

The Effect of Economic Development Incentives and Clawback Provisions on Job Creation: A Pre-Registered Evaluation of Maryland and Virginia Programs

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Abstract:

Economic development incentives target individual firms for financial or non-financial benefits to induce capital investment or job creation. Previous studies have found a mixed impact of incentives on economic development, with a large percentage of studies pointing to no impact of incentives on economic growth or job creation. I add to this literature by analyzing two different state economic development incentive programs using the same methods and time-period, allowing for direct comparability. My analysis is the first, “pre-registered” study of incentives, where all of the data collection, design and methodological decisions were made and documented prior to receiving the data. Using a pre-registered matching method design, I estimate the impact of Maryland and Virginia’s flagship economic development incentives on job creation. My main finding is that these incentives programs had essentially zero impact on job creation when they are compared to a control group of similar firms and that monitoring of incentive agreements through the use of “clawbacks” did not improve the overall performance of the program.

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1. Introduction:

Incentives ranging from tax abatements to grants have become a ubiquitous tool of economic development for cities and states¹. These programs target firms for special treatment by governments to induce companies to relocate, expand existing operation, or preempt an existing company from moving elsewhere. Politicians' enthusiasm for these programs isn't being driven by academic studies highlighting their effectiveness. Numerous studies find that these incentive programs have limited impact on investment decisions and that the programs' price tag often exceeds their benefits.

While there is a growing consensus on the potential problems with these programs, there is considerable heterogeneity in the literature across programs that has led to numerous studies of individual incentive programs. In this paper, I add to this debate by analyzing two flagship state incentive programs, one from Virginia and one from Maryland. I then estimate how these programs impact job creation.

I make two advances from previous studies. First, I use the same methods and data sources to generate two comparable estimates of the job creation impacts of these different state programs during the same estimation window (2006-2012). For both studies, I compare the job creation performance of incentivized firms with a control group of firms using the same variables for matching. This provides a clear comparison of these programs in terms of their effectiveness in targeting firms.

Second, my analysis, to my knowledge, is the first "pre-registered" study of the impact of investment incentives.² My data collection methods, coding decisions, and estimation strategy were all pre-specified *prior* to data collection. My pre-registration document (archived prior to receiving

¹ Klein and Moretti (2013).

²

the data) identifies my research design of matching incentivized firms to non-incentivized firms using coarsened exact matching (CEM) to compare firms receiving incentives with a control group of firms. My pre-registration document provided full details on my estimation strategy and mock tables that were left blank. These tables, complete now with estimates of the impact of incentives on job creation, are central to this paper (Table 2 and Table 3).

My findings cast considerable doubt on the effectiveness of these programs. For both the Maryland and Virginia programs, incentivized firms have no greater proclivity to generate jobs than comparable firms that did not receive incentives. Not only are the estimates statistically indistinguishable from zero, the substantive sizes of the estimates are small. The most generous estimate of the impact of incentives on jobs is equivalent to two jobs per incentive (relative to an average incentive of over \$400,000).

To assure compliance with incentive agreements, both Virginia and Maryland monitor performance (including job creation) and have the ability to cancel incentive agreements and claw back incentive rewards. Even when I purge these worst performing companies from the data set, I still find no impact of incentives on job creation.

My findings provide further criticisms of incentive programs using a pre-registered research design. In the conclusion, I describe how pre-registration can be used for internal evaluations or government oversight of incentive programs. Incentive programs can provide clear details on their evaluation methods that are agreed upon by all parties. This provides an additional benefit of limiting the politicization of these programs.

2. Theory and Context

Economic development incentives have become one of the most common tools utilized by US states and municipalities to help generate jobs, costing an excess of \$50 billion a year (Thomas

2000). Jensen (2016) reviews research on incentive programs across the globe, finding a common pattern of the ineffectiveness of these incentives.³ This is at least partially due of the “redundancy” of incentives, where research such as James (2009) shows that 70% of incentive are allocated to companies that were already going to invest or expand. Thus, they are provided incentives for engaging in an activity that they would have done absent incentives. To quote Paul O’Neill, former CEO of ALOCA, “As a businessman I never made an investment decision based on the tax code. If you give money away I will take it, but good business people don’t do things because of inducements” (O’Brian 2006).

