The Effect of Mergers and Acquisitions on Market Power and Efficiency

Bruce A. Blonigen*
University of Oregon
National Bureau of Economic Research

Justin R. Pierce#
Federal Reserve Board

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Abstract: A fundamental question in the analysis of mergers and acquisitions (M&As) is the potential tradeoff between increased market power and efficiency gains. The evidence on this tradeoff in the literature, however, is far from conclusive. Case studies of M&A find mixed evidence, and estimation in more general settings is hampered by the difficulty of separately identifying productivity from markups. In this paper, we use new techniques to separately estimate productivity and markups across heterogeneous industries using firm- or plant-level data, and then employ these estimates in a differences-in-differences framework to examine the average impact of M&A activity on productivity and mark-ups. We employ data on M&A transactions combined with U.S. Census plant-level data for the entire U.S. manufacturing sector over the 1997 to 2007 period. Robust to a variety of specifications, we find significant evidence of increased markups after M&A, but no evidence of any average impact on plant-level productivity. Moreover, we find that the increase in markups generates overstated effects of mergers on traditional revenue productivity measures.

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* Department of Economics, 1285 University of Oregon, Eugene, OR, 97403-1285; 541-346-4680; bruceb@uoregon.edu
# Board of Governors of the Federal Reserve System, 20th and C St. NW, Washington, DC 20551,202-452-2980; justin.r.pierce@frb.gov

1. Introduction

Merger and acquisition (M&A) activity by firms is an important phenomenon in the global economy, involving several trillions of dollars of worldwide assets annually.[1] As a result there is significant change in ownership of assets annually, especially for advanced economies. For example, Maksimovic and Phillips (2001) find that about 4% of large manufacturing plants in the United States change ownership every year. Relatedly, cross-border M&A activity is a primary mode by which multinational firms engage in foreign direct investment (FDI).[2]

Fundamental questions in finance and industrial organization concern the motivation for and effects of M&A activity. And perhaps the most fundamental question is the potential tradeoff between increased market power versus efficiency gains in the wake of a M&A transaction. Increased market power creates net welfare losses, while efficiency gains lower costs and create net welfare gains. While the theory is straightforward, estimating these effects empirically is difficult. Yet, understanding the actual effects of M&A activity in our economy is not only important for the academic literature, but also a key concern for social welfare.

There have been a number of approaches to estimate these potential effects of M&A activity, each associated with their own strengths and weaknesses. In the 1980s and 1990s, a finance literature developed that used stock market event studies to examine the impact of a variety of phenomena on firms' profitability, including the

[1] A recent Forbes article valued M&A deals in 2014 at around $3.5 trillion. (http://fortune.com/2015/01/05/2014-was-a-huge-year-for-ma-and-private-equity/)

[2] See the *World Investment Report* published annually by the United Nation's Conference on Trade and Development.

impact of M&A activity. These studies examine changes in returns to firm share prices after an announced M&A, generally finding that M&A activity leads to greater firm profitability, with the bulk of the profit gains accruing to shareholders of the target firm. (see Ravenscraft and Scherer, 1987, for an overview) The methodology of empirical event studies is simple to implement consistently across a wide range of settings. However, this approach is unable to identify whether the source of profitability changes from M&A activity is due to changes in market power, cost efficiencies, or some other factor.

Given these concerns, more recent analyses of the effects of M&A activity have taken primarily a case study approach, where the researcher can explicitly examine more closely the particular features of the firms and market where the M&A takes place. Ashenfelter, Hosken and Weinberg (2014) document 49 such studies, which have mainly focused on a few key sectors (airlines, banking, hospitals, and petroleum), primarily because these are the sectors where researchers can find detailed firm- and product-level price data.[3] Most of these studies focus on price and market share changes to infer market power effects, typically finding evidence for increased market power by the firms involved in the M&A activity with the exception of M&A activity in the petroleum sector. While these studies contribute to our understanding of the effects M&A activity, they do so in an incremental fashion. They typically focus on high profile acquisitions, making it more likely that their results suffer selection bias and are

[3] Some prominent examples include airlines (Borenstein, 1990; Kim and Singal, 1993, Kwoka and Shumilkina, 2010), appliances (Ashenfelter et al., 2013), banking (Focarelli and Panetta, 2003), and cotton spinning (Braguinsky et al. 2013).

therefore not generally representative of M&A effects on market power. Additionally, as mentioned, their data and analyses are specific to the particular market they study. Finally, with only a couple exceptions, they do not have the data to estimate efficiency effects of M&A activity.[4] As a result, we do not have a general picture of the effects of M&A activity on market power and efficiency in the economy from this prior literature using a case study approach.

In contrast, there have been a few analyses of the average effects of M&A activity on productivity and market power using micro-level data for a broad set of firms (or plants) across the economy, including McGuckin and Nguyen (1995), Maksimovic and Phillips (2001), Gugler (2003), and Bertrand (2008). With the exception of Gugler (2003), these papers use detailed plant- or firm-level data on the manufacturing sector to estimate the effect of M&A activity on total factor (or labor) revenue productivity, finding that M&A activity positively impacts these productivity measures. One challenge faced in these studies, which we find to be important in our setting as well, arises from the use of revenue as a proxy for output. In particular, when estimating the effect of M&A on traditional revenue productivity measures, the market power effect–operating through output prices–makes it impossible to identify whether changes in observed revenue productivity are due to changes in true productive efficiency or market power.[5] Gugler et al. (2003) examines M&A transactions worldwide using COMPUSTAT firm data. They take a reduced form approach and conduct a difference-in-difference (DID)

[4] The exceptions of which we are aware are Jaumandreu (2004) which found some efficiency effects from M&A activity in the Spanish banking industry and Braguinsky et al. 2013. Kulick (2015) finds positive effects on both prices and efficiency resulting from horizontal mergers in the cement industry, using output data measured in physical units of quantity.

