Evidence for the Effects of Mergers on Market Power and Efficiency*

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Abstract

Study of the impact of mergers and acquisitions (M&As) on productivity and market power has been complicated by the difficulty of separating these two effects. We use newly-developed techniques to separately estimate productivity and markups across a wide range of industries using detailed plant-level data. Employing a difference-in-differences framework, we find that M&As are associated with increases in average markups, but find little evidence for effects on plant-level productivity. We also examine whether M&As increase efficiency through reallocation of production to more efficient plants or through reductions in administrative operations, but again find little evidence for these channels, on average. The results are robust to a range of approaches to address the endogeneity of firms' merger decisions.

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1 Introduction

Merger and acquisition (M&A) activity by firms is a widespread and economically important phenomenon in the global economy, involving over four trillion dollars of worldwide assets annually.[1] In turn, this phenomenon leads to a substantial reallocation of economic activity among firms over time, especially for advanced economies. Maksimovic and Phillips (2001), for example, 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 issue is the potential tradeoff between increased market power versus efficiency gains in the wake of a M&A transaction. While changes in market power and efficiency due to M&As have important implications for welfare, estimating these effects empirically is difficult. Prior studies estimating effects of M&As across industries have been hampered by an inability to separately estimate market power and productivity effects. Case studies of specific firms or industries have attempted to disentangle these effects using detailed data or specific circumstances, but they may not be generally representative.

This paper combines an analysis of M&As across all U.S. manufacturing industries with a new approach to estimate efficiency and market power. We begin by applying techniques recently developed by De Loecker and Warzynski (2012) to separately estimate productivity and markups in a unified framework with minimal structural assumptions – only cost minimizing behavior is needed. We generate these estimates using plant-level data from the U.S. Census Bureau covering the entire manufacturing sector over the 1997 to 2007 period. These data are linked to information on M&A activity from the SDC Platinum database maintained by Thomson Reuters.[3] Importantly, each of these datasets includes data for both publicly traded and privately owned firms.

We use a differences-in-differences (DID) approach to identify the effects of M&A on

[1]The *Wall Street Journal* recently reported that the value of global M&A activity in 2015 was the highest on record, exceeding US$ 4.3 trillion (Farrell 2015).

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

[3]Around 50% of all U.S. M&As are in the manufacturing sector over recent decades, including our sample period from 1997 to 2007.

acquired plants relative to several potential comparison groups. Our analysis includes a rich set of fixed effects including plant, industry by year, and plant size by year fixed effects. These controls isolate the effect of M&As by capturing time-invariant plant characteristics, as well as shocks that affect plants in particular industries or size classes in a given year.

We find that M&As significantly increase markups on average, but have no statistically significant average effect on productivity. The magnitude of the markup increase is economically significant as well: The increase in markups for acquired plants relative to non-acquired plants ranges from 15 percent to over 50 percent of the average markup in the sample. These results are robust to comparison of acquired plants to several conceptually different control groups, and to different criteria for linking the M&A and Census data. We explore whether the M&A effects we estimate differ for horizontal M&As, where the merging firms are in the same industry, versus other types of M&As. We find evidence that markup effects of M&As are strongest with horizontal M&As as one might expect. We also find some evidence that M&As have positive impacts on plant-level productivity for M&As that are not horizontal.

We pursue several approaches to address the difficult issue of the endogeneity of firms' merger decisions with respect to current and expected changes in productivity and market power, a concern that is often not considered in earlier work on M&A effects. Our primary method is to compare acquired plants to plants in three separate control groups using our DID framework. The first control group consists of plants selected by propensity score matching procedures. While this is a familiar strategy in other settings, its use has been relatively limited in studies examining the effect of M&As on productivity and market power. The second control group is composed of plants for which an acquisition is announced, but never completed. Such plants have all the attributes necessary to lead to an announced acquisition, eliminating a portion of potential sample selection bias.[4] The third control group is made up of plants that will be acquired in subsequent years. This is a valid strategy if the attributes that make a plant more likely to be a target exist for a few years before a successful match with an acquirer is made. In addition to consideration of these three control groups, we employ an instrumental variables strategy, in which we instrument for successful merger completion – relative to announced mergers that are not completed – using interactions

[4]The identifying assumption for this control group 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.

of year dummies and the post-merger indicator to capture possible regulatory changes that may affect M&A activity.

Each of these approaches has strengths and weaknesses, but by showing that the results are robust – in terms of sign, significance and magnitude – across a range of conceptually different approaches, they offer compelling evidence that the results are not driven by statistical bias. We also note that an additional advantage of our data with respect to addressing potential endogeneity is that it is plant-level, whereas M&A decisions are typically made at the firm-level. Many of the firms in our sample have multiple plants, 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.[5]

M&As may have efficiency effects other than those observed via changes in the productive efficiency of existing manufacturing plants. Our data allow us to explore two other ways in which M&A activity may have efficiency effects. First, an acquiring firm may reallocate resources within 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. Second, efficiency gains from M&A may arise through realization of economies of scale with nonmanufacturing activities of the firm (management, marketing, advertising, etc.) after an M&A. We are able to examine evidence for these additional efficiency effects using data available to us.

With regards to firm-level efficiency effects through reallocations of activity across plants, we undertake two analyses. First, we aggregate our plant-level data to the firm level and then use an analogous DID estimation to estimate M&A effects. As in the plant-level results, we find no effects of M&A on productivity at the firm-level, though the positive effect of M&As on markups is also less precisely estimated at the firm-level. Second, we use a DID framework to determine whether lower productivity plants are more likely to exit after an M&A and find no evidence for this channel.

With regards to economies of scale for non-manufacturing activities of the firm, we examine the relationship between M&A and employment at the non-manufacturing establishments of the target firm using data from the Longitudinal Business Database (LBD). Specifically, we examine whether M&A allows for a reduction in employment for non-manufacturing establishments as the fixed costs of tasks such as accounting and marketing are consolidated in one firm. We find no evidence for efficiency gains

[5]We also present a placebo test that verifies that pre-existing trends for merging versus non-merging plants are not driving the results, a common concern with DID estimation.

operating through this non-manufacturing employment channel.

