Identifying the Economic Development Effects of Million Dollar Facilities

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Using tax abatements, financial incentives, and public investments to attract (or retain) firms is the primary economic development tool for many local governments. Recent estimates suggest Million Dollar Plants (MDPs) generate large productivity spillovers which may justify the substantial incentive packages used to lure them. This paper employs the Greenstone, Hornbeck, and Moretti (2010) identification strategy, as well as an alternative identification strategy, to estimate aggregate county effects from "winning" the competition for a MDP. Using both identification strategies, I test for the presence of agglomeration spillovers as well as for MDP effects on public revenues and expenditures. The identification strategies tell different stories about MDP effects. Evidence in favor of productivity spillovers is modest when identified by the alternative strategy. Neither identification strategy provides much evidence that MDPs induce the virtuous cycle of economic development.

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1. Introduction

Using tax abatements, financial incentives, and public investments to attract (or retain) firms 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 firm is seen by some as the holy grail of economic development. Consequently jurisdictions are willing to offer substantial financial incentives to attract large firms. Critics of economic development incentives assert they have negative efficiency, equity, and financial consequences. Advocates argue incentivized firms generate significant agglomeration externalities and incentives simply compensate firms for their productivity spillovers.

Using a set of incentivized firms, this paper investigates whether successful attraction of a large new firm induces a virtuous cycle of economic development or something closer to the winner's curse. In order for new firm to induce a virtuous cycle of economic development, it must generate agglomeration externalities sufficient to induce new economic activity as well as fiscal surplus. Fiscal surplus should manifest itself through lower tax rates or improved public services. Lower taxes and better public services also attract new economic activity, which brings the cycle full circle. On the other hand, the winner's curse scenario is characterized by fiscal deterioration.

This paper tests for the presence of agglomeration spillovers using the indirect measures suggested by theory. The literature suggests the impact of agglomeration economies on productivity may be measured indirectly through wages, new firm entry, and employment (Rosenthal and Strange 2003; Helsley and Strange 2001; Glaeser and Gotlieb 2008). Changes in wages, number of establishments, and employment growth are also measures of new economic activity that align with economic development policy goals. However, it is important to keep in mind that the indirect measures may also reflect general equilibrium effects on rents, public services, and taxes. Further, the existence of positive spillovers does not necessarily mean that successfully attracting one of these large firms induces a virtuous cycle of economic development in winning counties. Evidence of fiscal surplus is examined by estimating the

change public revenues and expenditures after successful attraction of a large new firm.

In a recent paper, Greenstone, Hornbeck, and Moretti (2010) (GHM) endeavor to quantify agglomeration spillovers generated from attracting a large new plant by examining its impact on incumbent plant total factor productivity (TFP). Their estimates suggest large, new firms generate productivity spillovers which may justify the substantial incentive packages used to lure them. GHM (2010) make an important theoretical contribution with respect to the interaction of incentives and agglomeration spillovers and provide a sensible empirical specification based upon that theory. However, the focus is on identifying the spillover and not on the economic development outcome in winning counties. This paper extends their analysis to address policy implications by estimating aggregate county effects from "winning" the competition for the GHM set of large firms. In addition, the paper develops an alternative identification strategy to investigate sensitivity to GHM identifying assumptions.

GHM employ a quasi-experimental research design that relies on a difference-in-differences (DID) estimator and firms' revealed rankings over potential locations as reported in the *Site Selection* magazine regular feature "Million Dollar Plant" (MDP). The authors make a compelling prima facie case for the quasi-experimental research design. In the spirit of Angrist and Pischke's (2010) call to focus on the institutional and empirical case for quasi-experimental research designs, this paper critically examines the GHM identifying assumption. In order to check robustness to GHM identifying assumptions, this paper employs their identification strategy as well as an alternative strategy. Specifically, the paper identifies winner county counterfactuals by matching on observables known to drive productivity as well as geography. Using both identification strategies, I test for the presence of agglomeration spillovers using the indirect measures suggested by theory as well as for evidence of fiscal surplus.

The two identification strategies tell somewhat different stories about MDP effects. Using the GHM identification strategy, MDPs generate significant spillovers that are reflected in output and new firm entry but not in wages and employment growth. Evidence in favor of productivity spillovers is more modest when identified by observable matches. Neither identification strategy

provides much evidence that MDPs induce the virtuous cycle of economic development.

Whether the reader is more convinced by revealed ranking identification or observable geographic matching strategies, the paper's results indicate successful attraction of an MDP isn't economic development's "magic bullet". The results suggest that if significant productivity spillovers exist, the general equilibrium effects of directing public resources towards MDPs may dominate them. The results also call into question the magnitude of the spillovers estimated in GHM. It seems unlikely that the unobservables captured by the GHM revealed rankings strategy eclipse observable productivity determinants and geography. Thus, the paper's findings also contribute to the ongoing debate surrounding quasi-experimental research design.

The paper proceeds in the next section with some brief background information. Section 3 outlines the data sources and econometric model. Section 4 presents the results for entry, output, wages, and employment as well as government revenues and expenditures using the revealed rankings strategy. Section 5 addresses identification in detail. Results identified by observable matches are presented in Section 6. Section 7 summarizes and concludes.

2. Background

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 2011). In this view, the induced firm 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 firms, increased employment, increased revenues, better public services, and/or lower tax rates (Eisinger 1988; Patrick 2011).

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).[1]

There are also those who argue competition causes communities to overbid for the firm 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[2], 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 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 firm and existing firms may be negatively impacted by revenue shortfalls. Firms 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 2011). 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.[3] GHM contributes theoretical and empirical frameworks for quantifying productivity spillovers, which

[1] Wilson (1999) gives a very thorough survey of the tax competition literature.
[2] Goodman (2003) and Dalehite, Mikesell, and Zorn (2008) are both case studies of locations with relatively sophisticated economic development organizations.
[3] 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.

is one possible MDP externality.

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 firm. The new firm generates significant spillovers, which enhance the productivity of all firms in the area. The productivity gains start a virtuous cycle, whereby more new firms locate to gain access to the productivity spillovers. As more firms 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.

Their model makes four empirical predictions: 1) incumbent plant productivity will increase as a result of the new firm; 2) firms that are economically close to the new plant will experience the largest increases in productivity; 3) economic density will increase as new firms enter to gain access to the spillovers; and 4) locally supplied input prices will increase. These predictions are in line with predictions from other agglomeration models. Thus, the productivity spillovers generated by the large, new firm should be reflected in output, new firm entry, wages, and employment growth. As Glaeser and Gotlieb (2008) point out, though, higher wages also attract new residents to the community. In fact, studies show most 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. It is impossible to disentangle productivity effects from the in-migration effects on rents and wages. Wages may also reflect underlying changes in public services and/or taxes. Thus, increases in wages and rents can't be attributed to agglomeration spillovers without additional information.

In order for successful attraction of a large, new firm to induce a virtuous cycle of economic development, it must generate agglomeration externalities sufficient to induce 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 indirect 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.[4] 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 services and the tax burdens induced by the incentives and MDP.

