity by including county and year fixed effects, and lagging their incentive measure. They also do some tests for reverse causality, from employment to incentives. These tests do not find strong evidence of reserve causality. However, this test for reverse causality does not rule out employment growth affecting incentive usage, which would also bias their regression estimates.

Based on Hoyt, Jepsen, and Troske (2008), and on incentives data for Kentucky in Bartik (2017), the implied cost of creating one export-base job in these estimates is about 10 times the annual dollar incentives per job that are provided in Kentucky. This implies a "but for" percentage of around 10 percent.

Faulk (2002) compares employment change from 1993 to 1995 for firms participating in the Georgia Jobs Tax Credit, versus similar firms that do not participate. As was previously noted, we would expect that growing firms are more likely to choose to participate in this program, which would positively bias regular regression estimates. Faulk attempts to correct for this problem; her corrections may help, but in my opinion they do not fully eliminate the possibility of positive bias.

Faulk (2002) tries to reduce selection bias by including an equation that determines whether firms participate in the program. The goal is to find variables that affect whether a firm is likely to use this program, and that do not directly affect firm job growth. The two variables with the greatest predictive value in the selection equation, that are also excluded from the employment growth equation, are the company's tax liability, and whether the firm is headquartered in Georgia. The former variable is a good instrument for selection: the Georgia Jobs Tax Credit is a nonrefundable tax credit, so the credit is much more valuable to firms with a larger tax liability, but it is not obvious that tax liability would directly be correlated with a firm's job growth. On the other hand, whether the firm is headquartered in Georgia is a problematic selection instrument. A Georgia headquarters might well be correlated, as Faulk argues, with the firm's information on the program. But whether a firm has a Georgia headquarters might also have its own independent influence on job growth. Because of these problems with Faulk's instruments, her estimated effects of incentives may still tend to be positively biased.

Faulk estimates that the total jobs created by the Georgia Jobs Tax Credit from 1993 to 1995 were about 23.5 percent of the incented jobs. Based on the arguments above, this estimated "but for" percentage is likely to exceed the true "but for" percentage.

Chirinko and Wilson (2008) calculate how variations across states and time in state investment tax credits (ITCs) affect the user cost of capital in different states. They then estimate how changes in the user cost of capital affect the manufacturing capital stock. Given that the variation in the user cost of capital is explicitly due to state policy choices, there is no obvious mechanical way in which states that tend to be faster or slower growing would necessarily as a result have different investment tax credit policies or different user costs of capital. They estimate that for the average state with an investment tax credit, the elimination of the investment tax credit would in the long-run reduce the state's manufacturing capital stock by 2.34 percent. For this to be consistent with effects of the investment tax credit on a firm's decision to invest in a new plant, or in a plant expansion, the average effect of a typical state ITC on the probability of locating a new plant or making an expansion decision would have to be 2.34 percent.[7] In other words, the implied "but for" percentage for the typical ITC is 2.34 percent.

Bartik and Hollenbeck (2012) look at how the Washington's State's R&D tax credit affects the marginal cost of expanding jobs for different Washington firms, and how these different marginal costs in turn affect the firm's employment growth. The Washington R&D credit is capped in dollar value per firm. The credit is also nonrefundable, so firms without tax liability cannot claim the credit. Therefore, some firms (large firms or firms without tax liability) have no reason to expand. In addition, the value of the R&D credit relative to the firm's costs depend on how much R&D the firm does relative to its costs.

To control for the reality that growing firms may also do more R&D, and hence tend to collect more R&D tax credits even if the credit had no effect on growth, the Bartik and

[7] In models in which long-run business activity is due to a combination of new investment with capital depreciation, and capital depreciation is relatively fixed, the long-run percentage effect on the capital stock of some policy variable or economic variable is equal to the percentage effect of that variable on new investment activity.

Hollenbeck (2012) analysis predicted the firm's marginal cost of expanding jobs and how it was affected by the R&D credit, based on whether the firm was capped or had no tax liability, and based on national trends in the R&D intensity of the firm's industry. In other words, Bartik and Hollenbeck used these variables as instruments in predicting the marginal cost of expansion for different firms. This can be seen as a quasi-experiment in which cross-firm variation in size, tax liability, and industry R&D intensity lead to different firms effectively facing different policy regimes that vary the net marginal costs of expanding.

The resulting estimates indicated that R&D-credit-induced variations in the marginal costs of expanding jobs had statistically significant effects on employment growth. However, the overall effect on job growth was quite modest. This is in part because many firms did not face lower marginal costs of expansions, despite the credit, because, for example, they were large enough that their credits were capped. The total estimated jobs created in each year by the program averaged only 3.4 percent of the job creation claimed by firms receiving the tax credits.[8]

The overall lesson from these studies is that among some of the stronger studies with positive bias, the estimated "but for" percentage is less than 25 percent, or one-quarter. Among the stronger studies with no obvious bias, the estimated "but for" percentage is less than 10 percent.

How Consistent Are These "But For" Estimates with Other Research?

Is this "but for" range of 2–25 percent for incentives in the United States consistent with other research? Yes. This research is consistent with both the best study of incentives in foreign

[8] Bartik and Hollenbeck's (2012) methodology measures the marginal costs of expansion relative to the value-added of that expansion. The resulting coefficient estimate implies that an incentive package of 1 percent of value-added would be expected to increase the number of jobs by 5 percent. If the average state and local incentive package in the United States is 1.24 percent (Bartik 2018), then the resulting "but for" percentage would be a little over 6 percent. The Washington State R&D credit has a lower "but for" percentage because the credit is a lower proportion of costs than the typical incentive package, even for these highly R&D intensive firms.

counties, by Devereux, Griffith, and Simpson (2007), and with the U.S. research literature on location effects of state and local business taxes.

