Bidding for Industrial Plants:
Does Winning a 'Million Dollar Plant' Increase Welfare?

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Abstract

Increasingly, local governments compete by offering substantial subsidies to industrial plants to locate within their jurisdictions. This paper uses a novel research design to estimate the local consequences of successfully bidding for an industrial plant, relative to bidding and losing, on labor earnings, public finances, and property values. Each issue of the corporate real estate journal *Site Selection* includes an article titled "The Million Dollar Plant" that reports the county where a large plant chose to locate (i.e., the 'winner'), as well as the one or two runner-up counties (i.e., the 'losers'). We use these revealed rankings of profit-maximizing firms to form a counterfactual for what would have happened in the winning counties in the absence of the plant opening. We find that the plant opening announcement is associated with a 1.5% trend break in labor earnings in the new plant's industry in winning counties, as well as increased earnings in the same industry in counties that neighbor the winner. Further, there is modest evidence of increased expenditures for local services, such as public education.

Property values may provide a summary measure of the net change in welfare, because the costs and benefits of attracting a plant should be capitalized into the price of land. If the winners and losers are homogeneous, a simple model suggests that any rents should be bid away. We find a positive, relative trend break of 1.1% in property values. Since the winners and losers have similar observables in advance of the opening announcement, the property value results may be explained by heterogeneity in subsidies from higher levels of government (e.g., states) and/or systematic underbidding. Overall, the results undermine the popular view that the provision of local subsidies to attract large industrial plants reduces local residents' welfare.

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Introduction

Over the past 30 years, state and local governments have assumed a greater responsibility for economic development. For example, they frequently offer substantial subsidies to businesses to locate within their jurisdictions. These incentives can include tax breaks, low-cost or free land, the issuance of tax-exempt bonds, training funds, the construction of roads, and other infrastructure investments. These policies are controversial: local politicians and the subsidized companies usually extol the benefits of these deals, while critics complain that they are a waste of public monies.[1] It is difficult to evaluate these competing claims, because there is little systematic evidence on the consequences of these policies.[2]

The traditional approach to evaluating policies designed at attracting new plants is to calculate the number of jobs gained and the cost of the tax breaks awarded to firms. For example, it is widely cited that Mercedes received a $250 million ($165,000 per job) incentive package for locating in Vance, Alabama, the Toyota plant in Georgetown, Kentucky was awarded $200 million ($80,000 per job) and Boeing was given $50 million ($100,000 per job) in tax abatements to locates its corporate headquarters in Chicago (Mitol 2001; Trogen 2002). This "accounting" approach produces eye-catching statistics, but it has two important limitations. First, these calculations are done ex-ante and are rarely verified ex-post. Second, and more fundamentally, this approach does not offer a framework for determining whether the policies increase or decrease welfare of local residents. For example, is $165,000 per job a good deal for the residents of Vance, Alabama?

Economic theory provides mixed results on this question, as emphasized by Glaeser (2001) in a recent survey.[3] On the one hand, some models suggest that the attraction of new businesses generates positive spillovers and/or increases producer surplus. In these cases, local subsidies may be welfare enhancing or, at least, neutral for local residents. On the other hand, there are models that indicate that local subsidies may reflect government officials' private interests by providing for their own financial gain or satisfying Leviathan goals. In these settings, local government officials grant subsidies whose costs to local residents are larger than the benefits and therefore reduce residents' welfare.

This paper empirically assesses the consequences for counties of successfully bidding for large industrial plants on county-level earnings, property values, and government finances. The empirical

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[1] For example, a recent front-page article in the New York Times describes the experience of several localities that provided generous subsidies to attract large manufacturing firms in the 1980s and 1990s and apparently obtained limited benefits to the local economy. ("Paying for Jobs Doesn't Always Pay Off", *New York Times*, November 10, 2003).

[2] In a recent survey of the literature, Glaeser (2001) concludes that, although location based-incentives "seem to be a permanent part of the urban economic landscape, economists do not yet know why these incentives occur and whether they are in fact desirable". Further a Standard & Poor's publication states, "Economic development wars of recent years have not developed a sufficient track record to assess their true cost-benefit ratio" (Standard & Poor's 1993). Bartik (1991) provides the most comprehensive evidence.

challenge is that plants choose to locate where their expected profits are highest, which are a function of their location-specific expected future costs of production and any subsidies. A plant's expected future costs of production in a county depend on many unobservable factors, including the presence of a suitable transportation infrastructure, the availability of workers with particular skills, and the local regulatory environment. These factors are typically difficult to measure and their relative importance varies across plants based on the generally unobserved plant-specific production functions. Similarly, the subsidies are likely to be a function of a number of unmeasured factors, including any potential spillovers and the degree of local politicians' malfeasance.

Heterogeneity in the factors that determine variation in costs of production and subsidies across counties is likely to bias standard estimators. Valid estimates of the impact of a plant opening require the identification of a county that is identical to the county where the plant decided to locate in both expected future production costs and the factors that determine subsidies. We have little faith in our ability to ascertain and measure all these factors.

As a solution, we rely on the revealed rankings of profit-maximizing firms to identify a valid counterfactual for what would have happened in the absence of the plant opening. These rankings come from the corporate real estate journal *Site Selection*, which includes a regular feature titled the "Million Dollar Plants" that describes how a large plant decided where to locate. When firms are considering where to open a large plant, they typically begin by considering dozens of possible locations. They subsequently narrow the list to roughly 10 or so locations, among which 2 or 3 finalists are selected. The "Million Dollar Plants" articles report the county that the plant ultimately chose (i.e., the 'winner'), as well as the one or two runner-up counties (i.e., the 'losers'). The losers are counties that have survived a long selection process, but narrowly lost the competition. Our identifying assumption is that the losers form a valid counterfactual for the winners, after adjustment for differences in pre-existing trends. Even if this assumption is invalid, we suspect that this pairwise sampling approach is preferable to comparing winners to the other 3,000 U.S. counties.

Consistent with our assumption, we find that before the announcement of the winner county, winning and losing counties have similar trends in wage bill, employment, per capita income and employment-population ratio. Several other observable characteristics are also similar. By contrast, trends in the winning counties tend to be different from trends in all other US counties. Notably in the 8 years before the announcement of the winner, the trends in property values in winning and losing counties are virtually identical. Under the plausible assumption that property markets are forward-looking, this finding suggests that not only are winning and losing counties similar in the years before the

[3] See also the classic Oates (1972) and Zodrow and Mieszkowski (1986) papers on tax competitions and the provision of local public goods. Wilson (1999) provides another recent review of this literature.

announcement, but the expected future changes in economic activity that are capitalized into property values are also ex-ante similar. Overall, these findings lend credibility to the identifying assumption that the losers form a valid counterfactual for the winners.

The primary employment result is that in the new plants' 1-digit industry the wage bill increased by a statistically significant $16.8 million per year in winning counties (relative to losing ones) after the opening announcement (relative to the period before). This is 1.5% of the average wage bill in the plant's 1-digit industry in winning counties in the year before the announcement. Taken literally, this implies that six years later the wage bill in the new plant's 1-digit industry is roughly $100 million higher due to the plant opening than predicted by pre-existing trends. We also find evidence of positive trend breaks in the total wage bill in the new plant's industry in counties that neighbor the winner.[4]

These results convincingly suggest that the successful attraction of a new plant directly and indirectly increases local economic activity. However, they are not informative about whether the subsidies increase local residents' welfare because they do not account for their cost. Direct information on the cost of the subsidies is unavailable. We follow the stylized Roback (1982) model and argue that the net welfare effect of attracting a plant will be reflected in property values. This is because property values capitalize the increased economic activity in the county (i.e. the benefit of the subsidy) and the increase in property taxes--or reduction in public services--necessary to pay for the subsidy (i.e. the cost of the subsidy).[5]

We derive a simple model that demonstrates that in the presence of bidding for plants by counties, a successful bid may cause property values to increase, decrease or remain unchanged. When counties are homogeneous and politicians solely maximize residents' welfare, the successful attraction of a plant will leave property values unchanged. This is because counties raise their bids until the costs equal the benefits and they are indifferent about winning or losing. When counties are not homogenous or if states pay for part of the subsidy, the successful attraction of a plant may result in increased property values. This is because the county that has the most attractive characteristics or the largest contribution from state government can bid less than the second best county, and still win the plant. Finally when local politicians derive private benefits from granting subsidies, they will overbid, and property values may decrease.

Ultimately, the question of the effect of successfully attracting a plant on property values is an empirical one. Using a unique self-collected data file, we find that the successful attraction of a plant

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[4] See Hanson (1998) for evidence that shocks in a given locale affect neighboring jurisdictions and that this effect dissipates with distance. See Chapter 2 of Bartik (1991) for a review of the effects of state and local economic development programs on employment and economic growth in local areas. On a related topic, Evans and Topoleski (2002) examine the social and economics consequences of the opening of an Indian casino.

results in increased property values. Specifically, we find that in winning counties there is a relative trend break of approximately 1.1% in annual property values after the plant opening announcement. If our model's assumptions are valid, this finding can be interpreted as an increase in welfare for local residents. Even if the assumptions are not valid, the property value results appear to undermine the popular view that the provision of local subsidies to attract large industrial plants decreases welfare.

The final part of the paper estimates the effect of winning a plant on local government finances. A popular concern about the provision of subsidies is that local governments pay for them by cutting important services, such as education and police protection.[6] Using data from the Annual Survey of Governments, we find that local governments in winning counties experienced positive and roughly equal trend breaks in revenues and expenditures. Notably, there is a substantial increase in education spending and no change in police expenditures. Overall, there is little evidence of deterioration in the provision of vital public services in counties that win "Million Dollar" plants.

We want to emphasize that the focus of this paper is the credible estimation of the consequences for a county of successfully bidding for a new plant, relative to bidding and losing. There are a number of related policy and/or welfare questions, only some of which this paper can address. For example, the analysis does not provide evidence on whether states or the nation as a whole benefit from tax competitions between local governments to attract new business. Further, it is not informative about whether individual counties should pursue industrial development policies at all. However, since virtually all large plant openings induce tax competitions between jurisdictions, the salient question for a local government interested in attracting one of these plants is not whether to bid at all but what are the consequences of successfully bidding. The analysis is structured to inform this very question.[7]

The paper proceeds as follows. Section I reviews the theoretical explanations for the provision of local subsidies by local governments to attract plants and the determinants of plants' location decisions. It also provides a case study of BMW's decision to locate a new plant in South Carolina and intuitively describes our research design. Section II reviews the Roback (1982) model in the context of a plant opening and presents a stylized model of counties' decisions to bid for a plant and the effect of winning on property values. Section III describes the data sources and presents some summary statistics. Section IV explains the econometric model and Section V describes the results. Section VI interprets the results and VII concludes.

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[5] Gyourko and Tracy (1991) demonstrate that the efficient provision of public services is capitalized into land. They find that with services held constant, higher taxes are associated with lower land values.

[6] Glaeser (2001) emphasizes the importance of answering this question empirically, "Economists need to estimate what happens when localities are deprived of the marginal dollar. Does this loss lead to eliminating very valuable services or are fairly marginal services cut off?" (pp. 11-12).

I. Local Subsidy and Plant Location Decisions

This section is divided into two parts. The first reviews the existing theoretical literature on why local governments use incentives to attract some plants to their jurisdictions. The second subsection provides a case study of BMW's decision to locate a car assembly plant in the Greenville-Spartanburg area of northwestern South Carolina and, more generally, describes the intuition of our research design.

A. Why do Local Governments Offer Subsidies to Some New Plants?

In standard models, the provision of incentives to firms reduces the welfare of a locality's residents.[8] This is because the incentives lead to inefficiently high levels of local production. Yet, in the real world, large businesses frequently receive subsidies in exchange for their location decision. The implication is that the standard models may be unable to capture an important feature of these location decisions. Here, we review a recent line of research that provides a number of possible explanations for why localities provide incentives to newly locating firms. This review aims to highlight some of the "structural" forces that are likely to underlie our "reduced-form" estimates of the welfare consequences of successfully attracting a plant.[9]

The first set of theories emphasizes the idea that local governments aim to maximize only the welfare of their residents. One example is the case where agglomeration economies are associated with the location of the firm. For example, firms in the same industry may experience productivity increases as the number or size of geographically concentrated firms increase (Henderson 2003; Garcia-Mila and McGuire 2001). These spillovers may be due to the sharing of information, the number of high skilled workers (Rauch 1993; Glaeser et al. 1995; Moretti 2004a and 2004b), or that the new firms will attract other firms in the future. Regardless of their source, in the presence of these spillovers, localities will bid to attract the firms that produce them and the resulting subsidies allow the firm to capture the spillovers that they produce.

Glaeser (2001) suggests that local incentives may represent bids by communities to attract firms that will generate producer surplus for the current residents of the community. Conventional welfare analysis suggests that there may be welfare triangles to be gained by local workers. In particular, if the

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[7] If outcomes in counties that bid and lose are similar to counties without industrial development policies, then the analysis sheds light on the broader question of the consequences of whether industrial development policies are welfare enhancing. There is no a priori reason to believe that this condition does or does not hold.

[8] See Wilson (1999) for a review of the theoretical literatures on tax incentives.

[9] This review follows a recent paper by Glaeser (2001). See that paper for further details.

labor supply curve is upward sloping, inframarginal workers are made better off by the presence of the new firm. The size of the bids will reflect the welfare gains generated.[10]

Another explanation for tax incentives is that these upfront payments are compensation for future tax payments (Wilson 1996). In particular, once a plant begins to operate at a site it can be costly to change its location, because of the "sunk" nature of many industrial investments (see e.g., Goolsbee and Gross 2000 and Ramey and Shapiro 2001). The resulting immobility makes firms easy targets for local tax collectors. Since it is difficult for governments to credibly commit in advance to future tax rates, the upfront tax incentives compensate firms for future expropriation.

All of the previous theories are consistent with local politicians solely acting in the interests of their citizens, but an alternative view is that local incentives are due to corruption and influence or a desire on the politician's part to maximize the size of government. In the case of corruption and influence, the side-payments may occur through direct payoffs, contributions to re-election campaigns, or future employment for politicians or their friends or family (Glaeser 2001). The magnitude of these bribes will depend on the probability of detection and the punishment. An alternative possibility is derived from the Leviathan view of government (Brennan and Buchanan 1980). As applied to this case, politicians will seek to maximize the welfare of their citizens and the size of government or the tax base.

For the purposes of our paper, it is worth noting that the first set of models--where local subsidies are motivated by agglomeration economies or increases in producer surplus or compensation for ex-post appropriation--suggest that subsidies may be welfare enhancing or, at least, neutral for local residents. (We will show later under what conditions subsidies can be welfare enhancing or welfare neutral.) The second set of models--where local subsidies reflect government officials' private interests--suggest that subsidies may be welfare decreasing. We return to this point in Section II, where we incorporate these two alternative views in a simple theoretical framework.

B. A Case Study of the BMW Plant Location Decision and a New Research Design

In this subsection, we use a concrete example to illustrate how a particular firm selected a site for its new plant. In particular, we use information from the "Million Dollar Plant" series in the corporate real estate journal *Site Selection* to describe BMW's 1992 decision to site a manufacturing plant in the Greenville-Spartanburg area of South Carolina. Notably, this is one of the plants in our sample. A second goal of this case study is to highlight the empirical difficulties that arise when estimating the effect of plant openings on local economies. Further, we use this case study to informally explain why our

[10] It is also possible that the new firm generates consumer surplus for the residents. The consumer gains are possible if there are local markets for outputs (e.g., sports teams). Because we focus mainly on manufacturing plants, this possibility is less relevant for our purposes.

research design may circumvent these identification problems. A more formal analysis is conducted in Section II.

After overseeing a worldwide competition and considering 250 potential sites for its new plant, BMW announced in 1991 that they had narrowed the list of potential candidates to 20 counties. Six months later, BMW announced that the two finalists in the competition were Greenville-Spartanburg, South Carolina, and Omaha, Nebraska. Finally, in 1992 BMW announced that they would site the plant in Greenville-Spartanburg and that they would receive a package of incentives worth approximately $115 million funded by the state and local governments.

