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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 confidential data on U.S. firm operations. We exploit these data to assess how offshoring affects domestic firm-level aggregate employment, output, wages and productivity. Consistent with heterogenous firm models where offshoring involves a fixed cost, we find that the average offshoring firm is larger and more productive than the average non-offshorer. After initiating offshoring, firms experience large declines in employment (46.2 per cent), output (38.5 per cent) and capital (28.8 per cent) relative to their industry peers. We find no significant change in average wages or in total factor productivity measures for offshoring firms. These results are consistent across two separate difference-in-differences (DID) approaches, an instrumental variables approach, and a number of robustness checks. Thus, we find offshoring to be a strong substitute for domestic activity in this large sample of offshoring events.

Keywords: Outsourcing, manufacturing, employment, trade, productivity, firm performance

JEL classifications: F16, F61, F66, F14, F23

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 2013, Pierce and Schott 2013, Harrison and McMillan 2011).[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 hampered by the lack of good quality data on offshoring (Kirkegaard, 2007).

In this paper, we assemble a new dataset of offshoring events and firm performance by linking offshoring-induced employment layoff events available from the Trade Adjustment Assistance (TAA) program to U.S. Census Bureau panel microdata. The TAA program helps workers who lose jobs for trade-related reasons, mainly in the form of financial assistance for training, and looking for and relocating to a new job. By connecting the identity of firms with offshoring-related layoffs (certified by the Department of Labor under the TAA program) to Census data on underlying firm operations, we can directly study the effects of offshoring on these firms. Importantly for our empirical evaluation, participation in the program itself is not limited to failing or struggling firms: indeed, firms positively identified as offshorers in this linked dataset are found to be larger and more capital intensive than non-offshoring firms, and have survival rates very similar to the overall population of U.S. firms. We then use the linked dataset to evaluate the effects of offshoring on the domestic activities of offshoring firms.

While media discourse about offshoring focuses largely on immediate job destruction at affected plants, theoretical and empirical work present a more mixed picture. Theoretical predictions about the effects of offshoring vary across models. When the offshored activity has vertical linkages to the remaining domestic activities, there is potential for complementarities between offshoring and domestic activity (Harrison and McMillan 2011, Desai, Foley and Hines 2009, Sethupathy 2013). For example, in an extension of Grossman and Rossi-Hansberg's (2008) model of offshoring,

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[1]Absolute employment levels in manufacturing have sharply declined over the last decade. Per Bureau of Labor Statistics figures (data.bls.gov), manufacturing employment remained relatively stable around 17 million from 1990 until 2000, declined sharply to about 14 million by 2004, then fell further to about 12 million in 2012.

Sethupathy (2013) 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 rise at offshoring firms (and fall at non-offshoring firms who lose market share). Measured productivity at the domestic firm level also goes up as a result of lower costs for offshored tasks. Further, restructuring through offshoring helps firms avoid failure relative to non-offshorers (Park 2014).[2]

However, 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 would be unaffected. Thus the extent to which offshoring affects firm-level employment and other outcomes is an interesting empirical question.

The TAA program administered by the U.S. Department of Labor (USDOL) is intended to help find reemployment for workers who lose jobs specifically because of trade-related reasons. Workers who receive certification under the program may receive help looking for and relocating to a new job, as well as receive training for a wholly new occupation. When layoffs occur, different concerned parties can file a petition with the USDOL, which are then investigated by the USDOL to verify that layoffs were indeed trade-related. Approved petitions are classified into four categories based the reason for layoff, two of which are directly related to offshoring activity.[3]

We use name-matching algorithms supplemented by extensive manual checks and modifications to link establishments in the TAA petition data to the U.S. Census Bureau's business register (details are provided in the Data Appendix). After cleaning and linking the two data sets, we are left with extensive information on domestic activity of about 1,000 unique offshoring firms. We use

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[2]Park (2014) analyzes the employment effect of offshoring in a heterogeneous firm framework calibrated to U.S. manufacturing sector, and finds the bulk of industry-level negative effects stem from the "cleansing effect" - job destruction from the downsizing or death of non-offshoring firms that lose price competitiveness against their offshoring rivals. Our focus in this paper is not on the aggregate effects of offshoring, but rather on domestic outcomes for offshorers.

[3]In our data 50% of petitions were filed by the company, 42% by the union, and the remaining 8% by state workforce offices. The rejection rate is non-trivial – in our sample about 45% of the petitions were rejected. More details about the TAA program and the petition data are provided in Section 4.

this data to understand offshorers and examine the effects of offshoring on a range of outcomes at the (domestic) aggregate firm level.

First, we examine the basic characteristics of this sample of offshoring firms relative to the overall population. Consistent with models where offshoring involves a fixed cost (e.g. Sethupathy 2013), we find that prior to initiation of offshoring, offshorers are larger, more capital intensive, and more productive than non-offshorers. Interestingly, offshoring firms are *not* more skill intensive than non-offshorers in the same industry.[4] These empirical regularities demonstrate that our sample of offshorers is not limited to firms that are contracting prior to offshoring, a key result underpinning our findings about post-offshoring performance described below.

Next, we examine the effects of offshoring. An important concern for this analysis is the potential endogeneity of the offshoring decision, whereby offshoring is triggered by factors that also directly affect firm level outcomes. We attempt to address this concern in a number of ways. First, because the key drivers of the offshoring decision are likely to be industry shocks (e.g., an increase in domestic input costs, or increase in competition from imports), for each offshoring firm, we select two "controls" closest in size from within the same 3-digit industry, and form cells consisting of the offshorer and matched peers. We then estimate difference-in-differences (DID) effects of offshoring by comparing offshorers to the matched controls, which allows for the possibility that the effect of industry shocks may vary by firm size.

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 the event. 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%. Importantly, this pattern of employment reduction is very similar if we restrict the sample to only non-offshoring plants within offshoring firms, 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

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[4]Because large firms are typically more skill-intensive than smaller firms, offshorers appear to have lower skill-intensity relative to similar sized peers. This is consistent with economic theory, as we may expect low skill activities to be precisely the ones to be offshored (e.g., Krugman 2008). But this is a noteworthy contrast to the stylized facts for exporters, who are both larger as well as more skill-intensive than non-exporters (e.g., Bernard and Jensen 1999).

of non-affected plants within offshoring firms.

We find no discernible change in wages for either production workers or non-production workers and small gains in labor productivity (measured as real output per worker or real value added per worker). These gains in labor productivity appear to be from 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 find that the survival rate of firms who offshore is very similar to that for the control group firms.

One potential source of bias in such DID analysis is the presence of pre-existing trends. We check for this in two ways. First, we plot the trends for both the treatment and control groups for a 13-year window around the offshoring event (see e.g. Figure 2) as suggested by Angrist and Pischke (2009, who cite Autor 2003). These figures show that: (a) the trends for the offshorers and control group of industry-employment matched firms are very similar prior to the offshoring event. For the variables where we find a stark decline (output and employment), the figures show that: (b) the offshoring firms do not show a significant declining trend prior to offshoring; and (c) there is a stark break in trend for offshorers relative to non-offshorers, consistent with changes being triggered by offshoring. In other words, the data suggest that offshorers in the sample do not have significantly different employment patterns from non-TAA participants until after the date when offshoring impacted the firm. Second, in the regression analysis, we test for pre-existing trends, and we confirm that the post-offshoring decline for employment, output and capital very significantly exceed the magnitude of pre-existing trend effects (if any).

While our baseline DID analysis controls for endogeneity from omitted industry-size variables, there could be concerns about differential trends based on other (non-size) initial characteristics. For this reason, our second approach is to adopt a propensity score matching approach, which matches firms with a similar probability of offshoring based on more variables (Rosenbaum and Rubin, 1984). In addition to employment, we include capital intensity as well as production and non-production wages in the propensity model. We then redo our analysis using controls matched on the propensity score, and find results similar to the baseline analysis.

As a third alternative to addressing endogeneity concerns, we check the robustness of the

sharp decline in employment, output, and capital to using an alternative instrumental variables (IV) approach. We draw on Pierce and Schott (2013), who find evidence that the decline in employment in manufacturing was stronger in those industries for which the threat of tariff hikes with China declined the most, following conferral of Permanent Normal Trade Relations (PNTR) on China. Specifically, they find "circumstantial evidence that these changes in employment are driven in part by offshoring." The idea behind our IV approach is that the awarding of PNTR status reduces expected costs of offshoring (as expected future tariffs form part of expected transport costs) and reduces uncertainty (which encourages sunk investments required to initiate offshoring). Because other industry-level shocks need to be controlled for, this variation alone does not provide a usable instrument. To generate within industry variation, in our primary IV specification, we use lagged employment levels interacted with the reduction in potential tariffs as an instrument for the offshoring decision. This relies on the idea that, in any model with a fixed cost of offshoring (e.g., Sethupathy 2013), reductions in offshoring costs are more likely to affect larger firms, as the smallest firms are not close to the margin for making the switch to offshoring. Our first stage results suggest that the instruments are sufficiently strong, and the IV results confirm the baseline conclusions of strong declines in all the size measures. The IV estimates show greater reductions for employment and capital, and smaller reductions for output and value added.

We undertake a number of additional checks of our results. First, we address the possibility that potential benefits from offshoring are transmitted mainly to non-manufacturing activities of the firms by using data from the Longitudinal Business Database, which includes employment and payroll information on all establishments in all sectors. Consistent with the baseline analysis, we find significant declines in firm-level employment, and no change in average wage. Thus, we find no evidence for significant gains in non-manufacturing establishments within offshorers. Second, we check robustness of the baseline and propensity-matched DID results to using more flexible cell-year fixed effects (which allows for industry-size and industry-propensity score specific shocks). We also undertake a series of other checks described in detail in Section 7.4

Thus, in our sample, offshoring was a strong substitute for domestic activity, with remaining domestic output, employment, and capital showing significant declines. Our results appear con-

sistent with shifting of entire product lines abroad, with offshored activity lacking strong vertical linkages with remaining home activities.[5]

Our paper contributes to the empirical literature that studies whether offshoring is a complement or substitute for domestic employment. Our finding of a stark negative impact on domestic firm output, employment and capital stand in contrast to a number of studies in this literature (which are reviewed in more detail in Section 2 below). The major novelty in our paper is the use of the linked dataset we construct, which allows us to examine events that are verified (by the U.S. Department of Labor) to be related to offshoring.[6]

The rest of the paper is organized as the following. Section 2 describes the related literature in the context of alternative approaches to measuring offshoring. Section 3 presents a model of offshoring drawn from Sethupathy (2013)'s extension of the Grossman and Rossi-Hansberg's (2008) work, and briefly discusses the case of horizontal FDI. Section 4 describes the data, as well as the TAA program, in more detail. Section 5 describes the empirical methodology used to evaluate the effects of offshoring. Section 6 presents our baseline results, and 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 industry-level share of imported inputs as a proxy for offshoring activity. At the industry level, this entails using input-output tables to identify offshoring industries. Amiti and Wei (2009) find that the impact is insignificant at the disaggregated level, but positive at a more aggregated level in the

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[5]To check for complementarity, we examined a sub-sample where the activity at the offshored plant was a significant supplier to activities in the remaining plants, per the Input-Output tables (following the approach in Atalay, Hortascu and Syverson, 2014). However, we find no significant difference in results for this sub-sample. We interpret this as suggesting that, as documented by Atalay et al (using Commodity Flow Survey data for the US) and by Ramondo, Rappoport and Ruhl (2014) (using MNC survey data from the Bureau of Economic Analysis (BEA)), actual input flows may be occurring only rarely within firms, even when plants appear vertically related per the Input-Output tables.