What is the evidence for the use of incentives for job creation in the United States? In an earlier review of over 300 incentive studies, Busse (2001) finds mixed results. Some studies of foreign firms, such as Bobonis and Shatz’s (2007) analysis of German manufacturing location decisions in the United States find incentives are ineffective. Cross-states studies such as Patrick (2014) find that incentives, if anything, have a negative impact on employment.⁴

These studies of incentives in the United States require us to make comparisons across incentive programs. More fine-grained studies of incentives have also isolated the impact of particular programs by states or municipalities. These incentive programs have been found to help generate employment in the Atlanta metro region (Bollinger and Ihlanfeldt 2003) and more mixed evidence in other regions (Wassmer and Anderson 2001; Walker and Greenstreet 2005). Other studies of Ohio (Gabe and Karybill 2002) and Michigan (Hicks and LaFaive 2011) find incentives are ineffective at creating jobs. Lester et al (2014) find that North Carolina’s economic development incentives are mediated by broader sectoral based support.

³ For additional summaries see Morisset and Pirnia 1999; Zee, Stotsly, and Ley 2002; Blomstrom and Kokko 2003; Easson 2004; LeRoy 2005; Klemm and Van Parys 2012; and Reese 2014. For research on the how incentives can generate growth and jobs see Klein and Moretti (2014).

⁴ See also Peters and Fisher (2004).

Also important for this discussion are the costs of job creation from these incentive programs relative to other alternatives. For example, Hanson and Rohlin (2011) find that enterprise zones are associated with job creation, but due to the small impact and job losses in other firms this amounts to a cost per job of \$2.9 million. A more modest estimate can be derived from simulations from Funderberg et al (2013, 557), in which they conclude:

When we isolate the value of industrial incentives from the basic tax system in our theoretically preferred marginal tax measure, we find that a 10 percent reduction in liability achieved by way of lowering taxes is associated with a 4.5 percent increase in value added while an equivalent reduction achieved by way of increasing incentives is associated with only 1.2 percent industrial growth, the latter elasticity not statistically different from zero.⁵

Reading the overall evidence on these incentives programs leads many scholars to express skepticism towards the use of incentive programs. One of the most recent works on the subject by Greenbaum and Landers (2009) uses the provocative title: “Why Are State Policy Makers Still Proponents of Enterprise Zones? What Explains Their Actions in the Face of a Preponderance of Research?”. Recent work has suggested that the competition for investment has led to a “cycle of destructive competition” in the use of incentives (Zheng and Warner 2010).

These studies provided limited comparisons across programs. In this paper, I examine two separate incentive programs, estimating the impact of the programs on job creation in Virginia and

⁵ Funderberg et al (2013, 575) make clear the limitations of their approach noting that: “we are prevented from concluding that incentives are more costly than reductions in basic system taxes to generate the same levels of new investment. The tax rates in this scenario are calculated as potential liability, distinct from the endogenous tax rates based on revenue. Costs will depend on the amount of exercised incentives that does not result in new investment. We can, however, conclude that incentives are much less effective than basic taxes at achieving a targeted level of new investment in this scenario.”

Maryland, and compliment published work on the main Kansas economic development program (Jensen 2016). Do these incentive programs generate jobs or do they incentivize companies that were already expecting to create jobs? This leads to the main hypothesis in this paper:

H1: Firms that receive incentives generate more jobs than a matched set of firm that did not receive incentives.

