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Striking a Balance: A National Assessment of Economic Development Incentives

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ABSTRACT

The use of incentive packages has intensified as local governments compete for new plants and corporate relocations, and as private firms increasingly demand a deal. While incentives promise jobs and tax revenue, scholars and practitioners criticize their high cost and limited accountability. Through a comparison of matched establishments, this paper explores how governmental incentive-granting strategy impacts incentive performance. We examine the overall impact of incentives and whether incentives granted to smaller firms perform better. Using economic development budget data, we also assess the state's overall approach to economic development to determine which strategies are prioritized through funding. By showing that incentivized firms fail to create more jobs than matched controls, our analysis casts doubt on claims that "but for" incentives job creation would not occur. Still, our findings suggest that states are smarter in their incentive use when they strike a balance between recruiting industry and supporting "homegrown" businesses and technology.

JEL Classification Codes: R0, R3, R5, H2, H7

Key Words: Incentives, mediating policies, employment, equity, economic development

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Incentive packages to private businesses have become the modus operandi among state and local governments competing for large-scale corporate relocations and new branch plants, bringing the promise of increased employment and tax revenue. But incentives are also widely criticized, with scholars and practitioners equally frustrated by their limitations and costs. As private businesses increasingly demand incentive payments to locate or remain within a region, economic developers have responded with efforts to make incentive payments more effective by using a mix of more sophisticated analytical tools, integrating incentives within broader sectoral or industrial targeting strategies, or adding additional checks to curb excessive incentive use.

New data sources create the possibility to study some of these moderating steps and explore their varying effect on incentive performance. In this paper, we present the results of a national, time-series, establishment-level study of state and local economic development incentive deals, comparing establishments that received an incentive package to a control group of similar, nonincentivized establishments. While incentives can be used to stimulate a range of economic outcomes, they are most frequently used with job creation and job retention goals in mind. Our analysis focuses on employment effects of incentivized economic development deals by using three national databases: the Good Jobs First Incentive Database, the National Establishment Time-Series database, and the State Economic Development Expenditure Database from the Council for Community and Economic Research.

Combining these data sources, we seek to answer three interrelated questions: First, do establishments that receive incentive details outperform nonincentivized firms? Second, does prioritizing incentives to establishments within a region's targeted or specialized industries enhance the efficacy of the incentive? Finally, do states with more balanced economic development "portfolios" (i.e., similar public investment in recruitment and entrepreneurship)

make more effective use of their incentive dollars with respect to employment ends, when compared to those states with unbalanced portfolios?

Our findings will inform critical policy debates surrounding economic development incentives. By conducting the first nationwide study with a causal research design, we provide new empirical evidence that shows that economic development incentives, on average, fail to produce new employment opportunities. By comparing incentivized establishments to a carefully selected control group, we cast doubt on the biggest claim made by incentive proponents that “but for” the incentive payment, job creation would not occur. This simple but direct finding—that incentives do not create jobs—should prove critical to policymakers. However, we also show *how* incentives can be more effective by examining the disparate impacts by firm size. Here, we find that incentives granted to smaller establishments have better performance in terms of job creation compared to very large establishments, which we find to have starkly negative employment effects. Finally, we show that these large negative impacts for larger establishments are mitigated in states that “balance” their economic development spending portfolios between traditional business attraction activities and support for entrepreneurial development. Before turning to our analysis, we provide an overview of the literature on incentive granting and common strategies to enhance incentive performance.

MOTIVATION AND LITERATURE REVIEW

Incentives have been widely criticized by economic development scholars, and for good reason (Eisinger 1988; Hanley and Douglass 2014; Markusen and Nesse 2007). Record-breaking mega-deals given to large multinationals involving millions of dollars are particularly concerning. Not only do these deals subsidize already wealthy corporations, they often fail to

perform as hoped by issuing municipal or state authorities. Much is known about these high-stakes incentive “failures,” including questionable practices by some footloose corporations that jump from location to location to extract additional rounds of public funding and support (Freyer 2017; LeRoy 2005). The result is not only increased interjurisdictional competition, but at best a “zero sum” outcome for national economic development (Chirinko and Wilson 2008; Malizia 1994).