[5] Bertrand and Zitouna (2008) also examine the impact of M&A activity on profits, but use accounting data on earnings, which can be affected by changes in accounting practices after an M&A transaction.

analysis of the extent to which sales and/or profits change significantly for a firm after an acquisition. Perhaps due to data limitations, they generally find mixed results.

This paper provides a number of new contributions to this literature addressing the important question of M&A effects on efficiency and market power. The first is the application of recent techniques developed by De Loecker and Warzynski (2012), where one can separately estimate productivity and market power effects for firms with minimal structural assumptions – only cost minimizing behavior by firms is needed. We use U.S. Census plant-level data for the manufacturing sector and match that with data on M&A activity from the SDC Platinum database maintained by Thomson Financial to examine the effects at the plant level after an acquisition over the 1997 to 2007 period.[6] This approach has important advantages over the prior studies. Unlike the case study evidence, we can examine M&A effects across a broad range of industries in a consistent framework. Unlike the prior studies estimating general effects of M&A on revenue-based measure of TFP, we can identify the separate M&A effects on productivity and market power in our estimates.

A second important contribution of our paper is that, unlike many studies in the prior literature, we address the difficult issue of endogenous selection due to the possibility that the decision to engage in M&A activity may be related to current and expected changes in productivity and market power. This would bias estimates in a DID framework that uses "all other firms" as the control group. First, one advantage of our data in this respect is that it is plant-level, whereas M&A decisions are typically at the

[6] Around 50% of all U.S. M&As are in the manufacturing sector over recent decades, including our sample period from 1997 to 2007. Use of Census data also means that our sample include both private and public firms.

firm-level. Many of the firms in our sample are multi-plant, which makes the M&A decision more independent of the performance of any one plant. While some prior studies have employed plant-level data, most have not. Beyond this, we explore other strategies to construct a plausible control group.

The main alternative we use in this preliminary draft is to use plants where an acquisition is announced, but is never completed, as our control group. Such plants have all the attributes necessary to lead to an announced acquisition, eliminating a portion of potential sample selection bias. Thus, the identifying assumption is that the reason for non-completion of the M&A is independent of future productivity and market power. Below we provide further discussion for why this may be a reasonable assumption. In future drafts, we also plan to examine the robustness of our results when using propensity score matching techniques to identify a control group. While this is not a novel strategy, it has not often been employed in studies of M&A effects, especially when examining effects of M&A on productivity and market power.[7] Finally, we plan to examine another more-novel strategy in future drafts – using plants that will be acquired in subsequent years in our sample as a control group for currently acquired plants. This is a valid strategy if the attributes that make a plant more likely to be a target often exist for a few years before a successful match with an acquirer is made.

Like prior studies, we find that M&A activity significantly increases traditional productivity measures that use revenue-related data. However, using our estimates that separately identify true technical productivity from markups using the methods of De Loecker and Warzynski (2012), we find new evidence for an important divergence.

[7] Exceptions include Fresard, Hoberg and Phillips (2015), among others.

M&As significantly increase markups on average, but have no statistically significant effect on productivity. The magnitude of the markup increase is economically significant as well, ranging from a 16% to 85% increase of the average markup in the sample. These results are robust to whether we define the control group as all other non-M&A plants or the much smaller group of plants that were involved in an announced M&A that was ultimately withdrawn. They are robust to 2SLS estimates, where we instrument for M&A selection, as well as the inclusion of a number of fixed effects, including plant fixed effects. They are also robust to whether we examine five-year intervals, using the full census of U.S. manufacturing plants, or estimate using annual survey data of only the largest plants from the American Survey of Manufactures. We also present a placebo test to verify that secular (pre-)trends are not driving our results, which is a common concern with DID estimation.

On a final note, our focus is on the effect of M&A on plant-level productivity, consistent with many prior studies of M&A. There are, however, additional ways in which M&A can affect overall firm-level efficiency. First, an acquiring firm may reallocate resources across the firm to more efficient plants and/or shut down less-efficient plants. In this way, an M&A could increase efficiency across the firm even if plant-level productivity was unaffected. We plan to examine this in future drafts, but have not addressed this source of efficiency gains in this version of the paper. A second source may be realization of economies of scale with headquarter services of the firm (management, marketing, advertising, etc.) after an M&A.

The rest of the paper proceeds as follows. In the next section, we discuss the data we use to examine the effects of M&A on manufacturing plants. We then describe our two-stage estimation process that begins with estimation of plant-level productivity and markup measures and is then followed by employing these as dependent variables in a DID framework. We then present and discuss our results before concluding.

2. Data

We make use of two datasets for this analysis. First, to calculate productivity and markups for U.S. manufacturing firms, we employ confidential data from the U.S. Census Bureau's Census of Manufactures (CM). The CM collects plant-level data for every U.S. manufacturer including, for example, total value of shipments, value added, cost of materials, employment, investment and the book value of capital. This analysis employs data from the 1997 and 2007 CMs, focusing on long differences in plant-level measures of productivity and markups.[8] Lastly, because the CM contains data for all U.S. manufacturers, we are able to calculate several useful control variables including the Herfindahl index for each industry, as well as each firm's share of industry output.