In summary, we find evidence that M&As increase markups on average across U.S. manufacturing industries, but find little evidence for channels often mentioned as potential sources of productivity and efficiency gains. We view our finding of a positive markup effect, on average, as novel given the heterogeneous motivations for M&As and the fact that the effect is estimated across a broad set of plants, firms and industries.

Our work builds on several approaches that have been used to estimate potential effects of M&A activity, each 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 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 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 (primarily airlines, banking, hospitals, and petroleum), because these are the sectors for which researchers can find detailed firm- and product-level price data.[6] 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 of M&A activity, they have limitations. They typically focus on high profile acquisitions, making it more likely that their results suffer selection bias and are 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

[6] 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), cement (Hortacsu and Syverson, 2007), and cotton spinning (Braguinsky et al. 2015).

a few exceptions, data are not available to estimate efficiency effects of M&A activity.[7]

There have been only 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 et al. (2003), and Bertrand (2008). With the exception of Gugler et al. (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. Gugler et al. (2003) use firm-level accounting data for publicly traded firms in Compustat to examine the effect of mergers on profitability and sales, and then use these results to draw inferences about efficiency, finding mixed results depending on whether mergers are horizontal or conglomerate. 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.[8]

The remainder 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 use of these measures as dependent variables in a DID framework. Next, we present and discuss our main results, as well as a number of specifications with alternative control groups and other robustness analyses. Finally, we provide evidence on the effects of M&As on other possible channels of efficiency gains – reallocation of production across plants within the firm and rationalization of headquarter services – before concluding.

[7]The exceptions of which we are aware are Jaumandreu (2004), which found some efficiency effects from M&A activity in the Spanish banking industry, Braguinsky et al. (2015), which examines the effect of acquisitions on productivity and profitability in the Japanese cotton spinning industry, and Kulick (2015), which finds positive effects on both prices and efficiency resulting from horizontal mergers in the concrete industry using output data measured in physical units of quantity.

[8]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.

2 Data

To provide estimates of the effects of M&A across a broad set of industries, we make use of two rich datasets covering the entirety of the manufacturing sector. 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.[9] The CM is conducted every five years, in years ending in 2 and 7, and this analysis employs data from the 1997, 2002, and 2007 CMs.

Our second dataset, Thomson Reuters's SDC Platinum database (SDC), contains information on merger and acquisition transactions involving both publicly-traded and private firms.[10] For each transaction, SDC provides data for merger target firms including the name, address and major industry, along 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 announced but later withdrawn. As mentioned above, we use the set of firms involved in these withdrawn mergers as a control group in some portions of the analysis. Finally, the SDC data allow us to observe the timing of mergers with more precision than is possible with only Census data, and the use of Census data allows us to include data for privately owned firms, which is not possible with datasets of publicly traded firms such as Compustat.

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. The sample is further restricted to mergers in which the acquirer is a manufacturer to avoid transactions such as buyouts by financial firms. Lastly, we 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.

[9] As is standard in research using the CM, we omit observations from so-called "administrative records" for which much of the data is imputed.

[10] Thomson Reuters collects these data from governmental regulatory filings, media reports, and reporting arrangements with investment banks. The resulting dataset theoretically includes data for all M&A transactions.

In terms of time period, we consider 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 lower 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.

Because the CM data are available in five year increments, the length of time since an 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 will estimate M&A effects for the average period since the plant was acquired.[11]

The merger transaction data in SDC are linked to the CM data via a name and address matching procedure, where the Census Bureau's Business Register is used as a bridge. Our matching procedure is similar to others that have been used to link firm-level information to Census data via name and address matching (see e.g. Davis et al. 2014). In our baseline results, we consider firms to be matched if one of three criteria is met: 1) There is an exact match in both the firm name and address; 2) There is an exact match in the firm name, and the city and state in both datasets; 3) There is an exact match of the firm address, and at least two words in the firm name match. While any name and address matching procedure is necessarily imperfect, we perform robustness checks to ensure that the details of the matching procedure are not driving the results. In particular, we obtain qualitatively similar results when we limit the sample to the set of observations with both exact name and address matches, and when we expand the sample to include firms with less exact name and address matches, provided they are determined to be accurate by a research assistant's manual review. Our matching procedure yields successful matches to the CM for 51 percent of manufacturing mergers in the SDC data.[12]

[11] We have experimented with estimating differential effects of mergers based on the time that has passed since acquisition, but have found little evidence for heterogeneity in this dimension.

[12] The set of mergers from which coefficient estimates are identified is smaller as it is limited to mergers in which an entire firm is acquired, and plants that are present both before and after the merger takes place.

3 Empirical Framework for Estimating Markups and Productivity

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) and De Loecker (2011), 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.

We follow the framework developed by De Loecker and Warzynski (2012) – DLW hereafter – to separately identify a plant's markup from its productivity.[13] Using their notation, we begin with a production function

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

where $X_{\text{it}}^{1}, \ldots, X_{\text{it}}^{V}$ 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, one can write the associated Lagrangian,

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

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

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

Rearranging the first-order conditions, one can write:

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

[13] 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 $\mu_{\text{it}} \equiv \frac{P_{\text{it}}}{\lambda_{\text{it}}}$, 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, DLW note that one can express the plant's markup as a surprisingly simple function of these two elements:

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

In order to obtain consistent estimates of the production function parameters, DLW follow the methods proposed by Ackerberg, Caves, and Frazier (2006). For tractability, they 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):

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

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

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

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 follow DLW in assuming 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: $m_{\mathrm{it}} = m_t\left(k_{\mathrm{it}}, \omega_{\mathrm{it}}, \mathbf{z}_{\mathrm{it}}\right)$. Inverting this function yields

$$\omega_{it} = h_t\left(m_{it}, k_{it}, \mathbf{z}_{it}\right), \tag{9}$$

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, $\mathbf{z}_{\mathrm{it}}$.