Why did BMW choose Greenville-Spartanburg? It seems reasonable to assume that firms are profit maximizers and choose to locate where their expectation of the present discounted value of the stream of future profits is greatest. Two factors determine their expected future profits. The first is the plant's expected future costs of production in a location, which is a function of the location's expected supply of inputs and the firm's production technology. The second factor is the present discounted value of the subsidy it receives at the site.

The BMW case provides a rare opportunity to observe the determinants of these two key site-selection factors. Consider first the county's expected supply of inputs. According to BMW, the characteristics that made Greenville-Spartanburg more attractive than the other 250 sites initially considered were: low union density, a supply of qualified workers; the numerous global firms, including 58 German companies, in the area; the high quality transportation infrastructure, including air, rail, highway, and port access; and access to key local services.

For our purposes, the important point to note here is that these county characteristics are a first potential source of unobserved heterogeneity. While these characteristics are well documented in the BMW case, they are generally unknown. If these characteristics also affect the outcomes of interest (i.e., labor earnings, property values and county finances), a standard regression that compares Greenville-Spartanburg with the other 3000 US counties will yield biased estimates of the effect of the plant opening. A standard regression will overestimate the effect of plant openings on outcomes, if, for example, counties that have more attractive characteristics (e.g., better transportation infrastructure) tend to have better outcomes (e.g., higher earnings).

Now, consider the second determinant of BMW's decision, the subsidy. The BMW "Million Dollar Plant" article explains why the Greenville-Spartanburg and South Carolina governments were willing to provide BMW with $115 million in subsidies.[11] According to local officials, the facility's estimated five-year economic impact on the region was $2 billion (although this number surely does not account for opportunity cost). As a part of this $2 billion, the plant was expected to create 2,000 direct and lead to another 2,000 in related industries by the late 1990s.[12] As we argued in subsection A, Greenville-Spartanburg's subsidy for BMW may be rationalized by the 2000 "spillover" jobs indirectly created by the new plant. As an example, Magna International began construction on an $80 million plant that was to produce roofs, side panels, doors and other major pieces for the BMW plant in 1993.[13]

It is notable that some counties may benefit more from a particular plant, depending on their industrial structure, labor force skills, unemployment rate and all the other factors that affect spillovers. For this reason, the factors that determine the total size of the spillover (and presumably the size of the subsidy) represent a <u>second</u> potential source of unobserved heterogeneity. If this unobserved heterogeneity is correlated with outcomes, standard regression equations will be misspecified due to omitted variables, just as described above. For example, if counties that have more to gain in terms of spillovers (and therefore offer more generous subsidies) also have better outcomes, then a regression that compares the winners with the other 3000 US counties will overestimate the effect of plant openings on outcomes.

In order to make valid inferences in the presence of these two forms of heterogeneity, knowledge of the exact form of the selection rule that determines plants' location decisions is generally necessary. As the BMW example demonstrates, the two factors that determine plant location decisions—the expected future supply of inputs in a county and the magnitude of the subsidy--are generally unknown to researchers and in the rare cases where they are known they are difficult to measure. In short, we have little faith in our ability to ascertain and measure all this information. Thus, the effect of a plant opening is very likely to be confounded by differences in factors that determine the plants' profitability at the chosen location.

As a solution to this identification problem, we rely on the revealed rankings of profit-maximizing firms to identify a valid counterfactual for what would have happened in the absence of the plant opening. In particular, the "Million Dollar Plants" articles typically report the county that the plant chose (i.e., the 'winner'), as well as its 2nd choice (i.e., the 'loser'). For example, in the BMW case, the loser is Omaha, Nebraska. In the subsequent analysis we assume that the winning and losing counties are identical in expected future profits, controlling for differences in pre-existing trends. Although this

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[11] Ben Haskew, chairman of the Spartanburg Chamber of Commerce, summarized the local view when he said, "The addition of the company will further elevate an already top-rated community for job growth" (Venable, 1992, p. 630).

[12] Interestingly, BMW later decided to open a second plant in the Greenville-Spartanburg area and relocated its U.S. headquarters from New Jersey to South Carolina.

[13] Although the Magna Plant was slated to hire 300 workers, state and local governments only provided about $1.5 million in incentives. Interestingly, the incentives offered to Magna are substantially smaller (even on a proportional basis) than those received by BMW, implying that local governments appear to be judicious in concentrating the incentives on plants that are likely to have the largest spillovers.

assumption is unlikely to hold exactly, we suspect that this pairwise approach is preferable to using regression adjustment to compare the winners to the other 3,000 U.S. counties or a matching procedure based on observable variables.[14] In Section V, we present empirical evidence that suggests that this identifying assumption may be valid.

II. Land Prices and Welfare When Counties Bids for Plants

This section presents a simple framework that guides the empirical analysis and helps to interpret the resulting estimates. One of the paper's empirical goals is to test whether the successful attraction of a new plant affects housing prices. The first subsection presents a stylized model that specifies some assumptions under which the change in land values induced by the exogenous opening of a plant can be interpreted as a change in residents' welfare. In the second subsection, we allow counties to bid for plants and specify some assumptions about the bidding process. The goal of this subsection is to show under what conditions a successful bid for a plant will result in higher or lower property values (and therefore welfare).

A. Land Prices and Welfare

Here, we follow the stylized Roback (1982) model, which is often used to model firm location decisions. We assume that individuals are perfectly mobile, have identical tastes and a fixed labor supply. Further, they rent land for homes in the county where they work. All firms are assumed to have constant returns to scale technologies and there is a fixed supply of land in each county.[15] In equilibrium, firm's unit costs equal the nationally determined product price and individuals' utility cannot be increased by moving to a different county.

Now, suppose that a county is exogenously assigned a new plant. This case may be unrealistic, but it is a useful starting point for expository purposes. Importantly, all other producers and all workers continue to choose their location to maximize profits and utility, respectively.

The opening of the new plant causes land values and nominal wages to increase in the county. The increase in land values occurs for two reasons. First, the new plant will directly increase the demand through its land purchase and by increasing the number of workers who need housing. Second, if there are agglomeration economies, the presence of the new plant will lower the costs of production for other firms in the county. In the parlance of Roback, this is referred to as a productive amenity. And, in the

[14] Propensity score matching is an alternative approach (Rosenbaum and Rubin 1983). Its principal shortcoming relative to our approach is its assumption that the treatment (i.e., winner status) is "ignorable" conditional on the observables. As it should be clear from the example, adjustment for observable variables through the propensity score is unlikely to be sufficient.

[15] See Hoehn, Berger, and Blomquist (1987) for a model that allows for flexible city boundaries.

presence of a productive amenity, firms will bid up the price of land to gain access to the spillovers. In order to retain workers, firms must pay higher wages to compensate them for the higher rental rate of land, so that real wages are unchanged. This is necessary because in equilibrium workers' utility must be constant across counties.

With this set-up, the increase in land values provides a one-time gain to property owners in the county that receives the new plant. This is the only change in welfare experienced by the county's residents because workers' utility is unchanged and the higher rental rate of land counterbalances any spillovers available to firms. Thus, under these assumptions, changes in land values translate one-to-one in changes in residents' welfare.

B. A Stylized Model of Bidding for Plants and Land Values

In the previous subsection, the opening of a new plant increased property values because we assumed that the county does not have to incur any costs to attract the plant. In practice, local governments frequently provide subsidies in exchange for a plant's location decision. In this subsection, and the remainder of the paper, we consider the possibility that the 3,000 U.S. counties compete for the new plant by offering subsidies or bids.

We assume that a county's residents elect a mayor that acts as their agent to bid to attract plants to its jurisdiction. A successful bid involves a trade-off for the county. On the one hand, subsidies are costly to the county, because they involve the provision of services and may reduce the future stream of tax revenues. The increase in public services includes the special services for the new plant stipulated by the incentive package (e.g., the construction of roads, or other infrastructure investments, tax abatements, job training funds, provision of low-cost or free land, the issuance of tax-exempt bonds, provision of cheap electric power, etc.), as well as the standard public services (e.g., garbage removal and police protection). We assume that the county's incentive package is financed by property taxes, so that its cost is capitalized into land values.[16]

On the other hand, the new plant directly increases the level of economic activity in the county, which raises the value of land. As the BMW case demonstrates, the presence of the new plant may also raise the value of land indirectly by generating spillovers, if, for example, it attracts other plants and/or lowers the costs of production for other plants. Thus, property values capitalize both the costs (i.e., the increased property taxes and/or reduced services) and benefits (i.e., the increased economic activity) of attracting the plant.

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[16] In the *Annual Survey of Governments* data, property taxes account for 49% of total revenues from own sources. For comparison, total sales and gross receipts taxes account for only 4.5% of total revenues.

This subsection's goal is to theoretically analyze when the successful attraction of a plant will increase or decrease property values (and therefore welfare) in the presence of county bidding. We denote property values as P, and assume that the change in the present discounted value of property values for the winning county can be expressed as $\Delta P_{ij} = V_{ij} - C_{ij}$. V_{ij} denotes the benefit of new plant j for county i, and it is equivalent to the increase in property values (in the absence of a subsidy). The size of this benefit is exogenous and known to the county. We also assume that it is known to all the other counties bidding on the plant. C_{ij} is the cost to the county of the subsidy provided to the plant.

In the real world, it is often the case that the state bears part of the cost of the incentive package. In this case the total subsidy received by the plant is $B_{ij} = C_{ij} + S_{ij}$, where S_{ij} denotes the state's contribution. We assume that S is exogenous to the county and is provided by the state to account for the benefits to other counties in the state.

The plants are the other side of this two-sided matching problem and we assume that they will locate in the county where their future profits are maximized. As described above, two factors determine their expected future profits in a given county: the subsidy and the expected future costs of production in that county. It is likely that there is heterogeneity in the maximum subsidy that counties are willing to offer, due to differences in counties characteristics (recall Section I-B). In order to obtain the highest subsidy, we assume that the firms conduct an English auction in the presence of independent, or private, values. We further assume that there is not any collusion in the bidding among counties.

The firm's choice also depends on the location-specific production costs. It is likely that there is heterogeneity in a plant's valuation of a county due to differences in the expected supply of future inputs. In the BMW case, recall these factors included the presence of qualified workers, the presence of German companies and air, rail, highway, and port access. We denote the value to the firm of all these factors as Z_{ij}. A higher Z_{ij} implies that production costs of firm j are lower in county i. We assume that Z is exogenous to the county and is known to all counties.

Overall, the total value for a firm of locating in a particular county is the sum of the subsidy and the county-specific cost advantages. We assume that a plant will select the county where this sum, $B_{ij} + Z_{ij}$, is maximized.

1. Homogeneous Counties. In general, counties differ in their valuations of a new plant, V_{ij}, and the plants' valuation of the county, Z_{ij}. Here, we begin by considering the case where counties are homogeneous in V and Z: $V_{ij} = V_0$ and $Z_{ij} = Z_0$ for all i. This would be the case if our key identifying assumption that winners and losers are identical were valid. The homogeneity case is important because in our empirical analysis, we will retain the homogeneity assumption when comparing winners and losers. Later, we analyze the more general model that allows for heterogeneity in V and Z to investigate how our conclusions differ if this identification assumption is not valid. Under homogeneity, the firm simply chooses the county that offers the highest subsidy, B. We consider four cases.

Case 1. This case is the simplest. We assume that the county's mayor maximizes residents' welfare and the state provides no subsidy (i.e., S = 0). The mayor raises the bid until she is indifferent between winning and losing. Formally, the equilibrium bid, B*, is determined by

(1) $B^* = V_0$

Consequently, $\Delta P = V_0 - B^* = 0$ and the successful attraction of the plant does not change land prices or residents' welfare.

Case 2. We now allow states to subsidize the incentives offered to the plant but retain the other assumptions. It is possible that different states provide different level of incentives if, for example, the magnitude of spillovers in neighboring counties differs across states. Here, the county that receives the most generous incentive from the state will win the new plant. The mayor of the county located in the most generous state does not need to raise the bid until she is indifferent between having the plant and not having the plant. She can win the plant by setting the county's bid at the point that makes the mayor of the county with the second most generous state subsidy indifferent between winning and losing. The optimal bid B* is

(2) $B^* = V_0 + S_{max-1}$

where S_{max-1} is the incentive provided by the second most generous state.

The state subsidy is effectively shared between the plant and the winning county. The portion of this rent that goes to the county is capitalized in land values. Specifically, land values increase by the difference between the state subsidy provided by the most and 2nd most generous states: $\Delta P = V_0 - (B^* - S_{max}) = S_{max} - S_{max-1} > 0$, where S_{max} is the incentive provided by the most generous state. Importantly, the source of the increase in housing values is the heterogeneity in the state subsidies and the county's capture of part of the state subsidy. It is not due to winning the plant.

Case 3. We now allow for the possibility that the mayor may have her own goals. In particular, we assume the mayor benefits from a higher incentive package because opportunities for graft or enlarging government are increasing in B. We define this personal benefit as T = f(B), with f'>0: the higher the subsidy provided by the mayor to the firm, the larger the kickback. Due to an exogenously determined probability of detection and punishment, the mayor chooses B to maximize her utility U, which depends on residents' increase in welfare ΔP and her benefit T: $U = U(\Delta P, T)$; where $U_1 > 0$ and $U_2 \geq 0$. In the case where $U_2 = 0$, there is no principal-agent problem and the mayor's and residents' interests are perfectly aligned. If $U_2 > 0$, the mayor's objective function includes residents' welfare as well as her own private gain from the subsidy. For simplicity, we assume that $U(\Delta P,T)$ is separable in its first and second argument and T is a fixed fraction γ of the bid (i.e., $T = \gamma B$), where $0 \leq \gamma \leq 1$. Thus, $U = \Delta P + \gamma B$.

We assume $U_2 \neq 0$ but reinstate the assumption that states do not subsidize the bid (i.e. S = 0). In this case, the mayor raises her bid to the point that makes her indifferent between having the plant and not having the plant. If all the mayors behave in the same way, the mayor's optimal bid is

(3) $B^* = V_0 / (1-\gamma)$

In this case, the mayor overbids, by choosing B* which is larger than the value of the plant to the residents. Such overbidding causes land values in the winning county to decline relative to the losing county. The magnitude of the decline depends on the mayor's weight on her own welfare: $\Delta P = - (\gamma / (1-\gamma)) V_0 \leq 0$.

Case 4. When $U_2 \neq 0$ and there is a positive state subsidy (i.e., $S_{ij}>0$), the change in land prices cannot be signed. The state subsidy increases land values, as shown above in case 2, but the inclusion of the mayor's personal gain in the objective function results in a decrease in land values as in Case 3. Depending on the magnitude of these two effects, land values may increase or decrease in the winner relative to the loser.[17]

2. Heterogeneous Counties. Although the key assumption in our empirical analysis is that the winning and losing counties are homogeneous, it is important to understand the consequences if this assumption is not valid. Here, we allow for heterogeneity in counties' valuations of attracting the plant, V, and plants' valuations of counties, Z. In section I-B, we have used the BMW example to argue that these two sources of unobserved heterogeneity would confound the effects of the plant opening with the differences across counties. Specifically in that section, we argued that a naive estimator that ignores the presence of unobserved heterogeneity would probably be biased. Here we make the same point more formally. This subsection underscores the importance of our identifying assumption.

For simplicity, we retain the assumptions that $S_{ij} = 0$ and $U_2 = 0$. If V and Z vary across counties, the firm chooses where to locate based not only on the bid B, but also on Z. Specifically, the value for firm j of choosing county i is $B_{ij}+Z_{ij}$. Assume for simplicity that there are only two levels of V, high V (V_H) and low V (V_L); and two levels of Z, high Z (Z_H) and low Z, (Z_L). Consider first the case where V and Z are positively correlated, so that the county with high V also has high Z. Thus, one county will gain the most from attracting firm j and is also the least cost production location for firm j. Its optimal bid is such that the county with low Z and low V is indifferent between having the plant and not having the plant:

(4) $B^* = V_L - (Z_H - Z_L)$.