This main hypothesis is important for our understanding of incentive programs, but as outlined in the pre-registration document, numerous governments and economic development agencies have reformed their incentive programs to better assure taxpayers that economic development efforts are being monitored to assure compliance with the agreement between the investor and agency. Most notably, many states and municipalities now employ controls, such as clawback provisions as part of their incentive programs.⁶ These clawback provisions are designed to monitor compliance with incentive agreements by canceling incentives with under-performing companies and often reclaiming incentives back from those failing firms.

States and municipalities can use these controls to mitigate the risks associated with these economic development policies, potentially reducing the risk that scarce economic development dollars are wasted (Sullivan and Green 1999). Sullivan (2002) finds that municipalities that make the most extensive use of incentives are also the governments that employ the strongest controls. Sharp and Mullinex (2012) and Jensen, Malesky and Walsh (2015) find that form of government shapes the implementation of oversight and clawbacks at the local level. Peters (1993) examines the application

⁶ This includes the following programs: Florida Quick Action Closing Fund, Georgia EDGE Fund, Illinois EDGE Tax Credit, Indiana EDGE Tax Credit, Massachusetts Economic Development Incentive Program, Michigan MEGA Tax Credits, Minnesota JOBZ Incentives New Jersey Business Employment Incentive Program, New York Empire Zone Program, North Carolina Job Investment Development Grant, Ohio Job Creation Tax Credit, Pennsylvania Opportunity Grant Program, South Carolina Governor's Closing Fund, Texas Enterprise Fund, and the Wisconsin Enterprise Zone Tax Credit.

of incentives across the Midwest, finding substantial differences in the application of clawbacks. Ledebur and Woodward (1990) provide advice on different forms of clawbacks that could be implemented by states.

Unfortunately, little research has focused on the *effectiveness* of clawbacks in policing the use of incentives.⁷ As outlined in my pre-registration document, I will perform an additional subgroup analysis, dividing companies that were subject to clawbacks based on job creation thresholds from companies that were not subject to clawbacks. Does the average job creation of firms remaining in the program improve after removing those firms that are sanctioned for not meeting their requirements?

3. Data Description

To analyze the impact of incentive programs on job creation, I chose the two flagship incentive programs in the DC metro area. The Virginia Governor's Opportunity Fund (GOF) and the Maryland Economic Development Assistance Authority Fund (MEDAAF). Both programs are major state level incentive programs that provide discretionary incentives to companies.

For both programs, I obtained information on their incentive offerings from open record requests at relevant agencies in both states. I then matched this data with a larger database of essentially all firms in Virginia and Maryland to generating a control group of firms. I provide details on all three of these data sets.

3.1 Virginia Incentive Data

⁷ See Mattera et al 2012 for a discussion of clawbacks and an analysis of the use of clawbacks across a large number of programs.

Virginia's flagship incentive program, the Governor's Opportunity Fund (GOF), renamed the Commonwealth Opportunity Fund, is the focus of the first part of my project. The Governor's Opportunity Fund, often called a deal closing fund, provides considerable discretion for the governor in the allocation of loans and grants to investors. The program is meant to attract capital and jobs to Virginia and is administered by the Virginia Economic Development Partnership.

The program requires a local matching incentive by municipalities interested in applying for the grant. As described on the program website⁸:

The Commonwealth's Opportunity Fund (COF), formerly known as the Governor's Opportunity Fund (GOF), is a discretionary incentive available to the Governor to secure a business location or expansion project for Virginia. Grants are awarded to localities on a local matching basis with the expectation that the grant will result in a favorable location decision for the Commonwealth.

