

# Identifying the Local Economic Development Effects of Million Dollar Facilities

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*Using incentives to attract firms is the primary economic development policy for many local governments. Yet, relatively little is known about the economic development outcomes induced by successful attraction of new establishments. The empirical challenge lies in correctly identifying the counterfactual outcome. This paper tests for induced economic development in winning counties using a set of highly incentivized large plants. The paper makes a methodological contribution by comparing results from a natural experiment (in which counterfactuals are losers reported by Site Selection magazine) with a geographically-proximate matching control by design strategy. Estimates are sensitive to identification strategy, with distributional and placebo tests suggesting geographically-proximate matching as the preferred approach. The preferred estimates indicate modest increases in new economic activity that does not generate fiscal surplus for winning counties.*

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## 1. Introduction

Using tax abatements, financial incentives, and public investments to attract (or retain) businesses is the primary economic development tool for many local governments. In the competition between geographically fixed jurisdictions for mobile capital, the attraction of a large, new establishment is seen by some as the holy grail of economic development. Consequently jurisdictions are willing to offer substantial financial incentives to attract large establishments. Critics of economic development incentives assert that large inducements have negative efficiency, equity, and financial consequences for local communities. Advocates argue incentivized establishments generate significant economic development externalities, and thus incentives are simply compensation for the spillovers produced by the establishment.

In a recent paper, Greenstone, Hornbeck, and Moretti (2010) (GHM) find large, new manufacturing plants generate large productivity spillovers which may justify the substantial incentive packages used to lure them. They report winning county incumbent plants experience a remarkable 12.5 percent productivity increase five years after a large, new plant opening. Representative large new plants increase winning county manufacturing employment by 2.7% and the number of manufacturing plants by 5%, implying elasticities of more than 4 with respect to employment and 2 with respect to number of own industry plants.<sup>1</sup> Compared to the typical range of productivity elasticities from doubling city size (employment or population) of 0.03-0.04 reported by Rosenthal and Strange (2004) and number of own-industry plant elasticities of 0.0-0.1 reported by Henderson (2003), the implied GHM elasticities suggest successful attraction of a large, new plant generates unique productivity spillovers.

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<sup>1</sup> The percentage increase in winning county manufacturing employment is calculated by determining the employment at MDPs five years after opening and the number of winning county manufacturing workers. A representative MDP employs 1,436 workers by applying 2080 hours per worker year to the GHM Table 1 reported hours of labor. Although GHM do not report total manufacturing employment, they do report winning county population (322,745), employment-population ratio (0.535), and share of manufacturing employment (31%) in Table 3. Using these values, winning counties have approximately 53,527 workers in manufacturing. Similarly, the percentage increase in county manufacturing plants is calculated using the number of sample manufacturing plants reported in GHM Table 3. Results in Section 5 using the GHM identification strategy indicate an 8% (direct and induced) increase in total wage employment and 8.1% increase in manufacturing establishments attributable to the large new plants, implying elasticities of approximately 1.5.

If large plants indeed generate unique benefits, then there are profound implications for economic development policy. Yet, GHM rely on reports in an economic development magazine as a natural experiment for identifying their estimates. The institutional features of the natural experiment raise concerns about this identification strategy. Further, evidence of positive productivity spillovers does not necessarily mean that incentives achieve the desired economic development goals.

Using the set of incentivized firms from which the GHM sample is drawn, this paper investigates whether successful attraction of a large new manufacturing plant achieves common economic development policy goals. In addition to addressing an important policy issue, the paper makes a methodological contribution by comparing the GHM natural experiment approach with a geographically-proximate matching control by design strategy. In doing so, the paper also provides a replication assessment of the influential GHM results.

Local economic development policy aims to increase a location's capacity to create or retain wealth, which is most often articulated in terms of economic and fiscal benefits. Economic benefits include increased economic activity. The productivity spillovers documented by GHM represents one type of economic benefit. Productivity spillovers can be, but are not necessarily, associated with general equilibrium increases in the economic activity typically stated as goals for local incentive policies, most notably jobs. Fiscal benefits accrue to the extent new economic activity generates more in revenue than costs for the local government – fiscal surplus. The attraction model of economic development postulates that a new establishment generates externalities sufficient to induce new economic activity as well as fiscal surplus for local governments. Fiscal surplus should manifest itself through lower tax rates or improved public services. Lower taxes and better public services also attract new economic activity. On the other hand, the winner's curse scenario is characterized by fiscal deterioration and reduced attractiveness to other firms.

This paper tests whether successful attraction of a large new plant induces new economic activity and fiscal surplus. Measures of new economic activity that align with typical economic

development policy goals include aggregate county changes in manufacturing establishments and output as well as wage employment and earnings in all industries. However, it is important to keep in mind that aggregate area changes in economic activity may also reflect new plant or incentive induced general equilibrium effects on factor prices, public services, and taxes. To the extent that new establishments induce upward pressure on factor prices, incentives induce reductions in public services, or incentives induce tax increases on other area businesses or residents, the general equilibrium effects on aggregate economic activity may be reduced (or even negative). The existence of positive spillovers does not necessarily mean that successfully attracting one of these large manufacturing establishments achieves economic development goals in winning counties – in terms of either new economic activity or fiscal surplus. This paper investigates fiscal surplus by estimating the level and per capita change in public revenues, select expenditures, and outstanding debt after successful attraction of a large new plant.

Estimating the local economic development effects from “winning” a large new plant requires a strategy to identify valid counterfactuals for the “winning” counties in the absence of the new plant. Selection as the location for a new plant is not random, but rather depends on a number of observable and unobservable factors. Like most economic development policies, an ideal experiment from which we can garner estimates is unlikely. This paper employs two quasi-experimental empirical strategies to address the identification problem: difference-in-differences estimation combined with either revealed rankings or geographically-proximate matching control by design. The revealed rankings design is the GHM natural experiment. It relies on firms’ revealed rankings over potential locations as reported in the *Site Selection* magazine regular feature “Million Dollar Plant” (MDP). The geographically-proximate matching strategy builds upon the recent literature that suggests estimates close to those produced by ideal experiments may be obtained by combining difference-in-difference estimation with research designs that pre-processing the data (Ho et al. 2007; Wooldridge and Imbens 2009; Ferraro and Miranda 2014). The paper identifies winner county counterfactuals by matching on observables known to determine selection and outcomes as well as geography. Limiting potential matches to

geographically proximate counties controls for unobservables, such as factor markets, access to ports and transportation networks and regional variation in economic development policies.

The two identification strategies tell different stories about MDP-induced economic development. Institutional features of the experiment and primary source documents cast doubt on the validity of identifying assumptions for the GHM revealed rankings strategy. Pre-treatment outcome and covariate distributional test further suggest that the geographically-proximate matching strategy produces counterfactual counties that more closely resemble winners (at least in terms of observables) than the revealed rankings strategy. Placebo tests confirm geographically-proximate matching as the preferred strategy. Thus, the paper's findings bolster Angrist and Pischke's (2010) call for better assessment of natural experiments' institutional and empirical validity for econometric identification.

Regardless of identification strategy, the paper's results indicate successful attraction of an MDP isn't economic development's "magic bullet". The results suggest that if significant spillovers exist, the general equilibrium effects of directing public resources towards MDPs dominate them (perhaps due to overbidding). The paper proceeds in the next section with some brief background information on economic development incentives and economic development. Section 3 outlines the data sources and econometric model. Section 4 addresses identification in detail. Section 5 presents the results for establishments, output, earnings, and employment as well as government revenues and expenditures. Placebo tests for both identification strategies are presented in Section 6. Section 7 summarizes and concludes.

## **2. Background**

Policymakers generally state two primary goals for local economic development: increased employment and improved fiscal health (Malizia and Fesser 1999; McDonald and McMillen 2011). Economic development was cited as a major local government responsibility by 86 percent of elected officials in one survey. They reported increasing jobs and increasing the local tax base as their top two economic development priorities (Bartik 2004). Attracting new businesses through financial incentives is the primary policy for achieving economic

development for many state and local governments.

After decades of research, there is no clear consensus on the effects of economic development incentives competition (see Thomas 2007, Glaeser 2001, and Bartik 1991 for similar literature survey conclusions). Some researchers assert economic development incentives enhance efficiency and welfare. Incentives direct firms towards the most productive location by compensating them for the positive externalities they generate (Black and Hoyt 1989; Bartik 1991; King, McAfee, and Welling 1993; Patrick 2014b). In this view, the induced establishment generates positive spillovers that outweigh the costs (to the government and/or residents) of the incentives. A virtuous cycle of economic development ensues, which is characterized by higher wages, new establishments, increased employment, increased revenues, better public services, and/or lower tax rates (Eisinger 1988; Patrick 2014a).

However, another view asserts the dynamics of competition dominate any potential benefits (including spillovers). Proponents of negative-sum game scenarios argue that incentives competition results in a Prisoners' Dilemma. The structure of the game is such that jurisdictions' best response is to offer incentives, even though competition causes efficiency losses and/or negative equity consequences (Oates 1972; Guisinger 1985; Zodrow and Mieszkowski 1986; Wilson 1986; Wilson 1999; Ellis and Rogers 2000; Thomas 2000; Crotty 2003).<sup>2</sup>

There are also those who argue competition causes communities to overbid for the business and suffer the 'winner's curse' (Ulberich 2002; Charlton 2003; Christiansen, Oman, and Charleton 2003; Schragger 2009). Although Greenstone and Moretti (2003), Goodman (2003), and Dalehite, Mikesell, and Zorn (2008) report no evidence of fiscal deterioration from incentives, numerous studies find incentives are revenue negative (Bartik 1994; Oman 2000; Rodriguez-Pose and Arbix 2001; LeRoy 2005; Chirinko and Wilson 2008). In cases where the incentive or location induces a revenue shortfall, the local government must compensate either by reducing services or increasing taxes on existing residents and businesses (Figlio and

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<sup>2</sup> Wilson (1999) gives a very thorough survey of the tax competition literature.

Blonigen 2000; Diechman *et al.* 2008). To the extent that reductions in services or higher taxes induce workers to locate elsewhere or demand higher wages (Lynch 2004; Thomas 2007), both the attracted establishment and existing businesses may be negatively impacted by revenue shortfalls. Establishments may also suffer from cuts in public services on which they rely (Bartik 1996; Fisher 1997; Bartik 2005). In the winner's curse scenario, the general equilibrium effect on wages, employment, and government finances is negative.

Since overbidding causes the 'winner's curse', the situation can be avoided by communities' bidding no more than the net expected benefits (Patrick 2014b). The problem lies in correctly anticipating those benefits. While direct tax effects are relatively simple to calculate, quantifying both the positive and negative externalities is much more difficult.<sup>3</sup> Productivity spillovers are one possible MDP externality. GHM contributes theoretical and empirical frameworks for quantifying MDP externalities, particularly productivity spillovers. GHM propose a model of spillovers between firms and interpret it within the Roback (1982) context. According to their model government inducements successfully attract a new establishment. The new establishment generates significant spillovers, which enhance the productivity of all businesses in the area. The productivity gains start a virtuous cycle, whereby more new establishments locate to gain access to the productivity spillovers. As more establishments enter, they contribute to increasing productivity but also increase competition for inputs. Input prices rise until the increased cost of production is equal to the value of the increase in output due to spillovers. At this point, with profits being equalized over space, long-run equilibrium is achieved.

Thus, the large, new establishment should generate increased output, new establishment entry, higher wages, and additional employment. In this way, productivity spillovers provide one plausible mechanism by which attracting a large new plant generates externalities that achieve local economic development goals and justify incentives. As Glaeser and Gottlieb (2008) point out, though, higher wages also attract new residents to the community. In fact, studies show most

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<sup>3</sup> See Fisher 2007 for a confirming discussion on the availability of good direct benefit estimates. See the references on the winner's curse for examples of gross miscalculations of expected multiplier effects.

new jobs are filled by in-migrants (Bartik 1991; Partridge, Rickman, and Li 2009) and in-migrants represent a net fiscal drain for local governments (Altsuler and Gomez-Ibanez 1993; Fisher 2007). New residents also put additional pressure on rents and wages. Wages may also reflect underlying changes in public services and/or taxes. Increased factor prices, reductions in public services, or increased taxes may repel new economic activity. Thus, even in the presence of positive productivity spillovers, it is unclear whether incentivizing large plant locations achieves local economic development goals.

In order for successful attraction of a large, new establishment to achieve economic development for winning counties, it must induce net new economic activity as well as fiscal surplus. Economic development incentives will have a positive fiscal effect if: i) they increase economic activity (beyond that which would have occurred otherwise), and ii) the new activity adds more in tax revenues than the cost of the incentives and additional public services (Fisher 2007). Lower taxes, better public services, or both result from the distribution of the fiscal surplus to taxpayers. Lower taxes and better public services also attract new economic activity, which brings the cycle full circle.

The aforementioned effects on population, wages, rents, taxes, and public services make determining fiscal surplus particularly difficult. Simply estimating changes in revenue and expenditure levels provides no information on fiscal surplus.<sup>4</sup> Naturally, expenditure will rise as a growing population requires additional services. Revenues rise in response to expenditure increases because local governments are nearly always subject to balance budget restrictions. Public service production costs may also increase if higher input costs outweigh savings from economies of scale (Ladd and Yinger 1991). Therefore, estimated changes in revenues and expenditure levels should be accompanied by estimated per capita and tax rate changes. Taken together, changes in these public finance outcomes provide evidence of changes in the level of

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<sup>4</sup> Greenstone and Moretti (2003) use manufacturing and non-manufacturing MDPs, continuously appearing units in the Annual Survey of Governments, and a different econometric specification to estimate a different set of public finance effects in levels. Fisher (2007) critiques their results for providing little insight into fiscal surplus.



services and the tax burdens induced by the incentives and MDP.

### **3. Empirical Implementation**

#### *3.1 Data*

This paper investigates the economic development outcomes generated by highly incentivized, large new plant openings. Thus, I must first determine the set(s) of new establishment locations for analysis. As discussed in more detail below, the revealed rankings identification strategy developed by GHM requires firms appear in *Site Selection* magazine articles. GHM base their analysis on the “Million Dollar Plant” (MDP) sample outlined in Greenstone and Moretti (2003) (GM). According to the authors, they obtain the sample from 1982-1993 *Site Selection* magazine regular features “Million Dollar Plant” (MDP).<sup>5</sup> *Site Selection* magazine is an internationally circulated business publication covering corporate real estate and economic development, which relies on state and local economic development organizations for advertising dollars. The MDP series describes how high profile plant location decisions were made, reporting the county where the plant located (the “winner”), and (sometimes) reports the other counties who may have been finalists in the site selection process (the “losers”). For our purposes, a firm’s site selection decision is referred to as a case. Appendix 1 outlines the sample of manufacturing MDPs used in this paper.<sup>6</sup> The primary results employ all manufacturing cases from GM with a few minor corrections, heretofore referred to as the GMc sample.

I consider the MDP effect on county manufacturing establishments, output, wages, employment, and several government finance variables. The paper utilizes data from the 1977, 1982, 1987, 1992, and 1997 rounds of the Census of Manufactures (CM) and Census of Government Finance (CG). Bureau of Economic Analysis (BEA) Local Area Personal Income and Employment data from 1975-1998 are also employed.

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<sup>5</sup> The precise source of the sample is more nuanced. See Appendix 1 for more details.

<sup>6</sup> GHM have firm level data which allows them to exclude the MDP and its output from the sample. In order to maintain confidentiality, they must use an undisclosed subset of the GM MDP manufacturing sample cases. Since my analysis does not employ firm level data, the entire manufacturing case sample can be used.

Estimates of the MDP effect on county manufacturing establishments and output employ Census of Manufactures (CM) data. Data are available every five years. The pre- and post-treatment period assignment method is detailed in Appendix 2. GHM present comparable estimates using CM value of shipments data; however, there are some differences. Most notably, MDP-owned facilities and MDP output cannot be removed from the aggregate county outcomes in this paper. The estimated changes are therefore the direct effect of the MDP and the spillover effect. There is some debate in the literature over the best measure of output. Thus, this paper reports results for changes in winning county manufacturing output measured by the deflated value of shipments and value-added.

Economic development policy goals are not limited to the manufacturing sector. Inter-industry spillovers could also generate positive earnings and employment effects. Non-manufacturing establishments may experience crowding-out as well as tax and service consequences associated with successful attraction of a highly incentivized manufacturing plant. This paper therefore also examines aggregate county changing in earnings and wage employment. Black *et al.* (2005) suggest estimating wage effects with annual BEA wage data (rather than the decennial Census of Population data employed by GHM to estimate effects on quality-adjusted wages).<sup>7</sup> This paper adopts Black *et al.*'s earnings per worker dependent variable. Pre-period trends incorporate data for the 1-7 years prior to the MDP opening. The post-period is defined as the 0-5 years after the MDP opening. Estimates of winning county employment use BEA annual data as well, with employment defined as log wage employment.<sup>8</sup> Examining aggregate county employment outcomes is a particularly important extension of the GHM analysis because policy-makers often justify incentive policies on the grounds of job creation.

Finally, MDP effects on government finances are explored using Census of Governments

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<sup>7</sup> Previous versions of the paper also estimated changes in quality-adjusted wages using the 1980, 1990, and 2000 Censuses of Population. Estimates are available from the author upon request.