In this case, the county that is the best match enjoys a rent that is capitalized into land values. Land values increase by an amount proportional to the difference in V and the difference in Z: $\Delta P = (V_H -$

$V_L) + (Z_H - Z_L) > 0$. Importantly, the increase in land values is due to the underlying differences in V and Z, not the presence of the new plant.

Consider now the case where V and Z are negatively correlated. County 1 has high V and low Z; while county 2 has high Z and low V. If $V_H + Z_L > V_L + Z_H$, county 1 wins the plant by bidding an amount B* that makes county 2 indifferent between having the plant and not having the plant:

(5) $B^* = V_L + (Z_H - Z_L)$

The winner county enjoys a rent that is capitalized in land values, although the rent is lower than the rent in the case of positive correlation between Z and V: $\Delta P = (V_H - V_L) - (Z_H - Z_L) > 0$. A similar conclusion applies if $V_H + Z_L < V_L + Z_H$. In this case county 2 is the winner and its land prices increase by $\Delta P = (Z_H - Z_L) - (V_H - V_L) > 0$. Again, the heterogeneity is the source of the change in prices.[18] The implications for our empirical analysis are discussed further in the econometrics section.

III. Data Sources and Summary Statistics

A. Data Sources

We implement the design using data on winning and losing counties. Each issue of the corporate real estate journal *Site Selection* includes an article titled the "Million Dollar Plants" that describes how a large plant decided where to locate.[19] These articles always report the county that the plant chose (i.e., the 'winner'), and usually report the runner-up county or counties (i.e., the "losers").[20] As the BMW case study indicated, the winner and losers are usually chosen from an initial sample of "semi-finalist" sites that in many cases number more than a hundred.[21] The articles tend to focus on large plants, and our impression is that they provide a representative sample of all new large plant openings in the US. The articles usually indicate the plant's output, which we use to assign the plant to the relevant 1-digit industry.

These data have two important limitations. First, the magnitude of the subsidy offered by the winning counties is in many cases unobserved and the bid is almost always unobserved for losing counties. This is unfortunate, because an interesting check of the validity of our research design would be

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[17] In this case, the optimal bid is $B^* = (V_0 + S_{max-1})/(1-\gamma)$. The change in land prices is $\Delta P = -(\gamma/(1-\gamma))V_0 + S_{max} - (1/(1-\gamma))S_{max-1}$.

[18] In the unlikely case that $V_H + Z_L = V_L + Z_H$, the two counties are equivalent, and the winner is randomly determined.

[19] In 1985, the journal *Industrial Development* changed its name to *Site Selection*. Henceforth, we refer to it as *Site Selection*. Also, in some years the feature "Million Dollar Plants" was titled "Location Reports."

[20] In some instances the "Million Dollar Plants" articles do not identify the runner-up county. For these cases, we did a Lexis/Nexis search for other articles discussing the plant opening and in 4 cases were able to identify the losing counties. The Lexis/Nexis searches were also used to identify the plant's industry when this was unavailable in *Site Selection*.

[21] The names of the semi-finalists are rarely reported.

to test whether the subsidies offered are equal in the winning and losing counties. Second, in many cases the articles do not report the expected size of the plant.

In order to conduct the analysis, we collected the most detailed and comprehensive county-level data available on employment, government finances, and property values available for the period from 1970 through 1999. The employment data comes from the Census Bureau's *County Business Patterns* (*CBP*) data file. These annual data report the number of employees and the total wage bill at the county by industry level. In order to protect the confidentiality of the respondents, these data are "zeroed out" for many industry by county cells. Consequently, we conduct our analysis at the 1-digit industry by county level.[22] The *CBP* data are used to test whether a plant opening is associated with changes in wage bill trends in its industry, as well as other industries. We focus on the total wage bill rather than total employment since the latter cannot detect changes in the skill content of labor. Unfortunately, the *CBP* does not report hourly wages.

We also test whether the successful attraction of a plant affects property values. We are unaware of any existing electronic files of annual county-level property value data, so we created our own county by year data file on property values from two sources. First, we contacted all the state and county governments in our winner and loser samples directly and requested all their historical data on property values.[23] These data exist because governments determine the value of property in their jurisdictions for the purpose of assessing property taxes. Second, we supplemented these data by hand entering data from the 1972, 1977, 1982, 1987, and 1992 *Census of Governments, Volume 2 Taxable Property Values and Assessment-Sales Price Ratios*. The censuses reports market values in the year before each census was conducted. In years where data was unavailable from both sources, we estimated county-level property values by linearly interpolating the Census of Government data, which likely causes the true variation to be understated. The property value data are missing for some counties.

Our preferred property value sample is comprised of 30 of the 82 winners and 62 of the 129 losers. One important limitation is that it was not possible to obtain separate measures of the value of land, excluding the structures, for our entire sample. The Data Appendix provides a fuller discussion of these data.

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[22] For the purposes of the analysis, we divide output into the 5 broad "1-digit" industries defined by the *CBP* for which uncensored wage bill data are available in most years. These industries are: Manufacturing; Transportation and Public Utilities; Trade (Wholesale plus Retail); Finance, Insurance, and Real Estate; Services. At this level of aggregation, 17% of the cells are "zeroed" out.

[23] We attempted to get this data for the primary sample of 166 counties dating back to the early 1970s. We collected at least 1 year of property value data from 153 counties. In general, these governments did not have data from the earlier years. For example, we have nonmissing property value date for 68 counties in 1977, 102 in 1980, 149 in 1990, and 153 in 1998.

The *Annual Survey of Governments: Finance Statistics Series* (*ASG*) is used to determine the fiscal consequences for local governments of new plant openings. The *ASG* is an annual survey of governments that asks detailed questions on governments' expenditures by function (e.g., education, administration, and public assistance) and type (i.e., intergovernmental transactions, current operations, and capital outlays). The data also contains information on revenues by source, indebtedness, and cash and securities holdings. We aggregate these data to the county level. This aggregation is done on the sample of governmental units that are surveyed continuously from 1970 through 1999 so that the units are held fixed. In our "Million Dollar Plant" sample of 166 winning and losing counties, 150 counties have at least one governmental unit that reports continuously. The continuous reporters comprise only 12.5% of all governmental units in these counties but account for approximately 75% of revenues and expenditures. This is because large government units are sampled with certainty, while smaller units are sampled with "varying probabilities within an area, type of government, and size ordering" (Census Bureau, 1990, p. 1-1).[24]

B. Summary Statistics

Table 1 presents summary statistics on the sample of plant location decisions that form the basis of the analysis. The first panel indicates that in our primary sample there are 82 separate plant openings and an average of 1.6 losers per winner or a total of 129 losers. There are 166 counties in this sample, so the average county appears roughly 1.3 times.[25] The second panel reports the distribution of the number of losers per winner. We refer to the winner and accompanying loser(s) associated with each plant opening announcement as a "case." In 57 of the 82 cases there is a single loser and in 14 there are 2 losers. The table also reveals that 63 of the 82 plants were in the manufacturing industry. Thus, our analysis is most informative about the consequences of attracting industrial plants. The final panel lists the distribution of the year of the announcement about the plant opening. 22 of the plant openings were announced in 1991 and 1992, suggesting that they may be countercyclical. Appendix Table 2 reports the identity of each plant, its industry, and the winning and losing counties.

Although we do not know the determinants of the counties' valuations of plants (i.e., V_{ij}) or the plants' valuations of counties (i.e., Z_{ij}), Table 2 presents the means across counties of some likely determinants of these variables in the three years before the announcement of the plant opening. These

[24] According to the data documentation, the following governments are sampled with certainty: "all county governments with a population of 50,000 or more, municipal governments with a population of 25,000 or more; township government in the New England and Middle Atlantic states with a population of 25,000 or more; school districts with an enrollment of 5,000 or more; and special districts with long-term debt outstanding of $10 million, or total revenue or expenditure of $5 million."

means are reported for winners, losers, and the entire U.S. in columns (1), (2), and (4), respectively.[26] Column (3) presents the t-statistic and p-value from the test that the entries in (1) and (2) are equal. We expect that winning and losing counties are similar so that any differences in columns (1) and (2) will be small. In contrast, we expect that a comparison of the winners (or losers) with all U.S. counties is likely to produce larger differences.

The first two panels reveal that the winning and losing counties are similar in both the levels and pre-announcement trends of the primary outcome variables. Specifically, the first panel reports the means of three outcome variables in the three years before the announcement of the plant opening. The mean total wage bill in the new plant's industry is approximately $1,127 million in winning counties, compared to $1,145 million in losing counties. This difference is not statistically meaningful and indicates that there were similar levels of activity in the two sets of counties. The count of full-time employees leads to the same conclusion. In contrast, the corresponding figures for all the US counties are much smaller.[27] The third row of this panel indicates that the hypothesis of equivalent aggregate property values across winning and losing counties cannot be rejected at conventional significance levels.

The second panel compares trends in the outcome variables in the three years before the announcement. The growth rates in earnings, employment and land values are not statistically different in winning and losing counties. We return to this issue in Section V, where we graphically show that the trends in wage bill, employment and land values in the 8 years before the announcement are similar in winning and losing counties. For now, we note that the finding that both the level and growth rate of property values is similar in winning and losing counties is an especially important test of the validity of our research design, if property markets are forward looking. Under the plausible assumption that property markets are forward looking, this finding indicates that the expected <u>future</u> changes in the level of economic activity capitalized into property values are also ex-ante comparable. Overall, the first two findings provide reassuring evidence on the quality of this research design

The third panel reports mean levels and changes of socioeconomic and demographic variables. In general, the null hypothesis of equal means in winning and losing counties cannot be rejected with conventional criteria. This is true both for the levels of the variables in the 3 years preceding the plant

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[25] 127 counties appear once in the data. 33 counties appear twice and 6 counties are present three times. Through the entire empirical analysis, we exclude 7 counties whose population is larger than 2 million, because the impact of a plant opening would be difficult to detect is such large counties.

[26] The losing county entries in column 2 are calculated in the following manner. First, we calculate the mean across all the losers for a given case. Second, we calculate the overall loser average as the unweighted mean across all cases so that each case is given equal weight.

[27] The figures in the top panel of column 4 are a weighted average for years 1982 to 1993, with weights proportional to the number of Million Dollar cases in each year and industry. The figures in panels 2 to 5 of column 4 are a weighted average for years 1982 to 1993, with weights proportional to the number of Million Dollar cases in each year (see bottom of Table 1 for the distribution of cases across years).

opening announcement and the percent growth in those years for selected variables. There are two important exceptions where the p-value is .05 or less: level of per capita income and fraction of college graduates. However, for these two variables, the losers' mean is closer to the winners' mean than is the mean across all U.S. counties in column (4). Furthermore, for per capita income, the percent growth is not statistically different in winners and losers counties. Interestingly, there are important differences between all U.S. counties and the winning counties. In particular, the winners are richer, have a substantially larger population, a higher employment-population ratio, a better educated population, and fewer people over the age of 65.

The fourth and fifth panels show the geographic distribution of plants across the four regions of the U.S. and the industry distribution of the labor force within the categories. Here, the means across the winning and losing counties are not well balanced. For example, 66% of the winners are in the South compared to 39% of the losers. This is potentially problematic, since it suggests that there may be unobserved differences (e.g., union density) across the winning and losing counties. This finding underscores the value of our identification strategy's reliance on comparisons of changes in outcomes between winners and losers, rather than cross sectional comparisons. In this setting cross-sectional differences will only bias the results if levels of these variables determine future changes. This would be the case if, for example, union density predicts growth in employment and property values. As the next section discusses, we estimate models that include region by year fixed effects to account for the uneven distribution of winner and losers across regions. To preview the results, our findings are unaffected by the inclusion of these controls, implying that the geographic imbalance is not an important issue here.

IV. Econometric Model

In light of the firm's selection rule, the goal is to estimate the causal effect of winning a plant on county-level outcomes. This section discusses the 2-step econometric model used to estimate this effect. In the first step, we fit the following equation:

$$(6) \qquad Y_{ijct} = \alpha_{ic} + \sum_{\tau=-19}^{17} \pi_{W\tau}\, W_{ijc\tau} + \sum_{\tau=-19}^{17} \pi_{L\tau}\, L_{ijc\tau} + \mu_{it} + \eta_{ij\tau} + \xi_{ijct}, \text{ or}$$

$$(6') \qquad Y_{jct} = \alpha_{c} + \sum_{\tau=-19}^{17} \pi_{W\tau}\, W_{jc\tau} + \sum_{\tau=-19}^{17} \pi_{L\tau}\, L_{jc\tau} + \mu_{t} + \eta_{j\tau} + \xi_{jct},$$

where i references industry, j indicates a case, c denotes county, and t indexes year. τ also denotes year, but it is normalized so that for each case the year the plant opening is announced is $\tau = 0$.[28] The outcome

[28] The date when the plant begins production is unknown, so we use the year of the announcement of the winner county as $\tau = 0$. In most cases, the construction of the new plant starts immediately after the announcement.

variable in equation (6), Y_{ijct}, is total wages. The outcome variable in (6'), Y_{jct}, is a county-level measure of property values or a government finance variable. ξ_{ijct} and ξ_{jct} are the respective stochastic error terms.

α_{ic} (α_c) is a full set of industry x county (county) fixed effects that adjust for permanent differences in the intercept of the outcome variables. These account for all fixed county characteristics. μ_{it} (μ_t) is a vector of indicators that nonparametrically controls for industry x year (year) effects. In some specifications the sample includes the entire U.S., while in others we restrict it to our "Million Dollar Plant" sample of 166 counties. The use of the smaller sample is equivalent to imposing the restriction that the industry x year (year) effects are the same in the 166 counties and the remainder of the country. $\eta_{ij\tau}$ ($\eta_{j\tau}$) is a set of separate fixed effects for each of the cases interacted with an indicator for whether $\tau >= -8$ and $\tau <= 5$. We restrict attention to these values of τ, because the sample is balanced over this range.

$W_{ijc\tau}$ and $W_{jc\tau}$ are indicator variables. $W_{ijc\tau}$ ($W_{jc\tau}$) equals 1 for observations on the new plant's industry in winning counties (the new plant's county) for a given value of τ. $L_{ijc\tau}$ and $L_{jc\tau}$ are defined analogously for losing counties. In the cases with multiple losers, this indicator variable will equal 1 for observations from multiple counties within a case.

The vectors π_W and π_L are the parameters of interest in these equations. They measure the period-specific means of the dependent variables in winning and losing counties, respectively, where the means are conditional on all the indicator variables described above. The period is determined by the years since (or until) the plant opening announcement. Thus, the effect of winner or loser status is allowed to vary with τ. For example, $\pi_{W\tau}$ when $\tau = 3$ is the conditional mean of the outcome in winning counties 3 years after the announcement and $\pi_{L\tau}$ when $\tau = -3$ is the conditional mean in the losing counties 3 years before the announcement.

A few details about the identification of the π's bear highlighting. First, and most importantly, the case fixed effects (i.e., $\eta_{ij\tau}$ and $\eta_{j\tau}$) guarantee that the π's are identified from comparisons <u>within a winner-loser pair</u> for $\tau >= -8$ and $\tau <= 5$ and are a way to retain the intuitive appeal of pairwise differencing in a regression framework. Second, it is possible to separately identify the π's and the industry x year (year) effects because the plant opening announcements occurred in multiple years. Third, some counties are winner and/or losers multiple times and any observation from these counties will simultaneously identify multiple π's. Fourth, the specification does not control for time-varying covariates, such as the variables listed in Table 2. This is because many, if not all, of these variables (e.g., per capita income and population) may be affected by the plant opening so are likely endogenous.

Table 2 suggested that there might be important unmeasured region-specific determinants of the outcome variables (e.g., differences in union density). Since the analysis is at the industry x county level in (6) and county level in (6'), it is possible to include region x year fixed effects in order to nonparametrically adjust for all unobservables that vary across regions over time. As a test of robustness, we present results from specifications that include these fixed effects.