[6]One prominent related paper that specifically examines employment effects of offshoring is Harrison and McMillan (2011). Using MNC survey data from the BEA, they find that on average, offshoring (particularly to low-income countries) substitutes for domestic employment, which is broadly consistent with our findings here. But their estimated overall negative effect of offshoring is small, and they find that offshoring involving tasks that are different from those undertaken domestically are a complement for domestic activity.

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 on employment (but this was offset by gains in exporting activities, so that overall effect of trade integration was positive).

Such a measure can also be constructed for firm-level data, when information on firm-level imports is available. 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 early studies (Berman, Bound and Griliches 1994; Feenstra and Hanson 1996 & 1999; Kurz, 2006). Unfortunately, the Census stopped asking this question after 1992.[7] Similar studies have used micro data of other countries: e.g., Hummels et al. (2014) 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.

An important limitation of using imported input usage as a measure of offshoring is that imported inputs could be related to newly introduced products rather than replacement of in-house inputs (Feenstra and Markusen 1994). These new inputs would not involve shifting of in-house production, and hence may not capture offshoring as traditionally defined. Further, 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 individual, independently-certified, offshoring events, avoiding these sources of potential measurement error.

A second source used to identify offshoring is survey data on the foreign operations of the U.S. multinationals, collected by the U.S. Bureau of Economic Analysis (BEA). This dataset has detailed

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[7]A sub-sample of establishments were asked this question in the 2007 Census, and used in work by Fort (2014) who investigates the determinants of production fragmentation.

operational information at the establishment level, including employment, wages, and location.[8]
Brainard and Riker (2001) find little substitution between U.S. facilities and foreign affiliates, and
larger substitution among foreign affiliates in low wage countries. Borga (2005) finds an insignificant effect as well. Stronger substitution between home and foreign affiliate employment is found
by Hanson, Mataloni, and Slaughter (2005). Harrison and McMillan (2011) find that while overall
offshoring substitutes for domestic employment, at a disaggregated level, the effects are nuanced.
For firms that do significantly different tasks at home and abroad, foreign and domestic employment are complements, whereas for firms that do similar tasks, foreign and domestic employment
are substitutes. Using industry and occupation aggregates of data on foreign affiliate employment
(from the BEA) and worker-level wage data from the Current Population Survey, Ebenstein, Harrison, McMillan and Phillips (2014) find that offshoring to low wage countries is associated with a
significant decline in wages for workers employed in routine tasks. On the other hand, Sethupathy
(2013) examines offshoring activities to Mexico using the same BEA data, and finds an increase
in wages and no evidence of greater job losses in domestic locations of offshoring firms. Similar
analysis was performed using 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, Jensen and Schott (2009), 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).[9]

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[8]We undertook a comparison of industry composition of employment in our TAA offshoring sample to data on
employment by industry of U.S.-based parents of U.S. MNCs, provided by the BEA. We found a very high (50%)
rank correlation in employment shares across industries, with some noteworthy outliers. Textiles, apparel and leather
(NAICS313), Furniture (NAICS337) and Wood Products (NAICS321) have much high ranks in the TAA data,
consistent with arms-length offshoring in these industries not being captured in the BEA data. Excluding these
industries increases the correlation to 71%.

[9]Desai, Hines and Foley (2009) describe their work as investigating the effect of foreign investments broadly (rather
than offshoring specifically). They find complementarity between home and foreign affiliates of U.S. multinationals;

The strength of our data is that, because of the nature of the TAA program and classification scheme used by the Department of Labor, we are able to include events of production shifting abroad irrespective of whether it was within-firm or to outside parties. Also, any outbound investments not related to production shifting are excluded from our data.

3   Theoretical Motivation

The theoretical predictions about the effect of offshoring on domestic activity depend crucially on whether the activity is vertically related to the remaining domestic activities of the firm (Harrison and McMillan 2011). We discuss the theoretical background for both vertical and horizontal FDI offshoring, with some more details for a horizontal FDI model with heterogenous firms. Because the nature of fixed costs and marginal cost savings are likely to be similar for both types of offshoring, the results about which type of firms benefits from lower offshoring costs is likely to be similar as well.

3.1   A model of vertical FDI offshoring

In this section, we present a brief version of Sethupathy (2013) extension of Grossman and Rossi-Hansberg's (2008) seminal model of offshoring, where tasks within a vertically linked chain are offshored. While the model in Grossman and Rossi-Hansberg (2008) allows two types of labor, skilled and unskilled, it limits firms to be homogeneous. Sethupathy (2013) allows firm heterogeneity while limiting workers to be homogeneous.

3.1.1   Set-up

There are two sectors, X and Y, and one factor, labor. Sector X has homogeneous goods produced using CRS technology. Sector Y has differentiated products with a monopolistically competitive

they find that when foreign investment (employment compensation) rises by 10%, U.S. domestic investment (employment) rises by 2.6% (3.7%). 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, 2010), and industry-level data for Canada (Hejazi and Pauly, 2001).

market. Workers first look for a job in sector Y and all residual workers are absorbed by the homogenous good, CRS, competitive sector X, where they are paid their marginal product w_X.

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.

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: $\underset{w_d}{\text{Max}}\ \theta ln(w_d - w_x) + (1 - \theta)ln(\pi_{op})$, where 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 $w_d = \eta\pi_{op} + w_x$ where $\eta = \frac{\theta}{1-\theta}$ is the rent sharing parameter.

Consumer demand is characterized by the quasi-linear utility function as in Melitz and Ottaviano (2008). Utility maximization yields demand for product i in sector Y: $p_i = \rho - \gamma q_i - \lambda Q_y$, 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.

3.1.2 Impact of a Reduction 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.

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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 a larger fraction of their production process. Third, the cut-off productivity for survival increases. Park (2014) terms 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. This hurts their profitability, and it becomes harder for non-offshorers to survive.

It is important to emphasize that the employment effect within offshoring firms is ambiguous: 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 parameters of the model (Park, 2014). In fact, the theory described above does not distinguish between different types of workers, nor whether workers that are laid off are re-absorbed into the same firm in the same capacity that they were in prior to offshoring. On the other hand, the fall in offshoring cost unambiguously improves profitability of offshorers and causes their wage rates to rise, if there is rent-sharing.

Thus, this model predicts: (i) an *ambiguous* net effect on firm-level employment; (ii) a *positive* effect on output; (iii) a *positive* effect on wage rates; and (iv) a *positive* effect on the survival rate of offshorers relative to non-offshorers. Further, if total factor productivity (TFP) measurement uses common input deflators for all firms within an industry (as we use in this study), *measured TFP would increase* for offshorers (as they actually face lower input prices, and hence would have relatively lower measured real inputs when a common deflator is used).

In the model above, the positive spillovers to domestic output arise due to vertical linkages between the offshored activity and the remaining domestic activity, with the offshored input now

being lower cost than before. More generally, as discussed in Desai et al (2009), there could also be complementarities 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.

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

If offshoring consists of a 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 also 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).[10] If part of the shifted production was sold through domestic establishments, there would be gains recorded in output of other domestic units (possibly in marketing units). We investigate this possibility by including non-manufacturing establishments in part of the analysis (see discussion in Section 7.2). But if the foreign plant sold directly to other firms directly, these sales would be recorded by the foreign plant, and this would not affect measured output of remaining domestic establishments.

Because the nature of the optimization problem faced by the firm is similar to that discussed above for vertical offshoring, the effect of reduction in offshoring costs can be expected to be similar as well. In particular, if offshoring involves a fixed cost, then offshoring may not be preferred by firms below a cutoff productivity level for whom lowered marginal costs are not sufficient (because of their small scale) to cover the fixed cost. Thus, even for horizontal FDI offshoring, under plausible assumptions, we expect the effect of lowering of the costs of offshoring to be similar to that in Figure 1.

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[10] Harrison and McMillan (2011) and Tintlenot (2013) study the role of this type of "export-platform" FDI.

4 Data & TAA Background

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 U.S. Census Bureau's Longitudinal Business Database (LBD), with basic operational information of the universe of establishments in the U.S.; and the U.S. Census Bureau's Annual Surveys of Manufactures/Censuses of Manufactures (ASM/CMF) that contain more detailed information for manufacturing establishments.

4.1 Trade Adjustment Assistance Program Background and Data

The information on trade-induced layoffs in U.S. manufacturing plants is obtained from administrative data of the U.S. Department of Labor's (USDOL) TAA program.

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. The petitions are filed at the plant level. The minimum requirement for petitioning is that three or more workers were laid off or had their work hours reduced. Historically, the majority of petitions were filed by labor unions, but an increasing fraction is being filed by companies. For our sample period – between 1999 and 2006 – 50% of petitions were filed by companies, 42% by unions and workers, and the remaining 8% by State Workforce Offices.

The petition filing process is straightforward. The petitioner(s) needs to fill out a two-page form with basic information about the employer or layoff event such as name and address of the employer, articles produced by the establishment, and the separation dates of the three workers listed on the form. The petition form is available on USDOL website, and can be found easily through a simple internet search. The petitioner may fax/mail the form, or file it online at no cost. The petition can be filed within a year from the separation date.

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. 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* (an increase in imports of the plant's product at the aggregate level).[11] 45% of petitions in our sample period are denied, as they were deemed not to be trade-related. Decisions made on TAA petitions are published in the Federal Register and on the DOL website.

Once certified, workers displaced from this plant between the "impact date" (i.e., the date the layoffs began as indicated on the TAA petition) and two years from the impact date (or certification date whichever comes later) are eligible for various benefits provided under the TAA program. The benefits, summarized in Appendix Table A.1 (taken from Park, 2012), include job training up to 2 years, remedial training, extended unemployment insurance during training, and other financial support such as relocation allowance and job search allowance. It should be noted that the dollar spending on the TAA program is very small relative to other transfer programs. Per Autor, Dorn and Hanson (2013), per capita spending in 2007 on in-kind medical transfer programs was about $2,500, on social security retirement insurance was about $1,400, on disability insurance was about $300, and on federal income assistance was about $300, whereas spending on TAA payments was just $2. Also, a substantial portion of TAA spending was spent on re-employment services, mainly training (see e.g., Table B-1 in Collins 2012).