Many economic development practitioners would agree that, all things equal, they would much rather retire tax incentive programs and related corporate “giveaways” (Rubin 1988). These programs often draw down essential yet scarce sources of public funding that could be better spent on other kinds of economic and community-enhancing activities, including public education, vocational training, or income-stabilizing social services (Bartik 2014). Yet empirical studies show that the practice of offering economic development incentives is deeply entrenched, having survived decades of legislative and lay challenges (Bartik 2017; Pollard 2015). In fact, state and local incentive use in the United States has seen a notable uptick in the wake of the 2008–2009 Great Recession (Osgood, Opp, and Bernotsky 2012; Warner and Zheng 2013), reflecting increased desperation for additional jobs. We need to look no further than Wisconsin’s \$3 billion incentive package to lure Foxconn in 2017, or more recent frenzy around Amazon and Google’s decision to establish a second U.S. headquarters to see that communities throughout the country and decision makers within them are willing to pull out all the stops.

But this rise in incentive use is also happening at a time when many U.S. cities and communities are facing hardening budget constraints (Donald et al.2014; Pollard 2015) and thus need to be smarter with their public investments and avoid costly deals that fail to deliver on their promises. With this in mind, we add our voices to a growing chorus of economic

development scholars and incentive reform advocates that are pushing to refocus debate over incentives from a simple yes or no proposition to a more nuanced consideration of how to improve incentive-granting implementation and practice—that is, how to ensure state and local governments more effectively allocate and manage incentives to support their intended economic development goals?

Scholarship on better incentive granting points to three reinforcing strategies, starting with actions taken to improve the inner workings of any incentive deal. Research in this area recognizes the transactional nature of incentive granting and thus opportunities for economic development practitioners to better negotiate contractual terms and conditions when extending a publicly funded incentive or subsidy to a private company (Weber 2007). At a basic level, practitioners can reduce community risk by including clawbacks and related performance mechanisms in their written contracts, thus ensuring they have legal recourse to recapture public money from companies that fail to meet or honor these terms (Ledebur and Woodward 1990; Markusen and Nesse 2007; Sullivan 2002; Weber 2007). But equally, practitioners can also secure encompassing, high-impact “community benefits” by linking incentive offers to explicit requirements for living wages, job training guarantees, and targeted local hiring provisions (Bartik 2005; Dewar 2013; Fitzgerald 2004; Lester 2011; Lowe and Morton 2008; Weber 2002; Wolf-Powers 2010).

Incentives can also produce stronger economic and employment returns when channeled toward smaller-sized establishments (Bartik 2018). That said, the general literature on economic development incentives has been rather agnostic to questions of firm size and thus has overlooked the potential for differential employment outcomes when incentives are given to smaller rather than larger firms. Important exceptions are studies that have focused on the use of

geographically bound incentives to revitalize blighted or underdeveloped urban neighborhoods and as part of state urban enterprise zone programs (see Wilder and Rubin [1996] for a review). While these studies find considerable variation in incentive performance at this microgeographic scale, a key take-away is that job growth is greatest and most consistent when incentives are used to support smaller-sized establishments, including expansion of incumbent businesses already located within the zone. Interestingly, employment outcomes also intensify in enterprise zones that layer on multiple incentives and target programmatic support to smaller firms (Harrison and Glasmeler 1997; Wilder and Rubin 1996).

At one level, this outcome reflects the fact that smaller, especially younger firms have greater propensity to add more jobs relative to larger, more mature establishments—a distinction noted in numerous studies of small business growth trends in the United States (Birch 1979; Davis, Haltiwanger, and Schuh 1996). But there is a moderating element at play here as well, insofar as economic development practitioners are more likely to gain the upper hand when negotiating incentives with smaller, resource-constrained firms (Weber 2007). Additionally, as they engage these firms, state and local economic developers can strengthen funding impacts by coupling incentives with other forms of small-business-oriented assistance (Wilder and Rubin 1996). As this implies, incentive outcomes are not just affected by the actions of a skillful deal maker—they can be furthered magnified through interventions that simultaneously resolve constraints that certain businesses face because of their smaller size or related organizational limits.