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

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

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

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

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

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

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.

A few observations are worth noting. First, we re-iterate that this method is general enough to apply across a broad range of heterogeneous industries. It only requires 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. (2016) highlights the complications when applying these techniques to multiproduct firms 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. Production for most plants is highly concentrated in a single product, and our results are robust to whether we exclude plants that have substantial production in more than one product.

4 Empirical Strategy and 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 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 indicated by the sizable standard deviations, 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. (2016) for Indian firms. However, an important difference is that we estimate these markups at the plant-level, rather than the firm-level as in those papers. 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 not only cover the plant's costs, but also the firm's non-production costs. 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 purpose of controlling for potential sample selection bias, one method we employ is a sample of only the plants where an M&A deal was announced, using plants of M&A deals that subsequently fail as our control group. 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.[14] 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 these first-stage estimates and use a DID specification to examine the impact of M&As on the three measures of productivity and the DLW estimate of markups. Our estimating equation can be expressed as:

$$y_{it} = \beta_1 Post_t \times Target_i + \beta_2 Post_t + \theta_i + \sum_j \gamma_j \times \tau_t + \alpha + \varepsilon_{it}. \tag{13}$$

The dependent variable, y_{it}, is the markup or productivity measure calculated as described above. The first term on the right-hand side is the DID term of interest, the interaction of an indicator for the post M&A period (Post_t) and an indicator for plants that are merger targets (Target_i). The coefficient β_1 denotes the effect of M&A

[14]As robustness checks, we employ a number of other strategies to control for endogenous selection that lead to other alternative samples. Due to disclosure concerns, we have refrained from reporting descriptive statistics for other samples beyond these two.

on productivity or markups.[15] Time-invariant plant-level characteristics are controlled for by plant-fixed effects (θ_i). Effects that are specific to the post-merger periods are captured by including the level of the variable Post_t.[16] Finally, as in Davis et al.'s (2014) study of the effect of private equity purchases, we include a set of fixed effects that interact year dummies with indicators for plant characteristics ($\sum_j \gamma_j \times \tau_t$), specifically year x (3-digit NAICS) industry and year x plant size category. This approach yields what we believe to be a tight identification that nets out *time-varying* changes in outcomes associated with industry, size and age categories, thus controlling for a wide range of potentially omitted variables. Unless otherwise indicated, standard errors are clustered at the firm-level, as the decision to merge is made at that level.

Table 2 provides results for our full sample of plants, where all non-merging plants form a control group for comparison to merging plants.[17]

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 positive, but statistically insignificant, M&A effects on both log revenue TFP and log revenue 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 from estimating equation (13) for the separate estimates of a measure of productivity and markup for each plant, calculated as in DLW. The results reveal important information. Like the other two productivity measures, we also estimate a statistically insignificant effect of M&A on the DLW productivity measure. However, the estimated M&A effect on the DLW markup measure is positive and statistically significant at the 1% level. The magnitude of the markup effect is also sizeable in economic terms, with the coefficient estimate of 0.706 corresponding to an increase for acquired plants relative to non-acquired plants that is equivalent to 13% relative to the average markup.

[15] In our tables of results, we label this interaction variable, "Target Firm in the Post M&A Period."

[16] The coefficient for the "Post M&A Period" indicator can be separately identified from the other fixed effects because the post-M&A period begins in different years for different plants, depending on the timing of their acquisition.

[17] Because we cannot define a specific year between 1997 and 2007 in which non-merging plants can be classified as "post-merger," for this full sample, we only consider the years 1997 and 2007, which can be unambiguously defined as belonging to the pre- or post-merger period, respectively. This restriction is not required in subsequent samples that we consider.

4.3 *Second-stage DID estimates of M&A effects: Propensity score matching*

Our baseline estimates do not control for sample selection issues, other than through inclusion of the fixed effects that capture time-invariant characteristics of plants and time-varying characteristics of industry and plant size bins. The concern is that acquiring firms find targets that are trending towards higher future productivity and/or markups so that an increase in these variables after the M&A could be spuriously assigned to an M&A effect, when none exists. Of course, the bias could also go the other way, mitigating any positive M&A effect, if firms are more likely to acquire targets with productivity or markups trending downward. Recent evidence by Blonigen et al. (2014) suggests that negative trends in these variables may be more likely.

One way to address this type of sample selection bias is through use of propensity score matching (PSM), where one forms a control group of "untreated" observations that are most similar to treated observations in terms of observables. PSM has been used in a wide variety of settings to control for sample selection bias, including a number of studies estimating the effects of M&As on firms and plants.[18]

The PSM procedure that we implement first runs a logit regression for all plants in the sample to estimate the probably that a plant will be acquired in a particular time period. We include a set of baseline covariates related to both the plant's characteristics and the characteristics of the firm to which the plant belongs.[19] In particular, we include log revenue TFP, log wage, log capital intensity (capital/labor ratio), log skill intensity (other employment/total employment ratio), firm age, an exporting indicator variable and 3-digit NAICS dummies. Predicted values from this first step generate a propensity score for each observation. "Treated" observations (i.e., acquired plants) are then matched to observations with the nearest propensity scores (i.e., a "neighbor") that serve as controls and an average treatment estimate (ATE) is calculated.

The first row of Table 3 provides the ATE for our variables of interest when merger targets are matched to a single nearest neighbor via the PSM approach. The ATEs for both log labor productivity and log revenue productivity are positive and strongly significant, unlike in the full sample. These results are in line with those of McGuckin and Nguyen (1995) who find that mergers are associated with gains in revenue productivity.

[18] 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).

[19] Firm-level characteristics are constructed by aggregating across all the manufacturing plants owned by the firm.

