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Gains from Offshoring? Evidence from U.S. Microdata

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Abstract

We construct a new linked data set with over one thousand offshoring events by matching Trade Adjustment Assistance program petition data to micro-data from the U.S. Census Bureau. We exploit this data to assess how offshoring impacts domestic firm-level aggregate employment, output, wages and productivity. A class of models predicts that more productive firms engage in offshoring, and that this leads to gains in output and (measured) productivity, and potential gains in employment and wages, in the remaining domestic activities of the offshoring firm. Consistent with these models, we find that offshoring firms are on average larger and more productive compared to non-offshorers. However, we find that offshorers suffer from a large decline in employment (32 per cent) and output (28 per cent) relative to their peers even in the long run. Further, we find no significant change in average wages or in total factor productivity measures at affected firms. We find these results robust to a variety of checks. Thus we find no evidence for positive spillovers to the remaining domestic activity of firms in this large sample of offshoring events.

Keywords: Outsourcing, employment, trade, productivity, firm performance
JEL classification codes: F16, F61, F66, F14, F23

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†Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed.

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1 Introduction

The impact of trade on the U.S. labor markets, particularly its contribution to the steep decline in manufacturing employment and increase in income inequality, has been a topic of intense academic and policy interest (Feenstra 2010, Krugman 2008, Autor, Dorn and Hanson 2012).[1] A major pathway through which trade can impact employment and wages is through the offshoring of production (Feenstra 2010, Blinder 2009).

However, empirical work has been significantly hampered by the lack of quality data on offshoring (Kirkegaard, 2007). In this paper, we assemble a new linked dataset of offshoring events and firm performance, and use it to evaluate firm-level effects of offshoring. We constructed this data by linking offshoring-induced employment layoff events available from the Trade Adjustment Assistance (TAA) program to U.S. Census Bureau panel microdata.

Theoretical predictions of the effects of offshoring vary across models. While media discourse about offshoring focuses largely on immediate job destruction at affected plants, theoretical models in the academic literature suggest potential improvements in output, wages, productivity, survival probability and even employment growth in non-offshored domestic activities of the offshoring firm.[2] In an extension of Grossman and Rossi-Hansberg's (2008) influential model of offshoring, Sethupathy (2011) finds that remaining domestic units benefit from lower input costs of the offshored input/task. While the net effect on employment is ambiguous, total output and profits at an offshoring firm go up; if workers share in the profits through bargaining, worker wages can be expected to rise at offshoring firms (and fall at non-offshoring firms who lose market share). It is plausible that firms failing to restructure in the face of global competition may perform worse than firms that offshore, leading to even greater firm failures and employment losses at non-offshoring firms relative to offshorers (Park 2012a).[3] In this class of models, measured productivity at the

[1] Absolute employment levels in manufacturing has sharply declined over the last decade. Per BLS figures (data.bls.gov), manufacturing employment stayed relatively stable around 17 million from 1990 till about 2000, and then declined sharply to about 14 million by 2004, and then further to about 12 million in 2012.

[2] Throughout this paper, we use the terms "establishment" and "plant" interchangeably to mean one geographic branch or unit of a "firm". A firm can have one or more establishments.

[3] Park (2012a) analyzes the theoretical employment effect of offshoring in a heterogeneous firm framework and finds the majority of industry-level negative effects stem from the "cleansing effect" - job destruction from the downsizing or death of non-offshoring firms that lost price competitiveness against their offshoring rivals. Our focus in this paper is not on the aggregate effects of offshoring (which is empirically more challenging to identify), but rather on firm

domestic firm level could be expected to go up as a result of lower costs for offshored tasks.

The complementarity of domestic output (and employment) with offshoring arises in these models due to vertical linkages between the offshored activity and the remaining domestic activity. If offshoring consists of unrelated "horizontal" activity (H-FDI), foreign employment may be a substitute for domestic employment, even in remaining domestic units, as support activities in other parts of the firm may be eliminated following offshoring (Harrison and McMillan 2011, Markusen and Maskus 2001). Further, with H-FDI, there is no linkage to other parts of the firm via lower input costs, so measured productivity at the (domestic) firm level may be unaffected. Thus the extent to which offshoring affects firm-level employment and other outcomes is an interesting empirical question.

The TAA data we use includes a total of 23,327 petitions over the period 1999 to 2006. We use name-matching algorithms supplemented by extensive manual checks and modifications to link the names (and state) of establishments in the TAA petition data to the U.S. Census Bureau's business register (see Data Appendix for details). We achieve a match rate of about 70 percent; after cleaning and linking to the underlying Census micro data sets, and focusing only on initial offshoring events within firms our analysis covers about 1,400 unique offshoring firms with a limited set of variables (from the Longitudinal Business Database) and about 1,000 unique offshoring firms with greater information (from the Census and Annual Survey of Manufactures). We use this data to examine the effects of offshoring on a range of outcomes, including employment, output, factor intensity, productivity and survival, at the (domestic) aggregate firm level.[4]

First, we examine the basic characteristics of offshoring firms relative to the overall population of firms. These statistics show, consistent with the prediction of the Grossman–Rossi-Hansberg class of models, that offshoring firms are larger, more capital intensive, and more productive relative to non-offshorers prior to their initiation of offshoring. Rare among large firms, we find that relative to industry peers, offshoring firms were *not* more skill intensive.[5]

level outcomes for offshorers (which we assess by comparing offshorers to industry peers).

[4] Because we rely on Census micro data, our analysis aggregates up domestic establishments of the firm in the U.S.; thus gains in profits at the global level will not be reflected in our results if they are not in the form of greater sales or profits domestic establishments. In particular, we will not capture profits in foreign operations retained abroad. We do capture potential gains in non-manufacturing operations in our analysis using LBD data (Section 7.2).

[5] This is surprising at first because larger firms, which offshoring firms tend to be, are usually both more capital and

Next, we examine changes at the firm level using a difference-in-differences (DID) framework. In our baseline analysis, for each offshoring firm, we select two "controls" closest in size to an offshored firm from within the same 3-digit industry. This approach addresses endogeneity from omitted variables, so long as omitted factors impact similar sized firms in the same industry in a similar fashion.[6] We find that firms experience a significant decline in employment coincident with the initiation of offshoring, with the decline continuing for 3 to 4 years after. We find no evidence of firm employment recovery: over a six-year window of time from the initiation of offshoring, firm-level employment remains well below the pre-offshoring levels, with an average drop of 32% employment. Importantly, this pattern of employment reduction is very similar if we restrict the sample to multi-unit firms only, or to only those non-offshoring plants within a offshoring firm.[7] The magnitudes of declines in employment are similar for the aggregate of non-offshoring plants, suggesting significant declines in supporting activities at other parts of the firm. Consistent with the decline in employment, we find stark declines in output (28%) and capital (22%) at the firm level; again similar patterns also hold for the aggregate of non-affected plants within offshoring firms.

We find no discernible change in wages for either production workers or non-production workers. We find small gains in labor productivity (measured as real output per worker or real value added per worker). However, these gains appear to be achieved through more intense use of capital (as capital declines less than employment); firm level total factor productivity (TFP) measures that account for capital show no significant change relative to controls. We also examine firm survival rates, and find that the survival rate of firms who offshore is very similar to control group firms. We verify the robustness of our results with a number of alternative specifications (see Section 7); we describe two of the major checks below.

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skill intensive on average. However, our finding suggests that offshorers are significantly less skill-intensive relative to *similar sized firms*. In fact in our propensity model of offshoring (Table 3), we find that offshoring is correlated with lower skill intensity, conditional on other variables. This finding is intuitive and consistent with economic theory, as we may expect low skill activities to be precisely the ones to be offshored (Krugman 2008).

[6] For example, if offshoring is prompted by increased competitive pressure - which also could have a direct effect on the outcome variable - this bias is controlled for by comparing to matched industry-size controls, so long as competition affects similar sized firms in the same industry is a similar fashion.

[7] We term the aggregate of non-offshoring plants "pseudo firms", and compare this aggregate to matched firms in the same industry.

First, one potentially important concern is the selection of establishments that file for TAA assistance. In particular, the TAA sample consists of those establishments where workers in the offshored activity are not re-absorbed in the plant itself (and have difficulty finding immediate employment). Despite this selection, we believe this sample is a valid one to test for positive spillovers for two reasons. One, the Grossman–Rossi-Hansberg class of models do not hinge on re-absorption of workers into the same establishment; while the fate of workers in the offshored activity is not explicitly modeled, it is clear that whether they are absorbed back into the establishment would depend on the specificity of the skills (and nature of domestic and offshored activities). In particular, the predictions about improvement in firm-level output, productivity and wages should hold even if offshored workers are not re-absorbed at the same establishment. Two, we find no evidence that the nature of selection skews the sample towards weak or failing firms, whose poor performance overall might bias our DID results. We specifically test for pre-existing trends in two ways. In our baseline analysis involves plotting the trends for both the treatment and control groups for a 13-year window around the offshoring event (see e.g. Figure 2) following an approach suggested by Angrist and Pischke (2009, chapter 5). These figures show that: (a) the offshoring firms do not show a significant declining trend in any of the key outcome variables prior to offshoring; (b) the trends for the control group of industry-employment matched firms are very similar prior to the offshoring event; and (c) there is a stark break in trend in offshored firms relative to non-offshorers, consistent with changes being triggered by offshoring. Further, in the regression analysis, we include a test for pre-existing trends, and we confirm that the post-offshoring decline for employment, output and capital significantly exceed the magnitude of preexisting trend effects (if any).

Nevertheless, to condition on a richer set of variables, we adopt a propensity score matching approach (Rosenbaum and Rubin 1985). We include employment growth, wage growth, labor productivity, skill intensity (share of non-production workers) and capital intensity as regressors on an offshoring indicator variable in the propensity model. We then redo our analysis of outcome variables using controls matched on the propensity score, and we find our baseline results quite robust to using these alternative controls.

Second, one possibility not captured in our baseline analysis using manufacturing sector data is that potential benefits from offshoring are transmitted mainly to non-manufacturing activities of the firm. For example, if the offshored product is distributed by domestic retail or wholesale establishments, employment gains may be observed mainly in these marketing units or at the headquarters. To examine this possibility, we used data from the Longitudinal Business Database (LBD), which includes employment and payroll information on all establishments in all sectors. We find results consistent with the baseline for two key variables; in particular, we find significant declines in firm-level employment, and no change in average wage.

Our paper contributes to the literature that has attempted to answer whether offshoring is a complement or substitute for domestic employment (e.g., Desai, Hines and Foley 2009; Harrison and McMillan 2011); a major novelty is the new linked data that allows us to examine events that are verified (by the U.S. Department of Labor) to be related to offshoring. Our finding of a stark negative impact on domestic firm output, employment and capital stand in contrast to the results in a number of studies in this literature, as we discuss in detail in Section 2 below. Our results appear consistent with shifting of entire product lines abroad, where offshoring occurs with a lack of strong vertical linkages between the offshored activity and remaining home activities.

The rest of the paper is organized as the following. Section 2 describes the related literature and the alternative approaches to measuring offshoring. Section 3 presents a model of offshoring drawn from Sethupathy (2011)'s extension of the Grossman–Rossi-Hansberg's (2008) work. Section 4 describes the data in more detail. Section 5 briefly describes the empirical methodology used to evaluate the effects of offshoring. Section 6 presents our baseline results. Section 7 describes our robustness checks; Section 8 discusses results and concludes.

2 Related Literature and Measurement of Offshoring

The most common approach to measure offshoring in the existing literature is to use the share of imported input usage. At the industry level, this entails using input-output tables to identify offshoring industries. The general consensus is that employment effects of offshoring are weak. Amiti and Wei (2006) find that the impact is insignificant at the disaggregated level, but positive at a more aggregated level in the U.S. manufacturing sector between 1992 and 2000. In a similar study, Amiti and Wei (2005) find an insignificant employment effect in the U.K. manufacturing industry between 1995 and 2001. For the Canadian manufacturing sector, Morissette and Johnson (2007) find that the industries with intense offshoring did not show significantly different employment growth rates compared to other industries. Koller and Stehrer (2010) use Austrian data and find that offshoring had a negative effect during 1995-2000, but a positive effect during 2000-2003.

Such a measure can also be constructed for firm-level data. For the U.S., the 1987 and 1992 Census of Manufactures conducted by the U.S. Census Bureau collects data on plant-level imported input usage. All manufacturing plants were asked whether they used any inputs of foreign origin. The answer 'yes' is used as a flag for an offshoring activity in many studies (Berman, Bound and Griliches, 1994; Feenstra and Hanson, 1996 & 1999; Kurz, 2006). Unfortunately, the Census stopped asking this question after 1992 (a subsample of establishments were asked this question in the 2007 Census, and used in work by Fort (2011) who investigates the determinants of importing). Similar attempts have been made with micro data of countries other than the U.S. Hummels et al.(2011) use Danish employer-employee matched data to explore a similar question with more focus on the impacts on wage rates. They find that offshoring increases high-skilled wages and decreases low-skilled wages, and that workers displaced by offshoring suffer from a larger wage loss than from other layoffs.

A significant limitation of using imported input usage as a measure of offshoring is that the imported inputs could be related to newly introduced products rather than replacement of in-house inputs (Feesntra and Markusen, 1994). These new inputs would not involve shifting of in-house production, and hence may not capture true offshoring. On the other hand, if an entire production line is offshored, no measured increase in imported inputs will be recorded even though offshoring is taking place (in fact if the offshored activity used some imported inputs, the fraction of inputs imported may even decline). Our data allows us to identify offshoring events certified by an independent investigator. This avoids these potential measurement errors and omissions that could impact the use of imported intermediate inputs as a proxy for offshoring.

