

# Domestic Gains from Offshoring? Evidence from TAA-linked U.S. Microdata<sup>\*†</sup>

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## Abstract

We construct a new linked data set with over one thousand offshoring events by matching Trade Adjustment Assistance (TAA) program petition data to U.S. Census Bureau microdata. We exploit these data to study the short- and long-term effects of offshoring on domestic firm-level employment, output, wages, and productivity in this large sample of offshoring events. As implied by heterogeneous firm models with high fixed costs of offshoring, we find that the average offshoring firm in the TAA sample is larger, more productive, older, and more likely to be an exporter, than the average non-offshorer. After initiating offshoring, TAA-certified offshorers experience large declines in employment (0.38 log points), output (0.33 log points) and capital (0.25 log points), and a concomitant increase in capital and skill intensity, relative to their industry peers. We find no significant change in average wages or productivity measures. Even six years after the initial offshoring event, we find no recovery in employment, output, or capital, and a higher probability of exit. We find similar results (including decline in output, and unchanged wages and productivity) for the aggregate of non-TAA certified plants of multi-plant offshoring firms. We find that the substitution of domestic activity by offshoring is stronger for relatively lower wage, lower capital intensity, lower productivity offshorers. Our results are consistent across two separate difference-in-differences (DID) approaches, and a number of robustness checks.

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†Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of the U.S. Census Bureau, the Board of Governors of the Federal Reserve System, or any other person associated with the Federal Reserve System. All results have been reviewed to ensure that no confidential information is disclosed.

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# 1 Introduction

The impact of trade on the U.S. labor markets, particularly its contribution to the steep decline in manufacturing employment and increase in income inequality, has been a topic of intense academic and policy interest (Feenstra 2010, Krugman 2008, Autor et al. 2013, Pierce and Schott 2013).<sup>1</sup> One of the major pathways through which trade can impact employment and wages is through the offshoring of production (Blinder 2009).

That said, theoretical predictions about the effects of offshoring vary widely across models. When the offshored activity has vertical linkages to domestic activity, there is the potential for within-firm complementarities (Harrison and McMillan 2011, Desai et al. 2009). For example, Sethupathy (2013) shows that domestic units benefit from lower input costs of the offshored inputs/tasks. While the net effect on employment is ambiguous, total output, profits, and productivity at an offshoring firm go up; if workers share in the profits through bargaining, wages can rise at offshoring firms. Further, restructuring through offshoring could help firms avoid failure relative to non-offshorers (Park 2015).<sup>2</sup> If offshoring instead consists of unlinked “horizontal” activity, foreign employment can substitute for domestic employment, as supporting activities in other parts of the firm may be eliminated (Harrison and McMillan 2011, Markusen and Maskus 2001). Further, without lower input costs, firm productivity, and hence domestic wages, would be unaffected.

Thus how offshoring affects firm-level employment and other outcomes is an important empirical question. However, empirical work has been hampered by the lack of direct data on offshoring (Kirkegaard, 2007), particularly at the level of an individual business.<sup>3</sup> In this paper, we assemble a new dataset of offshoring events and firm performance by linking offshoring-induced employment layoff events from the Trade Adjustment Assistance (TAA) program to U.S. Census Bureau business data, including the Census of Manufactures, Annual Survey of Manufactures, and the

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<sup>1</sup>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.

<sup>2</sup>Park (2015) 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. A more detailed summary of related theoretical models is presented in Appendix B.

<sup>3</sup>We survey the empirical literature on offshoring later in this section.

Longitudinal Business Database. The TAA program is explicitly designed to help workers who lose jobs for trade-related reasons, and approved TAA petitions are classified into categories that allow us to distinguish offshoring events from other types of job losses.<sup>4</sup> This data contains the identity of offshoring firms as well as the date their offshoring activity began; linking this information to Census data produces a domestic activity database for about 1,000 firms who offshored between 1999 and 2006. We use this sample to understand offshorers and analyze the effects of offshoring on a range of firm level outcomes.

First, we examine the characteristics of these offshoring firms relative to the overall population. Our sample of offshorers account for a peak share of over 13% of manufacturing employment (in 2003), and a peak share of 11.8 % (again in 2003) of employment losses in declining/closing firms.<sup>5</sup> Consistent with models where offshoring involves a fixed cost (meaning the most productive firms select into offshoring), offshorers in our sample are *ex ante* larger, more productive, more capital intensive, older and more likely to be exporters than non-offshoring firms. Interestingly, offshoring firms are *not* more skill intensive than non-offshorers in the same industry.<sup>6</sup> These empirical regularities demonstrate that our sample of offshorers is not limited to firms that are relatively small or unproductive, a key fact underpinning our findings about post-offshoring performance described below.

Next, we analyze post-offshoring outcomes for these firms. A fundamental concern for the analysis is the potential endogeneity of the offshoring decision, whereby offshoring is triggered by factors that also directly affect firm activity. We 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 an 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

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<sup>4</sup>Petition decisions can be classified into three types including offshoring-related, import competition-related, and non-trade-related (which are rejected under TAA). 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 2.

<sup>5</sup>We refer to declining/closing firms as those with employment losses from year-to-year, including firms who are not found in the following year. The share of employment losses is total change in employment among all offshoring firms we identify over total employment losses at declining/closing firms.

<sup>6</sup>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).

consisting of the offshorer and these controls. We then estimate difference-in-differences (DID) effects of offshoring by comparing offshorers to these industry-size matched controls, which allows for the effect of industry shocks to vary by firm size. While this procedure 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 utilize propensity score matching, where treated firms are matched to controls that have a similar probability of offshoring based on more covariates (Rosenbaum and Rubin, 1984). In addition to employment, we include capital intensity, production and non-production wages, firm age, and export status in the propensity model.

Relative to their controls, offshoring 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 employment recovery in the longer term: over a six-year period starting from the initiation of offshoring, firm-level employment remains relatively low. Comparing employment shortly before offshoring to six years after, we find an average drop of 0.38 log points. Consistent with the decline in employment, we find stark declines in output (0.33 log points) and capital (0.25 log points) at the firm level. Concomitant with the larger decline in employment than in capital, we find a significant increase in capital intensity (0.14 log points), and a modest increase in skilled share of employment (0.025 log points).

We find no discernible change in wages for either production or non-production workers, and small gains in labor productivity (measured either 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. Consistent with the contraction in output, we find that the survival rate of offshorers' domestic operations is modestly lower than control group firms, with greater hazard of exit 3-5 years after offshoring.

We check for potential bias from pre-existing trends in this analysis in two ways. First, we plot the trends for both the treatment and control groups for a 13-year window around the offshoring event as suggested by Angrist and Pischke (2009). Overall, trends for the offshorers

and their controls are very similar prior to the offshoring event. For the variables where we find a stark decline (output and employment), the figures show that: (a) the offshoring firms do not show a significant declining trend prior to offshoring; and (b) there is a stark break in trend for offshorers relative to non-offshorers, consistent with changes being triggered by offshoring. In other words, 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 any pre-existing trend effects.

A concern that affects interpretation of our results is the selective nature of this sample of offshorers. Specifically, in the TAA data we observe only those offshoring firms who did not re-absorb their workers within the *same plant* (as plants where all laid-off workers were re-absorbed would not file for TAA). Consequently, it may not be surprising to find *short-run* employment declines in the *particular plants* where layoffs were certified as caused by offshoring.<sup>7</sup>

Therefore, we next focus specifically on potential gains in other domestic parts of the firm (i.e., other than at the affected plants). First, we examine outcomes at the group of plants within an offshoring firm that were not certified by the TAA (which we term a “pseudo-firm”). We find that output, employment, and capital show declines in this aggregate of non-offshoring plants, suggesting that supporting activities in other plants were reduced following offshoring. There is also an increase in capital intensity (but no significant increase in skill intensity); wages and productivity remain unchanged. Second, we address the possibility that potential benefits from offshoring may be transmitted mainly to non-manufacturing activities of the firms by checking the Longitudinal Business Database (LBD), which includes employment and payroll information on all U.S. establishments in all sectors. Consistent with the baseline analysis, we find a significant decline in firm-level employment, and no significant change in average wage. Thus, we find no

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<sup>7</sup>Importantly for us, firm-level results that we examine (especially our results of the long-run effects), are still relevant for informing models of offshoring discussed above. In particular, models with vertical linkages between offshoring and domestic activity (e.g., Sethupathy, 2013), *do not assume/imply* that workers involved in the offshored activities/tasks will be necessarily absorbed back into the same plant or firm. Even for single establishment firms, potential wage improvements are plausible in these models, in both the short and long-term.

evidence for significant gains in non-manufacturing establishments within offshorers. Altogether, our results imply that offshoring was a substitute for domestic activity in this sample of offshorers.

How do different firm characteristics alter the effects of offshoring? We find that the decline in domestic activity (and concomitant increase in capital intensity) was most pronounced for firms in the bottom third of production worker wages, capital intensity, and labor productivity; these firms appear to have used offshoring to substitute out relatively labor intensive, low wage and low productivity tasks to offshore locations. To check for potential complementarity in vertically-related activities, we examined a sub-sample where the activity at the offshored plant was in a “supplier” industry to the remaining plants, per the Input-Output tables. However, we find no significant difference in results for this sub-sample. We interpret this as suggesting that, as documented by Atalay, Hortaçsu, and Syverson (2014) (using Commodity Flow Survey data for the U.S.) 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.

While our focus so far was to understand domestic (U.S.) outcomes for offshorers, an interesting extension is the impact of offshoring on global activity levels and profits. We explore this using a sub-sample of offshoring firms that we match to Compustat, a database derived from annual reports of publicly listed firms. We find no systematic post-offshoring changes in market value, global sales, employment, or profits, relative to matched controls, consistent with a diversion of activity from U.S. to international operations. A before-after analysis using a sub-set of listed TAA firms matched to Census micro-data indeed reveals a ramp-up in international (non-U.S.) employment in offshorers, both prior to, and after offshoring, offsetting the decline in U.S. employment. The diverging trends for domestic and international employment we find are consistent with our interpretation that offshoring was a substitute for domestic activity in this large sample of offshorers.

Our paper contributes to the empirical literature that studies effects of offshoring, particularly its effects on domestic employment, which falls into two broad camps. One common approach to measure offshoring is to use the industry-level share of imported inputs (identified using input-

output tables) as a proxy for offshoring activity (Amiti and Wei 2005, Morissette and Johnson 2007, Amiti and Wei 2009, Koller and Stehrer 2010). Results in this literature have been mixed.<sup>8</sup> Such a measure can also be constructed for firm-level data, when information on firm-level imports is available. An indicator variable for whether plants imported inputs was 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 collecting this data systematically after 1992.<sup>9</sup> A potential limitation of this approach 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 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; the fraction of imported inputs may even decline if the offshored activity used some such inputs. Our data allows us to identify individual, independently-certified offshoring events, avoiding these sources of potential measurement error.

A second source used to study 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 operational information at the establishment level, including employment, wages, and location.<sup>10</sup> Brainard and Riker (2001) and Borga (2005) find little substitution, but Hanson, Mataloni, and Slaughter (2005) find stronger substitution between home and foreign affiliate employment. Harrison and McMillan (2011) find that while overall offshoring substitutes for domestic employment, for firms that do significantly different (similar) tasks at home and abroad, foreign and domestic employment are complements (substitutes). Using BEA and Current Population Survey data, 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

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<sup>8</sup>See an earlier working paper version Monarch, Park and Sivadasan 2014 for a more detailed review of the findings in this literature.

<sup>9</sup>A sub-sample of establishments were asked this question in the 2007 Census, and used in interesting related work by Fort (2015) who investigates the determinants of production fragmentation. Similar studies have used micro data of other countries: e.g., Hummels et al. (2014) use Danish employer-employee matched data linked to trade shipments data, and 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.

<sup>10</sup>We compare our sample to this BEA data in Section 2.3.

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.<sup>11</sup> A potential 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 the replacement of domestic tasks with foreign tasks).<sup>12</sup> The nature of the TAA program and the scheme for classifying causes of job losses allows us to include events of production shifting abroad irrespective of whether it was within-firm or to outside parties, while excluding any outbound investments not related to production shifting.

## 2 Data & TAA Background

We use three main sets of data in our analysis: Trade Adjustment Assistance (TAA) petition data to provide offshoring-related layoff events from 1999 to 2006; the U.S. Census Bureau's Annual Survey of Manufactures/Census of Manufactures (ASM/CMF) that contains detailed information for manufacturing establishments; and the U.S. Census Bureau's Longitudinal Business Database (LBD), with basic operational information for all establishments in the U.S. This section describes each dataset, how they are merged, and the representativeness of the resulting sample of offshoring firms.

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<sup>11</sup>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.

<sup>12</sup>Desai, Hines and Foley (2009) who describe their work as investigating the effect of foreign investments broadly (rather than offshoring specifically) 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 (e.g., Feldstein 1995, Arndt, Buch, and Schnitzer 2010).



## 2.1 Data Sources

### 2.1.1 TAA Petition Data

The information on trade-induced layoffs in U.S. manufacturing plants is obtained from 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, and TAA certification applies to layoffs at the particular plant. 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 more recently, companies have been the main source of petitions: between 1999 and 2006 (our sample period), 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 is required to complete 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 plant, 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, within one year from the separation date.

Once filed, each petition is assigned an investigator from USDOL, who conducts interviews at the petitioning plant, upstream/downstream plants, and with customers to identify the reason for layoffs. TAA certification of a “trade-related layoff” is granted if the investigator identifies the reason to be one of the following: (i) *company imports* (the company 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).<sup>13</sup> 45% of petitions in our sample period are denied, as they were deemed not trade-related. Decisions made on TAA petitions are published in the Federal Register and on the

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<sup>13</sup>Sample USDOL investigation reports for each classification are available in Appendix A.4. The last category 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.

USDOL website.

Upon certification, 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- firms for which a petition is certified do not receive any form of assistance. The benefits, summarized in Appendix Table A1 (taken from Park (2012)), include job training, 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 related to 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 those certified due to company imports or production shifts (criteria (i) and (iii) above). The layoffs in these events reflect a company decision, indicating a strategic move to relocate activity abroad. Import-competition events, instead, are those driven by forces outside the company (categories (ii) and (iv) above). In cases of multiple petitions, we give priority to the first offshoring event, meaning that if a plant is certified for import-related reasons in 2001, for an offshoring-related reason in 2003, denied in 2004, and certified for offshoring again in 2005, we use the 2003 event of offshoring. More details on how we handle firms with multiple applications are provided in Appendix Section A.2; we check robustness of our results to excluding multiple petition filers in Section 4.4.

The bulk of the petition data we use was procured through a Freedom of Information Act (FOIA) request; this was then complemented with manual data collection from TAA websites.<sup>14</sup>

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<sup>14</sup>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.

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 classified a total of 19,603 petitions with impact years from 1999 to 2006.<sup>15</sup> As a first pass before linking to more detailed information, Appendix 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 some well-known struggling firms). Throughout our analysis, we check whether firms in the sample have pre-offshoring trends that are different from their control group.

### **2.1.2 Economic Census and Survey Data**

We link the information on layoff events from the TAA petition data to two sets of confidential microdata from the U.S. Census Bureau. The first is manufacturing data from the Economic Census (conducted every five years) and supplementary annual surveys for manufacturing plants and firms. The Census of Manufactures (CMF) is conducted in years ending in 2 or 7, and covers nearly all U.S. manufacturing establishments.<sup>16</sup> A rich set of variables are collected, including employment in production and non-production work, payroll in production and non-production workers, total value of shipments (output), value added, material costs, fixed assets, and investment. For between-Census years, a similar set of information is collected in the Annual Survey of Manufactures (ASM) for a representative sample of manufacturing establishments. The sampling probability is based on

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<sup>15</sup>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, totaling 19,603. Appendix Table A3 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).

<sup>16</sup>The smallest establishments in an industry are not sent official Census forms, and have information based on administrative record information collected in the Business Register; following the practice in the literature (e.g., Atalay, Hortaçsu, and Syverson 2014) we dropped all “Administrative Records” (AR) establishments from all of our analysis. As Atalay et al (2014) note, while AR establishments constitute about one-third of establishments in the CMF, their small size means their shares of industry-level output and employment are much lower.

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. Later, we show that a majority of offshoring firms appear to be engaged predominantly in manufacturing (based on establishment counts in the ASM/CMF), and are likely to be disproportionately sampled in the ASM due to their size.<sup>17</sup>

### 2.1.3 Longitudinal Business Database

The second dataset we use is a major refinement of the Business Register, the Longitudinal Business Database (LBD). As described in Jarmin and Miranda (2002), the LBD provides data on all private, non-farm U.S. establishments in existence that have at least one paid employee, including non-manufacturing establishments. The LBD contains annual information on total employment, total payroll, industry, location, and also the birth and exit year for each establishment, which can be used to construct establishment or firm age.<sup>18</sup> Although the set of variables is more limited than the Economic Census data, the full coverage of establishments, and availability of firm ownership linkages, make the LBD an important part of our analysis for forming matched controls (Section 3.1), and for checking robustness of our analysis (Section 5.1.2).

## 2.2 TAA Merge 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 then connect these matches to the manufacturing data described above. We provide full details on the merging process in the Data Appendix.

Next, using the firm identification codes available in both the LBD and the ASM/CMF,

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<sup>17</sup>Our ASM/CMF sample size is 64% of the LBD sample (discussed below); 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 5.1.2, we check robustness of our employment and wage analysis using the Longitudinal Business Database (LBD); in Section 4.4 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 4.4.

<sup>18</sup>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).

we aggregate establishments to the firm-level.<sup>19</sup> We undertake analysis at the firm 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 the residual of 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.