However, as with the full sample, the M&A effects on the productivity and markup terms estimated in our first-stage DLW approach are quite different. The ATE for productivity is negative and statistically significant, while the markup term is positive and marginally insignificant (p-value of 0.16). The second row of Table 3 reports results when we use the nearest three neighbors as controls for our treated plants. Adding additional neighbors might increase precision on one hand by increasing the number of observations, but could hurt precision by including control observations that are not "as close" to the treated observation as the nearest single neighbor. The estimated ATEs are qualitatively identical to the first row, but have more precision, with the ATE on markup now statistically significant at the 5% level. Overall, the PSM approach points out that M&A effects on the separately identified productivity and markup measures from our first-stage regression can provide a much different picture of M&A effects than standard log labor and log revenue productivity measures.

4.4 *Second-stage DID estimates of M&A effects: Alternative control groups*

Propensity score matching controls for sample selection bias by conditioning on observables. However, sample selection bias may remain if there are unobserved factors that are correlated with the selection of treatment or our focus outcome variables (i.e., future markups and productivity) for both the treated and control observations. In fact, the PSM approach may not be able to do better than – and can be specified to be essentially equivalent to – the rich set of controls we use based on Davis et al. (2014). As an alternative, we construct two different control groups that we argue could be plausibly identical to treated plants in both observed and unobserved baseline attributes and the trajectory of outcome variables *ex ante*. Both of these control group strategies are novel in the literature to our knowledge.

The first of these approaches is to use plants that we know will be part of an M&A in the future as control observations. These plants may have observed and unobserved characteristics in common with plants recently acquired (our "treated" group), since they will soon be acquired, but simply have not found (or pursued) a match with a partner firm.[20] With our data, this means that we compare outcomes over the 1997-

[20]It seems likely that many firms may have desirable attributes for an M&A, but do not become merger targets. There are a number of reasons why this may be true, from a costly process of finding suitable matches to market conditions (such as business cycles) that can significantly delay pursuit of

2002 period of plants that are part of an M&A during this same period to plants that we know will be part of an M&A in the 2002 to 2007 period. As with our full sample regression above, we also control for a rich set of fixed effects and cluster at the firm-level. This alternative sample has only 3,100 observations and is therefore much smaller than the full sample.

As indicated in panel A of Table 4, DID estimates of the M&A effect on the productivity and markup terms for this alternative sample are qualitatively identical to the full sample results. The log revenue TFP and log labor productivity effects are not statistically significant, nor is the effect on the DLW productivity term. In contrast, the M&A effect on the DLW markup term is positive and statistically significant with a coefficient that is nearly identical to that estimated in the full sample.

The second novel identification approach we pursue is the construction of a control group that consists of plants in firms that were announced as targets of M&A, but for which the merger was ultimately withdrawn. We call this the "announced M&A" sample, where completed transactions are the "treated" observations and withdrawn transactions are the "controls." As with the first approach using plants that will experience an M&A in the future, plants that were part of an announced M&A that was ultimately withdrawn likely share many to most of the same attributes – observed and unobserved – that lead them to be targeted for an M&A transaction. And the number of failed deals is not 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. We again include a rich set of fixed effects and cluster at the firm level. Unlike the approach with future M&A plants as controls, we can use our full sample of years, 1997-2007, which allows us to better control for time effects. Yet, this sample is also much smaller than the full sample with 4,200 observations.

A key worry with the announced M&A sample is that there may be 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. If there is a systematic reason for withdrawn M&A deals that is correlated with productivity or markups, our withdrawn plants would be a poor control group. However, the small literature that evaluates failure of M&A deal completion does not turn up much evidence for such an unobserved variable.[21] Most of the covariates with explanatory power are solely related to

a suitable M&A deal.

[21] Additional studies beyond those listed in the text includes Mitchell and Pulvino (2001), Officer (2007), and Branch, Wang, and Yang (2008). Hoberg and Phillips (2016) show that mergers with

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 is not 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, a reason that again seems unlikely to be correlated with our outcomes of interest.[22]

Estimates obtained with the announced M&A approach are qualitatively identical to those obtained with the full sample and our sample using future acquired plants as controls. As shown in panel B of Table 4, we estimate statistically insignificant coefficient estimates for the DID effect in regressions for log revenue TFP, log labor TFP, and DLW productivity. In contrast, the DID coefficient in the regression for DLW markups is positive and highly statistically significant. With this sample, the markup coefficient is around 2.8, approximately four times larger than the other samples and specifications we have estimated above. Part of this can be attributed to higher markups in this sample relative to the full sample. Markups average 7.2 in this announced M&A sample, whereas they average about 5.5 in the full sample (see Table 1), Nonetheless, this still implies a larger M&A effect in percentage terms. Specifically, these estimates suggest that the relative M&A effect on markups is about a 40% increase on the average markup in the announced sample.

4.5 *Second-stage DID estimates of M&A effects: Additional robustness checks*

We conduct two additional robustness exercises with this sample of plants that are subject to an announced M&A deal. First, to confirm that the results are not driven by spurious matches between the Thomson Reuters and Census data, we construct a sample composed only of firms with an exact match between the two databases (i.e. matching on firm name, address, city and state). Requiring the stricter criterion of an

high product integration risk are more likely to experience negative outcomes like lower profitability and a higher propensity to divest assets.

[22]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.

exact match gives more certainty of the match quality, but also reduces our sample by a fair amount. In Panel A of Table 5 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 Panel B of Table 4, with the less strict match criteria, though the magnitude of the markup effect falls by about one third.[23]

A second robustness check involves taking an additional step to control for sample section bias via a 2SLS strategy that instruments for whether an announced deal is completed or not.[24] 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 intuition for this approach 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, conditional on its announcement. The results of these 2SLS estimates, reported in Panel B of Table 5, are qualitatively identical to the OLS-estimated M&A effects – statistical insignificance in regressions for the various productivity measures but a large and statistically significant markup effect. In fact, this 2SLS estimate suggests that the relative M&A effect on markups may be over 70% on the average markup.