Devereux, Griffith, and Simpson (2007) consider the effect of a discretionary grant program to influence industrial location decisions in the United Kingdom. They explicitly model how grant offers to firms are determined in different locations, based on the industry and designation of the area as "distressed." They end up estimating that a grant whose average expected value (in 2015 U.S. dollars) is a one-time offer of around $4,500 per job would increase the probability of choosing that area by about 1 percent. In the United States, the average present value of incentives per project, over the life of a project, is roughly 5 times as great, so the expected effect on the probability of choosing a particular location would be around 5 percent.[9]

An extensive research literature exists on the effects of state and local business taxes on business location decisions. Holding public services constant, this research literature implies that a 10 percent decrease in state and local business taxes will increase the probability of a positive business location by somewhere in the range of 1.5–8.5 percent (Bartik 1992).[10] Given average levels of state and local business taxes, and average levels of incentives, this implies that the average business incentive package in the United States might tip between 4 and 21 percent of location decisions.[11]

---

[9] A one-time offer of $4,500 per job would amount to about 0.27 percent of value-added over an infinite time horizon for a new plant, discounting the future at 12 percent per year. U.S. incentives average about 1.2 percent of value-added over an infinite time horizon (Bartik 2018), so average U.S. incentives are about five times as great.

[10] This review of the older research literature is consistent with more recent research by Suárez Serrato and Zidar (2016), and Giroud and Rauh (forthcoming).

[11] Gross state and local business taxes in the United States have generally averaged about 5 percent of value-added (Bartik 2017; or see various reports done by Ernst and Young for Council on State Taxation). Average incentives, considered over a lifetime, and discounted at a 12 percent discount rate, average about 1.24 percent of value-added (Bartik 2018). Therefore, the average incentive package is equivalent to about a 25 percent cut in state and local business taxes. Its effect on the probability of business location decisions should be 2.5 times the effect of a 10 percent business tax cut, or between 4 percent (= 2.5 × 1.5 percent) and 21 percent (= 8.5 percent × 2.5).

Why Aren't "But For" Percentages Higher?

The main reason "but for" percentages aren't higher is that there are many other location and cost factors that have more major effects on a firm's costs and profitability. A secondary reason in some studies is that one firm locating in an area may reduce the likelihood of other firms choosing that location.

These other location and cost factors not only are a larger share of costs than typical incentives, but also vary quite a bit across state and local economies. Because these other location and cost factors vary greatly across diverse state and local areas, typical current levels of incentives are only able to tip a location decision toward a particular state or local area in a minority of cases, and probably less than one-quarter of cases.[12]

What are these other location factors? Consider, for example, worker wages and productivity. State and local incentives average about 1.24 percent of overall value-added for assisted export-base businesses. Wages are about half of value-added for export-base businesses (Bartik 2017). Therefore, an average incentive of 1.24 percent of value-added can be offset by variations in wages or labor productivity of about 2.5 percent, which is modest compared to likely cross-area variations in such labor costs.

To put it another way: Suppose one thought that this paper's estimate that the average state and local incentive package only tipped 2–25 percent of location and expansion decisions was too low. Suppose one instead hypothesized that the average state and local incentive package would tip 50 percent of location decisions. This hypothesis implies that one believes that if an area's wages declined by 2.5 percent, the area would double its number of favorable business location and expansion decisions. Such a large effect of a modest wage decline seems

[12] Mast's (2018) analysis of property tax administered policies of local governments in New York State implies that these governments act as if incentives only matter in swaying decisions within a 25 kilometer radius.

implausible. Firms just don’t seem to be that hypersensitive to costs in making location and expansion decisions.

Other important location factors include land suitable for industrial development. Having land that is appropriately zoned, with adequate utilities and good transportation infrastructure, can play a key role in business location decisions. For example, Chapple (2014) demonstrates that the availability of industrially zoned land plays a key role in industrial location decisions in the San Francisco Bay area.

As another example, specialized workforce development programs or applied research centers can frequently make large incentives less necessary. For instance, Lowe (2014) documents how North Carolina’s efforts to attract biotech manufacturing have benefitted from both specialized biotech workforce programs and specialized applied research centers at local universities. These programs reduce the need for large tax breaks and other cash incentives.

In studies that look at aggregate effects of incentives on county or state job growth, another offset is that one firm’s location decision may have negative effects on other location decisions. Firms’ location decisions may of course have input-output multiplier effects on suppliers and local retailers. The firm may buy from local suppliers, and its workers may buy from local retailers. In some cases, a firm’s location may have agglomeration economy effects that may attract similar firms, for example, if other firms can benefit from the ideas and skills of the first firm’s workers. But by occupying one industrial site, a firm precludes other firms locating at that same site. Furthermore, a firm locating in this state or local area may push up local wages and prices, which will discourage other firms from locating in this area. These displacement effects, from either direct usage of land or higher local wages and prices, may offset some or all of the various job creation effects from one firm's location decision, which is the sum of direct effects, input-output multiplier effects, and agglomeration economy effects.

There is mixed evidence on whether a firm's positive location decision toward a particular area ends up raising the area's overall net employment. Two prominent research papers by Greenstone and Moretti (2004) and Greenstone, Hornbeck, and Moretti (2010) provide some evidence that a positive location decision by one firm leads to overall positive local employment effects. In fact, in these two studies, the effect of landing a big firm are large enough to imply the existence of sizable local agglomeration economy effects, as the effects exceed what would be expected based on ordinary input-output multipliers. But Fox and Murray (2004) and Edmiston (2004) find evidence that, in some cases, a firm's decision to locate a new plant in an area may have net local job creation effects that are significantly less than the jobs at the new plant. Patrick (2016) finds results in between these two sets of papers: a new plant location decision probably has net positive direct effects on job growth, followed by some multiplier effects, but there is no strong evidence that agglomeration economy effects outweigh displacement effects.[13]

CONCLUSION

Overall, the research literature on incentives' "but for" effects is not as rigorous as one might hope. It suffers from two problems. The first is that incentive usage by a given firm or county is rarely an exogenous policy experiment, in which one firm or county has greater

[13] There are some unresolved methodological disputes in this research literature. Greenstone et al.'s two papers (2004, 2011) argue that their data provide a natural experiment; Patrick (2016) disputes their finding.

eligibility for an incentive for reasons that are unrelated to likely future growth.[14] Firms or counties that use more incentives are likely to be selected for reasons related to their growth. This biases most current studies, with a majority of studies biased in a positive direction, toward finding that incentives are more effective.