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.

## 2.3 Sample Description

Figure 1 and Table 1 present a number of different measures to illustrate how the sample of TAA offshoring firms lines up with the overall population of firms or multinationals. Figure 1 compares the distribution of industries in our sample of manufacturing offshorers (at the impact

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<sup>19</sup>As discussed above, some firms experienced multiple offshoring events during the observation period, and for such cases we use the first recorded event. 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.

date) to the distribution of industries among all manufacturing firms in 2002, and to the distribution of industries in the 2004 benchmark sample of U.S. multinational corporations in manufacturing gathered by the Bureau of Economic Analysis.<sup>20</sup> The distributions are similar overall, though some key differences are informative and make intuitive sense. The TAA sample is under-represented relative to manufacturing firms in the Economic Census in food, lumber, metal and machinery, which are either raw materials or primary inputs that are less likely to be offshored. TAA firms are over-represented in textiles, apparel, and electronics, which may be intermediate or final goods whose production may be more easily shifted abroad. The BEA data is often used in the literature to study the effects of offshoring, as it tracks international affiliate activity.<sup>21</sup> However, arm's-length offshoring, which is more likely to occur in simpler or less relationship-specific products, would not be included in this data, while TAA offshoring could be. Consistent with this reasoning, our sample tends to have slightly more textile and apparel firms compared to overall U.S. multinational activity, which is more concentrated in chemicals, machinery and computers. Even so, the composition of industries in our sample is not markedly different from the BEA data, and hence unlikely to be a source of distinctiveness for our results; nevertheless, in all of our analysis, we are careful to match to control firms from within the industry and/or condition on industry-period fixed effects.

Table 1 presents four other comparisons of our offshoring sample to the population of firms. The top panel shows that the size distribution (measured in employment bins) for offshorers tilts much more to the right – over half of our sample has over 100 employees, compared to about 2% in the LBD. Regionally, the spread is fairly homogeneous; offshorers are over-represented in the East, and under-represented in the West relative to the universe of U.S. firms. The age distribution of offshorers also skews older than the overall LBD, with 66% of all offshorers 11 or more years old, compared to 40% in the LBD. Relying on export information from the CMF, 86% of offshorers export in their impact year, compared to 21.5% of manufacturing firms in 2002. We find these tables as strong evidence that the sample of offshorers who file TAA petitions are more likely to be larger, older, and exporting than the overall population, and hence likely to be more productive than average firms.

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<sup>20</sup>The sample size of U.S. manufacturing multinational corporations in 2004 is 1,168.

<sup>21</sup>See for example, Harrison and McMillan (2011).

Finally, although the sample of firms classified in the TAA is small in number (with about 1000 firms in the manufacturing sample), because they are skewed towards much larger firms, they occupy nontrivial shares of total manufacturing employment and for total employment losses during this time. Table 2 shows that from 1998-2006, firms in our sample (with various impact years) accounted for about 12% of total employment in manufacturing. Furthermore, we measure one-year employment declines among those offshorers who reduced employment or exited (after their impact year), and compare them to total losses among all firms who reduced employment or exited in Table 2.<sup>22</sup> For example, manufacturing firms that were shrinking/exiting from 2002-2003 shed about 2.01 million jobs, of which shrinking/exiting firms classified as offshorers using TAA petitions accounted for 174,000, or 8.6%. In general, employment declines among TAA offshorers in our time frame accounted for 8-11% of total employment declines during this time period, reaching a peak of 11.8% from 2003-2004. These statistics demonstrate that our sample provides a window into a substantial proportion of employment, and that our sample of TAA-certified offshoring firms were a notable part of the remarkable decline in manufacturing that characterized our period of study (Autor, Dorn and Hanson 2013).

**Comparison by Petition Type:** How do offshorers differ from other TAA petitioners? We find that offshorers are significantly bigger (higher mean log employment and payroll) than firms classified as import-competing, and both tend to be bigger than those firms with denied petitions in the LBD sample (see Appendix Table D1). In the manufacturing sample, while all three petition types are bigger, older, and more likely to export than ordinary firms, offshorers exhibit a distinct premium on these variables relative to import-competing firm and denied petitioners. Firms certified as import-competing are closer to denied petitioners on these variables. Thus offshorers appear to be systematically different (bigger and older) than other petitioners.

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<sup>22</sup>The majority of offshorers in our sample- typically about 80%- reduce employment or exit year-to-year. The corresponding figure for all manufacturing firms is about 20%.

## 2.4 Cross-Sectional Comparison of Offshorers and Non-offshorers

How different are TAA-certified offshoring firms from other firms prior to offshoring? We present a basic comparison in firm characteristics, 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) firms 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 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 3. Our sample of offshorers exhibit premia consistent with what would be expected in a heterogeneous firm model with fixed costs for offshoring (such as the model presented in Appendix B.1). Specifically, lower marginal costs of production need to offset the fixed cost, which implies that only firms with a sufficiently large size find it profitable to offshore. Indeed, we find that 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 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 that our sample is biased towards weak and struggling firms. The final column shows how these outcomes compare when also conditioning on firm employment. Even when grouping similarly-sized firms together, offshorers still are older and more likely to export than other firms in the same industry.



## 3 Empirical Methodology

### 3.1 Industry-Size Matched Treatment Groups

The main challenge to estimating the effects of offshoring is potential endogeneity of the offshoring decision. A 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, increases in local input costs, e.g., an increase in wages, could lead to favorable conditions for offshoring. At the same time, higher 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 primarily be 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 2.4 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 a version of ‘nearest neighbor’ matching, choosing two controls close in employment (the next bigger and next smaller) to each offshored firm, one year prior to the impact year. Controls are picked from the same 3-digit industry using the LBD.<sup>23</sup> The matching creates one “cell” for each offshorer, comprising the offshorer and up to two matched controls; in the analysis the controls are assigned the same offshoring impact year as the offshorer firm in the cell. We then merge this sample of treated and control firms to the detailed data in the ASM/CMF, going back to 6 years prior to the impact year and 6 years following.

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 and to present tests of short and long-run changes (and pre-trends), we collapse the thirteen periods into

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<sup>23</sup>We impose a restriction that log employment at one of these ‘nearest neighbors’ cannot be more than 0.5 log points different from the comparison offshorer, meaning that not every offshorer is paired with exactly two controls. A small number of duplicate controls are dropped, each offshorer is matched to distinct controls.

four groups, two three-year periods prior and two three-year periods after the offshoring event. We then estimate the following regression specification:

$$y_{ijt} = \beta_0 + (\alpha_{LR\_PRE} + \delta_i \beta_{LR\_PRE}) LR\_PRE_{ijt} + (\alpha_{SR\_PRE} + \delta_i \beta_{SR\_PRE}) SR\_PRE_{ijt} + (\alpha_{SR\_POST} + \delta_i \beta_{SR\_POST}) SR\_POST_{ijt} + (\alpha_{LR\_POST} + \delta_i \beta_{LR\_POST}) LR\_POST_{ijt} + f_i + e_{ijt} \quad (1)$$

Above,  $y_{ijt}$  is an outcome variable for firm  $i$  belonging to a cell  $j$  observed at event year time  $t$ . Event year time  $t$  varies from -6 to +6 over the 13 year window.  $LR\_PRE$  ( $SR\_PRE$ ) is a dummy equal to 1 for the 4 to 6 (1 to 3) year period prior to the offshoring impact year,  $LR\_POST$  ( $SR\_POST$ ) is a dummy equal to 1 for the 4 to 6 (1 to 3) year period after the offshoring impact year,  $\delta_i$  is an indicator dummy for offshorers, and  $f_i$  are firm fixed effects. (As a robustness check, in Section 4.4, we run regressions with cell-period effects ( $f_{jk}$ ), which allows for flexible industry-size-period specific shocks.) Thus, the  $\beta$  coefficients are period-specific means for offshorers relative to controls. The actual impact year is the excluded period. DID estimates of the long-run (short-run) effect of offshoring is the difference  $\beta_{LR\_POST} - \beta_{SR\_PRE}$  ( $\beta_{SR\_POST} - \beta_{SR\_PRE}$ ). The difference  $\beta_{SR\_PRE} - \beta_{LR\_PRE}$  provides a test for pre-existing trend. (A stand-alone offshorer dummy  $\delta_i$  is not included as it gets absorbed by firm fixed effects.)

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:

$$y_{ijt} = \gamma_0 + \sum_{t=-6}^6 (\alpha_t + \beta_t \delta_i) D_{jt} + f_i + e_{ijt} \quad (2)$$

where  $D_{jt}$  is a dummy equal to one if the year is  $t$  years from the offshoring (impact) year for cell  $j$  (with  $t \in [-6, 6]$ ),  $f_i$  stands for firm fixed effects, and  $\delta_i$  is an indicator for an offshoring firm. In this case,  $\alpha_t$  provides the trend for the matched controls, and  $(\beta_t + \alpha_t)$  provides the trend for the offshorers. Therefore,  $\beta_t$  captures the impact of offshoring  $t$  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) offshoring firms and their control group to have similar trends

before the offshoring event, and (b) a clear break in trend at the offshoring impact year for offshorers. Standard-errors are clustered by matched offshorer-controls cells throughout. We use the year prior to the impact year ( $t = -1$ ) as the reference (omitted) year. Note that in the omitted year, estimates of outcome variables will coincide for 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.

### 3.2 Propensity Score-Matched Treatment Groups

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. Any post-offshoring effects driven by the interaction of firm characteristics included in the propensity-to-offshore 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:

$$Offshore_{ijt} = \beta X_{ijt} + f(\cdot) + \varepsilon_{ijt} \quad (3)$$

where  $Offshore_{ijt}$  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, and  $f$  is a flexible specification of different fixed effects. Unlike for the employment matching, in order to include rich set of firm characteristics, we undertake the propensity score matching in the ASM-CMF sample. Based on the ex-ante differences documented in Table 3, we include as predictors ( $X$ ) combinations of employment, non-production wage, production wage, capital-labor ratio, share of non-production wages in total payroll, labor productivity, exporting status, and firm age, while we try industry, size (in deciles), industry-year, industry-year-size, and industry-year-size-age fixed effects. We present results from our main specifications in Table 4. Our preferred specification includes the whole vector of firm characteristics

described, along with industry-size-year-age fixed effects (column 5). Consistent with the findings in Section 2.4, 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 and non-production wage rate are negatively correlated with offshoring. Older, exporting firms are also more likely to offshore.<sup>24</sup>

Next, using the predicted propensity from the specification in column (5) of Table 4, 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. We then estimate Equations (1) and (2) in the same way as above using these new treatment cells.

For both the employment-matched and propensity score-matched difference-in-differences analysis, potential differential rates of exit for offshorers relative to controls (analyzed in Section 4.3 below) might impact estimates; in Section 4.4 below, we check and confirm robustness of all of our results to using a balanced panel of firms.

## 4 Results

### 4.1 Industry-Size-Matched Treatment Groups

**Size and Wage Measures** The top rows of Table 5 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 27.8%, in value

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added is 0.391 log points or 32.36%, in employment is 0.380 log points or 31.61%, and in capital is 0.253 log points or 22.4%.<sup>25</sup> Finally, in the last column, we 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 cell. 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 four years after 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 noteworthy; there is no evidence that firms where workers qualified for TAA assistance were 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 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

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<sup>25</sup>Negative log points  $\beta$  translate to percentage changes as  $\frac{e^A - e^{A-\beta}}{e^A} = 1 - e^{-\beta}$ .

trends for offshorers and the control sample, 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.

**Factor Intensity and Productivity Measures** Returning to Table 5, offshorers also became 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), stemming from 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 output per worker variable shows improvement in both short- and long-run periods after offshoring, this is not true for value added per worker, or for any of the total factor productivity (TFP) measures. Sub-figures (e) and (f) of Figure 2 present output per worker and value-added per worker figures that line up with these results. The results are consistent with the larger decline in employment relative to output and capital, but a similar long-run decline in employment relative to value added.

## 4.2 Propensity Score-Matched Treatment Groups

Table 6 presents results from DID estimation using propensity matched controls from the ASM/CMF. The results are qualitatively identical to the estimation with employment-matched controls shown in Table 5. 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 on wage rates is 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 event study figures using propensity matching are very similar to that for employment matching.<sup>26</sup>

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.

### 4.3 Firm Survival

If offshoring is beneficial to the domestic activities of the firm, one potential consequence could be that domestic activities of the offshoring firms will be more likely to survive than peer firms in the industry. To study this possibility, first, we examine firm survival using aggregate data on offshoring plants and firms. 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)) still in existence in the indicated event-year, relative to employment-matched controls formed using the LBD data. The benchmark year is the year prior to the impact year, and therefore has a value of 100% for both offshorers and control firms.<sup>27</sup> The graphs indicate that offshoring plants close down faster, and in the long-run offshoring firms exit slightly faster than comparable control firms, though the survival rate at the end of the six year period is not much different between offshorers and matched controls.

We further investigate survival patterns over the six years after offshoring. We retain the

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<sup>26</sup>Figures for size variables are presented in Appendix Figure D1. Figures for other variables are omitted for brevity; they are qualitatively similar to baseline figures, and confirm the regression results in Table 6.

<sup>27</sup>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.

cross-section of data on offshorers and matched controls for the year before offshoring, and use the following regression specification:

$$Dk_i = \beta_k D_i^{\text{off}} + f(.) + e_i$$

where  $Dk_i$  is a dummy =1 if firm  $i$  survives beyond  $k$  years after the offshoring year (imputed to be the offshoring year of the matched offshorer for control firms),  $D_i^{\text{off}}$  is a dummy =1 if firm  $i$  is an offshorer, and  $f(.)$  denotes fixed effects. One strength of the regression model is that it allows for the use of detailed fixed effects (including matched cell fixed effects); a drawback is right censoring of survival data; e.g.,  $D6_i$  is not defined for firms (and matched controls) that offshore in year 2002, as our data ends in 2008. Right censoring is more appropriately handled using a hazard model (which for practical computational reasons limit use of detailed fixed effects). We consider two alternative models: exponential (or constant hazard), and Weibull (which allows for a more flexible hazard function specification).

Results are reported in Table 7. In Panel A, we define a firm as having exited in year  $k$  if the firm identifier is no longer in the LBD in after year  $k$ . As an alternative, in Panel B, we define a firm as having exited when the last surviving plant from among all plants in the year before offshoring exits the LBD.<sup>28</sup> Both alternative approaches yields results broadly consistent with Figure 3. In particular, offshorers are more likely to survive in the short-term, but have higher exit rates 3-5 years after offshoring. Hazard function results in columns 13 to 16 confirm the overall finding that exit happens somewhat quicker for offshorers relative to non-offshorers. By year 6, the difference in survival between offshorers and controls is smaller (only 3% and marginally statistically significant in column 12, row 1).

Thus we conclude that offshoring does not improve the long-term survivability of offshorers, and in fact we find some evidence that offshorers exit U.S. domestic operations faster than matched

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<sup>28</sup>We thank a referee for prompting this additional analysis, and alternative definitions for firm exit. Both definitions have pros and cons. In some cases, firm identifier may change for reasons not related to firm exit; e.g., when a single unit firm becomes a multi-unit firm or vice versa (as the firm identifiers are defined systematically differently by single/multi-unit status). This induces measurement error in the first definition. On the other hand, if a firm exits by selling off some of its plants and closing others, the latter definition will not capture this exit (if some of the sold plants remain open). In practice the errors may not be severe; nevertheless, the strong qualitative consistency of our results across the two approaches is reassuring. For the baseline analysis of other outcomes, we check robustness (in Section 4.4) to using a balanced panel, which implicitly excludes spurious changes in firm identifier such as that from conversion from a multi-unit to single-unit firm.



controls.

#### 4.4 Robustness Checks of Baseline Results

We undertake a number of robustness checks of the baseline results, which we describe briefly below.<sup>29</sup>

**Estimation using Cell-Year Fixed Effects** Our baseline regression specifications use firm fixed effects and period effects; while this controls for all firm-specific effects, any 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 one offshorer and (up to) two matched controls. Overall, these results (presented in Appendix Table D2) 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. Further, because cell-years with either only controls or only offshoring firms do not contribute to the estimated coefficients on  $\text{period} \times \text{Offshorer}$  dummies, this analysis implicitly avoids contributions from differential exits by controls or offshorers; i.e., these results reflect changes between offshorers and non-offshorers in cell-years where at least one control and one offshorer exist.

**Alternative Refinements of Offshoring Events** Our focus in the analysis is on short-run and long-run effects following what we know to be the first documented instance of offshoring.<sup>30</sup> As noted earlier, a number of offshorers in our matched sample filed multiple TAA applications; these multiple petitioners constitute about 50% of our sample. While we view subsequent petitions as a valid potential outcome that should not be excluded from the baseline analysis, it is nevertheless interesting to check if short and long-run outcomes are significantly different if we restrict our analysis to firms with a single offshoring event. Results using only the sample of firms that filed a single offshoring petition in the sample period are presented in Appendix Table D3. While the

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<sup>29</sup>Results in this section are either reported in Appendix D, or available on request.

<sup>30</sup>We thank a referee for suggesting this discussion and some of the analysis.

short-run effects are very similar, we do find somewhat smaller long-run declines size measures in this sample. However the differences are not large; the baseline long-run decline in employment-matched (propensity) DID was 0.38 (0.31) log points, while in this sample it is 0.33 (0.31) log points. A related concern is whether there were a significant fraction of firms that filed prior to 1999 (under the TAA petition regime where the sub-categories were not reported). Using data on TAA applications for the 1974 to 1998 period, we find that only 13% of the offshorers in our sample have any TAA filings in the pre-1999 period. Nevertheless, we check robustness of the baseline results to excluding firms that had applied to TAA before our sample period (by using data on all TAA applicants in the 1979 to 1998), in order to focus on true first instances of offshoring, as firms with prior petitions may have initiated offshoring earlier. Results in Appendix Table D4 are qualitatively very consistent with the baseline results.