The 2[nd] step provides a method to summarize the π's to infer the effect of attracting a plant. To implement this step, we stack the vectors π_W and π_L for $-8 <= \tau <= 5$ into a 28 x 1 vector. This vector is the dependent variable in the following equation:

$$(7) \qquad \pi_{s\tau} = \theta + \delta\, 1(\text{Winner}) + \psi\, \text{trend} + \lambda\, (\text{trend} * 1(\text{Winner})) + \gamma\, (\text{trend} * 1(\tau >= 0)) + \beta\, (\text{trend} * 1(\text{Winner}) * 1(\tau >= 0)) + \upsilon_{S\tau},$$

where s indexes winner status and τ remains the year relative to the plant opening announcement. This equation allows for a differential intercept for the winner parameters. It also includes a common time trend, ψ. The parameter λ measures whether the time trend differs for winners, while γ captures whether the trend differs after the announcement of the plant opening (i.e., when $\tau >= 0$).[29] [30]

β is the parameter of interest. It measures the difference in the time trend specific to winners (relative to losers) after the announcement of the plant's opening (relative to before the announcement). Formally, the consistency of this parameter requires the assumption that cov[(trend * 1(Winner) * 1(τ >= 0)), $\upsilon_{W\tau}$] = 0 be valid. Recall from Section II-B that we assume that plants choose a site based on the sum of B_{ij} and Z_{ij}. It is likely that these same unobserved variables predict the outcome variables in (6) and (6'). Therefore, the identifying assumption is that the industry by county fixed effects, industry by year fixed effects, case fixed effects, region by year fixed effects and the detrending by winner status and $\tau >= 0$ fully condition out B_{ij} and Z_{ij}.

It is interesting to note that if this assumption is not valid, the bias cannot be signed a priori. This is because the sign of the bias depends on whether $\text{cov}(B_{ij},\upsilon_{W\tau}) + \text{cov}(Z_{ij},\upsilon_{W\tau})$ is < 0 or > 0. One can think of cases where the sum is positive and cases where it is negative. For example, it is possible that $\text{cov}(B_{ij},\upsilon_{W\tau}) < 0$ if counties with "bad" outcomes (i.e., lower wage bill or lower land prices) are the ones that have the most to gain by attracting a new plant. At the same time, it is plausible that $\text{cov}(Z_{ij},\upsilon_{W\tau}) > 0$,

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[29] We include the winner fixed effect in (7), because (6) and (6') include all periods (i.e., from $-19 <= \tau <= 17$), while the second step only uses observations where $-8 <= \tau <= 5$. For this reason, the winner fixed effect in (7) is not collinear with the county by industry (county) fixed effects (6) and (6'). We also experimented with models that include the interaction of winner status and a dummy equal to one if $\tau >= 0$ to allow for a mean shift in addition to the trend break. The findings from this approach are qualitatively similar.

[30] Since the estimated π's for winners (losers) are obtained from a balanced panel, their standard errors are virtually identical. Consequently, the fact that the π's are estimated is unlikely to be a source of heteroskedasticity in the estimation of equation (7). Regardless, the subsequent results are insensitive to weighting by the square root of the inverse of the standard errors of the π's from equation (6).

since counties with characteristics that firms find desirable may also have "good" outcomes (i.e., higher wage bill or higher land prices).

V. Results

This section is divided into three subsections. The first reports the estimates of winner status on workers' earnings within the winning county and its surrounding counties. Separate results are reported for the new plant's 1-digit industry and for all other industries. The second subsection reports the association between winner status and property values. Under some assumptions, property values may provide a measure of overall welfare (Section II). The third subsection examines the association between winner status and a series of local government revenue and expenditures categories.

A. Employment Outcomes

Columns (1) and (2) of Table 3 report the estimated π_W's and π_L's and their standard errors from the fitting of equation (6) on total wage bill data from the *CBP* for all counties from 1970-1998. Recall, τ is normalized so that $\tau = 0$ is the year that the plant location decision is announced. Each row reports the estimated π_W and π_L for a given value of τ. Consequently, the point estimates are annual measures of the total wage bill in millions of dollars by winner/loser status in the 1-digit industry of the new plant for 5 => τ >= -8. These estimates are conditioned on county by industry, industry by time, and case fixed effects. Column (3) reports the difference between the estimates of π_W and π_L within each row.

The top panel of Figure 1 separately plots the estimates of π_W and π_L against τ. In order to get the lines on the same scale, the winner's line is shifted up so that the difference between the lines is 0 when $\tau = -1$. This same normalization is used in all the subsequent figures. The bottom panel of Figure 1 plots the difference in the estimated π_W and π_L against τ and is a graphical version of column (3) of Table 3. From these graphs, it is evident that in the 1-digit industry of the new plant, the winning and losing counties have almost identical trends from $\tau = -8$ through $\tau = -1$. A statistical test confirms that the trends in the wage bill in the winning and losing counties are statistically indistinguishable in the 8 years before the plant opening announcement (e.g., the t-statistic associated with the test of equal trends is 0.37.) Importantly, this is consistent with our identifying assumption that losing counties provide a valid counterfactual.

However beginning with the year that the plant opening is announced, there is a sharp increase in the trend in the wage bill in the 1-digit industry of the new plant in the winning counties. In contrast, the losing counties' trend is largely unchanged. The figure also demonstrates that it is appropriate to model the effect of the plant opening announcement with a trend-break, rather than the more typical difference in differences mean shift.

Table 4 reports estimates of β and its standard error that result from fitting equation (7). In column (1) (a), the π_W's and π_L's used to obtain these estimates are those that were presented in Table 3 and the top panel of Figure 1. In column (1) (b) region by year fixed effects are included in equation (6), so the π_W's and π_L's are adjusted for all time varying region-specific factors. The results in this column are intended as a robustness check and throughout the remainder of this table, as well as Tables 5 and 6, (a) and (b) refer to the estimation of equation (6), or (6'), with and without region by year fixed effects, respectively.[31]

The column (1) parameter estimates suggest that in the new plant's 1-digit industry, the wage bill increased by a statistically significant $16.7-16.8 million per year in winning counties (relative to losing ones) after the announcement of the plant's opening (relative to the period before the announcement). This is 1.5% of the average wage bill in winning counties in the year $\tau = -1$.[32] Taken literally, this implies that by the end of the period (i.e., $\tau = 5$) the wage bill in the new plant's 1-digit industry is roughly $100 million (9%) higher due to the plant opening than predicted by pre-existing trends. Interestingly, the point estimate is unchanged by the inclusion of the region by year fixed effects.[33]

Column (2) reports the results when the estimated π's are obtained from fitting equation (6) on the restricted sample of 166 winning and losing counties (rather than the entire U.S.). As before, the π's are used to estimate β from equation (7). The estimates in (a) and (b) suggests that the trend in the wage bill increased by $13.2 and $12.6 million, respectively. In the context of the standard errors, these estimates are essentially identical to those in column (1). Figure 2 presents the column (2) (a) results graphically.[34]

Column (3) reports a "naïve" estimator that is obtained by using a standard regression to test for a trend break in the winners after the announcement relative to before, controlling for observable characteristics. It is labeled "naïve" because the losers are not used as a counterfactual, so it is not conditioned on case fixed effects. This is the estimator that would typically be used in the absence of the

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[31] In all the figures, specification (a), which does not include region by year fixed effects, is used to obtain the π_W's and π_L's.

[32] In the column (1) (a) specification when the dependent variable is the ln of the total wage bill, the parameter estimate and standard error are 0.0079 (0.0037).

[33] When the 1st step standard errors are clustered by county and year, and the 2nd step is weighted by the 1st step standard errors, the results are virtually identical. For example, the β coefficient (standard error) from the column (1) (a) specification is 16.6 (5.3).

[34] We also fit models where the estimated π's are obtained from a sample that is restricted to only include observations on each new plant's industry from the winning and losing counties for each case so the observations from other industries in these counties are excluded. The estimated 1st step models are based on equation (6'). The β coefficient (standard error) with and without the region by year effects are 19.7 (7.1) and 15.9 (7.9), respectively.

Million Dollar Plant research design. Interestingly, the point estimates here are roughly 50% larger than those in column (1) and almost double the column (2) estimates. [35] This highlights the value of our research design.

Column (1) of Table 5 and Figure 3 test whether the announcement of the new plant opening is associated with changes in employment in other industries in the winning county. The point estimates suggest that the trend in the wage bill increased by \$42.0-32.1 million, which is approximately 0.9-1.1% of the τ = -1 level. However, these estimates are imprecise, and would not be judged statistically significant by conventional criteria.

The remainder of Table 5 and Figures 4 and 5 examines whether the successful attraction of the new plant affects the trend in the wage bill in counties that neighbor the winner. Here, a neighbor is defined as a county that physically abuts a county or is connected by a bridge or boat. We compare the neighbors of winners to the neighbors of losers. In Figure 4, there appears to be a trend break in the total wage bill in the same 1-digit as the new plant. The estimates in column 2 confirm this. They suggest that the post-announcement trend increased by \$39.1-40.4 million, or approximately 1.2%. This estimate borders on statistical significance as judged by conventional criteria. Figure 5 plots the results for all other 1-digit industries from the (a) specification. The bottom panel reveals that the relative downward trend before the announcement of the plant opening in winning counties is halted, but the data are noisy. This is reflected in the large point estimate (\$33.8-72.8 million) and standard errors in column (3) of Table 5.

One interpretation of the column (2) and (3) results from Table 5 is that the gains in the winning counties are not the result of losses in their neighbors. That is, the effect of the plant opening announcement is not zero sum locally.[36] However, the precision of the estimates for "other industries" should temper any conclusions about the effect outside the new plant's industry.[37]

As mentioned above, we focus on the total wage bill rather than total employment since the latter cannot detect changes in the skill content of labor. However, the employment results are generally similar to the wage bill ones. For example, Appendix Figure 1 presents graphs that are analogous to Figure 2 for total employment, rather than the total wage bill.[38]

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[35] Estimates are based on data from the entire U.S. as in column (1).

[36] We also explored whether there are effects on the wage bill at the state level. The point estimates are large and positive but the standard errors are so large that they have little empirical content.

[37] As a test of robustness, we included a full set of state by year fixed effects in equation (6) in order to control for all time-varying state factors. In this case, the estimated trend breaks are generally of a larger magnitude across all the dependent variables in Tables 4 and 5.

[38] The corresponding coefficient from the employment regression on the sample of Million Dollar Plant counties is 467.3 (176.2). The coefficient from a regression that includes all U.S. counties is 287.8 (192.0).

B. Property Values

Here, we assess whether winner status is associated with changes in property values. In a model with rational, perfect-foresight agents, the net effect of winning the plant should be capitalized into property values immediately after the announcement. In this case, it would be appropriate to test for an immediate mean shift in property values. As the Data Appendix describes, property values are only reappraised every few years in many states, so that changes in property values will emerges only gradually in our sample. Consequently, we continue to use the trend break model specified in equation (7).

The property value results are presented in Table 6 and Figure 6. In columns (1) and (2) of Table 6, the sample is limited to the "Million Dollar Plant" counties with nonmissing property value data for $-8 <= \tau <= 5$. This sample includes 32 of the 82 winners and 69 of the 129 losers. Column (3) adds the sample restriction that all observations from California are dropped, which reduces the sample by 2 winners and 7 losers. This restriction is imposed, because Proposition 13 restricts annual increases in property values in California to 2% annually.[39] This is the preferred sample.

In column (1) of Table 6, the point estimates imply that the trend in the property values increased by $103-140 million in winning counties (relative to losing ones) after the announcement of the plant's opening (relative to the period before the announcement). These estimates would not be judged to be statistically different than zero by conventional criteria. Nevertheless, this is approximately 0.6-0.8% of the average property value in winning counties at $\tau = -1$. The column (2) specifications add California by year fixed effects to (6') in order to account for the effect of Proposition 13. These estimates are modestly larger than those in column (1) but overall indicate a similar conclusion.

Figure 6 is based on the preferred sample that excludes California counties. This figure shows that the two sets of counties had remarkably similar trends in aggregate property values prior to the announcement of the plant opening. This result is similar to the findings for the wage bill in Figure 1 and 2, and supports the validity of our identifying assumption. Figure 6 also reveals a dramatic upward trend break in property values in winners that coincides with the announcement of the plant opening.

The corresponding point estimates in column (3) of Table 6 indicate that the trend break in winning counties ranges from $176-278 million. Conventional criteria would judge these estimates to be statistically different from zero. They are approximately 1.1-1.7% of the $\tau = -1$ property values in winning counties. This suggests that by the end of the examined period (i.e., 5 years after the announcement), property values in winning counties had increased by 5.5-8.5%, relative to losing counties. To put this in

[39] See the Data Appendix for a further discussion of the difficulties with California data.

context, the mean five-year change in property values in the years before the opening announcement is 47.7%.[40]

Taken together, the estimates in Table 6 fail to provide evidence that the successful attraction of a big plant reduces property values.[41] These findings are broadly consistent with previous research that estimates the effect of increased economic activity on land values.[42]

C. Local Government Finance Outcomes

A popular concern about the provision of these subsidies is that local governments fund them with cuts in important services, such as education and police protection. Table 7 empirically explores this issue. For a series of local government finance variables, column (1) reports the estimated β and standard error from equation (7). In this table, the π's that are used as the dependent variables in (7) are obtained from the estimation of equation (6') on the sample of 150 "Million Dollar Plant" counties that have at least one governmental unit reporting consecutively from 1970 through 1999.[43] Column (2) reports the mean of the budget variables in winning counties at $\tau = -1$ and column (3) lists the ratio of the point estimate in column (1) to this mean.

The table is divided into separate panels for expenditures, revenues, and indebtedness and within these broad categories separate results for subcategories are reported. In viewing these results, it is important to bear in mind that according to the *ASG* data the trend in population increased by approximately 0.8% in winning counties after the announcement. Thus, these counties were gaining residents**.** The Table reports the effects in absolute terms. It is straightforward to obtain per-capita effects by subtracting 0.008 from the entries in column 3.

The first panel reports the expenditure results. The "Total" row suggests that the trend in total expenditures increased by \$17.2 million or 1.8% of the $\tau = -1$ mean. The category can be divided into current expenditures, capital outlays, and payments to other governmental units. In percentage terms, the increase was roughly equivalent in the current and capital categories.

The "selected sub-categories" results reveal where the extra money was targeted. The trend break in capital education expenditures was 4.8% of the $\tau = -1$ mean and this accounted for roughly 55% of the increase in capital expenditures. This finding and the current education expenditure results suggests that spending per pupil may have increased, although this calculation is speculative and requires a number of

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[40] The point estimate (standard error) on the 1-digit wage bill is 15.0 (13.6) for this sample with specification (a).

[41] We also tried to estimate (6') with a full set of state by year fixed effects. This specification is excessively demanding of the data, because the property value data contain observations from 29 states and 92 counties, but only 17 of these states have multiple counties. The resulting estimates were so imprecise that they lacked meaning.

[42] See Chapter 5 of Bartik (1991) for a review. Also see Dye and Merriman (2000) for the effects of tax incremental financing schemes that fund economic development projects on property values.

[43] The region by year fixed effects are not included in this version of (6').

assumptions to be valid.[44] Interestingly, the trend break in housing and community development expenditures was larger than the overall trend break in expenditures. The increase in spending on publicly operated utilities is consistent with the expansion of the manufacturing sector. Notably, the results suggest that spending on police protection was unchanged but on a per capita basis it declined modestly.

The second panel reveals that the trend in revenues increased by $18.5 million. Approximately 1/3 of this increase was due to higher transfers from state governments, underscoring that states are an important source for the subsidies used to attract new plants. The results also reveal trend breaks in local tax revenues and utility charges of roughly $4 million. The larger trend break on utility expenditures compared to the charges trend break implies that the new plants may receive discounts on utility fees.

A comparison of the first and second panels reveals that the trend break in expenditures is roughly equal to the trend break in revenues. This is to be expected since local governments are generally required to balance their budgets annually. It is notable though that the balance was not achieved through cutbacks in valuable public services.