A second source used to identify offshoring is survey data on foreign operations of the U.S.

multinationals, collected by the U.S. Bureau of Economic Analysis (BEA). This dataset has detailed operational information at the establishment level, including location, employment and wages. Brainard and Riker (2001) find little substitution between U.S. facilities and foreign affiliates, and larger substitution among foreign affiliates in low wage countries. Stronger substitution between home and foreign affiliate employment is found by Hanson, Mataloni, and Slaughter (2005). On the other hand, Desai, Foley, and Hines (2009) find complementarity between home and foreign affiliates of U.S. multinationals; they find that when foreign investment (employment compensation) rises by 10%, U.S. domestic investment (employment) rises by 2.6% (3.7%). In contrast, Borga (2005) finds an insignificant effect. Harrison and McMillan (2011) find that while overall offshoring substitutes domestic employment, for MNCs with vertically-related operations in low (high) income countries, foreign employment seems to be a complement (substitute) for domestic employment. They argue that this is driven by dissimilarity (similarity) of tasks undertaken in poor(rich) country affiliates and the U.S. Sethupathy (2011) examines offshoring activities to Mexico using the same BEA data. He finds an increase in wages and no evidence of greater job losses in domestic locations at offshoring firms. Similar analysis was performed using the data on European firms. Muendler and Becker (2010) investigate German multinationals and find strong substitution. Braconier and Ekholm (2000) find substitution between Swedish facilities and affiliates in high-income countries, but neither substitution nor complementarity for affiliates in low-income countries.

One drawback of this type of data is that it does not capture the impact of offshoring through arm's length contracts, which according to Bernard et al. (2005), account for about half of offshoring activities of U.S. multinationals. Further, some of the outward investment observed in these data sets, even when they are in vertically-related industries may not be related to offshoring, as they could be related to expansions of activity abroad (rather than shifting of production from home).[8] Our dataset, by the nature of the TAA program, captures events of production shifting abroad,

[8] E.g., Desai et al (2009) describe their work as investigating the effect of foreign investments broadly (rather than offshoring specifically). While they find that FDI outflows and domestic investments are complementary, earlier work on the effects of foreign investment found mixed effects of foreign operations on domestic activity. A negative link was found for seven selected U.S. multinationals (Stevens and Lipsey, 1992) and for aggregate data in OECD economies (Feldstein, 1995). A positive link was found for cross-section of U.S. multinationals (Lipsey, 1995), aggregate data for Australia (Faeth, 2005), German firm-level data (Kleinert and Toubal, 2010), German industry-level data (Arndt, Buch, and Schnitzer, 2007), and industry-level data for Canada (Hejazi and Pauly, 2001).

irrespective of whether it was within the firm, or to outside parties.

3 Model

In this section, we motivate our empirical work by presenting a brief version of Sethupathy's (2011) extension of Grossman and Rossi-Hansberg's (2008) seminal model of offshoring. While the model in Grossman and Rossi-Hansberg (2008) allows two types of labor, skilled and unskilled, it limits firms to be homogeneous. Sethupathy (2011) allows firm heterogeneity while limiting workers to be homogeneous.

3.1 Set-up

There are two sectors, X and Y, and one factor, labor. Sector X has homogeneous goods produced using CRS technology. Offshoring is not possible in sector X and the product market is perfectly competitive. Workers are paid their marginal product, w_X. Sector Y has differentiated products with a monopolistically competitive market. Workers first look for a job in sector Y and all residual workers are absorbed by sector X.

First, firms in sector Y incur a sunk entry cost f_e and get a productivity draw ϕ from the Pareto distribution $G(\phi)$. After learning their productivity, firms enter the labor market to hire their workforce and start producing. The production function is $q = \phi N(\phi)$ where $N(\phi)$ denotes the total employment by this firm. Production is composed of a continuum of tasks z with a mass 1 ($z \in [0, 1]$). The employment share of each task is fixed as s. The cost of offshoring task z has two multiplicative components: heterogeneous offshoring cost $t(z)$ and policy cost β. Tasks are indexed according to the size of its offshoring cost so that $t'(z) > 0$. The domestic wage is w_d and the foreign wage rate is w_f. Therefore, the cost of performing task z is sNw_d at home and $\beta t(z)sNw_f$ in foreign country.

Workers begin their job search in sector Y keeping the job in sector X as an outside option. Firms with productivity ϕ pay a search cost $b(\phi)$ ($b'(\phi) > 0$) and receive a random match. The domestic wage rate in sector Y, w_d, is determined through Nash bargaining between an employer and a worker as the following:

$$\underset{w_d}{\text{Max}}\ \theta ln(w_d - w_x) + (1-\theta) ln(\pi_{op})$$

π_{op} is the marginal profit of an additional worker and θ denotes the Nash bargaining parameter. This maximization problem yields the rent sharing wage specification as the following:

$$w_d = \eta \pi_{op} + w_x \qquad \text{where } \eta = \frac{\theta}{1-\theta} \text{ : rent sharing parameter}$$

Consumer demand is characterized by the quasi-linear utility function as in Melitz and Ottaviano (2008). Utility maximization yields the following expression for the demand for product i in sector Y:

$$p_i = \rho - \gamma q_i - \lambda Q_y \quad ,$$

where ρ summarizes the degree of substitution among differentiated products in Y, γ indicates the degree of product differentiation, and λ is the degree of substitution between production in X and Y. Q_y denotes the total consumption of sector Y products.

3.2 Impact of a Fall in Offshoring Cost

As in Melitz (2003), the equilibrium is characterized by cut-off productivities of firms with different operational strategies. In this set-up, we have two cut-off productivities: one for survival and the other for offshoring. This is depicted in panel (a) of Figure 1. Each offshoring firm then has a marginal task that separates the offshored tasks and domestic activities.

If the policy cost of offshoring, β, decreases, firms with different productivity levels respond differently. These responses are summarized in panel (b) of Figure 1. First, the cut-off productivity for offshoring falls, since offshoring brings larger cost reduction for all tasks offshored. This implies that offshoring becomes profitable for more firms, including the firms with lower-productivity. Second, the extent of offshoring within an offshoring firm increases. Recall that costs of carrying out task z at home and in the foreign country are sNw_d and $\beta t(z)sNw_f$, respectively. As β falls, the marginal task z^* such that $w_d = \beta t(z^*) w_f$ falls. Therefore, offshoring firms enjoy cost reduction for larger fraction of their production process. Third, the cut-off productivity for survival increases. Park (2012a) calls this *the cleansing effect of offshoring.* The cost reduction from offshoring reduces the prices of the products by offshoring firms, raising the relative price of the non-offshoring firms and hurting their profitability. It becomes harder for non-offshorers to survive.

The employment effect within offshoring firms is ambiguous because there is job creation as well as job destruction. As they initiate offshoring of some tasks, their employment at home decreases. However, their prices fall from cost reduction which leads to larger sales. This could lead to job creation, potentially large enough to offset the initial job destruction. The sign of the net effect cannot be determined analytically and depends on various parameters of each industry (Park, 2012a). The fall in offshoring cost improves profitability of offshorers and causes their wage rates to rise through rent-sharing.

Thus this model predicts: (i) an ambiguous net effect on firm-level employment; (ii) positive effects on output; and (iii) positive effects on wage rates.

In the model above, the positive spill over to domestic employment and output arises due to vertical linkages between the offshored activity and the remaining domestic activity, with the offshored input now being lower cost than before. In general, as discussed in Desai et al (2009), there could be complementarities also if the remaining domestic activity is upstream (e.g., when the more skill or capital intensive activity is retained in the U.S. and labor intensive assembly of final product is offshored abroad) – even in this case, the lower overall cost of production would allow the firm to lower prices and gain market share, leading to an expansion in domestic activity.

3.3 Alternative model: Shifting entire product line (Horizontal FDI)

However, if offshoring consists of shift of an entire product line (unrelated to remaining domestic activity), foreign employment may simply involve a shift of employment, with no spillover effects. In fact, this type of "horizontal FDI" (H-FDI) could lead to job losses in remaining domestic units, if support activities in other parts of the firm are eliminated following offshoring (Harrison and McMillan 2011, Markusen and Maskus 2001). Further, with H-FDI, measured productivity at the (domestic) firm level would be unaffected, as there is no distinct effect on the marginal costs of other activities.

There would be no output gain at all if the shift involved movement of export production to another country (termed "export-platform FDI" by Harrison and McMillan, 2011). If part of the shifted production was sold via domestic establishments, there would be gains recorded in output of other domestic units (possibly in marketing units – see discussion in Section 7.2). But if the foreign plant sold directly to other firms directly, the sales would be recorded by the foreign plant, and this would not affect measured output of remaining domestic establishments.

4 Data

We use three main sets of data in our analysis: Trade Adjustment Assistance (TAA) petition data to provide information of layoff events related to offshoring; the Longitudinal Business Database (LBD), with basic operational information of the universe of establishments in the U.S.; and the Annual Survey of Manufactures/Census of Manufactures (ASM/CMF) with more detailed information for manufacturing establishments.

4.1 Trade Adjustment Assistance Data

The information on trade-induced layoffs in U.S. manufacturing plants is obtained from TAA program administrative data, administered by the U.S Department of Labor (USDOL).[9] The dataset was procured through a Freedom of Information Act request.

The TAA is a dislocated worker program that originated with the Trade Act of 1974. When layoffs occur, workers or any entity that represents them (company, union, or state)may file a petition with USDOL.[10] The petitions are filed at the plant level. Once filed, each petition is assigned an investigator from USDOL who conducts interviews at the petitioned plant, upstream/downstream plants, and with customers to identify the reason for layoffs and determine when they began (im-

[9] Some petitions are filed under the North American Free Trade Agreement-Transitional Adjustment Assistance (NAFTA-TAA) program for years between 1994 and 2003. NAFTA-TAA program was merged into the regular TAA by the Trade Act of 2002.

[10] For our sample period - layoff events between 1999 and 2006, 50% of petitions are filed by companies, 42% by unions or workers, and the rest by states.

pact date). Certification is issued if the reason for layoffs is determined to be one of the following: (i) *company imports* (the company itself replaced in-house tasks with imported tasks); (ii) *customer imports* (buyers now purchase from foreign firms instead of this plant); (iii) *production shift* (the company replaced tasks with activities at own subsidiaries abroad); and (iv) *increase in aggregate imports* (there has been an increase in imports of this product at the aggregate level).[11] 45% of petitions in our sample period are denied, as they were deemed not to satisfy any of the four criteria. Once certified, the workers displaced from this plant between the impact date and two years from the certification date (or impact date whichever comes later) are eligible for various benefits provided under the TAA program.[12]

Based on the reason for layoffs, we classify the petitions into three groups: offshoring events, import-competition events, and denied petitions. Offshoring events are the petitions certified due to company imports or production shifts (criteria (i) and (iii) above). The layoffs in these events are a *voluntary decision* of the company, indicating a strategic move. Import-competition events, instead, are those driven by external forces (categories (ii) and (iv) above). The petitions report company name, address (state, city, zip code, street address), impact date (the day layoffs began), and 4-digit SIC code. The reason for displacement, as described above, is reported in the collated data provided to us by the USDOL only after 2002 (after the Trade Reform Act of 2002 revised the coding guidelines). Though unreported, USDOL had began this classification process prior to 2002; for petitions between 1999 to 2001, we manually examined the investigation report of each certified petition (available on the USDOL website) to identify the reason for certification. Our sample's impact years range from 1999 to 2006. We classify a total of 19,603 petitions [13]

[11]This is the case where an establishment has many small buyers rather than a few large customers. Many petitions filed in paper industry are certified for this reason.

[12]Table 1 of Park (2012b) provides more details on these benefits.

[13]Between 1999 and 2006, total of 23,327 petitions were filed and 12,831 were certified. Of those certified, we were able to identify the reason for layoff for 9,107 petitions. In order to construct the sample of offshoring events and import competition events, we dropped the certified petitions for which the detailed classification was not documented. Thus our final sample includes 9,107 petitions certified with a reason identified and 10,496 denied, totalling 19,603. Table A1 shows the number of certified petitions and offshoring events for each impact year (before cleaning of data to focus on initial offshoring episode for affected firms).

4.2 Micro data from the U.S. Census Bureau

We link the information on layoff events from the TAA petition data to confidential micro data from the U.S. Census Bureau. There are two sets of data we can use to explore firm-level impacts of offshoring: the LBD and the ASM/CMF. The LBD consists of data on all U.S. establishments in existence, including non-manufacturing establishments, but collects limited operational information for each plant. The LBD contains annual information on total employment, total payroll, industry, location, and also the birth and exit year for each establishment.[14]

In order to analyze the impact of offshoring on other aspects of firm-level operations, we use ASM/CMF data as our main database. The ASM/CMF contains a rich set of variables such as employment and payroll separately for production and non-production workers, total value of shipments (output), value added, material costs, export shipment, and ways to estimate capital accumulation, but does not contain complete coverage of the universe of firms.

More specifically, the CMF is a quinquennial survey on the universe of U.S. manufacturing establishments, undertaken in years ending in 2 or 7. For between-Census years, a similar set of information is collected in the ASM for a representative sample of manufacturing establishments. The sampling weight is based on the employment size in the latest CMF with larger establishments receiving a larger weight. Establishments with employment of 1,000 or more are included with certainty. The ASM sample changes every five years. We retain approximately 65% of our LBD sample as we move from the LBD to the ASM/CMF sample.[15]

4.3 Construction of Firm-level Variables

Since TAA petitions are filed at the plant level, the merging of the TAA petition data and the micro data from the Census Bureau is performed at the plant-level. The matching of the names and state information in the petition data to the U.S. Census business register is done using name matching

[14] The birth year is left-censored at the start of the data (1976) and the exit year is right censored at the end of our LBD data period (2009).

[15] The explanation for why we get a significant fraction of the LBD sample in the ASM-CMF is twofold: (i) offshoring firms are predominantly engaged manufacturing activity; and (ii), as we find below, firms (and establishments) in that offshore are significantly bigger than average and hence they are disproportionately included in the ASM/CMF. Given the sample overlap, it is not surprising that we find our results for variables found in both datasets – employment and average wage – very similar in firm-level aggregates using the LBD data (see discussion in Section 7.2). Thus, that sampling in the ASM is not biasing our conclusions.

algorithms, supplemented with extensive manual checks and modifications; we provide details on the merging process in the Data Appendix. Using the firm identification codes in the LBD, we aggregate establishments to one firm. Some firms experienced multiple offshoring events during the observation period, either at different plants at the same time (cross-section) and/or at different times in the observation period (time-series). In such cases, we use the impact date of the first offshoring event as the firm's initiation of offshoring. The firm's industry-code is chosen based on the industry with the largest employment: we aggregate establishment-level employment by 3-digit 1987 SIC codes within each firm and picked the SIC code with the largest employment as the firm's industry. Other firm-level variables in the ASM/CMF are aggregates from establishments selected in ASM.