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 reflect a *voluntary decision* of the company, indicating a strategic move to relocate activity abroad. Import-competition events, instead, are those driven by external forces (categories (ii) and (iv) above).

One potential concern about studying offshoring using TAA events is whether there is a bias in the direction of capturing only weak struggling firms. We highlight two reasons why this is unlikely to be the case. First, as discussed above, the size of assistance available through this program is fairly limited, and the assistance was directed at training and job-search for displaced workers. Thus, TAA assistance could not serve as a lifeline for firms that were contracting due

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[11] This category (instead of category (ii) customer imports) usually applies when an establishment has many small buyers rather than a few large customers. Many petitions filed in paper industry were certified for this reason.

to other market pressures. Second and more importantly, applicants to the TAA included many successful, well-known companies, and not just small or struggling firms. As an illustration, Table A2 shows a sample of large firms in the TAA petition data; this list includes a number of large and profitable firms (as well as a couple of struggling firms). Nevertheless, in our analysis we will check carefully to see whether firms in the sample have pre-offshoring trends that are different from the control group we will compare them to.

The bulk of the petition data we use was procured through a Freedom of Information Act request; this was then complemented with manual data collection from TAA websites.[12] The petition data 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 is reported only after 2002, after the Trade Reform Act of 2002 revised the coding guidelines. Though unreported, USDOL 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. We classify a total of 19,603 petitions over our sample's impact years range of 1999 to 2006.[13]

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 U.S. Census micro data we use to explore firm-level impacts of offshoring. Our primary source is the Annual Surveys and Censuses of Manufactures (ASM/CMF); we conduct some supplementary analysis using the Longitudinal Business Database (LBD).

In order to analyze the impact of offshoring on different aspects of firm-level operations, we

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[12]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.

[13]USDOL began publishing the investigation reports some time in 1999 on the TAA website and in the Federal Register. However, the investigation reports are not available for all certified petitions. Specifically, between 1999 and 2006, total of 23,327 petitions were filed and 12,831 were certified; of those certified, we were able to manually review and identify the reason for layoff for 9,107 petitions. Thus our final sample includes 9,107 petitions certified with a reason identified and 10,496 denied, totalling 19,603. Appendix Table A6 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).

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, fixed assets, and investment for U.S. manufacturing establishments.

Coverage includes all manufacturing establishments in the Census (CMF) years, and a sub-sample in the ASM years. 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 most recent CMF, with larger establishments receiving a larger weight. Establishments with 1,000 or more employees, as well as all establishments of multi-unit firms, are included with certainty. The ASM sample changes every five years.

The LBD consists of data on all private, non-farm U.S. establishments in existence that have at least one paid employee, 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]

Two potential concerns when undertaking analysis with ASM/CMF variables relates to the potential loss of data in the ASM years, and the lack of non-manufacturing establishments in the ASM/CMF. These concerns are not too severe for two reasons: (i) in our matched sample, a majority of offshoring firms appear to be engaged predominantly in manufacturing activity (based on establishment counts in the ASM/CMF) ; and (ii) as we document in Section 4.4 below, firms (and establishments) that offshore are significantly bigger than average and hence they are dispropor-tionately included in the ASM samples.[15]

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[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]Our ASM/CMF sample size is 64% of the LBD sample; nevertheless, we check robustness of the results to concerns about potential bias from loss of data for ASM years in three ways: (i) in Section 7.2, we check robustness of our employment and wage analysis using the LBD sample; in Section 7.4 we examine robustness in a sub-sample of: (ii) multi-unit firms (all units of multi-unit firms are sampled with certainty in the ASM); and (iii) a balanced panel of establishments. Tests (ii) and (iii) are motivated by other reasons as well, as discussed in Section 7.4.

4.3 Merging of TAA to Census Microdata and Construction of Firm-level Variables

The matching of the plant name and state information in the petition data to the U.S. Census business register is done using name matching algorithms, supplemented with extensive manual checks and modifications; we provide full details on the merging process in the Data Appendix.

Using the firm identification codes available in the Census microdata, we aggregate establishments to the firm-level. 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).[16] In such cases, we use the impact date of the first offshoring event as the firm's initiation of offshoring.

We undertake analysis at the firm aggregate level, and use industry or industry-year effects in most specifications. For multi-unit firms, within each firm we aggregate establishment-level employment by 3-digit 1987 SIC codes, and pick the SIC code with the largest employment as the firm's industry. Other firm-level variables (e.g., employment or value added) are aggregates from establishments in the data. Firm-level factor intensity measures are obtained using firm-level aggregates of underlying variables (e.g., firm capital intensity is firm-level real capital stock divided by firm-level real output).

For productivity measurement, we use a number of different approaches: in addition to labor productivity measures (real output per worker and real 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; in the baseline results reported below, we aggregate productivity measures up to the firm level using the (unweighted) average across all plants at a firm. We check and confirm robustness (unreported) to using an employment-weighted average across all plants, as well as a relative (within-industry) ranking of each of these measures across firms. We also found results generally robust to using

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[16] A certified petition covers all workers laid off between the impact date and two years after the certification of the petition. So if the firm continues to lay off workers as part of a staggered offshoring process beyond two years after certification of an initial petition, it would need to file a second petition for the laid off workers to get TAA support.

a Solow Residual measure of TFP, as well as the residual from a production function regression estimated using the Blundell-Bond (2000) system GMM approach to controlling for endogeneity (which addresses Ackerberg, Caves and Fraser's (2006) critique of the Levinsohn-Petrin approach).

All nominal variables such as output (sales), capital, wages, and input variables (materials and energy) used in TFP measurement, are deflated using appropriate deflators taken from the NBER-CES manufacturing industry database (Becker and Gray 2009). More details on the definitions of real variables and construction of productivity measures are provided in the Data Appendix.

4.4 Summary Statistics: 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. We examine a range of outcomes (y_{ijt}) 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).

The results are shown in Table 1. Our sample of offshorers exhibit premia consistent with what would be expected in a heterogenous firm model with fixed costs for offshoring (such as the model presented in Section 3.1. Specifically, in these models, gains in the form of lower marginal costs of production need to be large enough to offset the fixed cost, which implies that only firms with a sufficiently large size finds it profitable to offshore. Indeed, in our data 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 Fixed Effects (FE) column). On average they pay higher wages (for both production and non-production workers) and are more capital

intensive. They are also significantly more productive, according to most productivity measures. Importantly, these findings of positive size, wage and productivity premia for TAA firms negate the potential concern about the TAA sample being biased towards weak and struggling firms.

Interestingly, the non-production wage and employment share measure shows that 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 offshorers appear to be significantly less skill-intensive relative to similar-sized firms. This finding is intuitive– 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). In the model sketched out in Section 3.1, low skill tasks could be the ones for which the wage gap between foreign and domestic locations are the biggest.

5 Empirical Methodology

The main challenge here is potential endogeneity of the offshoring decision. Reduction in transport costs or tariffs could make producing a good abroad relatively more attractive, and these same reductions could also lead to increased competition at the industry level, which in turn affects output and employment. Alternatively, increase in local input costs, e.g., an increase in wages, could lead to favorable conditions for offshoring. At the same time, increase in input costs could directly affect level of output and employment. These two key sources of endogeneity – reduction in transport costs and/or reductions in input prices – are both likely to be primarily industry-level shocks. Arguably, these shocks may affect small and big firms differently, e.g., wage increases may be concentrated at larger firms which are more likely to have unionized labor. Given our finding in Section 4.4 that offshorers are systematically larger than other firms, conditioning on firm size is necessary to rule out effects from size-correlated shocks.

To control for these potential sources of endogeneity, we adopt a difference-in-differences (DID) approach, comparing offshorers to firms within the same industry that are closest in size to them. Specifically, we use 'nearest neighbor' matching, choosing two controls closest in employment to each offshored firm within the same 3-digit industry, using the LBD. Firms that have one or more

identified offshoring events are excluded from the control group selection pool. Using the LBD for control group selection allows us to select control firms from a larger pool of firms which improves the similarity to the treated firms. We then merge this sample of treated and control firms to the detailed data in the ASM/CMF.[17] The matching creates one 'cell' for each offshorer, comprising the offshorers and two matched controls; in the analysis (specifically to draw event-year graphs), the controls are assigned the same event year as the offshorer firm in the cell.(As an alternative, we use a propensity-score matching approach (discussed in Section 6.2 below). Later, in Section 6.3, we use an IV approach to check robustness of our key results.)

We retain data for a 13 year window, six years before and six years after the offshoring impact year, for offshorers and their matched controls. To report summary DID effects, we collapse the thirteen periods into four groups, two three-year periods prior and two three-year periods after the offshoring event. An outcome variable, y_{ijt}, of firm i belonging to a cell j (where one cell consists of one *treated* firm and one to two controls) observed at time t. We then estimate the following regression specification:

$$y_{ijt} = \beta_0 + (\alpha_{LR\_PRE} + \delta_i\ \beta_{LR\_PRE})\ LR\_PRE_{ijt} + (\alpha_{SR\_PRE} + \delta_i\ \beta_{SR\_PRE})\ SR\_PRE_{ijt}$$

$$+(\alpha_{SR\_POST} + \delta_i\ \beta_{SR\_POST})\ SR\_POST_{ijt}(\alpha_{SR\_POST} + \delta_i\ \beta_{LR\_POST})\ LR\_POST_{ijt} + f_i + e_{ijt} \quad (1)$$

where LR_PRE (SR_PRE) is a dummy equal to 1 for the 4 to 6 (1 to 3) year period prior to the offshoring impact year, SR_POST (LR_POST) is a dummy equal to 1 for the 1 to 3 (4 to 6) year period after the offshoring impact year, δ_i in an indicator dummy for offshorers, and f_i are firm fixed effects. Thus, the β coefficients are period-specific means for offshorers relative to controls. The impact year is the excluded period (absorbed into firm fixed effects). As a robustness check, in Section 7.1.1, we run regressions with cell-period effects (f_{jk}), which allows for flexible industry-size-period specific shocks.

DID estimates of the short-run (long-run) effect of offshoring is the difference $\beta_{SR\_POST} - \beta_{SR\_PRE}$ ($\beta_{LR\_POST} - \beta_{SR\_PRE}$). The difference $\beta_{SR\_PRE} - \beta_{LR\_PRE}$ provides a test for pre-exiting trend.

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[17]We impose a restriction that log employment at one of these 'nearest neighbors' cannot be more than 4 log points different from the comparison offshorer, meaning that not every offshorer is paired with exactly two controls. Further, as discussed in earlier, there is also some potential loss of data for the ASM survey years.