In light of this finding, it is initially surprising that the trend in long-term debt increased by $16.3 million. The final entry in the table demonstrates that this is largely explained by the $12.8 million trend break in nonguaranteed debt, which is a form of debt that is not guaranteed by the full faith and credit of the government. This type of debt is generally issued under the authority of a government body to gain tax-exempt status, but it is used for private purposes and the government generally does not have any obligation to repay it.[45] It seems sensible to conclude that this is one of the types of incentives given to the new plants.

VI. Interpretation

The results suggest that the successful attraction of a 'Million Dollar' plant is on average associated with an increase in property values. Under the assumptions detailed in Section II, this increase in property values may be interpreted as an increase in welfare for residents of the winner county.

There are at least three different explanations for this result. First if the identifying assumption that winning and losing counties are ex-ante homogeneous is valid and the model in Section II is correct, then the results indicate that the effect of state subsidies dominates any malfeasance by politicians. The implication is that the increase in winners' property values is due to the transfer of resources from states to winning counties.

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[44] For example, the *ASG* population variable is a measure of all residents. It is not possible to separately determine the number of students in public schools.

[45] According to the *ASG* manual, this type of bond has been used to fund industrial and commercial development, pollution control, private hospital facilities, sports stadiums, and shopping malls (Census 1999, Section 9.3).

It is also possible that the new plant affects property values in the neighbors of winners (recall the wage bill results in columns (2) and (3) of Table 5) and/or the entire state. In this case, the state subsidies may be justified from an efficiency standpoint. Unfortunately, this possibility cannot be tested directly, because property value data on the neighbors of winners and entire states is unavailable. If this possibility has no empirical basis, then we speculate that winning counties gain at the expense of the rest of the state.

A second explanation is that both winning and losing counties systematically bid less than the benefit of attracting these plants. In this case, the counties do not compete away all the rents and this causes property values to increase in winning counties, relative to losing ones. The unappealing feature of this explanation is that there is not a readily apparent friction that would explain such systematic mistakes.

A third explanation is that winning and losing counties are not ex-ante homogeneous, which would invalidate our identifying assumption. In this case the findings may reflect the combined effect of unobserved heterogeneity in counties' valuations of plants and plants' valuations of counties (the terms V and Z in Section II), rather than the effect of the plant opening. However, the similarity of the pre-announcement levels and trends in the wage bill, property values, and observable characteristics of winning and losing counties, as well as the robustness of the results to the inclusion of region-year effects, suggests that our research design is valid. The similarity in the trends in property values in the 8 years before the announcement is especially reassuring. This is because under the plausible assumption that property markets capitalize expected future economic activity, this finding indicates that there were not differences in expectations about future economic outcomes in winning and losing counties.

Another central issue of interpretation is whether there is a connection between property values and residents' welfare. As Section II highlighted, the validity of the welfare interpretation rests on the validity of the Roback model's assumptions, some of which may not be justified. For example, the models assumes that there is no involuntary unemployment and moving is costless. Further, the model assumes that the supply of land for housing in each county is fixed. In practice, however, the supply of land for housing in some counties, particularly rural ones, may be elastic. For these counties, the value of the marginal plot of land is generally the value of agricultural land, which rarely includes structures. Since the available measure of property values includes both the value of structures and land, the *measured* increase in property values in these counties may overestimate the increase in welfare for landowners because it will capture the value of new structures, or improvements to old ones.[46]

If the Roback assumptions fail to hold, this paper's results still provide new estimates of the impact of successfully bidding for a large industrial plant on workers' earnings, property values and

[46] Separate data for the value of land and structures are available only for a handful of counties and therefore cannot be used for our analysis. See the Data Appendix for further details.

government finances. It is widely believed that all three of these outcomes are important measures of a policy's efficacy and local economic well-being.

There are at least four limitations to the paper's analysis that bear highlighting. First, the analysis fails to shed any light on the precise mechanism or channel by which counties benefit from attracting a "Million Dollar" plant. For example, we cannot ascertain whether the increase in employment is simply due to increases in demand, agglomeration economies, or other type of externalities. In future work, we will use this same research design to test the agglomeration theory by exploring whether the opening of a "Million Dollar Plant" lowers the production costs of other plants in the same industry in winning counties.

A second limitation is that the external validity of the results is unknown. In particular, the 82 plants in our sample likely differ from the average industrial plant in many important dimensions so we suspect that these results do not necessarily generalize to the opening of any plant. The most important dimension in which the "Million Dollar" plants differ from the average plant is in their size. Although we do not know the exact selection rule that determines which plants are featured in the "Million Dollar Plant" articles, it is quite clear that very large plants are the focus. We suspect that the 82 plants in our sample are a reasonably representative sample of all new large plant openings in the US. It is precisely these large plants that generate the type of bidding wars that are of interest to the public and policymakers, and that we seek to study in this paper. In this respect, our sample of large plants is the correct one for exploring the positive effects of winning a plant but the results cannot inform the normative question of which types of plants should be subsidized.

A third important limitation is that in general we don't observe the magnitude of the subsidies offered either by the winners or losers. This implies that we cannot measure how much an extra dollar of subsidies is worth. Moreover, the paper's results do not provide guidance on whether a local government should increase or decrease its bid for a particular plant.

Fourth, the focus of this paper is on local communities. As we mentioned from the outset, the question of whether states or the nation as a whole benefit from tax competitions between local governments to attract new business is beyond the scope of this paper.

VI. Conclusions

This paper makes two contributions. Substantively, it provides credible estimates of the effect of successfully bidding for an industrial plant, relative to bidding and losing, on local economies. We find that the announcement of a "Million Dollar" plant opening is associated with a 1.5% increase in the trend in earnings in the new plant's industry in winning counties (relative to losing ones) after the announcement (relative to the period before). We also find evidence of positive trend breaks in total wages in other industries in the same county and in neighboring counties, although these estimates are less precise. These results provide some of the first estimates of the increase in local economic activity directly and indirectly due to the attraction of a new plant.

Under some assumptions, property values may provide a summary measure of the net change in welfare, because the costs and benefits of attracting a plant should be capitalized into the price of land. If the winners and losers are homogeneous, a simple model suggests that any rents should be bid away. The most reliable data suggest that there is a positive, relative trend break of 1.1% in property values. Since winners and losers appear to have similar observable characteristics and similar expected future changes in property values in advance of the opening announcement, the property value results may be explained by heterogeneity in subsidies from higher levels of government (e.g., states) and/or systematic underbidding. Overall, the results undermine the popular view that the provision of local subsidies to attract large industrial plants reduces local residents' welfare.

The analysis further reveals that local government finances in winning counties are not adversely affected by the successful bidding for a plant. Local governments in winning counties experienced positive and roughly equal trend breaks in revenues and expenditures. Notably, there is a substantial increase in education spending.

The second contribution of the paper is methodological. The paper demonstrates that it is possible to use the revealed rankings of profit-maximizing agents to identify a credible counterfactual. In our particular case, we show that losing counties—counties that have survived a long selection process, but narrowly lost the competition—may form a valid counterfactual for what would have happened in the absence of the plant opening in winning counties. The same methodology may be useful in other contexts (e.g., auctions) to make causal inferences.

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DATA APPENDIX: Property Value Data

There are a number of issues about the property value data that may affect the interpretation of the paper's results. First, the linear interpolation procedure causes the true variability in the data to be understated and biases the standard errors in the regressions downwards. Further, it may obscure the timing of changes in property values associated with plant openings.

Second, the data measure the aggregate market value of all land and structures across residential and industrial land.[47] In many states, the assessed value differs from the market value but, in these cases, an assessment rate is available that allows for the conversion to market values. It was not possible to obtain separate measures of the value of the land, excluding the structures, for our entire sample. A drawback of this measure of property values is that it will include the value of the new plant's structure, which mechanically causes measured property values to increase. We were unable to obtain reliable measures of the size of these plants but we note that the mean aggregate property values in winning counties in the year before the plant opening announcement is roughly \$17.1 billion. Consequently, we suspect that the plant's structure is unlikely to have substantive effect on aggregate property values.

Third, there are important differences across states in the statutes that govern assessments. One important difference is that the frequency of appraisals varies dramatically across states. For our purposes it is important to note that because the property assessment cycles are mandated by the state, it is unlikely that the timing of assessment is systematically correlated with plant openings. Appendix Table 1 reports the distribution of state mandated property assessment cycles for the 31 states represented in the preferred property value sample. 14, or almost half of the states, require that all properties are reassessed or revalued every year, but 12 states reassess less frequently than once every 3 years. Interestingly, Connecticut allows 10 years between revaluations. In many of the states that allow for more than a year between reappraisals, 1/x of the properties are reassessed each year, where x is the number of years between mandated revaluations.

The relevance for our analysis is that these data are not designed to give accurate measures of property values each year. This is relevant because a model with rational, perfect-foresight agents would predict that the net effect of winning the plant should be capitalized into property values immediately after the announcement. Thus, it would be appropriate to estimate this effect with a 1-time change in mean property values. In light of the infrequent reappraisals in many states, a trend break model is more appropriate and this model is discussed further in the econometrics section.

A related issue is that since the reappraisal dates are unobserved, there are likely to be jumps in the property value data. It is possible that these jumps could induce a spurious correlation between plant opening announcements and property values. Further, the longer the period between reappraisals, the more difficult it is to detect changes in property values associated with a plant opening in the first few years after its opening. Consequently, we drop all observations from counties with at least one annual change in property values greater than 70% or less than –70%.

Due to the passage of Proposition 13 in 1978, the California data present a set of unique issues. Proposition 13 limits the increase in the assessed value of a home to 2% per year, unless there is a transfer of ownership or substantial improvements are made. As a consequence, it is difficult to detect positive changes in property values in California. To the best of our knowledge, no other states place a cap on the increase in assessed property values.[48] In our preferred sample, we drop all California counties from the sample. We also demonstrate that the results are robust to including these counties in the sample, with or without California by year fixed effects.

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[47] States have different names for the market value of land and structures. The most frequently used ones are "fair market value", "full cash value", or "true cash value".

[48] Appendix D of the 1992 Census of Governments, Volume 2 Taxable Property Values, Number 1 Assessed Valuations for Local General Property Taxation lists the state-specific requirements for periodic valuation of real property. According to this Appendix, California is the only state that places a cap on the increase in property values.

For a different set of reasons, another state for which we are not completely certain that our property value data fully reflect market value is Indiana. We have re-estimated all our property value models excluding Indiana, and obtained estimates that are similar to the ones on Table 6.[49]

Due to these data challenges, we correlated our measure of property values for counties in MSAs with an annual MSA-level produced by the Office of Federal Housing Enterprise Oversight (OFHEO) in order to ascertain its meaningfulness.[50] The OFHEO price index is based on repeated sales of the same property. For our purposes, one limitation is that the index is available for metropolitan areas, but not for counties, so that the match with our property value data is not perfect. For those observations that we can match, the correlation in the annual percentage change is 0.54.

Finally we note that when we estimate our property value models dropping the set of counties for which the property value data are entirely from the Annual Survey of Governments, we obtain estimates that are qualitatively similar-, although less precise than the estimates reported in Table 6.

[49] For example, the model in column 3a yields an estimate of 283.0 (98.0).

[50] The OFHEO price index is available from www.ofheo.gov.

Table 1: "The Million Dollar Plant" Sample

	(1)
Number of Observations	
Winners	82
Losers	129
Distribution of the Number of Loser Counties per Winner	
1	57
2	14
3	7
4	2
7	1
8	1
Distribution of Cases Across Industries	
Manufacturing	63
Transportation and Public Utilities	8
Trade	4
Finance, Insurance and Real Estate	1
Services	6
Distribution of Year of Announcement of Plant Location	
1982	5
1983	3
1984	3
1985	6
1986	6
1987	8
1988	9
1989	7
1990	6
1991	12
1992	10
1993	7

Notes: The "Million Dollar Plant" sample is derived from various issues of *Site Selection*. See the text for more details.

Table 2: County Characteristics by Winner Status

	Winning Counties (1)	Losing Counties (2)	(1)-(2) t-stat [p-value] (3)	All US Counties (4)
(1) Levels of Outcome Variables				
Total Wage Bill in New Plant's Industry ($millions)	1127	1145	0.17	175
	(1480)	(1455)	[0.87]	(760)
Employment in New Plant's Industry	40635	41568	0.13	6846
	(54143)	(49986)	[0.88]	(26990)
Aggregate Property Values ($millions)	17084	19099	-1.00	--
	(8773)	(10630)	[0.30]	
(2) Trends in Outcome Variables				
Percent Growth in Total Wage Bill in New Plant's Industry	.023	.016	1.04	.018
	(.089)	(.084)	[.30]	(.282)
Percent Growth in Employment in New Plant's Industry	.022	.011	1.57	.006
	(.092)	(.089)	[.12]	(.283)
Percent Growth in Property Values	.050	.055	-0.25	--
	(.128)	(.092)	[0.80]	
(3) Socio-Economic and Demographic Characteristics				
Per Capita Income	13660	15223	-2.70	11416
	(3211)	(4250)	[0.008]	(2636)
Percent Growth in Per capita Income	0.014	0.011	-1.02	0.011
	(0.010)	(0.019)	[0.31]	(0.057)
Per Capita Total earnings	8993	10102	-1.78	7236
	(4359)	(3730)	[0.08]	(2253)
Percent Growth in Per Capita Total Earnings	0.013	0.015	-0.89	0.011
	(0.028)	(0.025)	[0.37]	(0.124)
Employment-Population Ratio	0.541	0.573	-1.40	0.468
	(0.170)	(0.132)	[0.17]	(0.126)
Percent Growth in Employment-Population Ratio	0.010	0.011	-0.51	0.009
	(0.017)	(0.015)	[0.60]	(0.042)
Per Capita Transfers	1770	1930	-1.76	2333
	(628)	(554)	[0.080]	(1480)
Population	342876	449280	-1.79	90139
	(424939)	(346988)	[0.076]	(400341)
Fraction of Population with High- School Degree	0.729	0.762	-1.63	0.695
	(0.088)	(0.092)	[0.10]	(0.103)
Fraction of Population with College Degree	0.197	0.238	-2.22	0.134
	(0.074)	(0.089)	[0.02]	(0.063)
Fraction of Pop 17 or Younger	0.257	0.246	1.41	0.269
	(0.037)	(0.027)	[0.16]	(0.035)
Fraction of Pop 65 or Older	0.125	0.123	0.13	0.149
	(0.051)	(0.029)	[0.89]	(0.043)
(4) Geographic Distribution				
Northeast	0.093	0.250	-2.86	0.069
	(0.292)	(0.392)	[0.005]	(0.254)
Midwest	0.129	0.203	-1.43	0.340
	(0.320)	(0.375)	[0.16]	(0.473)

South	0.660	0.391	3.88	0.449
	(0.474)	(0.460)	[0.000]	(0.097)
West	0.127	0.152	-0.77	0.140
	(0.323)	(0.348)	[0.44]	(0.347)
(5) Industry Distribution of the Labor Force				
Construction	0.067	0.059	1.79	0.050
	(0.036)	(0.019)	[0.075]	(0.043)
Manufacturing	0.268	0.222	2.30	0.236
	(0.156)	(0.107)	[0.02]	(0.181)
Transportation, Utilities	0.052	0.059	-1.56	0.053
	(0.031)	(0.022)	[0.12]	(0.044)
Wholesale	0.068	0.068	-0.09	0.065
	(0.046)	(0.022)	[0.92]	(0.054)
Retail	0.217	0.216	0.17	0.263
	(0.053)	(0.045)	[0.87]	(0.104)
Finance, Insurance, Real Estate	0.059	0.074	-3.54	0.051
	(0.026)	(0.030)	[0.001]	(0.035)
Services	0.248	0.284	-2.99	0.246
	(0.088)	(0.070)	[0.003]	(0.101)

Notes: The figure in columns 1 and 2 are averages for the three years before the plant opening. The figures in the top panel of column 4 are a weighted average for years 1982 to 1993, with weights proportional to the number of Million Dollar cases in each year and industry. The figures in panels 2 to 5 of column 4 are a weighted average for years 1982 to 1993, with weights proportional to the number of Million Dollar cases in each year (see bottom of Table 1 for the distribution of cases across years). All monetary values are in 1983 dollars.