The second problem is that most of the incentive studies do not have a good measure of the size of the incentive being examined. A related issue is that studies rarely control for other possible incentives. Ultimately, rather than estimating the "but for" percentage, we would like to see how incentive effects vary with incentive magnitude. More ambitiously, we might want to get empirical results that discover how different incentive designs influence effectiveness. But most current studies lack the detail needed on the examined incentives to consider the effects of either incentive magnitude or incentive design.

Because of the problems with the current incentive research literature, I think it is preferable to simulate the likely effects of economic development incentives by extrapolating from the research literature on the effects of state and local business taxes. Incentives should have roughly similar effects to a business tax reduction of similar dollar value.

Compared to the incentives literature, the state and business tax literature has significant advantages in both rigor of econometric methodology and measurement. The state and local business tax literature has measures of business tax rates that are plausibly exogenous to other factors affecting the growth of businesses in state and local economies. Many of the business tax studies have good measures of the magnitude of the business taxes being examined.

[14] Thus, for example, the research literature on enterprise zones does not really solve this problem, because enterprise zones are generally chosen because they are expected to have poor growth prospects. This creates some real problems with evaluating zone success. They can be overcome (Busso, Gregory, and Kline 2013). However, whether these estimated effects for a distressed neighborhood are generalizable to incentives for an entire state is questionable.

However, despite the limitations of the incentives research literature, the estimated effects seem reasonable, both intuitively and relative to the business tax literature. The incentives research literature implies that average state and local incentives packages probably have a “but for” percentage of 2 percent to 25 percent. This is broadly consistent with the business tax literature.

Such a range of effects implies that in evaluating incentive benefits and costs, the devil is in the details. On the one hand, incentives’ “but for” effects are unlikely to be in the 50 percent and up range. If incentives did have such a large effect on business location, expansion, and retention decisions, almost any reasonably sized incentive package would have large payoffs for state and local residents. On the other hand, we cannot rule out incentive effects as large as 25 percent. If incentive effects were assuredly less than 2 percent, then we would know that incentives were a waste of money. But if incentives tip 5 percent or 10 percent or 15 percent or even 20 percent of business decisions, then whether the incentive pays off for state or local residents depends on the details of the incentive costs, multiplier effects, job quality, and who gets the created jobs.

The limitations of the current incentives research literature also imply that we need more and better measures of how the magnitude of incentive availability varies across different geographic areas or different firms over time. With such data, we might be able to estimate the effects of more natural experiments that alter the magnitude of incentive availability across different firms or geographic areas. The result estimates of incentive effects would be more reliable. Such estimates would also be more policy relevant, as they would relate incentive effects to incentive costs.

REFERENCES

Bartik, Timothy J. 1992. "The Effects of State and Local Taxes on Economic Development: A Review of Recent Research." *Economic Development Quarterly* 6(1): 102–110.

———. 2010. "Bringing Jobs to People: How Federal Policy Can Target Job Creation for Economically Distressed Areas." The Hamilton Project, Discussion Paper. Washington, DC: Brookings Institution.

———. 2017. *A New Panel Database on Business Incentives for Economic Development Offered by State and Local Governments in the United States.* Report prepared for the Pew Charitable Trusts. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research. http://research.upjohn.org/reports/225 (accessed June 29, 2018).

———. 2018. "Who Benefits from Economic Development Incentives? How Incentive Effects on Local Incomes and the Income Distribution Vary with Different Assumptions about Incentive Policy and the Local Economy." Upjohn Institute Technical Report No. 18-034. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research. http://doi.org/10.17848/tr18-034 (accessed June 29, 2018).

Bartik, Timothy J., and George Erickcek. 2014. "Simulating the Effects of the Tax Credit Program of the Michigan Economic Growth Authority on Job Creation and Fiscal Benefits." *Economic Development Quarterly* 28(4): 314–327.

Bartik, Timothy J., and Kevin Hollenbeck. 2012. "An Analysis of the Employment Effects of the Washington High Technology Business and Occupation (B&O) Tax Credit: Technical Report." Upjohn Institute Working Paper 12-187. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research. https://doi.org/10.17848/wp12-187 (accessed June 29, 2018).

Bundrick, Jacob, and Thomas Snyder. 2017. "Do Business Subsidies Lead to Increased Economic Activity: Evidence from Arkansas's Quick Action Closing Fund." Mercatus working paper. Arlington, VA: Mercatus Center, George Mason University.

Buss, Terry F. 2001. "The Effect of State Tax Incentives on Economic Growth and Firm Location Decisions: An Overview of the Literature." *Economic Development Quarterly* 15(1): 90–105.

Busso, Matias, Jesse Gregory, and Patrick Kline. 2013. "Assessing the Incidence and Efficiency of a Prominent Place Based Policy." *American Economic Review* 103(2): 897–947.

Calcagno, Peter T., and Henry Thompson. 2004. "State Economic Incentives: Stimulus or Reallocation?" *Public Finance Review* 32(6): 651–665.

Chapple, Karen. 2014. "The Highest and Best Use? Urban Industrial Land and Job Creation." *Economic Development Quarterly* 28(4): 300–313.

Chirinko, Robert S., and Daniel J. Wilson. 2008. "State Investment Tax Incentives: A Zero-Sum Game?" *Journal of Public Economics* 92: 2362–2384.

———. 2016. "Job Creation Tax Credits, Fiscal Foresight, and Job Growth: Evidence from U.S. States." Federal Reserve Bank of San Francisco Working Paper 2010-25. San Francisco: Federal Reserve Bank of San Francisco. http://www.frbsf.org/economic-research/publications/working-papers/wp10-25bk.pdf (accessed June 29, 2018).