In summary, our results are robust to a number of alternative specifications meant to address various concerns, primarily endogenous selection of targets. They give a consistent message that acquired plants do not experience statistically significant effects on productivity, on average, but do experience positive and statistically significant effects on markups that are substantial in magnitude.

[23]The results in this panel also point to the possibility that estimates of the effect of M&A on traditional revenue productivity measures – such as those in column 1 – may be inaccurately boosted by concomitant increases in markups (column 4). This topic is explored generally, by Foster, Haltiwanger and Syverson (2008), in the context of international trade by De Loecker (2011), Pierce (2011), Smeets and Warzynski (2013), and Goldberg et al. (2016), and in the case of horizontal mergers in the concrete industry by Kulick (2015).

[24]Our M&A observations vary in when they take place over the five-year window. However, we do not find significant differences in the M&A effects depending on when an M&A takes place in this five-year window.

4.6 *Second-stage DID estimates of M&A effects: Exploring heterogeneity with our announced M&A sample*

To this point, we have been estimating M&A effects averaged over all manufacturing sectors. While we are limited in the extent to which we can explore subsamples of our data to avoid disclosing confidential information, we now consider how M&A effects may vary for transactions that involve firms in the same industry. Such horizontal M&A transactions are most likely to lead to increases in markups, and they may also have different M&A effects on productivity than non-horizontal M&As.[25] We estimate these effects by adding a triple-difference term in equation (13), $\text{Post}_t \times \text{Treat}_i \times \text{SIC}_i$, where the SIC term is either the plant's 2-digit or 4-digit SIC industry.

Panels C and D of Table 5 provide results examining whether M&A effects differ for firms in the same 2-digit or 4-digit SIC industry, respectively. Approximately 39% of all M&As are by firms in the same 2-digit SIC industry in the entire SDC database, whereas about 26% of all M&As are by firms in the same 4-digit SIC industry. We caution that these are not precise ways of defining horizontal M&As, as there may be substantial purchasing of inputs (i.e., upstream-downstream relationships) from firms in the same 2-digit SIC, and even with in the same 4-digit SIC.

The results indicate interesting heterogeneity across merger types. First, the M&A effects on markups are consistent with the hypothesis that they will be larger with horizontal M&A activity. In the 2-digit SIC interactions in Panel C of Table 5, we now estimate an insignificant markup effect in general, but a statistically significant positive difference in markup for plants in M&As by firms in the same 2-digit SIC. The total M&A effect for M&As within the same 2-digit SIC is the sum of the two coefficients (3.349) and is in the same range as we estimate as the general effect for the sample in Panel B of Table 4. In the 4-digit interactions in Panel D of Table 5 we now see a positive markup effect on the general DID coefficient that is significant at the 10% level and a positive (though insignificant) coefficient on the 4-digit SIC interaction. Taken together, this suggests that the markup effects are primarily due to horizontal M&As involving firms from the same industry, consistent with where one might expect M&As to have the greatest potential to raise markups. These increases in market power for horizontal mergers are consistent with theoretical work by Farrell and Shaprio (1990)

[25] The main non-horizontal M&A types are vertical, involving firms from industries where there is a strong upstream-downstream relationship, and conglomerate, where firms are from relatively unrelated industries.

and empirical work for the concrete industry by Kulick (2015).

Exploring heterogeneity also uncovers significant differences in the M&A productivity effect. Both results in Panel C and D of Table 5 suggest that non-horizontal M&As see a positive and significant productivity effect at the plant level. In contrast, there is a negative and significant coefficient on the interaction of the DID effect with indicators that the M&A is between firms of the same industry (i.e., horizontal M&As). The net M&A effect on productivity for 2-digit horizontal M&As is zero, while it is negative for 4-digit M&As. Thus, horizontal M&As not only increase markups, but may also have negative effects on productivity.

4.7 *A Placebo Test*

Finally, we examine whether pre-existing secular trends for the treatment and control group could be driving spurious correlations for our estimated M&A effect, a common concern for DID analyses. To address this concern we construct a placebo test in which we add data for Census years 1987 and 1992 to our announced M&A sample and then add interactions of the indicator for plants that were merger targets between 1997 and 2007 with indicators for the years 1992 and 1997. These are years prior to when the targeted plants were acquired and, hence, the interactions constitute placebo treatments. If there are significant coefficients on these placebo treatments, it would cast doubt on the validity of the estimated 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 concern for any DID analysis. In other words, this particular placebo test is also a test for pre-trend differences.

As results in Table 6 show, our estimates 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 placebo 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 6 verify that our base specification yields qualitatively identical results to our previous analysis when it is applied to the expanded sample of years. As shown in the table, we continue to find no M&A effects on DLW productivity, but significant, positive M&A effects on markups.

5 Exploring Other Channels of Efficiency Gains from M&A

To this point our analysis has examined plant-level productivity as a measure of potential efficiency gains associated with mergers. However, there are other efficiency gains that an M&A may bring to the merged firm. In this section we examine two other possible sources or such gains: 1) firm-level efficiency gains from reallocating production to more efficient plants or closing down low-productivity plants, and 2) firm-level efficiency gains from combining non-production activities.

5.1 *Evidence for Firm-Level Efficiencies from Reallocating Production?*

M&As could have no impact on plant-level productivity, but could lead to firm-level efficiency gains if they allow the merged firm to shut down poorly performing plants and reallocate production to more efficient plants in its newly expanded portfolio of plants.[26] We examine whether there is evidence for this channel in two ways. First, we calculate firm-level productivity estimates as a shipment-weighted average of all the estimated plant-level productivities of the firm's plants. A firm can change these weights (via reallocation across plants) to improve firm-level productivity after an M&A even if average plant-level productivities do not improve, as indicated by our evidence above.