<sup>8</sup> Estimates using the change in log total employment were qualitatively and quantitatively similar to wage employment estimates.

(CG) data. The county-level variables are the aggregate of all local government finance activities for each of the county areas. Local governments comprise counties, municipalities, townships, special districts, and independent school districts. I estimate the change in the log of total own revenue, total property tax revenue, total outstanding debt, and total own expenditure on K-12 education, parks and recreation, police services, and fire services.<sup>9</sup> In order to disentangle changes caused by in- or out-migration from productivity-induced effects, I also estimate the change in per capita revenues, debt, and expenditure. Revenues divided by personal income provide information on rate changes. Pre- and post-period assignments follow the conventions outlined in Appendix 2.

### 3.2 Econometric Model

GHM derive an empirical specification based upon their theory and demonstrate its application for estimating aggregate county effects. Building upon the GHM empirical specification, variants of the following two empirical models form the basis for testing MDP-induced economic development at the county level:

Model 1

$$\ln(Y_{kjt}) = \delta 1(Winner)_{kj} + \kappa 1(\tau \geq 0)_{jt} + \theta_1 [1(Winner)_{pj} \times 1(\tau \geq 0)_{jt}] + c_k + \mu_t + \lambda_j + \varepsilon_{kjt},$$

and

Model 2

$$\begin{aligned} \ln(Y_{kjt}) = & \delta 1(Winner)_{kj} + \psi Trend_{jt} + \Omega [Trend_{jt} \times 1(Winner)_{kj}] + \kappa 1(\tau \geq 0)_{jt} + \\ & \gamma [Trend_{jt} \times 1(\tau \geq 0)_{jt}] + \theta_1 [1(Winner)_{pj} \times 1(\tau \geq 0)_{jt}] + \theta_2 [Trend_{jt} \times 1(Winner)_{kj} \times \\ & 1(\tau \geq 0)_{jt}] + c_k + \mu_t + \lambda_j + \varepsilon_{kjt}. \end{aligned}$$

where the subscripts  $k, j$ , and  $t$  indicate county, time, and case, respectively,  $Trend_{jt}$  is a time trend,  $1(Winner)_{pj}$  is an indicator for being located in a winning county,  $1(\tau \geq 0)_{jt}$  is an indicator for  $t$  being a year after the MDP opened, and  $\tau$  is year normalized such that  $\tau = 0$  in the plant announcement year for each case.

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<sup>9</sup> Own expenditure is direct expenditure by the local governmental units and excludes intergovernmental expenditures. Own revenue is revenue from own sources, excluding state and federal intergovernmental transfers.

In Models 1 and 2,  $Y_{kjt}$  represents the outcome of interest, namely, the number of manufacturing establishments, output, wages, employment growth, and government finances. . County, time, and case fixed effects are given by  $c_k$ ,  $\mu_t$ , and  $\lambda_j$ , respectively.

The parameters of interest are  $\theta_1$  and  $\theta_2$ . Under Model 1,  $\theta_1$  measures the difference in mean outcome for winning counties after successfully attracting an MDP. Thus, it is basically the difference-in-differences estimator of the “treatment” (winning) effect. Model 2 is more nuanced than Model 1. It allows for both a mean shift in outcome,  $\theta_1$ , and a differential trend in outcome, measured by  $\theta_2$ , in the winning county after an MDP opening.

#### **4. More on Identification/Alternate Strategy**

Estimating the local economic development effects from “winning” a large new plant requires a strategy to identify valid counterfactuals for the “winning” counties in the absence of the new firm. This paper employs two quasi-experimental research designs, with the goal of getting estimates that are close to the ideal of a properly designed experiment. The estimating equations produce the quasi-experimental difference-in-differences (DID) estimator. The DID estimator does not require strong functional form assumptions and under certain assumptions removes bias produced by time-invariant unobservable differences. Despite these advantages, unconditional DID doesn’t necessarily produce estimates that are very close to those from an experiment (Ferraro and Miranda 2014a). Model 1 and Model 2 condition treatment effect estimates on differences in preexisting trends, county fixed effects, year fixed effects, and case fixed effects. The key assumption is that the expected outcome in the control group represents the counterfactual trend of the treated group in the absence of treatment after conditioning on county, year, and case fixed effects. As discussed in more detail below, there are number of ways in which DID identifying assumptions may be violated and thus produce biased estimates of the “treatment” effect. Overcoming these threats to identification is the motivation behind recommendations to combine the DID estimator with research designs that pre-process the data (Ho et al. 2007; Wooldridge and Imbens 2009). Testing these recommendations, Ferraro and Miranda (2014a) demonstrate that combining the DID estimator with pre-processing the data

through matching produces estimates very close to those from an experimental design. Matching essentially “addresses the modeling of counterfactual trends by design” (Ferraro and Miranda 2014b). This paper augments the DID estimator with two control by design methods: revealed rankings and geographically proximate matches. The revealed rankings strategy is the GHM natural experiment. Both methods use the research design to “mimic the properties of the control in a properly designed experiment” (Blundell and Dias 2009), with the difference being how the design conditions the DID estimator.

The two strategies should be evaluated with respect to how well each addresses threats to identification within the context of DID. Again, the key assumption is that the expected outcome in the control group represents the counterfactual trend of the treated group in the absence of treatment. Simply showing parallel pre-treatment trends, however, does not guarantee that this assumption holds. In fact, Ferraro and Miranda (2014b) demonstrate that relying on parallel pretreatment outcome trends and ignoring covariates produces worse estimates than ignoring pretreatment trends and focusing only on covariates. The DID identifying assumptions imply that assignment to treatment and control groups is unrelated to the determinants of outcomes (Greenstone and Gayer 2009). Selection on unobservables is not ruled out, rather selection on changes affecting outcomes (Blundell and Dias 2009). Blundell and Dias point out that differential macro trends and selection on idiosyncratic shocks threaten identification. Treatment and control groups are particularly likely to experience differential macro trends when located in different labor and factor markets. Reactions in anticipation of treatment also threaten identification (Greenstone and Gayer 2009). When these reactions are not observable and change over time, the potential impact on outcomes are absorbed in the transitory unobservable. Thus, even similar groups “will be inherently different as observables are endogenously affected” by expectations about treatment (Blundell and Dias 2009). The distributional balance of observables provides an informal test (Greenstone and Gayer 2009). It is difficult to believe that the identifying assumptions hold when treated and control groups vary substantially in levels (even with similar trends) (Ferraro and Miranda 2014b). However, similarity doesn’t guarantee that the

identifying assumptions hold. Thus, as argued by Angrist and Pischke (2010), evaluation of the research design on the basis of institutional context and economic theory is still required.

The next sections describe the two quasi-experimental research strategies in more detail. The discussion highlights potential threats to identification within the context described above and provides some evidence regarding how well each design addresses these threats.

#### *4.1 Revealed Rankings Strategy*

The revealed rankings strategy is the natural experiment in GHM. It relies on firms' revealed rankings over potential locations as reported in the *Site Selection* magazine regular feature titled "Million Dollar Plant" (MDP). The strategy requires that the "loser" counties in the MDP articles are (nearly) identical to the "winner" county in terms of future expected profits for the firm as well as factors impacting outcomes—the only significant difference being that they did not receive the MDP.

Institutional features of the site selection process and the for-profit magazine raise concerns about using *Site Selection* magazine revealed rankings as a natural experiment for econometric identification. The strategy assumes that firms release the names of the second-best location for the magazine to make public. Economic development practitioners and scholars generally report that firms only release the name of communities with large incentive offers as part of a strategy for increasing incentive bids.<sup>10</sup> Being the community with the highest incentive offer does not necessarily imply the community is the actual runner-up that "narrowly" lost the competition as assumed by the GHM revealed rankings strategy. Moreover, offering the largest incentive packages may be a signal that the community is struggling. Communities with lagging growth rates may feel compelled to enter incentive bidding wars to enhance perceptions about their

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<sup>10</sup> See Bucholz's case study of Fed Ex in Schweke (2009) for an excellent discussion of this firm strategy. Not only is the firm's rationale for revealing its true counterfactual suspect, it is not clear that the identified "losers" were in fact identified as such by the company. As documented below, *Site Selection* reported "losers" sometimes differ significantly from those reported by other media outlets. Further, since *Site Selection* magazine relies on local and state economic development organization advertising dollars, it is possible it is in their best interest to report "losers" who were willing to spend a lot on attraction or that were identified by the "winning" community to justify the size of their bids.

economic development. This creates the potential for bias as particularly poor-performing locations are more likely to be included in the set of counterfactuals revealed by *Site Selection* magazine. GHM indirectly provide support for this threat to identification in their Figure 1, which plots “winners” versus “losers”. The figure suggests that the large productivity spillovers in winning counties estimated with the natural experiment were due to losing county productivity declines over times (rather than the gains in winning county productivity that one would expect in the presence of large spillovers). Finally, the GHM revealed rankings strategy implies that firms willingly release confidential information to competitors about the most profitable locations – i.e., although the winning community is best, competitors should consider the losing site as the next best location for similar facilities. The idea that profit maximizing firms routinely release such sensitive information to their competition is suspect.

As noted above, one informal test of the identifying assumptions is to examine observable outcome and covariate balance. Table 1 presents mean GMc county characteristics by winner status. The value of key outcomes and covariates for GMc losers is statistically different from winners. GMc losers are significantly larger than winners in terms of population. While the population difference is not significant in GHM, they report winners experienced significantly larger changes in population over the previous six years.<sup>11</sup> GMc losers are also much closer to metropolitan areas than winners. Like the GHM and GM samples, wages significantly differ between winner and loser counties. Specifically, earnings per employee are higher in GMc loser counties than winner counties. Winners are more concentrated in manufacturing, and less concentrated in farming, than GMc losers. Loser counties have significantly more manufacturing establishments than winners, which results in significantly higher value of shipments and value-added. Local government revenues, expenditure, and debt levels are significantly larger in loser counties than winner counties. Losers also spend significantly more per capita than winners.

[Insert Table 1 approximately here]

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<sup>11</sup> See Table 3 in GHM (2010) for similar output, wage, and population descriptives for their undisclosed sample of cases.

Improper counterfactuals are only an issue to the extent that they violate the identifying assumption that the “winner” and “loser” counties are (nearly) identical with respect to the factors influencing “winning” as well as the outcome variables of interest. Further, the DID estimator ensures imperfect counterfactuals only threaten the research design if important unobservables are either time-varying, level differences have varying impacts on outcomes over time, selection is unrelated to unobserved changes, treatment and control groups face differential macro trends, or counties react to expectations about treatment. Therefore, the revealed rankings strategy assumes the observable differences in Table 1 do not threaten identification through any of these channels and that conditioning on appearance in the magazine effectively conditions on all time-variant unobservables that influence “winning,” “losing,” and the outcome variable(s).

As outlined by GHM, the basic idea behind the revealed rankings strategy is that unobservables drive the site selection process and outcomes. The “winners” and “losers” identified in the magazine are “nearly” identical with respect to those unobservables. The literature suggests economic size, density, industrial composition, transportation, wages and other urbanization economies influence selection as a location for a new facility (Brouwer *et al.* 2002; Guimaraes *et al.* 2003; Devereux *et al.* 2007). If the revealed rankings identification strategy produces consistent “winner” effect estimates, then we would expect “winners” and “losers” to be “nearly” identical with respect to these factors. While Table 1 indicates important levels differences in these observable factors, it doesn’t reveal information on unobservables that may dominate selection and outcomes. To assess whether there is any other reasons to be concerned about the “losers” from the MDP sample, I examine primary evidence on the BMW case that GHM uses as an example to describe the revealed rankings approach. I also attempt to verify the validity of the identified “loser” in the last ten GM cases.

On June 29, 1992, BMW announced its first US manufacturing plant would locate in Greenville County, SC. The announcement was the culmination of South Carolina’s involvement in a 2+ year site selection process, which ended in a very public bidding war between Greenville, SC and Omaha, NE. Omaha is located in Douglas County, NE, and for this case, Douglas



County is the only “loser” identified in GM’s MDP sample. GHM argue the bidding war shows that their sample correctly identified the “loser”. However, if concerns about the strategic motives behind public bidding wars are taken seriously, then a closer look is warranted. A LexisNexis search for documents related to the BMW search reveals these concerns may be valid.

As detailed in Appendix 3, primary documents suggest that the automaker was looking for a site on the eastern seaboard with a preference for the South which focused on South Carolina.<sup>12</sup> Nebraska’s lucrative incentives package served a useful purpose for the company – raising South Carolina’s initial bid from \$35 million to \$150 million. Given BMW’s selection criteria and the bidding process described in Appendix 3, it is difficult to reason that Douglas County, NE serves as an appropriate counterfactual to productivity in Greenville, SC without the BMW plant. If Douglas County, NE were, in fact, an attractive place to make cars, then one of the bidding wars for subsequent auto facilities should have chosen to locate there. However, no major automaker has located there, but several have chosen the Southeastern US despite lucrative offers from Nebraska. Examining other selection and outcome factors, Douglas and Greenville appear to be substantially different with respect to economic size, manufacturing share of employment, and the pre-trends in manufacturing wages per worker (see Appendix 3 Figures A1-A3). A US federal government memo obtained by Automotive News quotes BMW’s Chairman as identifying Anderson, SC as the clear front-runner three-months prior to the Greenville announcement (Kurylko 1992a). The mostly likely correct counterfactual, Anderson, SC,

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<sup>12</sup> In late March 1992, *Automotive News* reported obtaining a US federal government memo on the project that quotes BMW Chairman Eberhard Von Keuhiem as saying the US site selection process was 80% complete, with the choices narrowed to 4 sites. The document’s author, US Consul General Andrew G. Thomas, Jr., reports the Chairman only mentions the state of South Carolina (Kurylko 1992a). Nebraska is noticeably absent from an April 13 *Automotive News* report on state governors flown to Bonn to meet with the company. Nebraska is also absent from the states asked to meet with the company Chairman during his visit to Washington in April (Henry 1992). South Carolina’s incentive package was valued at \$35 million in a May report. The report goes on to state, “A Nebraska site would not meet BMW’s stated criteria that a U.S. plant be within six time zones of Germany, or of proximity to a major port (Kurylko 1992b). On June 18, the site selection process was in the hands of BMW’s legal team and according to a company official, “While BMW is leaning toward Spartanburg, S.C., lucrative offers keep rolling in from Omaha, Neb., the source said. *The Omaha World-Herald* reported on June 7 that Nebraska has offered as much as \$240 million in tax, land and other incentives ... (Kurylko 1992c).”

displays similar manufacturing share and wage pre-trends. Since these factors are both determinants of selection and economic development outcomes, these differences cast some doubt on the validity of the revealed rankings identification assumption, or least the one case that GHM used to justify the approach.

It is possible that such concerns are isolated to the BMW case. In order to check this possibility, I attempt to verify the validity of the identified “loser” in the last ten cases in the GM sample. Using primary sources, I identified the correct counterfactual for 9 out of 10 cases. Of those 9, the GM *Site Selection* magazine sample reports the correct counterfactual for only 2 cases and both of these have “loser” counties that are within close geographic proximity (a directly adjacent county in one case). If the Mercedes case is added, then the number of correct counterfactuals rises to 3. However, GM list 7 “losers” for the case reported by *Site Selection* magazine, but only 2 of those 7 represent the actual finalists.

Four of the GM cases list the county from which the firm relocated as the single “loser”. For example, Everest & Jennings officials report suffering tremendous losses in their Ventura, California location. During the announcement of their move to St. Louis, the company makes clear the relocation was motivated by the high cost of doing business in California (*United Press International*, February 28, 1992). Similarly, Transkrit’s selection of Roanoke, VA followed a four-month search including 25 sites in Virginia and North Carolina, according to Transkrit Chairman Frank Neubauer (*The Washington Post*, January 25, 1993). Yet, the GM MDP sample lists Westchester, NY, the county from which the company moved, as the “loser”. Although it is possible that current location could serve as a fallback site in some site selection searches, the primary documents suggest these companies’ search for a new location was driven by a need to relocate from their current location for profitability.

The cases where the “losers” are the counties from which the companies were relocating particularly calls into question the revealed rankings identification assumption of (nearly) identical “losers”. The post-treatment outcomes for these counterfactual counties are clearly affected by treatment – the “winning” county receives the new firm only when the counterfactual

loses the firm. Further, the primary documents strongly suggest that future expected profits (the determinant of treatment) and expected outcomes vary substantially between treated and control counties for these cases. Without appealing to outside sources, the magazine articles reveal that over a third of the reported “loser” counties in the GM sample were locations where the firms were closing current operations. Existing plants lose any productivity benefits associated with the MDP as well as experiencing a mechanical decrease in the number of establishments and output. This further suggests that the assumption that the expected outcome in “losers” is the correct counterfactual for “winning” counties is violated.

#### *4.2 Geographically-Proximate Matching Strategy*

In a perfect world, the correct counterfactual for each location could be recovered from primary documents. However, primary source documents don’t always reveal the runner-up county and public revelation is suspect for the reasons outlined above. The primary sources overwhelmingly suggest that correct counterfactuals, even when specific counties aren’t named, are geographically proximate to winners. An alternative to the revealed rankings identification strategy is to match winners based upon the aforementioned determinants of treatment and outcomes as well as geographic proximity to the winning county.