Connecticut Department of Economic and Community Development. 2014. *An Assessment of Connecticut's Tax Credit and Abatement Programs*. Hartford, CT: State of Connecticut, Department of Economic and Community Development.

Devereux, Michael P., Rachel Griffith, and Helen Simpson. 2007. "Firm Location Decisions, Regional Grants and Agglomeration Externalities." *Journal of Public Economics* 91(3-4): 413–435.

Edmiston, Kelly D. 2004. "The Net Effects of Large Plant Locations and Expansions on County Employment." *Journal of Regional Science* 44(2): 289–319.

Faulk, Dagney. 2002. "Do State Economic Development Incentives Create Jobs? An Analysis of State Employment Tax Credits." *National Tax Journal* LV(2): 263–280.

Florida Office of Program Policy Analysis and Government Accountability. 2014. *Florida Economic Development Program Evaluations Year 1*. Legislative Report No. 14-01. Tallahassee, FL: Florida Office of Program Policy Analysis and Government Accountability.

———. 2015. *Florida Economic Development Program Evaluations—Year 3*. Florida Legislative Report No. 15-11. Tallahassee, FL: Office of Program Policy Analysis and Government Accountability.

Fox, William F., and Matthew N. Murray. 2004. "Do Economic Effects Justify the Use of Fiscal Incentives?" *Southern Economic Journal* 71(1): 78–92.

Gabe, Todd M., and David S. Kraybill. 2002. "The Effect of State Economic Development Incentives on Employment Growth of Establishments." *Journal of Regional Science* 42(4): 703–730.

Giroud, Xavier, and Joshua D. Rauh. Forthcoming. "State Taxation and the Reallocation of Business Activity: Evidence from Establishment-Level Data." *Journal of Political Economy*.

Goss, Ernest P., and Joseph M. Phillips. 1999. "Do Business Tax Incentives Contribute to a Divergence in Economic Growth?" *Economic Development Quarterly* 13(3): 217–228.

Greenstone, Michael, Richard Hornbeck, and Enrico Moretti. 2010. "Identifying Agglomeration Spillovers: Evidence from Winners and Losers of Large Plant Openings." *Journal of Political Economy* 118(3): 536–598.

Greenstone, Michael, and Enrico Moretti. 2004. "Bidding for Industrial Plants: Does Winning a 'Million Dollar Plant' Increase Welfare?" Unpublished manuscript, Massachusetts Institute of Technology.

Hansen, Tonya J., and Laura Kalambokidis. 2010. "How Are Businesses Responding to Minnesota's Tax-Free Zone Program?" *Economic Development Quarterly* 24(2): 180–192.

Hicks, Michael J., and Michael LaFaive. 2011. "The Influence of Targeted Economic Development Tax Incentives on County Economic Growth: Evidence from Michigan's MEGA Credits." *Economic Development Quarterly* 25(2): 193–205.

Horwitz, Jason, Traci Taylor, and Jonathan Waldron. 2016. *The Economic Impact of Business Tax Credits in Tennessee.* Report prepared for Tennessee Department of Revenue. Chicago: Anderson Economic Group.

Hoyt, William H., Christopher Jepsen, and Kenneth R. Troske. 2008. "Business Incentives and Employment: What Incentives Work and Where?" IFIR Working Paper No. 2009-02. Lexington, KY: University of Kentucky, James W. Martin School of Public Policy and Administration, Institute for Federalism and Intergovernmental Relations.

Indiana Office of Fiscal and Management Analysis. 2017. *2017 Indiana Tax Incentive Evaluation*. Indianapolis: Indiana Legislative Services Agency, Office of Fiscal and Management Analysis.

Jensen, Nathan M. 2017a. "The Effect of Economic Development Incentives and Clawback Provisions on Job Creation: A Pre-Registered Evaluation of Maryland and Virginia Programs." *Research and Politics* (April–June): 1–8.

———. 2017b. "Job Creation and Firm-Specific Location Incentives." *Journal of Public Policy* 37(1): 85–112.

———. 2017c. "Exit Options in Firm-Government Negotiations: An Evaluation of the Texas Chapter 313 Program." Washington Center for Equitable Growth Working Paper series. Washington, DC: Washington Center for Equitable Growth.

Jin, Zhong. 2015. *The Iowa New Jobs Tax Credit*. Tax Credits Program Evaluation Study, Tax Research and Program Analysis Section. Des Moines: Iowa Department of Revenue.

Jolley, G. Jason, Mandee Foushee Lancaster, and Jiang Gao. 2015. "Tax Incentives and Business Climate: Executive Perceptions from Incented and Nonincented Firms." *Economic Development Quarterly* 29(2): 180–186.

LaFaive, Michael D., and Michael Hicks. 2018. *An Assessment of the Michigan Business Development Program*. Midland, MI: Mackinac Center for Public Policy.

Lane, Brent, and G. Jason Jolley. 2009. *An Evaluation of North Carolina's Economic Development Incentive Programs*. Final report. Chapel Hill: UNC Center for Competitive Economies, University of North Carolina at Chapel Hill.

Lee, Yoonsoo. 2008. "Geographic Redistribution of US Manufacturing and the Role of State Development Policy." *Journal of Urban Economics* 64(2): 436–450.

Lester, T. William, Nichola J. Lowe, and Allan Freyer. 2014. "Mediating Incentive Use: A Time-Series Assessment of Economic Development Deals in North Carolina." *Economic Development Quarterly* 28(2): 132–146.

Loh, Eng Seng. 1993. "The Effects of Jobs-Targeted Development Incentive Programs." *Growth and Change* 24(3): 365–383.

Lowe, Nichola J. 2014. "Beyond the Deal: Using Industrial Recruitment as a Strategic Tool for Manufacturing Development." *Economic Development Quarterly* 28(4): 287–299.

Luger, Michael I., and Suho Bae. 2005. "The Effectiveness of State Business Tax Incentive Programs: The Case of North Carolina." *Economic Development Quarterly* 19(4): 327–345.