But there are additional gains to be leveraged when incentive granting is treated as one element of a much larger and more sophisticated strategy of industrial recruitment (Lowe 2014). The focus is not the incentive, *per se*, but the larger industrial recruitment strategy—more

specifically, the selective use of recruitment to build-out existing industrial strengths and the promotion of nonincentive advantages in order to attract, anchor, and retain firms within specialized sectors (Feser and Luger 2003; Goetz, Deller, and Harris 2009; Lowe and Freyer 2015). Strategies of targeted recruitment can also help reduce demand for cash-equivalent incentives by shifting the focus toward other regional advantages that offer greater value to firms as well as their host communities, including access to quality infrastructure, transferable workforce skills, and specialized industry support services or institutions (Bartik 2005, 2018; Lester, Lowe, and Freyer 2014; Schunk and Woodward 2003). In this respect, targeting reinforces what most scholars and practitioners already know: incentives are rarely a crucial factor in firm location. They may tip the balance but only once other factors are in play (Bartik 2005; LeRoy 1997).

These two approaches—better incentive deal making and targeted recruitment with selective incentive use—have received considerable attention from economic development scholars in recent years. However, most incentivized deals entail funding tools that are themselves embedded within an even larger state economic development system (Lowe and Feldman, forthcoming). Few states focus their entire economic development effort on incentive granting for business attraction or retention purposes; rather, most have other economic development programs in place, including robust funding streams to boost entrepreneurship and small business development; assist business and industry expansion through market development, including international trade; and even strengthen local economies through recurring investments in infrastructure and community development (Hanley and Douglass 2014).

Scholars tend to study each of these strategy areas in isolation, at times casting the others as competing policy priorities (Ross and Friedman 1990). Yet practitioners, for their part, take a more integrated approach. To illustrate this point, the director of North Carolina's Rural Economic Development Center recently used the analogy of a three-legged stool to describe economic development practice in that state. In public comments before a university economic development working group, he noted that one leg represents strategies that focus on retaining incumbent firms and supporting their continued growth, the second leg is industrial recruitment or attraction of new businesses to the state, and the third focuses on creating and nurturing homegrown entrepreneurial firms, technology, and talent. By default, if one leg is removed the stool would fall over because the weight of the economy would no longer be equally distributed.

But there is a stabilizing element in play with implications for increasing the effectiveness of incentive granting. The three elements of the economic development stool help strike a balance between exogenous and endogenous development strategies—that is, between recruitment business to a region versus supporting homegrown development. Each leg adds to the soundness of the state's overall economic development program, reducing the risk that any one leg will dominate and risk destabilizing the whole. Extending the analogy further, it could be that this mix of multiple budget priorities creates a form of “productive friction” (Stark 2011), in turn helping to moderate excessive or inappropriate incentive use. Additionally, this balanced portfolio could encourage new, creative forms of cooperation and knowledge sharing among practitioners, including the possibility for traditionally distinct areas of economic development practices to be better sequenced or intentionally coproduced and combined (Lowe and Feldman Forthcoming; Lowe and Wolf-Powers 2018). With this in mind, and including the aforementioned moderating steps of industry targeting and enhanced deal-making with smaller

firms, we turn to new national data on state incentive deals to explore the following three interrelated questions:

- 1) Do incentives perform better when granted to smaller-sized firms?
- 2) Are there benefits from industry targeting?
- 3) Do incentives perform better in states that adopt a balanced economic development portfolio, especially when they treat business recruitment and entrepreneurship as equally important strategy areas?

DATA AND METHODS

Data Sources and Sampling

We use three sources of data to build a national, time-series, establishment-level database: the Subsidy Tracker from Good Jobs First (GJF), the National Establishment Time-Series database (NETS), and the State Economic Development Expenditure Database from the Council for Community and Economic Research (C2ER). First, we use GJF to identify establishments that have received either a state or local (county or municipal) subsidy. While there is no single public source of incentive deals in the United States, the GJF data set is the most complete and comprehensive listing of deals and includes information on firm name, location (state, with some records including city, county, and street address), deal year, number of jobs promised, and incentives offered.¹ We downloaded a snapshot in May 2016, when the database contained 500,593 subsidy deals. To create our sample, we exclude federal and Washington, DC, deals (184,688 and 342, respectively), since a key purpose of this paper is to

¹ Good Jobs First (GJF), the developer of the searchable Subsidy Tracker database, is a nonpartisan, nonprofit national policy resource center. Subsidies recorded in their Subsidy Tracker include those issued for property tax abatements, corporate income tax credits, sales tax exemptions, and workforce training, among others.

assess the relationship between state budgeting patterns and deal efficacy. We next remove deals issued either before 1995 or after 2010 (28,902 and 132,071, respectively), as those deals will not allow for pre- or posttest employment trends. We exclude 199 deals recorded as “megadeals,” as those rare deals limit the external validity of findings. Given that a primary interest is postdeal employment changes, we exclude deals not promising to either retain or increase employment by 100 employees or more, bringing the sample down to 13,324 deals. Finally, to aid in matching these deals to their records in other data sources, we removed 2,048 deals that did not include city or county locations. This brought the sample down to 11,276 deals.