The program provides grants to firms, in support of municipalities' applications for incentives. Companies are required to meet numerous requirements on job creation, wages, and the amount of capital investment. These firms are required to report annually on their fulfillment of these targets and are subject to clawbacks if criteria are not met. An analysis found that from 2011-2015 this program is credited with 126 projects creating 21,702 jobs at an average cost of \$3,045 per job, if we assume that all the jobs associated with the program would not have been created absent incentives.⁹

In an email open records request on November 12, 2015 to the agency responsible for the program (YesVirginia), I requested data on incentives across the lifetime of the program. YesVirginia provided full documentation of their incentive allocations since the start of the program as well as information on the clawbacks at no cost to the researcher. This includes a total of 551

⁸ <http://www.yesvirginia.org/ProBusiness/BusinessIncentives>

⁹ <http://www.virginiaallies.org/assets/files/publications/HB1191.pdf>

projects for a total of \$234 million in incentives. These incentives are associated with \$22.4 billion in investment and 119,178 total jobs.

For compatibility with previous work, I will focus on incentives allocated from 2006-2012 (227 incentives). Each one of these incentives will be attributed to a single establishment using Dun and Bradstreet unique identifiers (DUNS numbers). Incentives will then be coded as either in good standing or subject to a clawback.¹⁰

3.2 Maryland Incentive Data

Closely following the Virginia analysis, I next focus on the Maryland Economic Development Assistance Authority Fund (MEDAAF). This program, originally created as a subsidized business loan program in 1999, absorbed ten standing incentive programs in 2000, and another program in 2004 (Maryland Department of Business and Economic Development 2012). This program now contains a heterogeneous mix of incentives from traditional business incentives, to day care centers, to direct financial support to local economic development budgets. I focus my analysis on traditional incentive programs provided to businesses for capital investment and job creation.

Through an email public records request on November 12, 2015 the Maryland Department of Economic Development provided me annual reports on their incentive performance. According to their 2015 annual report, to date, the Maryland Economic Development Assistance Authority Fund (MEDAAF) provided incentives of \$222.6 million to 496 investments. These incentives have been associated with retention of 29,578 jobs, 20,685 new jobs and \$3.8 billion in investment.

¹⁰ In the footnotes of excel file provided by the state 67 of the total incentives had additional documentation, including 57 incentives associated with a clawback. For example, Gateway's incentive contained the following note: "Original grant was for \$1,500,000; \$453,472 returned to date due to company not meeting thresholds." Any company that is identified for not meeting GOF thresholds is coded as clawed back.

Along with their annual reports Maryland's Department of Economic Development provided an excel worksheet of 113 investments that were associated with clawbacks.

3.3 The National Establishment Times Series Data (NETS)

Following Jensen (2016) I utilize the National Establishment Time Series (NETS) data to match Virginia and Maryland establishments that were provided incentives with similar firms in the state that were not provided incentives. The NETS database provides detailed information on essentially the full universe of firms in both states (described in Jensen 2016). I merge that Maryland MEDAAF and Virginia GOF data into the NETS data using Dun and Bradstreet numbers (DUNS). Some firms have gone bankrupt, relocated, been acquired, or otherwise are no longer in the database. Thus, my analysis will only compare surviving firms (firms with DUNS numbers) in the analysis.

4. Pre-Registered Research Design

Across disciplines, researchers are confronting the potential for publication bias in academic research. Put simply, publication bias is a problem that certain types of studies are more likely than others to get published due to factors not directly relative to the quality of the research. Most commonly, publication bias refers to the research and peer-reviewer evaluations of studies based on the statistical significance of their results and not the quality of the data, novel theory, or appropriateness of the analysis. In the content of this paper, a research project that finds a strong positive or negative impact of incentives on job creation is more likely to be published than a study that shows no effect.

This can lead to two related problems. First, scholars can either decided to not submit papers with null results to journals, the so called "file drawer problem" (Franco et al 2014). Alternatively, researchers may run a number of alternative specifications until a statistically

significant finding appears in their data analysis. Even experimental work is subject to these manipulations, where researchers selectively report some experimental results (Franco et al 2015). Second, publication bias could simply be a result of the review process where peer-reviewers are more likely to reject papers, *ceteris paribus*, that have null results and thus only strong negative or positive findings survive the peer review process (see Findley et al 2016). In either case, the distribution of studies in academic journals is towards papers that achieve (often barely achieve) statistical significance ($P < 0.5$).