Table 7 provides results when we conduct this analysis using our announced M&A sample, where cumulating across plants leaves us with 2200 firm-level observations. As indicated in Panel A of the table, we find no evidence for M&A effects on productivity through this firm-level channel. We also interact the DID term with indicators for a horizontal merger at the 2-digit or 4-digit level in Panels B and C, respectively, again finding no effect of M&As on firm-level productivity. In terms of the firm-level impact of M&As on markups, the estimated effects, while still positive, are less precisely estimated than at the plant-level. The coefficient on the DID term is not statistically

[26] A number of studies, including Kaplan and Weisbach (1992) and Maksimovic et al. (2011), show that a significant number of plants are sold or closed in the wake of M&A activity. Using data on plants from the U.S. Census' Longitudinal Research Database from 1981 through 2000, Maksimovic et al. (2011) find evidence that retained plants see increases in productivity after M&A, but that sold plants do not.

significant in panels A or B, but is positive and statistically significant at the ten percent level in panel C.

A second exercise we undertake is to examine any M&A effect on plant-level exit probabilities. We regress an indicator for plant exit on the same set of variables and fixed effects as in our regressions above, with results shown in Table 8. As column 1 indicates, there is some evidence (significant at the 10% level) that exit probabilities go up for plants that are part of an M&A, *ceteris paribus*, indicating that merger targets are more likely to be shut down than plants in the control group.

If firms use M&As as an opportunity to close down low-productivity plants and allocate production to higher productivity plants, we would expect an M&A effect on exit that is inversely correlated with a plant's productivity. In column 2, we interact our M&A DID effect with the pre-merger level of plant-level productivity, but surprisingly find exactly the opposite – the probability of exit associated with M&A rises in the plant's productivity level. Columns 3 and 4 examine whether the M&A effect on exit probabilities differs across horizontal and non-horizontal measures. We do not find any differences in the effect for M&As undertaken by firms in the same 2-digit SIC versus other M&As. However, we find evidence that M&As that are not in the same 4-digit industry have higher plant exit probabilities after an M&A, while those by firms in the same 4-digit industry see no change in exit probabilities. Overall, we see little evidence in these analyses that M&As are associated with greater exit probabilities in ways that would lead to greater efficiency at the firm level by reallocating production to higher productivity plants.

5.2 *Changes in non-manufacturing employment?*

A final possible channel of firm-level efficiencies from M&A activity that we explore is from the possibility of realizing economies of scale in non-production activities. In other words, firms might eliminate "redundancies" as a result of M&As (e.g., the merged firm only needs one headquarters and one accounting department). In our sample of manufacturing plants and firms, this should show up as declines in employment at the non-manufacturing establishments of the firm.

Table 9 provides results from estimating equation (13) at both the plant- and firm-level, where the dependent variable is now the log of employment at firms' non-manufacturing establishments. We show results from both the full sample (columns 1 and 2) and from the announced M&A sample (columns 3 and 4).[27] We find no significant M&A effects on non-production employment of the M&A plants and firms, ruling out efficiency effects from realizing scale economies in headquarter services after an M&A. We explore heterogeneity in these effects across M&As involving firms from the same 2-digit or 4-digit SIC, and do not uncover any statistically significant differences in this dimension either.

6 Conclusion

While mergers and acquisitions affect a substantial portion of economic activity worldwide, there is limited systematic evidence of their effect on productivity and market power. The existing literature has often focused on studies of specific firms or industries, making it difficult to infer average effects across broad sets of industries. Moreover, estimating separate effects on productivity and markups has been difficult, and endogeneity concerns have challenged the consistency of some estimates.

This paper estimates the effects of M&A on productivity and markups of plants and firms 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 Reuters in their SDC Platinum database. We use techniques developed by De Loecker and Warzynski (2012) to separately identify productivity and markups for plants and firms across a wide variety of industries in a consistent framework.

We find that evidence for increased average markups from M&A activity is significant and robust across a variety of specifications and strategies for constructing control groups that mitigate endogeneity concerns. In contrast, we find little evidence for plant- or firm-level productivity effects from M&A activity on average, nor for other efficiency gains often cited as possible from M&A activity, including reallocation of activity across plants or scale efficiencies in non-productive units of the firm.

[27] As the full sample in this case is composed of essentially all nonmanufacturing establishments in the U.S., the number of observations is much larger than the earlier analysis using only manufacturing establishments.

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FIGURE 1: Data and Coding of M&A Treatment