Next, we gather data on our GJF treatment establishment sample from the NETS, a national, private, longitudinal database comprised of 25 snapshots of Dun & Bradstreet (D&B) data (1990–2014). Each establishment has a D-U-N-S Number (the unique D&B identifier for each establishment) and allows us to gather data on firm name, location (state, county/municipality, and address), subsidiary status (i.e., whether the establishment operates as a subsidiary of another establishments), SIC code, employment, and sales. For a discussion of the use of NETS data to assess the efficacy of subsidies, see Lester, Lowe and Freyer (2014).

We rely on a text-matching algorithm that minimizes differences in company names to locate GJF records in the NETS.² Matches must meet four criteria. First, the GJF company name and the NETS company name must be recognizably similar. While an exact match in names would be ideal, firms occasionally change names or spellings; databases (in our case, both GJF and NETS) may shorten or misspell a firm’s name. Second, the NETS record must be in the city or county recorded in the GJF database either at the time of the subsidy deal or in the following

² The program relies on the COMPGED function in SAS, which calculates the Levenshtein distance between two string variables (Staum 2007). The algorithm calculates the distance between the GJF firm name and each firm name the NETS, sorts the numerical distances from smallest to largest, and returns the 20 pairs with the smallest numerical distances (see Donegan [2016] for full discussion).

three years. Third, the subsidy had to be the first deal the establishment had received—and not a repetition of an earlier deal. Fourth, if multiple NETS establishments in the city or county meet the first three criteria, no match is made and the deal is discarded. Two researchers reviewed algorithm results to ensure records marked as matches met all four criteria. Our final sample includes 2,604 subsidy deals, corresponding to 3,321 records in the NETS.³

Our third data source, C2ER, uses consistent funding categories to aggregate state budget data for economic development activity across 15 functional areas and multiple funding sources in 2007. We choose the data from 2007, rather than a more recent year (2013–2017), as it approximates the midpoint of the period in which we observe subsidy deals and thus more accurately reflects the policy orientation of states within the study period. We focus on two distinct categories of economic development funding. The first group, which we call entrepreneurial, includes the share of the state’s economic development spending that is directed toward supporting small business assistance, entrepreneurial development, and technology transfer. The second group, which we call recruitment, includes the proportion of a state’s budget that supports business recruitment and attraction activities, such as domestic recruitment, strategic business attraction, and strategic industry targeting. Here we also include funding within the state economic development budget that is explicitly allocated to workforce development. While some of this funding could potentially flow to homegrown firms, there is a strong likelihood that it used as a form of incentive in support of customized training for new employees at recruited establishments. We then construct a sample-limiting dummy variable

³ A multirecord, single-establishment, and firm match can occur when a single physical establishment chooses to report to D&B with two D-U-N-S Numbers. This can happen when multiple business activities occur at a location, as when a location has an engineering department and a manufacturing plant. In still other instances establishment addresses overlapped at some point in the study period before splitting into multiple locations or merging into one; we concluded that this was a single establishment for study purposes.

called “balanced,” which is coded at the state level when their budgeted economic development spending is within one quartile of the median (i.e., between the 25th and 75th percentile) for both the entrepreneurial development and recruitment funding groups.

Research Design and Identifying Control Establishments

We aim to identify five control establishments for each incentive-receiving treatment establishment. Control criteria include perfect matches on four treatment establishment characteristics: three-digit SIC code, state, subsidiary status (e.g., whether the establishment is its own headquarters), and employment category.⁴ We also require that the first year of recorded NETS data be within three years. We gather this data for the treatment record’s year of analysis. For treatment records with NETS data recorded in the year of the GJF subsidy (i.e., the establishment was born before or concurrent to the subsidy deal), the year of analysis is the year of the subsidy deal. For treatment records that do not have NETS data recorded until up to three years after the subsidy deal, the year of analysis is the establishment’s first year in the NETS data.