3. Empirical Implementation

3.1 Data

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).[5] *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.[6] 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

[4] Greenstone and Moretti (2003) use the MDP sample to estimate public finance effects using revenue and expenditure levels. Fisher (2007) similarly critiques their results for providing little insight into fiscal surplus.
[5] The precise source of the sample is more nuanced. See Appendix 1 for more details.
[6] It is also relevant to note the magazine's primary audience is economic development professionals.

(the "losers"). For our purposes, a firm's site selection decision is referred to as a case.

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 sample cases. Since my analysis does not employ firm level data, the entire (manufacturing) case sample can be used. Appendix 1 outlines the sample of MDPs used in this paper. 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 growth, 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) as well as the 1980, 1990, and 2000 Censuses of Population (CP). Bureau of Economic Analysis (BEA) Local Area Personal Income and Employment data from 1975-1998 are also employed.

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 data; however, there are some notable differences. MDP-owned facilities and MDP output cannot be removed from the aggregate county outcomes. Thus, the estimated changes are the direct effect of the MDP and the spillover effect.

Output is deflated using the Stata code provided in GHM supplementary materials.[7] The GHM TFP and aggregate output estimates use value of shipments minus inventories as the dependent variable. The productivity literature suggests a value-added specification may be preferable (Henderson 2003; Rosenthal and Strange 2004).[8] This paper reports results for

[7] I also estimated versions without deflating monetary values. Estimates were not sensitive to deflation.

[8] Value-added is not equal to value of shipments minus inventories. Value-added also subtracts the cost of materials, supplies, containers, fuel, purchased electricity, and contract work from the value of shipments. According to Hellerstein *et al*. (1999), a value-added specification is preferred because it can be derived for any elasticity of substitution. Further, it does not require the inclusion of the potentially endogenous value of materials variable on the right-hand side of production function specifications. The GHM aggregate county output

changes in winning county manufacturing output measured by the deflated value of shipments and value-added.

Estimates for the MDP effect on quality-adjusted wages employ individual-level wage data for winning and losing counties from the 1980, 1990, and 2000 Censuses of Population.[9] The specification includes dummy variables for the interaction of worker age and year, age-squared and year, education and year, sex and race and Hispanic and U.S. citizenship, as well as case fixed effects.[10] I depart from the GHM specification by using geographically *consistent* public use microdata areas rather than actual PUMAs. Counties in the PUMAs change from year to year, meaning that a treatment (control) county might be in one PUMA one Census year and in another the next Census year.

Imprecision introduced by PUMAs is one reason Black *et al*. (2005) suggest estimating wage effects with annual BEA wage data instead of Census of Population data. 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 growth use BEA annual data as well. Growth is defined as the change in log wage employment.[11]

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

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specification used in this paper does not include inputs on the right-hand side. I estimated specifications which include inputs and the results are reported in Appendix 5, Table A6. Substantial MDP effects practically disappear in all specifications. Only an increase in output given the same level of inputs suggests substantial productivity increases for winning county manufacturing firms.

[9] GHM (2010) state they use data from the 1970, 1980, 1990, and 2000 Censuses of Population; however, the Stata code only includes references from the 1980, 1990, and 2000 Censuses. For the purposes of this paper, these are the only Census years needed. Each case is assigned one pre-period and one post-period Census year. The pre-period is the most recent Census prior to the MDP opening and the post-period is the most recent Census at least 3 years after the MDP opening. Since the first case appears in the magazine in 1982, the earliest pre-period assignment is 1980.

[10] I employ GHM (2010) supplementary material Stata code to limit the sample to individuals who are at work, worked at least 26 weeks last year, usually work more than 20 hours per week, are not in school, and work for wages in the private sector. I also create the interaction dummies using their code.

[11] 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.[12] 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

If MDP's generate significant agglomeration spillovers, theory predicts that the spillovers will result in new firm entry and increased local input costs. In equilibrium, increased input costs and competition may have positive or negative effects on aggregate output and employment. The justification for significant public investment to attract MDPs assumes indirect and aggregate effects are positive in equilibrium and, therefore, result in positive public finance effects. GHM derive a model of plant TFP in which agglomeration spillovers are explicitly considered and provide a sensible empirical specification based upon that theory. As shown in GHM, variants of the following two empirical models may form the basis for testing these predictions 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

$$\ln(Y_{kjt}) = \delta 1(Winner)_{kj} + \psi Trend_{jt} + \Omega[Trend_{jt} \times 1(Winner)_{kj}] + \kappa 1(\tau \geq 0)_{jt} +$$

[12] Own revenue is revenue derived from own sourced and excludes State and Federal intergovernmental transfers. Own expenditure is direct expenditure by the local governmental units and excludes intergovernmental expenditures.

$\gamma\left[Trend_{jt} \times 1(\tau \geq 0)_{jt}\right] + \theta_1\left[1(Winner)_{pj} \times 1(\tau \geq 0)_{jt}\right] + \theta_2\left[Trend_{jt} \times 1(Winner)_{kj} \times 1(\tau \geq 0)_{jt}\right]_1 + c_k + \mu_t + \lambda_j + \varepsilon_{kjt}.$

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 τ is year normalized such that $\tau = 0$ in the plant announcement year for each case.

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, μ_t, and λ_j, respectively.[13]

The parameters of interest are θ_1 and θ_2. Under Model 1, θ_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, θ_1, and a differential trend in outcome, measured by θ_2, in the winning county after an MDP opening.

4. Revealed Rankings Strategy Results

All reported estimates are based upon the corrected GMc sample. Estimates using the cases exactly as presented in GM are available from the author upon request. Standard errors are robust to serial correlation and clustered at the county level.[14]

4.1 Indirect Measures

Table 1 presents estimates comparable to the aggregate county estimates in GHM. Panel A contains estimates for the difference-in-differences of manufacturing establishments and value of shipments using the Census of Manufacturers (CM) data. Panel B presents the change in quality-adjusted wages estimated from Census of Population (CP). Given that the CM occurs

[13] It should be noted that case and county fixed effects can't be separately identified when a county is unique to a case. However, estimates are sensitive to excluding county fixed effects. The GHM corollary specifications also encountered this issue. The authors did not respond to questions regarding separate identification. This issue is less prevalent for specifications with multiple "losers", such as the nearest 5 propensity score neighbor estimator.

[14] Although GHM do not calculate standard errors robust to serial correlation, Bertrand *et al*. (2004) suggest DID inference is incorrect without it. Thus, this paper follows their recommendation.

every five years and the CP occurs every 10 years, only Model 1 is estimated.

[Insert Table 1 approximately here]

Winning county manufacturing establishments significantly increased 9.61% (Table 1, Column 1), which is lower than the 12.5% change in CM plants reported by GHM. The disparity may be caused by i) aggregate versus plant level data, ii) their ability to exclude all MDP owned plants, or iii) differences in sample cases.