Our second dataset, Thomson Financial's SDC Platinum database (SDC), contains information on merger and acquisition transactions involving both publicly-traded and private firms. For each transaction, SDC provides data for the target and acquiring firms including the name, address and major industry of both, with additional information for the firms' corporate parents if applicable. SDC also reports a variety of detailed information about the transaction, including the dates the merger was announced and completed, the share of the target that was purchased and the share owned after completion of the merger. Moreover, SDC contains information for mergers that were

[8] The CM is collected every five years, in years ending in 2 and 7.

announced but later withdrawn. We use the set of firms involved in these withdrawn mergers as a control group in some portions of the analysis.

For purposes of this paper, we focus on the set of merger transactions in which a U.S. manufacturer is the target, since these are the firms for which CM production and input data are available. In terms of time period, we use merger transactions that were completed or withdrawn from 1998 to 2006. This timeframe ensures that we are able to observe each target firm both before and after they are acquired. It is also a period that includes both periods of high M&A activity in the late 1990s and less activity connected with a general slowdown of the world economy in the early 2000s. The sample period ends just before the start of the Great Recession. We also limit our analysis to merger transactions in which the entire target firm is purchased by the acquirer. Without this restriction, it would be impossible to determine in the Census data which portion of the target firm was acquired in the merger.

The merger transaction data in SDC are linked to the CM data via a name and address matching procedure. Such matching is far from perfect, but we find that our results are robust to a number alternative matching criteria such as limiting the sample to only the observations where both the firm names and addresses match to samples where we require a match in only one of these dimensions.

3. Empirical Framework

Our empirical analysis proceeds in two major steps. We first estimate plant-level markups and productivity following the methods of De Loecker and Warzynski (2012), which we briefly describe below. We then use these estimates in a second-stage DID framework to assess the impact of M&A on plant-level markups and productivity.

3.1. Estimation of markups and productivity

We closely follow the framework developed by De Loecker and Warzynski (2012) to separately identify a plant's markup from its productivity.[9] Using their notation, we begin with a production function

$$Q_{it} = Q\left(X_{it,\dots,}^{1} X_{it}^{V}, K_{it}, \omega_{it}\right), \tag{1}$$

where $X_{it}^{1}, \dots, X_{it}^{V}$ are the V variable input choices by plant i in time period t; K_{it} is the plant's capital stock; and ω_{it} is a productivity parameter. Assuming cost minimization, we can write the associated Lagrangian,

$$L\left(X_{it,\dots,}^{1} X_{it}^{V}, K_{it}, \omega_{it}, \lambda_{it}\right) = \sum_{v=1}^{V} P_{it}^{X^V} X_{it}^{V} + r_{it} K_{it} + \lambda_{it}\left(Q_{it} - Q_{it}(.)\right), \tag{2}$$

where $P_{it}^{X^1}, \dots, P_{it}^{X^V}$ and r_{it} are the variable input prices and cost of capital, respectively. The first-order condition for any given variable input (V) is

$$\frac{\partial L_{it}}{\partial X_{it}^{V}} = P_{it}^{X^V} - \lambda_{it} \frac{\partial Q_{it}(.)}{\partial X_{it}^{V}} = 0. \tag{3}$$

Rearranging the first conditions, we can write:

$$\frac{\partial Q_{it}(.)}{\partial X_{it}^{V}} \frac{X_{it}^{V}}{Q_{it}} = \frac{1}{\lambda_{it}} \frac{P_{it}^{X^V} X_{it}^{V}}{Q_{it}} \tag{4}$$

[9] This contrasts with literature that makes more specific assumptions on consumer preferences and market structure to study a particular industry. Perhaps the most well-known example is the seminal work of Berry et al. (1995) and Goldberg (1995) to model and estimate structural parameters of market behavior in the automobile market.

Define the markup as $\mu_{it} \equiv P_{it}/\lambda_{it}$, where λ_{it} is the marginal cost of production. Substituting in the expression for the markup, yields

$$\frac{\partial Q_{it}(.)}{\partial X_{it}^{V}}\frac{X_{it}^{V}}{Q_{it}} = \mu_{it}\frac{P_{it}^{X^V} X_{it}^{V}}{P_{it}Q_{it}} \tag{5}$$

The left-hand side of equation (5) is the elasticity of output with respect to a variable input (which we denote as θ_{it}^{X}), while the ratio on the righthand side is the share of expenditures on the variable input in total sales of the firm (which we denote as α_{it}^{X}). As a result, we can express the plant's markup as a surprisingly simply function of these two elements:

$$\mu_{it} = \theta_{it}^{X}(\alpha_{it}^{X})^{-1} \tag{6}$$

In order to obtain consistent estimates of the production function parameters, DLW follow the methods proposed by Ackerberg, Caves, and Frazier (2006). For tractability, we restrict attention to production functions with a Hicks-neutral scalar productivity term and assume common technology parameters for plants (within the same NAICS 3-digit industry):

$$Q_{it} = F\big(X_{it,\dots,}^{1}X_{it}^{V}, K_{it}; \beta\big)\exp\big(\omega_{it}\big). \tag{7}$$