Lynch, Robert G., Gunther Fishgold, and Dona L. Blackwood. 1996. "The Effectiveness of Firm-Specific State Tax Incentives in Promoting Economic Development: Evidence from New York State's Industrial Development Agencies." *Economic Development Quarterly* 10(1): 57–68.

Maryland Legislative Services. 2016. *Evaluation of the Job Creation Tax Credit*. Annapolis: State of Maryland, Department of Legislative Services, Office of Policy Analysis.

Mast, Evan. 2018. "Race to the Bottom? Local Tax Break Competition and Business Location." Unpublished manuscript. W.E. Upjohn Institute for Employment Research, Kalamazoo, MI.

Millsap, Adam A. 2015. *Does Pennsylvania's Redevelopment Assistance Capital Program Develop Its Economy?* Arlington, VA: Mercatus Center, George Mason University.

Minnesota Office of the Legislative Auditor. 2008. *JOBZ Program*. Evaluation report. St. Paul: State of Minnesota, Office of the Legislative Auditor, Program Evaluation Division.

———. 2017. *Minnesota Research Tax Credit*. 2017 Evaluation report. St. Paul: State of Minnesota, Office of the Legislative Auditor, Program Evaluation Division.

Mississippi Institutions of Higher Learning. 2016. *The Annual Tax Expenditure Report*. Jackson, MS: University Research Center.

Neumark, David, and Diego Grijalva. 2017. “The Employment Effects of State Hiring Credits.” *ILR Review* 70(5): 1111–1145.

Patrick, Carlianne. 2014. “Does Increasing Available Non-Tax Economic Development Incentives Result in More Jobs?” *National Tax Journal* 67(2): 351–386.

———. 2016. “Identifying the Local Economic Development Effects of Million Dollar Facilities.” *Economic Inquiry* 54(4): 1737–1762.

Sallee, Caroline M., Colby Spencer, Jason Horwitz, and Alex Rosaen. 2012. *Review of Kentucky’s Economic Development Incentives*. Report to Kentucky Legislative Research Commission. Chicago: Anderson Economic Group.

Suárez Serrato, Juan Carlos, and Owen Zidar. 2016. “Who Benefits from State Corporate Tax Cuts? A Local Labor Markets Approach with Heterogeneous Firms.” *American Economic Review* 106(9): 2582–2624.

Washington Joint Legislative Audit and Review Committee (JLARC). 2017. *JLARC Final Report: 2016 Tax Preference Performance Reviews*. State of Washington, Joint Legislative Audit and Review Committee. Olympia, WA: Washington Joint Legislative Audit and Review Committee.

Whitacre, Brian E., David Shideler, and Randi Williams. 2016. “Do Incentive Programs Cause Growth? The Case of the Oklahoma Quality Jobs Program and Community-Level Economic Growth.” *Economic Development Quarterly* 30(1): 62–74.

Table 1 "But For" Percentage in Different Estimates of Economic Development Incentives

Categorization of estimates (number of studies)	Mean "but for" % (standard error of mean)	Median "but for" % (25th to 75th percentile)
All studies (34 estimates)	23.2 (4.4)	12.7 (2.3 to 35.7)
Positively biased estimates (23)	32.3 (5.5)	23.5 (11.5 to 56.0)
Negatively biased estimates (4)	0 (0.0)	0 (0 to 0)
No obvious bias (7)	6.7 (2.8)	3.4 (0 to 11.4)

NOTE: This table summarizes the results for 34 estimates for the "but for" percentage: the percentage of incented firms that were induced to make a location or expansion or retention decision because of the incentive. Estimates are categorized by whether the methodology is likely to lead to a positive bias in the estimated "but for" percentage, a negative bias, or whether there is no obvious reason to think that the estimate is biased. For each category of estimate, the table reports the sample mean "but for" percentage, the standard error of that mean, the median "but for" percentage, and the 25th to 75th percentile of estimated "but for" percentages in that category. Estimates that would yield negative "but for" percentage are bottom-coded as zero, and estimates that would yield "but for" percentage exceeding 100 percent are top-coded as 100 percent.

Table A1 Summary of Salient Features for 34 Estimates of "But For" Percentage for Incentives

Study	Program state or local area	Incentive program	Nature of incentive program	Year range of incentives studied in their data	Survey vs. empirical	Aggregate vs. micro	Likely selection bias	"But for" (%)
LaFaive & Hicks (2018)	Michigan	Michigan Business Development Program	One-time grant for job creation	2012–2016	Empirical	Aggregate	Negative	0.0
Indiana OFMA (2017)	Indiana	EDGE	Discretionary refundable payroll tax withholding	2000–2016	Empirical	Micro	Positive	69.2
Bundrick & Snyder (2017)	Arkansas	Quick Action Closing Fund	Discretionary grant	2009–2015	Empirical	Aggregate	Positive	35.7
Jensen (2017a)	Maryland	Maryland Economic Development Assistance Authority Fund	Wide variety	2006–2012	Empirical	Micro	Positive	66.3
Jensen (2017a)	Virginia	Governors' Opportunity Fund	Deal closing fund	2006–2012	Empirical	Micro	Positive	68.2
Jensen (2017b)	Kansas	Promoting Employment Across Kansas	Payroll credit	2009–2013	Survey	Micro	Positive	20.8
Jensen (2017b)	Kansas	Promoting Employment Across Kansas	Payroll credit	2009–2013	Empirical	Micro	Positive	10.9
Jensen (2017c)	Texas	Chapter 313	Property tax abatement	2002–2014	Survey	Micro	Positive	11.5
Neumark & Grijalva (2017)	Multi-state	State job tax credits	Wide variety	1991–2011	Empirical	Aggregate	None	10.0
Horwitz, Taylor, & Waldron (2016)	Tennessee	Jobs tax credit	Tax credit for jobs created, with different tiers	2011–2012	Empirical	Micro	Positive	13.2
Horwitz, Taylor, & Waldron (2016)	Tennessee	Machinery and equipment tax credit	Tax credit for machinery and equipment purchased.	2011–2012	Empirical	Micro	Positive	0.0
Chirinko & Wilson (2016)	Multi-state	Job creation tax credits	Usually discretionary tax credits for job creation	1990–2010	Empirical	Aggregate	None	19.9