For productivity measurements, we use a number of different approaches: in addition to labor productivity measures (output per worker and value added per worker), we also estimate total factor productivity as residuals from a value added production function, estimated alternatively using OLS (with plant-fixed effects) and using the Levinsohn-Petrin (2003) approach to control for endogeneity of inputs. These estimation methods measure TFP at the plant level; we use a variety of different methods to aggregate these productivity measures up to the firm level, including the average across all plants at a firm (used in the baseline results reported below), the employment-weighted average across all plants, and the relative ranking of each of these measures across firms.[16]

5 Empirical Methodology

Our main interest is in the firm-level impact of offshoring. While it is expected that the subunit with the offshored activity will see reduction in employment and output, the model sketched out in Section 3 suggests that other domestic units of the offshoring firm would realize benefits from offshoring, so that firm-level employment and output could show improvements in medium- and long-run.

We carry out the impact analysis by exploiting the timing of layoff events identified by our data. In order to separate the operational changes caused by offshoring events from other industry-

[16]The main findings were robust to other productivity aggregation measures described above.

or even economy-wide factors surrounding the timing of offshoring, we use a difference-in-differences estimation approach, using a matched control group of peer firms. In our baseline analysis we use a 'nearest neighbor' matching, choosing two controls for each offshored firm based employment within the same industry (as discussed in Section 6.2); to undertake the analysis below, the two matched controls are assigned the same event year as the offshored firm. As an alternative robustness check, we use a propensity-score matching approach(as discussed in Section 6.3.1 below).

We build a longitudinal link for each treated and control firm for a 13-year window, six years before and after the impact year. We investigate the impacts of offshoring on a range of outcomes including size (sales, value added, employment, and capital), wage rates (overall, production and non-production), factor intensity (capital per employee, non-production share of employment and wage bill), and productivity (labor productivity and TFP measures). An outcome variable, y_{ijt}, of firm i belonging to group j (where one group consists of one *treated* firm and one to two controls) observed at time t is estimated using the following specification:

$$y_{ijt} = \gamma_0 + \sum_{k=-6}^{6} (\beta_k \delta_i + \alpha_k) D_{j,t+k} + f_i + e_{ijt} \tag{1}$$

where $t + k$ is the impact year (the offshoring event occurs k years away from the current time t, with $k \in [-6, 6]$), f_i stands for firm fixed effects, δ_i is an indicator for an offshoring firm, and $D_{j,t+k}$ is the indicator that the treated firm in group j underwent offshoring k periods from year t. In this case, α_k provides the trend for the matched controls, and $(\beta_k + \alpha_k)$ provides the trend for the treated firm. Therefore, β_k captures the impact of offshoring k years from the impact year. We plot the trends (and confidence intervals) for the treatment and control group; these figures provide a straightforward basis to assess: (a) whether there was clear break in trend around the initiation of offshoring, and (b) to assess whether the offshoring firms and the control group had similar trends before the offshoring event (Angrist and Pischke, 2009 Chapter 5). Note that since the equation is estimated with firm level fixed effects, these estimated coefficients are averages. Standard-errors are clustered by treatment group throughout. We use the year prior to the impact year ($k = -1$) as the omitted year.

To report summary DID effects in regression tables, we collapse the thirteen period into four groups, two three-year periods prior and two three-year periods after the offshoring event (explained in more detail in section 6.2.1 below).[17]

Equation (1) is our preferred specification, as we account for both time-invariant firm-specific characteristics and specific differential effects in the treated firm compared to its controls. In the omitted year, estimates of the variable of interest y_{ijt} will be the same for offshorers and their controls by construction ($\widehat{\gamma}_0$), as the firm fixed effects subsume mean differences. Thus in the results that follow, the comparative differences between the two groups, rather than the absolute magnitude, is the relevant statistic.

6 Baseline Results

6.1 Cross-Sectional Comparison of Offshorers and Non-offshorers

We first present a basic comparison in firm characteristics between offshorers and non-offshorers prior to offshoring, adopting the approach in Bernard and Jensen's (1999) study of exporters. To restrict attention to the cross-section for which we have maximum data availability, we use 2002 CMF data, and examine differences between (i) firms that have offshoring events in 2003 or later and (ii) the universe of firms that are not linked to any identifiable offshoring event. We do this by regressing dependent variables on an indicator for offshorers, both with and without 3-Digit SIC industry fixed effects.

The results are shown in Table 1. Our sample of offshorers exhibit premia consistent with what is expected in the model presented in Section 3. Offshorers tend to be significantly larger - in terms of sales, value added, employment and capital, both overall (OLS column) and relative to industry peers (Industry FE column). On average they pay higher wages (for both production and non-production workers) and are more capital intensive. They are also more productive, according to most productivity measures.

[17] As a robustness check, in section 7.1, we run regressions with group-period effects (f_{jk}), which allows for industry-size-period specific shocks (but provides only the relative DID estimates, as the control group effects are absorbed by the cell-year effects).

Interestingly, the non-production wage and employment share measure shows that the offshoring firms are not more skill-intensive than non-offshorers. This is noteworthy given that larger firms are typically both more capital and skill intensive on average; thus future offshorers appear to be significantly less skill-intensive relative to similar-sized firms. This finding is intuitive and consistent with economic theory – we may expect low skill activities to be precisely the ones to be offshored, as these activities would be the ones for which there are the largest gains in offshoring to a low-skill abundant developing country (Krugman 2008).

6.2 Baseline Analysis: DID using Industry-Size Matched Controls

In order to estimate Equation (1), we construct a control group of "similar" firms. The first approach we take is based on industry and employment size, using the LBD. For each offshoring firm, we use total employment in the year prior to the listed impact year, and select two firms with the closest employment directly above and below the offshoring firm in the same 3-digit SIC industry.[18] Firms that have one or more identified offshoring events are excluded from the control group selection pool. Using the LBD sample for control group selection allows us to take advantage of the fact that the LBD covers the entire universe of firms operating in the U.S.; we thus select control firms from a larger pool of firms to improve the similarity to the treated firms. We then merge this sample of treated and control firms to the detailed data in the ASM/CMF. Table 2 presents results of the difference-in-differences estimation for all firms in our sample with employment-matched controls.

6.2.1 Size and Wage Variables

The top rows of Table 2 shows the estimation results for size and wage measures. The column headings refer to the time periods. LR-PRE refers to a long run pre-offshoring period; in this context, we take this to be four to six years prior to the offshoring impact year. SR-PRE refers to the short-run pre-offshoring period (one to three years prior to the impact year), SR-POST the short-run post-offshoring period (one to three years after the impact year) and LR-POST the long-

[18]We impose a restriction that log employment at one of these 'nearest neighbors' cannot be more than 4 points different from the comparison offshorer, meaning that not every offshorer is paired with exactly two controls.

run post-offshoring period (four to six years after the impact year). In this specification, the impact year itself is omitted. All size measures - output, value-added, employment and capital - show a large decline in the short-run. We do not find any evidence of improvement in these size measures even in the long run; in fact, all size measures show continuous decline relative to their controls in the long run. We perform a t-test to explore the short-run and long-run impacts compared to the period leading up to the impact year (SR-PRE) rather than the impact year, with results presented in the columns headed with "Relative to SR-PRE". We again find significantly negative impacts in all size measures for offshorers in both the short-run and long-run; the long-run DID decline in output is 0.326 log points or 27.8%, in employment is 0.38 log points or 31.6%, and in capital is 0.253 log points or 22.4%.

Finally, in the last column, we test for evidence of a pre-existing trend in these offshorer-control comparisons that might be accounting for our results. We find no significant differences between the treatment and control group that would indicate trends in these variables prior to the offshoring event. As for firm wage variables, the differential trend between offshorers and non-offshorers are very small and statistically insignificant. This is the case also for the average wage rate, and production and non-production worker wage rates separately.

These results can be seen graphically in Figure 2. Here we compute coefficients for each event-year (where the omitted event-year is year -1, i.e., one year prior to the offshoring impact year) rather than broader time periods used above. The trend lines for offshorers and the controls with firm fixed effects are shown with 95% confidence bands with standard errors clustered by treatment group. Figure 2 shows that both employment and the total value of shipments for offshoring firms display a drastic decline in the impact year. The event associated with an impact date in the TAA petition data clearly matches a significant layoff event for the firm. More specifically, sub-figure (a) shows that the drastically negative adjustment occurs in the short-run up to four years from the event, then settles at a level that is permanently lower than that of control group. There is little evidence that employment recovers relative to the control group after the initial adjustment. This implies that if there is any job creation from offshoring, it is out-weighed by continuous downsizing within the firm. Sub-figure (b) shows the same trend for output (sales). The lack of wage impact from offshoring is shown more vividly in sub-figures (c) and (d).

6.2.2 Productivity and Factor Intensity

Table 2 also presents results of the difference-in-differences procedure for skill-intensity and firm productivity variables. Offshorers do appear to become more capital-intensive than their controls after the offshoring event, which is a result of a smaller decline in capital compared to the larger fall in employment. The share of non-production workers in total employment also rises at offshoring firms, suggesting that layoffs disproportionately affect production workers, consistent with low-skill activities being targeted for offshoring.

For productivity measurements, we use a number of different variables: in addition to labor productivity measures (output per worker and value added per worker), we also estimate total factor productivity as residuals from a value added production function, estimated alternatively using OLS (with plant-fixed effects) and using the Levinsohn-Petrin (2003) approach to controlling for endogeneity of inputs. While the value-added per worker variable shows improvement in both short- and long-run periods after offshoring, no TFP measures show significant improvement. Sub-figures (e) and (f) of Figure 2 present the labor productivity and Levinsohn-Petrin (LP) TFP measures. The TFP measure has a wide confidence band, and appears to show no systematic (DID) change in relative TFP levels, consistent with the results in Table 2.

6.2.3 Firm Survival

If offshoring is beneficial to the firm, one potential consequence is that offshoring firms will be more likely to survive in the highly competitive environment that manufacturing firms face. Figure 3 shows the survival rate of offshoring firms compared to control group firms. This simply depicts the percentage of plants (sub-figure (a)) or firms (sub-figure (b)) in our LBD sample still in existence for the indicated period. The benchmark year is the year prior to the impact year.[19] Within six years of post-impact observation period, almost 70% of firms disappear from the data.[20] However,

[19]Numbers less than 100% before the impact year indicates that some plants/firms were born between 6 and 1 years prior to their offshoring impact year.

[20]This six year period differs across firms due to the fact that they are aligned around the impact years. They range from 1999 to 2007.

the survival rates for offshoring firms and the controls are nearly identical. We find no evidence that offshoring improves the firm's chance of survival.

6.3 Potential Selection Bias with TAA Petition Data

Because TAA petitions are triggered by layoffs, our sample of offshoring events captures only events where laid-off workers were not absorbed back into the same establishment. This raises two potential issues: One, whether this is a valid sample to test the model presented in Section 3, and two, whether the sample we look at consists of weaker-than-average firms, so that overarching negative trends unrelated to offshoring may be biasing our DID estimates. We believe neither of these concerns apply in our context.

First, we believe our sample is *valid* for testing the model, because the analytical predictions are not based on any particular assumptions about the fate of laid-off workers. In particular, positive spillovers to the rest of the firm does not assume or imply that workers in the offshored activity will be reabsorbed in the same establishment. In fact, it is likely that the offshored tasks systematically differ from the non-offshored tasks, so that workers with skills suitable for the offshored tasks may not be a good fit for the tasks that expand due to gains from offshoring. For example, jobs destroyed due to offshoring could be low-skilled (as the wage advantage for the foreign country is likely to be higher for these tasks) while the newly created jobs may be in relatively high-skilled occupations. Thus workers who used to perform offshored activities are not necessarily likely to be absorbed by the same establishment, under the assumptions of the model. Accordingly, the sample of offshoring events identified using the TAA petition data is a valid one to look for positive spillovers to other parts of the firm.

Second, for every variable of interest, we perform two tests for pre-existing trends. One, following the suggestion in Angrist and Pischke (2009), we examine an event study graph that plots the trend for each of the key outcome variables, both before and after the offshoring event, for both the treatment and control group. As is evident from Figure 2, all the dependent variables show similar trends for the offshorers and the control group prior to the offshoring event; for employment and output there is a stark break in trend coincident for offshorers coincident with the initiation of offshoring. In particular, there is no evidence of a strong decline in output or sales prior to the offshoring event, in absolute terms or relative to the peer group. Two, we explicitly test for pre-existing trends in our regression analysis (column 7 in Table 2). None of the dependent variables shows any significant prior trend.

While we believe these tests provide considerable evidence that pre-existing differences in offshoring firm characteristics are not driving the baseline results, we check robustness of our results using an alternative method of constructing a control group – by matching on the propensity score (generated from a model predicting the propensity to offshore). This approach is discussed in detail below.

6.3.1 Propensity-matched Controls

We re-select control firms by matching on the propensity score. Specifically, we estimate the probability of offshoring for all firms based on a variety of firm characteristics, and find firms that did not offshore despite having an predicted probability very close to actual offshorers. The potential advantage of this alternative approach is that any post-offshoring effects driven by interaction of pre-existing characteristics with changes in the environment are controlled for by matching on this scalar propensity measure (Rosenbaum and Rubin 1985), assuming certain conditions hold. Specifically, this approach lets us incorporate a number of covariates other than size in forming the control group. First, we estimate the following linear propensity model:

$$\mathit{Offshore}_{ikt} = \beta X_{ikt} + \delta_t + \delta_k + \varepsilon_{ikt} \tag{2}$$

We couple the observed offshoring decision (zero or one) for firm i in industry k at time t, $\mathit{Offshore}_{ikt}$ with a vector of firm-level covariates, X_{ikt}, found in the ASM and CMF, including capital intensity, skill intensity, output, and three-year employment and wage growth rates, in order to predict the probability of offshoring given those characteristics. If baseline results are driven by pre-existing differences for offshoring firms on these characteristics, then matching on the propensity score will help control for that bias.