Table 3: The Effect of Plant Openings on the Wage Bill, by Year and Winner Status Relative to the Date of the Plant Location Announcement

Time	Winners (1)	Losers (2)	Difference (3)
τ = -8	-320.4	-75.5	-244.8
	(159.0)	(118.1)	
τ = -7	-313.1	-38.5	-274.6
	(158.9)	(118.0)	
τ = -6	-253.3	–8.2	-245.0
	(158.8)	(118.0)	
τ = -5	-225.1	28.5	-253.7
	(158.9)	(118.0)	
τ = -4	-191.7	66.3	-258.1
	(158.9)	(118.0)	
τ = -3	-136.4	121.9	-258.4
	(158.9)	(118.1)	
τ = -2	-109.0	156.7	-265.8
	(158.9)	(118.1)	
τ = -1	-92.7	180.1	-272.9
	(158.7)	(118.6)	
τ = 0	-39.0	203.1	-242.1
	(159.0)	(118.3)	
τ = 1	7.7	222.2	-215.0
	(158.7)	(118.1)	
τ = 2	54.3	269.4	-215.0
	(158.7)	(118.1)	
τ = 3	99.1.	295.1	-196.0
	(158.7)	(118.1)	
τ = 4	181.3	368.1	-186.7
	(158.6)	(118.1)	
τ = 5	243.6	412.6	-169.0
	(158.7)	(118.2)	

Notes: The Table reports the $\pi_{W\tau}$ (column 1) and $\pi_{L\tau}$ (column 2) coefficients and their standard errors from the estimation of equation (6) on the *County Business Patterns* data. Column (3) reports the difference between the column (1) and (2) entries. See the text for more details. The normalized coefficients are plotted in the top panel of Figure 1.

Table 4: The Effect of Plant Openings on 1-Digit Industry Wage Bill

	All US Counties (1)		Winner and Loser Sample (2)		All US Counties Naïve Estimator (3)	
	(a)	(b)	(a)	(b)	(a)	(b)
Wage Bill						
Change in Time Trend in Winner Counties Relative to Loser Counties	16.8 (5.2)	16.7 (5.2)	13.2 (5.6)	12.6 (5.6)	24.7 (4.9)	24.8 (4.9)
Average Wage Bill in Winner Counties at $\tau = -1$	1127 (1498)	1127 (1498)	1127 (1498)	1127 (1498)	1127 (1498)	1127 (1498)
Ratio of Row 1 and Row 2	0.015	0.015	0.012	0.012	0.022	0.022
N	356589	356589	17580	17580	356589	356589
Region by Year Fixed Effects	No	Yes	No	Yes	No	Yes

Notes: The entries in the first row are estimates of β and their heteroskedastic consistent standard errors from the fitting of equation (7). The second row reports the change in trend in one of the loser counties relative to the trend in other loser counties. The third row presents the average wage bill in the 1-digit industry of winner counties. Wage bill is measured in millions of dollars. N refers to the number of observations in the estimation of equation (6). The sample in columns 1 and 3 includes all US counties. The sample in column 2 includes only the 166 winner and loser counties.

Table 5: The Effect of Plant Openings on Wage Bill – Other Industries and Other Counties

	Other Industries, Same County (1)		Same Industry, Contiguous Counties (2)		Other Industries, Contiguous Counties (3)	
	(a)	(b)	(a)	(b)	(a)	(b)
Wage Bill						
Change in Time Trend in Winner Counties Relative to Loser Counties	42.0 (17.5)	32.1 (37.5)	40.4 (17.6)	39.1 (18.9)	72.8 (55.7)	33.8 (75.1)
Average Wage Bill in Relevant Industry in Winner Counties (or Winners' Neighbors) at τ =-1	3702 (5805)	3702 (5805)	3020 (4202)	3020 (4202)	10697 (13655)	10697 (13655)
Ratio of Row 1 and Row 2	0.011	0.009	0.012	0.012	0.007	0.003
N	17580	17580	17580	17580	17580	17580
Region by Year Fixed Effects	No	Yes	No	Yes	No	Yes

Notes: The entries in the first row are estimates of β and their heteroskedastic consistent standard errors from the fitting of equation (7). In column (1), the $\pi_{W\tau}$'s and $\pi_{L\tau}$'s are derived from the sample of all industries besides the new plant's industry in the winner and loser counties. In columns (2) and (3), the $\pi_{W\tau}$'s and $\pi_{L\tau}$'s are obtained from by fitting equation (6) to observations from counties that neighbor the winner and loser counties for the same industry as the new plant and all other industries, respectively. See the previous tables and the text for more details.

Table 6: The Effect of Plant Openings on Aggregate Property Values

	(1)		California by Year Fixed Effects (2)		Drop All California Observations (3)	
	(a)	(b)	(a)	(b)	(a)	(b)
Property Values						
Change in Time Trend in Winner Counties Relative to Loser Counties	140.2	103.3	187.7	167.4	277.7	176.0
	(83.5)	(79.0)	(82.9)	(77.9)	(98.0)	(81.9)
Average Aggregate Property Value in Winner Counties at $\tau = -1$	17084	17084	17084	17084	16428	16428
	(8773)	(8773)	(8773)	(8773)	(8729)	(8729)
Ratio of Row 1 and Row 2	0.008	0.006	0.011	0.010	0.017	0.011
Number of Winners	32	32	32	32	30	30
Number of Losers	69	69	69	69	62	62
Region by Year Fixed Effects	No	Yes	No	Yes	No	Yes

Notes: The entries in the first row are estimates of β and their heteroskedastic consistent standard errors from the fitting of equation (7). The units are millions of dollars. The property value data was collected from state and local governments and from the Census of Governments. In columns (1) and (2), the sample includes all counties in the Million Dollar Plant sample with nonmissing property value data for $\tau >= -8$ and $\tau <= 5$. The specification in column (2) adds California by year fixed effects to the specification. The sample that underlies the column (3) results adds the restriction that all observations from California are dropped. See the previous tables and the text for more details. The Data Appendix contains further details on the housing price data.

Table 7: The Impact of Winner Status on Government Expenditures, Revenues, and Indebtedness

Dependent Variables			β (std. error) (1)	τ = -1 Mean (2)	(1)/(2) (3)
<u>EXPENDITURES</u>					
Total			17.2 (4.7)	966.8	0.018
	Current		14.5 (4.3)	823.5	0.018
	Capital		2.7 (2.1)	143.3	0.019
	Intergovernmental		1.0 (0.5)	35.0	0.029
		<u>Selected Sub-Categories</u>			
		Education (Capital)	1.5 (0.5)	31.0	0.048
		Education. (Current)	4.3 (1.3)	290.7	0.015
		Police Protection	-0.1 (0.3)	50.4	-0.002
		Housing & Comm Dev.	1.2 (0.6)	25.8	0.047
		Utilities	5.4 (1.1)	70.7	0.076
<u>REVENUES</u>					
Total			18.5 (5.0)	953.6	0.019
	Intergovernmental		5.6 (2.0)	273.6	0.020
		From State Government	5.7 (1.6)	211.3	0.027
	Total Own		12.8 (3.5)	680.0	0.019
		All Taxes	4.1 (1.7)	392.2	0.010
		Utility Charges	4.0 (0.8)	56.3	0.071
<u>INDEBTEDNESS</u>					
Total Debt			16.3 (8.3)	904.3	0.018
	Nonguaranteed Debt		12.8 (6.7)	543.4	0.024

Notes: The entries in column (1) are estimates of β and their heteroskedastic consistent standard errors from the fitting of equation (7). The rows identify the dependent variables, all of which are measured in millions of dollars. Column (2) reports the mean of the dependent variable in t-1 in winner counties and column (3) presents the ratio of the point estimates from (1) to the mean. The data source is the *Annual Survey of Governments*. N = 4500. See the previous tables and the text for more details.

Appendix Table 1: Distribution of Real Property Assessment Cycles by States in 1991 and Subsequent Years.

Number of Years Between Revaluations (1)	Number of States (2)
1	14
2	2
3	1
4	8
5	1
6	2
7	0
8	0
9	0
10	1
Unknown	2

Notes: The table reports the number of years between revaluations for the states in the preferred sample for the housing price regressions. The results from this regression are reported in columns (3a) and (3b) of Table 6. In some states, the assessment cycle varies within the state. In these cases, the table reports the shortest of the revaluation periods. The "Unknown" category is reserved for states where it was not possible to determine the assessment cycle. The source for the information in this table is Appendix D of the *1992 Census of Governments, Volume 2 Taxable Property Values, Number 1 Assessed Valuations for Local General Property Taxation*.

Appendix Table 2: The "Million Dollar Plants" and the Winner and Loser Counties

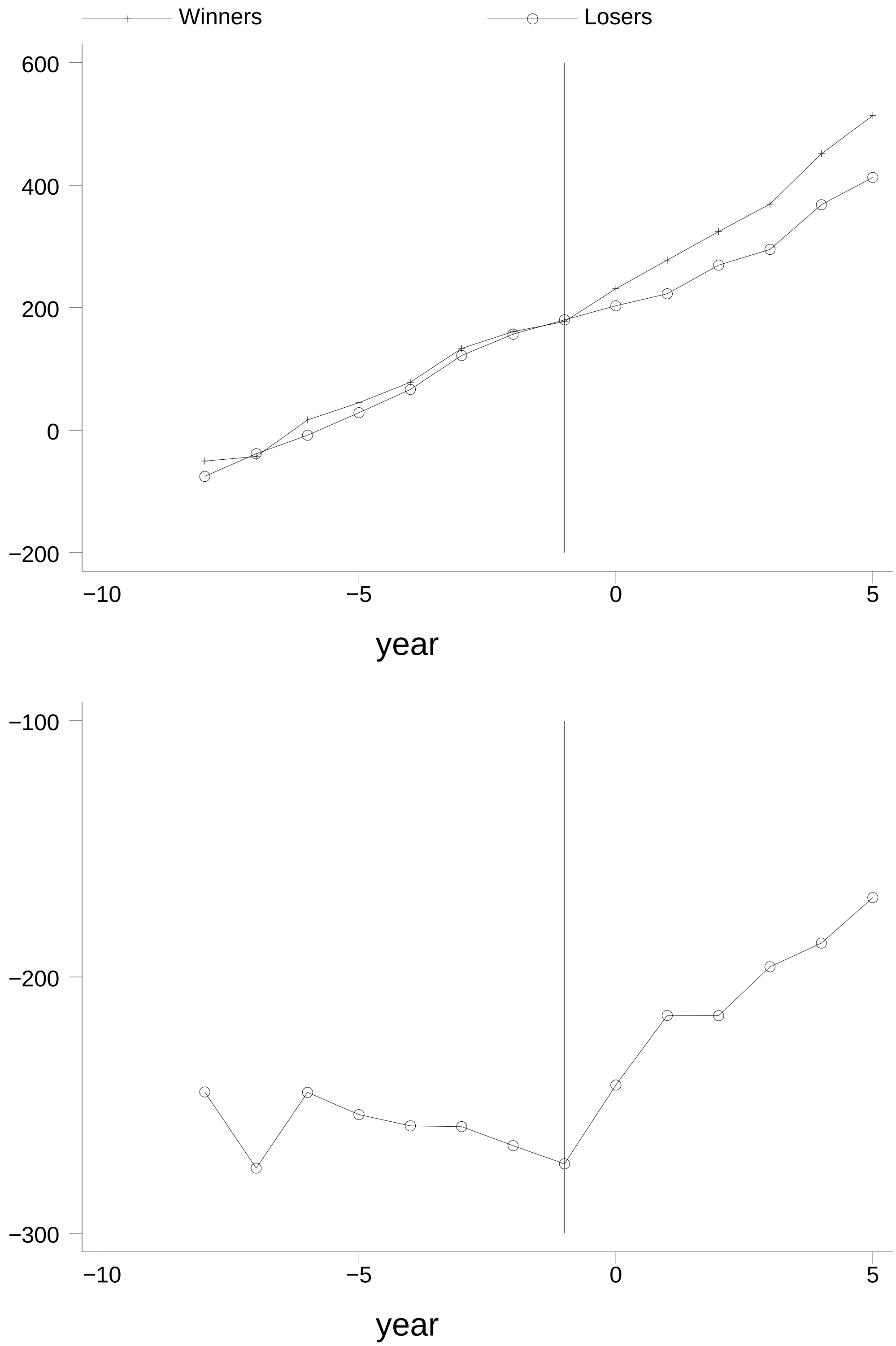
Case Number	Plant Owner (Industry)	Winner/ Loser	County
1	Timken (Mnfg)	Winner	Stark, OH
		Loser	Montgomery, VA
2	General Electric (Mnfg)	Winner	Lowndes, AL
		Loser	Posey, IN
3	Racal-Milgo (Services)	Winner	Broward, FL
		Loser	Dade, FL
		Loser	Pasco, FL
4	Pitney-Bowes (Services)	Winner	Fayette, GA
		Loser	Hamilton, OH
5	Corning/Kroger (Mnfg)	Winner	Clark, KY
		Loser	Montgomery, KY
6	Verbatim (Mnfg)	Winner	Mecklenburg, NC
		Loser	Wake, NC
7	American Solar King (Mnfg)	Winner	McLennan, TX
		Loser	Suffolk, MA
8	Hewlett-Packard (Mnfg)	Winner	Snohomish, WA
		Loser	King, WA
		Loser	Larimer, CO
		Loser	Santa Clara, CA
9	General Motors (Mnfg)	Winner	St. Charles, MO
		Loser	St. Louis, MO
9	Whirlpool (Mnfg)	Winner	Rutherford, TN
		Loser	Vanderburgh, IN
11	Codex (Motorola) (Mnfg)	Winner	Middlesex, MA
		Loser	Bristol, MA
12	Tubular Corp (Mnfg)	Winner	Muskogee, OK
		Loser	Phillips, AR
13	TRW (Services)	Winner	Fairfax, VA
		Loser	Loudoun, VA
		Loser	Montgomery, MD
14	Kyocera (Mnfg)	Winner	Clark, WA
		Loser	E. Baton Rouge, LA
		Loser	Travis, TX
		Loser	Bernalillo, NM
		Loser	Nueces, TX
15	AiResearch (Mnfg)	Winner	Pima, AZ
		Loser	El Paso, CO
		Loser	Bernalillo, NM
16	Ft. Howard Paper (Mnfg)	Winner	Effingham, GA
		Loser	Jasper, SC
17	Rockwell International (Mnfg)	Winner	Johnson, IA
		Loser	Linn, IA
18	Saturn (Mnfg)	Winner	Maury, TN
		Loser	Grayson, TX
		Loser	Kalamazoo, MI
		Loser	Shelby, KY
19	Toyota (Mnfg)	Winner	Scott, KY
		Loser	Wilson, TN
		Loser	Wyandotte, KS
20	Canon (Mnfg)	Winner	Newport News, VA
		Loser	Henrico, VA
21	DuPont/Phillips (Mnfg)	Winner	Cleveland, NC
		Loser	Durham, NC
22	Nippon Columbia (Mnfg)	Winner	Morgan, GA
		Loser	Buncombe, NC
23	Mack (Mnfg)	Winner	Fairfield, SC
		Loser	Richland, SC
		Loser	Lehigh, PA
24	Fuji/Isuzu (Mnfg)	Winner	Tippecanoe, IN
		Loser	Sangamon, IL
		Loser	Hardin, KY
25	Boeing (Mnfg)	Winner	Calcasieu, LA
		Loser	Oklahoma, OK
		Loser	Duval, FL
26	Yamaha (Mnfg)	Winner	Coweta, GA
		Loser	Kendall, IL
27	Carnation (Mnfg)	Winner	Kern, CA
		Loser	Stanislaus, CA
28	Knauf Fiber Glass (Mnfg)	Winner	Chambers, AL
		Loser	Muscogee, GA
		Loser	Russell, AL
		Loser	Troup, GA