Using GMc losers as the counterfactual, increases in winner manufacturing output are economically and statistically significant. Table 1, Column (2) presents the change in counties' value of shipments after an MDP opening. When GMc losers identify the MDP effect, the corresponding increase is an estimated 18.53% compared to the imprecisely measured 14.54% reported by GHM. The larger effect found here is likely due to the inability to exclude MDP output from the sample. According to GHM, MDP's account for at least 9% of winning county manufacturing output.[15] The output effects identified by GMc losers suggest productivity spillovers similar to those found in GHM. The remaining differences are attributable to differences in the sample cases, weighting variable, or the output measure. Output is measured as the log value of shipments rather than the value of shipments minus inventories due to data availability.[16]

GHM find quality-adjusted wages increased by 2.7% in winning counties after an MDP opening. Table 1, Column (3) reveals that quality-adjusted wages increased by an imprecisely measured 1.7%. Although the GHM supplementary material code was used to create the sample and variables, different results aren't surprising due to the use of geographically-consistent PUMAs discussed in Section 3.1. Again, there are also slight differences in the final MDP

[15] It should be noted that some MDPs account for more than 50% of winning county output. The summary MDP statistics in GHM are calculated using 28 of the 48 cases. GHM presents summary statistics which drop "large outlier plants so that the mean would be more representative of the entire sample." Thus, it is likely the mean output in this paper's sample of cases is larger than 9%.

[16] CM 1977 data constraints also prevented weighting observations by the lagged value of shipments or value added. Results weighted by the lagged number of establishments help account for differences in the economic importance of counties and are presented.

sample cases.

The GHM establishment and output specifications above weight each observation by the number of plants in the county 6-10 years prior to the MDP opening. The weighting scheme implies that counties with many existing establishments are more important than counties with few existing establishments. Thus, the mean shift parameter estimates the MDP effect for a winning county with the average number of existing establishments. On the other hand, a specification without lagged establishment weights estimates the MDP effect for the average winning county. For the purposes of this paper, the latter is preferred because of my interest for the MDP effects for the typical county—not the typical establishment as in the weighted case.

Table 2 presents the unweighted change in counties' number of establishments and output identified by GMc losers. Treating all winner and loser counties equally, Table 2, Column (1) reports that the number of manufacturing plants increased by 6.88% in the average winning county. This indicates counties with a large number of manufacturing establishments may benefit more than those with fewer existing establishments. If agglomeration spillovers are increasing in number of firms, then it makes sense that firm entry is greater in counties with more existing manufacturing establishments.

[Insert Table 2 approximately here]

The unweighted change in output as measured by value of shipments and value-added are presented in Table 2, columns (2) and (3), respectively. Using the log value of shipments as the dependent variable, winning counties experienced a statistically significant increase in output of 21.25%. The implied elasticity of 1.36 is very close to the GHM elasticity of 1.33. Value added as the dependent variable yields smaller estimated changes than with value of shipments. Winning county manufacturing value-added significantly increased by 18.92%. Thus, there is evidence of significant productivity spillovers reflected in output and new firm entry. Theory predicts significant productivity spillovers should also be reflected in wages. However, the quality-adjusted wage estimates give little evidence in support of large productivity spillovers.

Section 4.2 argues earnings per worker may be a better indicator of the MDP effect on

winning county wages. Table 3, Column (1) and (2) report the results of estimating Models 1 and 2 for earnings per worker and wage employment growth. 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.[17] There is not an economically or statistically significant change in winning county wages as measured by earnings per worker either.

[Insert Table3 approximately here]

After an MDP opening, winning county employment growth rates increased by 1.21% compared to GMc losers. The mean growth rate for all counties in that sample is 1.77% at $\tau = -1$. Model 1 suggests the mean growth rate rises to 2.98% in winning counties after the MDP opens. Model 2 confirms a positive level change in growth rates; however, the effect after five years is negative due to a decrease in the underlying trend. If we graphed the employment growth trend in counties, an MDP moves the line upward but makes it flatter. Although the five year effect isn't statistically significant, the level and trend estimates are separately significant. The magnitude of 5 year estimates suggests that the winning county employment annual growth rate falls to approximately 0.13% 5 years after the MDP opening.

When winning county effects are identified by GMc losers, changes in manufacturing output indicate substantial spillovers. Increases in value of shipments are similar in magnitude to those reported by GHM. Changes in value-added output are smaller, but still suggest positive agglomeration externalities. As theory predicts in the presence of spillovers, winning counties experience significant new firm entry. However, the increase in winning county wages is less than expected in the presence of significant agglomeration economies. Evidence for the predicted employment growth is also less than convincing and may even be negative after five years.

4.2 Fiscal Surplus

Although there is some evidence in support of new economic activity, the activity must generate fiscal surplus to induce the virtuous cycle of economic development. Tables 4-6 reports

[17] The estimated increase after five years is calculated by $\theta_1 + 6\theta_2$ because GHM allow an effect in $\tau = 0$.

results for changes 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. Winning counties experienced a significant 11.12% increase in mean general own revenue after an MDP opening (Table 4, 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.[18] A fiscal surplus is achieved when increased revenues are greater than increased expenditures. Recall, decreased tax rates and/or increased public services provide evidence of fiscal surplus. Revenue per person decreases by approximately $677 in Table 4, Column (2); while the rate of revenue collection per personal income doesn't really change in Table 4, Column (3). Since the rate remains unchanged, the decrease in revenue per person shouldn't be interpreted as evidence of fiscal surplus; rather, it suggests in-migration without commensurate income growth.

[Insert Table 4 approximately here]

From Table 4, Column (4), it is clear the increased revenue experienced by winning counties is driven by increases in property tax revenue. Winning counties collect 13.43% more property tax revenue after an MDP opening. 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 positive net MDP externalities.[19] Recall that there was not an economically or statistically significant change in earnings per worker compared to GMc losers and a 1.7% increase in quality-adjusted wages. 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 13% in winning counties, one might expect larger wage changes than those

[18] 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.

[19] Changes in property values reflect net agglomeration externalities to the extent that positive agglomeration spillovers and cost increases are capitalized into land values.

reported above. 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 GMc 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. At best, the property tax results support positive agglomeration spillovers. They do not provide any evidence of a fiscal surplus distributed through lower property tax rates.

As discussed above, a heavily incentivized MDP induces the hypothesized virtuous cycle of 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 5, Column (1) states that winning counties significantly increased their outstanding debt by 22.51% compared to GMc losers. Outstanding debt per capita also increases by approximately $2,244 per person, as reported in Column (2)of Table 5. These results provide evidence against a MDP-induced fiscal surplus.

[Insert Table 5 approximately here]

Fiscal surplus may be distributed through decreased tax rates and/or improvements in public services to induce the virtuous cycle of economic development. Table 6, Columns (1)-(4), 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.

I obtain the expected result that estimated winning county service expenditure increased by more than own revenue. Clearly, this is consistent with the debt findings. Under the GHM

identification strategy, service expenditure increases accompany declines or no change in spending per capita. This suggests that winning county service expenditures grew to keep pace with population, rather than to increase the level of services (i.e. distribute fiscal surplus).

Winning county property tax revenues increase substantially after an MDP opening. The increase could be interpreted as capitalization of agglomeration externalities into property values, which supports the finding that MDPs generate significant positive spillovers. Significant increases in service expenditures accompanied by no change in spending per capita suggest substantial in-migration after an MDP opening. A growing population in winning counties would also put upward pressure on property values. Thus, it is difficult to know how much of the property tax revenue increase is attributable to agglomeration spillovers.