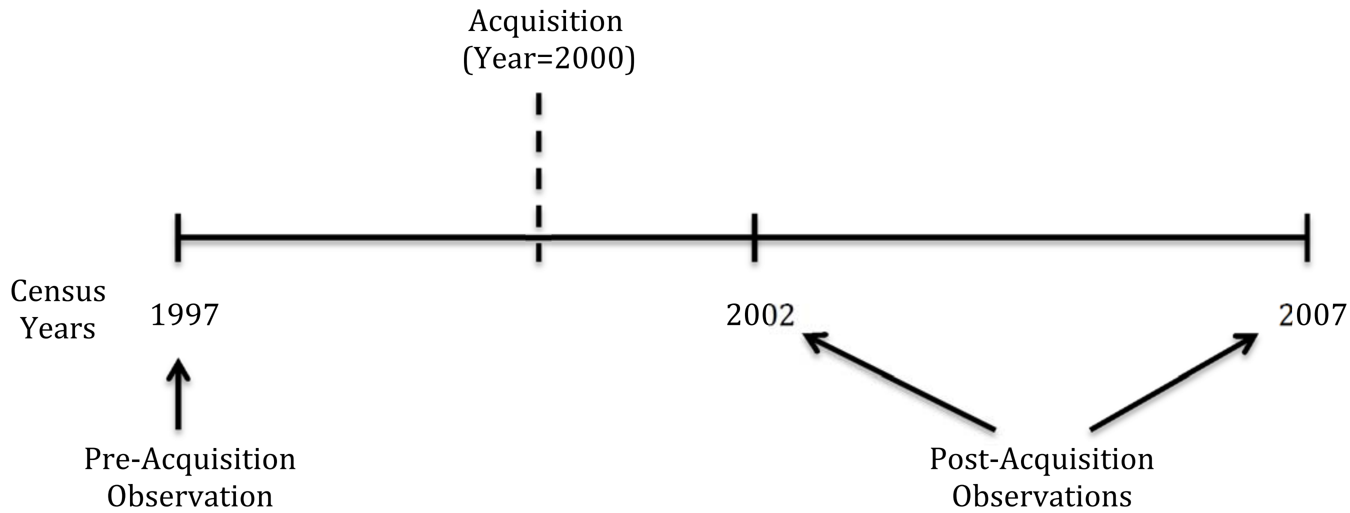

Table 1: First-stage Measures of Markups and Productivity

	Full Sample		Sample With Only Announced M&A Deals	
Variable	**Mean**	**Standard Deviation**	**Mean**	**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: Table displays summary statistics of productivity and markup measures. Log revenue TFP is estimated at the 3-digit NAICS level using the methodology of Foster, Haltiwanger, and Syverson (2008). Labor productivity is the total value of shipments divided by total employment. 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. Source: Authors' calculations using CM and Thomson Reuters SDC Platinum data.

Table 2: Baseline Results with Full Sample of Plants

Variables	Log Revenue TFP	Log Labor Productivity	DLW Productivity	DLW Markup
Target Firm in Post M&A Period	0.022	0.006	0.023	0.706***
	(0.016)	(0.029)	(0.067)	(0.254)
Observations	187,100	187,100	187,100	187,100
R-squared	0.91	0.83	0.95	0.88

Notes: Table displays results of OLS regressions of dependent variables noted in column headings on the interaction of a target firm indicator and post-M&A indicator, post-merger indicator and set of fixed effects (plant, industry by year, and size by year). Estimates for the constant, post-merger indicator and fixed effects are suppressed for brevity. Standard errors, displayed in parentheses, are clustered at the firm-level. Statistical significance at the 1 percent, 5 percent, and 10 percent levels are denoted by ***, **, and * respectively. Source: Authors' calculations using CM and Thomson Reuters SDC Platinum data.

Table 3: Average Treatment Effects Using a Propensity Score Matching Approach

Variables	Log Revenue TFP	Log Labor Productivity	DLW Productivity	DLW Markup
Average Treatment Effect on Target Plant (Nearest Neighbor)	0.174***	0.282***	-0.339**	0.496
	(0.037)	(0.037)	(0.171)	(0.354)
Average Treatment Effect on Target Plant (Nearest Three Neighbors)	0.209***	0.284***	-0.258**	0.720**
	(0.029)	(0.033)	(0.102)	(0.289)

Notes: Table displays the average treatment effect of M&A on the dependent variables noted in column headings. Propensity score matching is conducted for the nearest single neighbor (first row) and nearest three neighbors (second row). Robust standard errors are reported in parentheses. Statistical significance at the 1 percent, 5 percent, and 10 percent levels are denoted by ***, **, and * respectively. Source: Authors' calculations using CM and Thomson Reuters SDC Platinum data.

Table 4: M&A Effects on Alternative Control Group Strategies

Variables	Log Revenue TFP	Log Labor Productivity	DLW Productivity	DLW Markup
PANEL A: Using Plants That Will Merge Next Period as Controls				
Target Firm in Post M&A	-0.021	-0.016	-0.012	0.716**
Period	(0.021)	(0.032)	(0.070)	(0.278)
Observations	3,100	3,100	3,100	3,100
R-squared	0.91	0.80	0.96	0.81
PANEL B: Using Plants Where the M&A Was Withdrawn as Controls				
Target Firm in Post M&A	0.089	0.116	0.100	2.789**
Period	(0.081)	(0.084)	(0.126)	(1.185)
Observations	4,200	4,200	4,200	4,200
R-squared	0.89	0.76	0.93	0.86

Notes: Table displays results of OLS regressions of dependent variables noted in column headings on the interaction of a target firm indicator and post-M&A indicator, post-merger indicator and set of fixed effects (plant, industry by year, and size by year). Estimates for the constant, post-merger indicator and fixed effects are suppressed for brevity. Standard errors, displayed in parentheses, are clustered at the firm-level. Statistical significance at the 1 percent, 5 percent, and 10 percent levels are denoted by ***, **, and * respectively. Source: Authors' calculations using CM and Thomson Reuters SDC Platinum data.

Table 5: Exploring Robustness and Heterogeneity of M&A Effects Using the Announced M&A Sample

Variables	Log Revenue TFP	Log Labor Productivity	DLW Productivity	DLW Markup
PANEL A: Using Stricter March Criteria				
Target Firm in Post M&A Period	0.195*	0.196	0.132	1.797***
	(0.114)	(0.143)	(0.205)	(0.614)
Observations	1,900	1,900	1,900	1,900
R-squared	0.88	0.70	0.93	0.81
PANEL B: Using a 2SLS Approach				
Target Firm in Post M&A Period	0.095	0.157	-0.395	5.253**
	(0.213)	(0.281)	(0.610)	(2.630)
Observations	4,200	4,200	4,200	4,200
PANEL C: M&As Within the Same 2-digit SIC				
Target Firm in Post M&A Period	-0.025	0.017	0.660**	-2.415
	(0.182)	(0.241)	(0.268)	(3.018)
Target Firm in Post M&A Period × Same 2-Digit SIC	0.115	0.101	-0.644**	5.764*
	(0.200)	(0.255)	(0.299)	(3.471)
Observations	4,100	4,100	4,100	4,100
R-squared	0.88	0.76	0.93	0.86
PANEL D: M&As Within the Same 4-digit SIC				
Target Firm in Post M&A Period	0.237*	0.261*	0.461***	1.966*
	(0.139)	(0.143)	(0.120)	(0.614)
Target Firm in Post M&A Period × Same 4-Digit SIC	-0.258*	-0.239	-0.715***	1.308
	(0.149)	(0.159)	(0.195)	(2.057)
Observations	4,100	4,100	4,100	4,100
R-squared	0.88	0.70	0.93	0.86

Notes: Table displays results of regressions of noted dependent variables on reported covariates. Additional interaction terms and fixed effects are included in regression but suppressed for brevity. Standard errors are clustered at the firm-level. Statistical significance at 1, 5, and 10 percent levels denoted by ***, **, and * respectively. Source: Authors' calculations using CM and Thomson Reuters SDC Platinum data.