Case Number	Plant Owner (Industry)	Winner/ Loser	County	Case Number	Plant Owner (Industry)	Winner/ Loser	County
29	Nippon Kokan (NKK) (Mnfg)	Winner	Linn, OR			Loser	Hamilton, IN
		Loser	Pierce, WA			Loser	Ventura, CA
30	Dresser Rand (Ingers) (Mnfg)	Winner	Allegany, NY	43	Formosa Plastics (Mnfg)	Winner	Calhoun, TX
		Loser	Hartford, CT			Loser	Galveston, TX
31	Worlmark (Mnfg)	Winner	Hancock, KY			Loser	Nueces, TX
		Loser	Daviess, KY	44	Philips Display (Mnfg)	Winner	Washtenaw, MI
		Loser	Perry, IN			Loser	Seneca, NY
32	Eastman Kodak (Mnfg)	Winner	Chester, PA			Loser	Wood, OH
		Loser	Philadelphia, PA			Loser	Lucas, OH
		Loser	Delaware, PA	45	Wal-Mart Stores (Trade)	Winner	Larimer, CO
		Loser	Montgomery, PA			Loser	Laramie, WY
		Loser	Bucks, PA			Loser	Weld, CO
33	Albertson's (Trade)	Winner	Multnomah, OR			Loser	Boulder, CO
		Loser	Washington, OR	46	Ideal Security Hardw (Mnfg)	Winner	Washington, TN
		Loser	King, WA			Loser	Ramsey, MN
34	Metal Container (A-B) (Mnfg)	Winner	Jefferson, WI	47	Burlington Air Express (Tran & Util)	Winner	Lucas, OH
		Loser	Rock, WI			Loser	Allen, IN
		Loser	DeKalb, IL	48	Boeing (Mnfg)	Winner	Wichita, KS
35	Anheuser-Busch (Mnfg)	Winner	Bartow, GA			Loser	Washington, MS
		Loser	Hall, GA	49	Tennessee Eastman (Mnfg)	Winner	Sullivan, TN
		Loser	Knox, TN			Loser	Richland, SC
		Loser	De Kalb, GA	50	Bass (Services)	Winner	De Kalb, GA
36	Kimberly-Clark (Mnfg)	Winner	Tulsa, OK			Loser	Orange, FL
		Loser	Rogers, OK			Loser	Shelby, TN
37	Alumax (Mnfg)	Winner	Gwinnett, GA	51	Allied Signal (Mnfg)	Winner	Kershaw, SC
		Loser	San Mateo, CA			Loser	Rensselaer, NY
38	Toyota (Mnfg)	Winner	Scott, KY	52	Borden (Mnfg)	Winner	Cape May, NJ
		Loser	Alameda, CA			Loser	Cumberland, ME
39	Wella (Mnfg)	Winner	Henrico, VA	53	Reichhold Chemicals (Mnfg)	Winner	Durham, NC
		Loser	Bergen, NJ			Loser	Westchester, NY
40	Reebok International (Mnfg)	Winner	Middlesex, MA	54	Ford (Mnfg)	Winner	Montgomery, PA
		Loser	Suffolk, MA			Loser	Delaware, PA
41	Squibb (Mnfg)	Winner	Camden, NJ	55	Burlington Northern (Tran & Util)	Winner	Tarrant, TX
		Loser	Mercer, NJ			Loser	Johnson, KS
42	GTE (Tran & Util)	Winner	Dallas, TX			Loser	Ramsey, MN
		Loser	Hillsborough, FL	56	Holiday (Services)	Winner	De Kalb, GA

Case Number	Plant Owner (Industry)	Winner/ Loser	County
		Loser	Shelby, TN
57	Adidas USA (Mnfg)	Winner	Spartanburg, SC
		Loser	Somerset, NJ
58	American Auto (Services)	Winner	Seminole, FL
		Loser	Fairfax, VA
59	United Airlines (Tran & Util)	Winner	Denver, CO
		Loser	Champaign, IL
		Loser	Oklahoma, OK
		Loser	Marion, IN
		Loser	Guilford, NC
		Loser	Fairfax, VA
		Loser	Berkeley, WV
		Loser	Hamilton, OH
		Loser	Jefferson, KY
60	Sterilite (Mnfg)	Winner	Jefferson, AL
		Loser	Lauderdale, TN
61	Wal-Mart Stores (Trade)	Winner	Hernando, FL
		Loser	Polk, FL
62	Volvo North America (Mnfg)	Winner	Chesapeake, VA
		Loser	Bergen, NJ
63	AMF/Reece (Mnfg)	Winner	Hanover, VA
		Loser	Middlesex, MA
64	Boeing (Mnfg)	Winner	Snohomish, WA
		Loser	Kitsap, WA
65	United Airlines (Tran & Util)	Winner	Marion, IN
		Loser	Denver, CO
66	Scott Paper (Mnfg)	Winner	Daviess, KY
		Loser	Posey, IN
67	Safeway (Trade)	Winner	San Joaquin, CA
		Loser	Sacramento, CA
68	AT&T (Tran & Util)	Winner	Mecklenburg, NC
		Loser	Berkeley, WV
		Loser	Placer, CA
69	GE Capital Services (Financials)	Winner	Fulton, GA
		Loser	Fairfield, CT

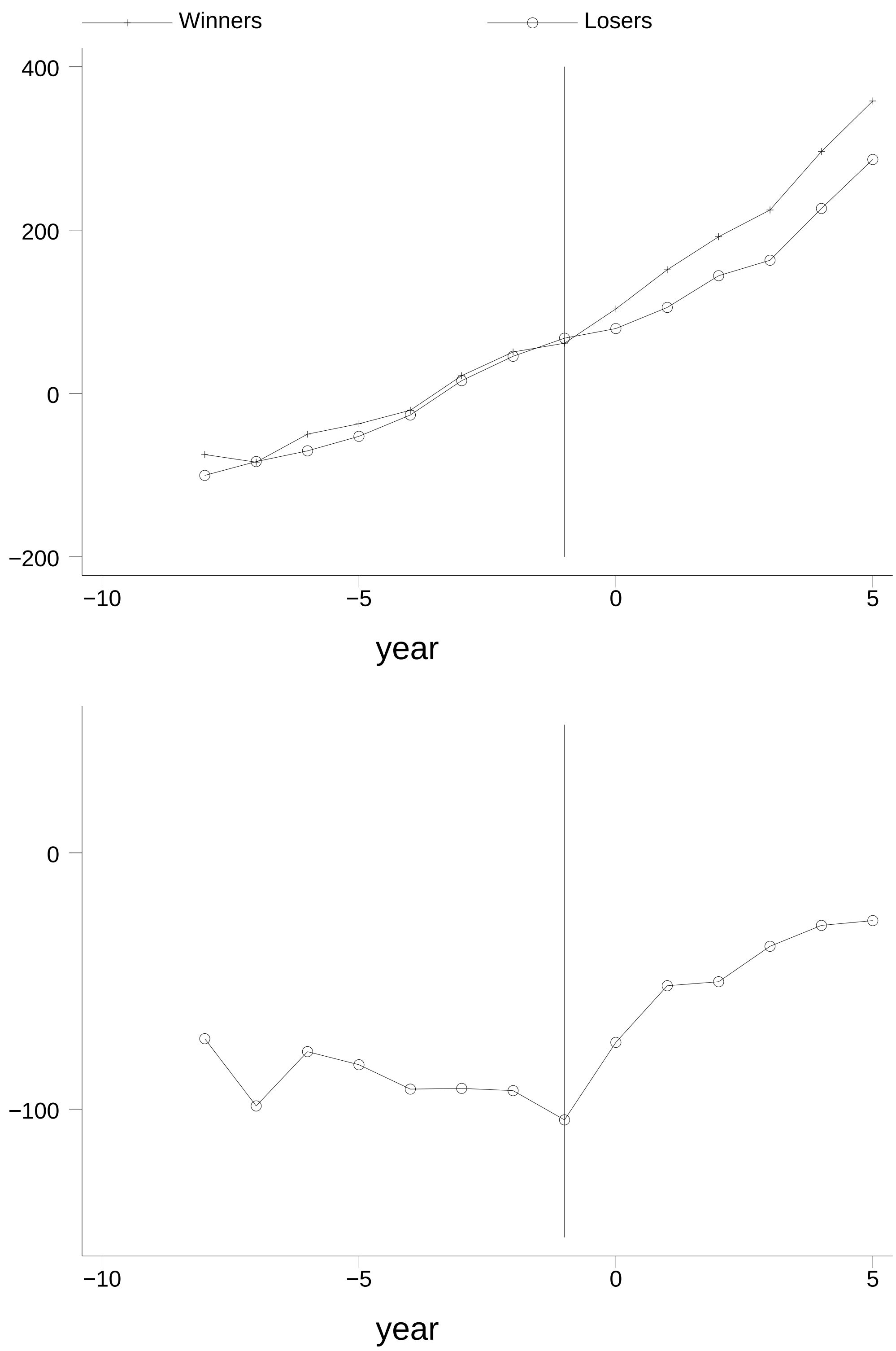
Case Number	Plant Owner (Industry)	Winner/ Loser	County
70	BMW (Mnfg)	Winner	Greenville, SC
		Loser	Douglas, NE
71	National Steel (Mnfg)	Winner	St. Joseph, IN
		Loser	Allegheny, PA
72	MCI Communications (Tran & Util)	Winner	Dade, FL
		Loser	Duval, FL
73	Everest & Jennings (Mnfg)	Winner	St. Louis, MO
		Loser	Ventura, CA
74	Swearingen Aircraft (Mnfg)	Winner	Berkeley, WV
		Loser	New Castle, DE
75	Evenflo Products (Mnfg)	Winner	Cherokee, GA
		Loser	Cuyahoga, OH
76	Sterling Drug (Mnfg)	Winner	Montgomery, PA
		Loser	Rensselaer, NY
77	JLM Industries (Mnfg)	Winner	Hillsborough, FL
		Loser	Fairfield, CT
78	B&W Tobacco (Mnfg)	Winner	Bibb, GA
		Loser	Jefferson, KY
79	Greyhound Lines (Tran & Util)	Winner	Dallas, TX
		Loser	Polk, IA
80	Transkrit (Mnfg)	Winner	Roanoke, VA
		Loser	Westchester, NY
81	Mercedes-Benz (Mnfg)	Winner	Tuscaloosa, AL
		Loser	Berkeley, SC
		Loser	Clarke, GA
		Loser	Alamance, NC
		Loser	Chester, SC
		Loser	Durham, NC
		Loser	Douglas, NE
		Loser	Anderson, TN
82	Schlegel (Mnfg)	Winner	Rockingham, NC
		Loser	Guilford, NC

Figure 1: The Effect of Plant Opening on 1-Digit Industry Wage Bill in Winner and Loser Counties



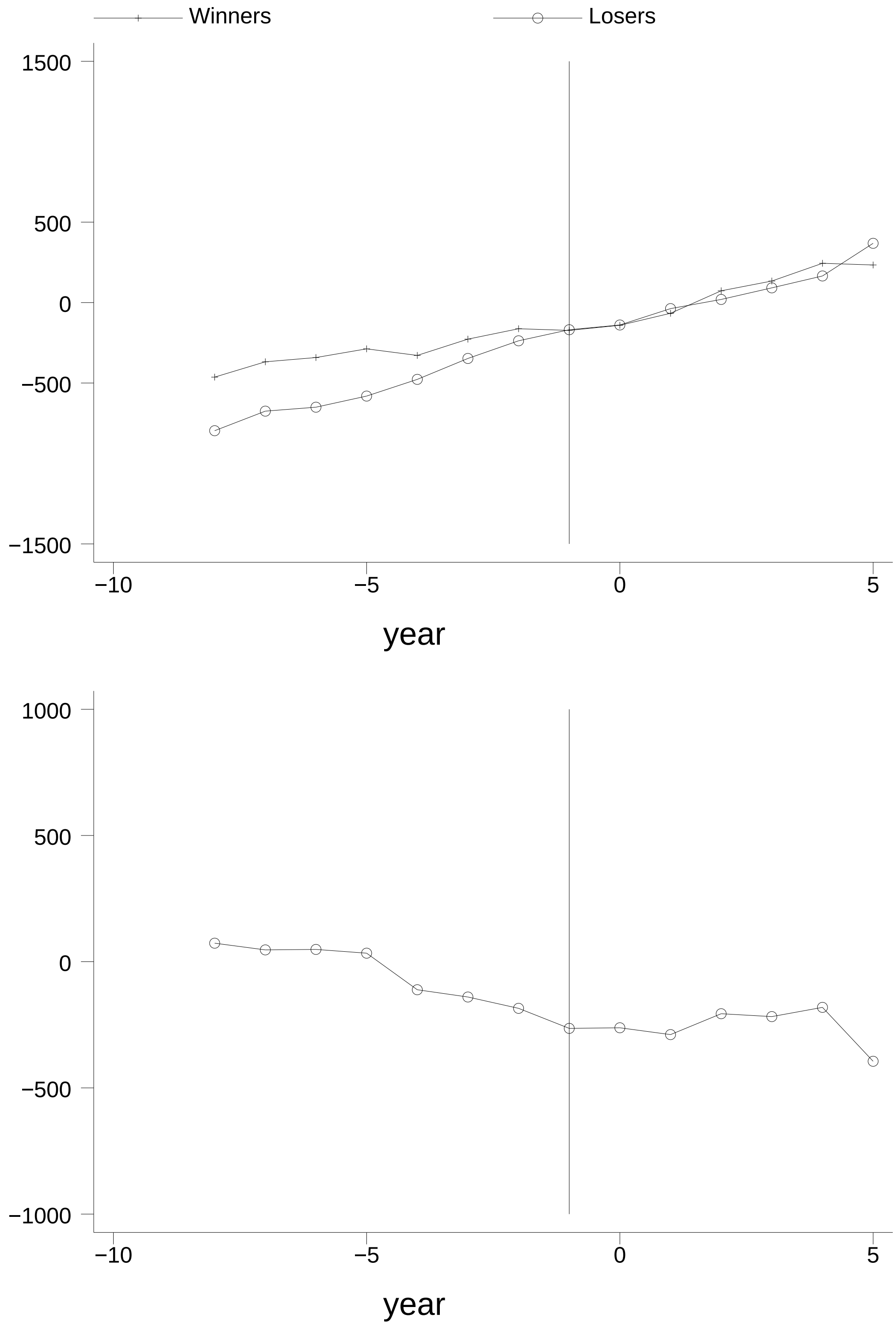
Notes: Top Panel: Conditional average wage bill in winner and loser counties. To facilitate the comparison, the scale of the average wage bill in winner counties has been adjusted to equal the wage bill in loser counties at time t-1.
Bottom panel: difference in conditional average wage bill in winner and loser counties.

Figure 2: The Effect of Plant Opening on 1-Digit Industry Wage Bill in Winner and Loser Counties - Winner and Loser Sample



Notes: Top Panel: Conditional average wage bill in winner and loser counties. To facilitate the comparison, the scale of the average wage bill in winner counties has been adjusted to equal the wage bill in loser counties at time t-1.
Bottom panel: difference in conditional average wage bill in winner and loser counties.