To identify control records, we rely on a matching-without-replacement program in SAS so that each control establishment is used just once. We ran this program five times and were unable to find control establishments for 147 treatment records; these treatment records were dropped from the analysis, leaving us with 3,173 establishments. For 2,615 of 3,173 treatment establishments we have five controls, and the remaining treatment records had between one and four control matches (see Table 1). This brought our final sample to 2,486 firms drawn from 35 states (see Table 2).

⁴ We use four employment categories. The first includes establishments with 1–19 employees, the second includes those with 20–99 employees, the third with 100–499 employees, and the fourth with 500 and more employees.

We next gathered data from the NETS for both the treatment and control records on yearly in-state sales and employment, our two dependent variables. For deals with multiple establishments, we create a single record with combined data.

Identification Strategy

Our basic research design consists of testing the impact of an economic development incentive given to a particular establishment on subsequent changes in employment and sales at that establishment relative to a set of control establishments. To test this basic question, we conduct a simple difference-in-differences analysis using the panel data set described above. Equation (1) summarizes our basic empirical strategy.

$$[1] \ln E_{it} = a + \beta_1 INCENTIVE_{it} + \delta_i + \gamma_{ct} + e_{it}$$

The dependent variable is the natural log of employment or sales at the establishment level. Logging the dependent variable helps smooth out scale differences in employment changes at small and large firms and enables us to interpret the value of β_1 as a semielasticity, or the percentage change in employment/sales resulting from an incentive. The key independent variable $INCENTIVE_{it}$ is coded 0 for each year before the incentive is granted and 1 for the year of the event and each subsequent year.

The model includes an establishment fixed effect δ_i , which controls for idiosyncratic differences across establishments that do not vary over time. The final fixed effect γ_{ct} is a time fixed effect that is specific to each “control set,” which is a dummy variable that represents each unique set of an individual treatment observation and the three to five control establishments matched to it. In this way each “control set” has its own unique time trend that not only controls for common macroeconomic trends over time but allows for the identification of β_1 to be limited to comparisons between the treated case and its own controls. Thus, we interpret β_1 as a

difference-in-differences estimator in that it is created by comparing employment (or sales) changes in establishments that received an incentive (i.e., in which the incentive dummy variable changes from zero to one) with those that never received an incentive (i.e., their control group of establishments). So, a statistically significant positive coefficient means that, on average, treatment establishments created more jobs after receiving an incentive compared to the set of matched control establishments. We do not include other controls for industry, firm size, or geography since these are the core variables used to select the matched pairs for each treatment case.

While the identification strategy presented above and summarized by Equation (1) is used to assess the overall effectiveness of all incentives and to answer the simple question of, “Do incentives work?” we move beyond this question and ask, “*How* do incentives work?” First, we seek to understand if incentives granted in industries in which the state granting the incentive has an established industry concentration—measured as having an employment location quotient above 1.1 in the same three-digit industry as the incentivized (treatment) establishment—perform better than incentives in nonspecialized sectors. While we do not specifically observe whether the industry in question is a strategic target industry of each state, we use the location quotient as a proxy for industry targeting.

Next, we break down our analysis by firm size to see whether incentives granted to small and medium-sized establishments are more efficient than those granted to large and very large companies. As discussed above, there are several reasons to suggest that incentives offered to smaller companies may produce better outcomes in terms of job growth, including the fact that deal makers may have more leverage compared to bargaining with very large firms that may be

more likely to relocate after the grant is made. Smaller firms may also be more likely to be in the earlier stages of a product life cycle, therefore offering more of an upside to local policymakers.

Finally, we seek to understand if incentives perform better in states that adopt a balanced economic development portfolio, especially when they treat industry recruitment and entrepreneurship as equally important strategy areas. As described above, we use data from C2ER to divide our national sample of states into two groups that balance economic development expenditures between traditional business attraction activities versus investments in local technologies and businesses. We again split our sample and rerun equation one separately for the sample of balanced states and all other states.