**Multi- and Single-unit Firm Samples** We analyze the impact of offshoring using only multi-unit firms. This addresses two concerns: one is that single unit firms may be focused on one narrow activity, so that the negative direct effect of offshoring on employment they generate may drive the results, while multi-unit firms have other domestic activity where potential positive effects may be better captured. Two, 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 5.1.2 below.) The results for multi-unit firms (in Appendix Table D5) are qualitatively identical to our baseline analysis of all firms, which is not surprising since close to 80% of our ASM/CMF sample is multi-unit. Appendix Table D6 also shows similar results for the much smaller sample of single-unit firms, though the pre-event matching is less exact.

**Balanced Panel Analysis** 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 discussed above, 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) in Appendix Table D7. 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.) This analysis also addresses a concern related to potential spurious changes in the firm identifier discussed in Section 4.3.

**Pre-2002/Post-2001 Split** We examine results separately for offshoring events before and after 2002 for two reasons. One, it is interesting to see if offshoring effects exhibit heterogeneity over time, if e.g., the underlying drivers or nature of offshorers varied over time. Two, this helps check robustness to potential measurement error for industry codes.<sup>31</sup> We match firms with offshoring events from 1999 to 2006 to controls that had the same SIC-3 industry in the year prior to offshoring, using industry classifications available in the LBD. The industry classification in the LBD transitioned to the North American Industrial Classification System (NAICS) after 2001. This fortunately has only a modest impact on our analysis because the vast majority of firms in our offshoring sample were in existence before 2002, and hence we were able to impute SIC classifications using pre-2002 data. Nevertheless, if some firms changed industries after 2001, a concern with our matching procedure could be the potential mis-measurement of industry classification after 2001. The results (in Appendix Tables D8 and D9) show remarkably consistent results across pre-2002 and post-2001 sub-samples.

**Using All Firms as Controls** While our results are robust across two alternative matching procedures, we wanted to see if the results would be notably different if we used a simple traditional difference-in-differences approach, with panel regressions using all firms in the data. This addresses concerns ever whether the nature of the matching process (e.g., the modified nearest neighbor approach we use) impact the analysis.<sup>32</sup> To allow for flexibility in period effects, while managing computational tractability given large set of fixed two-dimensional fixed effects, we include one-digit industry-year and firm fixed effects. The results (in Appendix Table D10) are qualitatively very consistent with baseline results. With the broader control group, we see bigger declines in

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<sup>31</sup>We thank a referee for pointing out this concern.

<sup>32</sup>We thank a referee for suggesting this alternative approach.

output (0.37), value added (0.43) and employment (0.47), compared to the baseline results in Table 5. Interestingly, there is no relative trend in most of the variables in the pre-offshoring periods, so that declines in size measures reflect a strong break in pattern. Wage changes are small and generally insignificant; capital intensity increases significantly, and skill intensity measures modestly. Productivity changes are small, and of mixed signs and significance. This confirms that the baseline results reflect true underlying changes within offshorers, and are not influenced by control group choice; this robustness is indeed consistent with the patterns in Figure 2 where it is clear that the observed size effects are driven by before-after changes in the offshoring firms rather than by potentially spurious changes within control firms.

**Other Robustness Checks** We also attempted a series of additional checks of our results. Results from an earlier analysis using an alternative propensity model are consistent with our baseline results D12. We tried alternative methods for aggregating TFP, and 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 (which addresses Akerberg, Caves, Fraser’s (2015) critique of the Levinsohn-Petrin approach). Finally, we performed a number of concurrent combinations of these checks (e.g., multi-unit firms in a balanced panel). Our baseline results remain robust to using these alternative specifications and definitions. Finally, following Pierce and Schott (2013) and together with firm employment, we instrument for the offshoring decision using the “Normal Trade Relations” variable in Appendix C. The declines in firm size accompanying the second-stage offshoring events are comparable to our earlier results.

## 5 Extensions

### 5.1 Changes to “remaining” domestic activities

#### 5.1.1 Pseudo-Firm: Non-offshoring Plants in Multi-unit Firms

We next try to disentangle the effects of offshoring within a firm by examining the non-offshoring plants of 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.<sup>33</sup> 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 and their controls drops to 12,400 (10,900 for propensity analysis), compared to 22,500 (19,200) in the original sample.

From the results in Table 8, 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-run 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 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 remaining units show a significant decline in output and employment, which is consistent with elimination of supporting activities in remaining units following offshoring. Thus our results suggest no complementarity between offshoring and activity at remaining domestic establishments.

### 5.1.2 Analysis using LBD Data

A key goal of our analysis is to see whether purely domestic activity in the U.S. benefitted from offshoring of certain activities (as in e.g., Sethupathy 2013). However, our baseline analysis using manufacturing data could be missing an important channel for gains. In particular, it could be the case that employment gains from offshoring are realized in non-manufacturing establishments of the firm; for example, 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

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<sup>33</sup>As discussed in Section 2.1.1, TAA petitions are filed at the plant level. We match the TAA data is by plant name and state; thus for multi-plant firms with plants in multiple states, the TAA event is matched to the plant in the specific state indicated on the TAA petition filing. E.g., if plant X located in Texas of a multi-location Firm A files for TAA assistance, we identify that particular plant X as the affected plant; then all other plants of firm A located in other states are “unaffected” plants used to create the pseudo-firm aggregate. Total employment and firm industry are reconstructed using these non-TAA 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.

arm. Because the LBD data includes data on headquarters as well as marketing (wholesale and retail trade) establishments, using this data allows us to examine domestic firm-level aggregates that includes these units.

Further, examining the LBD allows us to check robustness to potential bias from sampling in the ASM. 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.

In Table 9, we estimate Equation (1) using the LBD sample. 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, we use wage rate, 3-year employment growth rate, 3-year wage growth rate, and employment as our explanatory variables.

As seen in the top panel of Table 9, 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 5), the magnitude of decline in the propensity matched approach (-0.366) is close to that of the baseline (-0.32 log points in Table 6). 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. These results suggest: (i) no significant net employment gains in domestic activities, even including headquarters and marketing units; and (ii) the baseline findings for size (employment) and wages are not significantly impacted by loss of data from sampling in the ASM. (As discussed in Section 2.2, the robustness of the baseline results is not very surprising, given the large degree of overlap between the ASM/CMF and LBD samples.)

To further confirm that there were no notable gains in all other non-TAA establishments (including non-manufacturing establishments), we examined a “pseudo-firm” aggregate using LBD data. We find results (Appendix Table D13) similar to those in Section 5.1.1: a negative effect on employment and payroll, and generally insignificant effect on wages.

## 5.2 Exploring Heterogeneity: Effect of Initial Characteristics on Outcomes

**Firm Size, Wages and Capital Intensity** The baseline analysis documents that offshorers experience (a) significant decline in size measures, (b) increase in capital and skill intensity, and (c) no significant changes in wage rates or productivity. Here we explore whether the magnitudes of these results vary by initial firm characteristics. Results are reported in Table 10 for employment-matched difference-in-differences (propensity score-matched results are reported in Appendix Table D14). We examined differences by initial firm size (employment), production worker wage rate, capital intensity, labor productivity and export status. The most striking results are that size declines are most pronounced for firms with low initial production worker wage rate, low initial capital intensity and low initial labor productivity. Capital and skill intensity increases were also the most pronounced for these firms. It appears that firms adopting a relatively low wage/low capital intensity/low labor productivity production strategy were more likely to significantly shrink domestic activity following offshoring. This is unsurprising: offshore production, particularly in low wage destinations, is likely to be a stronger substitute for activity in lower wage, less capital intensive U.S. manufacturing plants.<sup>34</sup>

One explanation for the lack of complementarity, as discussed in Appendix Section B.2, would be shifting of export related activity abroad (termed “export-platform FDI” by Harrison and McMillan, 2011). As a potential test for this channel, we examine differences between exporting and non-exporting offshorers in the last row of Table 10. However, we find no significant differences in outcome changes after offshoring between exporters and non-exporters, suggesting that export

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<sup>34</sup>In an earlier version of the offshoring propensity model, where we used past employment and wage growth (Appendix Table D11) with a smaller set of controls, there was evidence that offshoring was correlated negatively with past wage growth, but past employment growth was not significant. That is, those results were consistent with offshoring firms having tried to keep wages down in the US (without cutting employment), and then resorting to offshoring.

platform FDI may not be playing a big role in observed post-offshoring declines in domestic activity.

**Vertical Linkages** As discussed in Appendix B, the vertical supply links between offshored plants and 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 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. Similar to the procedure outlined in Atalay, Hortaçsu, 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.<sup>35</sup> 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 11 summarizes the estimation results. While the reduction in sample size increases the standard errors of the estimated coefficients, the magnitudes of the short and long-run size declines, as well as the results for other measures, are very similar to the ones we find in baseline full sample specifications.<sup>36</sup>

We interpret these findings as suggesting that linkages measured using Input-Output tables do not necessarily translate to actual vertical intra-firm shipments, in line with findings of two recent papers. Using U.S. Commodity Flow Survey data, Atalay, Hortaçsu, 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, Rappoport, and Ruhl (2014) look at the cross-border intra-firm shipment of U.S. multinationals using the BEA data.

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<sup>35</sup>Results do not change by including or excluding the “reflexive” case, where an industry is defined as vertically linked to itself.

<sup>36</sup>The event-study graphs for employment and output at vertically linked firms are in Figure D2.



They find that while most multinationals display vertical linkages per the I/O tables, there are few intra-firm shipments; the majority of output from foreign subsidiaries are sold locally and the median subsidiary reports no shipment to the U.S. parent. Both studies attribute the identifiable vertical links among establishments without actual shipments to knowledge capital usable across the vertical chain.

This analysis, and the studies cited above, suggest a plausible explanation for 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 Appendix B.2, rather than as in the model for vertical FDI sketched in Appendix B.1.

### 5.3 Changes to Global Outcomes: TAA-Compustat-CRSP Merged Data

The main focus of this paper is on understanding the effect of offshoring on domestic operations of a firm; indeed this has been a key focus of a number of papers in the literature (e.g., Desai et al. 2009, Sethupathy 2013). While we find evidence that offshoring is a strong substitute for domestic operations in our sample, our analysis so far leaves open the question of how offshoring impacts the “global” activities of the firm. In this section, we address this question by linking TAA petition data to Compustat North America, a comprehensive dataset of publicly listed firms operating in the United States. In particular, we examine if offshoring is associated with significant changes to global (i.e., overall) firm employment, revenues, or profitability. To examine effects on market capitalization, we merge data from CRSP’s monthly stock price files.

We match certified TAA petitions to Compustat North America using the company name.<sup>37</sup> Merging of these two datasets yields 267 offshoring events from 1999 to 2003.<sup>38</sup> Construction of the

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<sup>37</sup>With the LBD, we use name and state as matching criteria because both petition and LBD data are at the establishment-level. Compustat is a firm-level data, so the reported addresses are for the headquarter location.

<sup>38</sup>See discussion below and Appendix A.6 for a discussion of Compustat variables. A comparison of TAA-matched firms to all firms in Compustat yield size premia for TAA similar to the premia observed in Table 3 for the Census sample (Appendix Table D15); relative to industry peers, offshorers are larger in employment, revenue, capital and total assets, capital intensive, have higher labor productivity and profitability ratios, have larger stock market value, and are more diversified (active in more industry segments). While the Tobin’s Q - the ratio of market value of all firm liabilities to its book value (e.g., Gompers, Ishii, and Metrick, 2003) - is smaller, this seems to be because of larger size; Q (and ROE) premium reverses when employment is conditioned on.

control group takes the same method as above: we use nearest neighbor matching (employment) within a 3-digit SIC industry in the year prior to the impact year, and use a propensity model similar to that in Section 3.2. For both the employment-matched and propensity analysis, the matching criteria and restricting to a common sample with data on all our key dependent variables leaves us with a sample of 185 offshoring firms.<sup>39</sup> Differences in the datasets precludes using the same set of outcome measures that we used in the Census analysis. While the Census data has more detail for production and non-production worker counts and wages, Compustat has more information on profitability measures. Further, stock price data from CRSP allows us to form an average yearly market value measure (using month-end closing stock prices). We also undertake a simple regression-based survival analysis with post-offshoring data, using a dummy dependent variable indicating survival (measured using the firm CUSIP identifier.)

Results are presented in Table 12. We begin with looking at measures of global size and profitability. In the employment-matched approach, we find small, generally positive but insignificant (except for employment) relative changes to size variables in the short-run after offshoring. In the longer term, there is a relative decline in size measures, though this is not statistically significant in either approach. In the propensity approach, there is a decline in size variables, with larger declines in the long-run, though the DID effects are not statistically significant. Capital intensity shows mixed results with a small positive short-term increase but a negative longer-term effect, in both alternative DID approaches. Profit and productivity measures show small and insignificant changes, both in employment- and propensity score-matched analysis.

Next, we analyze the impact of offshoring on market value in two ways. The first measure we use is log market value of the firm. However, this measure is potentially sensitive to changes in the leverage (debt-equity ratio) of the firm; in particular, the market value can decline (increase) significantly for a firm that increases (decreases) long term debt relative to equity. A more commonly used alternative to assess effects on firm valuation is Tobin's Q.<sup>40</sup> In the employment-matched ap-

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<sup>39</sup>Results of the propensity model estimation are reported in Appendix Table D17. The selection of a control firm is restricted to those whose log employment is within 0.5 log points, and within 0.01 of the propensity score, from the offshorer firm.

<sup>40</sup>Some papers in the literature (e.g., Gompers et. al (2003)) adjust for industry-level Q, by subtracting industry median Q. Because we use industry controls, our DID approach implicitly adjusts for industry level variation.

proach, we find little change in market value and Tobin's Q in the short-run, and mixed effects in the longer term (decline in market value but a small increase in our preferred Q measure). For the propensity approach, consistent with effects for the size variables, there is a decline both in market value and Q. While the long-run market value decline is sizeable and statistically significant, the effect is smaller and noisy for Tobin's Q.

Finally, we examine firm survival. In post-offshoring data, we regress a dummy for firm survival on year-specific effects and interactions of year-specific effects with offshorer status. As in the survival analysis in Table 7, we include matched cell-year fixed effects to control for industry-size/propensity shocks. The coefficients on these interactions are reported in the last row of Table 12. The results indicate a lower survival rate for offshoring firms – about 5 to 7% in the employment-matched and 3 to 4% in the propensity analysis – but the effects are not statistically significant. Examining tests for pre-offshoring trends, both the employment- and propensity approaches show non-significant pre-trends for all variables, indicating general co-movement with the matched controls prior to offshoring.

Overall, the results suggest that: (i) global size measures do not exhibit a sharp statistically significant decline in the short-term, though there is some evidence of downsizing in the longer term; (ii) global capital intensity, labor productivity, and profitability measures show no systematic change around the offshoring date; (iii) there is also no systematic evidence for a strong short-term effect on Tobin's Q, our preferred measure for market value effects, and (iv) some weak evidence of modestly faster exit for offshoring firms.

These conclusions are supported by the event-study figures (for employment-matched analysis) presented in Appendix Figure D3; employment and revenue were on an increasing trend for both offshorers and control firms, and there is a flattening out for both groups after offshoring. Further, pre- and post-event trends are similar for productivity, profits, market value and Tobin's Q between for offshorers and their controls.

Our conclusion is that global employment does not appear to decline after offshoring. Combined with the stark decline we saw for the domestic employment (albeit not for the same sample), there may have been offsetting gains in international (non-U.S.) employment. To directly check

for this, we matched the Compustat-TAA matched sample to the Census microdata using the Compustat-SSEL bridge file. This allows us to impute international (non-U.S.) employment as the difference between global employment reported in Compustat and the domestic firm employment from the LBD. We then examined the before-after changes in employment for domestic, international and global (total) employment for TAA firms. The graph, plotted in Figure 4, confirms that there is indeed a significant ramp-up in overseas employment. Interestingly, the increase in employment abroad precedes the offshoring event, suggesting that offshore employees may be trained and ready before employment is downsized in the U.S. The increase in offshore employment continues after offshoring, and offsets the decrease in domestic employment, so that the overall global employment level flattens out after offshoring.

## 6 Discussion and Conclusion

We use information on the source of trade-related layoffs available in petitions filed under the U.S. Trade Adjustment Assistance program to identify offshoring events, then link them to rich U.S. Census micro datasets, namely the Longitudinal Business Dataset (LBD), Census of Manufactures (CMF), and Annual Survey of Manufactures (ASM). USDOL investigators verify and classify the causes of trade-related layoffs, providing an especially reliable method of separating offshoring activity from other types of job losses. We find that – prior to the onset of offshoring activity – firms from this sample have higher employment, value added, output, and capital stocks than other firms in the same industry, and are also older, more productive and more likely to export.

We examine changes in key outcome variables for our sample of offshorers relative to controls using a standard difference-in-differences (DID) methodology. We find that employment declines significantly at the firm level following the 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 also find that output, value added, and capital drastically decline after offshoring, with little evidence of any significant change in productivity or wages. Firms reduce labor 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.