The results from estimation of the propensity model are presented in Table 3. We find employment growth has no significant predictive power; however wage growth enters negatively, suggesting some prior cost pressure on offshorers. Consistent with the cross-sectional differences documented in Table 1, we find that higher labor productivity, lower skill-intensity and higher capital intensity predict higher propensity to offshore. Next, we use the predicted propensity to form control groups. and then undertake analyze using Equation 1.

6.3.2 Difference-in-Differences Estimation using Propensity-matched Controls

Table 4 presents results from DID estimation using propensity score matched controls. The results are qualitatively identical to the estimation with employment-matched controls shown in Table 2. As in the estimation with employment-matched control, all size measures - output, value added, employment, and capital - decline significantly immediately after the impact year and the downward trend continues into the long-run. We perform a t-test to capture the differences between the short-run pre-offshoring period and both the short-run and long-run post-offshoring periods, again finding significant negative differences for offshorers. Finally, in the last column, we test for evidence of any significant pre-offshoring differential trends between the treatment and control groups, and we find none.

The impact of wage rates are also qualitatively identical to what we found using employment-matched control. Neither production nor non-production worker wage rates are significantly influenced by offshoring in both short- and long-run.

The bottom panel of Table 4 presents results for factor intensity and productivity measures. Again, the results are qualitatively identical to what we find using employment-matched controls. Offshorers do appear to become more capital-intensive than their controls after the offshoring event, which is again the result of lower decline in capital relative to employment. The share of non-production workers in total employment also rises at offshoring firms. Measures of labor productivity improve - weakly for shipment per worker, more strongly for value added per worker - consistent with a lower decline in output relative to employment. However, again there is little evidence of comparative TFP gains at these offshoring firms compared to their controls in both the short- or long-run.

These results are graphically presented in Figure 4. All sub-figures display a striking resemblance to Figure 2. This demonstrates that our results, particularly the lack of evidence for firm-level benefits from offshoring, are not driven by the nature of the control group we selected using employment-matching for the baseline analysis.

7 Robustness Checks

In order to check the robustness of our results, we perform the same difference-in-differences estimation using various alternative specifications and sub-samples.

7.1 Estimation using Treatment Group-Year Fixed Effects

Our baseline regression specifications use firm fixed effects and period effects; while this controls for all possible fixed firm specific effects, the time-varying effects are assumed to effect all controls and offshored firms similarly. In order to allow for group-specific shocks, which effectively allows period effects to vary by industry-size groups, we estimate a variant of Equation (1) that includes group-year fixed effects. The coefficients in these regressions then report variations in offshored firms relative to their matched controls for every period. Those set of results on firm performance measures are included in Table 5, using both employment- and propensity-score-matched controls.

Overall, these results are qualitatively similar to the baseline findings described above, except that changes in skill intensity and labor productivity measures are no longer statistically significant. All size measures decline rapidly compared to the control firms in the short-run. Wage measures show no significant changes. Capital intensity increases, but this is significant only in the propensity-matching analysis; skill intensity measures show no statistically significant changes. Here none of the productivity measures show a significant change following offshoring.

7.2 Longitudinal Business Database Results

In this subsection, we use employment and payroll (and hence average wage) data available on all establishments in the LBD, to check robustness of the baseline results to two concerns.

One, it could be the case that employment gains from offshoring are experienced in non-manufacturing establishments of the firm; in particular at the headquarters, or in wholesale or retail establishments of the firm. The latter would be the case if the product offshored was sold in the U.S. through the firm's marketing arm. This would be missed in the baseline analysis that uses manufacturing ASM/CMF data. Because the LBD data includes data on headquarters as well as marketing (whole and retail trade) establishments, using this data would allow us to examine domestic firm-level aggregates that include potential gains in these units.

Two, examining the LBD allows us to check robustness to potential bias from sampling in the ASM, discussed in footnote 15 in Section **??**. Because ASM sampling puts more weight on the larger establishments, small establishments in our TAA petition are less likely to be selected into our ASM/CMF sample of offshoring events. Using the LBD sample allows us to check robustness of our findings to potential bias from this sampling procedure.

Table reports results form estimation of Equation (1) using the LBD sample. This raises the sample size from 7,000-9000 offshorer-year observations (depending on the matching technique) to over 12,000. Note that since we drop offshoring firms that cannot be matched to any control, we have different observation counts, even for underlying offshorer-year observations. Such an approach has many fewer observations in the propensity score matching results, as any firm without one of the covariates used to compute the probability of offshoring get be excluded. The total number of offshoring events increases from approximately 1,000 (in the ASM/CMF analysis to 1,400 here.

Due to the fact that there is a limited number of operational variables in LBD, we can only perform the analysis on total employment, total payroll, and average wage rate. For propensity score matching analysis with the LBD, we have less variables to "match" on, and thus use only employment, wage rate, 3-year employment growth rate, and 3-year wage growth rate as our covariates for Equation (2).

We find results similar to those using the ASM/CMF sample. Employment and total payroll drop greatly compared to the control groups in the short-run and it remains low in the long-run. While the magnitude of the long-run effect for employment in the employment-matching approach (-0.138 log points) is lower than the long-run decline in the baseline approach (-0.38 log points in

Table 2), the magnitude of decline in the propensity matched approach (-0.366) is very similar to that in the baseline (-0.37 log points in Table 4). The DID effect on average wage rates for offshoring firms is a statistically significant decline of 0.029 log points in the short run, and a gain of 0.061 log points in the long run when we use employment-matched sample; however in the propensity-matched sample we find no statistically significant changes (though magnitudes are similar to that with the employment matched sample). Payroll shows a significant decline, both in the short and long-term, with long-term decline being considerably larger in the propensity-matching analysis.

These results suggest: (i) no significant net employment gains in domestic activities, even including headquarters and marketing units, and no significant increases in wage rates; and (ii) baseline findings for size (employment) and wages are not impacted by loss of data from sampling in the ASM. As discussed in footnote 15, the robustness of the baseline results is not very surprising , given the large degree of overlap between the ASM/CMF and LBD samples.

7.3 Multi-Unit Firms

In the model in Section 3, as some tasks of a firm are offshored, the other tasks that remain at home benefit through the vertical supply links. At the offshored plant itself, the destructive nature of offshoring might dominate any potential job creation making it difficult to capture the positive effects. For single-unit firms, this offshored plant constitutes the offshoring firm.

In this section, we analyze the impact of offshoring using only the multi-unit firms to allow the potential positive effects to be better captured. Close to 80% of our ASM/CMF sample is multi-unit. Table 7 presents the estimation results. The results are qualitatively identical to our baseline analysis of all firms including single-unit firms. Figure 5 shows the trend for total employment and total value of shipments for the estimation using employment-matched controls. One can see that both employment and shipment figures are very similar to subfigures (a) and (b) of Figure 2.

7.4 Pseudo-Firm: Non-offshored Plants in Multi-unit Firms

We take one step deeper into separating the potential positive effects of offshoring from the destructive effects at the offshored plants by looking only at non-offshored plants within the offshoring firms. Specifically, we construct a "pseudo-firm" by aggregating all plants at a firm that are not matched with any offshoring events from the TAA petition data. We then construct the firm-level variables using only these plants.[21] By construction, only multi-unit firms are candidates to be pseudo-firms. Our sample of offshorer-year observations drops to 2,161, out of over 7,000 offshorer-years in the original sample.

The results are shown in Table 8. It is clear that even those plants that are not hit directly by offshoring do not display any sign of gains in size, wages, or productivity compared to their controls. In fact, we find that size variables (output, value added, employment and capital) decline significantly both in the short and long-term for these pseudo-firm aggregates of non-TAA plants within offshoring firms. Wage rates and productivity generally show no significant changes; capital and skill intensity show some increase consistent with the baseline effects.

These results strongly confirm that remaining domestic activity of the offshoring firms in our sample *do not* experience positive spillovers in output, employment, wages or productivity; in fact, the results suggest significant decline in output and employment in unaffected units as well. This is suggestive of elimination of supporting activities in remaining units following offshoring.

7.5 Vertical Linkages

As discussed in Section 3.3, the vertical supply links between offshored plant and remaining domestic plants is crucial for positive spillovers from offshoring. If there are no vertical linkages, we cannot expect to see improvement in firm-level operation. Thus we could expect to see more gains for firms where the offshored plant is vertically linked to the remaining domestic plants. We check if this is the case in this section.

In order to build the vertical supply links, we use the Input-Output (IO) table of industries for 2007 published by the Bureau of Economic Analysis. The Input-Output table distinguishes between *Final Products* and *Intermediate Products*, listing the purchase value of each intermediate product used to create a final product. Similar to the procedure outlined in Atalay, Hortacsu, and

[21] Total employment and firm industry are reconstructed using these non-offshored plants only, then are used to construct the employment-matched controls. The controls selected using propensity-score matching also utilized the variables of the pseudo-firms.

Syverson (2012), we classify two industries as vertically linked if one industry makes up more than 1% of the total purchase value of all inputs used to produce the final goods. Using the industry code for each establishment, we determine an offshoring firm as vertically linked if the offshored plant is vertically linked to at least one other plant within the firm. About 30% of the original sample fits this definition of vertically-linked offshoring firms. Table 9 summarizes the estimation results. The results for employment and shipments are shown in Figure 6 for employment-matched controls. While the reduction in sample size increases the standard errors of the estimations, the overall pattern of the short-run and long-run impacts are very similar to the ones we find in other specifications.

The lack of significant difference between vertically-linked firms and the others in the wake of offshoring can potentially be explained by the fact that linkages measured using Input-Output tables do not necessarily translate to actual vertical linkages in the form of intra-firm shipment in the data, as carefully documented by Atalay, Hortacsu, and Syverson (2012) using U.S. Commodity Flow Survey data. They find that firms that are identified as vertically-linked rarely use inputs made by other establishments within the firm. Their analysis is for domestic intra-firm shipment in the U.S. manufacturing sector. Ramondo, Rappaport, and Ruhl (2012) look at the cross-border intra-firm shipment of U.S. multinationals using the BEA data. They find that while most multinationals display vertical linkages per the I/O tables, there is very little actual intra-firm shipments. They find that the majority of output from the foreign subsidiaries are sold locally and that the median subsidiary reports no shipment to the U.S. parent. Both studies attribute the identifiable vertical links among establishments without actual shipment to knowledge capital.

This analysis, and the studies cited above, suggest a plausible explanation of our baseline results: vertical linkages across establishments within firms are weak, even if the plant is vertically linked per the IO table. This suggests that the effects of offshoring are likely to be similar top that envisaged in H-FDI models, rather than as in the model sketched in Section 3.

7.6 Balanced Panel Results

Finally, we investigate whether our results are affected by firm entry and exit. In particular, differential patterns in exit by offshoring firms relative to controls, could affect the baseline results. For example, short-term exit by the largest offshoring firms could lead to smaller relative sizes for offshorers in the long-term after offshoring.[22]

In order to address the concern described above, we re-estimate our results using only firms who were present for all 13 years of the 13 year event window (the true balanced panel).[23]. The balanced panel consists of approximately 30% of our baseline sample and contains 2,330 offshorer-years. We find baseline results are robust in this sub-sample – results are available upon request.

7.7 Other Robustness Checks

We also undertook a number of additional robustness checks, which we summarize without reporting tables for brevity. First we tried alternative methods for aggregating TFP, as described in Section 4.3. Second, we checked robustness of key results to using only a sample of firms that filed a single offshoring petition in the sample period. Third, we repeated the analysis only for single-unit firms. Fourth, we altered the composition of covariates in the propensity score estimation to exclude 3-year growth rates or overall employment. Fifth, we repeated the analysis at the plant level only. Sixth, we checked robustness to examining a subsample of pre-2002 offshorers; results suggest no changes in the pattern of findings over different years. Finally, we performed a number of concurrent checks: multi-unit firms in a balanced panel, pseudo-firms that were vertically linked, pseudo-firms using LBD data. Our baseline results robust to using these alternative specifications and definitions.

8 Discussion and Conclusion

We use specific information on the source of trade-related layoffs available in the assessments of petitions filed under the U.S. Trade Adjustment Assistance program to identify offshoring events.

[22] We note that this bias is controlled for in the analysis with treatment group-year fixed effects in Section 7.1, as exiting firms do not contribute to estimated effects (their groups get absorbed by the group-year effects).

[23] ASM/CMF data go up to 2009. In order to retain all six years after offshoring, we need to drop offshoring events that occurred after 2003. Again, our offshoring events in the baseline sample ranges from 1999 to 2006.

We link this data on initiation of offshoring activity to rich U.S. Census micro datasets, namely the Longitudinal Business Dataset (LBD), Census of Manufactures (CMF), and Annual Survey of Manufactures (ASM). We examine changes in key outcome variables for offshorers relative to controls (matched alternatively on size and propensity score within the same industry) using a standard difference-in-differences methodology.

We find that employment declines significantly at the firm level following offshoring. The DID decline in employment relative to controls is statistically and economically significant – about 19% in the short run and 32% in the longer run. These findings are robust to using alternative control groups. We verify that this is not simply the result of decline at the affected plant; we find that employment falls significantly (only slightly lower in percentage terms than at the affected plants) at aggregated non-affected establishments. This shows that for our sample of offshoring events, offshoring results in net negative employment effects, as well as negative effects at remaining domestic units. While our baseline analysis uses only manufacturing sector data, we obtained similar results using data from the LBD, which covers non-manufacturing establishments as well; there was no employment increase, even including non-manufacturing establishments.

We also examine a number of other outcomes including output, value added, capital, wage rates, labor and total factor productivity, as well as capital and skill intensity using the ASM/CMF data. We find offshoring firms drastically reducing their size (output, value added and capital) compared to controls, with little evidence of increases (or decreases) in productivity or wages. Firms reduce workers more than capital, so capital intensity goes up; this is also reflected in higher labor productivity, but we find no change in more detailed TFP measures relative to the control group.