To obtain a richer picture of changes associated with offshoring, we plot a standard event study graph (Angrist and Pischke 2009, Autor 2003) over the 13-year window, six years before and after the impact year, using the following specification:

$$y_{ijt} = \gamma_0 + \sum_{k=-6}^{6} (\beta_k \delta_i + \alpha_k) D_{jk} + f_i + e_{ijt} \tag{2}$$

where D_{jk} is a dummy equal to one if the year is k years from the offshoring (impact) year for cell j (with $k \in [-6,6]$), f_i stands for firm fixed effects, and δ_i is an indicator for an offshoring firm. In this case, α_k provides the trend for the matched controls, and $(\beta_k + \alpha_k)$ provides the trend for the offshorers. Therefore, β_k captures the impact of offshoring k years from the impact year, relative to the matched controls. We plot the trends (and confidence intervals) for the treatment and control group. As discussed in Angrist and Pischke (2009, Chapter 5), these figures allow for a test of causality in the spirit of Granger (1969). If changes in outcome variables are caused by offshoring, we would expect: (a) the offshoring firms and the control group had similar trends before the offshoring event, and (b) a clear break in trend around the initiation of offshoring relative to the control group. Standard-errors are clustered by matched offshorer-controls cells throughout. We use the year prior to the impact year ($k = -1$) as the reference (omitted) year. Note that in the omitted year, estimates of employment will be similar between offshorers and their controls by construction, as the firm fixed effects subsume mean differences. Thus in the results that follow, our focus will be on the comparative differences between the two groups, rather than the absolute magnitude of coefficients for offshorers and controls.

6   Results

6.1   Baseline Analysis: DID using Industry-Size Matched Controls

6.1.1   Size and Wage Measures

The top rows of Table 2 show the estimation results for size and wage measures. All size measures – output, value-added, employment and capital – show a large decline in the short-run. We do not find any 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 DID effects relative to the period leading up to the impact year (SR-PRE), with results presented in the columns headed with "Relative to SR-PRE". We again find significantly negative effects 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 38.5%, in value added is 0.391 log points or 47.8%, in employment is 0.38 log points or 46.2%, and in capital is 0.253 log points or 28.8%. Finally, in the last column, we test and find no evidence of significant prior trends for any of the size variables.

As for firm wage variables, we find no evidence of any significant DID effects, particularly in the long run (though there is a weak (p-value 0.051) decline in blue collar wage rates in the short-run, this is not sustained in the long-run). There is no evidence of prior trends for offshorers relative to non-offshorers in any of the wage measures either.

These results can be seen graphically in sub-figures (a) to (d) of Figure 2, which plots the event-year coefficients for offshorers and controls from Equation 2 with 95% confidence bands, calculated with standard errors clustered by matched offshorer-controls cells. These figures show that both employment and output (real sales) for offshoring firms declined drastically in the impact year, confirming that the impact date in the TAA petition data matches a significant layoff event for offshorers. 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 outweighed by continued downsizing within the firm. Sub-figure (b) shows the same trend for output (real sales). The lack of any prior differential trend between offshorers and their control group is particularly noteworthy; this provides further evidence that the firms where workers qualified for TAA assistance were *not* struggling relative to peer firms, prior to their initiation of offshoring.

These figures suggest causal effects in the sense of Granger (1969), as discussed by Angrist and Pischke (2009). Specifically, these figures show that: (a) the trends for the control group of industry-employment matched firms are very similar prior to the offshoring event; (b) offshoring

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firms do not show a significant declining trend in any of the size prior to offshoring; and (c) there is a stark break in trend for offshorers relative to non-offshorers, consistent with changes being triggered by offshoring.

The lack of any effect of offshoring on either production (blue collar) or non-production (white collar) wages from offshoring is clear from sub-figures (c) and (d). The close similarity in the wage trends for offshorers and the control sample for wages, particularly prior to offshoring, is reassuring, as it suggests that the controls are comparable to the offshorers on multiple dimensions, even though we matched specifically only on employment.

6.1.2   Factor Intensity and Productivity Measures

Results in Table 2 suggest that offshorers become more capital-intensive than their controls after the offshoring event (by 0.142 log points in the short-run and 0.137 log points in the long-run), which is a result of a smaller decline in capital (short-run/long-run declines of -0.121/-0.253 log points) compared to the larger fall in employment(short-run/long-run declines of -0.252/-0.380 log points). The share of non-production workers in total employment also rises modestly at offshoring firms (0.024/0.025 log points in the short-run/long-run), suggesting that layoffs disproportionately affect production workers, consistent with low-skill activities being targeted for offshoring. Small increases are also found for the non-production share of the wage bill.

Among the productivity measures, while the value-added per worker variable shows improvement in both short- and long-run periods after offshoring, this is not true for output per worker, or for any of the total factor productivity (TFP) measures. 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.

The results for labor productivity and TFP are consistent with the larger decline in employment relative to output and capital, and the relative increase in capital compared to output. In particular, the lack of increase in TFP measures is explained by the fact that adjusting for capital, the ratio of value added relative to inputs does not go up for offshorers.

6.1.3 Firm Survival

If offshoring is beneficial to the firm, one potential consequence is that offshoring firms will be more likely to survive in the marketplace. Figure 3 shows the survival rate of offshoring firms compared to control group firms. Specifically, the figure depicts the percentage of plants (sub-figure (a)) or firms (sub-figure (b)) in our LBD sample still in existence in the indicated event-year. The benchmark year is the year prior to the impact year, and therefore has a value of 100% for both offshorers and control firms.(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, while lower than 100% after year +1 indicate plant (firm) exits.)

Within six years of post-impact observation period, almost 70% of firms disappear from the data. However, the survival rates for offshoring firms and controls are nearly identical, particularly at the end of the six-year post impact time-frame. Thus, we find no evidence that offshoring improves the firm's long-run probability of survival.

6.2 Alternative DID Approach: Matching on Propensity

The striking similarity in pre-impact-year trends between the offshorers and industry-size matched controls used in the analysis in Section 6.1, even for wage and productivity measures, addresses one key concern with using a DID approach to control for endogeneity (Angrist and Pischke, 2009).

However, there could be remaining concerns about differential trends based on other (non-size) initial characteristics. To condition on a richer set of variables, we adopt a propensity score matching approach. The potential advantage of this alternative approach is that any post-offshoring effects driven by interaction of pre-existing characteristics (included in the propensity model) with changes in the environment are controlled for by matching on this scalar propensity measure (Rosenbaum and Rubin 1984). For example, suppose some unobserved industry shocks impacted more capital intensive firms (even conditioning on size) negatively, that could potentially bias our baseline estimates; this bias can be controlled by including capital intensity (in addition to size) in the propensity model.

Specifically, we use the following linear propensity model:

$$Offshore_{ijt} = \beta X_{ijt} + f_t + f_j + \varepsilon_{ijt} \qquad (3)$$

where $Offshore_{ikt}$ is the observed offshoring decision (zero or one) for firm i in industry j at time t, X_{ijt} is a vector of firm characteristics including employment, capital intensity, non-production worker wage rate and production worker wage rate, and f_t and f_j are year and 3-digit industry effects.

The results from estimation of the propensity model are presented in Table A3. Consistent with the findings in Section 4.4, we find that employment size is strongly predictive of the probability of offshoring. We also find that, even conditioning on log employment, capital intensity is also a significant predictor of offshoring. While production wages are not significant (conditional on size and capital intensity), non-production wage rate is significantly negatively correlated with offshoring. One plausible explanation is that firms with high white collar wages make higher quality products that are harder to offshore.

Next, using the predicted propensity from the specification in Column 3 of Table A3, we match each offshorer to two firms from within the same 3-digit industry that are closest in propensity score to the offshorer one year prior to offshoring,. As before, we form cells for each offshorer, with up to two similar control firms, where similarity is now based on the composite likelihood of offshoring given a set of observable characteristics.

Table 3 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. All size measures - output, value added, employment, and capital - show significant DID declines both in the short and long-run, with no evidence for statistically significant relative prior trends for any of the size variables, just as described above.

The impact of wage rates are also qualitatively identical to what we found using employment-matched controls. Neither production nor non-production worker wage rates are significantly affected by offshoring in the short- or long-run. The results for factor intensity and productivity measures, are also very similar to what we find using employment-matched controls. Offshorers do become more capital-intensive, again apparently as the result of a 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 either the short- or long-run.

The size results are presented in Figure A1.[18] Both employment and output sub-figures are similar to those in Figure 2. This demonstrates that our key conclusions – strong declines in size measures, some increase in capital and skill intensity, no gains in wages or TFP following offshoring – are robust to conditioning on the broader range of observable characteristics captured by the propensity model, and hence less likely to be biased due to endogeneity of the offshoring decision.

6.3 An Instrumental Variables Approach

As an alternative to the two DID approaches used above, we undertake an Instrumental Variables (IV) analysis, to check the robustness of the sharp declines in size measures. An ideal instrument for offshoring would be a firm-specific reduction in the cost of offshoring, as this would induce the firm to undertake offshoring, without directly impacting output and employment variables.

While such a clean measure is unavailable, we draw on Pierce and Schott (2013), who find evidence that the decline in employment in manufacturing was stronger in those industries for which the threat of tariff increases with China declined most strongly, following conferral of Permanent Normal Trade Relations (PNTR) by the U.S. Congress on China. Interestingly, they find "circumstantial evidence that these changes in employment are driven in part by offshoring."

For a firm contemplating whether to offshore or not, the expected (relative) costs of offshoring could be written as *Expected transport costs + Expected tariff costs - Expected savings in production costs.* The key argument for our IV approach is that the PNTR status, by reducing the probability of a tariff hike, reduced expected tariff costs and hence also reduces the expected overall cost of offshoring. Further, if offshoring involves sunk upfront investments, the reduction of uncertainty from conferral of PNTR status would have reduced benefits from waiting, and thus helped prompt investments required to undertake offshoring (Pierce and Schott, 2013).

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[18]Figures for other variables are omitted for brevity; they are qualitatively similar to baseline figures, and confirm the regression results in Table 3.

The reduced threat of tariff increases for an industry of course provides only industry-level (or product-level) variation. Because other industry-level shocks need to be controlled for using industry-year fixed effects, this PNTR measure alone does not provide a usable instrument (as a reduction of tariff threats would get absorbed by industry fixed or industry-year effects). However, under the plausible assumption that offshoring involves fixed costs, in models with heterogeneous firms (such as the model sketched in Section 3.1), reductions in offshoring costs are more likely to affect larger firms, as the smallest firms are not close to the margin for making the switch to offshoring (e.g., see Figure 1). Thus the decline in expected offshoring costs stemming from granting of PNTR status to China arguably has a stronger effect on larger firms within those sectors where the threat diminished most.