Importantly, there is no evidence of increases in domestic activity at non-TAA certified plants of offshoring firms; output, employment and capital decline at non-TAA certified plants as well. While our baseline analysis uses only manufacturing sector data, we found no net employment gains even including non-manufacturing establishments using data from the LBD. Interestingly, we find that firms that had low capital intensity, wages and labor productivity – likely to be firms following a low cost strategy – were the ones who shrunk domestic activity the most. This suggests offshoring in our sample is a stronger substitute for low-wage, low-capital intensity domestic activities.

Our findings suggest that the pathway of vertical linkages, crucial for complementarity in some offshoring models, 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 is in line with the findings of Atalay, Hortaçsu, and Syverson (2014) and Ramondo, Rappoport and Ruhl (2014), who find very little evidence of intra-firm shipments (even within firms who have establishments 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 few qualifications are to be noted when interpreting our results. First, as noted in the introduction, 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 we argue that 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 debate on the effect of offshoring on U.S. firms’ domestic operations by providing 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, as noted above, the offshorers in our sample are *ex ante* bigger, more productive, and pay higher wages than the average industry peers, and the pre-offshoring trends in size, wages and productivity are no different than matched control firms. Second, it is important to note that our DID matching procedure has limitations, and is only feasible if the assumptions that underlie the econometric meaning of treatment effect analysis (such as the ignorable treatment assignment assumption and the stable unit treatment value assumption) hold. In other words, our findings are predicated on the assumption that a firm’s status as an offshorer does not affect outcomes at any of its control group firms.

Third, we strongly emphasize that our results *do not* imply negative overall welfare effects from offshoring. Given data limitations, a key channel for potential gains – reduced output prices – are not examined in this paper. Our analysis of Compustat data suggest that offshoring firms experience gains in international employment and sales, so that global sales, employment, and profits are not negatively impacted; thus our results do not imply losses to shareholders of offshoring firms. Potential welfare losses from under-utilization of displaced labor resources would depend on the how long the laid-off workers take to find new jobs, which we cannot address with our data.

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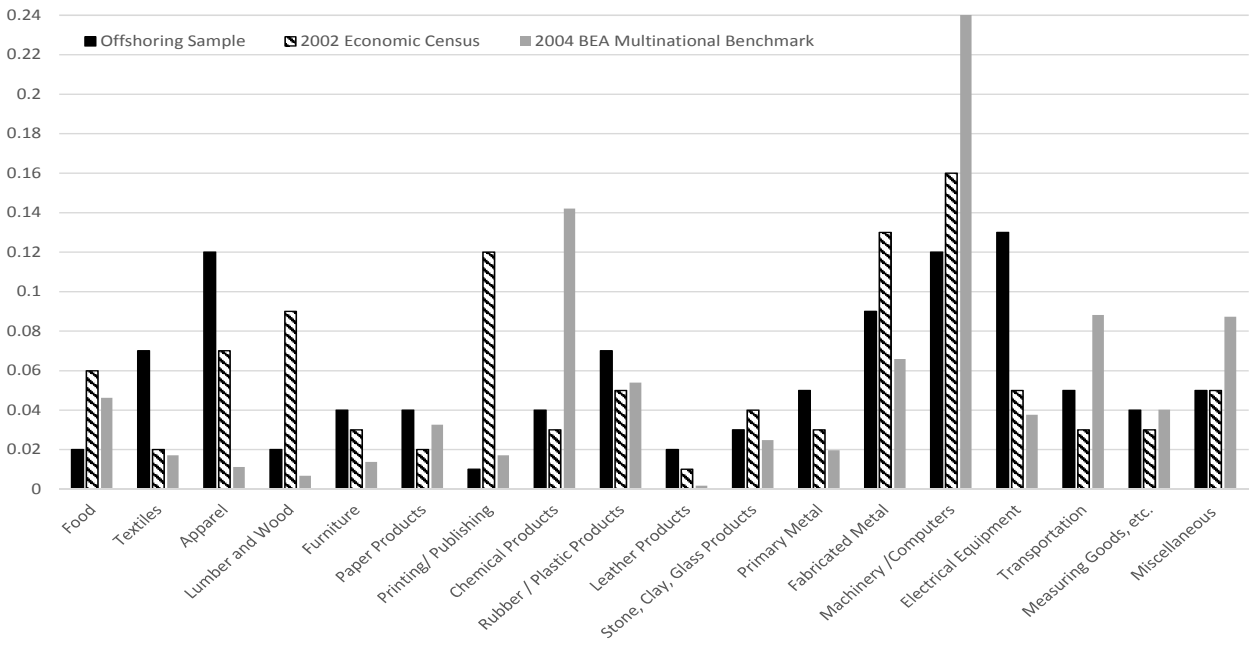
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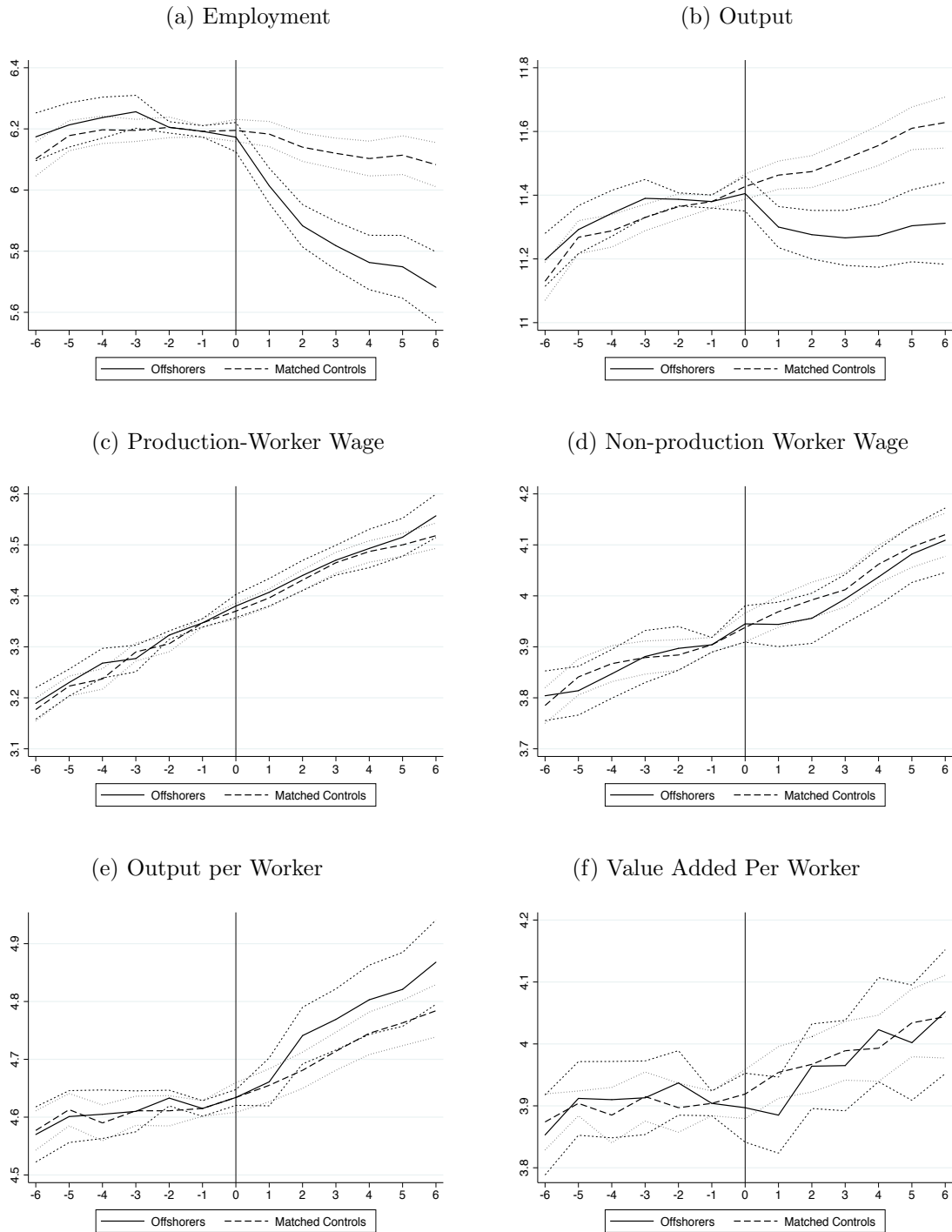
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**Figure 1: Industry Composition: Offshoring Sample vs. 2002 Economic Census vs. 2004 BEA Multinational Sample**



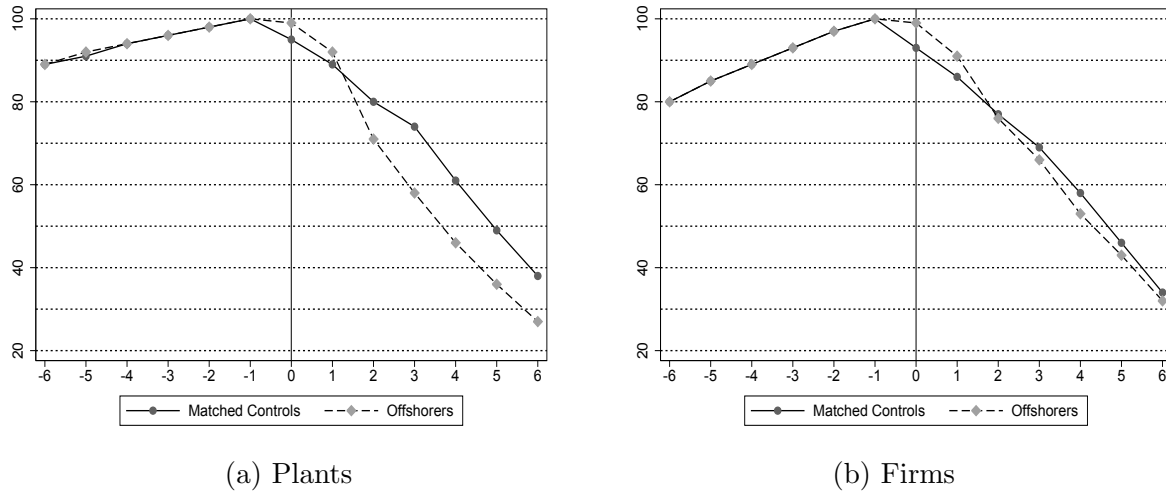
Notes: The figure plots the share of firms in each SIC2 manufacturing industry (a few very small industries are left out for disclosure purposes) in the petition impact year (for offshorers), and Census of Manufactures (CMF) for 2002, and the BEA Multinational Sample for 2004. Source for BEA: [http://www.bea.gov/international/pdf/usdia\\_2004f/Table%20II%20Group/IItables-a1\\_b13.pdf](http://www.bea.gov/international/pdf/usdia_2004f/Table%20II%20Group/IItables-a1_b13.pdf)

**Figure 2:** Employment-Matched Difference-in-Differences Estimation Results



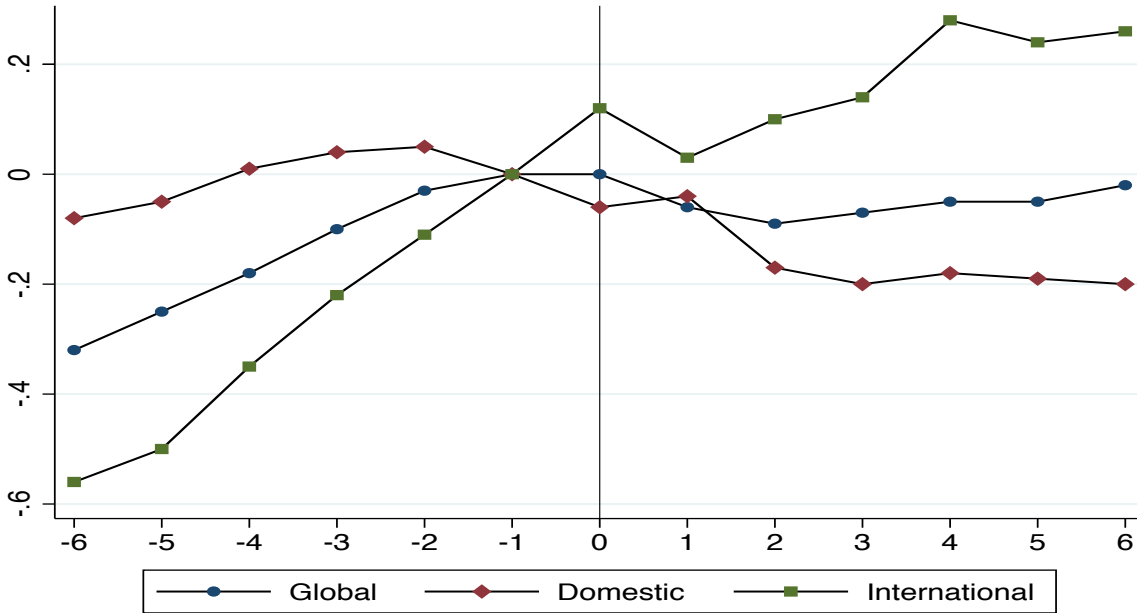
Notes: The figures plot coefficients on event-year dummies (i.e., dummies for number of years relative to the offshoring impact year) for offshorers (labeled “Offshorers”) and the control group (labeled “Control”), in a regression of the dependent variable on the event-year dummies and firm fixed effects (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 3. The dotted lines represent 95% confidence bands using standard errors clustered by industry-size cells.

**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 offsholders (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).

**Figure 4: Offsholders (Compustat-Census Matched Sample): Before-After Changes in Log Employment**



Notes: The figure plots the mean change in log employment (relative to year prior to the petition impact year, for subsample of offsholders in the Compustat sample that we were able to match to the Census Business Register. The “Global” line corresponds to global employment reported by the firm in Compustat, “Domestic” refers to total domestic employment from the LBD, and “International” refers to the (implicit) employment abroad measured as the difference between the global employment reported in Compustat and the domestic employment estimated from the LBD.

**Table 1:** Representativeness of Offshoring-Certified Firms**Panel A:** Employment Shares

# of Employees	Offshoring Firms	All LBD
$\leq 20$	0.046	0.898
21-50	0.091	0.062
51-100	0.115	0.021
101-500	0.307	0.016
501-1000	0.130	0.002
$\geq 1000$	0.311	0.002

Notes: The figures in the table are the share of each sample in each employment bin in the petition impact year (for offshorers) or 2002 (for the whole sample). The total number of unique offshoring firms successfully linked to employment information is 1,340 (rounded), while the total number of firms in the LBD in 2002 is 6,100,600 (rounded).

**Panel B:** Region Shares

Location in U.S.	Offshoring Firms	All LBD
Central	0.245	0.215
East	0.407	0.288
South	0.192	0.247
West	0.156	0.251

Notes: The figures in the table are the share of each sample in each regional bin in the petition impact year (for offshorers) or 2002 (for the whole sample). The total number of unique offshoring firms successfully linked to geographic information is 1,340 (rounded), while the total number of firms in the LBD in 2002 is 6,100,600 (rounded). Multi-Unit firms are assigned the region where the largest fraction of employment is located.

**Panel C:** Age Shares

Firm Age	Offshoring Firms	All LBD
$\leq 5$	0.194	0.395
6-10	0.149	0.197
11-20	0.261	0.233
$\geq 20$	0.396	0.175

Notes: The figures in the table are the share of each sample in each age bin in the petition impact year (for offshorers) or 2002 (for the whole sample). The total number of unique offshoring firms successfully linked to age information is 1,340 (rounded), while the total number of firms in the LBD in 2002 is 6,100,600 (rounded).

**Panel D:** Export Status

	Offshoring Firms	All CMF
Exporter	0.863	0.215

Notes: The figures in the table are the share of each sample that has at least one establishment reporting exports in the petition impact year (for offshorers) or 2002 (for the whole sample). The total number of unique offshoring firms successfully linked to the manufacturing data is 1,100 (rounded), while the total number of manufacturing firms in the CMF in 2002 is 840,200 (rounded).

**Table 2:** Offshoring-Certified Firm Employment versus Aggregate Employment

**Panel A:** Offshorer Employment Share

Year	Emp. Share
1998	0.112
1999	0.121
2000	0.125
2001	0.128
2002	0.133
2003	0.127
2004	0.117
2005	0.106
2006	0.094

**Panel B:** Offshorer Employment Loss Share

Year	Loss Share
1998	-
1999	0.014
2000	0.032
2001	0.068
2002	0.086
2003	0.118
2004	0.102
2005	0.085
2006	0.084

Notes: The figures in the left panel are the share of total manufacturing employment in the LBD accounted for by firms classified as offshoring in the TAA. The figures in the right panel are the share of total losses (measured year-to-year) among declining/exiting manufacturing firms accounted for by offshoring firms that have declining employment or are exiting (after their petition impact date). For example, manufacturing firms that were shrinking/exiting from 2002 to 2003 shed about 2.01 million jobs, of which TAA offshorers shrinking/exiting accounted for 174,000, or 8.6%.