In order to produce consistent estimates of the “winner” effect with a DID matching estimator, the conditioning variables should capture the time-variant characteristics that systematically influence both selection as a “winner” and the outcome. After matching and differencing out unobservables, potential for bias will exist to the extent that unobservable time-variant factors determine selection and outcomes. Therefore, the difference between the revealed rankings and observable DID matching estimators lies in how well each controls for time-varying determinants of outcome and treatment as well as level differences which have unstable effects over time.

There is no algorithm for choosing the set of observable covariates upon which to match. Theory, statistical measures, and institutional knowledge should be used to determine the appropriate conditioning variables (Rosenbaum 2004; Hill *et al.* 2004; Sianesi 2004; Smith and

Todd 2005; Stuart and Rubin 2008). Based on the discussion of site selection and economic development outcome determinants above, this paper utilizes the following covariates to determine matches: total county population, presence of an interstate in the county, distance to the nearest metropolitan area, share of population that is working aged, minority share of total population, earnings per employed worker, and the share of total employment in manufacturing, farming, services, FIRE, and military.

This paper defines covariate distance between winner counties and potential counterfactuals using the two methods. The first matches directly on the covariate values and is referred to as covariate matching. Covariate matching determines the optimal match(es) on all covariates weighted by the diagonal matrix of the inverse sample standard errors. The propensity score distance is defined as the absolute difference in (true or estimated) propensity scores between the winner county and potential counterfactual counties. Matching on propensity score is more bias-reducing/robust than covariate matching on more than five covariates (Gu and Rosenbaum 1993; Rubin and Thomas 2000).

In fact, matching on a misspecified propensity score model can still be bias-reducing and efficiency-enhancing, particularly when coupled with the DID estimator. (Rubin and Thomas 1992, 1996; Hill *et al.* 2004; Stuart and Rubin 2008; Wooldridge and Imbens 2009). Drake (1993) shows that ATT results are more sensitive to misspecification in the outcome model than in the propensity score model. Other research confirms ATT estimates aren't very sensitive to propensity score specification (Dehejia and Wahba 1999, 2002; Zhao 2004; Stuart and Rubin 2008). Thus, it is the preferred distance measure in this study.

However, it is possible there are still important unobservables omitted from the propensity score model. In order to control for additional unobservables, this paper restricts the potential pool of losers to which a winner may be paired in two ways: year and geographic location. For each case, match year is defined as the year that is 3 years prior to the MDP location announcement. Neighbors are chosen to minimize the distance between winner values in the match year and potential counterfactuals in the same match year. Not only are these the covariate

values likely observed during the site search, but they are also unaffected by treatment. This study also employs geographic location as a way of controlling for potentially confounding unobservables. Site selections usually take place within a specified geographic region (Brouwer *et al.* 2002; Guimaraes *et al.* 2003; Devereaux *et al.* 2007). Geographically proximate locations share factor and labor markets. Thus, input (such as labor) quality, quantity, and price differences are minimized by limiting potential matches to geographically proximate counties. Tiebout sorting models, tax and public service competition models, and yardstick competition models also suggest tax and public services will be similar in geographical proximate areas (Geys 2007; Hall and Ross 2010). The dynamics of competition cause locations to replicate policies from nearby locations, including economic development incentive policies. Thus, regional factors are likely highly correlated with both selection and outcomes. The geographic restriction also helps control for regional productivity shocks coincident with the MDP opening.

For example, consider the after-tax return on capital. It could be argued that the after-tax return on investment is a critical determinant of site selection. However, using geographically proximate counterfactuals should substantially reduce, if not eliminate, this concern. Papke's (1995) study found that after-tax returns on investment were so similar in six Great Lake states that one could not be preferred. These findings substantiate theoretical predictions in many tax competition models (see Wilson 1999 for a thorough review).

In this study, the "match" or set of matches for each "winner" must be located within a specified distance (50-100 miles) of the winning county (calculated as the distance between centroids) for each case.<sup>13,14</sup> The covariates in the propensity score model, as well as the geographic proximity restriction, are in the spirit of List *et al.* (2003). Michalopoulos *et al.* (2004) find that comparing treated observations to counterfactuals in the same state is bias-

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<sup>13</sup> As a robustness check, all outcomes were also analyzed using matches located within 100-250 miles of the winning county. The results are qualitatively and quantitatively similar and available upon in Appendix 5.

<sup>14</sup> Henderson (2003) finds no evidence of significant agglomeration spillovers between firms beyond county borders. Using 50-100 miles excludes adjacent counties and any possibility of confounding MDP spillovers; yet counties are still close enough to reflect large unobserved productivity shocks such as transportation upgrades and human capital influxes that are not attributable to the MDP.

reducing. Smith and Todd (2000) and Hill *et al.* (2004) also argue for matching based upon geographic proximity to treated observations. Using treated and controls located in the same factor markets is also one of the recommendations for good propensity score models found in Heckman *et al.* (1997), Heckman *et al.* (1998a), Heckman *et al.* (1998b), and Glazerman *et al.* (2004).

The number of propensity score neighbors implies a well-known bias-efficiency trade-off (Dehejia and Wahba 2002; List *et al.* 2003; Stuart and Rubin 2008). ATT estimates are most precise when winners are matched to only one nearest neighbor. However, they are inefficient due to loss of information from excluded potential matches. Increasing the number of matches increases efficiency, but at the cost of increased bias.

With the above issues in mind, this paper reports results for four sets of geographically-proximate, observable matches. Using multiple matching techniques will give an indication of the sensitivity of results to the matching method and the extent of bias-efficiency trade-off. The first 3 sets are matched on propensity score estimates or the log odds ratio from the propensity score estimates. Two sets of nearest neighbor matches are created by using the closest 1, and 5, propensity scores to each “winner”. The third set uses the log odds ratio to find all matches within a specified radius.<sup>15,16</sup> The final set are the distance-based covariate matches.

#### 4.3. Implications

Table 2 reports the results of balancing tests for outcomes in all matched samples. The number of manufacturing establishments, value of shipments, value-added, and government finance variables were not included as match criteria. The samples are well balanced with respect

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<sup>15</sup> There is not a well-established algorithm for defining the radius, or caliper, size in terms of distance between treated and untreated. This paper follows Lechner *et al.* (2010) and sets the caliper as 1.5 times the largest distance calculated from pair-matching each sample. Distance is calculated using the log odds ratio for each observation.

<sup>16</sup> The paper uses the log odds ratio for radius matching to avoid any inconsistencies from choice-based sampling. The frequency of winners in the sample is higher than the frequency in the population of counties. Matching on the log odds ratio produces results that are invariant to choice-based sampling (Heckman and Todd 2004; Smith and Todd 2005; Todd 2006; Caliendo and Kopeinig 2008). This is not a concern for the nearest one and five neighbors.

the match variables by construction. Table 2 reveals that the matching strategy creates counterfactual samples that are statistically indistinguishable from the winner county samples in terms of the number of manufacturing establishments, value-added, and value of shipments. Counterfactual samples also closely resemble winner counties with respect to the government finance variables. The null hypothesis of equality for government finance means generally cannot be rejected. Recall from Table 1 that the revealed rankings counterfactuals were statistically different along these dimensions.

[Insert Table 2 approximately here]

It is possible that the unobservables captured by the GHM revealed rankings strategy dominate the observables and unobservables captured by geography from the matching strategy in determining the “winner” effect on outcomes. If so, then those estimates are more reliable than propensity score or covariate matching estimators. If not, then geographically proximate observable matching estimators produce more reliable results.

## 5. Results

### 5.1 *Economic Benefits*

Table 3 presents the change in counties' number of establishments identified by revealed rankings (Column (1)) and geographically-proximate observable matches (Columns (2) – (5)). Table 3, Column (1) reports that the number of manufacturing plants increased by 8.10% in the average winning county compared to the average GMc loser county.<sup>17</sup> While the revealed rankings strategy indicates MDPs induce significant additional economic activity as measured by the change in county establishments, the story is quite different when the effect is identified by geographically-proximate observable matches. Table 3, Columns (2) – (5) reports the change in establishments compared to the nearest one, five, and caliper propensity score neighbors as well as the nearest covariate neighbor, respectively. The estimates are generally negative, but not

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<sup>17</sup> This estimate is slightly lower than the 12.5% change in CM plants reported by GHM. The difference may be caused by (i) aggregate versus plant level data, (ii) using all manufacturing cases rather than the undisclosed subset, or (iii) weighting scheme.

statistically different from zero either.<sup>18</sup> Winning counties experience a significant 5.27% decrease in the number of manufacturing establishments when identified by the nearest covariate neighbors. This suggests that MDPs may “crowd-out” existing establishments, particularly those in the lower end of the productivity distribution. MDPs may induce upward pressure on factor prices that negatively affect existing establishments or deter new entrants. If non-MDP establishments pay for MDP incentives through increased taxes or reduced public services, then MDPs could also be associated with fewer establishments. Observable matching provides little evidence in support of net new economic activity as measured by the change in establishments.

[Insert Table 3 approximately here]

The change in output as measured by value of shipments and value-added are presented in Table 4. Under the matching strategies the effect of winning an MDP on county manufacturing output is smaller in magnitude than under the revealed rankings strategy. Using the log value of shipments as the dependent variable, winning counties experienced a statistically significant increase in output of 24.56% compared to revealed losers. Using geographically-proximate observable matches, the winning counties experienced a statistically significant increase in output ranging from 10.97% (nearest propensity score neighbor) to 13.32% (five nearest propensity score neighbors).<sup>19</sup> While both identification strategies indicate significant increases in winning county manufacturing output, the revealed rankings estimates indicate an increase approximately twice that identified by the geographically-proximate matching counterfactuals. Recall that MDP’s account for at least 9% of winning county manufacturing output and these estimates include both the direct and spillover effect. Thus, observable matching indicates 1-4%

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<sup>18</sup> As shown in Appendix Table A3, the change in winning county establishments is also generally negative and statistically insignificant when identified by observable matches within 100-250 miles of the winning county. The exception is a statistically significant increase in winning county establishments compared to the nearest caliper matches within 100-250 miles. The increase is approximately half of the estimated increase identified by the revealed rankings strategy. Given the well-known bias-efficiency trade-off, these are also the matching estimates with the largest potential bias.

<sup>19</sup> The estimated increase in value of shipments ranges from 7.2-14.4 percent when identified by observable matches within 100-250 miles of winning counties (Table A4), with statistically significant increases of 9.08 percent (nearest caliper propensity score matches) and 14.4 percent (nearest five propensity score neighbors).



increase in winning county output above that which is attributable to the MDP; while the revealed rankings strategy suggests an increase of approximately 13% due to incumbent plants and new establishments. The output effects identified by GMc losers found here are very similar to the imprecisely measured 14.5% increase in aggregate value of shipments reported by GHM, with the differences likely attributable to differences in sample case composition, weighting, or the inability to exclude MDPs from the sample.

[Insert Table 4 approximately here]

Value added as the dependent variable yields smaller estimated changes than with value of shipments. Winning county manufacturing value-added significantly increased by 22.33% compared to the loser counties reported by *Site Selection* magazine. Winning county manufacturing output significantly increased by 10.85% and 9.62% compared to the five nearest propensity score and caliper neighbors, respectively.<sup>20</sup> The geographically-proximate matching estimates indicate output increased by less than half of the increase estimated with the revealed rankings strategy.

Establishments and output are important economic benefits, but jobs and earnings often garner the most policy attention. Tables 5 and 6 report the results of estimating Models 1 and 2 for earnings per worker and wage employment. Model 1 estimates the mean shift in winning counties' outcome after an MDP opening; while Model 2 also identifies the change in the outcome trend.<sup>21</sup>

While there isn't an economically or statistically significant change under the revealed rankings strategy, observable matching estimators suggest MDPs are associated with significant earnings per worker increases. Model 1 estimates the mean shift in winning county earnings per worker after an MDP opening. Table 5 reports a significant increase in winning county wages ranging from 2.65% (nearest propensity score radius neighbors) to 3.54% (nearest covariate

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<sup>20</sup> The estimated increase identified by the nearest five and caliper propensity score neighbors within 100-250 miles are 12.61% and 7.91% (Table A4), respectively, which are within the confidence interval for the corresponding 50-100 mile estimates.

<sup>21</sup> The estimated increase after five years is calculated by  $\theta_1 + 6\theta_2$  to allow an effect in  $\tau = 0$ .

neighbors), with most over 3%.<sup>22</sup> Model 2 implies similar increases after 5 years, although estimates are less precise than Model 1. This lends credence to the notation that upward pressure on factor prices may explain the establishment effects above.

[Insert Table 5 approximately here]

As with the other economic development outcomes, the two identification strategies tell different stories about the magnitude of the MDP effect on total wage and salary employment. Model 1 and 2 estimated changes in winning county wage employment are presented in Table 6. After an MDP opening, winning counties' experienced a mean shift in employment of 8% compared to GMc losers. Model 2 confirms a positive level change in employment as well as a positive trend break. Winning counties' wage employment levels increased by approximately 7.6% five years after the MDP opening.<sup>23</sup> Observable matching estimators produce smaller estimates of the MDP effect than the revealed rankings estimates. The mean shift in winning counties' wage employment was significantly positive, with estimates ranging from 3.10% (Column 5 nearest covariate neighbor) to 5% (Column 4 nearest propensity score caliper neighbors).<sup>24</sup> Unlike the revealed rankings estimates, geographically-proximate matching results from estimating Model 2 for wage employment are generally insignificant.

[Insert Table 6 approximately here]

When winning county effects are identified by the revealed ranking strategy, changes in economic activity outcomes generally suggest that MDPs induce substantial new economic activity. Increases in value of shipments are similar in magnitude to those reported by GHM. Changes in value-added output are smaller, but still suggest a large increase in manufacturing

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<sup>22</sup> Earnings also significantly increase compared to neighbors within 100-250 miles, ranging from 1.53% (nearest propensity score neighbor)-3.48%(nearest propensity score radius neighbors). See Table A5 in the online appendix for more detail.

<sup>23</sup> An earlier version of the paper included estimates for changes in employment growth rates rather than levels. The estimated five year effect was negative for growth rates.

<sup>24</sup> Table A6 presents the estimated change in winning counties' employment identified by observable matches within 100-250 miles of winners. The nearest one and five propensity score neighbor estimates are virtually identical to the estimates identified by counterfactuals within 50-100 miles; while the estimates for the nearest propensity score caliper and covariate neighbors are slightly larger. Table A6 also indicates statistically insignificant employment changes five years after opening.

output. Accompanying the increased output, winning counties experience significant new firm entry and new employment. However, earnings did not significantly increase. .

The story is somewhat different when MDP winners are compared to their nearest neighbors within 50-100 miles based upon observable covariates. Winning counties experienced much smaller increases in manufacturing output. There is not strong evidence in support of firm entry. Indeed, estimates indicate winning counties' may have lost establishments. While earnings per worker significantly increase, this could simply be offset by higher housing prices or land prices. The upward pressure on wages may also explain the lack of establishment change and smaller employment increases.

## *5.2 Fiscal Surplus*

Although there is some evidence in support of new economic activity, the activity must generate fiscal surplus to achieve both goals of economic development and induce a virtuous cycle of economic development. Tables 7-12 report results for changes in winner counties' local government revenues, debt, and expenditures, respectively. As discussed in Section 3.1, the county variables measure all local government finance activities in their respective categories for each of the county areas.

Table 7 reports the change in winning counties' general own revenue, or revenue from local governments' own sources and excluding intergovernmental transfers. Under the revealed rankings strategy, winning counties experienced a significant 14.5% increase in mean general own revenue after an MDP opening (Table 7, Column (1)). As discussed in Section 2, rising revenues may indicate budget balancing for increased service expenditures associated with a growing population and may not necessarily represent a positive fiscal outcome.<sup>25</sup> Table A2, Column (1) reports winning counties' population increased by 8.8% after an MDP opening. A fiscal surplus is achieved when increased revenues are greater than increased expenditures.

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<sup>25</sup> As noted below in the discussion on expenditures, the results indicate estimated own revenue increases are smaller than own expenditure increases. Balanced budget requirements are generally limited to current revenues and expenditures. The expenditure measures used below include operating and capital expenditures. The latter of which may be financed with debt even under balanced budget requirements.

Recall, decreased tax rates and/or increased public services provide evidence of fiscal surplus. Revenue per person decreases by an imprecisely measured \$56 in Table 7, Column (1), approximately 6.6% of the mean revenue per person prior to opening. The rate of revenue collection per personal income also decreases in Table 7, Column (1). Taken together without expenditure and debt information, these results suggest evidence of MDP-induce fiscal surplus.

[Insert Table 7 approximately here]

From Table 7, Column (1), winning counties collect 15.4% more property tax revenue after an MDP opening compared to GMc losers. This could be taken as an indication of either increased property tax rates or increased property tax base. The former is indicative of the winner's curse scenario. The latter may reflect upward pressure on land prices. Recall that there was not an economically or statistically significant change in earnings per worker under the revealed rankings strategy. In spatial equilibrium, a change in rents is associated with a change in wages to compensate for higher housing prices. If property values increased by 15% in winning counties, one expects a compensating change in wages. However, rents and wages will also reflect productivity, tax, service, and labor supply changes. In-migration may be placing downward pressure on wages while putting upward pressure on property values. As discussed below, there is little evidence of increased service levels in winning counties compared to *Site Selection* losers. Thus, the increase in property tax revenues must be driven by productivity spillovers, increased housing demand, and/or increased property tax rates. Unfortunately, data limitations prevent determination of each mechanism's relative explanatory power. The property tax results do not provide any evidence of a fiscal surplus distributed through lower property tax rates.