Although there is evidence MDPs generate significant increases in winning county productivity compared to GMc losers, the wage and employment growth results are unexpected. Any gains in productivity do not appear to induce the virtuous cycle of economic development. Employment growth rates increase initially, but fall below GMc losers after a few years. There is 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

5. More on Identification/Alternate Strategy

5.1 GHM Revealed Rankings Strategy

The GHM identification strategy relies on firms' revealed rankings over potential locations as reported in the *Site Selection* magazine regular feature titled "Million Dollar Plant" (MDP). Ignoring the potential for strategic revelation, the GHM "identifying assumption is that the incumbent plants in the losing counties form a valid counterfactual for the incumbents in the winning counties, after conditioning on differences in preexisting trends, plant fixed effects, industry by year fixed effects, and other control variables (GHM 2010, p. 539).[20] In other words,

[20] To anyone who has not spent a great deal of time thinking about economic development incentives competition, the assumption that the "losers" identified in the MDP series are the correct counterfactual makes intuitive sense. However, to economic development incentive scholars, the public announcement of competing communities is part of a strategy for increasing incentives bids (See Bucholz's case study of Fed Ex in Schweke (2009) for an

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 incumbent plant TFP—the only significant difference being that they did not receive the MDP. By utilizing appropriate econometric techniques to control for any unobservable differences, the impact of the MDP may be isolated by comparing to the "loser" counterfactual.

GHM estimate the effect of "winning" an MDP using what amounts to a difference-in-difference (DID) matching estimator. Their "matches" are (approximately) those identified as "losers" in the magazine articles. There are at least two related ways in which the GHM identifying assumption could be invalid and thus bias effect estimates: i) the MDP "losers" are not a true counterfactual to the "winners", and ii) there are unobserved productivity shocks systematically correlated with "winning".

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 or have unstable impacts on outcomes over time. Assuming that any unobservable characteristic affecting both selection and outcomes are time-invariant, the DID estimator will produce consistent estimates of the "winner" effect even if there are important differences between "winners" and "losers". However, if unobservable differences are not stable over time in their impact on outcomes or time-varying, the DID matching estimator will be biased. The GHM identification strategy thus assumes that conditioning on appearance in the magazine effectively conditions on all time-variant unobservables that influence "winning," "losing," and the outcome variable(s). They must

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excellent discussion of this firm strategy.) Not only is the firm's rationale for revealing its true counterfactual suspect, but it is not clear that the identified "losers" were in fact identified as such by the company. 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. It very well could be then that so-called losers were the ones with lagging growth rates who felt compelled to enter the economic development incentives bidding war.

assume any remaining systematic differences between "winners" and "losers" in their sample are time invariant (and persistent level differences have a stable impact on outcomes over time).

GHM argue that unobservables drive the site selection process and outcomes. They assert the "winners" and "losers" identified in the magazine are "nearly" identical with respect to those unobservables. Agglomeration spillovers are the postulated mechanism for direct and indirect MDP effects in GHM. The agglomeration literature suggests economic size, density, industrial composition, transportation, wages and other urbanization economies influence spillover effects (see Rosenthal and Strange 2004 for a review). Site selection studies suggest many of these same factors influence selection as a location for a new facility (Brouwer *et al*. 2002; Guimaraes *et al*. 2003; Devereux *et al*. 2007).

If the GHM identification strategy produces consistent "winner" effect estimates, then we would expect "winners" and "losers" to be "nearly" identical with respect to these factors. To assess whether there is any reason 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 their 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 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.

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. 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 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.

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 documents, I identified the correct counterfactual for 9 out of 10 cases. Of those 9, GM report 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, but only 2 of those 7 represent the actual finalists.

Four of 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 Roanake, 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 further calls into question the GHM identification assumption of (nearly) identical "losers". 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.

5.2 Matching Strategy

An alternative to the GHM identification strategy is to "match" winners based upon the aforementioned agglomeration factors. In fact, the GHM estimator can be seen as a DID matching estimator where "matches" are determined by *Site Selection* magazine.

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 GHM 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 productivity and site selection 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 Rosenbaum1993; Rubin and Thomas 2000).

In fact, matching on a misspecified propensity score model can still be bias-reducing and efficiency-enhancing (Rubin and Thomas 1992, 1996; Hill *et al*. 1999; Stuart and Rubin 2008). 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 (Dehajia 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. The latter is necessary for consistent estimation (Frangakis and Rubin 2002; Imbens 2004; Stuart and Rubin 2008).

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. 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. Thus, regional factors are likely highly correlated with both selection and outcome. 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.[21,22] The covariates in the propensity score model, as well as the geographic proximity restriction, are in the spirit of List *et al*. (2003). Michalopoulas *et al*. (2004) find that comparing treated observations to counterfactuals in the same state is bias-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 one of the recommendations for good propensity score models found in

[21] As a robustness check, all outcomes were also analyzed using matches located within 100-250 miles of the winning county. The results were qualitatively and quantitatively similar. Estimates are available upon request from the author.

[22] 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.

Heckman *et al*. (1997), Heckman *et al*. (1998a), Heckman *et al*. (1998b), and Glazerman *et al*. (2004).

The number of propensity score neighbors within 50-100 miles of the winner and the same year implies a well-known bias-efficiency trade-off (Dehajia and Wahba 2002; List *et al*. 2003; Ho *et al*. 2007; 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 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.[23,24] The final set are the distance-based covariate matches.

5.3 Implications

Table 7 reports the results of balancing tests for all samples. The value of key covariates for GMc losers is statistically different from winners. GMc losers are much larger than winners in terms of population. Given that economic size and density are important productivity determinants, this difference raises some concerns. GMc losers are also much closer to metropolitan areas than winners. Without weighting GMc losers, earnings per employee are also

[23] 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.

[24] 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.

higher in GMc loser counties than winner counties. Winners are more concentrated in manufacturing than GMc losers. They have significantly different shares of employment in farming and FIRE. However, the matched samples are well-balanced.

[Insert Table 7 approximately here]

It is possible that the unobservables captured by the GHM revealed rankings strategy dominate the observables 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 observable matching estimators produce more reliable results.

6. Matching Strategy

6.1 Indirect Measures

Under the matching strategies, the effect of winning an MDP on manufacturing establishments and output is smaller in magnitude than under the revealed rankings strategy. When counties are weighted by lagged number of establishments, matching estimates range from a decrease of 3.37% in the number of plants to an increase of 5.35%; although they are not statically significant at conventional levels. Table 8, Columns (1) unweighted change in establishments compared to the nearest covariate, and one, five, and caliper propensity score neighbors. The estimates are generally negative, but not statistically different from zero either. Observable matching provides little evidence in support of new entry.

Table 8, Columns (2) and (3) report the change in output as measured by value of shipments and value-added, respectively. Using value of shipments, the winning counties experienced a statistically significant increase in output of 13.2% when losers are defined as the five nearest propensity score neighbors and 10.3% when caliper neighbors are used. When GMc losers identify the MDP effect, the corresponding increase is an estimated 21.25%. 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 little increase in winning county output above that which is attributable to the MDP. Value-added output changes are smaller, with lower significance levels than the result from value of shipments changes.

Winning county manufacturing output significantly increased by 10.76% and 9.63% compared to the five nearest propensity score and caliper neighbors, respectively. Under the GHM identification strategy, winning counties experienced value-added output increases of 18.92%.