Table A1 (Continued)

Study	Program state or local area	Incentive program	Nature of incentive program	Year range of incentives studied in their data	Survey vs. empirical	Aggregate vs. micro	Likely selection bias	"But for" (%)
Whitacre, Shideler & Williams (2016)	Oklahoma	Quality Jobs program	Tax credit as % of wages	1990–2005	Empirical	Aggregate	Negative	0.0
Jin (2015)	Iowa	Job creation tax credit	Tax credit as % of wages	2006–2013	Empirical	Micro	Positive	47.9
Florida OPPAGA (2015)	Florida	Manufacturing/defense/ spaceport investment incentives	Investment tax credits by exempting sales tax on machinery and equipment	2012–2014	Survey	Micro	Positive	6.3
Florida OPPAGA (2015)	Florida	Manufacturing/defense/ space sales tax credits	Sales tax credits	2012–2014	Survey	Micro	Positive	37.5
Jolley, Lancaster, & Gao (2015)	North Carolina	Lee Tax Act credits for job creation	Wide variety	2003–2006	Survey	Micro	Positive	29.3
Florida OPPAGA (2014)	Florida	7 state incentive programs	Wide variety	2010–2012	Survey	Micro	Positive	57.0
Lester, Lowe, & Freyer (2014)	North Carolina	Retention incentives	Wide variety	1996–2006	Empirical	Micro	Positive	19.9
Patrick (2014)	Multi-state	Incentive environment	State constitution restrictions on incentives	1970–2000	Empirical	Aggregate	None	0.0
Bartik & Hollenbeck (2012)	Washington	R&D tax credit	R&D tax credit	2005–2009	Empirical	Micro	None	3.4
Hicks & LaFaive (2011)	Michigan	MEGA	Payroll credit	1996–2002	Empirical	Aggregate	Negative	0.0
Hansen & Kalambokidis (2010)	Minnesota	JobZ program	Package of incentives	2004–2005	Empirical	Aggregate	Positive	16.1
Lane & Jolley (2009)	North Carolina	Wide variety of NC incentives	Wide variety of credits	1996–2006	Empirical	Micro	Positive	0.0
Chirinko & Wilson (2008)	Multi-state	ITCs	Investment tax credits	1982–2004	Empirical	Aggregate	None	2.3
Hoyt, Jepsen, & Troske (2008)	Kentucky	Various state programs	Investment tax credits tied to job creation	1992–2004	Empirical	Aggregate	Positive	10.6
Lee (2008)	Multi-state	A wide variety of tax exemptions	Tax incentives	1972–1992	Empirical	Micro	None	11.4
Minnesota OLA (2008)	Minnesota	JobZ program	Package of incentives	2004–2006	Survey	Micro	Positive	56.0

Table A1 (Continued)

Study	Program state or local area	Incentive program	Nature of incentive program	Year range of incentives studied in their data	Survey vs. empirical	Aggregate vs. micro	Likely selection bias	"But for" (%)
Calcagno & Thompson (2004)	Multi-state	State expenditures on incentives per labor force	State agency incentive spending	1981–1989	Empirical	Aggregate	None	0.0
Faulk (2002)	Georgia	Georgia Jobs Tax Credit	Tax credit	1993–1995	Empirical	Micro	Positive	23.5
Gabe & Kraybill (2002)	Ohio	Five state incentive programs	Wide variety	1993–1995	Empirical	Micro	Negative	0.0
Goss & Phillips (1999)	Nebraska	LB775, Nebraska main incentive program	Wide variety	1987–1995	Empirical	Aggregate	Positive	100.0
Lynch, Fishgold, & Blackwood (1996)	New York	IDAs	Property tax break	1977–1991	Survey	Micro	Positive	31.0
Loh (1993)	Ohio	State spending on incentives	State agency spending	1982–1990	Empirical	Aggregate	Positive	12.3

Table A2 Notes on Studies of "But For" Percentage for Incentives (from Table A1)

LaFaive & Hicks (2018). Empirical approach first differences at quarterly level, and then regresses first differenced job growth by county on lags in first differenced job growth and first differenced incentives disbursements going back four quarters. Negative selection likely because estimate is capturing whether if past job growth more associated with incentives, what would one expect for current job growth.

Indiana OFMA (2017). From their reported data in Figure 11, a simple pre-post comparison of firms that received EDGE credits suggests maximum "but for" percentage of 69.2 percent. This is likely positively biased because firms that expand more likely to apply for EDGE.

Bundrick & Snyder (2017). They find insignificant positive effects. However, number of jobs incented are also quite small. If one divides estimated jobs created by county by implied estimates of jobs incented per county, implied "but for" percentage is actually quite high, at 35.7 percent. Increase of private employment by 0.211 per 1,000 population yields increase of 8.229 in average county of 39,000. If multiplier of 2, then direct increase is 4.115. This is effect of $100,000 in incentives. Because incentives average around $8,667 per direct job incented ($156 million reported in the paper divided by 18,000 direct jobs), $100,000 in incentives would incent 11.53 jobs. 4.115 divided by 11.53 is 35.7 percent.

Jensen (2017a). Virginia, Maryland. Compares each firm to matched firm based on log of 2006 employment, 3-digit SIC (NAICS) and dummy if firm was subsidiary. To derive "but for" percentages from his estimates, use column (1) of his Tables 1 and 2 to derive average size post-incentive of incented firms, and use his appendix Table A1 to derive percentage effect on incentives. For this calculation, I assume the distribution of incented firms' employment is log-normal with the same standard deviation pre- and post-incentive. I use his reported means of log-normal distribution post-incentive, and pre-incentive effects, and infer from his figures that standard deviation of log-normal distribution is 1.5 in Virginia, and 1.375 in Maryland. Together, this allows estimate of average job effect of incentives. This can be compared with his estimates in text of average jobs incented per incentive deal to get "but for" percentage. Because expanding firms more likely to apply for incentives, these estimates are positively biased.