RESULTS

When we examine the overall effectiveness of state incentive grants on firm-level performance, we find little evidence that they generate new jobs or other direct economic benefits to the states that employ them. As described above, our difference-in-differences research design identifies the impact of an incentive treatment on establishments that received a grant relative to their set of matched control establishments. Since the control establishments were located in the same state and operate in the same industry (three-digit SIC), we can interpret the results summarized in Table 3 as what would have happened to employment “but for” the incentive. Thus, our analysis is one of the few empirical studies to evaluate incentives based on a plausible counterfactual of economic trends common to similar nonincentivized establishments.

The overall effect of incentives in our full national sample is listed in the first row of Table 3. The estimated impact on employment is slightly negative and statistically significant.

The point estimate of -0.037 implies that establishments that received an incentive experienced employment growth that was 3.7 percent slower than nonincentivized establishments. The impact on sales (column 2) growth is close to zero and not significant.

While understanding that the overall impact of incentives is critical for policymakers, we also explore whether incentives are more or less effective in different industrial contexts and for certain types of establishments. First, we limit our sample to analyze only those incentive deals made where the state offering the incentive has a preexisting specialization in the establishment's industry. Specifically, we define an "existing specialization" as having an employment location quotient above 1.1 in the same three-digit NAICS code as the incentivized establishment. This sample limitation focuses the analysis on deals that may have the potential to complement a state's previously targeted industrial sectors or competitive clusters. While we cannot directly observe targeting activity, focusing on states with a high location quotient can test whether incentives are more effective if they occur alongside existing economic strengths. The results indicate otherwise, however, as the point estimates for both logged employment and sales are negative and statistically significant. The results are similar when we define state location quotients in either 1990 or 2010.

Next, we segment our analysis by the size of the establishment receiving the incentive. Here we use four different employment size categories ranging from small (less than 50 employees) to very large (over 1,000). Interestingly, our findings are quite different across establishment size categories. The estimated employment and sales impacts are positive and significant for both the smallest and medium-sized categories (50–250). This implies that incentives seem to be more effective for smaller enterprises. While policymakers could interpret this finding as a signal to shift incentive activities to smaller firms, it could also be the case the

states and local governments that offer incentives to smaller firms may have more information about the future growth prospects of incentivized firms. Alternatively, these smaller firms that seek out incentives are in a growth cycle and are intending to expand production or enter new markets. While we cannot detect the precise reason why incentives granted to smaller firms are more successful, when compared to the large negative coefficients for the largest category, incentive use for smaller firms appears to be less risky. The large negative estimates observed for very large establishments appear to be driving the overall negative result. There are many possible interpretations as to why this figure is so negative for very large establishments. It could be that large firms that seek incentives are moving activity from one state to another in a process of consolidation that is proximate in time with an overall downward trend in business activity. Alternatively, large establishments may simply be more likely to play the incentive game and are less likely to be experiencing a positive growth cycle or inventing new goods and services. Whether these large incentives are retention or recruitment deals (which we cannot directly observe in the GJF database), our findings could also reflect the fact that large establishments have greater bargaining power and thus are able to extract incentives, even though they are not producing or promising higher than average employment gains. Regardless of the reasons why, our findings should be a caution to policymakers interested in chasing very large establishments.

Leaving the story here might suggest the lesson for states is to avoid incentivizing large establishments and instead put incentive dollars behind smaller establishment deals. However, there might be mediating effects in play that complicate this simplified small versus large firm narrative. For this reason, we split the sample again based on the orientation of each state's overall economic development strategy. As described above, we use the C2ER database to construct a variable to represent states that have what we consider balanced budget portfolios and

those that do not. We use the term *balanced portfolio* to categorize a subset of states that divide their economic development budget more equally across entrepreneurial support programs and more traditional recruitment activities, including incentive-based recruitment efforts and firm-specific training subsidies. In other words, these states strike a balance between endogenous and exogenous strategies—recognizing these not just as competing interests but also potentially complementary approaches to economic development.

After taking balanced portfolios into account, we find a rather striking divergence in results among these largest establishments (see Table 4). In balanced states, establishments that receive incentives no longer stand out for their consistent negative employment effects when compared to their non-incentive-receiving controls—there is no statistically significant difference between the two. Those results do not hold in nonbalanced states, where incentivized large firms underperform their control group peers.