[Insert Table 8]

The estimated quality-adjusted wage increase does not appear very sensitive to the identification strategy. Panel B, Column (4) of Table 8 presents the nearest propensity score neighbor, nearest five propensity score neighbors, and nearest covariate neighbor matching estimators.[25] Using the five nearest propensity score neighbors, there is a statistically significant 1.82% increase in winning county wages after an MDP opening. The estimated magnitude is similar under other matching methods, but not statistically significant at conventional levels.

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 9 reports an increase in winning county wages over more than 3% compared to the nearest and nearest five propensity score neighbors as well as the nearest covariate neighbors. Model 2 implies similar increases after 5 years, although estimates are less precise than Model 1.

[Insert Table 9 approximately here]

Model 1 and 2 estimated changes in winning county employment growth rates are presented in Table 10. Although the observable matching estimators produce smaller estimates of the MDP effect, the difference is slight. When the nearest propensity score neighbors identify the winner county mean shift, growth rates significantly increase by 0.95%. Model 2 confirms a positive level change in growth rates; however, the effect after five years is negative due to a decrease in the underlying trend. If we graphed the employment growth trend in counties, these results suggest an MDP moves the line upward but makes it flatter. Although the five year effect

[25] Nearest propensity score radius neighbor estimates omitted due to computation space required.

isn't statistically significant, the level and trend estimates are separately significant. The magnitude of 5 year estimates suggests that the winning county employment growth rate falls to less than 1% from 1.77% per year.

[Insert Table 10 approximately here]

Using the GHM identification strategy, MDPs generate significant spillovers that are not reflected in all indirect measures of productivity. 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. Although there is some indication of positive spillovers in output, the vast majority of increased output is attributable directly to the MDP. There is not strong evidence in support of firm entry. Quality-adjusted wages rise modestly, while earnings per worker significantly increase, though this could simply be offset by higher housing prices. This could also be taken as evidence of MDP-induced externalities. Taken with the output changes, wages suggest spillovers closer in magnitude to 3% than the 12% suggested by revealed rankings estimates used by GHM. However, employment growth rates do not reflect theoretical predictions.

6.2 Fiscal Surplus

The GHM and matching strategies tell different stories with respect to winning county revenue. Table 11 reports results for the DID estimated change in counties' revenue. Under the revealed rankings strategy, winning counties experienced a significant 11.12% increase in general own revenue. However, the change is indistinguishable from zero when compared to observable matches within 50-100 miles. There is also no evidence of fiscal surplus distribution through decreased tax rates.

[Insert Table 11 approximately here]

Estimates also suggest MDPs don't affect revenue collection as a share of area income. However, revenue per capita increases. Winning counties collect $1,138 more per person than their nearest covariate neighbors. The sign and magnitude are similar under the other observable matching strategies, although they are not statistically significant. Given the lack of rate effect,

the increase in revenues per person indicates income growth which outpaces population growth and/or increased property tax base. 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 over increased property tax rates. Increased property values provide some evidence in favor of positive externalities. They do not provide evidence of fiscal surplus unless the resulting increase is greater than expenditure, which seems unlikely given the debt and expenditure results in Tables 12 and A2-A5.

Observable matching estimators in Table 12 confirm the size and significance of winning county debt increases estimated under the revealed rankings strategy. Winning counties significantly increased outstanding debt by 23.58%, 24.53%, and 21.48% compared to the nearest one, five, and caliper propensity score neighbors. However, the estimated increase in debt per capita is much larger than the revealed rankings estimate. Winning counties significantly increased their outstanding debt by $8,151 and $7,713 per person compared to their nearest five and caliper propensity score neighbors, respectively.

[Insert Table 12 approximately here]

Fiscal surplus may be distributed through decreased tax rates and/or improvements in public services to induce the virtuous cycle of economic development. Appendix 4, Tables A2-A5 present the changes in *own* expenditure and expenditure per capita on K-12 education, parks and recreation, fire, and police services. 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.

Regardless of identification strategy, I obtained the expected result that estimated winning county service expenditure increased by more than own revenue. Clearly, this is consistent with the debt findings. Under the observable matching strategy, winning counties

experience smaller expenditure increases than under the revealed ranking strategy. Tables A2-A5 also suggest increased spending per person. Thus, winning counties appear to provide an increased level of service to their (growing) populations. However, observable estimators of debt and revenue effects suggest any increased service levels are funded by debt rather than a distribution of fiscal surplus. Since increased production costs may be part of the increase per person as well, expenditures provide little evidence of fiscal surplus in winning counties after an MDP opening.

After an MDP opening, winning counties experienced increased revenues compared to observable matches. General own and property tax revenue changes are a third of those estimated under the GHM strategy. If property value increases reflect capitalization of spillovers, then these estimates also suggest MDPs generate more modest spillovers than reported by GHM.

Under the observable matching strategy, winning counties spent more on services as well services per person after an MDP opening. This could suggest that the improved services part of the virtuous cycles is true. Yet, the substantial increase in winning county debt and debt per capita casts doubt on that conclusion. The debt findings indicate service improvements are funded by borrowing and not a distribution of fiscal surplus. Demand for increased public services appears to outpace increase revenue in winning counties.

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. Some proponents argue that induced firms generate significant agglomeration externalities and incentives simply allow firms to internalize their spillovers. Critics assert that the general equilibrium effects of shifting public resources towards MDPs dominate the spillover effects.

This paper contributes to the debate by investigating whether a set of heavily incentivized large firms induce a virtuous cycle of economic development or something closer to the winner's curse. It employs the Greenstone, Hornbeck, and Moretti (2010) identification strategy, as well as an alternative identification strategy, to estimate aggregate county effects from "winning" the

competition for a MDP. Specifically, the paper identifies winner county counterfactuals by matching on observables known to drive productivity as well as geography. Using both identification strategies, it tests for the presence of agglomeration spillovers using the indirect measures suggested by theory. In order for a MDP to induce a virtuous cycle of economic development, it must generate agglomeration externalities sufficient to induce new economic activity as well as fiscal surplus. Thus, the paper also tests for MDP effects on public revenues and expenditures.

The GHM estimates suggest large, new firms generate productivity spillovers which may justify the substantial incentive packages used to lure them. The GHM productivity gains are larger than average estimates for increases in human capital, own-industry firms, FDI and doubling city size. Local governments often must choose between allocating scarce resources to education, infrastructure, attracting an MDP, or other economic development activities. Thus, the GHM results could have profound economic development policy implications.

However, this paper's results indicate successful attraction of an MDP is not economic development's "magic bullet". The analysis suggests MDP effects are sensitive to identification strategy as well as a number of econometric choices. Using the GHM identification strategy, MDPs generate significant spillovers that are not reflected in all indirect measures. Evidence in favor of productivity spillovers is more modest when identified by geographically proximate, observable matches. Neither identification strategy provides much evidence that MDPs induce the virtuous cycle of economic development.

The results suggest that if significant productivity spillovers exist, the general equilibrium effects of directing public resources towards MDPs may dominate them. The results also call into question the magnitude of the spillovers estimated in GHM. Does conditioning on revelation in the magazine capture the most important unobservables driving future expected profits, productivity, or outcomes? Does it do so better than conditioning on observable determinants and geography? It seems unlikely that the unobservables captured by the GHM revealed rankings strategy eclipse known determinants and geography. Thus, the paper's findings also contribute to

the ongoing debate surrounding quasi-experimental research design.