**Table 3:** Pre-Offshoring Premia of TAA-Certified Offshoring Firms

Variable	Definition	(1)	(2)	(3)
<b>Size Measures</b>				
Output	Log(Real total sales = deflated value of shipments)	3.044 (0.000)	2.607 (0.000)	
Value Added	Log(Real value added)	2.919 (0.000)	2.521 (0.000)	
Employment	Log(Employment)	2.679 (0.000)	2.313 (0.000)	
Capital	Log(Real capital stock)	3.336 (0.000)	2.949 (0.000)	
<b>Productivity Measures</b>				
Output per Worker	Log(Total sales/employment)	0.364 (0.000)	0.294 (0.000)	
VA per Worker	Log(Value added/employment)	0.240 (0.000)	0.208 (0.000)	
TFP-Levpet	TFP (Levinsohn-Petrin), Value added	0.088 (0.026)	0.054 (0.028)	
TFP-OLS	TFP (OLS, fixed effects), Value added	0.618 (0.000)	0.559 (0.000)	
<b>Factor Intensity Measures</b>				
Capital Intensity	Log(Capital/ total employment)	0.656 (0.000)	0.636 (0.000)	
NPW Emp Share	Non-production share of employment	-0.009 (0.131)	-0.009 (0.112)	0.02 (0.00)
NPW Wage Share	Non-production share of wage bill	0.003 (0.660)	-0.012 (0.018)	-0.01 (0.07)
<b>Wage Measures</b>				
Wage Rate	Log(Total wage bill/ total employment)	0.045 (0.001)	0.044 (0.000)	0.01 (0.60)
NPW Wage Rate	Log(Non-production wage bill/ non-production employment)	0.082 (0.000)	0.040 (0.016)	0.06 (0.00)
PW Wage Rate	Log(Production wage bill/ production employment)	0.011 (0.447)	0.049 (0.000)	-0.09 (0.00)
<b>Other Measures</b>				
Firm Age	Years in Operation (from LBD)	6.82 (0.000)	6.91 (0.000)	1.33 (0.00)
Exporter	Indicator for Export Status	0.40 (0.000)	0.30 (0.000)	0.11 (0.00)
SIC2 Fixed Effects		No	Yes	Yes
Employment Controls		No	No	Yes

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 third column includes log employment as an independent variable, thus illustrating how offshorers compare to firms of similar sizes in 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.5).



**Table 4: Propensity Model Estimates**

	(1)	(2)	(3)	(4)	(5)
Employment	3.5893**	3.2191**	8.0987**	7.5247**	6.4278**
Capital Intensity	0.7397**	0.7563**	0.371**	0.3554**	0.3676**
PW Wage Rate	0.0000	-0.722*	-0.9711**	-0.7338*	-0.7359*
NPW Wage Rate	-1.2155**	-1.421**	-0.7142**	-0.5629*	-0.5182*
Output per Worker		0.6281**	0.4076**	0.0000	0.0000
Export Status		1.3759**	1.0798**	0.8368**	0.8269**
Firm Age		0.0786**	0.0883**	0.0713**	0.0778**
Fixed Effects	SIC-Year	SIC-Year	SIC-Year, Size	SIC-Year-Size	SIC-Year-Size-Age
R-squared	0.05	0.05	0.05	0.12	0.19

Notes: Dependent variable is a dummy=1 if the firm offshored in any year in the sample period. Refer to Table 3 for definitions of the control variables. Number of observations is 302,000 (rounded, per Census disclosure rules). \*\* denotes significance at 1% level and \* at 5% level. Coefficients are multiplied by 1000 for readability.

**Table 5:** Difference-in-Differences Estimation: All Firms, Employment-Matched

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

Notes: The number of observations for each regression (row) is 22,556. Variables are as defined in Table 3. Each offshorer is matched to up to two firms closest in employment within the same 3-digit industry; in each group comprising an offshorer and up to two matched controls, the controls are imputed the event-year of the focal offshorer in the group. Each row corresponds to a regression of the variable listed in column 1 on event-period dummies interacted with an offshorer dummy, event-period, and firm fixed effects. Results reported in column LR\_PRE correspond to the coefficient  $\beta_{LR\_PRE}$  in equation 1, which is the coefficient on the interaction of the offshorer dummy with a dummy equal to one for offshorer and matched control groups' long run pre-offshoring period (four to six years prior to the offshoring impact year for each matched cell group). Similarly, columns SR\_PRE, SR\_POST and LR\_POST report coefficients  $\beta_{SR\_PRE}$ ,  $\beta_{SR\_POST}$  and  $\beta_{LR\_POST}$  in equation 1), corresponding to interactions of the offshorer dummy with short-run pre-offshoring period (one to three years prior to the impact year), short-run post-offshoring period (one to three years after the impact year), and long-run post-offshoring period (four to six years after the impact year), respectively. Thus reported coefficients represent event-period means for offshorers relative to controls. The impact year is the excluded period; a stand-alone offshorer dummy is not included as it gets absorbed by firm fixed effects. The figures in parenthesis are p-values based on standard errors clustered by industry-size cells.

**Table 6:** Difference-in-Differences Estimation: All Firms, Propensity Score-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
<b>Size</b>							
Output	0.03 (0.49)	-0.01 (0.69)	-0.16 (0.00)	-0.26 (0.00)	-0.15 (0.00)	-0.25 (0.00)	-0.04 (0.22)
Value Added	0.04 (0.40)	0.00 (0.99)	-0.23 (0.00)	-0.31 (0.00)	-0.23 (0.00)	-0.31 (0.00)	-0.04 (0.24)
Employment	0.03 (0.40)	-0.02 (0.49)	-0.19 (0.00)	-0.34 (0.00)	-0.17 (0.00)	-0.32 (0.00)	-0.05 (0.09)
Capital	0.05 (0.29)	0.00 (0.98)	-0.10 (0.00)	-0.18 (0.00)	-0.10 (0.00)	-0.18 (0.00)	-0.05 (0.16)
<b>Wage</b>							
Wage Rate	0.01 (0.34)	0.02 (0.05)	0.02 (0.10)	0.05 (0.00)	0.00 (0.92)	0.03 (0.00)	0.01 (0.42)
NPW Wage Rate	0.02 (0.50)	0.01 (0.42)	-0.02 (0.31)	0.05 (0.06)	-0.03 (0.09)	0.04 (0.14)	-0.01 (0.92)
PW Wage Rate	0.02 (0.23)	0.01 (0.17)	0.02 (0.16)	0.03 (0.06)	0.01 (0.87)	0.02 (0.29)	-0.01 (0.87)
<b>Factor Intensity</b>							
Capital Intensity	0.02 (0.62)	0.01 (0.48)	0.08 (0.00)	0.16 (0.00)	0.07 (0.01)	0.15 (0.00)	-0.01 (0.94)
NPW Emp Share	0.00 (0.51)	-0.01 (0.19)	0.02 (0.00)	0.02 (0.03)	0.03 (0.00)	0.03 (0.00)	-0.01 (0.64)
NPW Wage Share	-0.01 (0.37)	-0.01 (0.14)	0.01 (0.03)	0.02 (0.01)	0.02 (0.00)	0.03 (0.00)	0.00 (0.86)
<b>Productivity</b>							
Output per Worker	0.00 (0.90)	0.01 (0.74)	0.03 (0.16)	0.08 (0.01)	0.02 (0.32)	0.07 (0.02)	0.01 (0.61)
VA per Worker	0.01 (0.80)	0.02 (0.54)	-0.04 (0.09)	0.03 (0.47)	-0.06 (0.03)	0.01 (0.69)	0.01 (0.77)
TFP- Levpet	-0.02 (0.66)	-0.01 (0.74)	-0.10 (0.00)	-0.07 (0.13)	-0.09 (0.00)	-0.06 (0.00)	0.01 (0.79)
TFP- OLS	-0.02 (0.53)	-0.01 (0.68)	-0.08 (0.01)	-0.06 (0.19)	-0.07 (0.02)	-0.05 (0.27)	0.01 (0.65)

Notes: The number of observations for each regression (row) is 19,200 (rounded). Variables are as defined in Table 3. Each offshorer is matched to up to two firms closest in predicted propensity (based on Column 5 of Table 4), within the same 3-digit industry; in each group comprising an offshorer and up to two matched controls, the controls are imputed the event-year of the focal offshorer in the group. Each row corresponds to a regression of the variable listed in column 1 on event-period dummies interacted with an offshorer dummy, event-period, and firm fixed effects. Results reported in column LR\_PRE correspond to the coefficient  $\beta_{LR\_PRE}$  in equation 1, which is the coefficient on the interaction of the offshorer dummy with a dummy equal to one for offshorer and matched control groups' long run pre-offshoring period (four to six years prior to the offshoring impact year for each matched cell group). Similarly, columns SR\_PRE, SR\_POST and LR\_POST report coefficients  $\beta_{SR\_PRE}$ ,  $\beta_{SR\_POST}$  and  $\beta_{LR\_POST}$  in equation 1), corresponding to interactions of the offshorer dummy with short-run pre-offshoring period (one to three years prior to the impact year), short-run post-offshoring period (one to three years after the impact year), and long-run post-offshoring period (four to six years after the impact year), respectively. Thus reported coefficients represent event-period means for offshorers relative to controls. The impact year is the excluded period; a stand-alone offshorer dummy is not included as it gets absorbed by firm fixed effects. The figures in parenthesis are p-values based on standard errors clustered by 3-digit industry-propensity score cells.

**Table 7: Difference-in-Differences Analysis: Firm Survival**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	D1	D2	D3	D4	D5	D6	D1	D2	D3	D4	D5	D6	Exp	Weibull	Exp	Weibull
	Exit Hazard Model															
<b>PANEL A: Survival/exit defined using firm identifier</b>																
Offshorer Dummy	0.05	-0.01	-0.04	-0.06	-0.05	-0.04	0.05	-0.01	-0.03	-0.06	-0.04	-0.03	0.08	0.09	0.14	0.17
	(0.00)	(0.10)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.27)	(0.02)	(0.00)	(0.01)	(0.07)	(0.01)	(0.01)	(0.00)	(0.00)
Constant	0.77	0.59	0.50	0.40	0.30	0.17	0.86	0.77	0.69	0.62	0.55	0.48	-3.27	-4.28	-1.62	-2.57
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.02)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	3700	3700	3500	3100	2600	2100	3700	3700	3500	3100	2600	2100	3700	3700	3700	3700
R-squared	0.10	0.12	0.14	0.17	0.20	0.25	0.56	0.61	0.65	0.69	0.71	0.73	-4043.14	-3892.24	-4043.14	-3892.24
Log Likelihood																
<b>PANEL B: Survival/exit defined using last surviving plant</b>																
Offshorer Dummy	0.03	0.03	-0.03	-0.07	-0.08	-0.08	0.03	0.03	-0.03	-0.07	-0.08	-0.08	0.31	0.32	0.48	0.54
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	0.94	0.85	0.68	0.58	0.51	0.41	0.96	0.93	0.91	0.89	0.86	0.82	-4.85	-6.22	-1.46	-2.9
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Observations	3600	3600	3600	3400	3000	2600	3600	3600	3600	3400	3000	2600	3600	3600	3600	3600
R-squared	0.07	0.09	0.14	0.16	0.18	0.21	0.33	0.37	0.39	0.41	0.43	0.45	-2316.45	-2248.16	-2316.45	-2248.16
Log Likelihood																
Fixed Effects Controls	Industry, year Log Employment															
	Matched cell None												Industry, year Log Employment			

Notes: As in Table 5, each offshorer is matched to up to two firms closest in employment within the same 3-digit industry using data from the Longitudinal Business Database (LBD). The analysis is cross-sectional, with one observation per firm, and offshorers compared to matched controls in the year prior to offshoring. D1 is dummy = 1 if firm survives beyond year 1 after the offshoring year (year zero); D3 to D6 are defined similarly. In panel A, survival is based on whether the firm identifier (“firmid”) exists in the LBD; in panel B, we define survival based on the longest surviving plant among all plants in year -1 (one year before offshoring year). (Note that all matched controls are assigned offshoring year of the focal treatment firm.) We define D3 as missing for firms and matched controls where offshoring was in year 2005, as our data ends in 2008; similarly, D4 to D6 are also truncated. The figures in parenthesis are p-values based on standard errors clustered by industry-size cells.

**Table 8: Difference-in-Differences Estimation: Pseudo-Firms**

	Employment-Matched						Propensity-Matched						Pre-Trend Test			
	Relative to SR_PRE			Relative to SR_POST			Relative to SR_PRE			Relative to SR_POST			Relative to SR_PRE - SR_POST			
	LR_PRE	SR_PRE	SR_POST	LR_POST	LR_POST	SR_PRE	SR_PRE	SR_POST	LR_POST	LR_POST	SR_PRE	SR_POST	LR_POST	LR_POST	SR_PRE	SR_POST
<b>Size</b>																
Output	0.17 (0.00)	0.07 (0.05)	-0.11 (0.00)	-0.12 (0.03)	-0.18 (0.00)	-0.19 (0.00)	-0.10 (0.01)	-0.15 (0.03)	-0.12 (0.03)	-0.04 (0.20)	-0.15 (0.00)	-0.12 (0.03)	-0.12 (0.03)	-0.11 (0.01)	-0.08 (0.19)	-0.10 (0.01)
Value Added	0.20 (0.00)	0.08 (0.08)	-0.11 (0.01)	-0.15 (0.02)	-0.19 (0.00)	-0.23 (0.00)	-0.12 (0.00)	-0.15 (0.02)	-0.14 (0.03)	-0.03 (0.31)	-0.17 (0.00)	-0.14 (0.03)	-0.14 (0.03)	-0.14 (0.01)	-0.11 (0.06)	-0.09 (0.03)
Employment	0.18 (0.00)	0.07 (0.05)	-0.14 (0.00)	-0.21 (0.00)	-0.21 (0.00)	-0.28 (0.00)	-0.11 (0.00)	-0.21 (0.00)	-0.19 (0.00)	0.07 (0.14)	-0.03 (0.29)	-0.14 (0.00)	-0.19 (0.00)	-0.11 (0.00)	0.00 (0.01)	-0.10 (0.00)
Capital	0.16 (0.00)	0.05 (0.15)	-0.12 (0.00)	-0.10 (0.06)	-0.17 (0.00)	-0.15 (0.01)	-0.11 (0.01)	-0.10 (0.06)	-0.08 (0.20)	0.05 (0.46)	-0.04 (0.21)	-0.09 (0.01)	-0.08 (0.20)	-0.05 (0.30)	-0.04 (0.59)	-0.09 (0.06)
<b>Wage</b>																
Wage Rate	-0.01 (0.52)	-0.01 (0.28)	-0.02 (0.18)	-0.01 (0.68)	-0.01 (0.81)	0.00 (0.00)	0.00 (0.71)	-0.01 (0.81)	0.01 (0.72)	-0.02 (0.25)	-0.01 (0.32)	-0.01 (0.32)	0.01 (0.72)	0.00 (0.98)	0.02 (0.00)	0.01 (0.63)
NPW Wage Rate	-0.04 (0.17)	-0.02 (0.26)	-0.03 (0.21)	-0.03 (0.26)	-0.01 (0.91)	-0.01 (0.80)	0.02 (0.54)	-0.03 (0.26)	0.01 (0.87)	-0.03 (0.39)	-0.01 (0.81)	-0.02 (0.36)	0.01 (0.87)	-0.01 (0.47)	0.02 (0.70)	0.02 (0.35)
PW Wage Rate	0.01 (0.43)	0.00 (0.94)	0.00 (0.83)	0.01 (0.64)	0.00 (0.90)	0.01 (0.58)	-0.01 (0.23)	0.01 (0.64)	-0.01 (0.78)	0.00 (0.45)	0.00 (0.28)	0.00 (0.81)	-0.01 (0.78)	0.01 (0.41)	0.00 (0.63)	0.00 (0.93)
<b>Factor Intensity</b>																
Capital Intensity	-0.02 (0.63)	-0.01 (0.70)	0.02 (0.62)	0.11 (0.03)	0.03 (0.39)	0.12 (0.00)	0.01 (0.76)	0.11 (0.03)	0.10 (0.05)	-0.02 (0.58)	-0.01 (0.61)	0.05 (0.05)	0.10 (0.05)	0.06 (0.04)	0.11 (0.00)	0.01 (0.77)
NPW Emp Share	0.00 (0.70)	-0.01 (0.29)	0.00 (0.89)	0.01 (0.51)	0.01 (0.35)	0.02 (0.16)	-0.01 (0.51)	0.01 (0.51)	0.01 (0.57)	-0.01 (0.35)	-0.01 (0.33)	0.00 (0.92)	0.01 (0.57)	0.01 (0.43)	0.02 (0.29)	0.00 (0.77)
NPW Wage Share	-0.01 (0.19)	-0.01 (0.10)	-0.01 (0.49)	0.00 (0.94)	0.00 (0.36)	0.01 (0.20)	0.00 (0.98)	0.00 (0.94)	0.01 (0.31)	0.00 (0.27)	-0.01 (0.40)	0.00 (0.57)	0.01 (0.31)	0.01 (0.77)	0.02 (0.15)	0.00 (0.56)
<b>Productivity</b>																
Output per Worker	-0.01 (0.70)	0.01 (0.78)	0.03 (0.21)	0.09 (0.01)	0.02 (0.35)	0.08 (0.02)	0.02 (0.40)	0.03 (0.40)	0.06 (0.09)	-0.01 (0.76)	-0.01 (0.57)	-0.01 (0.55)	0.06 (0.09)	0.00 (0.98)	0.07 (0.05)	0.00 (0.90)
VA per Worker	0.02 (0.68)	0.01 (0.69)	0.03 (0.39)	0.06 (0.22)	0.02 (0.61)	0.05 (0.31)	-0.01 (0.88)	0.06 (0.22)	0.04 (0.33)	-0.01 (0.82)	0.00 (0.96)	-0.03 (0.36)	0.04 (0.33)	-0.03 (0.36)	0.04 (0.33)	0.01 (0.70)
TFP- Levpet	-0.10 (0.03)	-0.04 (0.19)	0.03 (0.42)	0.10 (0.11)	0.07 (0.08)	0.14 (0.00)	0.06 (0.08)	0.10 (0.08)	0.07 (0.11)	-0.17 (0.00)	-0.04 (0.21)	-0.04 (0.34)	0.07 (0.24)	0.00 (0.96)	0.11 (0.00)	0.13 (0.00)
TFP-OLS	0.12 (0.01)	0.07 (0.03)	-0.05 (0.21)	-0.04 (0.54)	-0.12 (0.00)	-0.11 (0.06)	-0.05 (0.14)	-0.04 (0.54)	-0.12 (0.01)	0.10 (0.02)	0.09 (0.01)	-0.12 (0.00)	-0.14 (0.01)	-0.21 (0.00)	-0.23 (0.00)	-0.01 (0.68)

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 12,400 (rounded), and for the propensity-matched sample regressions (last seven columns in each row) is 10,900 (rounded). Refer to Table 3 for variable definitions. See notes to Table 5 for explanation of column titles. All specifications include firm fixed effects and event-period effects; the figures in parenthesis are p-values based on standard errors clustered by industry-size/industry-propensity score cells.