A key question from the perspective of an IV approach is whether reduction in tariff hikes could have had a direct effect on employment, e.g., reduction in uncertainty may have increased import competition by prompting Chinese firms to undertake sunk investments required for entry into the US market. This would not confound the IV analysis, so long as these import competition effects affected firms within an industry uniformly. In fact, influential theoretical models (e.g., Melitz 2003) and empirical work (e.g., Bernard, Jensen and Schott, 2006) suggest that import competition at the industry level has a stronger negative effect on *smaller*, low-productivity firms within the industry, whereas our instrument (based on summary statistics in Section 4.4) would rely on a positive correlation between size and the offshoring decision. To the extent that the instruments predict offshoring for larger firms who may be experiencing lower employment losses from import competition as a result of tariff threats disappearing, our estimates of employment declines may in fact be biased towards zero, and provide a lower bound for offshoring effects.

To implement the IV estimation, we first recast the baseline analysis as a linear long-difference specification (which differences out firm-specific effects):

$$y_{t+3} - y_{t-1} = \beta D_o + f_j$$

Here, D_o is dummy for offshorers, and f_j denotes 3-digit industry fixed effects. The data for offshorers are restricted to the long difference $y_{\tau+3} - y_{\tau-1}$, where τ is the offshoring (impact) year. Thus, the coefficient β reports the mean change in dependent variable after offshoring (three years

post less one year prior to offshoring) relative to similar long-differences for industry peers.

In our benchmark IV analysis, the first stage involves instrumenting for the offshoring dummy using lagged employment, as well as its interaction with the "NTR gap" variable. The "NTR gap" variable was constructed based on Pierce and Schott (2013), as the average of the difference between maximum possible tariff and the MFN (Most-Favored-Nation) tariff rate, over HS8 product lines. These are then concorded to 3-digit SIC codes and merged with our data.

The results from our IV analysis are presented in Table 4. Our first stage results suggest instruments are sufficiently strong, as the Cragg-Donald F statistics for the first stage exceeds 66, well above the range of critical values suggested by Stock and Yogo (2005) for tests for weak instruments. The same is true for the Kleinbergen-Paap F statistic, which may be more appropriate given that errors may not be i.i.d (Baum, Schaffer and Stillman 2007). Further, in Column 3 where we have two excluded instruments (lagged log employment as well as its interaction with the NTR gap variable), Hansen's j statistic is very low (below 0.6) for each case, so that in all cases the test of overidentifying restrictions is far from rejection of the null. To facilitate comparisons between un-instrumented OLS and 2SLS, we normalize the predicted value from the first stage to a [0,1] interval, by subtracting the minimum and dividing by the maximum value.

The IV results confirm the baseline conclusions of strong declines in all size measures. Relative to the un-instrumented OLS results (in Column 1) as well as relative to the DID short-run results in Table 2, the IV estimates in Columns 2 and 3 show greater reductions for employment and capital, and smaller reductions for the sales output and value added.

We checked the robustness of this approach to using a larger set of instruments, including lagged capital intensity, lagged non-production worker and lagged production worker wage rates, and their interactions with the reduction in threat of tariff hikes as additional instruments. The results, reported in Appendix Table A4, are similar to Table 2 in that all size measures show sharp declines. While the first stage F statistics continue to be above the Stock-Yogo critical values, these larger sets of instruments fail Hansen's j test for overidentification.

7 Robustness Checks

7.1 Alternative Fixed Effects and Sub-Samples

7.1.1 Estimation using Cell-Year Fixed Effects

Our baseline regression specifications use firm fixed effects and period effects; while this controls for all fixed firm-specific effects, the time-varying effects are assumed to affect all controls and offshored firms similarly. In order to allow for richer, industry-size-specific and industry-propensity score-specific shocks, we estimate a variant of Equation (1) that includes cell-year fixed effects, where each cell comprises of one offshorer and (up to) two matched controls. Overall, these results, presented in Table 5 for both employment- and propensity-score-matched analysis, are qualitatively very similar to the baseline findings, except that changes in skill intensity and labor productivity measures are no longer statistically significant.

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

We go further towards separating the potential positive effects of offshoring on remaining domestic activity from the destructive effects at the offshored plants by examining non-offshored plants within the offshoring firms. Specifically, for offshorers, we retain only those plants that are not matched with any offshoring events from the TAA petition data, and then construct a "pseudo-firm" aggregate using only these plants.[19] By construction, only multi-unit firms (with at least one non-petition plant) 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.

From the results in Table 6, it is clear that even the remaining domestic plants do not display any 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. Wage rates and productivity generally show no significant changes; capital and skill intensity show some increase consistent with the baseline

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[19]Total employment and firm industry are reconstructed using these non-offshored plants only to create a new set of employment-matched controls. The controls selected using propensity-score matching also utilize the variables of the pseudo-firms.

effects.

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

7.2   Longitudinal Business Database Results

In this subsection, we use data available on all establishments in the LBD to check robustness of the baseline results to two possible difficulties. One, it could be the case that employment gains from offshoring are realized in non-manufacturing establishments of the firm; in particular at the headquarters, or in wholesale or retail establishments of the firm. This would be the case if the product offshored was sold in the U.S. through the firm's marketing arm. Such gains would be missed in our baseline analysis that strictly uses manufacturing data. Because the LBD data includes data on headquarters as well as marketing (wholesale 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, as discussed in Section 4.2. Because ASM sampling puts more weight on larger establishments, small firms in our TAA petition are less likely to be selected into our ASM/CMF sample of offshoring events. Using the LBD, which contains the universe of establishments, allows us to check robustness of our findings to potential bias from this sampling procedure.

We estimate 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. The total number of offshoring events increases from approximately 1,000 (in the ASM/CMF analysis) to 1,400. Because the variables available in the LBD are limited to employment and payroll, we perform the analysis only on total employment, total payroll, and average wage rate (defined as payroll over employment). For propensity score matching analysis, we use wage rate, 3-year employment growth rate, and 3-year wage growth rate, in addition to employment, as our explanatory variables in the propensity model. Results are reported in Table 7.

We find results very similar to those using the ASM/CMF sample. 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 close to that of the baseline (-0.37 log points in Table 3). 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 (ii) baseline findings for size (employment) and wages are not significantly impacted by loss of data from sampling in the ASM. As discussed in Section 4.3, 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 Vertical Linkages

As discussed in Section 3, the vertical supply links between offshored plant and remaining domestic plants play a crucial role in models where there are positive spillovers from offshoring. Thus, if an offshored plant is vertically linked to the remaining domestic plants, there could be different effects compared to non-vertically linked firms. Here we attempt to investigate if this is the case.

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 Syverson (2014), 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 in the other industry. Using the industry code for each establishment, we define an offshoring firm as vertically

linked if the offshored establishment's industry is vertically linked to the industry of at least one other plant within the firm.[20] About 30% of the original sample fits this definition of vertically-linked offshoring firms, and we undertake the baseline DID analyses (both industry-employment and industry-propensity matched) for the subsample of vertically linked firms (and their matched controls).

Table 8 summarizes the estimation results, while results for employment and shipments are graphed in Figure A2 for employment-matched controls. While the reduction in sample size increases the standard errors of the estimated coefficients, surprisingly the magnitudes of the short-run and long-run declines in size measures, as well as the results for other measures, are very similar to the ones we find in baseline full sample specifications.

We interpret these findings as suggesting that linkages measured using Input-Output tables do not necessarily translate to actual vertical linkages in the form of intra-firm shipment, in line with findings of two recent papers. Using U.S. Commodity Flow Survey data, Atalay, Hortacsu, and Syverson (2014) carefully document that firms that are identified as vertically-linked in U.S. Census microdata rarely use inputs made by other establishments within the firm. Ramondo, Rappaport, and Ruhl (2014) 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 usable across the vertical chain.

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 industry linkages from the IO table. Thus, the effects of offshoring are likely to be similar to that envisaged in H-FDI models of Section 3.2, rather than as in the model for vertical FDI sketched in Section 3.1.

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[20]Results do not change by including or excluding the "reflexive" case, where an industry is defined as vertically linked to itself.

7.4 Other Robustness Checks

We also undertook a number of additional robustness checks, which we summarize without reporting tables in most cases, for brevity.

First, we analyze the impact of offshoring using only a sub-sample of multi-unit firms. This addresses the concern, also addressed in our "pseudo-firm analysis in Section 7.1.2, that single unit firms may be focused on one narrow activity, so that the negative direct effect of offshoring on employment may dominate – for multi-unit firms there may be other domestic activity where potential positive effects may be better captured. Also, multi-unit firms are sampled with certainty in the ASM, so using this sample helps additionally check whether baseline results are affected by loss of sample size in ASM years. (We also addressed this concern using LBD data in Section 7.2.) The results in Table A5 are qualitatively identical to our baseline analysis of all firms including single-unit firms, which is not surprising since close to 80% of our ASM/CMF sample is multi-unit.

Second, we investigated whether differential patterns in exit by offshoring firms relative to controls 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. This is controlled for in the treatment cell-year fixed effects analysis in Section 7.1.1, as exiting firms do not contribute to estimated effects. Nevertheless, as an additional check, we re-estimated our results using only firms who were present for all 13 years of the 13 year event window (a balanced panel). We find baseline results are consistent for this sub-sample. (This test further reconfirms that the baseline results are not confounded by changes in sample size in the ASM years.)

Third, we tried alternative methods for aggregating TFP, as described in Section 4.3. Fourth, we checked robustness of key results to using only a sample of firms that filed a single offshoring petition in the sample period. Fifth, we repeated the analysis only for single-unit firms. Sixth we altered the composition of covariates in the propensity score estimation to include employment growth rates and productivity measures. Seventh, we checked robustness to examining a subsample of pre-2002 offshorers; results suggest no changes in the pattern of findings over different years. Eighth, as discussed before, we checked robustness of the TFP results to using a Solow residual measure, and residual from production function estimated using the Blundell and Bond (2000)

methodology. Finally, we performed a number of concurrent combinations of these checks including multi-unit firms in a balanced panel, pseudo-firms that were vertically linked, and pseudo-firms using LBD data. Our baseline results remain 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. 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 (DID) methodology. We find that employment declines significantly at the firm level following initiation of 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. We verify that this is not simply the result of decline at the affected plant; employment falls significantly (only slightly lower in percentage terms than at the affected plants) at aggregated non-offshoring establishments. We checked and confirmed robustness of the decline in employment using an instrumental variables approach, where we use as instrument the lagged size of firms interacted with a reduction in threat of tariff increases following conferral of PNTR status to China (which reduced both the expected value and uncertainty about the marginal costs of offshoring).