As with the economic development outcomes, the revealed rankings and matching strategies tell different stories with respect to winning county revenue. Table 7 Columns (2) – (5) report results for the geographically-proximate matching DID estimated change in counties' revenue. Winning counties experienced a significant increase in own revenue of approximately 6%. The estimated increase in general own revenues outpaces estimated increases in population,

respectively 3.3% and 4.5% (see appendix Table A2). This is reflected in winning counties' significantly increased revenue per capita. Estimates range from \$83 per person, or 9.8% of pre-opening revenue per capita, to \$125 per person, or 14.8% of pre-opening revenue per capita.

Geographically-proximate matching estimates also suggest MDPs increase revenue collection as a share of area income. These results indicate an increase in tax rates that is inconsistent with MDP-induced fiscal surplus. Increased property tax revenue accounts for most of the increase in general own revenue. Recall that earnings per worker increased by approximately 3% in winning counties compared to observable matches. Taken together, these results provide support for increased property values and increased property tax rates.<sup>26</sup>

As discussed above, a heavily incentivized MDP achieves economic development if it is associated with new economic activity and the new activity results in fiscal surplus. Changes in outstanding debt provide further insight into the relative magnitudes of revenue changes described above and cost changes described below. Table 8, Column (1) states that winning counties significantly increased their outstanding debt by 26% compared to *Site Selection* losers. Outstanding debt per capita also increases by approximately \$366 per person, as reported in the bottom panel of Table 8, Column (1). The outstanding debt per capita increase is roughly 22% of winning counties' average debt per capita in the three years prior to the MDP opening. These results provide evidence against MDP-induced fiscal surplus.

[Insert Table 8 approximately here]

Geographically-proximate observable matching estimators in Table 8, Columns (2) – (5) confirm the size and significance of winning county debt increases estimated under the revealed rankings strategy. Winning counties significantly increased outstanding debt by 24.08%, 23.84%, and 24.98% compared to the nearest one, five, and caliper propensity score neighbors,

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<sup>26</sup> Table A7 reports results corresponding to Table 7 when counterfactuals are drawn from a 100-250 mile radius of winning counties. The general own revenue and property tax revenue results are indistinguishable from those reported in Table 7, with the exception of larger reported caliper propensity score estimated increases. The increases in revenue per capita compared to the nearest covariate and nearest one and five propensity score neighbors are no longer statistically significant. The increase identified by the nearest propensity score caliper neighbors are larger than the increase reported in Table 7.

respectively. However, the estimated increase in debt per capita is much larger than the revealed rankings estimate. Winning counties significantly increased their outstanding debt per person, with estimates ranging from \$809 per person (48% of pre-opening values) to \$973 per person (58% of pre-opening debt per person).<sup>27</sup>

Fiscal surplus may be distributed through decreased tax rates and/or improvements in public services. Tables 9 – 12 present the changes in *own* expenditure and expenditure per capita on K-12 education, parks and recreation, police, and fire services, respectively. Recall from Section 2, changes in expenditure levels don't necessarily reflect changes in the level of service.

Expenditure levels will rise as population grows in response to an MDP. Expenditure per capita provides better insight into service levels, but confounding effects of factor price increases prevent attribution of all expenditure per capita changes to service level changes.

In general, estimated winning county service expenditure increased by more than own revenue. Clearly, this is consistent with the debt findings. The increase in expenditure levels is larger when identified by revealed rankings than when identified by geographically-proximate matches. Service expenditure increases coincide with mixed results on spending per capita, depending upon identification strategy.

The revealed rankings strategy estimates suggest declines or no changes in winning counties' per capita expenditures. Table 9, Column (1) indicates an insignificant decrease in education expenditure per capita despite the 13.64% increase in expenditure levels. Similarly, Table 10, Column (1) reports no change in parks and recreation spending per capita coincident with a 26.41% increase in parks and recreation expenditures. Column (1) from Tables 11 and 12 reveal declines in police and fire expenditures per capita, respectively, despite significant increases in public safety expenditures. This suggests that winning county service expenditure levels grew to keep pace with population, rather than to increase the level of services (i.e. distribute fiscal

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<sup>27</sup> The outstanding debt results identified by observable matches within 100-250 miles reported in Table A8 mirror those from Table 8 here, with the only significant difference being larger level increases for nearest propensity score caliper and covariate neighbor estimates.

surplus).

[Insert Tables 9-12 approximately here]

The geographically-proximate matching estimates suggest increased expenditure levels as well as increases in some per capita spending after successful attraction of a MDP. Table 9 Columns (2) – (5) indicate winning counties education expenditure levels increased between 4-8%; while per capita expenditure results are generally indistinguishable from zero. Parks and recreation spending levels and per capita spending increased compared to geographically-proximate matches (Table 10 Columns (2) – (5)), with per capita expenditure rising by as much as \$7 per person (nearest propensity score caliper neighbors). Similarly, public safety expenditure and expenditure per capita increased (although imprecisely estimated for some specifications). Table 11 Columns (2) – (5) suggest per capita police spending increases from \$5 - \$7 per person. Table 12 Columns (2) – (5) reveal more modest increases in fire safety spending, with per capita spending increases by as little as a penny compared to the nearest propensity score neighbor and by as much as a \$4 compared to the nearest propensity score caliper neighbors.<sup>28</sup>

When the MDP fiscal effects are identified by geographically-proximate matches, winning counties spent more on services as well some services per person after an MDP opening. This could suggest distribution of fiscal surplus through improved service levels. Yet, the substantial increase in winning county debt and debt per capita casts doubt on that conclusion indicating increased service expenditures are funded by borrowing and not a distribution of fiscal surplus. Since increased production costs may be part of the increase per person as well, expenditures

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<sup>28</sup> Tables A9-A12 in Appendix 5 report analogous estimates using counterfactual counties drawn from a 100-250 mile radius. The results are very similar to those presented in Tables 9-12, although there are a few notable exceptions. The increase in education expenditures and education expenditure per capita compared to the nearest propensity score neighbor disappears in Table A9. Table A9 also indicates a larger increase in education expenditures relative to the nearest five and caliper propensity score neighbors, but the same change in education expenditure per capita. The increase in parks and recreation spending and spending per capita identified by the nearest five propensity score neighbors in Table 10 disappears in Table A10, while the increase compared to the nearest caliper neighbors is larger in Table A10. Similarly, the increase in police and fire expenditure per capita compared to the nearest five propensity score neighbors evaporate in Tables A11 and A12; while the increase compared to the nearest caliper neighbors is larger.

provide little evidence of fiscal surplus in winning counties after an MDP opening. Coupled with the estimated increase in tax rates compared to geographically-proximate matched counties, these results do not indicate MDP-induced fiscal surplus.

The revealed rankings revenue, expenditure, and debt results also provide no evidence of fiscal surplus distributed through improved services or lower tax rates. The debt and expenditure per person results might even be interpreted as fiscal deterioration. Thus, the revealed rankings estimated increase in economic activity does not appear to generate more revenue than it costs.

## **6. Placebo Tests**

The two identification strategies tell different stories about the economic development outcomes for counties that successfully attract a large new manufacturing plant. The revealed rankings strategy results indicate that MDPs induce substantial economic activity in terms of establishments, output, and employment. However, the new economic activity is not associated with significant earnings increases or fiscal surplus. The geographically-proximate matching strategy indicates smaller increases in output and employment than the revealed rankings estimates. This strategy also didn't reveal any establishment effects. Compared to matched counties, earnings increased significantly as did tax rates, expenditures, and debt.

As discussed in the identification section above, comparison of covariate means across samples suggest that geographically-proximate matched counties more closely resemble winning counties than the revealed rankings losers. However, this resemblance is along observable dimensions and it is possible that unobservable factors dominate observable factors in determining treatment and outcomes. In order to further investigate the identification strategies, this section presents the results from placebo tests in which I estimate "treatment" effects for a set of "fake" winners compared to the counterfactuals from each strategy. The specifications are identical to those described in Section 3. "Fake" winners are the nearest propensity score neighbor to the true winner selected from anywhere in the continental United States.<sup>29</sup> If counties

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<sup>29</sup> These are not the same counties as the nearest geographically-proximate propensity score neighbors used as counterfactuals.



that look like winners, but did not receive treatment and are not a geographically-proximate match, experience “treatment” effects compared to the sets of counterfactuals used above, there is reason to doubt the validity of the identifying assumptions.

Tables 13 and 14 present the placebo test results for the economic activity and fiscal surplus outcomes, respectively. These estimates indicate significant “treatment” effects for fake winners with the revealed rankings strategy, but not with the geographically-proximate matching strategy. As can be gleaned from Table 13 Column (1), fake winning counties experience large changes in economic activity after a MDP opening when compared to the loser counties revealed by *Site Selection* magazine. Counties that resemble winners along observable dimensions increase establishments by 7.4%, value of shipments by 14.7%, value-added by 13.5%, and employment by 4.3% after MDP “treatment” without actually winning the competition for the MDP. Given that fake winners did not receive the positive MDP shock, this suggests systematically different economic performance in *Site Selection* identified loser counties generates the effects identified by the revealed rankings strategy. The geographically-proximate matching strategy does not produce changes in economic activity, with estimates generally indistinguishable from zero (Table 13 Columns (2)-(5)). Minimally, this indicates that the effects identified by geographically-proximate observable matches are not biased by systematic positive or negative differences in to counterfactual counties. Similarly, Table 14 suggests changes in fiscal outcomes induced by “treatment” when identified by revealed ranking strategy but not geographically-proximate matches. Education spending per capita is an exception, with significant increases estimated by the geographically-proximate matching strategy.

The results in Tables 13 and 14 cast further doubt on the validity of the identifying assumptions for the revealed rankings identification strategy. They suggest that the outcomes in the losing counties identified by *Site Selection* magazine do not represent the outcomes in the winning counties in the absence of treatment (winning the MDP). As discussed in Section 4, there are a number of ways in which the identifying assumptions may be violated. In over one-third of the cases, “treatment” is negatively influencing economic outcomes in the counterfactual

counties because these are the counties from which the firm relocated. Expectations about poor, future economic development outcomes may lead the *Site Selection* losers to make large incentives bids – leading companies to reveal these counties as runners-up for strategic reasons rather than because they are the second-best profit-maximizing location. Assignment to treatment and control groups is therefore related to the determinants of outcomes, with *Site Selection* losers having systematically worse economic outlooks that bias results. It is also possible that selection depends on changes that affect outcomes, which violates the DID identifying assumptions.

Tables 13 and 14 also suggest that the geographically-proximate matching strategy addresses many of these threats to identification. The placebo test results provides additional evidence in favor of the Ho et al. (2007) and Wooldridge and Imbens (2009) claim that combining DID estimation with pre-processing the data provides robust estimates of treatment effects. Ferraro and Miranda (2014a) demonstrate that combining the DID estimator with pre-processing the data through matching produces estimates very close to those from an experimental design. Pre-processing the data based upon observable covariate and geographic proximity ensures treatment and control groups look similar (in levels) prior to treatment and face similar macro trends. Unlike using the counties from which the plants relocated as counterfactuals, the geographically-proximate matching strategy also minimizes the threat that assignment to treatment and control groups is related to determinants of outcomes. Thus, the geographically-proximate matching estimates are the preferred estimates of MDP-induced economic development outcomes.

The revealed rankings and geographically-proximate matching strategies both assume that outcomes are not driven by important time-varying county characteristics unaccounted for by their respective conditioning sets. As a final robustness check, Tables A13 and A14 in the online appendix present results from a covariate-augmented version of Model 1 that includes controls for major industry shares and population. The establishment, output, and employment effects compared to *Site Selection* losers with additional control variables are smaller than those reported in Section 6; while there is generally little change in the results for geographically proximate propensity score matches. For example, the covariate-augmented revealed rankings

estimated increase in winning counties' value of shipments is 14.3% compared to the 24.6% in Table 4. This suggests that a substantial portion of the effect identified by the revealed rankings strategy may be attributed to industrial composition and population changes. On the other hand, Table A14 reports winning counties' value of shipments increased by 12.2% compared to the nearest five propensity score neighbors, which is very close to the 13.3% reported for estimates without covariate controls in Table 4. These results provide further evidence in support of geographically-proximate matching as the preferred strategy.

## **7. Conclusions**

Despite the lack of scholarly consensus on the effects of economic development incentives, they remain the primary economic development tool for many local governments. Local officials justify luring large firms with large incentives packages on the basis that such firms generate significant economic development. This paper contributes to the debate by investigating whether a set of heavily incentivized large firms induce economic development – new economic activity and fiscal surplus- in winning counties. Estimating the effect of winning the competition for a large new firm presents an identification challenge. Selection as the location for a new plant is not random, but rather depends on a number of observable and unobservable factors. Like most economic development policies, an ideal experiment from which we can garner estimates is unlikely. This paper also provides a methodological contribution by comparing results from two quasi-experimental research designs in an attempt to get close to the experimental ideal. Specifically, this paper augments the DID estimator with two control by design methods: revealed rankings and geographically proximate matches. The revealed rankings approach is the Greenstone, Hornbeck, and Moretti (2010) identification strategy and identifies counterfactual counties from *Site Selection* magazine reports. The geographically-proximate matching strategies follows recommendations from the treatment effects literature and identifies winner county counterfactuals by matching on observables known to determine selection and outcomes as well as geography. The addition of geographic proximity as a match criteria controls for a large number of unobservables correlated with both winning and outcomes, such as factor markets and

incentives policies.

Local governments often must choose between allocating scarce resources to education, infrastructure, attracting an MDP, or other economic development activities. The large productivity spillovers documented by GHM suggest that successful attraction of an MDP may generate unique benefits. However, this paper's results indicate successful attraction of an MDP is not economic development's "magic bullet". The analysis also demonstrates that estimates of MDP effects are sensitive to identification strategy. From a policy perspective, the differences in estimated effects imply a large range of cost-benefit ratios. Using the cases for which subsidy values are available, the back-of-the-envelope average subsidy cost per (direct plus induced) job is \$40,829 under the GHM revealed rankings strategy and an average of \$79,500 per job under the geographically-proximate matching strategy. The paper's comparison of the GHM natural experiment and the geographically-proximate matching methodologies also contributes to the ongoing debate surrounding quasi-experimental research design. Does conditioning on revelation in the magazine capture the most important unobservables driving future expected profits (selection) and outcomes? Does it do so better than conditioning on observable determinants and geography? It seems unlikely that the unobservables captured by the revealed rankings strategy eclipse known determinants as well as shared factor markets and unobservables captured by geography. Further inspection of the institutional environment surrounding the *Site Selection* magazine losers casts doubt on the revealed rankings strategy, with counties that offer substantial incentive packages or counties from which the plant relocated much more likely to be included as a *Site Selection* counterfactual. The results from placebo tests also indicate that the *Site Selection* magazine losers provide invalid counterfactual outcomes for the winners in the absence of treatment. Thus, the geographically-proximate matching estimates are preferred.

The preferred estimates suggest that MDPs induce small increases in output and employment as well as significant increases in earnings. Upward pressure on wages may explain the lack of establishment effects. Highly incentivized MDPs are also associated increased tax rates and debt. The results suggest that even with significant productivity spillovers, the general equilibrium

effects of directing public resources towards MDPs may dominate them.

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Table 1: GMc County Characteristics by Winner Status

Variable	GMc Winners	GMc Losers	%bias	p>t
Panel A				
Total Population (1,000's)	230	340	-33.6	0.026
Interstate	0.8889	0.8976	-2.8	0.845
Nearest Metro (km)	32.6030	22.0270	30.2	0.018
Working Age Share of Pop.	0.4046	0.4053	-2	0.875
Minority Share of Pop.	0.1518	0.1764	-17.4	0.218
Earnings	17.3310	18.4270	-25.2	0.063
Mfg Share of Employment	0.2114	0.1693	42	0.005
Farm Share of Employment	0.0480	0.0210	47.7	0.000
FIRE Share of Employment	0.0611	0.0628	-5.7	0.716
Service Share of Employment	0.2170	0.2223	-8	0.642
Military Share of Employment	0.0143	0.0206	-18.6	0.243
N	63	93		
Panel B				
Manufacturing Establishments	380.17	714.84	-51.9	0.007
Value of Shipments (\$1,000's)	3,100,000	4,800,000	-37.90	0.045
Value-Added (\$1,000's)	1,500,000	2,300,000	-37.50	0.043
General Own Revenue (\$1,000's)	230,000	500,000	-61.10	0.002
Total Property Tax Revenue (\$1,000's)	120,000	280,000	-60.80	0.004
K-12 Expenditure(\$1,000's)	150,000	300,000	-61.30	0.002
Fire Expenditure (\$1,000's)	10,091	23,492	-59.00	0.003
Parks & Rec Expenditure (\$1,000's)	7,274	19,108	-56.90	0.005
Police Expenditure (\$1,000's)	18,037	40,876	-56.90	0.002
Outstanding Debt (\$1,000's)	370,000	770,000	-48.60	0.008
G.O. Revenue Per Capita	0.8458	0.9549	-22.00	0.248
Education Expend. Per Capita	0.5630	0.6325	-33.80	0.081
Fire Expenditure Per Capita	0.0311	0.0420	-49.50	0.012
Parks & Rec Per Capita	0.0217	0.0330	-51.40	0.010
Police Expenditure Per Capita	0.0591	0.0736	-44.10	0.020
Outstanding Debt Per Capita	1.675	1.5242	7.30	0.693
N	60	90		

Notes: Panel A reports mean county characteristics by winner status for three years prior to the announcement of the MDP opening. Panel B reports mean county characteristics by winner status for the Census of Governments or Census of Manufactures survey year prior to the announcement. Per capita values are measured as \$1,000's per capita. The losers for each case are weighted by the inverse of their number.