**Table 9:** Difference-in-Differences (DID) Estimation: LBD Sample

<b>Panel A: Employment-Matched DID</b>							
					<b>Relative to SR_PRE</b>		<b>Pre-Trend Test</b>
	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST - SR_PRE	SR_PRE - LR_PRE
<b>Employment</b>	0.116 (0.000)	-0.001 (0.960)	-0.293 (0.000)	-0.431 (0.000)	-0.292 (0.000)	-0.138 (0.000)	-0.117 (0.000)
<b>Wage Rate</b>	-0.024 (0.165)	0.013 (0.352)	-0.016 (0.368)	0.045 (0.085)	-0.029 (0.012)	0.061 (0.001)	0.037 (0.017)
<b>Payroll</b>	0.117 (0.000)	0.013 (0.447)	-0.315 (0.000)	-0.366 (0.000)	-0.328 (0.000)	-0.051 (0.000)	-0.104 (0.000)

<b>Panel B: Propensity Score-Matched DID</b>							
					<b>Relative to SR_PRE</b>		<b>Pre-Trend Test</b>
	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST - SR_PRE	SR_PRE - LR_PRE
<b>Employment</b>	0.028 (0.610)	0.005 (0.912)	-0.226 (0.000)	-0.361 (0.000)	-0.231 (0.000)	-0.366 (0.000)	-0.023 (0.581)
<b>Wage Rate</b>	0.034 (0.131)	0.004 (0.857)	-0.025 (0.263)	0.062 (0.031)	-0.029 (0.137)	0.058 (0.701)	-0.030 (0.072)
<b>Payroll</b>	0.076 (0.174)	0.023 (0.653)	-0.237 (0.000)	-0.289 (0.000)	-0.260 (0.000)	-0.312 (0.000)	-0.053 (0.201)

Notes: The number of observations for the employment-matched sample regressions is 37,207, and for the propensity-matched sample regressions is 37,400. Refer to Table 3 for variable definitions. See notes to Table 5 for explanation of column titles. All specifications include firm fixed effects and event-period effects; the figures in parenthesis are p-values based on standard errors clustered by industry-size/industry-propensity score cells.

**Table 10: Heterogeneity of Offshoring Effects by Initial Firm Characteristics: Employment-Matched DID**

Sample Split by:	Short-run Effects (SR_POST - SR_PRE)						Long-run Effects (LR_POST - SR_PRE)									
	Output	Emp	Wage	Capital Intensity	NPW	Output per Worker	Levpet	TFP	Output	Emp	Wage	Capital Intensity	NPW	Output per Worker	Levpet	TFP
<b>Initial Firm Employment</b>																
Low	-0.33 (0.000)	-0.35 (0.000)	-0.01 (0.620)	0.16 (0.020)	0.04 (0.000)	0.02 (0.750)	-0.09 (0.220)	0.02 (0.750)	-0.45 (0.000)	0.01 (0.810)	0.11 (0.330)	0.06 (0.000)	0.11 (0.230)	0.11 (0.230)	0.08 (0.530)	
High	-0.16 (0.000)	-0.16 (0.000)	0.00 (0.820)	0.11 (0.000)	0.01 (0.130)	0.01 (0.680)	-0.13 (0.000)	0.01 (0.680)	-0.33 (0.000)	0.00 (0.960)	0.17 (0.000)	0.01 (0.210)	0.02 (0.520)	0.02 (0.520)	-0.16 (0.010)	
<b>Initial PW Wage Rate</b>																
Low	-0.26 (0.000)	-0.33 (0.000)	0.06 (0.010)	0.23 (0.000)	0.05 (0.000)	0.07 (0.070)	-0.13 (0.040)	0.07 (0.070)	-0.52 (0.000)	0.11 (0.000)	0.28 (0.000)	0.05 (0.000)	0.09 (0.100)	0.09 (0.100)	-0.15 (0.110)	
High	-0.18 (0.000)	-0.16 (0.000)	-0.05 (0.000)	0.05 (0.290)	0.01 (0.070)	-0.01 (0.750)	-0.04 (0.350)	-0.01 (0.750)	-0.28 (0.000)	-0.07 (0.000)	0.07 (0.290)	0.02 (0.260)	0.02 (0.630)	0.02 (0.630)	0.04 (0.490)	
<b>Initial Capital Intensity</b>																
Low	-0.28 (0.000)	-0.39 (0.000)	0.04 (0.040)	0.37 (0.000)	0.05 (0.000)	0.11 (0.000)	-0.07 (0.210)	0.11 (0.000)	-0.52 (0.000)	0.08 (0.010)	0.45 (0.000)	0.06 (0.000)	0.13 (0.010)	0.13 (0.010)	-0.05 (0.590)	
High	-0.19 (0.000)	-0.16 (0.000)	-0.02 (0.150)	-0.02 (0.540)	0.01 (0.070)	-0.03 (0.420)	-0.07 (0.120)	-0.03 (0.420)	-0.26 (0.000)	-0.05 (0.030)	-0.07 (0.290)	0.01 (0.350)	-0.04 (0.380)	-0.04 (0.380)	-0.1 (0.130)	
<b>Initial Output per Worker</b>																
Low	-0.32 (0.000)	-0.42 (0.000)	0.04 (0.030)	0.29 (0.000)	0.05 (0.000)	0.11 (0.000)	-0.12 (0.030)	0.11 (0.000)	-0.60 (0.000)	0.09 (0.010)	0.34 (0.000)	0.06 (0.000)	0.23 (0.000)	0.23 (0.000)	-0.01 (0.850)	
High	-0.24 (0.000)	-0.15 (0.000)	-0.04 (0.040)	0.03 (0.420)	0.02 (0.040)	-0.08 (0.010)	-0.11 (0.030)	-0.08 (0.010)	-0.32 (0.000)	-0.04 (0.070)	0.03 (0.640)	0.01 (0.560)	-0.09 (0.070)	-0.09 (0.070)	-0.17 (0.010)	
<b>Initial Export Share</b>																
Non-exporter	-0.36 (0.000)	-0.41 (0.000)	-0.05 (0.280)	0.27 (0.010)	0.05 (0.020)	0.06 (0.370)	-0.04 (0.790)	0.06 (0.370)	-0.64 (0.000)	-0.06 (0.380)	0.24 (0.160)	0.03 (0.370)	0.09 (0.640)	0.09 (0.640)	0.14 (0.530)	
High	-0.09 (0.040)	-0.12 (0.000)	0.00 (0.950)	0.06 (0.120)	0.02 (0.010)	0.03 (0.240)	-0.09 (0.020)	0.03 (0.240)	-0.21 (0.000)	0.00 (0.870)	0.08 (0.130)	0.03 (0.020)	0.04 (0.250)	0.04 (0.250)	-0.10 (0.060)	

Notes: The table presents results similar to Table 5 with samples split into the top and bottom terciles of initial firm characteristics in column 1, except for export share where the split is by "Non-exporters" vs "Above median (by export share) exporters". All specifications include firm fixed effects and event-period effects; the figures in parenthesis are p-values based on standard errors clustered by industry-size/industry-propensity score cells.

**Table 11: Difference-in-Differences Estimation: Vertically Related Firms Only**

	Employment-Matched						Propensity-Matched						Pre-Trend Test				
	Relative to			Pre-Trend			Relative to			Pre-Trend							
	LR_PRE	SR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE		SR_POST - SR_PRE			
<b>Size</b>																	
Output	-0.07 (0.34)	-0.02 (0.67)	-0.19 (0.00)	-0.30 (0.00)	-0.17 (0.00)	-0.28 (0.00)	0.05 (0.34)	0.04 (0.54)	-0.01 (0.80)	-0.09 (0.02)	-0.30 (0.00)	-0.08 (0.11)	-0.29 (0.00)	-0.05 (0.36)			
Value Added	-0.07 (0.37)	-0.01 (0.82)	-0.21 (0.00)	-0.34 (0.00)	-0.33 (0.00)	-0.33 (0.00)	0.06 (0.30)	0.04 (0.59)	-0.02 (0.77)	-0.15 (0.00)	-0.35 (0.00)	-0.13 (0.03)	-0.33 (0.00)	-0.06 (0.32)			
Employment	-0.01 (0.89)	0.01 (0.76)	-0.16 (0.00)	-0.30 (0.00)	-0.17 (0.00)	-0.31 (0.00)	0.02 (0.63)	0.01 (0.91)	-0.04 (0.30)	-0.12 (0.00)	-0.40 (0.00)	-0.08 (0.10)	-0.36 (0.00)	-0.05 (0.29)			
Capital	-0.09 (0.27)	-0.02 (0.64)	-0.16 (0.00)	-0.25 (0.00)	-0.14 (0.02)	-0.23 (0.01)	0.07 (0.30)	0.00 (0.97)	-0.02 (0.67)	-0.09 (0.02)	-0.19 (0.01)	-0.07 (0.17)	-0.17 (0.04)	-0.02 (0.80)			
<b>Wage</b>																	
Wage Rate	0.00 (0.88)	-0.01 (0.60)	0.00 (0.83)	0.03 (0.14)	0.01 (0.47)	0.04 (0.04)	0.00 (0.77)	0.03 (0.08)	0.04 (0.01)	0.01 (0.01)	0.06 (0.00)	0.00 (0.84)	0.02 (0.00)	0.01 (0.61)			
NPW Wage Rate	0.01 (0.68)	0.02 (0.38)	0.02 (0.41)	0.04 (0.25)	0.00 (0.90)	0.02 (0.63)	0.01 (0.71)	0.02 (0.46)	0.03 (0.30)	0.04 (0.16)	0.06 (0.11)	0.01 (0.68)	0.03 (0.38)	0.01 (0.93)			
PW Wage Rate	0.00 (0.90)	-0.02 (0.25)	-0.01 (0.54)	0.01 (0.72)	0.01 (0.61)	0.03 (0.20)	-0.02 (0.51)	0.04 (0.06)	0.03 (0.10)	0.02 (0.16)	0.06 (0.02)	-0.01 (0.74)	0.03 (0.19)	-0.01 (0.45)			
<b>Factor Intensity</b>																	
Capital Intensity	-0.08 (0.12)	-0.04 (0.29)	-0.01 (0.85)	0.04 (1.36)	0.03 (0.47)	0.08 (0.18)	0.04 (0.32)	-0.01 (0.85)	0.02 (0.49)	0.03 (0.39)	0.21 (0.00)	0.01 (0.90)	0.19 (0.00)	0.03 (0.46)			
NPW Emp Share	-0.01 (0.62)	-0.01 (0.44)	0.01 (0.39)	0.02 (0.03)	0.01 (0.16)	0.03 (0.01)	0.00 (0.83)	0.00 (0.98)	0.00 (0.98)	0.01 (0.09)	0.03 (0.03)	0.01 (0.21)	0.03 (0.04)	0.00 (1.00)			
NPW Wage Share	-0.01 (0.52)	0.00 (0.79)	0.01 (0.25)	0.03 (0.02)	0.01 (0.20)	0.03 (0.02)	0.01 (0.57)	0.00 (0.80)	0.00 (0.99)	0.02 (0.04)	0.03 (0.03)	0.02 (0.10)	0.03 (0.03)	0.00 (0.75)			
<b>Productivity</b>																	
Output per Worker	-0.06 (0.11)	-0.03 (0.25)	-0.03 (0.30)	-0.01 (0.87)	0.00 (0.88)	0.03 (0.52)	0.03 (0.29)	0.03 (0.44)	0.03 (0.24)	0.03 (0.39)	0.10 (0.03)	0.00 (0.82)	0.07 (0.18)	0.00 (1.00)			
VA per Worker	-0.06 (0.18)	-0.03 (0.51)	-0.05 (0.25)	-0.05 (0.44)	-0.02 (0.54)	-0.02 (0.68)	0.03 (0.29)	0.04 (0.57)	0.03 (0.49)	-0.03 (0.44)	0.05 (0.45)	-0.06 (0.18)	0.02 (0.72)	-0.01 (0.88)			
TFP- Levpet	-0.02 (0.68)	0.00 (0.99)	0.08 (0.07)	0.08 (0.16)	0.08 (0.09)	0.08 (0.17)	0.02 (0.58)	-0.01 (0.89)	0.00 (0.98)	-0.07 (0.09)	-0.02 (0.78)	-0.07 (0.13)	-0.02 (0.00)	0.01 (0.87)			
TFP-OIS	0.01 (0.85)	0.01 (0.78)	-0.05 (0.25)	-0.07 (0.34)	-0.07 (0.20)	-0.08 (0.29)	0.00 (0.96)	-0.06 (0.34)	-0.02 (0.75)	-0.06 (0.18)	-0.01 (0.84)	-0.04 (0.38)	0.01 (0.98)	0.04 (0.27)			

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 5,900 (rounded). Refer to Table 3 for variable definitions. See notes to Table 5 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 12:** Difference-in-Differences (DID) Estimation: Compustat Analysis

	Employment-Matched						Propensity-Matched						Pre-Trend Test						
	Relative to SR_PRE			Relative to SR_POST			Relative to SR_PRE			Relative to SR_POST									
	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST	LR_POST	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST	LR_POST		SR_PRE	LR_POST	SR_POST	LR_POST	SR_POST	LR_POST
<b>Size</b>																			
Employment	-0.002 (0.978)	-0.055 (0.134)	-0.072 (0.066)	-0.134 (0.039)	-0.018 (0.731)	-0.080 (0.289)	-0.052 (0.351)	-0.009 (0.896)	-0.014 (0.692)	-0.100 (0.066)	-0.120 (0.074)	-0.009 (0.896)	-0.014 (0.692)	-0.100 (0.066)	-0.120 (0.074)	-0.086 (0.080)	-0.106 (0.172)	-0.005 (0.905)	
Total Revenue	-0.054 (0.483)	-0.050 (0.218)	-0.020 (0.568)	-0.090 (0.123)	0.030 (0.575)	-0.041 (0.570)	0.004 (0.946)	-0.027 (0.687)	-0.009 (0.813)	-0.052 (0.102)	-0.099 (0.087)	-0.027 (0.687)	-0.009 (0.813)	-0.052 (0.102)	-0.099 (0.087)	-0.043 (0.400)	-0.090 (0.214)	0.018 (0.675)	
Capital	-0.069 (0.430)	-0.066 (0.131)	-0.058 (0.232)	-0.180 (0.027)	0.008 (0.904)	-0.114 (0.251)	0.003 (0.965)	-0.042 (0.610)	-0.049 (0.245)	-0.129 (0.002)	-0.193 (0.014)	-0.042 (0.610)	-0.049 (0.245)	-0.129 (0.002)	-0.193 (0.014)	-0.081 (0.190)	-0.144 (0.112)	-0.007 (0.903)	
Total Assets	-0.005 (0.953)	-0.041 (0.284)	-0.012 (0.759)	-0.097 (0.116)	0.029 (0.601)	-0.056 (0.460)	-0.036 (0.532)	-0.008 (0.904)	-0.020 (0.588)	-0.054 (0.131)	-0.118 (0.048)	-0.008 (0.904)	-0.020 (0.588)	-0.054 (0.131)	-0.118 (0.048)	-0.034 (0.529)	-0.098 (0.180)	-0.011 (0.821)	
<b>Factor Intensity</b>																			
Capital Intensity	-0.067 (0.225)	-0.012 (0.729)	0.014 (0.688)	-0.046 (0.421)	0.026 (0.564)	-0.034 (0.580)	0.055 (0.168)	-0.033 (0.471)	-0.034 (0.274)	-0.029 (0.303)	-0.073 (0.121)	-0.033 (0.471)	-0.034 (0.274)	-0.029 (0.303)	-0.073 (0.121)	0.006 (0.883)	-0.039 (0.422)	-0.002 (0.961)	
<b>Productivity/Profit</b>																			
Labor productivity	-0.052 (0.272)	0.005 (0.867)	0.052 (0.130)	0.044 (0.347)	0.048 (0.245)	0.039 (0.445)	0.056 (0.118)	-0.018 (0.629)	0.005 (0.837)	0.049 (0.057)	0.021 (0.603)	-0.018 (0.629)	0.005 (0.837)	0.049 (0.057)	0.021 (0.603)	0.044 (0.140)	0.016 (0.712)	0.024 (0.337)	
EBIT/Total Assets	-0.012 (0.384)	-0.006 (0.572)	0.006 (0.701)	0.008 (0.584)	0.012 (0.457)	0.014 (0.230)	0.006 (0.524)	-0.002 (0.820)	-0.002 (0.830)	-0.009 (0.298)	-0.006 (0.526)	-0.002 (0.820)	-0.002 (0.830)	-0.009 (0.298)	-0.006 (0.526)	-0.008 (0.474)	-0.004 (0.673)	0.001 (0.949)	
Net Income/Equity	-0.063 (0.593)	-0.087 (0.454)	-0.139 (0.200)	-0.080 (0.455)	-0.053 (0.481)	0.007 (0.924)	-0.023 (0.741)	-0.003 (0.977)	-0.069 (0.499)	-0.081 (0.381)	-0.061 (0.515)	-0.003 (0.977)	-0.069 (0.499)	-0.081 (0.381)	-0.061 (0.515)	-0.012 (0.834)	0.008 (0.890)	-0.066 (0.195)	
<b>Market Value</b>																			
Market Value	0.111 (0.400)	0.079 (0.339)	0.093 (0.262)	0.001 (0.995)	0.014 (0.872)	-0.078 (0.468)	-0.032 (0.729)	0.090 (0.392)	0.025 (0.725)	-0.071 (0.311)	-0.219 (0.044)	0.090 (0.392)	0.025 (0.725)	-0.071 (0.311)	-0.219 (0.044)	-0.096 (0.222)	-0.244 (0.022)	-0.065 (0.387)	
Tobin's q	-0.145 (0.200)	-0.054 (0.548)	-0.071 (0.400)	-0.033 (0.762)	-0.017 (0.880)	0.022 (0.870)	0.091 (0.370)	-0.061 (0.559)	0.016 (0.853)	-0.064 (0.376)	-0.114 (0.213)	-0.061 (0.559)	0.016 (0.853)	-0.064 (0.376)	-0.114 (0.213)	-0.079 (0.355)	-0.129 (0.179)	0.076 (0.352)	
<b>Survival</b>																			
Dsurvival	-0.050 (0.278)	-0.050 (0.278)	-0.050 (0.278)	-0.070 (0.194)	-0.034 (0.420)	-0.039 (0.464)	-0.034 (0.420)	-0.034 (0.420)	-0.034 (0.420)	-0.034 (0.420)	-0.039 (0.464)	-0.034 (0.420)	-0.034 (0.420)	-0.034 (0.420)	-0.039 (0.464)	-0.034 (0.420)	-0.039 (0.464)	-0.034 (0.420)	-0.034 (0.420)

Notes: The number of observations for the employment-matched sample regressions is 4,248, and for the propensity-matched sample regressions is 4,967. The number of offshoring firms in the sample is 185. Refer to Appendix Section A.6 for details on variable definitions. See notes to Table 5 for explanation of column titles. All specifications except for Dsurvival include firm fixed effects and event-period effects. Dsurvival specification includes industry-size/industry-propensity score cell-year fixed effects. The figures in parenthesis are p-values based on standard errors clustered by industry-size/industry-propensity score cells.