We also find that output, value added and capital drastically declined after offshoring, with little evidence of any significant change 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 total factor productivity measures relative to the control groups.

Interestingly, we find no difference in survival rate between offshorers and matched industry peers. So offshorers are not failing or completely shutting down U.S. activity at a faster rate than

matched peers. This result, and the facts that offshorers are larger, pay higher wages and are more productive than average, and that pre-offshoring trends of output, employment, wages and productivity for offshorers are very similar to matched peers, imply that our results are not driven by struggling firms disproportionately choosing to offshore.

While our baseline analysis uses only manufacturing sector data, we found that there was no net employment gains even including non-manufacturing establishments, using data from the LBD, which covers non-manufacturing establishments as well. We conclude that for our sample of offshoring events, offshoring was a strong substitute for domestic activity, reflected in negative effects at remaining domestic units.

Our findings suggest that the pathway of vertical linkages, crucial for complementarity in models such as the one sketched in Section 3.1, is not operational in our data. Thus, the offshoring activities in our sample appear related to 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 could generate negative employment and output spillovers as we find, if some supporting activities in other domestic units are closed down following the production shift.

This interpretation of our results are in line with the findings of Atalay, Hortacsu, and Syverson (2014) and Ramondo, Rappoport and Ruhl (2014), 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 conjecture that most of our offshoring events are related to horizontal shifts echoes Ramondo, Rappoport and Ruhl's (2014) conclusion that most foreign affiliate activity is "horizontal" in nature rather than truly vertically linked to home activities of MNCs.

A couple of qualifications are to be noted when interpreting our results. One, in the TAA data we observe only those offshoring firms who did not re-absorb their workers within the same plant (as plants where workers were re-absorbed would not file for TAA assistance); while this is still a valid sample to check for potential complementarities in other parts of the firm, our results should be considered as average effects for non-absorbers, rather than for offshorers as a whole. Despite this qualification, we believe our findings contribute to the very important debate on the effect of offshoring on US firms' domestic operations by provide evidence from a large sample of

verified offshoring events; our findings are particularly relevant in the context of other recent papers that have documented positive spillovers from offshoring (e.g., Sethupathy 2013, Desai et al 2009). Further, the data do not suggest that this sample of "non-absorbers" were struggling firms. In fact, the offshorers in our sample are bigger, more productive and pay higher wages in level terms than the average industry peers, and the pre-offshoring trends in size, wages and productivity are no different than matched control firms.

Two, we strongly emphasize that our results *do not* imply negative welfare effects from offshoring. Given data limitations, two important channels for potential gains – reduced output prices and increased global firm profits – are not measured in this paper.[21] Potential 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 our data.

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[21]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 at domestic establishments. In particular, we will not capture profits in foreign operations retained abroad.

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

Variable	Definition	OLS	Industry FE
Size Measures			
Output	Log(Real total sales = deflated value of shipments)	3.044	2.607
		(0.000)	(0.000)
Value Added	Log(Real value added)	2.919	2.521
		(0.000)	(0.000)
Employment	Log(Employment)	2.679	2.313
		(0.000)	(0.000)
Capital	Log(Real capital stock)	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 Measures			
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 Measures			
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 (Levinsohn-Petrin), 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 reported figures are the coefficient on a dummy that equals one for firms that offshored after the year 2002; the figures in parenthesis are p-values. The first column (OLS) captures the mean difference between offshorers and all other firms, while the second column (Industry FE) includes 3-digit SIC industry fixed effects and hence captures the mean difference between offshorers and all other firms within the same industry. The number of observations for all of the statistics is 131,377. The data source is the Census of Manufactures for 2002. More details on the size and productivity measures are provided in the Data Appendix (Section A.4).

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

	LR_PRE	SR_PRE	SR_POST	LR_POST	Relative to SR_PRE SR_POST - SR_PRE	Relative to SR_PRE LR_POST - SR_PRE	Pre-Trend Test SR_PRE - LR_PRE
Size							
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							
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. Variables are as defined in Table 1. Each offshorer is matched to up to two firms closest in employment within the same 3-digit industry. Each row corresponds to a regression of the variable listed in column 1 on offshorer-specific period dummies (given in column headings), and event-year and firm fixed effects, so reported coefficients are period-specific means for offshorers relative to controls. LR_PRE is a dummy equal to one for offshorers in the long run pre-offshoring period (four to six years prior to the offshoring impact year). SR_PRE is a dummy equal to one for offshorers in the short-run pre-offshoring period (one to three years prior to the impact year), SR_POST is a dummy equal to one for offshorers in the short-run post-offshoring period (one to three years after the impact year), and LR_POST is a dummy equal to one for offshorers in the long-run post-offshoring period (four to six years after the impact year). The impact year is the excluded period (absorbed into event-year and firm fixed effects). The figures in parenthesis are p-values based on standard errors clustered by industry-size cells.

Table 3. Difference-in-Differences Estimation: All Firms, Propensity Score-Matched

	LR_PRE	SR_PRE	SR_POST	LR_POST	Relative to SR_PRE		Pre-Trend Test
					SR_POST - SR_PRE	LR_POST - SR_PRE	SR_PRE - LR_PRE
Size							
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							
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. Variables are as defined in Table 1. Each offshorer is matched to up to two firms closest in predicted propensity (based on Column 3 of Table A3), within the same 3-digit industry. Each row corresponds to a regression of the variable listed in column 1 on offshorer-specific period dummies (given in column headings), and event-year and firm fixed effects, so reported coefficients are period-specific means for offshorers relative to controls. LR_PRE is a dummy equal to one for offshorers in the long run pre-offshoring period (four to six years prior to the offshoring impact year). SR_PRE is a dummy equal to one for offshorers in the short-run pre-offshoring period (one to three years prior to the impact year), SR_POST is a dummy equal to one for offshorers in the short-run post-offshoring period (one to three years after the impact year), and LR_POST is a dummy equal to one for offshorers in the long-run post-offshoring period (four to six years after the impact year). The impact year is the excluded period (absorbed into event-year and firm fixed effects). The figures in parenthesis are p-values based on standard errors clustered by 3-digit industry-propensity score cells.

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Table 4. Instrumental Variables Estimation: All Firms, Four-year Long Differences

Dependent Variable	OLS	2SLS	2SLS
ΔOutput	-0.1434	-0.0937	-0.0737
	(0.001)	(0.001)	(0.000)
			[0.5861]
ΔValue Added	-0.1823	-0.1666	-0.1352
	(0.000)	(0.000)	(0.001)
			[0.0377]
ΔEmployment	-0.1971	-0.4715	-0.3866
	(0.000)	(0.000)	(0.000)
			[0.065]
ΔCapital	-0.1447	-0.3369	-0.2716
	(0.000)	(0.000)	(0.000)
			[0.5428]
			Lagged Log (Emp),
Instrument(s)		Lagged Log (Emp) $\times$ NTR gap	Lagged Log (Emp) $\times$ NTR gap
Cragg-Donald-Wald F		306.94	157.41
Kleinbergen-Paap F		123.05	66.68

Notes: Number of observations is 39,676. Each statistic reports, for a distinct regression, the coefficient on a dummy equal to one for offshorers. In all regressions, the dependent variable is a four-year long difference, with the data for offshorers restricted to the long difference between three years after offshoring and one year before offshoring. To make comparisons to the un-instrumented case in Column 1, we normalize the predicted variable from first stage, by subtracting the minimum value and scaling by the maximum value (this only scales the coefficients and does not affect standard errors or other test statistics). All specifications include 3-digit industry fixed effects, so that the reported coefficients provide the mean difference between the post offshoring change for offshorers and a similar long-difference for industry peers. The figures in parenthesis are p-values based on standard errors clustered by 3-digit industry-year. In column 3, where number of instruments (2) exceed number of endogenous variables (1), the Hansen's j statistic (overidentification test) is reported in square brackets.

Table 5. Difference-in-Differences Estimation: All Firms, Cell-Year Fixed Effects

	Employment-Matched				Relative to SR_PRE		Pre-Trend Test	Propensity-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	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST - SR_PRE	SR_PRE - LR_PRE
Size														
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														
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)	(0.803)	(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 an offshoring dummy and cell-year fixed effects, where each cell comprises one offshorer and up to two matched controls based on size (in first seven columns) and on the propensity (in the last seven columns) from within the same 3-digit industry. The figures in parenthesis are p-values based on standard errors clustered by industry-size/industry-propensity score cells.

Table 6. Difference-in-Differences Estimation: Pseudo-Firms

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

Notes: In this analysis, for the offshoring firms we construct a "Pseudo-firm" aggregate including only non-offshoring establishments (i.e., excluding the specific establishment(s) for which TAA petitions were filed; single unit offshorers, as well as multi-unit firms where all establishments filed TAA petitions get excluded). 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 based on standard errors clustered by industry-size/industry-propensity score cells.

Table 7. Difference-in-Differences Estimation: LBD Sample
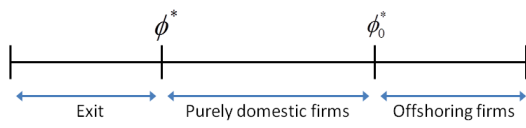
| | | Employment-Matched | | | | | | | Propensity-Matched | | | | | | |
| | | | | | Relative to SR_PRE | | Pre-Trend Test | | | | | Relative to SR_PRE | | Pre-Trend Test |
	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST - SR_PRE	SR_PRE - LR_PRE	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST - 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 based on standard errors clustered by industry-size/industry-propensity score cells.
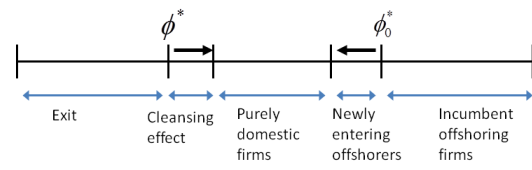
Table 8. Difference-in-Differences Estimation: Vertically Related Firms Only