To address this issue of publication bias—specifically p-hacking by researchers—scholars in fields including economics, medicine, political science, and psychology have adopted pre-registration as a means of providing greater transparency in the research process (Humphreys et al 2013; Miguel et al 2014; Olken 2015). A pre-registration is simply a detailed plan by the researcher specifying the theory, data, and analysis prior to beginning a research project. Obviously, researchers encounter numerous challenges in the actual implementation of their projects, and are thus not legally or even ethically bound to follow their plans precisely. Nevertheless, researchers often feel compelled to document any changes because the original plan is publicly available.

For this project, I pre-registered by data, analysis and provided a mock table of the core results. This plan was archived at the Evidence in Governance and Politics design registry (<http://egap.org/design-registrations>) prior to obtaining any of the data. Thus, the analysis presented in Table 2 and Table 3 matches the pre-registered research design document prior to beginning this study.

My analysis utilizes coarsened exact matching (Iacus et al. 2012) to compare Maryland and Virginia incentive recipients with a matched set of control firms. I specifically match firms based on the natural log of employment in 2006, the three-digit Standard Industrial Classification (SIC) code,

and a dummy variable if the firm was a subsidiary of a parent company. I registered the following hypothesis for the evaluation of the program:

H1: Firms that receive incentives generate more jobs than a matched set of firm that did not receive incentives.

5. Results

For all models, I use the natural log of 2012 employment at the level of the establishment as the dependent variable. I present the results of this study in Table 1. The first model takes the naïve approach and simply examines the relationship between jobs in 2012 and whether or not a firm receives incentives using OLS. This doesn't control for the fact that the firms that applied for incentives were already larger than the non-incentivized firms, nor does it account for other factors that could affect employment. Model 2 includes a variable for the natural log of employment in 2006.

Table 1: Matching Estimates of Job Creation (Virginia)

	(1) OLS	All Firms (2) OLS	(3) CEM	Disqualified (4) CEM	Approved (5) CEM
Incentive	1.895*** (0.230)	0.263 (0.163)	0.443 (0.288)	0.253 (0.809)	0.470 (0.309)
2006 Employment		0.929*** (0.001)			
Constant	2.839*** (0.045)	0.281*** (0.021)	3.242*** (0.006)	3.242*** (0.06)	3.242*** (0.06)
N	716,342	362,149	149,009	149,009	149,009

Note: Models 1 and 2 are ordinary least squares regressions with the natural log of 2015 employment as the dependent variable. Models 3-5 present coarsened exact matching estimates using the natural log of 2006 employment, a dummy for subsidiary (as opposed to a parent firm), and three digit SIC dummy variables.

Model 3 presents the direct test of Hypothesis 3 using coarsened exact matching (CEM) estimates using previous employment, three-digit industry code, and a dummy variable for whether or not the company is a subsidiary of a parent company for matching. This matching allows us to

generate the counterfactual by comparing job growth across similar firms. I present the kernel density before matching and after matching in Figures 1A and Figures 1B providing visual evidence of comparability of incentivized and non-incentivized firms after matching.

The main finding from this matching is that incentives have no discernable impact on job creation. This null result may be surprising to some readers, where the use of incentives should be tied to job growth. Yet evidence directly from the state's "Announcement" data base on company relocations and expansions suggests that these incentivized firms are only a small subset of all firms that expanded or relocated to Virginia.¹¹ Of the 2,422 expansions or relocations from 2006-2012, only roughly 5% were provided economic development incentives through this program. The matching estimates suggest that job growth in the GOF firms would be similar even without the incentive.

In Models 4 and 5, I categorize firms that are in good standing or were subject to "clawbacks" by the states. These clawbacks are simply coded based on FOIA requests from both states indicating that the incentives were cancelled and/or part of the incentive was repaid for non-compliance with the incentive agreement. In Model 4, I estimate the impact of participating in the incentive program on employment for the disqualified firms. In Model 5, I perform coarsened exact matching that no longer includes the disqualified firms in the estimates.