In the TAA data we observe only those offshoring firms who did not re-absorb their workers ('non-absorbers'); as we argue in Section 6.3, even so, we believe that this is a valid sample to check for potential complementarities in other parts of the firm. Further, our figures plotting prior trends for the offshored sample and controls, as well as explicit tests for prior-trends, suggest no strong prior downward trend for offshored firms relative to controls. Nevertheless, our results should be considered as average effects for non-absorbers, rather than for offshorers as a whole.

Our findings suggest that the pathway of vertical linkages crucial in the Grossman and Rossi-Hansberg class of models is not operational in our data. Thus the offshoring activity in our sample may be the shifting of whole product lines abroad, more closely resembling horizontal FDI (H-FDI) in the Markusen and Maskus (2001) model. This type of H-FDI would not result in any positive spillovers, and may generate negative employment and output spillovers, if some supporting activities in other domestic units are closed down following the production shift. In this sense, our results are in line with the findings of Atalay, Hortacsu, and Syverson (2012) and Ramondo, Rappoport and Ruhl (2012) who find very little evidence of intra-firm shipments (even within firms who have establishments in that are vertically linked per the IO tables). Our tentative conclusion that most of our offshoring events may be related to horizontal shifts echoes Ramondo, Rappoport and Ruhl's (2012) conclusion that most foreign affiliate activity is "horizontal" in nature rather than truly vertically linked to domestic activities of MNCs.[24]

It should be strongly emphasized that our results *do not* imply negative welfare effects from offshoring. In fact, given data limitations, two of the key channels of gains – reduced output prices and increased global firm profits – are not measured in this paper. Welfare losses from under-utilization of labor resources would depend on the how long the displaced workers take to find new jobs, which we cannot address with this data. We hope to undertake follow-up work to address these issues using different data sources: Comtrade for publicly listed companies to assess impact of offshoring on global profits, and U.S. Census shipment level trade data to measure unit values which can be used for estimating price effects for products exported by offshoring firms. Using the link between TAA and the business register that we have developed, it is also possible to use the U.S. Census' employee-employer linked dataset (LEHD) to examine unemployment durations and wage effects on workers at affected plants.

[24]One point to keep in mind while interpreting our results are that we focus on the first offshoring event in the sample, while other studies that use imported inputs or MNC data may be looking at changes within existing offshorers. The employment effect of offshoring, even in the case of vertically linked firms, is arguably more negative for new offshorers than for incumbent ones because they go through the initial large-scale job destruction when tasks are first sent abroad. As discussed in Sethupathy (2011), a fall in offshoring cost makes the already-offshored tasks even cheaper, which generates new jobs in domestic operation without necessarily causing any additional displacement in incumbent offshorers. New offshorers do not enjoy this purely positive employment impact. Thus one interpretation of the steep decline we find in employment in the short run could be that this captures the new offshorer's employment adjustment. However, in such models with positive spillovers via vertical linkages, it would be very puzzling to see strong decline in output and employment in the non-affected plants as well.

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A Data Appendix

In this appendix, we describe how we created a baseline dataset of offshoring plants.

A.1 Linking TAA to SSEL

The operational information of manufacturing establishments used in this paper is obtained from the Longitudinal Business Database (LBD) and Annual Survey and Census of Manufactures (ASM/CMF). The information on offshoring events is obtained from the petition data of the Trade Adjustment Assistant program (TAA). Unfortunately, direct matching of these two data are not possible because TAA petition data do not have establishment or firm identifiers. The information that can identify a particular establishment is company name and address (state, city, street address, and zip code), but neither the LBD nor the ASM/CMF contains address information. For this reason, we first match the TAA petition data to the SSEL (formerly known as the Business Register) using name and state, then match the merged data to LBD using plant identifiers.

Name and address matching between TAA petition data and SSEL is imperfect because TAA petitions are filed by workers and unions, rather than the authority that generally responds to various surveys conducted by the U.S. Census Bureau. The company names and address reported in the TAA petition form is not necessarily the official name or address. Also, there is no rule against using P.O. Box address for the purpose of survey response for both TAA petitions and any survey from the Census Bureau. To avoid being too restrictive, we use only name and state as matching criteria. Company names have inconsistencies and ambiguities too. The majority of the issues here stems from variations in the legal endings of companies such as 'Limited,' 'Incorporated,' 'Corporation.' We drop those legal endings before merging. Other corrected issues, where possible, are numerics (e.g. '1' v. 'one'), other abbreviations (mfg, tech, bros, and so on), and simple typos.

We made separate merging for petitions with different years. Since our petition dataset contains petitions with impact date from 1999 to 2006, we performed merging of eight separate years. TAA petitions with each impact year is merged with four SSEL years surrounding the impact year; more specifically, two years prior to the impact year, impact year, and one year after. For instance, petitions with impact year of 2003 is merged with SSEL files from 2001, 2002, 2003, and 2004. Using additional matching criteria (zip code), we selected the year of the best match among these four years merged and obtain plant identifiers from the corresponding SSEL files. Table A1 summarizes the matching rate for each impact year for aggressive matching. Out of total of 19,603 petitions in our sample, 13,645 are matched to SSEL yielding a matching rate of 69.61%. Among the matched petitions, 5,167 petitions are identified as offshoring events.

A.2 Linking to LBD

In order to make a longitudinal link for surveys of different years for one establishment, we use the LBD. For each petition we match the petition information to the LBD file of the year of best SSEL match rather than impact year because the plant identifiers of the best SSEL year are most reliable. This SSEL-LBD matching rate is 76.41% for all sample. Since the first impact year of the petition data is 1999, and it is matched to one of four years surrounding the impact year, the range of SSEL years thus goes from 1997 to 2007. Merging is carried out for each year separately, then was appended.

Once the establishment ID is retrieved for all offshoring events, we build the event window of 13 years; six years before and six years after the event. Before we construct the event window, we first deal with the issue of multiple petitions per establishment. Some establishments file the petition more than once over time. All petitions are not necessarily filed for the same reason. We give priority to offshoring event, import-related event, and denied event. Among the petitions certified for the same reason, or denied petitions, we keep the first event. For instance, if a plant A is certified for import-related reasons in 2001, for an offshoring-related reason in 2003, and denied in 2004, we keep the 2003 event of offshoring. If a plant is certified for offshoring in 2002 and 2004, then we keep the 2002 event. Multiple offshoring events for a firm in the same year are treated as one offshoring event for the firm since all analysis are carried out at the firm-level. In construction of pseudo firms (aggregation of non-offshored plants of offshoring firms), all offshored plants are dropped. Table A2 summarizes the total number of events after this sorting with petitions matched to LBD. At this stage, we have 3,400 offshoring events, 1,618 import-related events, and 3,835 denied petitions to be total of 8,853 petitions.

A.3 Building firm-level links

A firm link is built with the variable firmid in LBD. For each year, we group all establishments by firmid, including non-manufacturing units. For each firm, we construct three firm-level variables. We first construct firm-level employment by aggregating all establishment-level employment. Average wage rate is constructed by dividing the aggregate payroll by aggregate employment. Lastly firm-level 3-digit SIC code is selected. We aggregate employment by industry within the firm, then select the 3-digit SIC industry that has the largest employment in the firm. Offshoring firm is selected by matching the firmid of the offshored establishment to the firm-level data constructed as described above. The matching is obtained for the year before the offshoring event. We build the event window of 13 years by adding six years before and after the offshoring event.

Table 1. Cross-sectional Comparison of Offshoring Firms to Non-offshorers Prior to Offshoring

Variable	Definition	OLS	Industry FE
Size Measures			
Output	Log(Total Sales)	3.044	2.607
		(0.000)	(0.000)
Value Added	Log(Value Added)	2.919	2.521
		(0.000)	(0.000)
Employment	Log(Employment)	2.679	2.313
		(0.000)	(0.000)
Capital	Log(Capital)	3.336	2.949
		(0.000)	(0.000)
Wage Measures			
Wage Rate	Log(Total Wage Bill/ total employment)	0.045	0.044
		(0.001)	(0.000)
NPW Wage Rate	Log(Non-production wage bill/ non-production employment)	0.082	0.040
		(0.000)	(0.016)
PW Wage Rate	Log(Production wage bill/ production employment)	0.011	0.049
		(0.447)	(0.000)
Factor Intensity			
Capital Intensity	Log(Capital/ total employment)	0.656	0.636
		(0.000)	(0.000)
NPW Emp Share	Non-production share of employment	−0.009	−0.009
		(0.131)	(0.112)
NPW Wage Share	Non-production share of wage bill	0.003	−0.012
		(0.660)	(0.018)
Productivity			
Output per Worker	Log(Total sales/employment)	0.364	0.294
		(0.000)	(0.000)
VA per Worker	Log(Value added/employment)	0.240	0.208
		(0.000)	(0.000)
TFP-Levpet	TFP- Levpet, Value Added	0.088	0.054
		(0.026)	(0.028)
TFP-OLS	TFP- OLS (fixed effects), Value Added	0.618	0.559
		(0.000)	(0.000)

Notes: The number of observations for all of the statistics is 131,377. The p-values are reported in parenthesis. Industry-level fixed effects are at the 3-Digit SIC level. The data source is the Census of Manufactures for 2002.

Table 2. Difference-in-Differences Estimation - All Firms, Employment-Matched

					Relative to SR_PRE		Pre-Trend Test
	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST-SR_PRE	SR_PRE-LR_PRE
Size Measures							
Output	0.069	0.048	-0.179	-0.278	-0.227	-0.326	-0.021
	(0.055)	(0.052)	(0.000)	(0.000)	(0.000)	(0.000)	(0.416)
Value Added	0.097	0.075	-0.228	-0.316	-0.303	-0.391	-0.022
	(0.016)	(0.012)	(0.000)	(0.000)	(0.000)	(0.000)	(0.452)
Employment	0.07	0.041	-0.211	-0.339	-0.252	-0.38	-0.029
	(0.039)	(0.059)	(0.000)	(0.000)	(0.000)	(0.000)	(0.247)
Capital	0.004	0.005	-0.116	-0.248	-0.121	-0.253	0.001
	(0.920)	(0.841)	(0.000)	(0.000)	(0.000)	(0.000)	(0.770)
Wage Measures							
Wage Rate	-0.002	-0.003	-0.004	0.002	-0.001	0.005	-0.001
	(0.834)	(0.711)	(0.719)	(0.904)	(0.980)	(0.701)	(0.892)
NPW Wage Rate	-0.019	-0.003	-0.036	-0.027	-0.033	-0.024	0.016
	(0.373)	(0.865)	(0.046)	(0.254)	(0.051)	(0.272)	(0.337)
PW Wage Rate	0.007	0.008	-0.004	0.006	-0.012	-0.002	0.001
	(0.596)	(0.430)	(0.757)	(0.682)	(0.659)	(0.291)	(0.122)
Factor Intensity							
Capital Intensity	-0.066	-0.046	0.096	0.091	0.142	0.137	0.02
	(0.038)	(0.029)	(0.000)	(0.020)	(0.000)	(0.001)	(0.406)
NPW Emp Share	-0.001	-0.003	0.021	0.022	0.024	0.025	-0.002
	(0.904)	(0.589)	(0.000)	(0.004)	(0.000)	(0.002)	(0.690)
NPW Wage Share	-0.007	-0.003	0.015	0.013	0.018	0.016	0.004
	(0.313)	(0.603)	(0.005)	(0.099)	(0.003)	(0.055)	(0.444)
Productivity							
Output per Worker	0.027	0.034	-0.015	0.024	-0.049	-0.01	0.007
	(0.313)	(0.139)	(0.555)	(0.503)	(0.045)	(0.775)	(0.720)
VA per Worker	-0.002	0.006	0.035	0.062	0.029	0.056	0.008
	(0.936)	(0.689)	(0.043)	(0.017)	(0.117)	(0.033)	(0.585)
TFP- Levpet	0.017	0.049	-0.057	-0.03	-0.106	-0.079	0.032
	(0.589)	(0.055)	(0.040)	(0.453)	(0.000)	(0.052)	(0.156)
TFP- OLS	0.034	0.048	0.016	0.022	-0.032	-0.026	0.014
	(0.267)	(0.060)	(0.549)	(0.603)	(0.023)	(0.519)	(0.506)

Notes: The number of observations for each regression (row) is 22,556. Refer Table 1 for variable definitions. The column headings refer to time periods. LR-PRE refers to a long run pre-offshoring period (four to six years prior to the offshoring impact year). SR-PRE refers to the short-run pre-offshoring period (one to three years prior to the impact year), SR-POST the short-run post-offshoring period (one to three years after the impact year) and LR-POST the long-run post-offshoring period (four to six years after the impact year). The variables correspond to those in Table 1. All specifications include event-year and firm fixed effects. The figures in parenthesis are p-values.

Table 3. Propensity Model Estimates

Variable	Coeff
3-year Employment Growth	0.01
3-year Wage Growth	-0.007^{**}
Output per Worker	0.012^{**}
NPW Emp Share	-0.009^{**}
Capital Intensity	0.012^{**}
Constant	-0.077^{**}
R-sq	0.06

Notes: Dependent variable is a dummy=1 if the firm offshored in any year in the sample period. Refer Table 1 for variable definitions. Number of observations is 16,296. ** denotes significance at 1% level and * at 5% level.