Table 6: Placebo Regressions Using Data from 1987 through 2007

Variables	Log Revenue TFP	Log Labor Productivity	DLW Producti-vity	DLW Markup	Log Revenue TFP	Log Labor Productivity	DLW Producti-vity	DLW Markup
Target Firm in Post M&A Period	0.079	0.135	0.036	1.301**	0.079	0.123	0.006	1.519**
	(0.081)	(0.096)	(0.132)	(0.662)	(0.073)	(0.086)	(0.123)	(0.751)
Target Firm in Year 1992	-0.007	0.012	0.068	0.575				
	(0.041)	(0.061)	(0.076)	(0.442)				
Target Firm in Year 1997	-0.094	-0.140	0.027	-0.777				
	(0.082)	(0.097)	(0.131)	(0.623)				
	6,200	6,200	6,200	6,200	6,200	6,200	6,200	6,200
	0.86	0.73	0.93	0.79	0.86	0.73	0.93	0.79

Notes: Table displays results of OLS regressions of dependent variables noted in column headings on the interaction of a target firm indicator and post-M&A indicator, post-merger indicator and set of fixed effects (plant, industry by year, and size by year). The first four columns also include interactions of the target firm indicator with indicators for the years 1992 and 1997. Estimates for the constant, post-merger indicator and fixed effects are suppressed for brevity. Standard errors, displayed in parentheses, are clustered at the firm-level. Statistical significance at the 1 percent, 5 percent, and 10 percent levels are denoted by ***, **, and * respectively. Source: Authors' calculations using CM and Thomson Reuters SDC Platinum data.

Table 7: Firm-level Estimates of M&A Effects Using the Announced M&A Sample

Variables	Log Revenue TFP	Log Labor Productivity	DLW Productivity	DLW Markup
PANEL A: Baseline Estimates				
Target Firm in Post M&A	-0.127	-0.096	-0.013	1.552
Period	(0.10)	(0.12)	(0.17)	(2.16)
Observations	2,200	2,200	2,200	2,200
R-squared	0.87	0.72	0.89	0.86
PANEL B: M&As Within the Same 2-digit SIC				
Target Firm in Post M&A	-0.059	-0.104	-0.176	0.632
Period	(0.22)	(0.26)	(0.33)	(1.13)
Target Firm in Post M&A	-0.105	0.006	0.257	1.35
Period × Same 2-Digit SIC	(0.24)	(0.28)	(0.40)	(3.07)
Observations	2,200	2,200	2,200	2,200
R-squared	0.87	0.72	0.89	0.86
PANEL C: M&As Within the Same 4-digit SIC				
Target Firm in Post M&A	-0.091	-0.03	-0.091	1.48*
Period	(0.12)	(0.15)	(0.19)	(0.76)
Target Firm in Post M&A	-0.12	-0.22	-0.348	0.242
Period × Same 4-Digit SIC	(0.20)	(0.24)	(0.45)	(6.87)
Observations	2,200	2,200	2,200	2,200
R-squared	0.87	0.72	0.89	0.86

Notes: Table displays results of firm-level OLS regressions of noted dependent variables on reported covariates. Additional interaction terms and fixed effects are included in regression but suppressed for brevity. Standard errors are clustered at the firm-level. Statistical significance at 1, 5, and 10 percent levels denoted by ***, **, and * respectively. Source: Authors' calculations using CM and Thomson Reuters SDC Platinum data.

Table 8: Exit probabilities after M&A

Variables	Baseline	With Productivity Interaction	Same 2-digit SIC	Same 4-digit SIC
Target Firm in Post M&A Period	0.078*	0.115*	-0.01	0.245***
	(0.047)	(0.067)	(0.257)	(2.474)
Target Firm in Post M&A Period × Productivity		0.056***		
		(0.016)		
Target Firm in Post M&A Period × Same SIC 2-Digit Industry			0.091	
			(0.265)	
Target Firm in Post M&A Period × Same SIC 4 Industry				-0.247***
				(0.073)
Observations	2,400	2,400	2,400	2,400
R-squared	0.66	0.66	0.66	0.66

Notes: Table displays results of regressions of an indicator for plant exit on reported covariates. Additional interaction terms and fixed effects are included in regression but suppressed for brevity. Standard errors are clustered at the firm-level. Statistical significance at 1, 5, and 10 percent levels denoted by ***, **, and * respectively. Source: Authors' calculations using CM and Thomson Reuters SDC Platinum data.

Table 9: The Effect of M&A on Employment in Nonmanufacturing Activities

	Full Sample		Announced M&A Only	
	Plant	Firm	Plant	Firm
Target Firm in Post M&A Period	-0.012	-0.109	-0.008	0.046
	(0.022)	(0.290)	(0.023)	(0.410)
Number of Observations	5,268,200	3,737,600	13,500	200
R-squared	0.97	0.96	0.97	0.97

Notes: Table displays results of regressions of the log of nonmanufacturing employment on noted on the interaction of target firm and post-M&A indicators. Additional interaction terms and fixed effects are included in regression but suppressed for brevity. Standard errors are clustered at the firm-level. Statistical significance at 1, 5, and 10 percent levels denoted by ***, **, and * respectively. Source: Authors' calculations using LBD and Thomson Reuters SDC Platinum data.