¹¹ <http://vedpweb.yesvirginia.org/announcements#/NewExpanding>

Figure 1A: Kernel Density before Matching (Virginia)

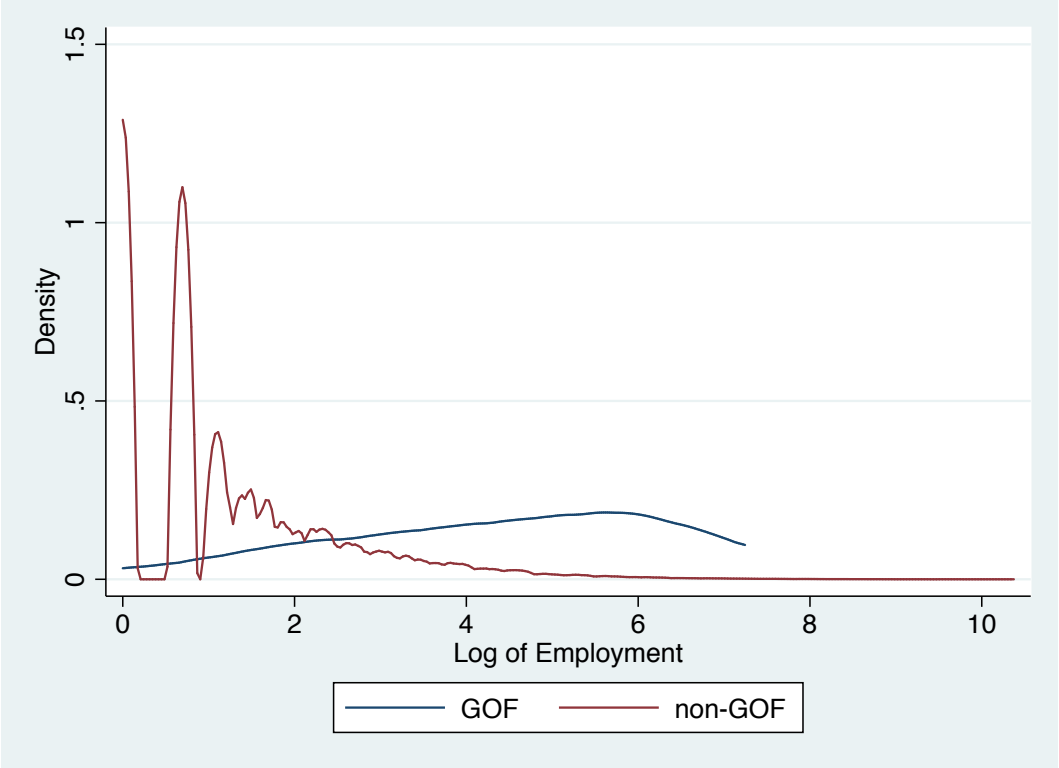


Figure 1B: Kernel Density after Exact Matching (Virginia)



In Table 2, I present the same models, this time focusing on the Maryland MEDAAP incentive program. In Figure 2A and 2B, I present the kernel density before and after matching. These results from Table 2 are consistent with the results from Table 1. While MEDAAP firms have larger numbers of employees in 2012, this is largely due to larger firms participating in the program. When previous employment (2006 Employment) is considered, these incentives have a very small impact on job creation. Models 3-5 present the matching estimates, mirroring the previous tables' results. The MEDAAP program has no impact on employment creation, even when we consider the use of clawbacks.