Table 4. Difference-in-Differences Estimation - All Firms, Propensity Score Matching

					Relative to SR_PRE		**PE Trend Test**
	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST- SR_PRE	SR_PRE- LR_PRE
Size Measures							
Output	-0.019	0.016	-0.128	-0.239	-0.144	-0.255	0.035
	(0.624)	(0.529)	(0.000)	(0.000)	(0.000)	(0.000)	(0.228)
Value Added	0.035	0.061	-0.159	-0.295	-0.22	-0.356	0.026
	(0.424)	(0.055)	(0.000)	(0.000)	(0.000)	(0.000)	(0.412)
Employment	-0.011	0.011	-0.193	-0.359	-0.204	-0.37	0.022
	(0.741)	(0.624)	(0.000)	(0.000)	(0.000)	(0.000)	(0.392)
Capital	0.019	-0.019	-0.052	-0.171	-0.033	-0.152	-0.038
	(0.660)	(0.484)	(0.073)	(0.003)	(0.288)	(0.009)	(0.239)
Wage Measures							
Wage Rate	-0.011	-0.008	0.007	0.037	0.015	0.045	0.003
	(0.384)	(0.412)	(0.503)	(0.015)	(0.145)	(0.002)	(0.731)
NPW Wage Rate	0.011	-0.006	-0.009	0.053	-0.003	0.059	-0.017
	(0.674)	(0.757)	(0.674)	(0.072)	(0.870)	(0.030)	(0.374)
PW Wage Rate	-0.028	-0.015	0.001	0.009	0.016	0.024	0.013
	(0.048)	(0.187)	(0.992)	(0.603)	(0.185)	(0.131)	(0.238)
Factor Intensity							
Capital Intensity	0.031	-0.03	0.141	0.188	0.171	0.218	-0.061
	(0.379)	(0.190)	(0.000)	(0.000)	(0.000)	(0.000)	(0.017)
NPW Emp Share	-0.006	-0.003	0.016	0.021	0.019	0.024	0.003
	(0.379)	(0.610)	(0.003)	(0.016)	(0.001)	(0.008)	(0.510)
NPW Wage Share	0.001	-0.002	0.016	0.03	0.018	0.032	-0.003
	(0.992)	(0.734)	(0.008)	(0.002)	(0.007)	(0.001)	(0.761)
Productivity							
Output per Worker	0.047	0.05	0.034	0.063	-0.016	0.013	0.003
	(0.114)	(0.059)	(0.219)	(0.129)	(0.562)	(0.735)	(0.876)
VA per Worker	-0.007	0.004	0.065	0.119	0.061	0.115	0.011
	(0.741)	(0.803)	(0.000)	(0.000)	(0.001)	(0.000)	(0.462)
TFP- Levpet	-0.012	0.025	-0.043	0.009	-0.068	-0.016	0.037
	(0.734)	(0.384)	(0.177)	(0.841)	(0.025)	(0.712)	(0.130)
TFP- OLS	-0.031	0.023	0.017	0.064	-0.006	0.041	0.054
	(0.384)	(0.424)	(0.589)	(0.177)	(0.856)	(0.348)	(0.029)

Notes: The number of observations for each regression (row) is 18,949. Refer Table 1 for variable definitions. See notes to Table 2 for explanation of column titles. All specifications include firm fixed effects and event-year effects; the figures in parenthesis are p-values.

Table 5. Difference-in-Differences Estimation - All Firms, Treatment Group-Year Fixed Effects

	Employment-Matched							Propensity-Matched						
					Relative to SR_PRE		PE Trend Test					Relative to SR_PRE		PE Trend Test
	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST- SR_PRE	SR_PRE- LR_PRE	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST- SR_PRE	SR_PRE- LR_PRE
Size Measures														
Output	0.081	0.05	-0.132	-0.253	-0.182	-0.303	-0.031	-0.123	0.038	-0.25	-0.224	-0.288	-0.262	0.161
	(0.177)	(0.285)	(0.007)	(0.004)	(0.001)	(0.001)	(0.497)	(0.129)	(0.529)	(0.001)	(0.072)	(0.004)	(0.036)	(0.003)
Value Added	0.117	0.079	-0.167	-0.251	-0.246	-0.33	-0.038	-0.062	0.091	-0.248	-0.267	-0.339	-0.358	0.153
	(0.069)	(0.126)	(0.004)	(0.009)	(0.000)	(0.001)	(0.444)	(0.465)	(0.168)	(0.003)	(0.043)	(0.000)	(0.007)	(0.008)
Employment	0.093	0.041	-0.17	-0.263	-0.211	-0.304	-0.052	-0.104	0.018	-0.262	-0.311	-0.28	-0.329	0.122
	(0.067)	(0.242)	(0.000)	(0.000)	(0.000)	(0.000)	(0.199)	(0.156)	(0.741)	(0.000)	(0.006)	(0.000)	(0.004)	(0.015)
Capital	0.047	0.001	-0.082	-0.235	-0.083	-0.236	-0.046	-0.099	-0.024	-0.154	-0.178	-0.13	-0.154	0.075
	(0.549)	(0.992)	(0.208)	(0.027)	(0.194)	(0.033)	(0.457)	(0.267)	(0.704)	(0.052)	(0.197)	(0.115)	(0.260)	(0.242)
Wage Measures														
Wage Rate	-0.009	-0.002	0.007	-0.019	0.009	-0.017	0.007	-0.014	0.001	-0.02	0.014	-0.021	0.013	0.015
	(0.667)	(0.904)	(0.704)	(0.484)	(0.617)	(0.514)	(0.662)	(0.472)	(0.984)	(0.254)	(0.976)	(0.225)	(0.583)	(0.292)
NPW Wage Rate	-0.022	-0.003	-0.024	-0.04	-0.021	-0.037	0.019	0.012	0.011	-0.026	0.043	-0.037	0.032	-0.001
	(0.516)	(0.912)	(0.401)	(0.303)	(0.413)	(0.298)	(0.471)	(0.719)	(0.682)	(0.368)	(0.162)	(0.162)	(0.382)	(0.941)
PW Wage Rate	0.002	-0.006	0.004	-0.005	0.01	0.001	-0.008	-0.019	-0.005	-0.013	0.003	-0.008	0.008	0.014
	(0.920)	(0.726)	(0.865)	(0.865)	(0.619)	(0.957)	(0.610)	(0.379)	(0.772)	(0.497)	(0.920)	(0.654)	(0.767)	(0.380)
Factor Intensity														
Capital Intensity	-0.046	-0.041	0.088	0.029	0.129	0.07	0.005	0.004	-0.042	0.108	0.132	0.15	0.174	-0.046
	(0.472)	(0.435)	(0.126)	(0.741)	(0.014)	(0.425)	(0.909)	(0.928)	(0.238)	(0.013)	(0.082)	(0.000)	(0.018)	(0.245)
NPW Emp Share	-0.004	-0.002	0.016	0.015	0.018	0.017	0.002	-0.005	-0.002	0.006	0.016	0.008	0.018	0.003
	(0.667)	(0.779)	(0.095)	(0.327)	(0.060)	(0.252)	(0.803)	(0.624)	(0.834)	(0.549)	(0.294)	(0.452)	(0.235)	(0.650)
NPW Wage Share	-0.008	-0.001	0.014	0.008	0.015	0.009	0.007	-0.001	0.001	0.003	0.023	0.002	0.022	0.002
	(0.490)	(0.912)	(0.208)	(0.603)	(0.188)	(0.562)	(0.437)	(0.920)	(0.992)	(0.749)	(0.156)	(0.751)	(0.146)	(0.899)
Productivity														
Output per Worker	0.025	0.038	0.004	0.012	-0.034	-0.026	0.013	0.041	0.073	0.014	0.044	-0.059	-0.029	0.032
	(0.582)	(0.332)	(0.936)	(0.849)	(0.405)	(0.669)	(0.694)	(0.332)	(0.052)	(0.734)	(0.478)	(0.132)	(0.601)	(0.274)
VA per Worker	-0.012	0.009	0.038	0.011	0.029	0.002	0.021	-0.02	0.02	0.012	0.087	-0.008	0.067	0.04
	(0.772)	(0.787)	(0.276)	(0.841)	(0.390)	(0.968)	(0.467)	(0.575)	(0.484)	(0.704)	(0.064)	(0.795)	(0.143)	(0.100)
TFP- Levpet	0.068	0.096	-0.029	0.002	-0.125	-0.094	0.028	0.029	0.077	-0.054	-0.01	-0.131	-0.087	0.048
	(0.150)	(0.020)	(0.555)	(0.976)	(0.007)	(0.177)	(0.402)	(0.624)	(0.057)	(0.267)	(0.889)	(0.004)	(0.199)	(0.119)
TFP- OLS	0.055	0.076	-0.014	-0.039	-0.09	-0.115	0.021	-0.029	0.056	-0.028	0.031	-0.084	-0.025	0.085
	(0.246)	(0.075)	(0.772)	(0.617)	(0.062)	(0.139)	(0.514)	(0.555)	(0.184)	(0.555)	(0.682)	(0.066)	(0.722)	(0.010)

Notes: The number of observations for the employment-matched sample regressions (first seven columns in each row) is 22,556, and for the propensity-matched sample regressions (last seven columns in each row) is 18,949. Refer Table 1 for variable definitions. See notes to Table 2 for explanation of column titles. All specifications include treatment group-year fixed effects and a treatment dummy; the figures in parenthesis are p-values.

Table 6. Difference-in-Differences Estimation - LBD Sample

	Employment-Matched							Propensity-Matched						
					Relative to SR_PRE		**PE Trend Test**					**Relative to SR_PRE**		**PE Trend Test**
	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST- SR_PRE	SR_PRE- LR_PRE	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST- SR_PRE	SR_PRE- LR_PRE
Employment	0.116	-0.001	-0.293	-0.431	-0.292	-0.138	-0.117	0.028	0.005	-0.226	-0.361	-0.231	-0.366	-0.023
	(0.000)	(0.960)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.610)	(0.912)	(0.000)	(0.000)	(0.000)	(0.000)	(0.581)
Wage Rate	-0.024	0.013	-0.016	0.045	-0.029	0.061	0.037	0.034	0.004	-0.025	0.062	-0.029	0.058	-0.03
	(0.165)	(0.352)	(0.368)	(0.085)	(0.012)	(0.001)	(0.017)	(0.131)	(0.857)	(0.263)	(0.031)	(0.137)	(0.701)	(0.072)
Payroll	0.117	0.013	-0.315	-0.366	-0.328	-0.051	-0.104	0.076	0.023	-0.237	-0.289	-0.26	-0.312	-0.053
	(0.000)	(0.447)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.174)	(0.653)	(0.000)	(0.000)	(0.000)	(0.000)	(0.201)

Notes: The number of observations for the employment-matched sample regressions (first seven columns in each row) is 37,207, and for the propensity-matched sample regressions (last seven columns in each row) is 33,393. Refer Table 1 for variable definitions. See notes to Table 2 for explanation of column titles. All specifications include firm fixed effects and event-year effects; the figures in parenthesis are p-values.

Table 7. Difference-in-Differences Estimation - Multi-Unit Firms Only

	Employment-Matched							**Propensity-Matched**						
					Relative to SR_PRE		**PE Trend Test**					**Relative to SR_PRE**		**PE Trend Test**
	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST- SR_PRE	SR_PRE- LR_PRE	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST- SR_PRE	SR_PRE- LR_PRE
Size Measures														
Output	0.032	0.024	0.171	-0.292	0.147	-0.316	-0.008	-0.032	0.006	-0.119	-0.247	-0.125	-0.253	0.038
	(0.407)	(0.368)	(0.000)	(0.000)	(0.000)	(0.000)	(0.768)	(0.430)	(0.826)	(0.000)	(0.000)	(0.000)	(0.000)	(0.229)
Value Added	0.066	0.054	-0.216	-0.332	-0.27	-0.386	-0.012	0.035	0.056	-0.143	-0.288	-0.199	-0.344	0.021
	(0.134)	(0.089)	(0.000)	(0.000)	(0.000)	(0.000)	(0.716)	(0.459)	(0.091)	(0.000)	(0.000)	(0.000)	(0.000)	(0.540)
Employment	0.044	0.017	-0.195	-0.335	-0.212	-0.352	-0.027	-0.017	0.003	-0.184	-0.362	-0.187	-0.365	0.02
	(0.238)	(0.459)	(0.000)	(0.000)	(0.000)	(0.000)	(0.332)	(0.660)	(0.904)	(0.000)	(0.000)	(0.000)	(0.000)	(0.506)
Capital	-0.017	-0.026	-0.12	-0.258	-0.094	-0.232	-0.009	0.005	-0.026	-0.056	-0.174	-0.03	-0.148	-0.031
	(0.704)	(0.347)	(0.000)	(0.000)	(0.003)	(0.000)	(0.778)	(0.920)	(0.373)	(0.072)	(0.004)	(0.367)	(0.018)	(0.398)
Wage Measures														
Wage Rate	-0.003	-0.004	-0.001	0.001	0.003	0.005	-0.001	-0.011	-0.009	0.003	0.028	0.012	0.037	0.002
	(0.795)	(0.689)	(0.889)	(0.920)	(0.826)	(0.701)	(0.922)	(0.363)	(0.332)	(0.764)	(0.070)	(0.256)	(0.014)	(0.783)
NPW Wage Rate	-0.013	0.008	-0.024	-0.022	-0.032	-0.03	0.021	0.014	-0.013	-0.002	0.045	0.011	0.058	-0.027
	(0.562)	(0.660)	(0.201)	(0.358)	(0.068)	(0.178)	(0.225)	(0.582)	(0.497)	(0.912)	(0.136)	(0.614)	(0.039)	(0.161)
PW Wage Rate	0.006	-0.011	-0.004	0.004	0.007	0.015	-0.017	-0.028	-0.016	-0.003	0.001	0.013	0.017	0.012
	(0.646)	(0.294)	(0.749)	(0.779)	(0.492)	(0.280)	(0.094)	(0.055)	(0.142)	(0.810)	(0.952)	(0.252)	(0.286)	(0.332)
Factor Intensity														
Capital Intensity	-0.06	-0.043	0.075	0.077	0.118	0.12	0.017	0.021	-0.029	0.127	0.188	0.156	0.217	-0.05
	(0.064)	(0.048)	(0.002)	(0.060)	(0.000)	(0.004)	(0.489)	(0.562)	(0.230)	(0.000)	(0.000)	(0.000)	(0.000)	(0.073)
NPW Emp Share	-0.004	-0.005	0.018	0.02	0.023	0.025	-0.001	-0.009	-0.001	0.013	0.015	0.014	0.016	0.008
	(0.478)	(0.317)	(0.000)	(0.012)	(0.000)	(0.002)	(0.926)	(0.159)	(0.897)	(0.020)	(0.091)	(0.028)	(0.089)	(0.095)
NPW Wage Share	-0.009	-0.003	0.014	0.013	0.017	0.016	0.006	-0.003	-0.001	0.015	0.025	0.016	0.026	0.002
	(0.187)	(0.589)	(0.009)	(0.131)	(0.006)	(0.078)	(0.255)	(0.726)	(0.897)	(0.017)	(0.010)	(0.026)	(0.010)	(0.735)
Productivity														
Output per Worker	0.021	0.037	-0.019	0.003	-0.056	-0.034	0.016	0.051	0.053	0.04	0.073	-0.013	0.02	0.002
	(0.447)	(0.121)	(0.459)	(0.928)	(0.029)	(0.349)	(0.450)	(0.095)	(0.050)	(0.159)	(0.084)	(0.646)	(0.612)	(0.933)
VA per Worker	-0.013	0.006	0.026	0.044	0.02	0.038	0.019	-0.015	0.003	0.064	0.114	0.061	0.111	0.018
	(0.549)	(0.704)	(0.136)	(0.099)	(0.288)	(0.162)	(0.218)	(0.484)	(0.881)	(0.000)	(0.000)	(0.001)	(0.000)	(0.287)
TFP- Levpet	0.01	0.053	-0.056	-0.044	-0.109	-0.097	0.043	-0.007	0.03	-0.043	0.016	-0.073	-0.014	0.037
	(0.757)	(0.046)	(0.054)	(0.298)	(0.000)	(0.023)	(0.067)	(0.841)	(0.317)	(0.204)	(0.734)	(0.024)	(0.752)	(0.153)
TFP- OLS	0.032	0.053	-0.021	-0.015	-0.074	-0.068	0.021	-0.016	0.034	0.025	0.081	-0.009	0.047	0.05
	(0.317)	(0.048)	(0.459)	(0.734)	(0.013)	(0.120)	(0.361)	(0.667)	(0.250)	(0.447)	(0.097)	(0.766)	(0.305)	(0.054)

Notes: The number of observations for the employment-matched sample regressions (first seven columns in each row) is 19,245, and for the propensity-matched sample regressions (last seven columns in each row) is 16,066. Refer Table 1 for variable definitions. See notes to Table 2 for explanation of column titles. All specifications include firm fixed effects and event-year effects; the figures in parenthesis are p-values.