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

(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
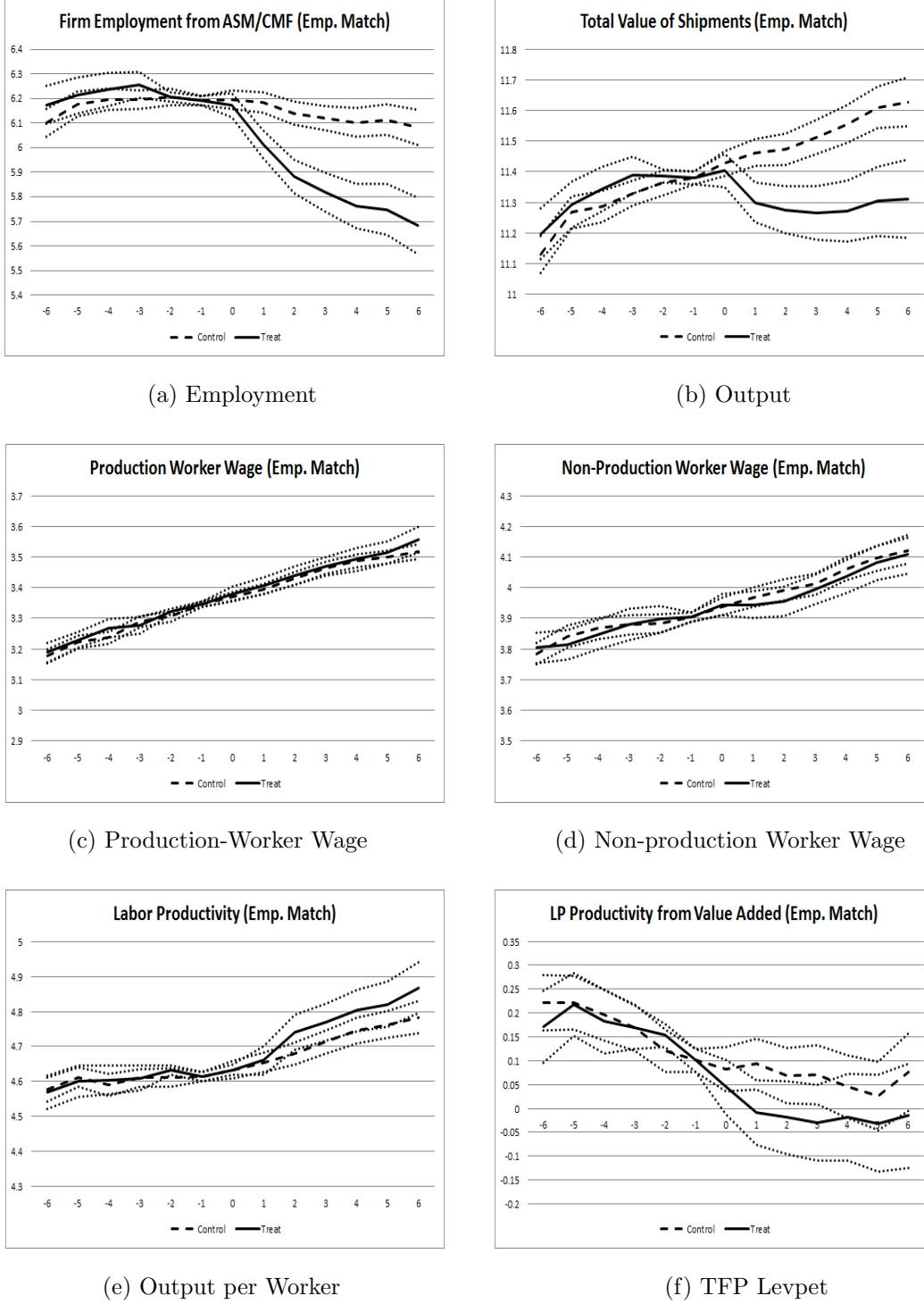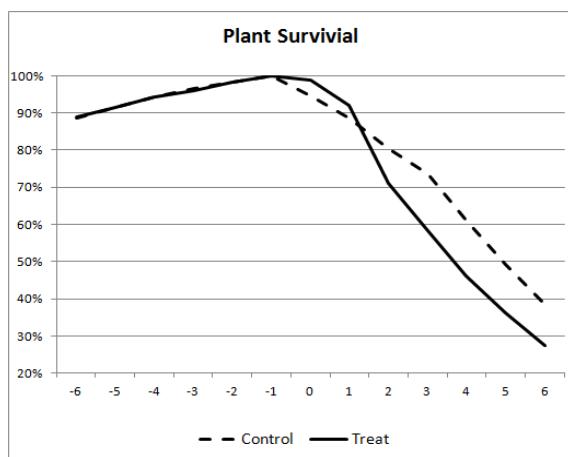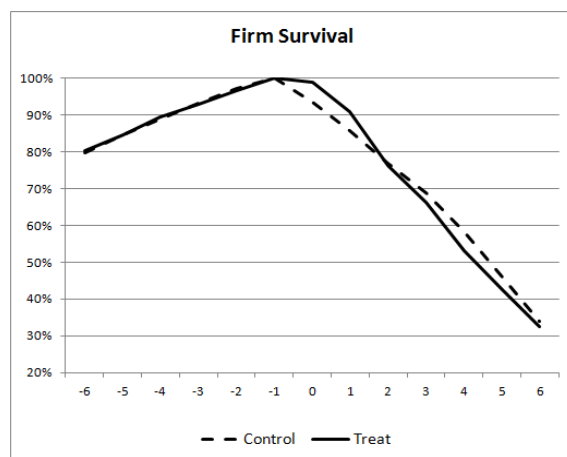
Figure 2. Employment-Matched Difference-in-Differences Estimation Results

Notes: The figures plot coefficients on event-year dummies (i.e., dummies for number of years relative to the offshoring impact year) for offshorers (labeled "Treat") and the control group (labeled "Control"), in a regression of the dependent variable on the event-year dummies and firm fixed effects (see Equation 2 for the precise specification). Each offshorer is matched to up to two firms closest in employment within the same 3-digit industry. The number of observations for each regression (row) is 22,556. Variables are as defined in Table 1. The dotted lines represent 95% confidence bands using standard errors clustered by industry-size cells.

(a) Plants   (b) Firms

Figure 3. Survival Analysis

Notes: The figures on the left (right) plots the fraction plants (firms) from the year prior to offshoring (event year -1) that are in the sample in any of the other event years, separately for offshorers (labeled "Treat") and controls (labeled "Control"). The lower than 100% numbers for years < -1 are because some plants (firms) present in event year -1 are not yet born in those years, while lower than 100% numbers for years > -1 are because some plants (firms) close down (close down or are acquired).

Table A1. Benefits and Services Provided by the TAA Program

Services and Benefits	*Description*
Rapid Response Assistance	Inform workers of various services available for them. Available for all displaced workers, certification not necessary
Reemployment Services	Assist workers with reemployment by providing career counseling and assessment, job search related workshops, job search assistance and referrals. Career assessment determines whether and which training is beneficial to each participant.
Relocation Allowance	When a participant gets a job that requires moving, the program compensates 90% of moving expenses with a stipend of three weeks wage. Maximum of $ 1,250 [a]
Job Search Allowance	Compensates 90% of the cost of job searches outside commuting area. Maximum of $ 1,250[a]
Training	Participants are eligible for training up to 104 weeks. a. Classroom Training: Targeted to obtain skill sets that are specific to an occupation of choice. Training provided by local community colleges or vocational training schools. b. Remedial Training: e.g. Literacy, English as a Second Language, and GED, Can occur concurrently with other training or during additional 26 weeks from the end of regular training c. On the Job Training (OJT): If a participant is employed under OJT, the TAA program pays 50% of the wage rate to the employer during the training d. Customized Training: The training is customized to tasks of a specific firm, but the trainees are not necessarily employed by this firm. * Training waiver may be issued to a participant if (i) she will be recalled soon, (ii) she has marketable skills, (iii) she has a health problem, (iv) training is not available, (v) enrollment is not available
Trade Readjustment Allowance (TRA)	A participant is eligible to receive income support for up to 104 weeks as the following: a. Unemployment Insurance: During the first 26 weeks from separation b. Basic TRA: During the first 26 weeks from exhaustion of UI. This requires training enrollment unless[b] (i) the participant has obtained a training waiver, or (ii) has completed approved training c. Additional TRA: During 52 weeks from exhaustion of Basic TRA. Training enrollment is required without exception. d. Remedial TRA: Participants who are enrolled in remedial training qualify for 26 weeks of income support in addition to 104 weeks of UI, basic TRA, and additional TRA.
Health Insurance Tax Credit[c]	This is a subsidy of 65% of the qualifying health insurance premium paid. The subsidy will be paid as a Tax Credit. All TAA and NAFTA-TAA participants all are eligible.

Table A2. Sample of large firms in TAA (2002 data)

	Employment	Revenue ($ mn)	Profits ($ mn)
Bausch & Lomb	11,500	1,817	73
Bayer AG	122,600	30,213	1,081
Black and Decker	22,300	4,394	230
Boeing	166,000	54,069	492
Chevron	53,014	91,685	1,132
Honeywell	108,000	22,274	(220)
Lucent Technology	75,940	17,350	(4,975)
Sony	168,000	57,108	115

Notes: Employment, revenue and profits (net income) compiled from 10K filings and the Compustat database. Due to confidentiality restrictions, we cannot indicate which, if any, of these firms we were able to match to the US Census microdata.

Table A3. Propensity Model Estimates

	(1)	(2)	(3)
Employment	0.0036**	0.0035**	0.0036**
Capital Intensity		0.0007**	0.0007**
PW Wage Rate			0.0001
NPW Wage Rate			−0.0014**
R-squared	0.05	0.05	0.05

Notes: Dependent variable is a dummy=1 if the firm offshored in any year in the sample period. All specifications include year and 3-digit industry fixed effects. Refer to Table 1 for definitions of the control variables. Number of observations is 302,978. ** denotes significance at 1% level and * at 5% level.

**Table A4.** Alternative Instrumental Variables Estimation: Expanded Instruments Set
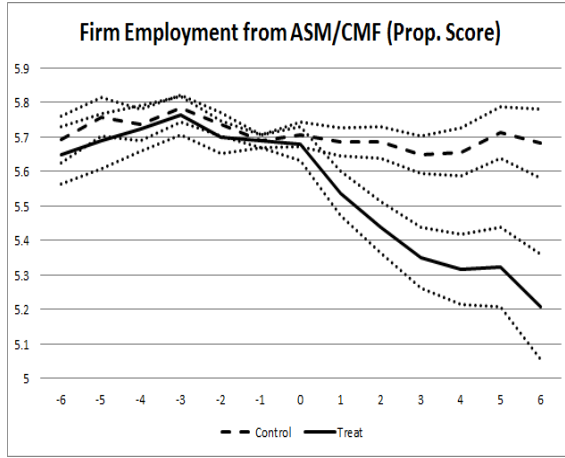
Dependent Variable	OLS	2SLS	2SLS
ΔOutput	-0.1434	-0.0604	-0.0480
	(0.001)	(0.017)	(0.039)
		[182.411]	[212.637]
ΔValue Added	-0.1823	-0.1237	-0.1050
	(0.000)	(0.000)	(0.000)
		[127.636]	[166.186]
ΔEmployment	-0.1971	-0.4240	-0.3953
	(0.000)	(0.000)	(0.000)
		[510.742]	[651.771]
ΔCapital	-0.1447	-0.4944	-0.4851
	(0.000)	(0.000)	(0.000)
		[1942.232]	[2170.942]
Instrument(s)		Lagged Log (Emp) $\times$ NTR gap	Lagged Log (Emp) $\times$ NTR gap
		Lagged Cap. Int. $\times$ NTR gap	Lagged Cap. Int. $\times$ NTR gap
		Lagged PW Wage $\times$ NTR gap	Lagged PW Wage $\times$ NTR gap
		Lagged NPW Wage $\times$ NTR gap	Lagged NPW Wage $\times$ NTR gap
			Lagged Log (Emp)
			Lagged Cap. Int.
			Lagged PW Wage
			Lagged NPW Wage
Cragg-Donald-Wald F		78.11	40.44
Kleinbergen-Paap F		31.94	13.33