# Online Appendix for “Domestic Gains from Offshoring? Evidence from TAA-linked U.S. Microdata”<sup>41</sup>

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<sup>41</sup> Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of the U.S. Census Bureau, the Board of Governors of the Federal Reserve System, or any other person associated with the Federal Reserve System. All results have been reviewed to ensure that no confidential information is disclosed.

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## A Data Appendix

In this appendix, we describe how we created the baseline dataset of offshoring plants, and provide definitions of key variables.

### 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 the full 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 A4 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 Classification of Petitions: Samples of USDOL Petition Decisions

The classification of petitions under four broad categories comes from determinations made by the USDOL based on their investigation of TAA petitions. For most of the petitions, we rely on the classification provided in the data obtained through our FOIA request. For cases where we made the classification manually, we relied on individual readings of the text of each USDOL decision. The decisions are not in standardized forms but as short letters. To make the classification criterion clearer, we provide a sample extract from the text of the USDOL petition decision for each of the different classification criteria.

- (i) Classification – Company Imports : Example TAW 39788, Lancer Partnership, Screw Machine Department, San Antonio, Texas

*The investigation revealed that sales, production and employment declined at the subject plant during the relevant periods. The subject firm ceased all production in its San Antonio, Texas plant of nuts, bolts, and fittings used in beverage dispensing equipment in July 2001. The company has increased their imports of nuts, bolts, and fittings used in beverage dispensing equipment. The production of nuts, bolts, and fittings at the San Antonio, Texas plant is being replaced by company imports.*

- (ii) Classification – Customer Imports : Example TAW 50921, NVF Hartwell, Container Division, Hartwell,

Georgia): *The Department conducted a survey of major declining customers of the subject firm regarding their purchases of cans, material handling trucks and springs in 2001 and 2002. The survey revealed that a respondent that accounted for an important percentage of the subject firm's sales decline increased their imports of cans, material handling trucks and springs during the relevant period.*

(iii) Classification – Shift in Production: Example TAW 50931, Mead Westvaco Corporation): *I conclude that there was a shift in production from the workers' firm or subdivision to Mexico of articles that are like or directly competitive with those produced by the subject firm or subdivision.*

(iv) Classification – Aggregate Imports: Example TAW 38321, International Paper, Lock Haven, Pennsylvania:

*The reprographic and printing paper produced by International Paper are sold both directly and indirectly to a large number of customers nationwide. Because of the nature of their market, an analysis of aggregate United States imports of the products manufactured at the subject plant can best reflect the impact of imports on sales, production and employment at that plant. From 1999 to 2000 there was an increase in aggregate U.S. imports for consumption of papers like or directly competitive with those produced by the workers at Lock Haven, Pennsylvania.*

## A.5 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) “Employment” the log of the total number of employees reported in the data.
- (d) “Capital” is 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:

$$v_{it}^j = \beta_l^j \cdot l_{it} + \beta_n^j \cdot n_{it} + \beta_k^j \cdot k_{it} + \epsilon_{it}^j \quad (4)$$

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 SIC) 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  $\epsilon_{it}$ .

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  $\omega_{it}$  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.  $\eta_{it}$ , 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  $\omega_{it}$ . Making mild assumptions about the firm’s production technology, Levinsohn and Petrin (2003)  $\omega_{it}$  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  $\omega_{it}$  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.

## A.6 Definitions of Compustat Variables

Key variables from Compustat-CRSP data are defined below. (More details are available in Compustat/CRSP data manuals, accessible at [wrds.wharton.upenn.edu](http://wrds.wharton.upenn.edu).)

- “Employment” is log of the Compustat variable “EMP”, which is the number of people employed by the company and its consolidated subsidiaries (in thousands).

- “Total Revenue” is log of the Compustat variable “REVT”, which represents Net Sales plus other operating revenue.
- “Capital” is the log of the Compustat variable “PPENT”, which represents the cost, less accumulated depreciation, of tangible fixed property (including buildings, plant and equipment) used in the production of revenue.
- “Total Assets” is the log of the Compustat variable “AT”, which represents the total assets/liabilities of a company at a point in time.
- Capital Intensity is  $\log(\text{capital}/\text{employment})$ .
- Labor Productivity is  $\log(\text{revenue}/\text{employment})$ .
- EBIT/Total Assets is Compustat variable “EBIT”, which is earnings before interest and taxes, divided by total assets (i.e., Compustat variable “AT”). Because interest is paid out from these earnings, this could be seen as profits accruing to both debt and equity holders, and hence return to total assets/liabilities.
- Net Income/Equity is Compustat variable “NI” or net income (which represents the fiscal period income or loss reported by a company after subtracting expenses and losses from all revenues and gains), divided by total equity capital. Equity capital is obtained as the difference between Compustat variable “LSE” (Total of all liabilities and capital accounts including stockholders equity) and Compustat variable “LT” (total of current liabilities and long term).
- Market Value is the log of the annual average of the month-end market values. Monthly market value is obtained as the product of CRSP variable “PRC” (closing price on last trading day of the month) and CRSP variable “SHROUT” (number of publicly held shares in the thousands).
- Tobin’s Q is defined as the ratio of market value of total liabilities divided by the book value of total liabilities. The numerator is defined as the sum of the annual average of month-end market values plus total liabilities (Compustat variable “LT”) plus (par) value of preferred stock (Compustat variable “PSTK”).

**Table A1:** Benefits and Services Provided by the TAA Program

<i>Services and Benefits</i>	<i>Description</i>
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 <sup>(a)</sup>
Job Search Allowance	Compensates 90% of the cost of job searches outside commuting area. Maximum of \$ 1,250 <sup>(a)</sup>
Training	Participants are eligible for training up to 104 weeks. <ul style="list-style-type: none"> <li>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.</li> <li>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</li> <li>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</li> <li>d. Customized Training: The training is customized to tasks of a specific firm, but the trainees are not necessarily employed by this firm.</li> </ul> <p>* 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</p>
Trade Readjustment Allowance (TRA)	A participant is eligible to receive income support for up to 104 weeks as the following: <ul style="list-style-type: none"> <li>a. Unemployment Insurance: During the first 26 weeks from separation</li> <li>b. Basic TRA: During the first 26 weeks from exhaustion of UI. This requires training enrollment unless<sup>(b)</sup> (i) the participant has obtained a training waiver, or (ii) has completed approved training</li> <li>c. Additional TRA: During 52 weeks from exhaustion of Basic TRA. Training enrollment is required without exception.</li> <li>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.</li> </ul>
Health Insurance Tax Credit <sup>(c)</sup>	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.

Notes: Source – Employment and Training Administration, U.S. Department of Labor (<http://www.doleta.gov/tradeact/benefits.cfm>). (a) Max \$800 prior to Reform Act of 2002; (b) These exceptions do not apply to NAFTA-TAA participants. Training enrollment is required for NAFTA-TAA participants to receive basic TRA; (c) This is added to TAA benefits by 2002 Reform Act.



**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 U.S. Census microdata.

**Table A3:** 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 A4:** Counts of Offshoring Events Matched to LBD

Impact Year	Total	# Import		
		# Offshoring	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

## B 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 heterogeneous 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.

### B.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.

#### B.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 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  $\phi$  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  $\beta$ . 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  $\phi$  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:  $\text{Max}_{w_d} \theta \ln(w_d - w_x) + (1 - \theta) \ln(\pi_{op})$ , where  $\pi_{op}$  is the marginal profit of an additional worker and  $\theta$  denotes the Nash bargaining parameter. This maximization problem yields the rent sharing 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  $\rho$  summarizes the degree of substitution among differentiated products in  $Y$ ,  $\gamma$  indicates the degree of product differentiation, and  $\lambda$  is the degree of substitution between production in  $X$  and  $Y$ .  $Q_y$  denotes the total consumption of sector  $Y$  products.

### B.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 B1. Each offshoring firm then has a marginal task that separates the offshored tasks and domestic activities.

If the policy cost of offshoring,  $\beta$ , decreases, firms with different productivity levels respond differently. These responses are summarized in panel (b) of Figure B1. 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  $\beta$  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.

## B.2 Alternative model: Shifting entire product line (Horizontal FDI)

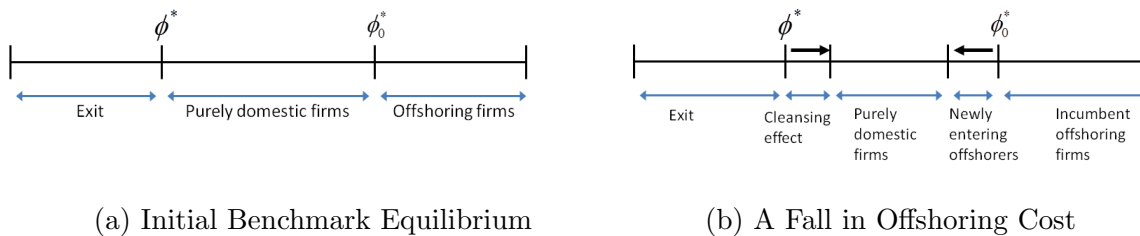
If offshoring consists of a shift of an entire product line (unrelated to remaining domestic activity), foreign employment may simply involve a shift of employment, with no spillover effects. In fact, this type of

“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).<sup>45</sup> 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 2.1.3). 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 B1.

**Figure B1:** Cut-off Productivities in Equilibria




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<sup>45</sup> Harrison and McMillan (2011) and Tintlenot (2013) study the role of this type of “export-platform” FDI.

## C 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).

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 Appendix B.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 B1). 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 U.S. 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 2.4) 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  $\tau$  is the offshoring (impact) year. Thus, the coefficient  $\beta$  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 C1. 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 Kleibergen-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 5, 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 C2, are similar to Table 5 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.

**Table C1:** Instrumental Variables Estimation: All Firms, Four-year Long Differences

Dependent Variable	OLS	2SLS	2SLS
$\Delta$ Output	-0.1434 (0.001)	-0.0937 (0.001)	-0.0737 (0.000) [0.5861]
$\Delta$ Value Added	-0.1823 (0.000)	-0.1666 (0.000)	-0.1352 (0.001) [0.0377]
$\Delta$ Employment	-0.1971 (0.000)	-0.4715 (0.000)	-0.3866 (0.000) [0.065]
$\Delta$ Capital	-0.1447 (0.000)	-0.3369 (0.000)	-0.2716 (0.000) [0.5428]
Instrument(s)		Lagged Log (Emp) $\times$ NTR gap	Lagged Log (Emp), 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 C2:** Alternative Instrumental Variables Estimation: Expanded Instruments Set

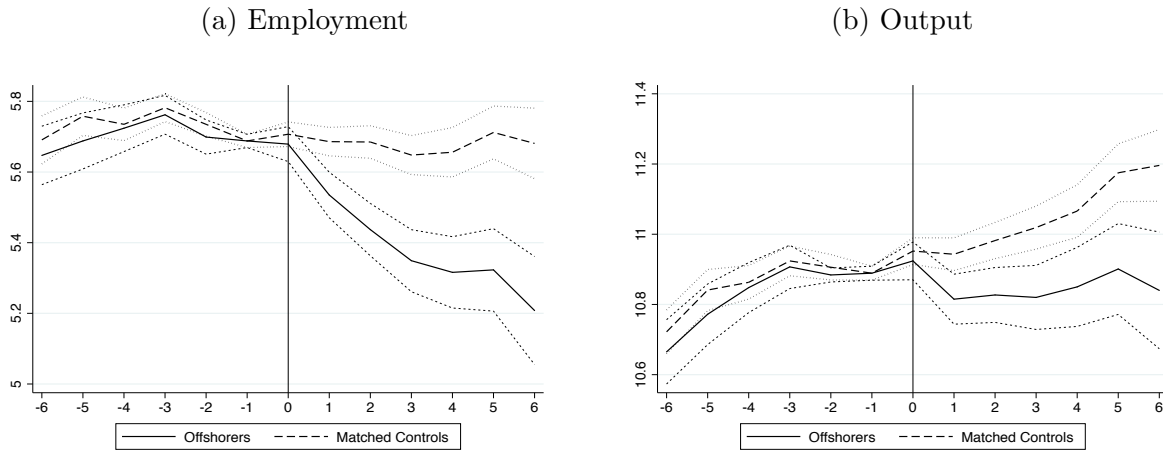
Dependent Variable	OLS	2SLS	2SLS
$\Delta$ Output	-0.1434 (0.001)	-0.0604 (0.017) [182.411]	-0.0480 (0.039) [212.637]
$\Delta$ Value Added	-0.1823 (0.000)	-0.1237 (0.000) [127.636]	-0.1050 (0.000) [166.186]
$\Delta$ Employment	-0.1971 (0.000)	-0.4240 (0.000) [510.742]	-0.3953 (0.000) [651.771]
$\Delta$ Capital	-0.1447 (0.000)	-0.4944 (0.000) [1942.232]	-0.4851 (0.000) [2170.942]
Instrument(s)		Lagged Log (Emp) $\times$ NTR gap Lagged Cap. Int. $\times$ NTR gap Lagged PW Wage $\times$ NTR gap Lagged NPW Wage $\times$ NTR gap	Lagged Log (Emp) $\times$ NTR gap Lagged Cap. Int. $\times$ NTR gap Lagged PW 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.