Table 8. Difference-in-Differences Estimation - Pseudo-Firms

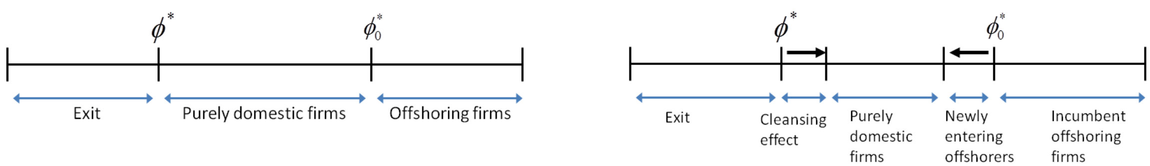
	Employment-Matched							**Propensity-Matched**						
					Relative to SR_PRE		**PE Trend Test**					**Relative to SR_PRE**		**PE Trend Test**
	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST- SR_PRE	SR_PRE- LR_PRE	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST- SR_PRE	SR_PRE- LR_PRE
Size Measures														
Output	0.12	0.068	-0.185	-0.262	-0.253	-0.33	-0.052	0.02	0.035	-0.132	-0.208	-0.167	-0.243	0.015
	(0.006)	(0.032)	(0.000)	(0.000)	(0.000)	(0.000)	(0.072)	(0.631)	(0.222)	(0.000)	(0.000)	(0.000)	(0.000)	(0.624)
Value Added	0.148	0.075	-0.211	-0.298	-0.286	-0.373	-0.073	0.072	0.088	-0.151	-0.229	-0.239	-0.317	0.016
	(0.001)	(0.024)	(0.000)	(0.000)	(0.000)	(0.000)	(0.027)	(0.134)	(0.015)	(0.000)	(0.000)	(0.000)	(0.000)	(0.633)
Employment	0.139	0.062	-0.178	-0.271	-0.24	-0.333	-0.077	0.024	0.028	-0.175	-0.313	-0.203	-0.341	0.004
	(0.001)	(0.036)	(0.000)	(0.000)	(0.000)	(0.000)	(0.008)	(0.535)	(0.294)	(0.000)	(0.000)	(0.000)	(0.000)	(0.897)
Capital	0.084	0.031	-0.147	-0.188	-0.178	-0.219	-0.053	0.07	0.005	-0.104	-0.181	-0.109	-0.186	-0.065
	(0.080)	(0.337)	(0.000)	(0.000)	(0.000)	(0.000)	(0.128)	(0.134)	(0.889)	(0.002)	(0.001)	(0.002)	(0.002)	(0.062)
Wage Measures														
Wage Rate	-0.022	-0.018	-0.007	-0.009	0.011	0.009	0.004	0.016	-0.016	0.002	0.005	0.018	0.021	-0.032
	(0.077)	(0.066)	(0.522)	(0.535)	(0.304)	(0.504)	(0.682)	(0.246)	(0.112)	(0.873)	(0.741)	(0.099)	(0.462)	(0.966)
NPW Wage Rate	-0.028	-0.026	-0.017	-0.023	0.009	0.003	0.002	-0.007	-0.026	-0.001	0.012	0.025	0.038	-0.019
	(0.208)	(0.134)	(0.373)	(0.358)	(0.628)	(0.896)	(0.923)	(0.803)	(0.204)	(0.952)	(0.682)	(0.235)	(0.189)	(0.314)
PW Wage Rate	-0.006	-0.014	-0.004	0.004	0.01	0.018	-0.008	-0.018	-0.012	-0.004	-0.02	0.008	-0.008	0.006
	(0.674)	(0.230)	(0.741)	(0.818)	(0.379)	(0.240)	(0.464)	(0.254)	(0.332)	(0.779)	(0.267)	(0.479)	(0.649)	(0.637)
Factor Intensity														
Capital Intensity	-0.038	-0.022	0.034	0.101	0.056	0.123	0.016	0.046	-0.023	0.071	0.132	0.094	0.155	-0.069
	(0.289)	(0.368)	(0.973)	(0.022)	(0.064)	(0.004)	(0.541)	(0.201)	(0.347)	(0.012)	(0.003)	(0.001)	(0.001)	(0.010)
NPW Emp Share	-0.004	-0.003	0.012	0.015	0.015	0.018	0.001	-0.004	-0.005	0.002	0.007	0.007	0.012	-0.001
	(0.569)	(0.610)	(0.040)	(0.052)	(0.012)	(0.021)	(0.797)	(0.569)	(0.379)	(0.757)	(0.412)	(0.256)	(0.177)	(0.859)
NPW Wage Share	-0.011	-0.006	0.01	0.007	0.016	0.013	0.005	-0.006	-0.009	0.002	0.014	0.011	0.023	-0.003
	(0.159)	(0.298)	(0.134)	(0.412)	(0.021)	(0.126)	(0.398)	(0.459)	(0.107)	(0.719)	(0.156)	(0.072)	(0.017)	(0.508)
Productivity														
Output per Worker	0.023	0.027	-0.024	-0.021	-0.051	-0.048	0.004	0.05	0.061	0.024	0.084	-0.037	0.023	0.011
	(0.447)	(0.242)	(0.358)	(0.569)	(0.061)	(0.175)	(0.848)	(0.142)	(0.028)	(0.424)	(0.043)	(0.187)	(0.563)	(0.607)
VA per Worker	-0.019	0.004	-0.006	0.005	-0.01	0.001	0.023	-0.002	0.008	0.043	0.105	0.035	0.097	0.01
	(0.430)	(0.818)	(0.779)	(0.841)	(0.657)	(0.957)	(0.190)	(0.920)	(0.667)	(0.022)	(0.000)	(0.086)	(0.001)	(0.551)
TFP- Levpet	0.028	0.033	-0.012	-0.014	-0.045	-0.047	0.005	-0.012	0.024	0.005	0.061	-0.019	0.037	0.036
	(0.447)	(0.238)	(0.674)	(0.741)	(0.167)	(0.290)	(0.843)	(0.749)	(0.412)	(0.873)	(0.204)	(0.555)	(0.436)	(0.151)
TFP- OLS	0.041	0.044	0.01	0.03	-0.034	-0.014	0.003	-0.03	0.033	0.031	0.072	-0.002	0.039	0.063
	(0.242)	(0.116)	(0.726)	(0.478)	(0.275)	(0.753)	(0.890)	(0.418)	(0.276)	(0.332)	(0.129)	(0.948)	(0.393)	(0.009)

Notes: The number of observations for the employment-matched sample regressions (first seven columns in each row) is 18,431, and for the propensity-matched sample regressions (last seven columns in each row) is 15,882. Refer Table 1 for variable definitions. See notes to Table 2 for explanation of column titles. All specifications include firm fixed effects and event-year effects; the figures in parenthesis are p-values.

Table 9. Difference-in-Differences Estimation - Vertically Related Firms Only

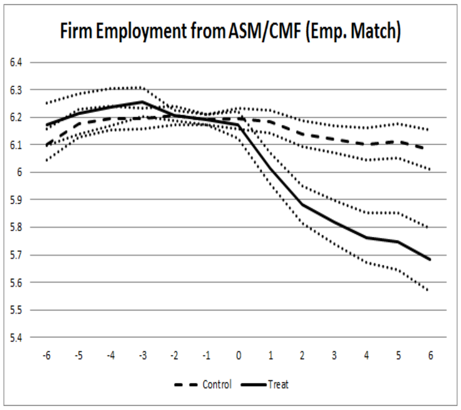
	Employment-Matched							Propensity-Matched						
					Relative to SR_PRE		PE Trend Test					Relative to SR_PRE		PE Trend Test
	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST- SR_PRE	SR_PRE- LR_PRE	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST- SR_PRE	SR_PRE- LR_PRE
Size Measures														
Output	-0.07	-0.02	-0.188	-0.302	-0.168	-0.282	0.05	-0.104	-0.004	-0.071	-0.22	-0.067	-0.216	0.1
	(0.337)	(0.667)	(0.000)	(0.000)	(0.001)	(0.001)	(0.336)	(0.134)	(0.928)	(0.119)	(0.003)	(0.143)	(0.005)	(0.064)
Value Added	-0.07	-0.012	-0.207	-0.344	-0.195	-0.332	0.058	-0.072	0.034	-0.078	-0.314	-0.112	-0.348	0.106
	(0.374)	(0.818)	(0.000)	(0.000)	(0.000)	(0.000)	(0.295)	(0.327)	(0.490)	(0.136)	(0.000)	(0.040)	(0.000)	(0.058)
Employment	-0.01	0.014	-0.156	-0.295	-0.17	-0.309	0.024	-0.085	0.004	-0.084	-0.28	-0.088	-0.284	0.089
	(0.889)	(0.757)	(0.000)	(0.000)	(0.001)	(0.000)	(0.632)	(0.190)	(0.928)	(0.063)	(0.000)	(0.053)	(0.000)	(0.066)
Capital	-0.09	-0.024	-0.163	-0.252	-0.139	-0.228	0.066	-0.035	0.031	-0.021	-0.128	-0.052	-0.159	0.066
	(0.271)	(0.638)	(0.000)	(0.001)	(0.019)	(0.010)	(0.303)	(0.697)	(0.535)	(0.660)	(0.162)	(0.353)	(0.101)	(0.339)
Wage Measures														
Wage Rate	-0.004	-0.007	0.003	0.028	0.01	0.035	-0.003	0.008	0.01	0.014	0.045	0.004	0.035	0.002
	(0.834)	(0.603)	(0.834)	(0.139)	(0.474)	(0.044)	(0.769)	(0.675)	(0.509)	(0.363)	(0.027)	(0.791)	(0.084)	(0.935)
NPW Wage Rate	0.012	0.021	0.018	0.036	-0.003	0.015	0.009	0.023	-0.008	0.02	0.069	0.028	0.077	-0.031
	(0.682)	(0.384)	(0.407)	(0.246)	(0.904)	(0.634)	(0.707)	(0.555)	(0.779)	(0.509)	(0.093)	(0.377)	(0.048)	(0.326)
PW Wage Rate	-0.002	-0.018	-0.01	0.009	0.008	0.027	-0.016	-0.019	0.005	0.009	0.024	0.004	0.019	0.024
	(0.904)	(0.254)	(0.535)	(0.719)	(0.609)	(0.200)	(0.506)	(0.478)	(0.772)	(0.617)	(0.332)	(0.823)	(0.432)	(0.300)
Factor Intensity														
Capital Intensity	-0.081	-0.038	-0.007	0.043	0.031	0.081	0.043	0.05	0.027	0.062	0.152	0.035	0.125	-0.023
	(0.124)	(0.289)	(0.849)	(1.559)	(0.466)	(0.179)	(0.322)	(0.441)	(0.472)	(0.136)	(0.026)	(0.454)	(0.089)	(0.658)
NPW Emp Share	-0.005	-0.006	0.006	0.023	0.012	0.029	-0.001	-0.001	0.002	0.004	0.015	0.002	0.013	0.003
	(0.624)	(0.441)	(0.390)	(0.033)	(0.160)	(0.013)	(0.833)	(0.952)	(0.834)	(0.617)	(0.230)	(0.840)	(0.329)	(0.766)
NPW Wage Share	-0.008	-0.002	0.01	0.028	0.012	0.03	0.006	0.002	0.00	0.007	0.024	0.007	0.024	-0.00199
	(0.522)	(0.787)	(0.250)	(0.024)	(0.203)	(0.018)	(0.574)	(0.897)	(0.992)	(0.441)	(0.084)	(0.536)	(0.114)	(0.866)
Productivity														
Output per Worker	-0.06	-0.026	-0.049	-0.049	-0.023	-0.023	0.034	0.014	0.031	0.004	-0.034	-0.027	-0.065	0.017
	(0.177)	(0.509)	(0.254)	(0.441)	(0.537)	(0.678)	(0.286)	(0.757)	(0.453)	(0.920)	(0.576)	(0.503)	(0.244)	(0.592)
VA per Worker	-0.06	-0.034	-0.03	-0.007	0.004	0.027	0.026	-0.019	-0.008	0.012	0.059	0.02	0.067	0.011
	(0.107)	(0.246)	(0.303)	(0.873)	(0.885)	(0.519)	(0.291)	(0.603)	(0.795)	(0.682)	(0.165)	(0.484)	(0.097)	(0.683)
TFP- Levpet	-0.02	0.00	0.08	0.083	0.08	0.083	0.02	0.024	0.014	0.019	-0.038	0.005	-0.052	-0.01
	(0.682)	(0.992)	(0.070)	(0.165)	(0.094)	(0.172)	(0.581)	(0.667)	(0.749)	(0.675)	(0.582)	(0.918)	(0.727)	(0.790)
TFP- OLS	0.01	0.012	-0.053	-0.069	-0.065	-0.081	0.002	-0.039	0.015	0.01	-0.039	-0.005	-0.054	0.054
	(0.849)	(0.779)	(0.250)	(0.342)	(0.197)	(0.286)	(0.958)	(0.497)	(0.734)	(0.841)	(0.562)	(0.912)	(0.411)	(0.191)

Notes: The number of observations for the employment-matched sample regressions (first seven columns in each row) is 5,935, and for the propensity-matched sample regressions (last seven columns in each row) is 5,546. Refer Table 1 for variable definitions. See notes to Table 2 for explanation of column titles. All specifications include firm fixed effects and event-year effects; the figures in parenthesis are p-values.