Table 2: Matching Estimates of Job Creation (Maryland)

	All Firms			Disqualified	Approved
	(1) OLS	(2) OLS	(3) CEM	(4) CEM	(5) CEM
Incentive	1.680*** (0.241)	0.288** (0.123)	0.008 (0.263)	0.747 (0.640)	-0.142 (0.289)
2006 Employment		0.936*** (0.001)			
Constant	2.548*** (0.061)	0.265*** (0.028)	3.152*** (0.005)	3.151*** (0.05)	3.152*** (0.05)
N	524,527	237,770	138,710	138,710	138,710

Note: Models 1 and 2 are ordinary least squares regressions with the natural log of 2015 employment as the dependent variable. Models 3-5 present coarsened exact matching estimates using the natural log of 2006 employment, a dummy for subsidiary (as opposed to a parent firm), and three digit SIC dummy variables.

In summary, both analyses point to the ineffectiveness of these incentive programs. Even for the Virginia Governor's Opportunity Fund, which also includes a matching local grant, I find no impact of these program on job creation. Both states have economic development programs that utilize clawbacks to assure that firms comply with incentive agreements. My results suggest that compliance with agreements, even if perfect, doesn't address the underlying redundancy issue with these programs. If incentives are given to companies that are likely to be expanding employment even without incentives, these programs are providing very few jobs.

Figure 2A: Kernel Density before Matching (Maryland)

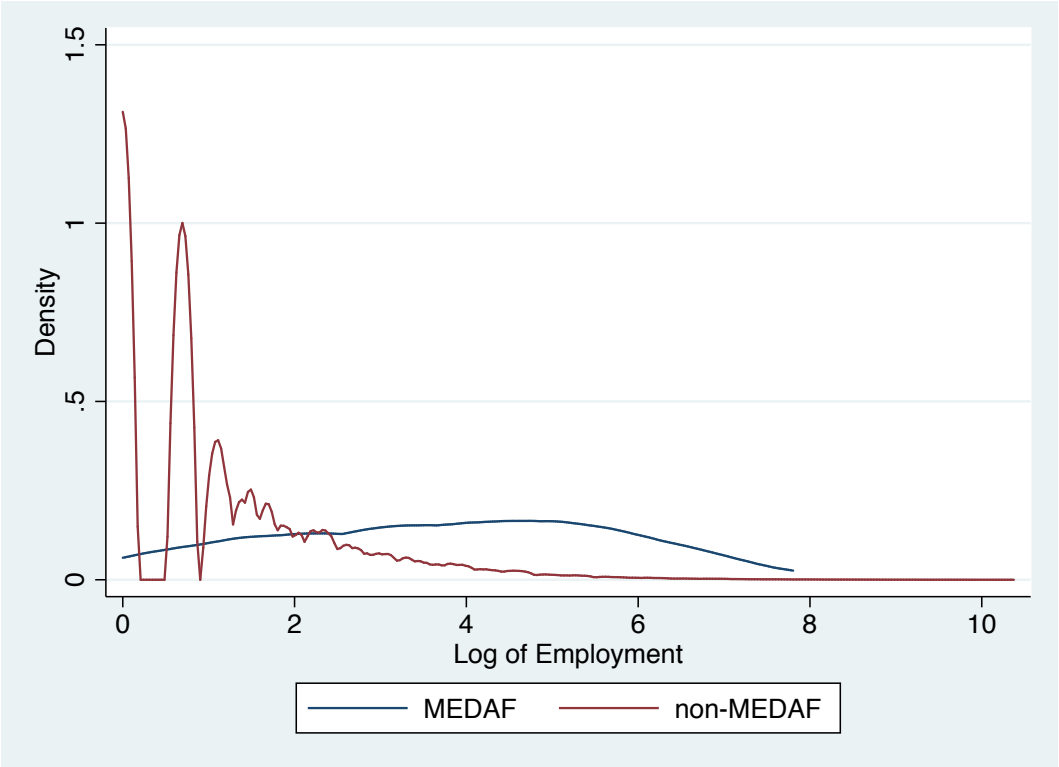
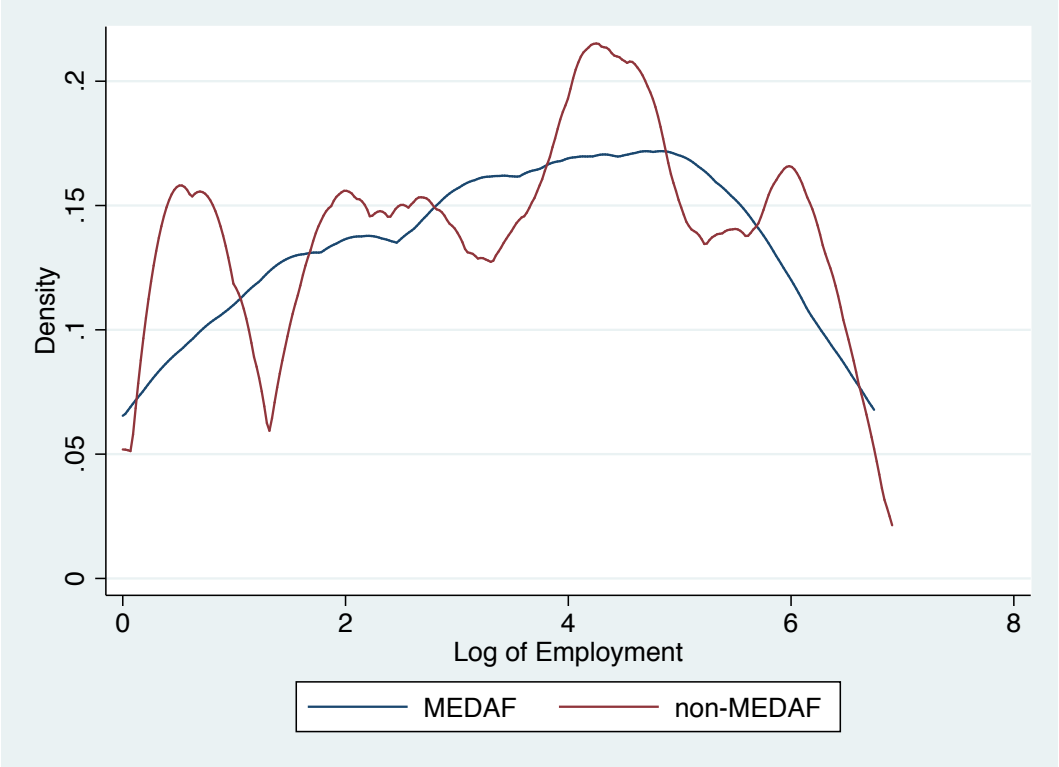