Table 2: Matched County Characteristics and Balancing Tests

Variable	Winners	Nearest 1 PS		Nearest 5 PS		Nearest Odds Ratio		Nearest Covariate	
	(n=60)	Neighbors (n=56)		Neighbors (n=278)		Neighbors (n=746)		Neighbors (n=54)	
	Mean	Mean	p>t	Mean	p>t	Mean	p>t	Mean	p>t
Manufacturing Establishments	380.17	311.64	0.493	355.22	0.897	285	0.391	285.3	0.327
Value of Shipments (\$1,000's)	3,100,000	2,000,000	0.105	2,300,000	0.431	1,800,000	0.099	2,200,000	0.272
Value-Added (\$1,000's)	1,500,000	900,000	0.188	1,100,000	0.476	890,000	0.157	990,000	0.231
General Own Revenue	230,000	180,000	0.407	190,000	0.649	170,000	0.361	180,000	0.589
Total Property Tax Revenue	120,000	82,395	0.289	93,369	0.601	73,393	0.337	100,000	0.813
K-12 Expenditure	150,000	120,000	0.467	130,000	0.698	110,000	0.375	120,000	0.565
Fire Expenditure	10,091	8,343	0.558	9,104	0.833	7,572	0.483	7,307	0.552
Parks & Rec Expenditure	7,274	7,824	0.841	8,811	0.762	7,393	0.976	5,488	0.771
Police Expenditure	18,037	14,636	0.488	17,728	0.974	14,824	0.647	15,108	0.816
Outstanding Debt	370,000	330,000	0.786	320,000	0.725	330,000	0.773	290,000	0.679
G.O. Revenue Per Capita	0.8458	0.7305	0.130	0.7340	0.147	0.7200	0.104	0.7705	0.379
Education Expend. Per Capita	0.5630	0.5555	0.808	0.5688	0.860	0.5718	0.792	0.5805	0.777
Fire Expenditure Per Capita	0.0311	0.0294	0.641	0.0284	0.465	0.0262	0.186	0.0304	0.997
Parks & Rec Per Capita	0.0217	0.0234	0.608	0.0229	0.741	0.0211	0.869	0.0211	0.893
Police Expenditure Per Capita	0.0591	0.0550	0.434	0.0557	0.557	0.0537	0.335	0.0584	0.886
Outstanding Debt Per Capita	1.675	0.9751	0.067	0.9930	0.075	1.0715	0.170	1.2827	0.825

Notes: The table reports mean county characteristics by winner status for the Census of Governments or Census of Manufactures survey year prior to the announcement.

Government finance variables are the aggregates of all local governments within the county and measured in \$1,000s. The losers for each case are weighted by the inverse of their number.

Table 3: Change in Winning Counties' Establishments

	(1)	(2)	(3)	(4)	(5)
Difference-in-Differences	0.0810*** (0.0286)	-0.0083 (0.0314)	-0.0011 (0.0240)	0.0122 (0.0223)	-0.0527* (0.0314)
R <sup>2</sup>	0.9918	0.9891	0.989	0.9895	0.989
N	598	461	1339	3189	429

Notes: The table presents results the Model 1 estimated change in winning counties' (log) county establishments. Column headings refer to identification strategies as follows: (1) Revealed rankings; (2) Nearest propensity score neighbor; (3) Nearest 5 propensity score neighbors; (4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors. Robust standard errors are in parentheses. Census of Manufactures pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

Table 4: Change in Winning Counties' Output (Value of Shipments and Value Added)

	(1)	(2)	(3)	(4)	(5)
Value of Shipments					
Difference-in-Differences	0.2456*** (0.0595)	0.1097* (0.0632)	0.1332*** (0.0505)	0.1105** (0.0464)	0.0709 (0.0654)
R <sup>2</sup>	0.9743	0.9736	0.9728	0.9753	0.9734
Value Added					
Difference-in- Differences	0.2233*** (0.0564)	0.0924 (0.0623)	0.1085** (0.0478)	0.0962** (0.0430)	0.0316 (0.0634)
R <sup>2</sup>	0.9755	0.9743	0.972	0.9754	0.9731
N	571	434	1258	2971	404

Notes: The table presents Model 1 estimated changes in winning counties' log value of shipments (measured in thousands of dollars and deflated) and log value-added (measured in thousands of dollars and deflated). Column headings refer to identification strategies as follows: (1) Revealed rankings; (2) Nearest propensity score neighbor; (3) Nearest 5 propensity score neighbors; (4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors. Robust standard errors are in parentheses. Census of Manufactures pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

Table 5: Change in Winning Counties' Earnings per Worker

	(1)	(2)	(3)	(4)	(5)
Model 1					
Mean Shift	-0.0009 (0.0045)	0.0319*** (0.0049)	0.0304*** (0.0040)	0.0265*** (0.0039)	0.0354*** (0.0050)
R <sup>2</sup>	0.9815	0.9762	0.9758	0.9761	0.977
Model 2					
Effect after 5 years	0.0106 (0.0166)	0.0255 (0.0176)	0.0201 (0.0143)	0.0207 (0.0139)	0.0325* (0.0186)
Level Change	0.0044 (0.0067)	0.0096 (0.0072)	0.0101* (0.006)	0.007 (0.0059)	0.0162** (0.0075)
Trend Break	0.001 (0.0026)	0.0027 (0.0027)	0.0017 (0.0023)	0.0023 (0.0022)	0.0027 (0.0029)
R <sup>2</sup>	0.9815	0.9763	0.9759	0.9762	0.9771
N	2028	1586	4628	11193	1482

Notes: The table presents results from ten separate regressions. Column headings refer to identification strategies as follows: (1) Revealed rankings; (2) Nearest propensity score neighbor; (3) Nearest 5 propensity score neighbors; (4) Nearest propensity score

radius neighbors; (5) Nearest covariate neighbors. Robust standard errors are in parentheses. The top panel reports the mean shift in winning counties' logged wage employment from estimating Model 1. The bottom panel reports the results from estimating Model 2 with logged wage employment as the dependent variable. The estimated change after five years is calculated by  $\theta_1 + 6\theta_2$  to allow an effect in  $\tau = 0$ .

Table 6: Change in Winning Counties' Wage and Salary Employment

	(1)	(2)	(3)	(4)	(5)
Model 1					
Mean Shift	0.0800*** (0.0082)	0.0419*** (0.0088)	0.0471*** (0.0074)	0.0500*** (0.0073)	0.0310*** (0.0092)
R <sup>2</sup>	0.997	0.9966	0.9963	0.9964	0.9963
Model 2					
Effect after 5 years	0.0760 (0.0313)	0.0615 (0.0337)	0.0389 (0.0286)	0.0445 (0.0282)	0.0393 (0.0354)
Level Change	0.0265** (0.0125)	0.0118 (0.0133)	0.0132 (0.0113)	0.0144 (0.0112)	0.01 (0.0138)
Trend Break	0.0083* (0.0048)	0.0083 (0.0051)	0.0043 (0.0043)	0.005 (0.0043)	0.0049 (0.0053)
R <sup>2</sup>	0.9971	0.9966	0.9964	0.9964	0.9963
N	2028	1586	4628	11193	1482

Notes: The table presents results from ten separate regressions. Column headings refer to identification strategies as follows: (1) Revealed rankings; (2) Nearest propensity score neighbor; (3) Nearest 5 propensity score neighbors; (4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors. Robust standard errors are in parentheses. The top panel reports the mean shift in winning counties' logged wage employment from estimating Model 1. The bottom panel reports the results from estimating Model 2 with logged wage employment as the dependent variable. The estimated change after five years is calculated by  $\theta_1 + 6\theta_2$  to allow an effect in  $\tau = 0$ .

Table 7: Change in Winning Counties' Revenue

	(1)	(2)	(3)	(4)	(5)
ln(General Own Revenue)	0.1450*** (0.0378)	0.0631 (0.0426)	0.0611* (0.0330)	0.0608* (0.0316)	0.0618 (0.0422)
R <sup>2</sup>	0.985	0.9817	0.9827	0.9835	0.9806
Revenue Per Capita	-0.0565 (0.0424)	0.0830* (0.0450)	0.0876** (0.0350)	0.1036*** (0.0327)	0.1253*** (0.0423)
R <sup>2</sup>	0.875	0.8518	0.853	0.8540	0.8649
Revenue Per Personal Income	-0.0789*** (0.0264)	0.0471** (0.0222)	0.0328* (0.0181)	-0.0003 (0.0021)	0.0016 (0.0022)
R <sup>2</sup>	0.8758	0.87	0.8661	0.7544	0.7525
ln(Property Tax Revenue)	0.1540*** (0.0341)	0.047 (0.0401)	0.0601** (0.0293)	0.0501* (0.0283)	0.0341 (0.0412)
R <sup>2</sup>	0.989	0.9856	0.9863	0.9870	0.9838
N	750	570	1670	3985	540

Notes: The table presents results from twenty separate regressions Model 1 regressions. Columns report estimated changes from four counterfactual matching method as follows: (1) Revealed rankings; (2) Nearest propensity score neighbor; (3) Nearest 5

propensity score neighbors; (4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated. Data is from the Census of Governments. Expenditure is measured in \$1,000s. County variables are the aggregates of all local governments within the county. Pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

Table 8: Change in Winning Counties' Outstanding Debt

	(1)	(2)	(3)	(4)	(5)
ln(Outstanding Debt)	0.2609*** (0.0749)	0.2408*** (0.0852)	0.2384*** (0.0632)	0.2498*** (0.0594)	0.1524 (0.0930)
R <sup>2</sup>	0.9453	0.9431	0.9444	0.9472	0.9319
Outstanding Debt Per Capita	0.3661 (0.4412)	0.8089* (0.4347)	0.9447** (0.4088)	0.9734** (0.3970)	0.6147** (0.2772)
R <sup>2</sup>	0.596	0.5854	0.5855	0.5862	0.5533
N	750	570	1670	3985	540

Notes: The table presents results from ten separate Model 1 regressions. Columns report estimated changes from counterfactual methods as follows: (1) Revealed rankings; (2) Nearest propensity score neighbor; (3) Nearest 5 propensity score neighbors; (4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated. Data is from the Census of Governments. Expenditure is measured in \$1,000s. County variables are the aggregates of all local governments within the county. Pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

Table 9: Change in Winning Counties' Education Expenditure

	(1)	(2)	(3)	(4)	(5)
ln(Education Expenditure)	0.1364*** (0.0252)	0.0804*** (0.0297)	0.0432* (0.0262)	0.0516** (0.0225)	0.0457 (0.0293)
R <sup>2</sup>	0.9916	0.9895	0.9807	0.9848	0.9888
Education Expenditure Per Capita	-0.0144 (0.0175)	0.0400** (0.0158)	0.0181 (0.0120)	0.0183 (0.0116)	0.0177 (0.0163)
R <sup>2</sup>	0.9325	0.9412	0.94	0.9365	0.9424
N	750	570	1670	3985	540

Notes: The table presents the mean shifts from ten separate Model 1 regressions. Columns report estimated changes from counterfactual methods as follows: 1) Revealed rankings; (2) Nearest propensity score neighbor; (3) Nearest 5 propensity score neighbors; (4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated. Data is from the Census of Governments. Expenditure is measured in \$1,000s. County variables are the aggregates of all local governments within the county. Pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

Table 10: Changes in Winning Counties' Parks and Recreation Services Expenditure

	(1)	(2)	(3)	(4)	(5)
ln(Parks and Recreation Expenditure)	0.2641*** (0.0797)	0.1057 (0.0871)	0.1419** (0.0675)	0.1413** (0.0652)	0.1422 (0.0889)
R <sup>2</sup>	0.9564	0.9478	0.9517	0.9523	0.9478

### Parks and Recreation

Expenditure Per	-0.0005	0.0045	0.0059**	0.0066***	0.0045
Capita	(0.0029)	(0.0028)	(0.0024)	(0.0023)	(0.0033)
R <sup>2</sup>	0.7275	0.6786	0.6785	0.6815	0.6465
N	750	570	1670	3985	540

Notes: The table presents the mean shifts from ten separate Model 1 regressions. Columns report estimated changes from counterfactual methods as follows: 1) Revealed rankings; (2) Nearest propensity score neighbor; (3) Nearest 5 propensity score neighbors; (4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated. Data is from the Census of Governments. Expenditure is measured in \$1,000s. County variables are the aggregates of all local governments within the county. Pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

**Table 11: Change in Winning Counties' Police Service Expenditure**

	(1)	(2)	(3)	(4)	(5)
ln(Police	0.1597***	0.0934**	0.0768***	0.0825***	0.1047***
Expenditure)	(0.0331)	(0.0375)	(0.0297)	(0.0283)	(0.0404)
R <sup>2</sup>	0.9893	0.9868	0.9862	0.9863	0.9855
	-				
Police Expenditure	0.0104***	0.0055*	0.0052**	0.0070***	0.0062*
Per Capita	(0.0033)	(0.0029)	(0.0024)	(0.0023)	(0.0032)
R <sup>2</sup>	0.8862	0.8796	0.8758	0.8616	0.8758
N	750	570	1670	3985	540

Notes: The table presents the mean shifts from ten separate Model 1 regressions. Columns report estimated changes from counterfactual methods as follows: 1) Revealed rankings; (2) Nearest propensity score neighbor; (3) Nearest 5 propensity score neighbors; (4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated. Data is from the Census of Governments. Expenditure is measured in \$1,000s. County variables are the aggregates of all local governments within the county. Pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

**Table 12: Change in Winning Counties' Fire Service Expenditures**

	(1)	(2)	(3)	(4)	(5)
ln(Fire Expenditure)	0.1936***	0.0679	0.0683	0.0720	0.1428**
	(0.0575)	(0.0641)	(0.0507)	(0.0476)	(0.0710)
R <sup>2</sup>	0.9756	0.9719	0.97	0.9715	0.9654
Fire Expenditure Per	-0.0074***	0.0001	0.0030*	0.0036**	0.0013
Capita	(0.0022)	(0.0023)	(0.0016)	(0.0016)	(0.0025)
R <sup>2</sup>	0.8788	0.8439	0.8508	0.8443	0.8286
N	750	570	1670	3985	540

Notes: The table presents the mean shifts from ten separate Model 1 regressions. Columns report estimated changes from counterfactual methods as follows: 1) Revealed rankings; (2) Nearest propensity score neighbor; (3) Nearest 5 propensity score neighbors; (4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated. Data is from the Census of Governments. Expenditure is measured in \$1,000s. County variables are the aggregates of all local governments within the county. Pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

Table 13: Economic Activity Placebo Tests

	(1)	(2)	(3)	(4)	(5)
Establishments	0.0740*** (0.0277)	-0.0151 (0.0308)	-0.0081 (0.0233)	0.0054 (0.0215)	-0.0500* (0.0298)
N	596	459	1337	3187	451
Value of Shipments	0.1473*** (0.0530)	0.0122 (0.0573)	0.0368 (0.0439)	0.0136 (0.0396)	-0.0292 (0.0571)
N	569	432	1256	2969	424
Value Added	0.1350** (0.0558)	0.0052 (0.0616)	0.0218 (0.0475)	0.0091 (0.0424)	-0.0601 (0.0618)
N	569	432	1256	2969	424
Earnings	-0.0291*** (0.0043)	0.0037 (0.0047)	0.0023 (0.0037)	-0.0016 (0.0037)	0.0038 (0.0046)
N	2028	1586	4628	11193	1560
Employment	0.0428*** (0.0077)	0.0050 (0.0083)	0.0103 (0.0068)	0.0132* (0.0068)	-0.0030 (0.0087)
N	2028	1586	4628	11193	1560

Notes: The table presents the mean shifts from twenty-five separate Model 1 placebo test regressions using fake winners. Columns report estimated changes from counterfactual methods as follows: 1) Revealed rankings; (2) Nearest propensity score neighbor; (3) Nearest 5 propensity score neighbors; (4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated.