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Table 1: Changes in Counties' Number of Establishments, Total Value of Shipments, and Quality-Adjusted Waged Identified by GMc Losers

	A. Census of Manufactures		B. Census of Population
	Log(Establishments)	Log(Value of Shipments)	Log (Wages)
	(1)	(2)	(3)
Difference-in-Differences	0.0961**	0.1853**	0.0171
	(0.0482)	(0.0936)	(0.0126)
R^2	0.98952583	0.99639356	0.35958457
N	598	571	4,661,204

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01

Note-The table reports results from three regressions. Census of Manufactures pre- and post-treatment Census year assignments are made according to the conventions detailed in Appendix 2. Results for Sample B are also available in Appendix 2. Observations are weighted by the county's total number of manufacturing establishments in years -6 to -10. Census of Population estimates employ the same weighting scheme as GHM.

Table 2: Unweighted Changes in Counties' Number of Establishments and Output Identified by GMc Losers

	(1) Establishments	(2) Value of Shipments	(3) Value-Added
Difference-in-differences	0.0688*	0.2125**	0.1892**
	(0.0392)	(0.0818)	(0.0726)
R^2	0.99228646	0.99554197	0.99556196
N	598	571	571

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01

Table 3: Changes in Counties' Earnings per Worker and Wage Employment Growth Following an MDP Opening

	Earnings per Worker	Wage Employment Growth
	(1)	(2)
	Model 1	
Mean Shift	0.0046	0.0121**
	(0.0133)	(0.0048)
R^2	0.98182458	0.43913687
N	2028	2028
	Model 2	
Effect after 5 years	0.0181	-0.0164
	(0.0216)	(0.0119)
Level Change	0.0053	0.0144**
	(0.0065)	(0.0062)
Trend break	0.0021	-0.0051***
	(0.0030)	(0.0018)
R^2	0.98184125	0.44312628
N	2028	2028

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01

Table 4: Changes in Counties' General Own and Property Tax Revenue Identified by GMc Losers

	A. General Own Revenue			B. Property Tax Revenue
	Level	Per Capita	Per Personal Income	Level
	(1)	(2)	(3)	(4)
Difference-in-differences	0.1112*	-0.0677	0.0002	0.1343**
	(0.0669)	(0.0713)	(0.0045)	(0.0581)
R^2	0.985522	0.87447979	0.74737648	0.98950143
N	624	624	624	624

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01

Table 5: Changes in Counties' Outstanding Debt Identified by GMc Losers

	Outstanding Debt (1)	Outstanding Debt Per Capita (2)
Difference-in-differences	0.2251*	0.2244
	(0.1155)	(0.4939)
R^2	0.95371811	0.60974807
N	624	624

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01

Table 6: Mean Shift in Counties' Service Expenditures Identified by GMc Losers

	K-12 Education (1)	Parks & Recreation (2)	Police (3)	Fire (4)
Expenditure Level	0.1305***	0.19159956	0.1563**	0.2559**
	(0.0426)	(0.1219)	(0.0599)	(0.1032)
R^2	0.991739	0.95654981	0.990009	0.975923
N	624	621	624	623
Expenditure Per Capita	0.0030	-0.0025	-0.0082	-0.0045
	(0.0281)	(0.0039)	(0.0054)	(0.0036)
R^2	0.933387	0.74512046	0.898734	0.881287
N	624	624	624	624

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01

Table 7: Balancing Tests

	GMc Winner	GMc Losers (unweighted)				GMc Losers (weighted)				Nearest 1 PS Neighbors			
Variable	**Mean**	**Mean**	**%bias**	**t**	**p>t**	**Mean**	**%bias**	**t**	**p>t**	**Mean**	**%bias**	**t**	**p>t**
Total Population (1,000's)	230	390	-46.8	-2.78	0.006	340	-33.6	-2.23	0.026	240	-0.9	-0.05	0.96
Interstate	0.8889	0.8925	-1.1	-0.07	0.944	0.8976	-2.8	-0.2	0.845	0.9153	-8.8	-0.49	0.628
Nearest Metro	32.6030	20.7950	33.7	2.17	0.032	22.0270	30.2	2.39	0.018	31.3220	3	0.17	0.868
Working Age	0.4046	0.4043	0.9	0.05	0.956	0.4053	-2	-0.16	0.875	0.4001	12.6	0.7	0.488
Minority	0.1518	0.1638	-8.5	-0.52	0.601	0.1764	-17.4	-1.24	0.218	0.1462	4.1	0.22	0.823
Earnings	17.3310	18.3610	-23.7	-1.44	0.153	18.4270	-25.2	-1.87	0.063	16.6960	16	0.88	0.379
Mfg Share	0.2114	0.1700	41.3	2.56	0.011	0.1693	42	2.83	0.005	0.1900	20.3	1.12	0.266
Farm Share	0.0480	0.0242	42.1	2.71	0.007	0.0210	47.7	3.89	0.000	0.0486	-1	-0.06	0.956
FIRE Share	0.0611	0.0691	-28.2	-1.69	0.093	0.0628	-5.7	-0.36	0.716	0.0598	5.8	0.32	0.749
Service Share	0.2170	0.2316	-21.9	-1.27	0.207	0.2223	-8	-0.47	0.642	0.2203	-5.3	-0.29	0.774
Military Share	0.0143	0.0226	-24.4	-1.39	0.167	0.0206	-18.6	-1.17	0.243	0.0127	15.4	0.84	0.401

		Nearest 5 PS Neighbors (weighted)				Nearest Odds Ratio Radius				Nearest Covariate Neighbors			
Variable	**Mean**	**Mean**	**%bias**	**t**	**p>t**	**Mean**	**%bias**	**t**	**p>t**	**Mean**	**%bias**	**t**	**p>t**
Total Population (1,000's)	230	210	5.3	0.33	0.743	170	18.5	0.98	0.331	170	18.3	0.98	0.33
Interstate	0.8889	0.8903	-0.5	-0.04	0.971	0.8712	5.7	0.29	0.771	0.8772	5.5	0.29	0.771
Nearest Metro	32.6030	41.1480	-19.5	-1.48	0.139	42.9090	-25.2	-1.29	0.199	36.6730	-4.4	-0.24	0.814
Working Age	0.4046	0.3999	13	1.03	0.302	0.3987	16.2	0.87	0.384	0.3971	15.8	0.84	0.401
Minority	0.1518	0.1405	7.9	0.63	0.531	0.1406	7.5	0.41	0.684	0.1512	-0.6	-0.03	0.972
Earnings	17.3310	16.3810	23.9	1.93	0.054	16.1680	28.1	1.55	0.124	16.5370	10.8	0.58	0.564
Mfg Share	0.2114	0.2092	1.9	0.15	0.883	0.2087	2.5	0.13	0.895	0.1945	12.9	0.69	0.491
Farm Share	0.0480	0.0531	-7.9	-0.65	0.514	0.0608	-18.9	-1.01	0.314	0.0497	1.1	0.06	0.955
FIRE Share	0.0611	0.0571	17.4	1.46	0.144	0.0557	22.8	1.22	0.224	0.0592	4.9	0.26	0.793
Service Share	0.2170	0.2088	14.5	1.05	0.294	0.2033	23.2	1.28	0.204	0.2121	6	0.32	0.753
Military Share	0.0143	0.0147	-2.6	-0.19	0.853	0.0146	-2.4	-0.13	0.900	0.0134	-8.5	-0.45	0.652