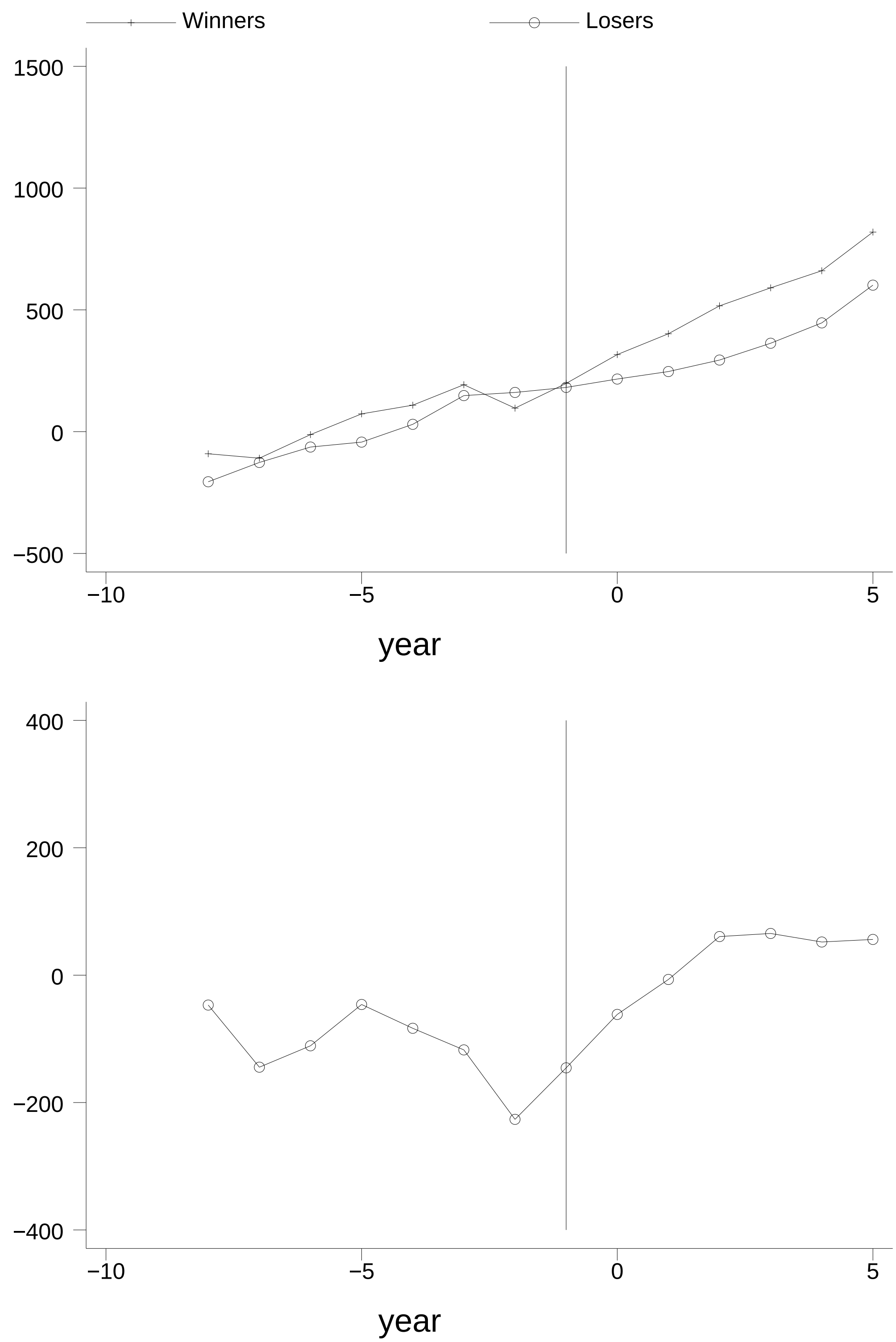
Figure 3: The Effect of Plant Opening on Wage Bill in Winner and Loser Counties - Other Industries, Same County - Winner and Loser Sample



Notes: Top Panel: Conditional average wage bill in winner and loser counties. To facilitate the comparison, the scale of the average wage bill in winner counties has been adjusted to equal the wage bill in loser counties at time t-1.

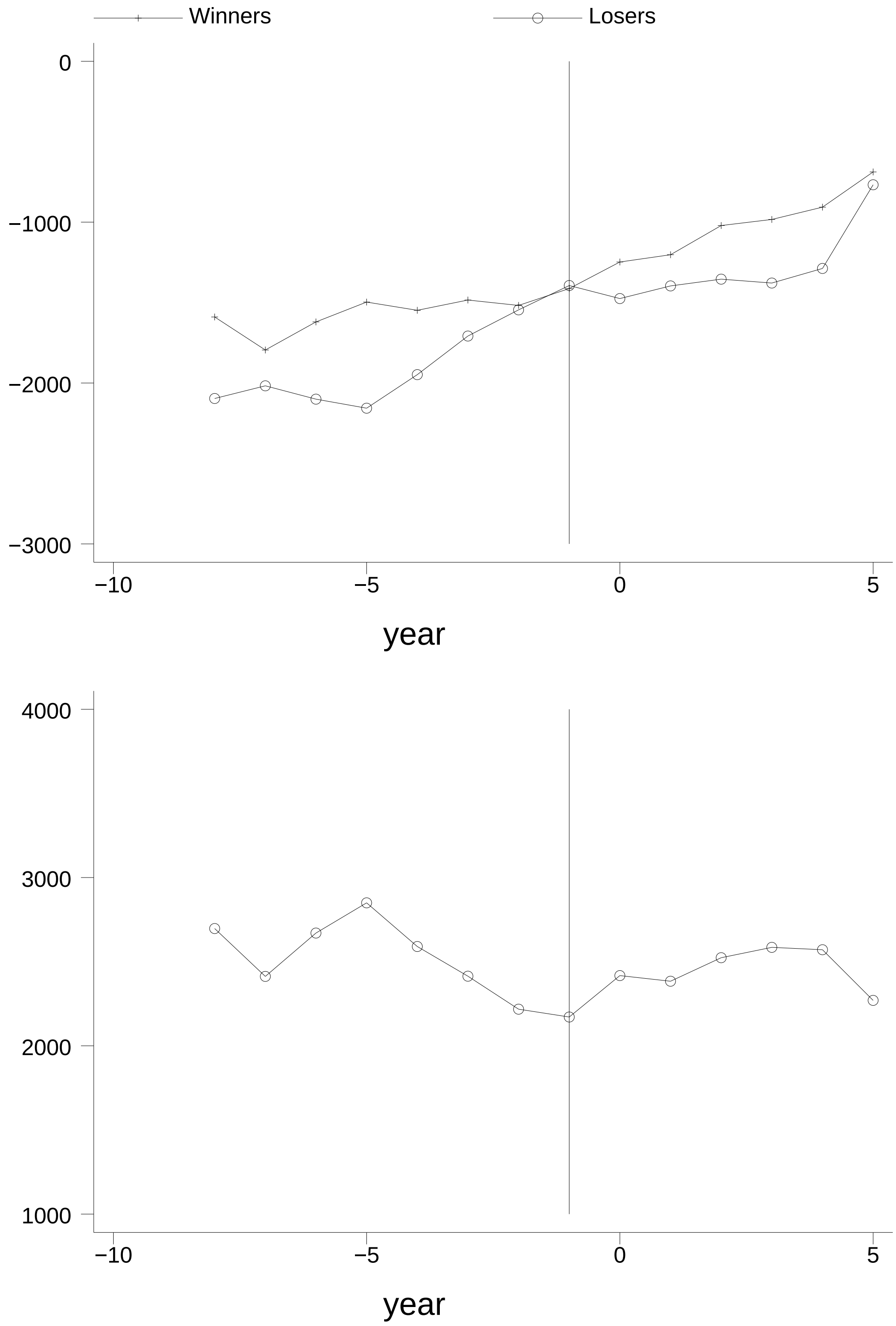
Bottom panel: difference in conditional average wage bill in winner and loser counties.

Figure 4: The Effect of Plant Opening on Wage Bill - Same Industry, Neighboring Counties - Winner and Loser Sample



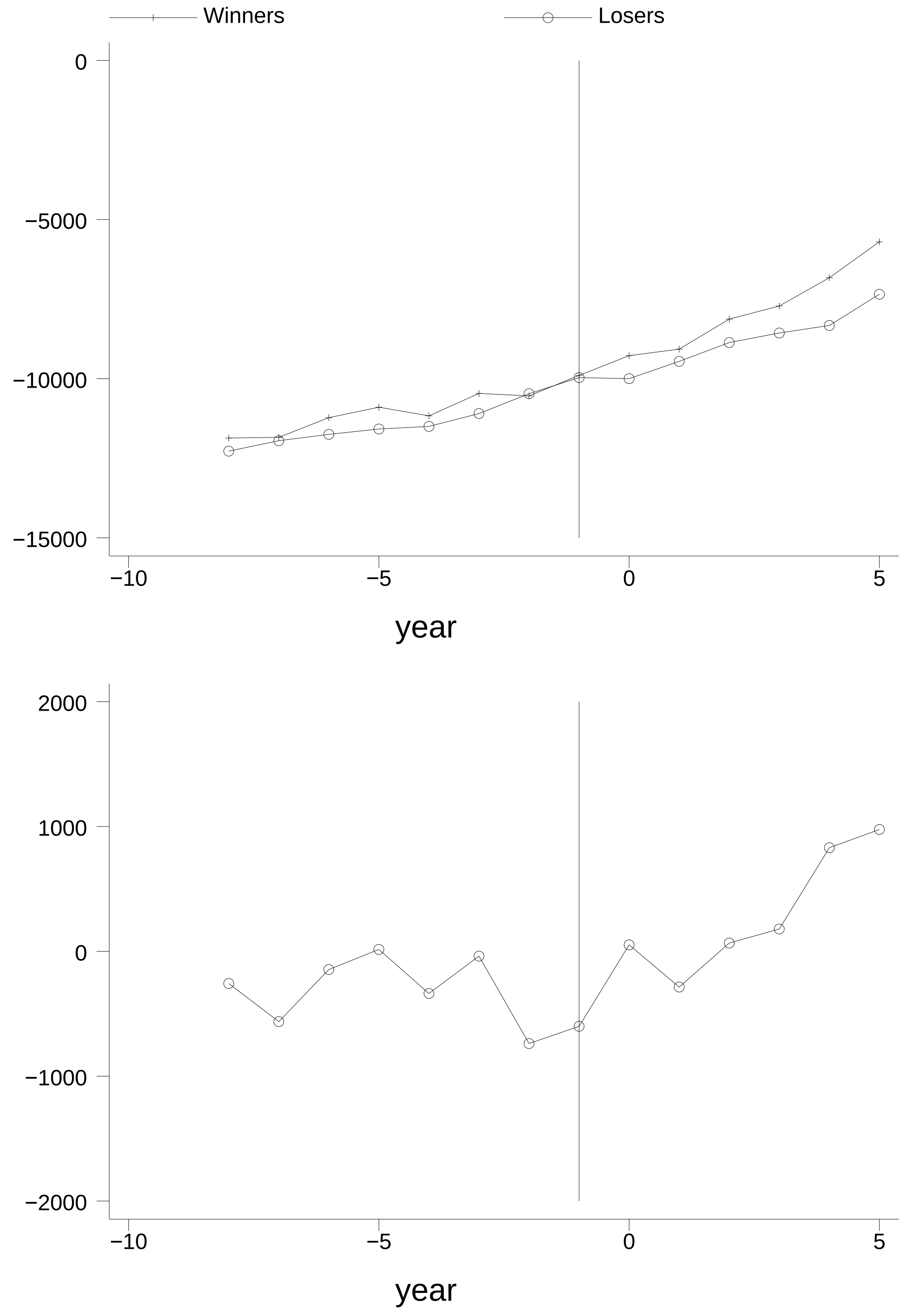
Notes: Top Panel: Conditional average wage bill. To facilitate the comparison, the scale of the average wage bill in winner counties has been adjusted to equal the wage bill in loser counties at time t-1.
Bottom panel: difference in conditional average wage bill between winner and loser counties.

Figure 5: The Effect of Plant Opening on Wage Bill - Other Industres, Neighboring Counties - Winner and Loser sample



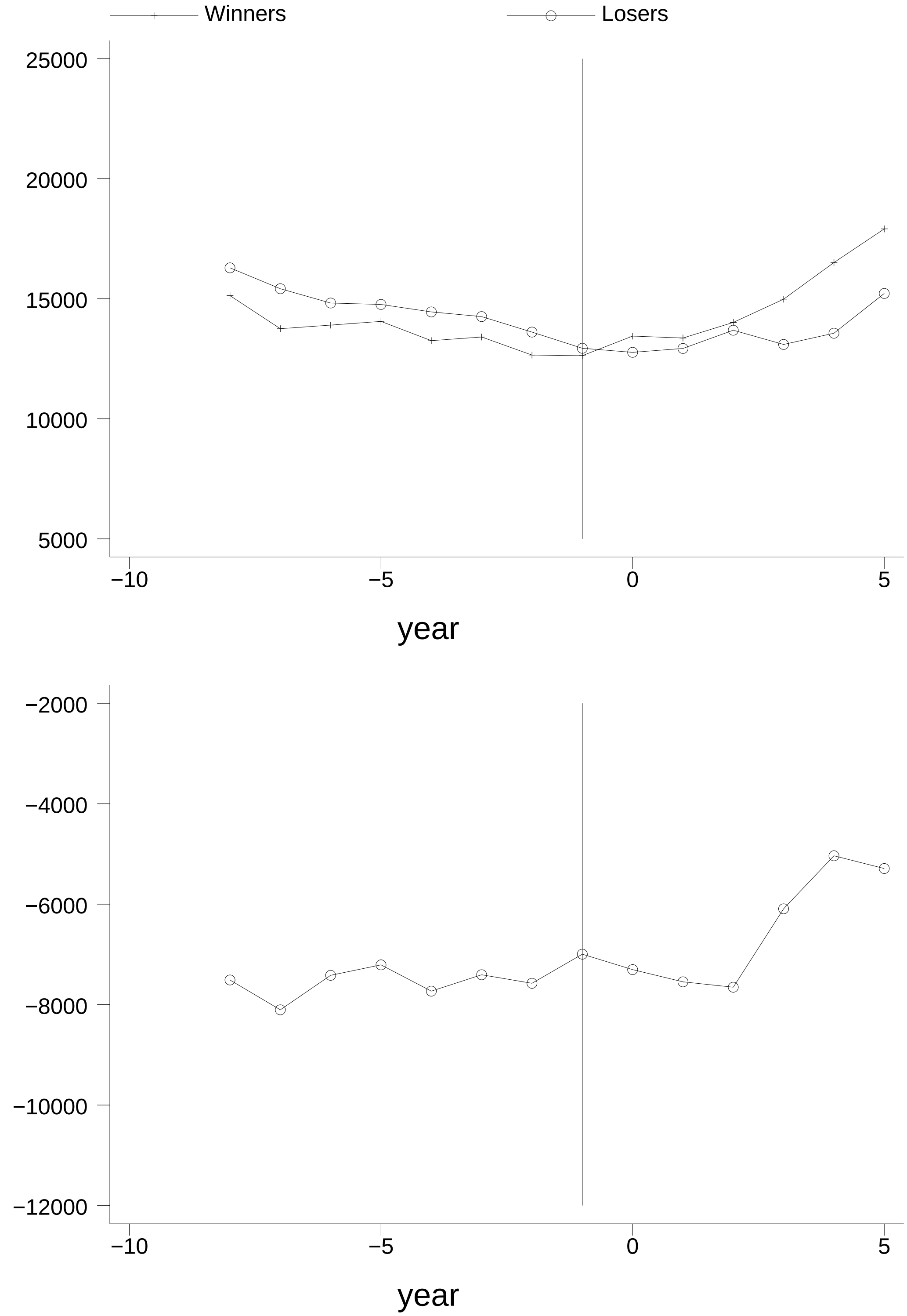
Notes: Top Panel: Conditional average wage bill. To facilitate the comparison, the scale of the average wage bill in winner counties has been adjusted to equal the wage bill in loser counties at time t-1.
Bottom panel: difference in conditional average wage bill between winner and loser counties.

Figure 6: The Effect of Plant Opening on Property Values in Winner and Loser Counties



Notes: Top Panel: Conditional average property value in winner and loser counties. To facilitate the comparison, the scale of the conditional average property value in winner counties has been adjusted to equal the conditional average property value in loser counties at time t-1.
Bottom panel: difference in conditional average property value in winner and loser counties.

Appendix Figure 1: The Effect of Plant Opening on 1-Digit Industry Employment in Winner and Loser Counties - Winner and Loser Sample



Notes: Top Panel: Conditional average employment in winner and loser counties. To facilitate the comparison, the scale of the conditional average employment in winner counties has been adjusted to equal the conditional average employment in loser counties at time t-1.
Bottom panel: difference in conditional average employment in winner and loser counties.