Taking logs and assuming a random error term, we can express the production function as:

$$y_{it} = F(x_{it}, k_{it}; \beta) + \omega_{it} + \epsilon_{it}. \tag{8}$$

Productivity and shocks to productivity are unobserved to the econometrician, but may be endogenous with input choices made by the plant. This is handled through a control function approach. In particular, we assume that the plant's current choice of materials depends on the current level of any dynamic variables (here, capital stock), productivity, and any other observable variables that could affect optimal material demand: $m_{it} = m_t(k_{it}, \omega_{it}, \boldsymbol{z}_{it})$. Inverting this function we obtain,

$$\omega_{it} = h_t(m_{it}, k_{it}, \boldsymbol{z_{it}}), \tag{9}$$

which serves as a proxy indexing a plant's productivity, provided that material demand is monotonic in productivity after conditioning on a plant's capital stock and other observables in the vector, $\boldsymbol{z_{it}}$.[10]

We now assume that productivity follows a simple law of motion:

$$\omega_{it} = g_t(\omega_{it-1}) + \xi_{it}. \tag{10}$$

Using labor as our variable input and assuming a translog production function, we can derive current productivity as a function of our parameters via equation (8):

$$\omega_{it}(\beta) = \hat{\phi}_{it} - \beta_l l_{it} - \beta_k k_{it} - \beta_{ll} l_{it}^2 - \beta_{kk} k_{it}^2 - \beta_{lk} l_{it} k_{it}. \tag{11}$$

Using this, we can derive an expression for the unobserved productivity shock as a function of our production function parameters. Last period's input decisions should be highly correlated, but independent, of this period's input decisions. Thus, we can use these as instruments and form the following moments

$$E\left(\xi_{it}(\beta)\begin{pmatrix} l_{it-1} \\ k_{it} \\ l_{it-1}^2 \\ k_{it}^2 \\ l_{it-1}k_{it} \end{pmatrix}\right) = 0. \tag{12}$$

and then use General Method of Moment (GMM) estimation techniques to recover consistent estimates of our production function parameters. With these in hand, we can construct consistent estimates of our index of productivity and markups.

[10] The monotonicity of material demand in productivity is a condition that holds for a broad class of models of imperfect competition as discussed in DLW.

A few observations are worth noting. First, we re-iterate that these methods are general enough to apply across a broad range of heterogeneous industries. We only require assumptions of cost minimization and some basic functional forms for the production function. However, following previous studies using this technique, we do not assume common production function parameters across all plants in our sample, but estimate separate parameters for each NAICS 3-digit sector. The method above also assumes production of a single product. A recent paper by De Loecker et al. (2012) highlights the complications when applying these techniques to multiproduct firm because one often has only information on a firm's total input usage, not input usage by each product it produces. Here, we have plant-level, rather than firm-level data, which allows us to largely avoid the issue. The vast majority of plants are single-product or very close, and our results are robust to whether we exclude the small share of plants that have substantial production in more than one product. A final note is that our main sample has data at five-year intervals, which could lead to a weak correlation of our (lagged) instruments. However, as described below, we also find our results to be robust to using a smaller sample where we have annual observations.

3.2. DID estimation of markup and productivity effects of M&A

In our second stage of estimation, we identify the effects of M&A activity on plant-level productivity and markup measures through a DID strategy. There are a number of features of our data that we must address in how we devise our particular DID strategy. The main aspect is that our data are repeated observations on plants at three five-year intervals: 1997, 2002 and 2007. In order to ensure that we have at least one observation of acquired plants before the acquisition and one after the acquisition, we focus on acquisitions that occurred between 1998 and 2006. As a result, the length of time since the acquisition has occurred can vary across observations. We show an example in Figure 1, where an acquisition occurs in the year 2000. Plants involved in the acquisition will be coded as not subject to an M&A in the first year of our sample 1997, but then coded as subject to M&A in 2002 and 2007. As one can see, this structure means that we are estimating M&A effects for the average period since the plant was acquired.[11]

We estimate our DID specification with two control groups. In order to provide a baseline, the first is simply all other plants in the sample that are not acquired. Of course, there is the real concern of sample selection bias with this relatively generic control group. As mentioned, using plant data may substantially mitigate selection bias because M&A decisions are made at the (multi-plant) firm-level, not the plant level. Past studies of M&A effects have often controlled for this sample selection bias using propensity score methods.[12] We leave this analysis for future drafts of this paper, but also note that this strategy is effective only to the extent that one can eliminate such bias through observables.

Instead, we estimate M&A effects using a novel control group – plants that were part of an announced M&A deal that was not ultimately completed. Such plants share all the same attributes–observed and unobserved–that lead them to be targeted for an M&A transaction, a relatively infrequent event. And the number of failed deals is not

[11] We have experimented with estimating different lag lengths, but do not find much variation in effects over time subsequent to the acquisition.