Our initial interpretation is that a more balanced economic development budget means that states recognize that employment gains can come from a wide range of economic development activities and enterprise types—some endogenous, some exogenous. This knowledge helps shape decision making with respect to incentive granting, especially when large establishments are involved. One possibility may be that states with more balanced economic development budgets employ more careful selection criteria when evaluating potential large firm deals, and as a result, pick only the more promising deals among multiple options to incentivize. These states might also be better at negotiating with larger establishments, pushing for better contractual terms that are tied to better labor market outcomes (and related to that, they might also be better at enforcing these contracts, so they meet their intended employment targets).

SUMMARY AND BROADER IMPLICATIONS

Economic development incentives are widely used yet remain highly controversial. Policymakers and political leaders often claim incentives are needed to generate job growth and private investment. However, others present a compelling counterclaim, noting that scarce public tax dollars would be better spent on infrastructure, education, or other state programs. Businesses undoubtedly gain from these indirect institutional investments as well, but so do other members of the wider community. Still, attempts to use this logic to push an outright ban on incentive use have failed to gain traction at the federal, state, or local level, creating the need for timely research on a national scale to understand moderating steps that can be taken to ensure economic development incentives generate anticipated gains in employment and job quality.

This paper offers some insights from analysis of a national study of incentive granting that integrates three innovative data sets—the Good Jobs First Subsidy Tracker, the National Establishment Time Series, and the C2ER State Economic Development Expenditure Database. When combined, these data sources allow us to compare employment growth at companies that receive a government-funded incentive to a carefully selected group of nonincentivized control establishments. Overall, we find that incentivized establishments lead to lower employment gains than their nonincentivized peers. However, we also observe important sources of variation when we factor in differences in firm size. As our results indicate, incentives offered to firms that employ 1,000 or more workers produce far fewer jobs compared to their nonincentivized counterparts. By contrast, incentivized firms that are smaller-sized generate positive and significant employment effects when compared to nonincentivized control establishments.

At first glance, these results could support an argument for states and localities to redirect their incentive dollars to small and medium-sized firms—those that, on average, face greater

resource constraints relative to their larger counterparts and thus may be better poised to benefit from public assistance. But this conclusion, in isolation, overlooks the role that incentives can also play in boosting employment effects of larger firms. Recognizing this, we take the additional step of factoring in the breakdown of state economic development budgets, which allows us to examine whether incentives offered to larger firms perform better in states that strike a balance in their economic development spending. In particular, we look at the relative share of the state economic development budget going to industrial recruitment (where incentive granting is most common) versus spending on small business assistance, including entrepreneurial-supporting activities and support programs.

We find that there is an important tempering effect in states that more equally distribute funding streams so as to simultaneously support both major economic development functions, business recruitment, and homegrown business development. In these balanced states, larger firms that receive a public incentive display similar employment effects to their nonincentivized peers. In other words, the labor-market penalty that is initially associated with large-firm incentive deals is greatly diminished.

Additional research is needed to unpack the underlying mechanisms that are in play. Admittedly, there is some potential that this cross-state variation simply reflects a spurious economic effect, meaning that states with already strong economic conditions can better support budgetary balance—and where those same underlying economic strengths are also reflected in stronger labor market outcomes, irrespective of the incentive. However, we believe our findings speak to a more nuanced interpretation, namely, the need to situate incentive granting within a broader economic development framework—one that recognizes the potential for incentive

performance to be further enhanced through the act of balancing multiple economic development objectives.

Viewed through that lens, it could also be the case that incentive deals are more highly scrutinized by state agencies that are juggling other competing uses of public spending. And by extension, economic development practitioners in that same context might be less inclined to extend incentive offers to job-shedding companies. Furthermore, state agencies might also feel more emboldened to make strong demands of large firms, requiring those that receive an incentive to commit to retaining or adding jobs, rather than using public money to subsidize labor-displacing technologies or restructuring.⁵ These possibilities speak to an expanded research opportunity to explore the dynamic relationship between budget allocations in economic development and economic performance of incentivized firms. They also align well with recent calls to action in support of greater incentive transparency (Markusen and Glasmeier 2008)—most recently, visible in coordinated campaigns that call into question excessive incentive offers by cities hoping to lure Amazon’s second headquarters.