Jensen (2017b). Kansas. To infer "but for" estimates from regression, I use percentage effect estimates from Model 2 in Jensen's Table 1. I also assume distribution of incented firms' employment is log normal with same standard deviation both pre- and post-incentive. I use reported mean of log employment post-incentive from his Table 2, and assume from his Figure 3 that standard deviation of log employment either pre-incentive or post-incentive is 1.75. This allows calculation of average job effect of incentives from regression, which can be compared with proposed jobs in incented firms to get "but for" percentage. This is positively biased because of expanding firms selecting program. Survey of incented firms suggests "but for" percentage is 20.8 percent—only 5 out of 24 respondents indicate they would have hired fewer employees. This is also likely to be positively biased because firms will overestimate effects of program.

Jensen (2017c). Chapter 313 property tax abatement program in Texas. Solicited experts' opinion as to whether incentives were necessary for firms to locate in Texas. Related this to supplemental payments made to school districts for school districts to sign off on hold harmless tax abatements. Found strong correlation. Mean reported probability that incentive was needed was 10 to 13 percent. 24 out of 106 is the opinions of the five experts—the 10 to 13 percent is extrapolation to full sample of 257 projects. These 257 are from 2002–2014.

Neumark & Grijalva (2017). Complex paper in which regress change in employment on current and 12 months of lag change in adoption of employment tax credits that reward employment. Find that effect on gross hiring is 10 times that on net employment change, so 10 percent used as but for percentage. Because this particular type of credit is likely to respond to state doing worse, negative selection likely.

Table A2 (Continued)

Horwitz, Taylor, & Waldron (2016). Evaluation of TN's economic development programs, principally jobs tax credits, at various tiers, and machinery investment tax credit. For the jobs tax credit, found that jobs in LR went up either 11.0 percent or 15.3 percent for 2011 and 2012, average of 13.2 percent. For machinery tax credit, LR effect is negative, which I treat as zero. Both of these should be positively biased.

Chirinko & Wilson (2016). "But for" percentage derived from their Table 9. Use estimates of portion of job creation tax credits which are NOT inframarginal subsidies. No obvious bias from their estimation procedure.

Whitacre, Shidelar, & Williams (2016). Found negative effects of program on manufacturing job growth. I treat this as "but for" of 0 percent. However, there is some sign that matched treatment counties had lower prior manufacturing growth. Therefore, likely negative bias to estimated "but for."

Jin (2015). Iowa New Jobs Tax Credit. Among businesses working with community colleges, 73 jobs created by those receiving credit, 38 jobs by those not receiving credit. This implies "but for" percentage of 47.9 percent (= (73−38) / 73). Given the likely selection effect, this is probably positively biased.

Florida OPPAGA (2015). For Manufacturing and Spaceport Investment Incentives, 7 out of 8 reported they would have made same decisions without incentive and remaining firm would have proceeded at smaller scale. The assumed but for is one-half of 1 out of 8, or 6.3 percent. Semiconductor/defense/space sales tax exemption: 2 of 8 would have proceeded the same, 6 at smaller scale. "But for" is assumed to be one-half of 6 out of 8, or 37.5 percent. These survey results are probably positively biased as firms have reasons to say that incentive mattered more than it did.

Jolley/Lancaster/Gao (2015). Survey firms that received Lee Act tax credit. Only 29.3 percent were aware they had received, which suggests that this is maximum "but for" percentage.

Florida OPPAGA (2014). Look at 7 incentive programs. Reported from survey that only 36 percent said would not have done project at all without incentive, although another 42 percent said would have scaled back. So, if assume that half of the scaled back activity would have proceeded without incentives, but for percentage is 36 percent plus 21 percent = 57 percent. This is from survey of 144 firms receiving at least one incentive, of which 74 responded, 54 complete responses. Don't report sample size for individual questions.

Lester, Lowe, & Freyer (2014). North Carolina. Discretionary incentives. Find that effect of retention incentives is to increase employment about 19.9 percent compared to similar firms. For recruitment incentives, find that number of jobs created is 11.53 ahead of other firms already in state that are matched, which is about 5.8 percent of average incented jobs of 199 jobs, so this is indication of better performance of recruited firms. But this doesn't really enable you to determine "but for" for original recruitment.

Patrick (2014). Regresses county employment levels and growth on controls for unobserved effects and trends, and in some cases, uses border county effects. Incentive variable is state constitution provisions, which she seems to use as level. Finds few significant positive effects, and they're not robust. In fact, effects may be negative. She concludes effects are probably overall close to zero, which I use as "but for" percentage. No clear bias either way.

Bartik & Hollenbeck (2012). For each year from 2005 to 2009, calculate ratio of jobs created (Table 5) to claimed jobs created (Table 2). The "but for" is simple average of this ratio over these five years. The paper uses an elaborate methodology to instrument for the endogeneity of the R&D credit variable, and account for whether it effectively lowers marginal costs, and for the importance of R&D costs in overall costs for firm. There is no obvious bias in this estimation.

Table A2 (Continued)

Hicks & LaFaive (2011). Although does instrumental variable analysis, the instruments include variables correlated with the county's growth, such as whether any MEGA credits approved in that year, and county population. In addition, because equation does not control for year effects, the MEGA variable in part reflects likely negative correlation between state job trends and MEGA. Finally, given that the MEGA program heavily targeted auto sector, it seems likely that the MEGA variable is correlated with negative time trends in more auto-dependent counties. Therefore, it seems likely that the overall estimation is negatively biased.