[12] Such studies include Heyman, Sjöholm, and Tingvall (2007), Bertrand and Zitouna (2008), Arnold and Javorcik (2009), Bandick and Görg (2010), and Fresard, Hoberg, and Phillips (2013).

trivial. For example, Branch and Yang (2003) show that about 11% of the over 1000 U.S. mergers they evaluate over the 1991-2000 period fail to complete. A key worry with this control group strategy is that there are also factors unobserved to us that become observed to the involved firms (especially with respect to the target firm) that lead to a M&A deal failure. There is a small literature that evaluates failure of M&A deal completion.[13] Most of the covariates with explanatory power are solely related to the acquiring firm, including the type of financing it uses to fund the deal, its size, and measures of their attitude toward completing the deal, which Baker and Savasoglu (2002) indicate is the best single predictor of deal completion. There isn't any obvious correlation between these factors related to the acquiring firm and the future market power and/or productivity of the target plants, which is the focus of our study. The media often reports that disputes between managers of the two firms–often termed "social issues" –can lead to failed M&A deals.[14]

4. Results

4.1. First-stage estimates of markups and productivity

We begin by using the methods in DLW as described in section 3.1 to estimate a markup and a measure of productivity for each of the 187,100 manufacturing plants in our full sample. Table 1 provides the mean and standard deviation of these DLW measures of markup and productivity. The measure of DLW productivity is simply an index from a

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[13] Additional studies beyond those listed in the text includes Mitchell and Pulvino (2001), Officer (2007), and Branch, Wang, and Yang (2008)

[14] The inherent problem is that there are two sets of all senior managers coming into an M&A and this duplication must be eliminated. Willis, A. (2001) "'Social issue' may be key to bank mergers," *The Globe and Mail (Canada)*, August 28, p. B17 is an example article in the business press on this issue.

translog production function. Thus, its level is not meaningful *per se*, but changes in the index will reflect percentage changes in the plant's productivity. As one can see, there is substantial variation in this productivity measure across plants.

The markups measure how much more is the price charged by the firm than its marginal cost. We find quite high markups, with an average markup (the ratio of price to marginal cost) of around 5.5 with a standard deviation over 7. The range of these markups is much higher than what DLW find for Slovenian firms, and about twice as high on average as that found by De Loecker et al. (2012) for Indian firms. However, an important difference is that we estimate these markups at the plant-level, not the firm-level. While the price is what the *firm* can charge, the (marginal) costs are specific to the *plant*. Inputs used and costs incurred by the firm for headquarter services, such as advertising, distribution, central management and R&D, will typically not be accounted for in these production plants. In other words, we should expect larger markups at the plant-level because the price has to only cover the plant's costs, but the firm's non-production costs as well. Therefore, like our DLW productivity measure, we are not as interested in the level *per se*, but how the markup changes with an M&A.

For comparison purposes, we will be examining the impact of M&A on more traditional revenue productivity measures; specifically, the log of total factor productivity, which we calculate using the same methodology as Foster, Haltiwanger and Syverson (2008), and a simple measure of log labor productivity. Table 1 also provides the mean and standard deviation of these measures.

For the purposes of controlling for potential sample selection bias, we employ a sample of only the plants where a M&A deal was announced. This reduces our sample substantially to 4,200 plants. The last two columns of Table 1 provide the mean and standard deviation of our markup and productivity parameters for this sample of plants. There are not large differences in these descriptive statistics between this reduced sample and the full sample, though both average markup and all three productivity measures are slightly higher in the smaller sample of plants subject to an announced M&A deal, suggesting that there is targeting of firms with plants that have higher than average markups and productivity.

4.2. Second-stage DID estimates of M&A effects: Baseline estimates

We now take our first-stage estimates and use a DID specification to examine the impact of M&As on these three measures of productivity and the DLW estimate of markups. Following a standard DID, our independent variable of interest is an interaction between an indicator variable that a plant has been "treated" by being subject to an M&A and an indicator that it is the period after treatment has taken place. We label this variable as the "Target Firm in the Post-M&A Period." We also include the indicator variable for the "Post M&A Period" to control for any secular trends, year fixed effects to capture macroeconomic shocks affecting all plants identically in a particular year, and plant fixed-effects, which control for any time invariant factors (observed or unobserved) that affect an individual plant's productivity or markup.

Table 2 provides results for our full sample of plants. For the full sample, we only look at the long difference of our sample, including only the years 1997 and 2007, to avoid concerns about ambiguity of defining when a control plant is in a "post M&A period." The first two columns of Table 2 provide estimates of the impact of M&A on traditional revenue-based measures of productivity, log revenue TFP and log labor productivity. We estimate a positive, but statistically insignificant, M&A effect on log revenue TFP, but a positive and statistically significant M&A effect on our measure of labor productivity. As mentioned, these traditional revenue-based measures can confound changes in market power with changes in true productivity.

To address this, the second two columns of Table 2 provide results when we apply our DID exercise to our first-stage estimates where we estimate separate estimates of a measure of productivity and markup for each plant. Now we see a sharp divergence. There is a negative, but statistically insignificant, effect on productivity when a plant has been acquired, but a statistically significant increase in markup. The magnitude of the markup effect is sizeable as well, with the 1.167 effect corresponding to roughly a 20% increase relative to the mean markup.

4.3. Second-stage DID estimates of M&A effects: Announced M&A sample

Our baseline estimates do not control for sample selection issues, other than through inclusion of plant-level fixed effects, which capture time-invariant characteristics. However, plants of firms targeted for M&A may have unobserved attributes that will affect changes in future markups and productivity that could spuriously show up as M&A effects. To address this potential for selection on unobservables we construct a novel control group.

Specifically, this alternate control group consists of plants in firms that were announced as targets of M&A, but for which the merger was ultimately withdrawn. By definition, these plants possess the attributes necessary to lead to an announced merger, eliminating a portion of potential sample selection bias. One consequence of restricting the control group to the set of firms with withdrawn mergers is that it substantially reduces our sample size from over 187,000 observations to just 4,200 observations.