Beyond this, we also recognize the need for additional inquiry on the question of firm size, and especially to consider whether incentives offered to smaller firms might be further enhanced through better economic development oversight. Although our findings suggest that incentives generate greater employment effects when channeled to smaller-sized firms, they say nothing yet about the quality of those additional jobs. In his seminal 1994 article titled “The Small Firm Myth,” Bennett Harrison (1994) raised concerns about diminished job quality with shrinking firm size. Twenty-five years later, that concern still resonates (see Berger [2013] and Osterman and Weaver [2014]). A recent paper even notes that wage inequality is heavily

⁵ We thank Ned Hill for raising this possibility at American Collegiate Schools of Planning 2017 Annual Conference.

influenced by variation in firm-specific pay and with interfirm variance greatest among small and medium-sized firms (Song et al. 2015). In other words, the gap between businesses that pay high versus low wages is greatest among smaller firms.

This clearly has implications for the types of specific performance criteria that are tied to economic development incentives. At a fundamental level, economic development practitioners should develop mediating strategies that use incentives as a tool for improving wage standards among smaller firms. This includes layering on performance standards that more intentionally tie those incentives to living wages and enhanced working conditions. But more broadly, this also creates the potential for worker advocacy campaigns—including community groups involved in the Fight for \$15—to engage in the micropolitics of incentive accountability, critically assess contemporary incentive use, and in the process, push greater demands that public funding for economic development be used to support ethical and equity commitments.

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Table 1 Control Establishments Per Treatment Establishment

Number of controls	Establishments
1	167
2	136
3	126
4	129
5	2,615
Total	3,173

Table 2 Incentive-Receiving Sample Firms, by State

State	Number of deals
Alabama	20
Arizona	80
California	130
Colorado	14
Delaware	16
Florida	226
Georgia	24
Iowa	88
Indiana	86
Kansas	8
Kentucky	195
Louisiana	63
Massachusetts	57
Maryland	36
Maine	18
Michigan	180
Minnesota	1
Missouri	21
Mississippi	3
Montana	5
North Carolina	66
Nebraska	60
New Hampshire	19
New Jersey	73
New Mexico	3
Nevada	5
New York	722
Ohio	128
South Carolina	29
South Dakota	4
Texas	38
Utah	12
Virginia	30
Wisconsin	6
West Virginia	20
Total	2,486

Table 3 Impact of State-Level Incentives on Employment and Sales, by Industry Specialization and Establishment Size

Specification	(1) Ln(Emp)	(2) Ln(Sales)
Full sample N=253,462; R2=0.923	-0.0372*** (0.00775)	-0.0137 (0.00838)
<u>Industry Targeting</u>		
State LQ>1.1 in 1990 N=77,916, R2=0.917	-0.0639*** (0.0155)	-0.0498*** (0.0167)
State LQ>1.1 in 2010 N=84,958; R2=0.916	-0.108*** (0.0149)	-0.0840*** (0.0160)
<u>Establishment Size</u>		
Less than 50 employees N=47,889; R2=0.931	0.0759*** (0.0122)	0.0892*** (0.0141)
50–250 employees N=90,895; R2=0.867	0.0731*** (0.0117)	0.0864*** (0.0129)
250–1000 employees N=76,102; R2=0.879	-0.0497*** (0.0147)	-0.0142 (0.0158)
1,000 or more employees N=32,785; R2=0.893	-0.588*** (0.0320)	-0.529*** (0.0334)

NOTE: *** p<0.01, ** p<0.05, * p<0.1. All specifications include establishment fixed effects as well as a control-set specific time fixed effect. Robust standard errors listed below each estimate (of beta from equation 1). Significance levels are indicated as follows:

Table 4 Impact of State-Level Incentives on Employment, by Budget Type and Establishment Size

	Less than 50 employees		50–250 employees		250–1,000 employees		1,000 or more employees	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(ln)emp	0.0619*	0.0780***	0.0683**	0.0739***	-0.0696**	-0.0445***	-0.0943	-0.675***
	(0.0344)	(0.0131)	(0.0338)	(0.0123)	(0.0302)	(0.0168)	(0.0962)	(0.0338)
Observations	6,481	41,408	14,258	76,637	15,819	60,283	4,413	28,372
R^2	0.927	0.932	0.832	0.874	0.875	0.879	0.847	0.900
Balanced	YES	NO	YES	NO	YES	NO	YES	NO

NOTE: *** p<0.01, ** p<0.05, * p<0.1. All specifications include establishment fixed effects as well as a control-set specific time fixed effect. Robust standard errors listed below each estimate (of beta from equation 1).