Figure 2B: Kernel Density after Exact Matching (Maryland)



Conclusion

In this paper, I analyze the flagship Maryland and Virginia economic development programs, matching incentivized firms with non-incentivized firms and coding if the incentivized firms were subject to a clawback for failing to comply with the incentive agreement. My findings point to the ineffectiveness of both programs for job creation in the 2006-2012 time period. This result is one contribution of this study, but the larger contribution is methodological.

My analysis is the first pre-registered study of incentives, where the details for the project were pre-specified before purchasing the data. This limits researcher discretion in the analysis, and generally requires documentation of deviations from the pre-registered design. Note that there is nothing legally binding from this registration, only that the research process—from hypothesis generation, to measurement, to analysis—is made transparent prior to conducting the study. Researchers can deviate from the registered design and can engage in induct data analysis, but the important thing is that pre-registration makes this transparent.

Pre-registering incentives can also mitigate potential bias in the self-evaluation of economic development incentive programs by economic development agencies, legislative audits, or other government actors. Many audits have become contentious: the Kansas Legislative Audit has been a back and forth between auditors and economic development agencies. Pre-registered analysis plans can allow all parties to agree on the proper way to evaluate incentives prior to data collection. This could lead to a more productive focus on how to best analyze economic policy, rather than defending or attacking the results of a program evaluation.

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