Estimates obtained with this alternate control group are qualitatively very similar to those obtained with the baseline sample. As shown in the first two columns of Table 3, our revenue-based productivity measures continue to show a positive M&A effect. Coefficient estimates for the merger effect variable are statistically significant for both revenue-based measures, though the effects are of fairly modest magnitudes – less than 5% of the average productivity. Moreover, we obtain the same qualitative results for the DLW measures on productivity and markups with this announced M&A sample and the full sample – there is no discernible M&A effect on the productivity of acquired plants, but substantial M&A effects on their markups. Here, the coefficient estimates suggest a much larger markup effect of something around 60% of the average markup in the sample. Moreover, the results indicate that even after our initial control for sample selection, we continue to find that the effect of mergers on revenue productivity measures is overstated for the average plant in the manufacturing sector, due to the increase in markups.[15]

We conduct several additional robustness exercises with this sample of plants that are subject to an announced M&A deal. First, to confirm that our results are not driven by spurious matches between the Thomson Financial and Census data, we construct a sample composed only of firms with an exact match between the two

[15] Kulick's (2015) examination of horizontal mergers in the cement industry also finds evidence of over-estimated effects of mergers on revenue productivity, using an entirely different estimation procedure.

databases (i.e. matching on firm name, address, city and state). Requiring the stricter criterion of an exact match gives more certainty of the match quality, but also reduces our sample by a fair amount. In Table 4 we show results when we limit our announced M&A sample to only those observations that meet a strict match criterion. This limits the number of observations even further from 4,200 to just 1,900. Nevertheless, the estimates are qualitatively identical to those in Table 3 with the less strict match criteria.

The second robustness check involves a second technique to control for sample section bias via a 2SLS strategy that that instruments for whether an announced deal is completed or not. In particular, we instrument for M&A completion by interacting the "Post M&A Period" variable with indicator variables for the year the M&A deal was announced for the plant. The idea is that secular trends, such as business cycles and changes in antitrust enforcement in the year that the M&A deal is announced could affect whether the deal is completed. The results of these 2SLS estimates, reported in Table 5, are qualitatively very similar to the OLS estimated M&A effects.

In Table 6, we explore a possible source of heterogeneity in our results. In particular, one might think that the M&A effect on markups would be greater for targets that already have significant market share for the product they produce. We examine this by interacting the target firm's market share in the plant's product with our DID regressors. As Table 6 shows, there is an estimated positive coefficient on the interaction of "Target Firm in Post M&A Period" with "Firm Market Share" in the markup equation, but it is not statistically significant. These interactions are statistically insignificant in the other equations except for the log revenue TFP equation, where this same focus interaction does have a positive and statistically significant coefficient. The weak statistical results may be due to low power from a relatively small sample size.

A final robustness check is examination of only the plants in the announced M&A sample for which we have a balanced panel of annual data. While the Census of Manufactures data is gathered on five-year intervals, the Annual Survey of Manufactures (ASM) gathers data on larger plants annually. Use of annual data may be helpful for DLW estimation methods, as noted above, and also allows for more precise coding of the timing of M&A announcements in our data. Because the sample frame for the ASM changes every five years, we restrict attention to the set of plants that are present in every year, to avoid spurious instances of entry and exit. This restriction reduces the announced M&A sample from 4,200 to 3,300. Table 7 provides results for this sample of annual observations on larger plants. Once again, we obtain qualitatively identical estimates.

In summary, our results are robust to a number of alternative specifications meant to address various concerns one may have with our estimates. They give a consistent message that acquired plants do not see statistically significant effects on productivity, but do experience positive significant effects on markups. We think these results are surprisingly robust given the small number of observations in our announced M&A sample. Future drafts will explore a couple other alternative control-group strategies to provide further robustness exercises.

4.4. Second-stage DID estimates of M&A effects: A placebo test

A final concern with our analysis is the worry that pre-existing secular trends for the treatment and control group could be driving spurious correlations for our estimated M&A effects. To address this concern we construct the following placebo test. We add data for Census years 1987 and 1992 to our announced M&A sample and then add indicator variables that flag our targeted plants in 1992 and 1997. These are years when these targeted plants were not yet acquired; hence, these are the placebo treatments. If there are significant coefficients on these placebo treatments, it would cast substantial doubt on the validity of the estimate effect we are obtaining for the true treatment variable. Relatedly, if significant, these variables would indicate pre-trend differences in our treated and control groups, which is a major concern for any DID analysis. In other words, this particular placebo test is also a test for pre-trend differences.

As results in Table 8 show, our estimates easily pass this placebo and pre-trend test. The first four columns show the results when we include the placebo variables, "Target Firm in 1992" and "Target Firm in 1997." Estimated coefficients on these variables are statistically insignificant for all three measures of productivity and our markup measure. This sample of observations is somewhat different from our previous samples discussed above due to the additional years of 1987 and 1992. So columns 5 through 8 of Table 8 verify that our base specification yields qualitatively identical results to our previous analysis showing no M&A effects on DLW productivity, but significant, positive M&A effects on markups.