Hansen & Kalambakidis (2010). JOBZ in MN. Although ostensibly enterprise zone program, most of state outside of Twin Cities metro is eligible. They do a county study where they regress employment growth, and other variables, from 2004 to 2007 on various determinants, including JOBZ jobs created per capita based on 2004 and 2005 contracts. For my analysis, I assume that employment growth must be measured in their study in proportional terms for estimated coefficients to make sense. They report coefficient of 0.5929 on jobs per capita, with t-stat of 1.13, in employment growth regression. With ratio of population to employment of 1.8446 in Minnesota in that period (from BEA data), and multiplier of 2, this implies "but for" percentage of 16.1 percent (= (0.5929/2) / 1.8446)). This is probably positively biased because faster growing counties will be able to apply for more JOBZ job credits.

Lane & Jolley (2009). Evaluate Lee Act incentives (tiered entitlement) and JDIG and One NC incentives (discretionary incentives). Do time series analysis of individual firms receiving incentives. Little evidence that growth trajectory affected in before and after analysis. This is likely to be positively biased, as growing firms more likely to claim incentives.

Chirinko & Wilson (2008). The elimination of ITC would increase user cost by 2.78 percent in ITC states and reduce capital stock by 2.34 percent. Therefore, I use 2.3 percent as "but for." The implicit assumption is this long-run effect of subsidy would be evident in effect on probability of specific location and expansion decisions. No obvious bias.

Hoyt, Troske, & Jepsen (2008). Kentucky. All incentives to firm, located to counties, plus all training grants. 1992–2004. Panel data. Relate employment today to incentives two years ago. Deal with endogeneity largely by testing for lagged causality. I still think employment growth could bias things, despite what they do, so their analysis probably biased in positive direction. In fact, the reverse causality equation is consistent with that—lower employment growth in past is correlated with greater incentives, which we would expect. If you look at annual dollar cost of incentives, versus average county employment, and the elasticities for all counties, you get cost in 2015 dollars of $21,483. With multiplier of 2, this is cost per directly induced job of twice as much at $42,965. This program actually has costs per job that at 12 percent discount rate average $4,546, based on Bartik (2017) database, over the time period used in the paper. So implied "but for" is 10.6 percent.

Lee (2008). Examines the employment behavior and relocation of manufacturing firms, 1972–1992. Uses census data. Looks at behavior over five-year intervals using census of manufacturers. Actual relocations are relatively small. Uses a variety of measures of "incentives," all of which are relatively universal tax breaks that really would be provided to all firms. With state fixed effects, finds some effects on probabilities of shut-downs and relocations, and on average arc percentage employment change of plant. If you add together all the "incentives," collectively doing all of them results in an "effect" at plant level of 11.4 percent. I interpret this as "but for" level of 11.4 percent. No obvious bias.

Minnesota OLA (2008): Based on survey, 19 percent of surveyed businesses would have done same thing, 50 percent would have invested less. I assume that 19 percent plus half of 50 percent would have stayed same, or 44 percent, so 56 percent is implied "but for" percentage. The study does not report sample sizes. This estimate of "but for" is probably positively biased.

Table A2 (Continued)

Calcagno & Thompson (2004). Infer "but for" of zero based on wrong sign for 1981, and 1989 interaction insignificant.

Faulk (2002). Georgia Jobs Tax Credit. Technique is to look at employment change of participating versus nonparticipating firms from 1993–1995 for firms in Georgia, as a function of participating in the program for eligible firms, and with a participation rate equation for why firms participate, with perhaps the key variable being tax liability. For the treated, find that 23.5 percent of jobs created can be attributed to the incentive.

Gabe & Kraybill (2002). Ohio. Got sample of firms that announced expansions between 1993 and 1995. Estimated selection equation for whether these firms also received incentives, where incentives included a wide variety of programs. Have sample of 366 out of 2,400 business expansions that announced creation of 50 or more new jobs, or capital investments of $1 million or more dollars, or facility expansions of 20K square feet or more. Sent questionnaire to 1,000 establishments. Focused on growth of existing businesses, so omitted start-ups or relocations. Also omitted those that didn't provide employment info for year expanded, year prior, and two years later. Growth rate was change in log of employment two years after expansion minus employment in expansion year; 129 received incentives, including JCTC, customized training; infrastructure, and also CDBG dollars. They do three equation models with separate growth equations for firms receiving incentives, and those that don't, and selection equation whose only different variable is county unemployment rate, which positively affects incentives. They look at both announced growth rate and actual growth rate. Incentives *positively* affect announced growth rate but negatively affect *actual* growth rate. This is among sample of firms growing. Therefore, incentive variable is likely to reflect negative selection of firms receiving incentives.

Goss & Phillips (1999). They look at whether the provision of LB775 subsidized investment boosts economic growth by county in Nebraska. They do 2SLS to control for endogeneity, but the instruments, such as past manufacturing investment, are likely to be correlated with current manufacturing investment. They report that $2.8 million in such investment creates 66 jobs. Given that the ratio of such investment to jobs over the entire sample is $136,000, the implication is that 21 subsidized jobs yield 66 total jobs. Even with a multiplier of 2 or 3, this essentially implies "but for" percentage of close to 100 percent. This is probably positively biased because counties that grow more will attract more LB775 investment.

Lynch, Fishgold, & Blackwood (1996). Evaluated IDAs in NY State, which effectively provide long-term tax abatements. Two of 7 relocating said due to IDA; 7 of 22 expanding said due to IDA. So, overall, 9 of 29 said that their location decision was due to IDAs, which yields estimate of "but for" of 31.0 percent.

Loh (1993). Looks at 88 counties in Ohio, growth from 1980 to 1990 as functions of annual spending on various development programs in each county from 1982 to 1990. Has 1980 controls, but almost surely is positively biased. Total subsidized jobs might be 19.8 percent of labor force. This is 21.3 percent of private employment in Ohio in 1980. Effect using dollar terms on employment is 5.22 percent. The "but for" percentage might be 24.5 percent = 5.22 percent divided by 21.3 percent. This is then divided by 2 on the assumption that the average multiplier is 2.