Table 8: Changes in Counties' Number of Establishments, Output, and Quality-Adjusted Waged Identified by Matching

	A. Census of Manufactures			B. Census of Population
	Log(Establishments)	Log(Value of Shipments)	Log(Value-Added)	Log (Wages)
	(1)	(2)	(3)	(4)
1 Nearest Propensity Score Neighbor	-0.0083	0.1097	0.0924	0.0190
	(0.0442)	(0.0896)	(0.0849)	(0.0128)
R^2	0.98906093	0.99534277	0.99523379	0.35305631
N	461	434	434	3580113
5 Nearest Propensity Score Neighbors	-0.0011	0.132*	0.1076*	0.0182*
	(0.0327)	(0.0700)	(0.0624)	(0.0107)
R^2	0.98860862	0.99473598	0.99389099	0.34765223
N	1339	1258	1258	11655745
Nearest Odds Ratio Radius Neighbors	0.00872852	0.103*	0.0963*	
	(0.0289)	(0.0625)	(0.0535)	
R^2	0.98784172	0.99448485	0.99392283	
N	3189	2971	2971	
Nearest Covariate Score Neighbors	-0.0527	0.0709	0.0316	0.02006507
	(0.0431)	(0.0943)	(0.0878)	(0.0167)
R^2	0.98899786	0.99537298	0.99509192	0.3561917
N	429	404	404	3066861

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01;

Table 9: Changes in Earnings per Worker

	(1)	(2)	(3)	(4)
	Model 1			
Mean Shift	0.0319**	0.0306**	0.0164	0.0354**
	(0.0153)	(0.0125)	(0.0119)	(0.0156)
R^2	0.976182	0.978407	0.981409	0.976976
N	1586	4628	11193	1482
	Model 2			
Effect after 5 years	0.0256	0.0200	0.0213	0.0325
	(0.0228)	(0.0200)	(0.0190)	(0.0242)
Level Change	0.0096	0.0102	0.0049	0.0162**
	(0.0072)	(0.0062)	(0.0059)	(0.0078)
Trend break	0.0027	0.0016	0.0027	0.0027
	(0.0032)	(0.0027)	(0.0026)	(0.0034)
R^2	0.976335	0.978476	0.981439	0.977114
N	1586	4628	11193	1482

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01; (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors

Table 10: Changes in Wage Employment Growth Indentified by Observable Matches

	(1)	(2)	(3)	(4)
	Model 1			
Mean Shift	0.0095*	0.0064	0.0064	0.0057
	(0.0051)	(0.0044)	(0.0042)	(0.0052)
R^2	0.442404	0.438151	0.433606	0.459157
N	1586	4628	11193	1482
	Model 2			
Effect after 5 years	-0.0112	-0.0131	-0.0084	-0.0089
	(0.0148)	(0.0100)	(0.0090)	(0.0142)
Level Chai	0.0034	0.0076	0.0087	0.0070
	(0.0073)	(0.0057)	(0.0054)	(0.0067)
Trend brea	-0.0024	-0.0034**	-0.0029*	-0.0026
	(0.0021)	(0.0016)	(0.0015)	(0.0022)
R^2	0.444368	0.439198	0.433956	0.461341
N	1586	4628	11193	1482

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01; (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors

Table 11: Mean Shifts in Counties' Revenue Identified by Observable Matches

	(1)	(2)	(3)	(4)
General Own Revenue	0.0499	0.0456	0.0313	0.0470
	(0.7081)	(0.0576)	(0.0527)	(0.0726)
R^2	0.982291	0.984893	0.985786	0.981347
N	480	1408	3408	456
Revenue Per Capita	0.0705	0.0696	0.0808	0.1138*
	(0.0758)	(0.0575)	(0.0519)	(0.0676)
R^2	0.847985	0.860512	0.848935	0.858586
N	480	1408	3408	456
Revenue Per Personal Income	-0.0017	-0.0016	-0.0020	0.0015
	(0.0045)	(0.0040)	(0.0037)	(0.0031)
R^2	0.711881	0.785804	0.781809	0.732947
N	480	1408	3408	456
Property Tax Revenue	0.0438	0.0481	0.0153	0.0370
	(0.0643)	(0.0513)	(0.0451)	(0.0753)
R^2	0.986678	0.98631	0.988579	0.98466
N	480	1408	3408	456

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01; (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors

Table 12: Mean Shifts in Counties' Outstanding Debt Identified by Observable Matches

	(1)	(2)	(3)	(4)
Outstanding Debt	0.2358*	0.2453**	0.2148**	0.1541
	(0.1337)	(0.0964)	(0.0859)	(0.1508)
R^2	0.951257	0.955052	0.952164	0.937648
N	480	1408	3408	456
Outstanding Debt Per Capita	0.6833	0.8151*	0.7713*	0.5390
	(0.4898)	(0.4303)	(0.4121)	(0.4779)
R^2	0.593031	0.617978	0.602317	0.607471
N	480	1408	3408	456

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01; (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors

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)[26], 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 the GHM sample, that is not so for the cases with incorrectly identified losers and included losers

[26] 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.

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 GHM sample but article cites Palm Beach, FL.

There are quite a few more GM cases were 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 the article but not listed as a loser; Squibb (41); Yamaha (26); DuPont/Phillips (21) – only says search concentrated on Research Triangle area; Ft. Howard Paper (16) – Effingham, SC never mentioned, 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. If GM were able to go to primary documents and identify these losers, then why was the decision made to do so for these cases and not for the plethora of other potential cases in the articles which cite winner counties without citing losers?

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

						Montgomery, MD	loser	y	y
17	14	1985	1985	Kyocera	Mfg	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
						Jasper, SC	loser	y	n
20	17	1985	1985	Rockwell International	Mfg	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
						Troup, GA	loser	y	y
32	29	1987	1987	Nippon Kokan (NKK)	Mfg	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	Toyata	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
						Washington, MS	loser	y	n
61	49	1990		Tennessee Eastman	Mfg	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	
						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
						Lauderdale, TN	loser	y	y
77	61	1991	1991	Wal-mart stores	Trade	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
						Rennsselaer, 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 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.[27]
- 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

[27] 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.