5. Conclusion

This paper estimates the effects of mergers and acquisitions on productivity and markups of plants across all U.S. manufacturing industries. Our analysis makes use of high-quality U.S. Census Bureau data covering the universe of U.S. manufacturing plants, which are matched to the set of private and public mergers and acquisitions tracked by Thomson Financial in their SDC Platinum database. Using a novel technique developed by De Loecker and Warzynski (2012), we find that controlling for market power effects is important when estimating the effect of M&A on firm and plant-level productivity. In particular, we find that while M&A is associated with a statistically significant increase in revenue-based productivity measures, there is also positive and significant effect on markups. We find no effect of M&A on a productivity measure that controls for this change in market power.

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FIGURE 1: Data and Coding of M&A Treatment

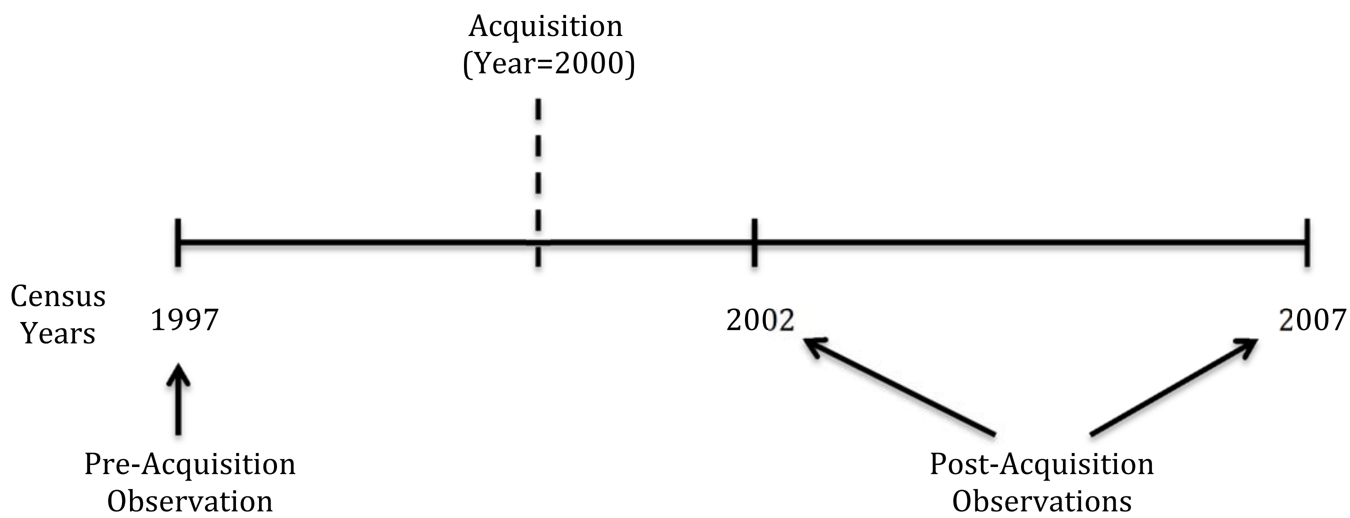

Table 1: First-stage measures of markups and productivity

Variable	Full Sample: Mean	Full Sample: Standard Deviation	Sample With Only Announced M&A Deals: Mean	Sample With Only Announced M&A Deals: Standard Deviation
Log revenue total factor productivity	4.07	0.60	4.38	0.63
Log labor productivity	4.33	0.65	4.69	0.69
DLW productivity measure	-0.74	1.87	-1.67	2.57
DLW markup	5.49	7.97	7.20	11.27

Notes: We calculate total factor productivity at the 3-digit NAICS level using the same methodology as in Foster, Haltiwanger, and Syverson (2008). The DLW markup and productivity measures are estimated at the 3-digit NAICS level using the techniques in De Loecker and Warzynski (2012). There are 187,100 observations in the full sample and 4,200 observations in the sample that only includes plants that were part of announced M&A deals.

Table 2: Baseline Results with Full Sample of Plants

Variables	Log Revenue TFP	Log Labor TFP	DLW Productivity	DLW Markup
Target Firm in Post M&A Period	0.022	0.044*	-0.063	1.167***
	(0.016)	(0.024)	(0.060)	(0.215)
Post-M&A Period	0.067***	0.161***	-0.322***	0.557***
	(0.002)	(0.002)	(0.004)	(0.025)
Constant	4.031***	4.235***	-0.556***	5.154***
	(0.001)	(0.002)	(0.003)	(0.018)
Observations	187,100	187,100	187,100	187,100
R-squared	0.89	0.82	0.93	0.88

Notes: Robust standard errors in parentheses. Estimates for plant and year fixed effects are suppressed. P-values of 0.01, 0.05, and 0.10 are denoted by ***, **, and * respectively.

Table 3: Baseline Results Where Control Plants are from Withdrawn M&A Deals (Reduced Sample)

Variables	Log Revenue TFP	Log Labor TFP	DLW Productivity	DLW Markup
Target Firm in Post M&A Period	0.150***	0.123*	-0.078	4.638**
	(0.057)	(0.070)	(0.112)	(2.320)
Post-M&A Period	-0.158***	-0.125*	0.156	-4.237*
	(0.058)	(0.071)	(0.121)	(2.314)
Constant	4.358***	4.590***	-1.477***	6.442***
	(0.009)	(0.013)	(0.028)	(0.156)
Observations	4,200	4,200	4,200	4,200
R-squared	0.85	0.73	0.90	0.84

Notes: Robust standard errors in parentheses. Estimates for plant and year fixed effects are suppressed. P-values of 0.01, 0.05, and 0.10 are denoted by ***, **, and * respectively.