Notes: Number of observations is 39,676. Each statistic reports, for a distinct regression, the coefficient on a dummy equal to one for offshorers. In all regressions, the dependent variable is a four-year long difference, with the data for offshorers restricted to the long difference between three years after offshoring and one year before offshoring. To make comparisons to the un-instrumented case in Column 1, we normalize the predicted variable from first stage, by subtracting the minimum value and scaling by the maximum value (this only scales the coefficients and does not affect standard errors or other test statistics). All specifications include 3-digit industry fixed effects, so that the reported coefficients provide the mean difference between the post offshoring change for offshorers and a similar long-difference for industry peers. The figures in parenthesis are p-values based on standard errors clustered by 3-digit industry-year. In column 2 and 3 where number of instruments exceed number of endogenous variables, the Hansen's j statistic (overidentification test) is reported in square brackets.
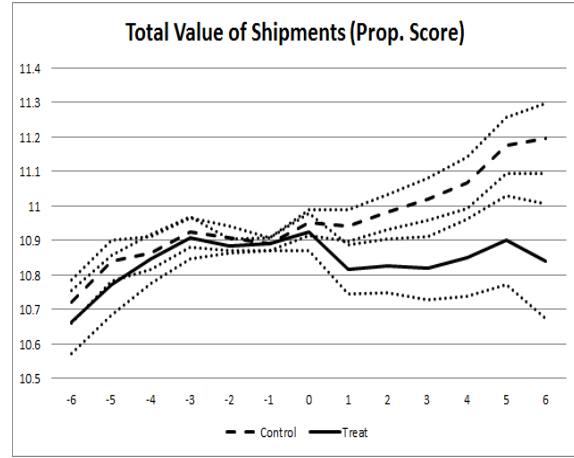
Table A5. Difference-in-Differences Estimation – Multi-Unit Firms Only

	Employment-Matched				Relative to SR_PRE		Pre-Trend Test	Propensity-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	LR.PRE	SR.PRE	SR.POST	LR.POST	SR.POST - SR.PRE	LR.POST - SR.PRE	SR.PRE - LR.PRE
Size														
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														
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 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 based on standard errors clustered by industry-size/industry-propensity score cells.
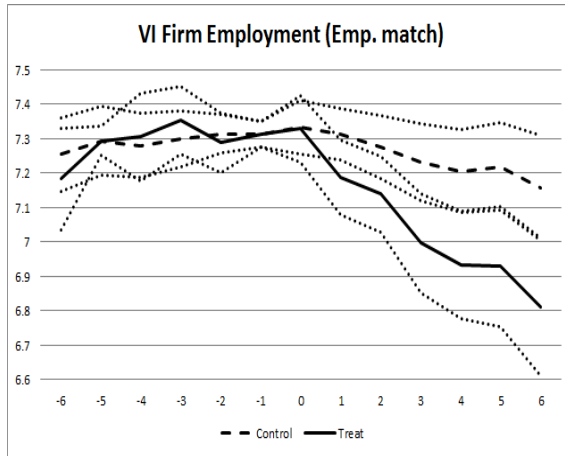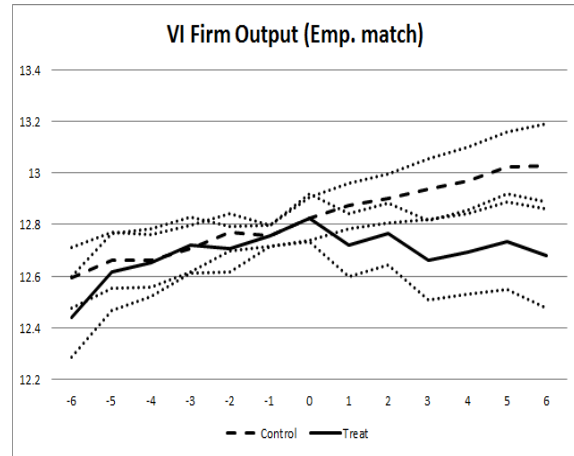
(a) Employment (b) Output

Figure A1. Propensity Score Matched DID Estimation Results

Notes: The figures plot coefficients on event-year dummies (i.e., dummies for number of years relative to the offshoring impact year) for offshorers (labeled "Treat") and the control group (labeled "Control"), in a regression of the dependent variable on the event-year dummies and firm fixed effects (see Equation 2 for the precise specification). Each offshorer is matched to up to two firms closest in predicted propensity (based on Column 3 of Table A3), within the same 3-digit industry. The number of observations used for each figure is 18,949. Variables are as defined in Table 1. The dotted lines represent 95% confidence bands using standard errors clustered by industry-propensity score cells.

(a) Employment (b) Output

Figure A2. Employment-Matched DID Estimation Results: Vertically Linked Firms Only

Notes: The figures plot coefficients on event-year dummies (i.e., dummies for number of years relative to the offshoring impact year) for offshorers (labeled "Treat") and the control group (labeled "Control"), in a regression of the dependent variable on the event-year dummies and firm fixed effects (see Equation 2 for the precise specification). Sample includes only offshorers where the offshored plant is vertically linked to other domestic units (i.e., industry of offshored plant purchases or supplies substantial input from/to industries of other plants in the firm, per Input-Output Tables). Each offshorer is matched to up to two firms closest in employment from within the same 3-digit industry. The number of observations used for each figure is 5,935. Employment and Output are as defined in Table 1. The dotted lines represent 95% confidence bands using standard errors clustered by industry-employment cells.

A Data Appendix

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

A.1 Linking TAA to the Business Register

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) accessed through the U.S. Census' Michigan Research Data Center. The information on offshoring events is obtained from the petition data of the Trade Adjustment Assistant program (TAA). Direct matching of these two data are not possible because TAA petition data do not have establishment or firm identifiers used in the Census datasets. The information that can identify a particular establishment is company name and address (state, city, street address, and zip code). We first match the TAA petition data to the Business Register (BR, formerly known as the Standard Statistical Establishment List or SSEL) using name and state, and then match the merged data to LBD using plant identifiers available in both the BR and the LBD.

Name and address matching between TAA petition data and the BR 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 borrow from algorithms used in an earlier project that involved matching NBER patent data to the business register (Balasubramanian and Sivadasan, 2011).

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 BR 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 BR 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 BR 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 BR 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 BR match rather than impact year because the plant identifiers of the best BR year are most reliable. This BR-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 BR 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 A7 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

For each year, we group all establishments by the firm identifier (available in the LBD), 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 firm identifier of the offshored establishment to the firm-level data constructed as described above. The matching is done for the year before the offshoring event.

A.4   Details on size and productivity variables

Key variables used in the analysis are as defined below. Deflators used for obtaining real values are taken from the NBER-CES manufacturing industry database (Becker and Gray 2009).

1. **Size measures**

   (a) "Output" is log real sales, which is defined as value of shipments deflated using 4-digit SIC industry-specific output deflators.

   (b) "Value Added" is log real value added, which is defined as log of (real sales - real materials - real energy costs).

   (c) Log employment is the log of the total number of employees reported in the data.

   (d) Log real capital is defined as the log the real depreciated capital stock. The real depreciated capital stock is constructed using the perpetual inventory method. The depreciation rates (and deflators) used to construct the plant specific real depreciated structures and equipment stocks were taken from Becker and Gray (2009).

2. **Input measures (used to define real value added)**

   (a) Log real materials is the log of the deflated cost of materials used.

(b) Log real energy costs is the log of the deflated cost of fuel, electricity and other energy sources used.

3. **Productivity measures**

   (a) **Output per worker:** This measure of labor productivity is defined as log real value of shipments divided by employment.

   (b) **Value added per worker:** This measure of labor productivity is defined as log real value added divided by employment.

   (c) **TFP-Levpet:** To estimate the TFP-Levpet measure, we assume a Cobb-Douglas value-added production function:

$$v_{it}^j = \beta_l^j.l_{it} + \beta_n^j.n_{it} + \beta_k^j.k_{it} + \epsilon_{it}^j \tag{4}$$

   where v is the log real value added (gross output net of intermediate outputs), l is the log of the number of production (blue collar) employees, n is the log of the number of non-production (white collar) employees and k is the log of the real capital employed. We allow the coefficients in the production function to vary by (2-digit NIC) industry (indexed by j), by estimating the production function separately for each industry. The index i stands for the plant and t stands for the year. We define total factor productivity as the residual ϵ_{it}.

   We assume that the productivity residual has two components (and drop the industry index j from our notation to reduce clutter): $\epsilon_{it} = \omega_{it} + \eta_{it}$ where ω_{it} is the component of the productivity shock that is known to the decision-maker before she makes the choice of inputs (k_{it}, l_{it} and n_{it}), but is unobserved by the econometrician. This "transmitted" component thus leads to a correlation between the input variables (regressors) and the productivity residual (error term), potentially biasing OLS coefficients. η_{it}, which is assumed to be orthogonal to the regressors, captures all other deviations arising from classical measurement error, optimizing errors, etc. The LP method assumes the demand of the intermediate input (in our case the log of real materials) is a function of the firm's state variables k_{it} and ω_{it}. Making mild assumptions about the firm's production technology, Levinsohn and Petrin (2003) ω_{it} can be written as a function of k_{it} and the intermediate input. Thus, a first stage regression of value added on labor inputs and a polynomial (or semi-parametric) function of capital and materials, allows us to estimate coefficients on labor inputs. To recover the coefficient on capital, the LP methodology relies on two assumptions. One is that the ω_{it} follows a first-order Markov process. Then, assuming that k_{it} is chosen prior to realization of period t shocks, k_{it} is orthogonal to innovations in productivity. Over-identifying moment conditions are available if we assume lagged material and other inputs are orthogonal to the innovation in productivity as well. Further details are available in Levinsohn and Petrin (2003).

   (d) **TFP-OLS measure:** The TFP-OLS productivity measure is defined as the residual from an ordinary least squares (OLS) regression (as in specification 4 above) of log real value added on log blue-collar employment, log white-collar employment, and log real capital with establishment fixed effects. The establishment fixed effects control for potential endogeneity from unobserved (but fixed) variations in productivity across establishments.

Table A6. Results of Aggressive Matching Procedure of TAA to BR

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 A7. 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