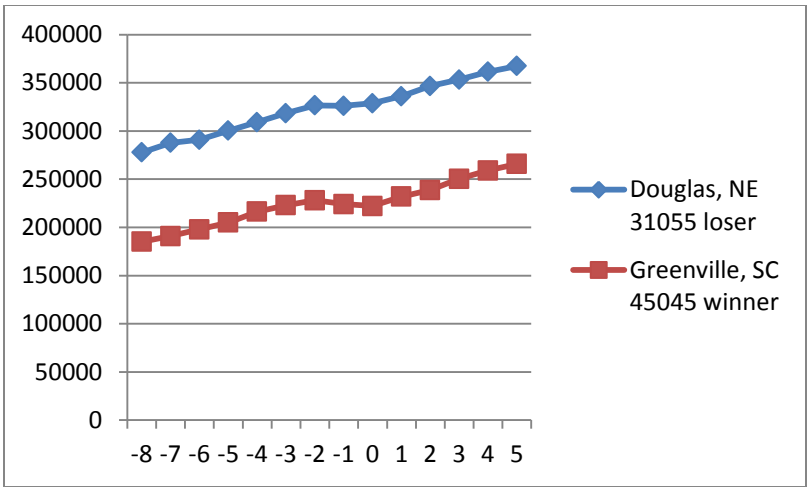

Figure A1: Total Employment

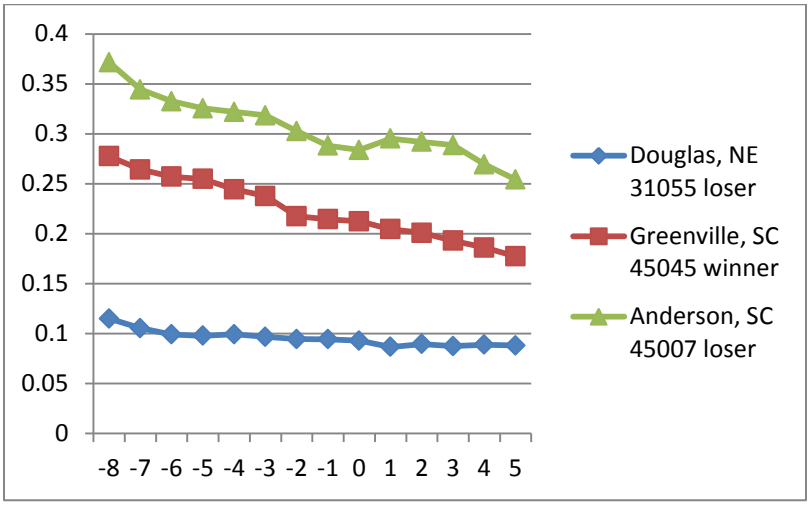

Figure A2: Mfg Share

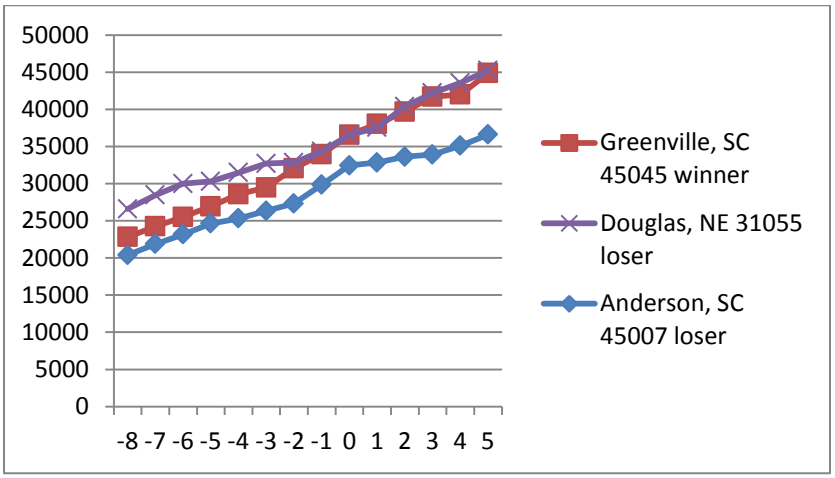

Figure A3: Mfg Wage per Worker

Appendix 4: Local Government Service Expenditures Identified by Observable Matches

Table A2: Mean Shifts in Counties' Education Expenditure Identified by Observable Matches

	(1)	(2)	(3)	(4)
Education Expenditure	0.0790	0.0298	0.0443	0.0362
	(0.0483)	(0.0461)	(0.0344)	(0.0521)
R^2	0.989765	0.971969	0.982572	0.989333
N	480	1408	3408	456
Education Expenditure Per Capita	0.0423	0.0188	0.0220	0.0166
	(0.0255)	(0.0189)	(0.0166)	(0.0273)
R^2	0.942392	0.937003	0.93149	0.943263
N	480	1408	3408	456

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01; (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors

TableA3: Mean Shifts in Counties' Parks and Recreation Services Expenditure Identified by Observable Matches

	(1)	(2)	(3)	(4)
Parks & Rec Expenditure	0.0762	0.0968	0.0959	0.0607
	(0.1252)	(0.1005)	(0.0881)	(0.1170)
R^2	0.946026	0.954294	0.949732	0.944738
N	475	1400	3368	453
Parks & Rec Expenditure Per Capita	0.0033	0.0044	0.0069***	0.0023
	(0.0039)	(0.0029)	(0.0026)	(0.0045)
R^2	0.690883	0.733636	0.732502	0.66355
N	480	1408	3408	456

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01; (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors

Table A4: Mean Shifts in Counties' Police Service Expenditure Identified by Observable Matches

	(2)	(3)	(4)	(5)
Police Expenditure	0.0975	0.0796	0.1014**	0.0901
	(0.0675)	(0.0539)	(0.0496)	(0.0710)
R^2	0.986768	0.986535	0.984672	0.985462
N	480	1408	3407	456
Police Expenditure				
Per Capita	0.0056	0.0054	0.0095***	0.0056
	(0.0050)	(0.0039)	(0.0035)	(0.0056)
R^2	0.884451	0.88246	0.851636	0.880652
N	480	1408	3408	456

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01; (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors

Table A5: Mean Shifts in Counties' Fire Service Expenditures Identified by Observable Matches

	(1)	(2)	(3)	(4)
Fire Expenditure	0.1419	0.1177	0.1377	0.1782
	(0.1161)	(0.0960)	(0.0886)	(0.1350)
R^2	0.971466	0.970263	0.968932	0.962288
N	479	1407	3403	455
Fire Expenditure				
Per Capita	0.0007	0.0039	0.0075***	0.0026
	(0.0040)	(0.0027)	(0.0024)	(0.0046)
R^2	0.849712	0.848465	0.844044	0.833365
N	480	1408	3408	456

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01; (1) Nearest propensity score neighbor; (2) Nearest 5 propensity score neighbors; (3) Nearest propensity score radius neighbors; (4) Nearest covariate neighbors

Appendix 5: Conditioned Changes in Output

Table A.6: Conditioned Changes in Output, GMc Sample B

	(1)	(2)	(3)	(4)	(5)
	Value of Shipments				
Difference-in-differences	0.0372	0.0276	0.0264	0.0238	0.0154
	(0.0328)	(0.0361)	(0.0279)	(0.0226)	(0.0371)
R^2	0.999741	0.999774	0.999695	0.999561	0.999721
N	269	191	576	1377	184
	Value Added				
Difference-in-differences	0.0649	0.0328	0.0213	0.0316	0.0167
	(0.0762)	(0.0894)	(0.0660)	(0.0550)	(0.095)
R^2	0.998726	0.998672	0.998053	0.997469	0.998324
N	269	191	576	1377	184

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01; (1) GMc Losers

(2) Nearest propensity score neighbor; (3) Nearest 5 propensity score neighbors;

(4) Nearest propensity score radius neighbors; (5) Nearest covariate neighbors