Table 14: Fiscal Surplus Placebo Tests

	(1)	(2)	(3)	(4)	(5)
General Own Revenue	0.0724** (0.0348)	-0.0094 (0.0400)	-0.0110 (0.0301)	-0.0112 (0.0288)	-0.0289 (0.0409)
G.O. Revenue Per Capita	-0.1277*** (0.0383)	0.0119 (0.0411)	0.0170 (0.0303)	0.0331 (0.0280)	0.0342 (0.0375)
Revenue Per Personal Income	0.0022 (0.0017)	-0.0006 (0.0019)	-0.0006 (0.0014)	-0.0006 (0.0014)	-0.0009 (0.0020)
Property Tax Revenue	0.1051*** (0.0334)	-0.0019 (0.0395)	0.0116 (0.0285)	0.0016 (0.0275)	-0.0158 (0.0403)
Outstanding Debt	0.1281 (0.0877)	0.1086 (0.0971)	0.1070 (0.0781)	0.1186 (0.0742)	0.0055 (0.1018)
Debt Per Capita	-0.1419 (0.3256)	0.3070 (0.3176)	0.4423 (0.2917)	0.4702* (0.2823)	0.3134 (0.3229)
Education Expenditure	0.0795*** (0.0258)	0.0237 (0.0305)	-0.0134 (0.0271)	-0.0049 (0.0233)	-0.0099 (0.0299)



Education Spending Per Capita	-0.0005 (0.0180)	0.0539*** (0.0164)	0.0320** (0.0128)	0.0322*** (0.0123)	0.0265 (0.0163)
Parks and Rec. Expenditure	0.2201** (0.0904)	0.0634 (0.0976)	0.0998 (0.0809)	0.0989 (0.0775)	0.1333 (0.0983)
Parks & Rec. Spending Per Capita	-0.0067*** (0.0025)	-0.0017 (0.0024)	-0.0003 (0.0020)	0.0004 (0.0019)	0.0007 (0.0029)
Police Expenditure	0.0661* (0.0367)	-0.0002 (0.0408)	-0.0164 (0.0339)	-0.0104 (0.0323)	0.0154 (0.0427)
Police Spending Per Capita	-0.0116*** (0.0040)	0.0042 (0.0037)	0.0040 (0.0033)	0.0058* (0.0032)	0.0051 (0.0039)
Fire Expenditure	0.1611*** (0.0605)	0.0356 (0.0669)	0.0361 (0.0546)	0.0400 (0.0513)	0.0929 (0.0717)
Fire Spending Per Capita	-0.0113*** (0.0022)	-0.0038* (0.0023)	-0.0009 (0.0016)	-0.0003 (0.0017)	-0.0018 (0.0025)
N	750	570	1670	3985	570

Notes: The table presents the mean shifts from twenty-five separate Model 1 placebo test regressions using fake winners. Columns report estimated changes from counterfactual methods as follows: 1) Revealed rankings; (2) Nearest propensity score neighbor; (3) Nearest 5 propensity score neighbors; (4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated.

## ONLINE APPENDIX

### Appendix 1: MDP Sample

The sample of cases was constructed to replicate the sample cases from Greenstone, Hornbeck, and Moretti (2010) (GHM). The GHM sample cases were drawn from the “Million Dollar Plant” (MDP) sample outlined in Greenstone and Moretti (2003) (GM). GM states that they construct the sample from the “Million Dollar Plant” (MDP) articles in *Site Selection Magazine*. A number of irregularities are encountered when trying to reproduce their sample from the primary source documents. This section documents the paper’s sample.

During the sample period, the name of the publication changes three times and the “Million Dollar Plant” feature ceases to appear in the magazine. The magazine referenced in GM, *Site Selection Magazine*, doesn’t exist as a publication until 1995 – two years after the end of the GM sample period. From 1982-1984, there exist two publications called *Site Selection Handbook* and *Industrial Development*. MDP feature articles appear in *Industrial Development*. The two publications were merged into one publication called *Industrial Development and Site Selection Handbook* from 1985-1988 (issues 1-4). The name was then changed to *Site Selection and Industrial Development* 1988(issues 5 -6)-1994.

MDP ceases to appear as a feature in the magazine in 1988. During the period when the MDP feature was appearing, there was another regular feature in the magazine called “Scoreboard” which appears to be a source used for the GM sample. In 1988, a new regular feature called Location Report (LR) begins and appears to be the source feature for GM.

There are also methodological irregularities in case selection from the sources documents. Specifically, it isn’t clear how the cases were selected from MDP, Location Report, and Scoreboard features. Additionally, it isn’t clear where some cases come from at all.

Note that the case numbers referenced here are those presented in GM.

Examining the years where the MDP feature is there (1983-1987), the following GM cases are not in the MDP feature articles: Boeing (25), Fuji/Isuzu (24), Toyota (19), Saturn (18), Tubular Corp (12), Whirlpool (9), General Motors (9). Although Ft. Howard Paper (16) does not

appear in the MDP feature, it can be found in the Scorecard feature. However, both Combustion Engineering and Otsuka Pharmaceuticals Manufacturing appeared as MDP articles during the period, have the winner and losers identified; yet, do not appear in the GM sample.

Examining the years where the feature is called Location Report [1988-1993], the following cases are in the GM sample, but not in LR: Eastman Kodak (32), Albertson's (33), Boeing (48), Tennessee Eastman (49), Ford (54), Scott Paper (66)<sup>1</sup>, Safeway (67), Sterling Drug (76). The following cases appear in LR with winner and loser identified, but are not in the GM sample: US West, Sematech, Chase Manhattan, Phoenix Research Corp., Avon, USAA, Bridgestone, Exxon, Heinz, Lockheed Corp., UPS, J.C. Penney, BASF Corp., Computer Logics, Fujitsu Business Communications Systems, Lane Bryant, Marriott Corp., Michelin Aircraft Tire, Salomon Bros., Hewlett-Packard, Key Communications, Dollar Rent A Car, CARE, Southwestern Bell Corp., Spiegel, Peterbilt Motor Company, Dell, Transamerica Life.

Many of the missing cases are in the same article as included cases. A particularly odd example is a June 1991 list of recent (last 3 years) corporate headquarter relocations which includes the excluded cases of J.C. Penney, BASF Corp., Computer Logics, Fujitsu Business Communications Systems, Lane Bryant, Marriott Corp., Michelin Aircraft Tire . The same list is the only appearance of the included Adidas USA and American Auto cases (these two GM companies don't appear in any other articles).

There are also some minor errors in the GM sample construction from the primary documents. For example, there are cases that are counted twice in the sample because the same search is mentioned in multiple features. Specifically, the double counted cases are: United Airlines (59) and (65), with the wrongly identified winner in (59); Holiday Inn (56) and Bass (50), (56) only lists one of the previous locations.

It is also unclear how the winning and losing counties were determined for some cases. While cases that GM lists a different winner than the magazine are likely corrected or omitted in

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<sup>1</sup> GM include a Scott Paper case from the year 1992. There is a LR on Scott Paper in 1990 with the same winner and loser as well as an additional loser.

the GHM sample, that is not so for the cases with incorrectly identified losers and included losers not mentioned in the articles. Specifically, cases with incorrectly identified winners were: Codex (Motorola) (11) – listed as Middlesex in GM, but actually in Norfolk; Squibb- listed as Camden, but located in Middlesex; United Airlines (59) – lists the leading contender, Denver, as the winner; however, the actual winner is in a later article, which also receives a case number, United Airlines (65).

In two cases, the wrong loser (based on the article information only) is included in the GM sample: Formosa Plastics (43) – Galveston, TX is in the GM sample and Jefferson, TX is the runner up location identified in the article; Racal-Milgo (3) – Pasco, FL listed as the loser in the GM sample but article cites Palm Beach, FL.

There are quite a few more GM cases where the listed loser is not mentioned in the article: Timken Co (1) – article does not specifically mention a loser county, only that other sites in Kentucky, Tennessee, Virginia, and Ohio were considered; GE (2) – article does not specifically mention a loser county, only that the four finalists were all in the Southeast and loser in the sample is in Indiana; Boeing (64) – winner county mentioned in an article that year as being the location of a move between two cities in the county; Formosa Plastics (43) – Galveston not mentioned in the article but not listed as a loser; Squibb (41); Yamaha (26); DuPont/Phillips (21) – article only says search concentrated on Research Triangle area; Ft. Howard Paper (16) – Effingham, SC is never mentioned in the article, it only says across the river in SC; Schlegel (82); Codex (Motorola) (11) – Briston, MA is identified as the loser, but it is only mentioned in the article as the location of an existing plant that was one of two facilities they wanted to be near; Mercedes (81) – the article says that Melba, NC was the runner-up site, the other counties included in the GM sample aren't. Table A1 (at the end of this section) summarizes the GM cases as well as the magazine cases with both the winner and loser identified. The paper utilizes the GM sample with minor corrections that were likely either: identified and corrected in the GHM sample or lead to the cases' exclusion in the GM sample. Specifically, the following classes of minor corrections were made:

- a. Cases where the winner was incorrectly identified in the GM sample had the winner replaced with the winner identified in the magazine article. However, cases which do not appear in the magazine at all are retained.
- b. Cases which are double-counted in the GM sample have the most accurate case retained. The least accurate case is dropped.
- c. Cases where the GM loser is different than the loser identified in the magazine article have that loser replaced with the one identified in the magazine. However, GM cases in which no loser is mentioned in the article are retained.

**Table A1: MDP Sample Summary**

My Case	GM Case	GM Year	Pub Year	Company	Major Divison	County	winner/ loser	GM Sample	Site Selection Mag
1	1	1982	1982	Timken	Mfg	Stark, OH	winner	y	y
						Montgomery, VA	loser	y	n
2	2	1982	1982	General Electric	Mfg	Lowndes, AL	winner	y	y
						Posey, IN	loser	y	n
3	3	1982	1982	Racal-Milgo	Services	Broward, FL	winner	y	y
						Dade, FL	loser	y	y
						Pasco, FL	loser	y	n
						Palm Beach, FL	loser	n	y
4	4	1982	1982	Pitney-Bowes	Services	Fayette, GA	winner	y	y
						Hamilton, OH	loser	y	y
5	5	1982	1982	Corning/Kroger	Mfg	Clark, KY	winner	y	y
						Montgomery, KY	loser	y	y
6	6	1983	1983	Verbatim	Mfg	Mecklenburg, NC	winner	y	y
						Wake, NC	loser	y	y
7	7	1983	1983	American Solar King	Mfg	McLennan, TX	winner	y	y
8	8	1983	1983	Hewlett-Packard	Mfg	Snohomish, WA	winner	y	y
						King, WA	loser	y	y
						Larimer, CO	loser	y	y
						Santa Clara, CA	loser	y	y
9			1983	Merrill Lynch	FIRE	Shelby, TN	winner	n	y
						Davidson, TN	loser	n	y
10	9	1984		Whirlpool	Mfg	Rutherford, TN	winner	y	n
						Vanderburgh, IN	loser	y	n
11	9	1984		General Motors	Mfg	St. Charles, MO	winner	y	n
						St. Louis, MO	loser	y	n
12	11	1984	1984	Codex (Motorola)	Mfg	Middlesex, MA	winner	y	n
						Bristol, MA	loser	y	n
13		1984	1984	Codex (Motorola)	Mfg	Norfolk, MA	winner	n	y
14			1984	Otsuka Pharmaceutical	Mfg	Montgomery, MD	winner	n	y
						San Diego, CA	loser	n	y
					Mfg	Suffolk, MA	loser	n	y
						New York, NY	loser	n	y
						Santa Clara, CA	loser	n	y
15	12	1985		Tubular Corp	Mfg	Muskogee, OK	winner	y	n
						Phillips, AR	loser	y	n
16	13	1985	1985	TRW	Services	Fairfax, VA	winner	y	y
						Loudoun, VA	loser	y	y

17	14	1985	1985	Kyocera	Mfg	Montgomery, MD	loser	y	y
						Clark, WA	winner	y	y
						E. Baton Rouge, LA	loser	y	y
						Travis, TX	loser	y	y
						Bernalillo, NM	loser	y	y
						Nueces, TX	loser	y	y
18	15	1985	1985	AiResearch	Mfg	Pima, AZ	winner	y	y
						El Paso, CO	loser	y	y
						Bernalillo, NM	loser	y	y
19	16	1985	1985	Ft. Howard Paper	Mfg	Effingham, GA	winner	y	y
20	17	1985	1985	Rockwell International	Mfg	Jasper, SC	loser	y	n
						Johnson, IA	winner	y	n
						Linn, IA	loser	y	n
21	18	1986		Saturn	Mfg	Maury, TN	winner	y	n
						Grayson, TX	loser	y	n
						Kalamazoo, MI	loser	y	n
						Shelby, KY	loser	y	n
22	19	1986		Toyota	Mfg	Scott, KY	winner	y	n
						Wilson, TN	loser	y	n
						Wyandotte, KS	loser	y	n
23	20	1986	1986	Canon	Mfg	Newport News, VA	winner	y	y
						Henrico, VA	loser	y	y
24	21	1986	1986	DuPont/Phillips	Mfg	Cleveland, NC	winner	y	y
						Durham, NC	loser	y	n
25	22	1986	1986	Nippon Columbia	Mfg	Morgan, GA	winner	y	y
						Buncombe, NC	loser	y	y
26	23	1986	1986	Mack	Mfg	Fairfield, SC	winner	y	y
						Richland, SC	loser	y	y
						Lehigh, PA	loser	y	y
27	24	1987		Fuji/Isuzu	Mfg	Tippecanoe, IN	winner	y	n
						Sangamon, IL	loser	y	n
						Hardin, KY	loser	y	n
28	25	1987		Boeing	Mfg	Calcasieu, LA	winner	y	n
						Oklahoma, OK	loser	y	n
						Duval, FL	loser	y	n
29	26	1987	1986	Yamaha	Mfg	Coweta, GA	winner	y	y
						Kendall, IL	loser	y	n
30	27	1987	1987	Carnation	Mfg	Kern, CA	winner	y	y
						Stanislaus, CA	loser	y	y
31	28	1987	1987	Knauf Fiber Glass	Mfg	Chambers, AL	winner	y	y
						Muscogee, GA	loser	y	y
						Russell, AL	loser	y	y

32	29	1987	1987	Nippon Kokan (NKK)	Mfg	Troup, GA	loser	y	y
						Linn, OR	winner	y	y
						Pierce, WA	loser	y	y
33	30	1987	1987	Dresser Rand (Ingers)	Mfg	Allegany, NY	winner	y	y
						Hartford, CT	loser	y	y
34	31	1987	1987	Worldmark	Mfg	Hancock, KY	winner	y	y
						Daviess, KY	loser	y	y
						Perry, IN	loser	y	y
35			1987	Combustion Engineering (CE)	Mfg	Allegany, NY	winner	n	y
						Lake, IN	loser	n	y
						Hamilton, TN	loser	n	y
						Dickinson, KS	loser	n	y
						Washington, PA	loser	n	y
						Lycoming, PA	loser	n	y
						Hartford, CT	loser	n	y
36	32	1988		Eastman Kodak	Mfg	Chester, PA	winner	y	n
						Philadelphia, PA	loser	y	n
						Delaware, PA	loser	y	n
						Montgomery, PA	loser	y	n
						Bucks, PA	loser	y	n
37	33	1988		Albertson's	Trade	Multnomah, OR	winner	y	n
						Washington, OR	loser	y	n
						King, WA	loser	y	n
38	34	1988	1988	Metal Container (A-B)	Mfg	Jefferson, WI	winner	y	y
						Rock, WI	loser	y	y
						Dekalb, IL	loser	y	y
39	35	1988	1988	Anheuser-Busch	Mfg	Bartow, GA	winner	y	y
						Hall, GA	loser	y	y
						Knox, TN	loser	y	y
						Dekalb, GA	loser	y	y
40	36	1988	1988	Kimberly-Clark	Mfg	Tulsa, OK	winner	y	y
						Rogers, OK	loser	y	y
41	37	1988	1988	Alumax	Mfg	Gwinnett, GA	winner	y	y
						San Mateo, CA	loser	y	y
42	38	1988	1988	Toyota	Mfg	Scott, KY	winner	y	y
						Alameda, CA	loser	y	y
43	39	1988	1988	Wella	Mfg	Henrico, VA	winner	y	y
						Bergen, NJ	loser	y	y
44	40	1988	1988	Reebok International	Mfg	Middlesex, MA	winner	y	y
						Suffolk, MA	loser	y	y
45	41	1989	1988	Squibb	Mfg	Camden, NJ	winner	y	n



						Mercer, NJ	loser	y	n
						Middlesex, NJ	winner	n	y
47			1988	US West	Trans and Utilities	Boulder, CO	winner	n	y
						Larimer, CO		n	y
						Maricopa, AZ	loser	n	y
						Pima, AZ	loser	n	y
						King, WA	loser	n	y
						Hennepin, MN	loser	n	y
48			1988	Sematech	Mfg	Travis, TX	winner	n	y
						Santa Clara, CA	loser	n	y
49	42	1989	1989	GTE	Trans and Utilities	Dallas, TX	winner	y	y
						Hillsborough, FL	loser	y	y
						Hamilton, IN	loser	y	y
						Ventura, CA	loser	y	y
50	43	1989	1989	Formosa Plastics	Mfg	Calhoun, TX	winner	y	y
						Galveston, TX	loser	y	n
						Nueces, TX	loser	y	y
						Jefferson, TX	loser	n	y
51	44	1989	1989	Philips Display	Mfg	Washtenaw, MI	winner	y	y
						Seneca, NY	loser	y	y
						Wood, OH	loser	y	y
						Lucas, OH	loser	y	y
52	45	1989	1989	Wal-Mart Stores	Trade	Larimer, CO	winner	y	y
						Laramie, WY	loser	y	y
						Weld, CO	loser	y	y
						Boulder, CO	loser	y	y
53	46	1989	1989	Ideal Security Hardw	Mfg	Washington, TN	winner	y	y
						Ramsey, MN	loser	y	y
54	47	1989	1989	Burlington Air Express	Trans and Utilities	Lucas, OH	winner	y	y
						Allen, IN	loser	y	y
55			1989	Chase Manhattan	Services	New York, NY	winner	n	y
						Hudson County, NJ	loser	n	y
56			1989	Phoenix Research Corp.	Mfg	Mohave County, AZ	winner	n	y
						San Diego, CA	loser	n	y
57			1989	Avon	Mfg	Gwinnett, GA	winner	n	y
						Dekalb, GA	loser	n	y
58			1989	USAA	FIRE	Norfolk, VA	winner	n	y
						Mecklenburg, NC	loser	n	y
59			1989	Bridgestone	Mfg	Shelby, TN	loser	n	y
						Summit, OH	winner	n	y
60	48	1990		Boeing	Mfg	Wichita, KS	winner	y	n