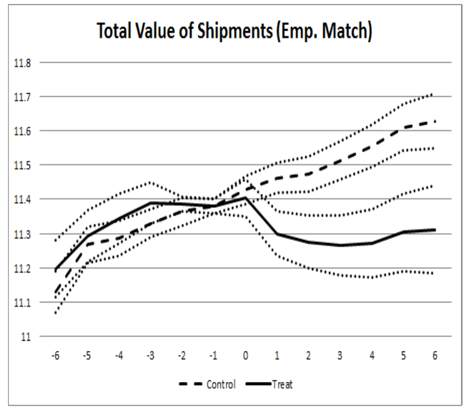


(a) Initial Benchmark Equilibrium (b) A Fall in Offshoring Cost

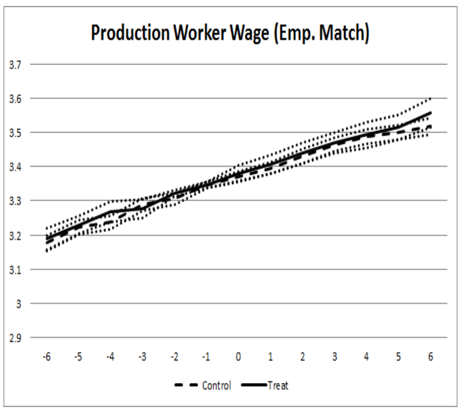
Figure 1. Cut-off Productivities in Equilibria



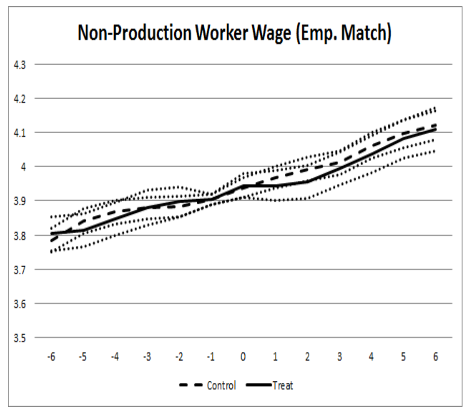
(a) Employment



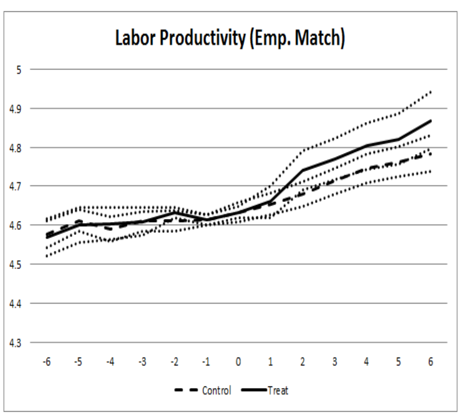
(b) Output



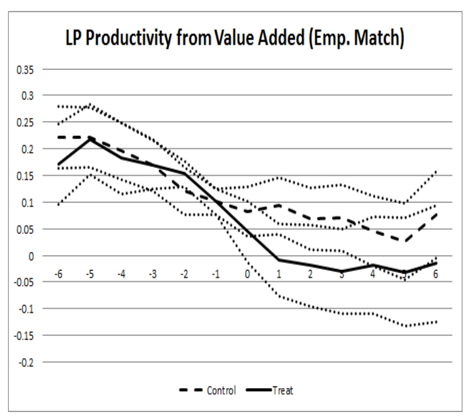
(c) Production-Worker Wage



(d) Non-production Worker Wage

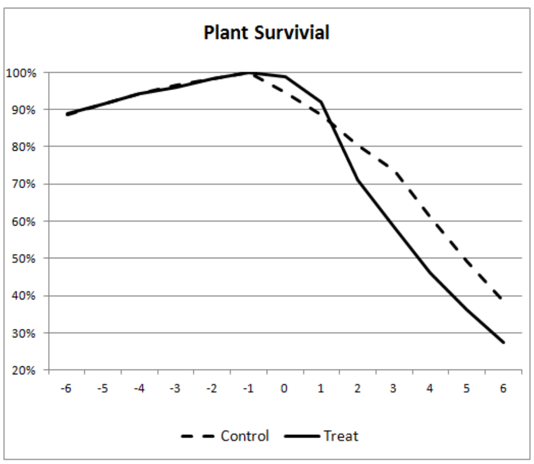


(e) Output per Worker

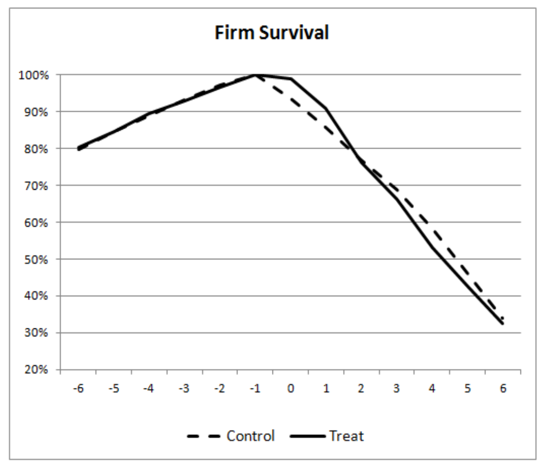


(f) TFP Levpet

Figure 2. Employment-Matched Difference-in-differences Estimation Results

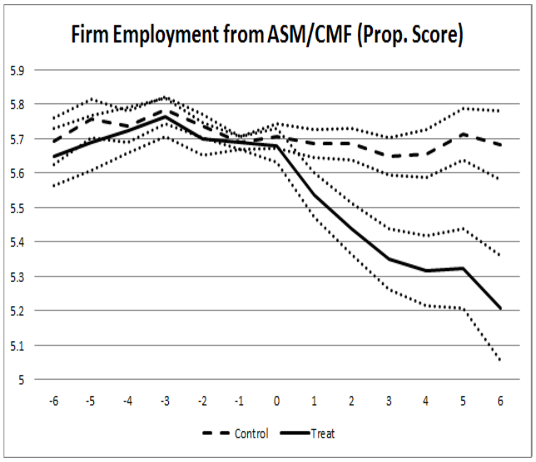


(a) Plants

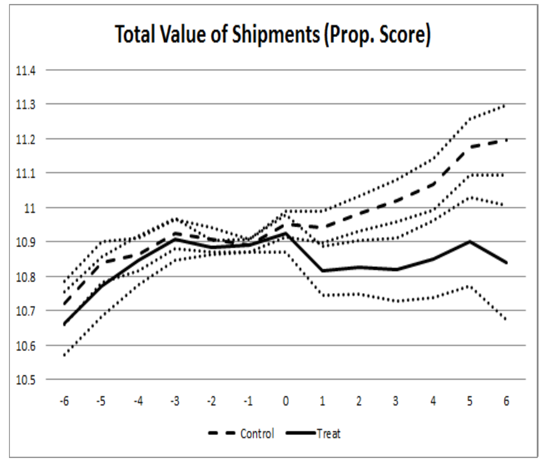


(b) Firms

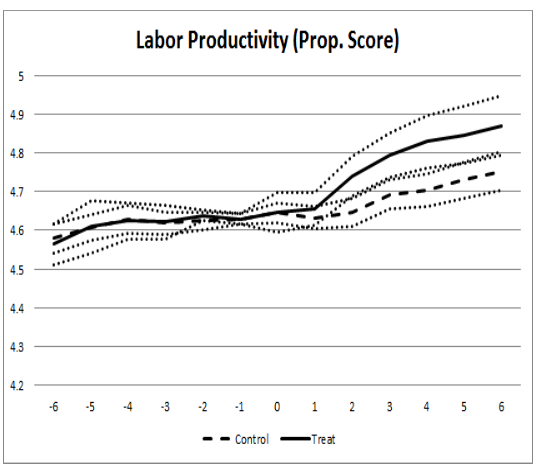
Figure 3. Survival Analysis



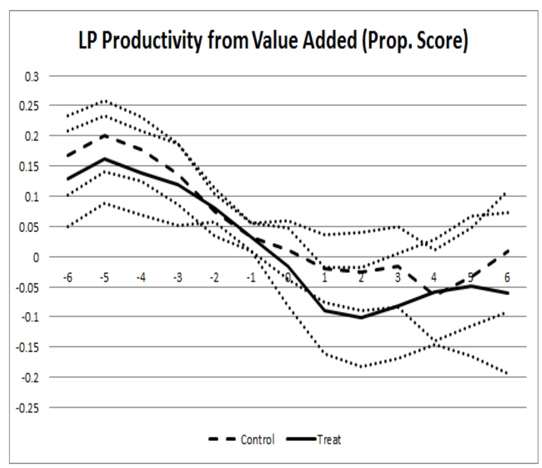
(a) Employment



(b) Output

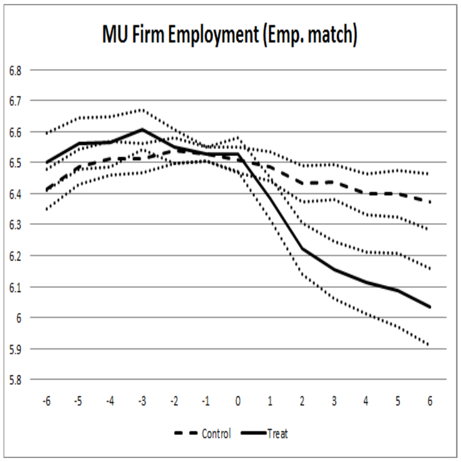


(c) Labor Productivity

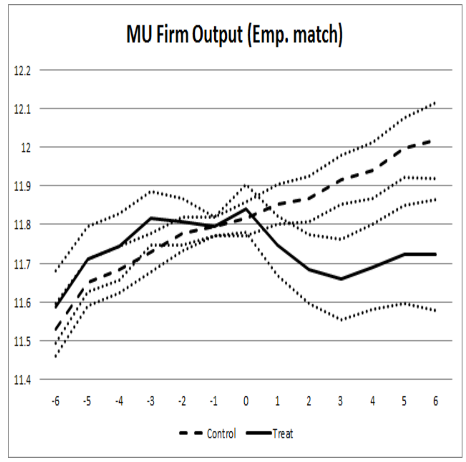


(d) TFP Levpet

Figure 4. Propensity Score Matched Difference-in-differences Estimation Results

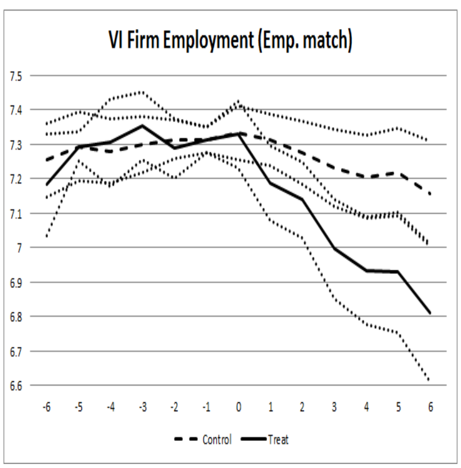


(a) Employment Results

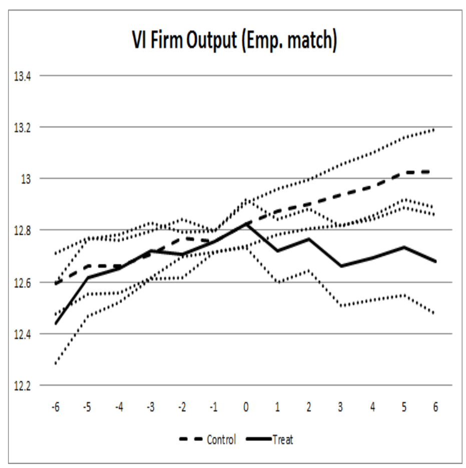


(b) Output Results

Figure 5. Employment-Matched DID Estimation Results: Multi-Unit Firms Only



(a) Employment Results



(b) Output Results

Figure 6. Employment-Matched DID Estimation Results: Vertically Linked Firms Only

Table A1. Results of Aggressive Matching Procedure of TAA to SSEL

Impact Year	Total # of Petitions	# Certified	# Offshored	# Matched	Matching Rate (%)	Among Matched Petitions: Offshoring	Import Competition	Denied
1999	998	328	200	803	80.46	153	118	532
2000	2,593	1,489	833	2,267	87.43	702	658	907
2001	3,329	1,094	794	2,090	62.78	810	275	1,005
2002	3,825	1,757	1,211	2,585	67.58	990	476	1,119
2003	2,505	1,266	887	1,718	68.58	733	271	714
2004	2,545	1,320	876	1,614	63.42	620	320	674
2005-6	3,808	1,853	1603	2,568	67.44	1,159	217	1,192
Total	19,603	9,107	6,404	13,645	69.61	5,167	2,335	6,143

Table A2. Counts of Offshoring Events Matched to LBD

Impact Year	Total	# Offshoring	# Import Competition	# Denied
1999	503	96	82	325
2000	1,396	423	404	569
2001	1,269	490	162	617
2002	1,946	784	381	781
2003	1,125	492	202	431
2004	1,009	383	233	393
2005-6	1,606	732	154	719
All	8,853	3,400	1,618	3,835