Table 4: Reduced Sample with Stricter Match Criteria

VARIABLES	Log Revenue TFP	Log Labor TFP	DLW Productivity	DLW Markup
Target Firm in Post M&A Period	0.283***	0.181*	-0.011	2.696***
	(0.076)	(0.097)	(0.153)	(0.626)
Post-M&A Period	-0.320***	-0.265***	0.108	-1.928***
	(0.078)	(0.101)	(0.163)	(0.684)
Constant	4.364***	4.562***	-1.474***	5.752***
	0.014	0.019	0.037	0.146
Observations	1,900	1,900	1,900	1,900
R-squared	0.83	0.66	0.90	0.79

Notes: Robust standard errors in parentheses. Estimates for plant and year fixed effects are suppressed. P-values of 0.01, 0.05, and 0.10 are denoted by ***, **, and * respectively.

Table 5: 2SLS Estimates with Reduced Sample

Variables	Log Revenue TFP	Log Labor TFP	DLW Productivity	DLW Markup
Target Firm in Post M&A Period	-0.079	0.292	-0.183	6.222**
	(0.164)	(0.241)	(0.558)	(2.996)
Post-M&A Period	0.054	-0.281	0.252	-5.699**
	(0.153)	(0.225)	(0.520)	(2.793)
Constant	4.357***	4.591***	-1.478***	6.452***
	(0.009)	(0.012)	(0.029)	(0.155)
Observations	4,200	4,200	4,200	4,200

Notes: Robust standard errors in parentheses. Estimates for plant and year fixed effects are suppressed. P-values of 0.01, 0.05, and 0.10 are denoted by ***, **, and * respectively.

Table 6: Interactions with Firm Market Share

Variables	Log Revenue TFP	Log Labor TFP	DLW Productivity	DLW Markup
Target Firm in Post M&A Period	0.115*	0.095	-0.138	4.515*
	(0.061)	(0.075)	(0.119)	(2.392)
Post-M&A Period	-0.127**	-0.100	0.195	-4.153*
	(0.062)	(0.076)	(0.127)	(2.383)
Target Firm in Post M&A Period × Firm Market Share	1.283***	1.031	2.229	4.611
	(0.461)	(0.715)	(1.762)	(10.479)
Post-M&A Period × Firm Market Share	-1.123**	-0.94	-1.425	-3.051
	(0.439)	(0.671)	(1.688)	(10.298)
Constant	4.358***	4.590***	-1.477***	6.443***
	(0.009)	(0.013)	(0.028)	(0.156)
Observations	4,200	4,200	4,200	4,200
R-squared	0.85	0.73	0.90	0.84

Notes: Robust standard errors in parentheses. Estimates for plant and year fixed effects are suppressed. P-values of 0.01, 0.05, and 0.10 are denoted by ***, **, and * respectively.

Table 7: Results from Reduced Sample with Annual Data

Variables	Log Revenue TFP	Log Labor TFP	DLW Productivity	DLW Markup
Target Firm in Post M&A Period	-0.047	-0.084	-0.432***	2.815***
	(0.070)	(0.077)	(0.118)	(1.091)
Post-M&A Period	0.046	0.053	0.377***	-2.408**
	(0.069)	(0.075)	(0.134)	(1.078)
Constant	4.554***	4.887***	3.881***	4.969***
	(0.015)	(0.023)	(0.125)	(0.194)
Observations	3,300	3,300	3,300	3,300
R-squared	0.90	0.83	0.99	0.76

Notes: Robust standard errors in parentheses. Estimates for plant and year fixed effects are suppressed. P-values of 0.01, 0.05, and 0.10 are denoted by ***, **, and * respectively.

Table 8: Placebo Regressions Using Data from 1987 through 2007

Variables	Log Revenue TFP	Log Labor TFP	DLW Producti-vity	DLW Markup	Log Revenue TFP	Log Labor TFP	DLW Producti-vity	DLW Markup
Target Firm in Post M&A Period	0.102*	0.114	-0.196**	2.249**	0.103*	0.102	-0.262***	2.425**
	(0.062)	(0.076)	(0.092)	(1.122)	(0.057)	(0.071)	(0.082)	(0.988)
Post M&A Period	-0.137**	-0.097	0.220**	-1.680	-0.138**	-0.088	0.276***	-1.822*
	(0.062)	(0.077)	(0.098)	(1.106)	(0.058)	(0.072)	(0.095)	(0.984)
Target Firm in Year 1992	0.052	0.062	0.124	-1.397				
	(0.061)	(0.086)	(0.091)	(0.996)				
Target Firm in Year 1997	-0.044	-0.010	0.125	0.541				
	(0.052)	(0.070)	(0.104)	(0.933)				
Constant	4.205***	4.115***	-0.416***	4.013***	4.205***	4.115***	-0.416***	4.017***
	(0.011)	(0.016)	(0.032)	(0.105)	(0.011)	(0.016)	(0.032)	(0.105)
	6,200	6,200	6,200	6,200	6,200	6,200	6,200	6,200
	0.81	0.71	0.89	0.75	0.81	0.71	0.89	0.75

Notes: Robust standard errors in parentheses. Estimates for plant and year fixed effects are suppressed. P-values of 0.01, 0.05, and 0.10 are denoted by ***, **, and * respectively.