61	49	1990		Tennessee Eastman	Mfg	Washington, MS	loser	y	n
						Sullivan, TN	winner	y	n
						Richland, SC	loser	y	n
62	50	1990	1990	Bass	Services	Dekalb, GA	winner	y	y
						Orange, FL	loser	y	y
						Shelby, TN	loser	y	y
63	51	1990	1990	Allied Signal	Mfg	Kershaw, SC	winner	y	y
						Rensselaer, NY	loser	y	y
64	52	1990	1990	Borden	Mfg	Cape May, NJ	winner	y	y
						Cumberland, ME	loser	y	y
65	53	1990	1990	Reichhold Chemicals	Mfg	Durham, NC	winner	y	y
						Westchester, NY	loser	y	y
66	66	1992	1990	Scott paper	Mfg	Daviess, KY	winner	y	y
						Clark County, IN	loser	n	y
						Posey, IN	loser	y	y
67			1990	Exxon	Mfg	Dallas, TX	winner	n	y
						New York, NY	loser	n	y
68			1990	Heinz Pet Products	Mfg	Campbell, KY	winner	n	y
						Los Angeles, CA	loser	n	y
69			1990	Lockheed Corp	Mfg	Los Angeles, CA	loser	n	y
						Cobb, GA	winner	n	y
70	54	1991		Ford	Mfg	Montgomery, PA	winner	y	n
						Delaware, PA	loser	y	n
71	55	1991	1991	Burlington Northern	Trans and Utilities	Tarrant, TX	winner	y	y
						Johnson, KS	loser	y	y
						Ramsey, MN	loser	y	y
72	56	1991	1991	Holiday	Services	Dekalb, GA	winner	y	y
						Shelby, TN	loser	y	y
73	57	1991	1991	Adidas USA	Mfg	Spartanburg, SC	winner	y	y
						Somerset, NJ	loser	y	y
74	58	1991	1991	American Auto	Services	Seminole, FL	winner	y	y
						Fairfax, VA	loser	y	y
75	59	1991	1991	United Airlines	Trans and Utilities	Denver, CO	winner	y	y
						Champaign, IL	loser	y	y
						Oklahoma, OK	loser	y	y
						Marion, IN	loser	y	y
						Guilford, NC	loser	y	y
						Fairfax, VA	loser	y	y
						Berkeley, WV	loser	y	y
						Hamilton, OH	loser	y	y
						Jefferson, KY	loser	y	y
76	60	1991	1991	Sterilite	Mfg	Jefferson, AL	winner	y	y

77	61	1991	1991	Wal-mart stores	Trade	Lauderdale, TN	loser	y	y
						Hernando, FL	winner	y	y
						Polk, FL	loser	y	y
78	62	1991	1991	Volvo North America	Mfg	Chesapeake, VA	winner	y	y
						Bergen, NJ	loser	y	y
79	63	1991	1991	AMF/Reece	Mfg	Hanover, VA	winner	y	y
						Middlesex, MA	loser	y	y
80	64	1991	1991	Boeing	Mfg	Snohomish, WA	winner	y	y
						Kitsap, WA	loser	y	n
81	65	1991	1991	United Airlines	Trans and Utilities	Marion, IN	winner	y	y
						Denver, CO	loser	y	y
						Jefferson, KY	loser	n	y
						Oklahoma, OK	loser	n	y
82			1991	UPS	Tran and Util	Dekalb, GA	winner	n	y
						Fairfield, CT	loser	n	y
83			1991	J.C. Penney	Trade	Collin, TX	winner	n	y
						New York, NY	loser	n	y
84			1991	BASF Corp.	Mfg	Durham, NC	winner	n	y
						Morris, NJ	loser	n	y
85			1991	Computer Logics		Maricopa, AZ	winner	n	y
						Erie, NY	loser	n	y
86			1991	Fujitsu Business Communications Systems	Mfg	Maricopa, AZ	winner	n	y
						Orange, CA	loser	n	y
87			1991	Lane Bryant	Trade	Franklin, OH	winner	n	y
						New York, NY	loser	n	y
88			1991	Marriott Corp	Services	Montgomery, MD	winner	n	y
						Washington, DC	loser	n	y
89			1991	Michelin Aircraft Tire Co	Mfg	Mecklenburg, NC	winner	n	y
						Summit, OH	loser	n	y
90			1991	Salomon Brothers	FIRE	Hillsborough, FL	winner	n	y
						Franklin, OH	loser	n	y
						New York, NY	loser	n	y
91			1991	Hewlett-Packard	Mfg	Dekalb, GA	winner	n	y
						Cobb, GA	loser	n	y
92			1991	Key Communications	Tran and Util	Floyd, IN	winner	n	y
						Mecklenburg, NC	loser	n	y
93	67	1992		Safeway	Trade	San Joaquin, CA	winner	y	n
						Sacramento, CA	loser	y	n
94	68	1992	1992	ATandT	Trans and Utilities	Mecklenburg, NC	winner	y	y
						Berkeley, WV	loser	y	y
						Placer, CA	loser	y	y

95	69	1992	1992	GE Capital Services	Financials	Fulton, GA	winner	y	y
						Fairfield, CT	loser	y	y
96	70	1992	1992	BMW	Mfg	Greenville, SC	winner	y	y
						Douglas, NE	loser	y	y
						Anderson, SC	loser	n	n
97	71	1992	1992	National Steel	Mfg	St. Joseph, IN	winner	y	y
						Allegheny, PA	loser	y	y
98	72	1992	1992	MCI Communications	Trans and Utilities	Dade, FL	winner	y	y
						Duval, FL	loser	y	y
99	73	1992	1992	Everest and Jennings	Mfg	St. Louis, MO	winner	y	y
						Ventura, CA	loser	y	y
100	74	1992	1992	Swearingen Aircraft	Mfg	Berkeley, WV	winner	y	y
						New Castle, DE	loser	y	y
101	75	1992	1992	Evenflo	Mfg	Cherokee, GA	winner	y	y
						Cuyahoga, OH	loser	y	y
						Summit, OH	loser	n	y
102			1992	Dollar Rent A Car	Services	Tulsa, OK	winner	n	y
						Los Angeles, CA	loser	n	y
103			1992	CARE		Fulton, GA	winner	n	y
						New York, NY	loser	n	y
104	76	1993		Sterling Drug	Mfg	Montgomery, PA	winner	y	n
						Rennselaer, NY	loser	y	n
105	77	1993	1993	JLM Industries	Mfg	Hillsborough, FL	winner	y	y
						Fairfield, CT	loser	y	y
						Duval, FL	loser	n	n
						Mecklenburg, NC	loser	n	n
106	78	1993	1993	BandW Tobacco	Mfg	Bibb, GA	winner	y	y
						Jefferson, KY	loser	y	y
107	79	1993	1993	Greyhound Lines	Trans and Utilities	Dallas, TX	winner	y	y
						Polk, IA	loser	y	y
108	80	1993	1993	Transkrit	Mfg	Roanoke, VI	winner	y	y
						Westchester, NY	loser	y	y
109	81	1993	1993	Mercedes	Mfg	Tuscaloosa, AL	winner	y	y
						Berkeley, SC	loser	y	n
						Clarke, GA	loser	y	n
						Alamance, NC	loser	y	y
						Chester, SC	loser	y	n
						Durham, NC	loser	y	n
						Douglas, NE	loser	y	n
						Anderson, TN	loser	y	n
						Dorchester, SC	loser	n	n
						Charleston, SC	loser	n	n

						Orange, NC	loser	n	n
						Roane, TN	loser	n	n
110	82	1993	1993	Schlegel	Mfg	Rockingham, NC	winner	y	y
						Guilford, NC	loser	y	n
111			1993	Southwestern Bell Corp	Tran and Util	Bexar, TX	winner	n	y
						St. Louis, MO	loser	n	y
112			1993	Spiegel	Trade	Franklin, OH	winner	n	y
						Cook, IL	loser	n	y
113			1993	Peterbilt Motor Co (Paccar)	Mfg	Denton, TX	winner	n	y
						Alameda, CA	loser	n	y
114			1993	Dell	Mfg	Williamson, TX	winner	n	y
						Travis, TX	loser	n	y
115			1993	Transamerica Life	FIRE	Jackson, MO	winner	n	y
						Los Angeles, CA	loser	n	y

## **Appendix 2: Pre-Period and Post-Period Assignment**

GHM describes the pre-treatment period as the Census of Manufacturers (CM) 1-5 years prior to the MDP opening and the post-treatment period as the CM 4-8 years after the MDP opening. “Thus, each MDP opening is associated with one earlier date and one later date” (GHM 2010). However, Stata code in the article’s supplementary materials suggests one or more pre- and post-treatment periods for each case. Pre-treatment periods include any 1977-1992 CM that is at least one year prior to the MDP opening. Post-periods include any 1982-1997 CM that is zero or more years after the MDP opening. In order to determine sensitivity to pre- and post-period assignment methods, this paper presents results for two samples. CM Sample A includes all available pre- and post-period CMs for each case. CM Sample B contains one pre-period and one post-period for each case.

CM and CG Sample A are constructed using the pre- and post-period assignment method described in GHM supplementary files. Specifically, assignment is made as follows:

- If treatment (winning) occurs in 1982, use data from 1977 as pre-period and data from 1982/1987/1992/1997 as post-period.<sup>2</sup>
- If treatment (winning) occurs in 1983-1987, use data from 1977/1982 as pre-period and data from 1987/1992/1997 as post-period.
- If treatment (winning) occurs in 1988-1992, use data from 1977/1982/1987 as pre-period and data from 1992/1997 as post-period.
- If treatment (winning) occurs in 1993-1997, use data from 1977/1982/1987/1992 as pre-period and data from 1997 as post-period.

CM and CG Sample B restrict each case to one pre- and post-period each. Assignment follows the method described with the text of GHM. Specifically, the pre-treatment period is the CM 1-5 years prior to the MDP opening and the post-treatment period is the CM 4-8 years after

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<sup>2</sup> Cases from 1982 are dropped for most of the analyses due to 1977 data issues. In analyses not shown, 1982 cases are retained and estimates are not qualitatively different.

the MDP opening, as follows:

- Pre-period assignments
  - If treatment (winning) occurs in 1983-1988, use data from 1982 as pre-period.
  - If treatment (winning) occurs in 1988-1992, use data from 1987 as pre-period.
  - If treatment (winning) occurs in 1993-1997, use data from 1992 as pre-period.
- Post-period assignments:
  - If treatment (winning) occurs in 1983, use data from 1987 as post-period.
  - If treatment (winning) occurs in 1984-1988, use data from 1992 as post-period.
  - If treatment (winning) occurs in 1989-1993, use data from 1997 as post-period.

Comparing results from Sample A and B, using all available pre- and post-period data consistently produces more precise and larger estimated effects than restricting the sample to one pre- and post-period per winner or loser. It is difficult to precisely interpret the difference. It could be that effects gain momentum over time because some counties have multiple post-periods in Sample A. However, some cases have only one post-period and many pre-periods. The paper reports findings for Sample A. Sample B estimates are available from the author upon request. Although Sample B coefficients are smaller in magnitude, they have the same sign as Sample A estimates.

### **Appendix 3: Revisiting BMW**

On June 29, 1992, BMW announced its first US manufacturing plant would locate in Greenville County, SC. The announcement was the culmination of South Carolina's involvement in a 2+ year site selection process, which ended in a very public bidding war between Greenville, SC and Omaha, NE. Omaha is located in Douglas County, NE, and for this case, Douglas County is the only "loser" identified in GHM's MDP sample. GHM argue the bidding war shows that their sample correctly identified the "loser". However, if concerns about the strategic motives behind public bidding wars are taken seriously, then a closer look is warranted. A

LexisNexis search for documents related to the BMW search reveals these concerns may be valid.

In late March 1992, *Automotive News* obtained a US federal government memo on the project. The memo quotes BMW Chairman Eberhard Von Keuhiem as saying the US site selection process was 80% complete, with the choices narrowed to 4 sites. The Chairman notes proximity to an international airport, port, rail, union presence, and the number of time zones between Bonn and the site as the critical factors in site selection. The document's author, US Consul General Andrew G. Thomas, Jr., reports the Chairman only mentions the state of South Carolina, with the Anderson, SC site listed as the clear front-runner (Kurylko 1992a). An April 6, 1992 *Automotive News* report says that the Greenville site has replaced Anderson as the front-runner. This is the first time Nebraska is mentioned as a potential candidate along with sites in North Carolina, Georgia, and Massachusetts (*Automotive News*, April 6, 1992). Nebraska's inclusion appears curious given over 15% of Nebraska labor was unionized in 1992 (compared to less than 3% of South Carolina labor) and the Chairman's reiteration of union issues in Germany being a significant reason "it may be a practical problem" to continue to supply cars from Germany (likewise, access to a port and an international airport also being problematic). Nebraska is noticeably absent from an April 13 *Automotive News* report on state governors flown to Bonn to meet with the company. Nebraska is also absent from the states asked to meet with the company Chairman during his visit to Washington (Henry 1992).

Nebraska's governor doesn't get invited to Germany until a month after the leading states. On May 18, *Automotive News* reports he went to offer an undisclosed incentives package. According to the report, South Carolina was offering the company \$35 million in incentives and the decision was between a few locations in South Carolina and the Omaha site. The report goes on to state, "A Nebraska site would not meet BMW's stated criteria that a U.S. plant be within six time zones of Germany, or of proximity to a major port. However, the state government and the Union Pacific presumably would attempt to offset these disadvantages by offering major incentives . . . (Kurylko 1992b)."



On June 18, the site selection process was in the hands of BMW's legal team and according to a company official, "While BMW is leaning toward Spartanburg, S.C., lucrative offers keep rolling in from Omaha, Neb., the source said. *The Omaha World-Herald* reported on June 7 that Nebraska has offered as much as \$240 million in tax, land and other incentives to lure the German carmaker. The South Carolina package was estimated to be worth \$150 million (Kurylko 1992c)."

Thus, there is considerable reason to believe that the automaker was looking for a site on the eastern seaboard with a preference for the South which focused on South Carolina. Nebraska's lucrative incentives package served a useful purpose for the company – raising South Carolina's initial bid from \$35 million to \$150 million. Given the circumstances and selection criteria described above, it is difficult to reason that Douglas County, NE serves as an appropriate counterfactual to productivity in Greenville, SC without the BMW plant. If it did, then why haven't any other auto facilities located there since this decision?

Examining the other agglomeration factors, Douglas and Greenville appear to be substantially different with respect to economic size, manufacturing share of employment, and the pre-trends in manufacturing wages per worker (see Appendix 3 Figures A1-A3). The mostly likely correct counterfactual, Anderson, SC, displays similar manufacturing share and wage pre-trends. Since the agglomeration literature suggests these factors are important determinants of productivity, these differences cast some doubt on the validity of the GHM identification assumption, or least the one case that GHM used to justify their approach.

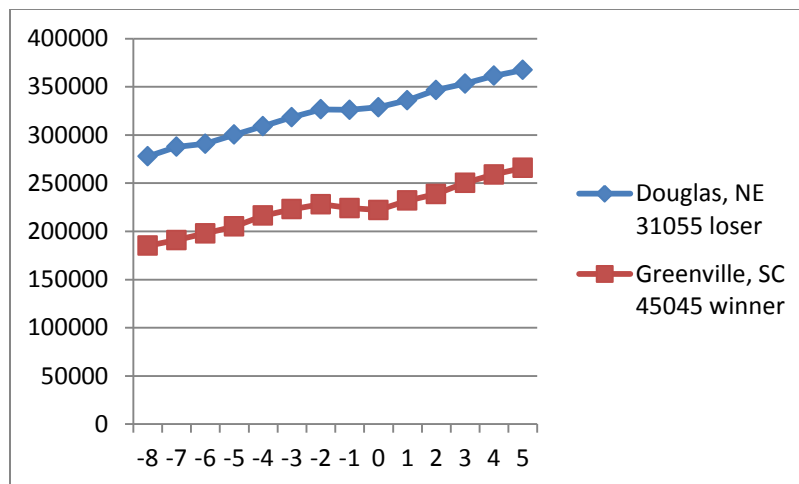


Figure A1: Total Employment

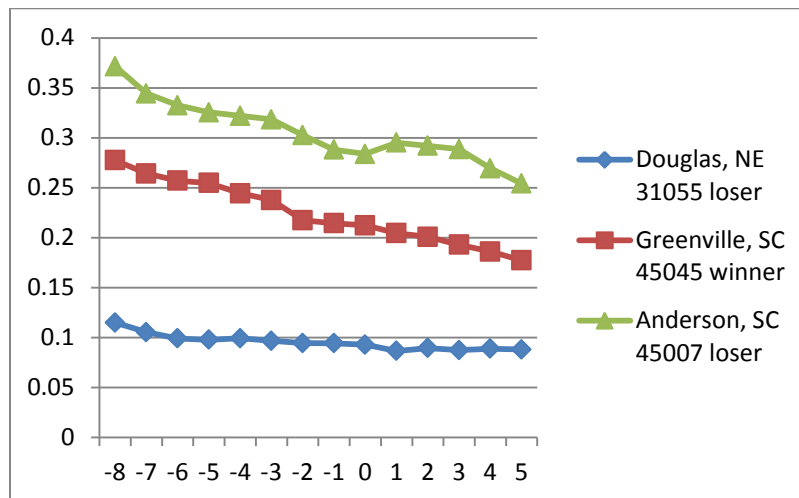


Figure A2: Manufacturing Share

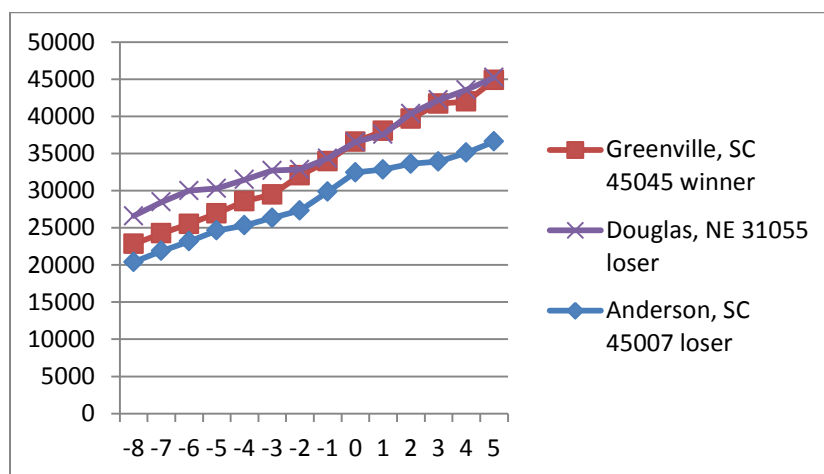


Figure A3: Manufacturing Wage per Worker

## Appendix 4: Winning County Population Changes

Table A2: Mean Shifts in Winning Counties' Population

	(1)	(2)	(3)	(4)	(5)
ln(Population)	0.0882***	0.0452**	0.0410***	0.0411***	0.0331*
	(0.0167)	(0.0202)	(0.0154)	(0.0152)	(0.0198)
	750	570	1670	0.9942	0.9936
	0.9952	0.9937	0.9942	3985	540

Standard errors in parentheses; \* p<0.10, \*\* p<0.05, \*\*\* p<0.01;  
Notes: The table presents the mean shift in log population (measured in 1,000s) from five separate Model 1 regressions. Columns report estimated changes from five counterfactual matching method as follows: (1) Revealed rankings; (2) Nearest propensity score neighbor; (3) Nearest 5 propensity score neighbors; (4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors. Data is from the Census of Government and thus corresponds to the population changes coincident with the local government finance changes reported in the body of the paper. Pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

## Appendix 5: Sensitivity Analysis Using Observable Matches within 100-250 mile radius

Table A3: Mean Shifts in Winning Counties' Establishments

	(1)	(2)	(3)	(4)
Difference-in-Differences	-0.0073	0.0164	0.0464**	-0.0302
	(0.0328)	(0.0238)	(0.0197)	(0.0311)
R <sup>2</sup>	0.9901	0.9905	0.9879	0.9904
N	459	1343	40790	468

Notes: The table presents results the Model 1 estimated change in winning counties' (log) county establishments. Column headings refer to identification strategies as follows: (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors. Robust standard errors are in parentheses. Census of Manufactures pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

Table A4: Change in Winning Counties' Output (Value of Shipments and Value Added)

	(1)	(2)	(3)	(4)
Value of Shipments				
Difference-in-Differences	0.0724	0.1440***	0.0908**	0.0774
	(0.0612)	(0.0503)	(0.0414)	(0.063)
R <sup>2</sup>	0.9783	0.9742	0.9789	0.9738
N	431	1234	37078	443
Value Added				
Difference-in- Differences	0.0721	0.1261***	0.0791**	0.0547
	(0.06)	(0.0465)	(0.0375)	(0.0578)
R <sup>2</sup>	0.9783	0.9751	0.9784	0.9772
N	431	1234	37058	443

Notes: The table presents Model 1 estimated changes in winning counties' log value of shipments (measured in thousands of dollars and deflated) and log value-added (measured in thousands of dollars and deflated). Column headings refer to identification strategies as follows: (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors. Robust standard errors are in parentheses. Census of Manufactures pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

Table A5: Change in Winning Counties' Earnings per Worker

	(1)	(2)	(3)	(4)
Model 1				
Mean Shift	0.0153*** (0.0051)	0.0205*** (0.0039)	0.0348*** (0.0036)	0.0265*** (0.0047)
R <sup>2</sup>	0.9747	0.9768	0.9733	0.9782
Model 2				
Effect after 5 years	0.0305* (0.0180)	0.0146 (0.0143)	0.0155 (0.0129)	0.0293* (0.0173)
Level Change	0.0072 (0.0075)	0.0059 (0.0059)	0.0073 (0.0054)	0.0062 (0.0071)
Trend Break	0.0039 (0.0028)	0.0014 (0.0023)	0.0014 (0.0021)	0.0039 (0.0027)
R <sup>2</sup>	0.9747	0.9769	0.9735	0.9784
N	1586	4654	147264	1612

Notes: The table presents results from ten separate regressions. Column headings refer to identification strategies as follows: (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors. Robust standard errors are in parentheses. The top panel reports the mean shift in winning counties' logged wage employment from estimating Model 1. The bottom panel reports the results from estimating Model 2 with logged wage employment as the dependent variable. The estimated change after five years is calculated by  $\theta_1 + 6\theta_2$  to allow an effect in  $\tau = 0$ .

Table A6: Change in Winning Counties' Employment

	(1)	(2)	(3)	(4)
Model 1				
Mean Shift	0.0463*** (0.0102)	0.0488*** (0.0075)	0.0706*** (0.0068)	0.0423*** (-0.0084)
R <sup>2</sup>	0.9957	0.9965	0.9963	0.9969
Model 2				
Effect after 5 years	0.0538 (0.0391)	0.0431 (0.0291)	0.0257 (0.0263)	0.0496 (0.0324)
Level Change	0.0167 (0.0152)	0.0199* (0.0113)	0.0134 (0.0105)	0.0159 (0.0127)
Trend Break	0.0062 (0.0059)	0.0039 (0.0044)	0.002 (0.004)	0.0056 (0.0049)
R <sup>2</sup>	0.9957	0.9965	0.9964	0.9969
N	1586	4654	147264	1612

Notes: The table presents results from ten separate regressions. Column headings refer to identification strategies as follows: (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors. Robust standard errors are in parentheses. The top panel reports the mean shift in winning counties' logged wage employment from estimating Model 1. The bottom panel reports the results from estimating Model 2 with logged wage employment as the dependent variable. The estimated change after five years is calculated by  $\theta_1 + 6\theta_2$  to allow an effect in  $\tau = 0$ .

Table A7: Change in Winning Counties' Revenue

	(1)	(2)	(3)	(4)
ln(General Own Revenue)	0.0406 (0.0422)	0.0836** (0.0328)	0.1132*** (0.0286)	0.0544 (0.0407)
R <sup>2</sup>	0.9830	0.9840	0.9841	0.9824
Revenue Per Capita	0.0482 (0.0462)	0.0300 (0.0360)	0.1801*** (0.0300)	0.0549 (0.0474)
R <sup>2</sup>	0.8537	0.8438	0.8384	0.8496
Revenue Per Personal Income	-0.0005 (0.0025)	0.0004 (0.0022)	0.0000 (0.0020)	-0.0007 (0.0026)
R <sup>2</sup>	0.7558	0.7499	0.7340	0.7346
ln(Property Tax Revenue)	0.0434 (0.0363)	0.0800*** (0.0287)	0.1099*** (0.0247)	0.0321 (0.0365)
R <sup>2</sup>	0.9876	0.9878	0.9878	0.9875
N	580	1690	52615	590

Notes: The table presents results from twenty separate regressions Model 1 regressions. Column headings refer to identification strategies as follows: (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated. Data is from the Census of Governments. Expenditure is measured in \$1,000s. County variables are the aggregates of all local governments within the county. Pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

Table A8: Change in Winning Counties' Outstanding Debt

	(1)	(2)	(3)	(4)
ln(Outstanding Debt)	0.2637*** (0.0787)	0.2801*** (0.0624)	0.3194*** (0.0536)	0.3519*** (0.0793)
R <sup>2</sup>	0.9452	0.9449	0.9420	0.9407
Outstanding Debt Per Capita	0.9782** (0.4222)	0.7983* (0.4115)	1.0800*** (0.3766)	0.8971** (0.4447)
R <sup>2</sup>	0.5848	0.5758	0.6161	0.5870
N	580	1690	52615	590

Notes: The table presents results from ten separate Model 1 regressions. Column headings refer to identification strategies as follows: (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated. Data is from the Census of Governments. Expenditure is measured in \$1,000s. County variables are the aggregates of all local governments within the county. Pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

Table A9: Change in Winning Counties' Education Expenditure

	(1)	(2)	(3)	(4)
ln(Education Expenditure)	0.0273 (0.0281)	0.0882*** (0.0216)	0.0987*** (0.0183)	0.0376 (0.0279)
R <sup>2</sup>	0.9904	0.9907	0.9904	0.9902

Education Expenditure Per Capita	0.0061 (0.0172)	0.0156 (0.0122)	0.0088 (0.0100)	-0.0076 (0.0165)
R <sup>2</sup>	0.9342	0.9365	0.9292	0.9436
N	580	1690	52615	590

Notes: The table presents the mean shifts from ten separate Model 1 regressions. Column headings refer to identification strategies as follows: (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated. Data is from the Census of Governments. Expenditure is measured in \$1,000s. County variables are the aggregates of all local governments within the county. Pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

Table A10: Changes in Winning Counties' Parks and Recreation Services Expenditure

	(1)	(2)	(3)	(4)
ln(Parks and Recreation Expenditure)	0.0513 (0.0965)	0.0963 (0.0681)	0.1602*** (0.0578)	0.2349*** (0.0865)
R <sup>2</sup>	0.9462	0.9543	0.9468	0.9513
	579	1682	51153	588
Parks and Recreation Expenditure Per Capita	0.0041 (0.0030)	0.0015 (0.0024)	0.0117*** (0.0021)	0.0041 (0.0028)
R <sup>2</sup>	580	1690	52615	590
N	0.6620	0.6968	0.6576	0.7032

Notes: The table presents the mean shifts from ten separate Model 1 regressions. Column headings refer to identification strategies as follows: (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated. Data is from the Census of Governments. Expenditure is measured in \$1,000s. County variables are the aggregates of all local governments within the county. Pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

Table A11: Change in Winning Counties' Police Service Expenditure

	(1)	(2)	(3)	(4)
ln(Police Expenditure)	0.0751* (0.0387)	0.1028*** (0.0294)	0.1378*** (0.0253)	0.0972** (0.0389)
R <sup>2</sup>	0.9874	0.9879	0.9856	0.9857
Police Expenditure Per Capita	0.0033 (0.0032)	0.0035 (0.0024)	0.0157*** (0.0020)	0.0029 (0.0033)
R <sup>2</sup>	0.8778	0.8784	0.8599	0.8673
N	580	1690	52615	590

Notes: The table presents the mean shifts from ten separate Model 1 regressions. Column headings refer to identification strategies as follows: (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated. Data is from the Census of Governments. Expenditure is measured in \$1,000s. County variables are the aggregates of all local governments within the county. Pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

Table A12: Change in Winning Counties' Fire Service Expenditures

	(1)	(2)	(3)	(4)
ln(Fire Expenditure)	0.0078 (0.0678)	0.0853 (0.0519)	0.0973** (0.0433)	0.1678** (0.0684)
R <sup>2</sup>	0.9717	0.9706	0.9651	0.9695
Fire Expenditure Per Capita	0.0011 (0.0021)	-0.0012 (0.0026)	0.0104*** (0.0013)	0.0006 (0.0023)
R <sup>2</sup>	0.8601	0.6012	0.7871	0.8440
N	580	1690	52615	590

Notes: The table presents the mean shifts from ten separate Model 1 regressions. Column headings refer to identification strategies as follows: (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated. Data is from the Census of Governments. Expenditure is measured in \$1,000s. County variables are the aggregates of all local governments within the county. Pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2.

### Appendix 6: Sensitivity Analysis Controlling for Covariates

The regressions in this section follow the specifications used in the paper with the addition of control variables for county industrial composition (as measured by the share of employment in major SIC industries) and total county population.

Table A13: Economic Activity Covariate Sensitivity Tests

	(1)	(2)	(3)	(4)	(5)
Establishments	0.0667*** (0.0235)	0.0024 (0.0291)	0.0045 (0.0212)	0.021 (0.0193)	-0.0642** (0.0298)
N	584	446	1311	3129	417
Value of Shipments	0.1431*** (0.0503)	0.1263*** (0.0454)	0.1222*** (0.0420)	0.1048* (0.0570)	0.0071 (0.0555)
N	558	1231	2918	420	392
Value Added	0.1284** (0.0497)	0.088 (0.0567)	0.1071** (0.0440)	0.1096*** (0.0388)	-0.0104 (0.0581)
N	558	1231	2918	420	392
Earnings	-0.0125*** (0.0038)	0.0217*** (0.0040)	0.0209*** (0.0032)	0.0219*** (0.0031)	0.0223*** (0.0043)
N	1978	1530	4537	11006	1438
Employment	0.0565*** (0.0061)	0.0384*** (0.0074)	0.0410*** (0.0067)	0.0491*** (0.0065)	-0.0003 (0.0080)
N	1978	1530	4537	11006	1438

Notes: The table presents the mean shifts from twenty-five separate covariate control-augmented Model 1 regressions. Columns report estimated changes from counterfactual methods as follows: 1) Revealed rankings; (2) Nearest propensity score neighbor; (3) Nearest 5 propensity score neighbors; (4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated.

Table A14: Fiscal Surplus Covariate Sensitivity Tests

	(1)	(2)	(3)	(4)	(5)
General Own Revenue	0.0987*** (0.0345)	0.0879** (0.0395)	0.0736** (0.0299)	0.0823*** (0.0286)	-0.0151 (0.0400)
G.O. Revenue Per Capita	-0.0374 (0.0399)	0.0895** (0.0428)	0.0630** (0.0317)	0.0886*** (0.0294)	0.0696* (0.0413)
Property Tax Revenue	0.1116*** (0.0352)	0.0535 (0.0394)	0.0551* (0.0293)	0.0494* (0.0286)	-0.0340 (0.0388)
Outstanding Debt	0.1584** (0.0733)	0.2277*** (0.0809)	0.2171*** (0.0587)	0.2476*** (0.0550)	0.0789 (0.0886)
Debt Per Capita	-0.2459 (0.2290)	0.5588** (0.2311)	0.5346*** (0.1697)	0.6394*** (0.1799)	0.3470 (0.2325)
Education Expenditure	0.0856*** (0.0234)	0.0807*** (0.0285)	0.0470* (0.0256)	0.0579** (0.0228)	-0.0055 (0.0277)
Education Spending Per Capita	-0.0196 (0.0183)	0.0338** (0.0165)	0.0106 (0.0129)	0.0121 (0.0123)	0.0022 (0.0177)
Parks and Rec. Expenditure	0.1107 (0.0755)	0.0579 (0.0819)	0.1237* (0.0643)	0.1429** (0.0617)	0.0352 (0.0822)
Parks & Rec. Spending Per Capita	-0.0014 (0.0030)	0.0035 (0.0026)	0.0049** (0.0022)	0.0053** (0.0021)	0.0014 (0.0026)
Police Expenditure	0.0898*** (0.0297)	0.0839** (0.0354)	0.0756** (0.0294)	0.0877*** (0.0283)	0.0743* (0.0408)
Police Spending Per Capita	-0.0087*** (0.0031)	0.0041 (0.0027)	0.0032 (0.0022)	0.0049** (0.0021)	0.0044 (0.0031)
Fire Expenditure	0.0913* (0.0503)	0.0862 (0.0594)	0.0984** (0.0447)	0.1175*** (0.0423)	0.0866 (0.0672)
Fire Spending Per Capita	-0.0065*** (0.0021)	-0.0004 (0.0021)	0.0030** (0.0014)	0.0031** (0.0015)	0.0001 (0.0022)
N	735	553	1639	3901	528

Notes: The table presents the mean shifts from twenty-five separate covariate control-augmented Model 1 regressions. Columns report estimated changes from counterfactual methods as follows: 1) Revealed rankings; (2) Nearest propensity score neighbor;



(3) Nearest 5 propensity score neighbors; (4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors. Row headings correspond to the dependent variable for which the mean shift is estimated.