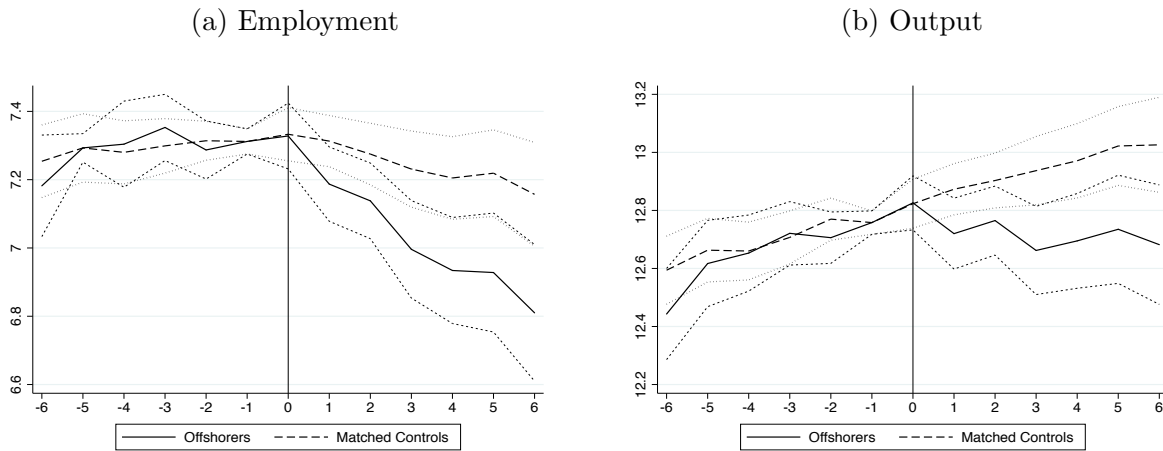
## D Additional Results

**Figure D1: Propensity Score-Matched DID Estimation Results**



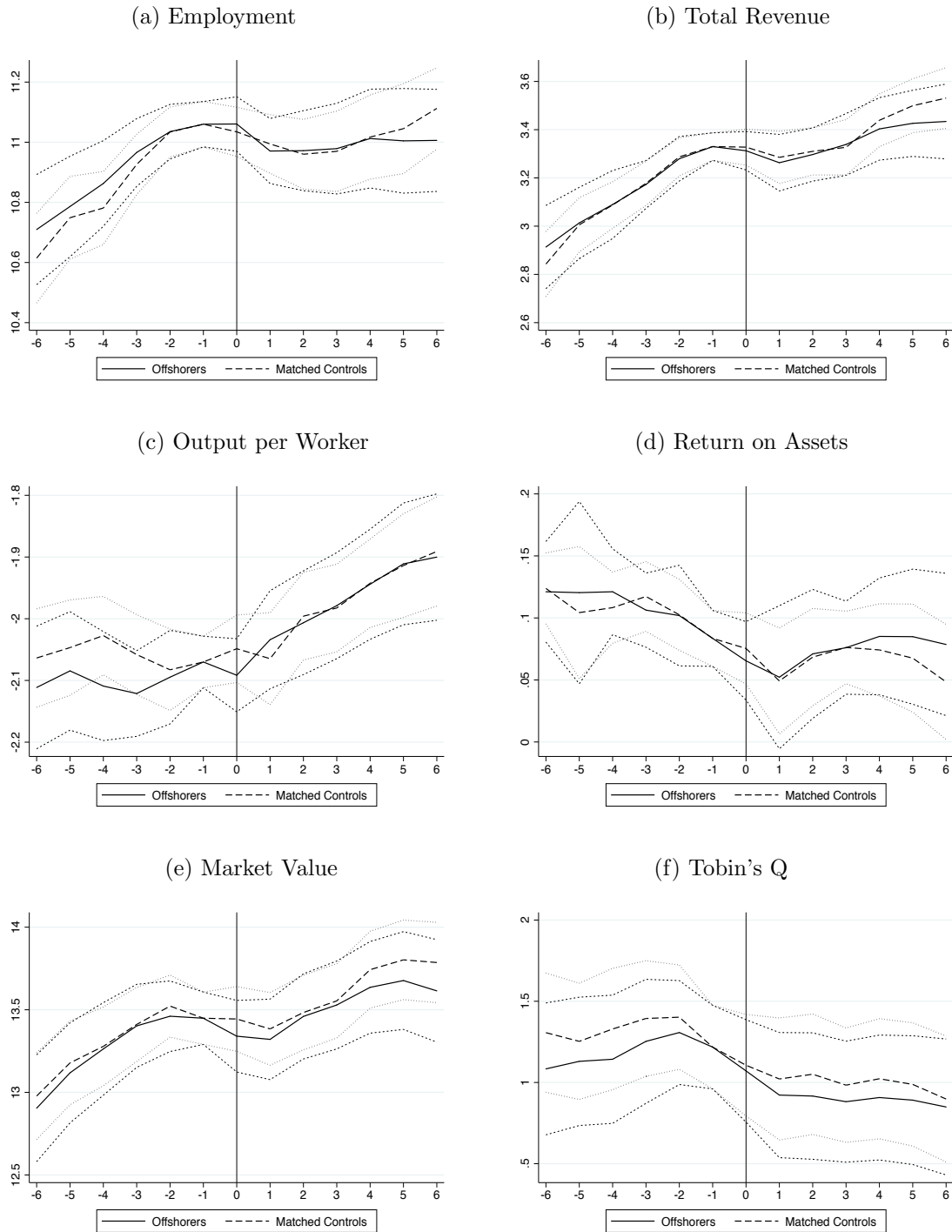
Notes: The figures plot coefficients on event-year dummies (i.e., dummies for number of years relative to the offshoring impact year) for offshorers (labeled “Offshorers”) and the control group (labeled “Matched Controls”), in a regression of the dependent variable on the event-year dummies and firm 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 4), within the same 3-digit industry. The number of observations used for each figure is 18,949. Variables are as defined in Table 3. The dotted lines represent 95% confidence bands using standard errors clustered by industry-propensity score cells.

**Figure D2: 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 “Offshorers”) and the control group (labeled “Matched Controls”), 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 BEA 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 3. The dotted lines represent 95% confidence bands using standard errors clustered by industry-employment cells.

**Figure D3:** Compustat (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 “Offshorers”) 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 4248. Variables are as defined in Appendix A.6. The dotted lines represent 95% confidence bands using standard errors clustered by industry-size cells.

**Table D1:** Comparison of Firms with Different Petition Types**Panel A:** Linked TAA-Business Register Data

	(1)	(2)
	Employment	Payroll
Offshoring	0.74	0.76
	(0.000)	(0.000)
Import Competing	0.51	0.43
	(0.000)	(0.000)

Notes: This panel compares different firms with different petition classifications- offshoring, import competing, or denied (the omitted group)- at the time of the reported impact. These are the results of an indicator regression of log employment/payroll on a petition type categorical variable. 3-Digit SIC Industry fixed effects are included. The number of observations is 3,400, the number of unique events successfully linked to the LBD (rounded, per U.S. Census disclosure rules).

**Panel B:** Linked TAA-Manufacturing Data

	(1)	(2)	(3)	(4)	(5)
	Employment	Output	Value Added	Age	Export Status
Offshoring	2.28	2.58	2.48	6.79	0.3
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Import Competing	1.6	1.67	1.65	5.19	0.22
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Denied	1.58	1.8	1.76	5.12	0.25
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)

Notes: This panel compares different firms with different petition classifications- offshoring, import competing, or denied- with the population of manufacturing firms (the omitted group) at the time of the reported impact. These are the results of an indicator regression of log employment/output/value added, firm age, or an exporting status indicator on a petition type categorical variable. 3-Digit SIC Industry fixed effects are included. The number of observations is 130,700 (rounded, per U.S. Census disclosure rules).

**Table D2:** Difference-in-Differences Estimation: All Firms, Cell-Year Fixed Effects

	Employment-Matched							Propensity-Matched										
	Relative to			Pre-Trend				Relative to			Pre-Trend							
	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST	SR_PRE	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST	SR_PRE	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST	LR_PRE
<b>Size</b>																		
Output	0.08 (0.18)	0.05 (0.28)	-0.13 (0.01)	-0.25 (0.00)	-0.18 (0.00)	-0.30 (0.00)	-0.03 (0.50)	-0.27 (0.00)	0.02 (0.72)	-0.22 (0.00)	-0.52 (0.00)	-0.24 (0.00)	-0.54 (0.00)	0.01 (0.00)	0.02 (0.72)	-0.22 (0.00)	-0.52 (0.00)	-0.24 (0.00)
Value Added	0.12 (0.07)	0.08 (0.13)	-0.17 (0.00)	-0.25 (0.01)	-0.25 (0.00)	-0.33 (0.00)	-0.04 (0.44)	0.04 (0.40)	0.00 (0.99)	-0.23 (0.00)	-0.31 (0.00)	-0.23 (0.00)	-0.31 (0.00)	0.00 (0.24)	0.00 (0.99)	-0.23 (0.00)	-0.31 (0.00)	-0.23 (0.00)
Employment	0.09 (0.07)	0.04 (0.24)	-0.17 (0.00)	-0.26 (0.00)	-0.21 (0.00)	-0.30 (0.00)	-0.05 (0.20)	0.03 (0.40)	-0.02 (0.49)	-0.19 (0.00)	-0.34 (0.00)	-0.17 (0.00)	-0.32 (0.00)	-0.05 (0.09)	-0.02 (0.49)	-0.19 (0.00)	-0.34 (0.00)	-0.05 (0.09)
Capital	0.05 (0.55)	0.00 (0.99)	-0.08 (0.21)	-0.24 (0.03)	-0.08 (0.19)	-0.24 (0.03)	-0.05 (0.46)	0.05 (0.29)	0.00 (0.98)	-0.10 (0.00)	-0.18 (0.00)	-0.10 (0.00)	-0.18 (0.00)	-0.05 (0.16)	0.00 (0.98)	-0.10 (0.00)	-0.18 (0.00)	-0.05 (0.16)
<b>Wage</b>																		
Wage Rate	-0.01 (0.67)	0.00 (0.90)	0.01 (0.70)	-0.02 (0.48)	0.01 (0.62)	-0.02 (0.51)	0.01 (0.66)	0.01 (0.34)	0.02 (0.05)	0.02 (0.10)	0.05 (0.00)	0.00 (0.92)	0.03 (0.00)	0.01 (0.42)	0.02 (0.05)	0.02 (0.10)	0.05 (0.00)	0.00 (0.92)
NPW Wage Rate	-0.02 (0.52)	0.00 (0.91)	-0.02 (0.40)	-0.04 (0.30)	-0.02 (0.41)	-0.04 (0.30)	0.02 (0.47)	0.02 (0.50)	0.01 (0.42)	-0.02 (0.31)	0.05 (0.06)	-0.03 (0.09)	0.04 (0.14)	-0.01 (0.92)	0.01 (0.42)	-0.02 (0.31)	0.05 (0.06)	-0.01 (0.92)
PW Wage Rate	0.00 (0.92)	-0.01 (0.73)	0.00 (0.87)	-0.01 (0.87)	0.01 (0.62)	0.00 (0.96)	-0.01 (0.61)	0.02 (0.23)	0.01 (0.17)	0.02 (0.16)	0.06 (0.06)	0.02 (0.87)	0.02 (0.29)	0.01 (0.87)	0.02 (0.17)	0.02 (0.16)	0.06 (0.06)	0.01 (0.87)
<b>Factor Intensity</b>																		
Capital Intensity	-0.05 (0.47)	-0.04 (0.44)	0.09 (0.13)	0.03 (0.74)	0.13 (0.01)	0.07 (0.42)	0.01 (0.91)	0.02 (0.62)	0.01 (0.48)	0.08 (0.00)	0.16 (0.00)	0.07 (0.01)	0.15 (0.00)	-0.01 (0.94)	0.01 (0.48)	0.08 (0.00)	0.16 (0.00)	-0.01 (0.94)
NPW Emp Share	0.00 (0.67)	0.00 (0.78)	0.02 (0.09)	0.02 (0.33)	0.02 (0.06)	0.02 (0.25)	0.00 (0.80)	0.00 (0.51)	-0.01 (0.19)	0.02 (0.00)	0.02 (0.03)	0.03 (0.00)	0.03 (0.00)	-0.01 (0.64)	-0.01 (0.19)	0.02 (0.00)	0.02 (0.03)	-0.01 (0.64)
NPW Wage Share	-0.01 (0.49)	0.00 (0.91)	0.01 (0.21)	0.01 (0.60)	0.02 (0.19)	0.01 (0.56)	0.01 (0.44)	-0.01 (0.37)	-0.01 (0.14)	0.01 (0.03)	0.02 (0.01)	0.02 (0.00)	0.03 (0.00)	0.00 (0.86)	-0.01 (0.14)	0.01 (0.03)	0.02 (0.01)	0.00 (0.86)
<b>Productivity</b>																		
Output per Worker	-0.01 (0.77)	0.01 (0.79)	0.04 (0.28)	0.01 (0.84)	0.03 (0.39)	0.00 (0.97)	0.02 (0.47)	0.00 (0.90)	0.01 (0.74)	0.03 (0.16)	0.08 (0.01)	0.02 (0.32)	0.07 (0.02)	0.01 (0.61)	0.01 (0.74)	0.03 (0.16)	0.08 (0.01)	0.01 (0.61)
VA per Worker	0.03 (0.58)	0.04 (0.33)	0.00 (0.94)	0.01 (0.85)	-0.03 (0.40)	-0.03 (0.67)	0.01 (0.69)	0.01 (0.80)	0.02 (0.54)	-0.04 (0.09)	0.03 (0.47)	-0.06 (0.03)	0.01 (0.69)	0.01 (0.77)	0.02 (0.54)	-0.04 (0.09)	0.03 (0.47)	0.01 (0.77)
TFP- Levpet	0.07 (0.15)	0.10 (0.02)	-0.03 (0.56)	0.00 (0.98)	-0.13 (0.01)	-0.09 (0.18)	0.03 (0.40)	-0.02 (0.66)	-0.01 (0.74)	-0.10 (0.00)	-0.07 (0.13)	-0.09 (0.00)	-0.06 (0.00)	0.01 (0.79)	-0.01 (0.74)	-0.10 (0.00)	-0.07 (0.13)	0.01 (0.79)
TFP-OLS	0.06 (0.25)	0.08 (0.08)	-0.01 (0.77)	-0.04 (0.62)	-0.09 (0.06)	-0.12 (0.14)	0.02 (0.51)	-0.02 (0.53)	-0.01 (0.68)	-0.08 (0.01)	-0.06 (0.19)	-0.07 (0.02)	-0.05 (0.27)	0.01 (0.65)	-0.01 (0.68)	-0.08 (0.01)	-0.06 (0.19)	0.01 (0.65)

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 19,200 (rounded). Refer to Table 3 for variable definitions. See notes to Table 5 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 D3:** Difference-in-Differences Estimation: Offshoring Firms with Single Certification in 1999-2006 Period

	Employment-Matched						Propensity-Matched						Pre-Trend Test		
	Relative to			Pre-Trend			Relative to			Pre-Trend			Test		
	LR_PRE	SR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE
<b>Size</b>															
Output	0.10 (0.04)	0.08 (0.02)	-0.14 (0.00)	-0.16 (0.02)	-0.22 (0.00)	-0.24 (0.00)	-0.02 (0.64)	-0.01 (0.81)	-0.17 (0.00)	-0.20 (0.01)	-0.16 (0.00)	-0.19 (0.02)	-0.06 (0.14)		
Value Added	0.12 (0.04)	0.11 (0.01)	-0.18 (0.00)	-0.19 (0.02)	-0.29 (0.00)	-0.30 (0.00)	-0.01 (0.86)	0.00 (0.94)	-0.25 (0.00)	-0.25 (0.01)	-0.25 (0.00)	-0.25 (0.00)	-0.06 (0.18)		
Employment	0.09 (0.06)	0.06 (0.07)	-0.19 (0.00)	-0.27 (0.00)	-0.25 (0.00)	-0.33 (0.00)	-0.03 (0.43)	-0.01 (0.76)	-0.20 (0.00)	-0.32 (0.00)	-0.19 (0.00)	-0.31 (0.00)	-0.05 (0.20)		
Capital	0.00 (0.94)	0.01 (0.76)	-0.07 (0.06)	-0.17 (0.03)	-0.08 (0.05)	-0.18 (0.03)	0.01 (0.73)	0.00 (0.94)	-0.10 (0.01)	-0.09 (0.28)	-0.10 (0.03)	-0.09 (0.31)	-0.05 (0.42)		
<b>Wage</b>															
Wage Rate	-0.02 (0.33)	0.00 (0.92)	0.00 (0.94)	0.01 (0.69)	0.00 (0.99)	0.01 (0.00)	0.02 (0.13)	0.01 (0.42)	0.01 (0.62)	0.05 (0.06)	0.00 (0.84)	0.04 (0.00)	0.02 (0.07)		
NPW Wage Rate	-0.03 (0.26)	0.00 (0.90)	-0.05 (0.08)	-0.01 (0.83)	-0.05 (0.09)	-0.01 (0.90)	0.03 (0.22)	0.01 (0.37)	-0.03 (0.26)	0.08 (0.04)	-0.05 (0.05)	0.06 (0.11)	0.01 (0.72)		
PW Wage Rate	-0.01 (0.75)	0.00 (0.97)	0.01 (0.52)	0.02 (0.36)	0.01 (0.44)	0.02 (0.31)	0.01 (0.70)	0.00 (0.43)	0.02 (0.32)	0.03 (0.33)	0.01 (0.75)	0.02 (0.59)	0.01 (0.23)		
<b>Factor Intensity</b>															
Capital Intensity	-0.09 (0.06)	-0.05 (0.16)	0.12 (0.00)	0.10 (0.10)	0.17 (0.00)	0.15 (0.00)	0.04 (0.24)	0.01 (0.70)	0.11 (0.00)	0.23 (0.00)	0.10 (0.02)	0.22 (0.00)	0.00 (0.86)		
NPW Emp Share	0.00 (0.68)	-0.01 (0.34)	0.02 (0.01)	0.02 (0.12)	0.03 (0.00)	0.03 (0.03)	-0.01 (0.62)	-0.01 (0.05)	0.01 (0.10)	-0.01 (0.70)	0.02 (0.00)	0.00 (0.47)	0.01 (0.89)		
NPW Wage Share	-0.01 (0.32)	-0.01 (0.31)	0.01 (0.45)	0.01 (0.60)	0.02 (0.11)	0.02 (0.25)	0.00 (0.80)	-0.01 (0.05)	0.00 (0.74)	0.00 (0.87)	0.01 (0.07)	0.01 (0.24)	0.01 (0.93)		
<b>Productivity</b>															
Output per Worker	0.02 (0.60)	0.03 (0.28)	0.05 (0.05)	0.11 (0.01)	0.02 (0.32)	0.08 (0.05)	0.01 (0.60)	0.00 (0.94)	0.03 (0.29)	0.12 (0.01)	0.03 (0.36)	0.12 (0.01)	-0.01 (0.62)		
VA per Worker	0.03 (0.41)	0.05 (0.13)	0.02 (0.66)	0.08 (0.17)	-0.03 (0.32)	0.03 (0.64)	0.02 (0.48)	0.01 (0.87)	-0.05 (0.27)	0.06 (0.37)	-0.06 (0.20)	0.05 (0.38)	-0.01 (0.61)		
TFP- Levpet	0.05 (0.32)	0.04 (0.27)	-0.05 (0.20)	0.00 (0.98)	-0.09 (0.02)	-0.04 (0.00)	-0.01 (0.92)	-0.02 (0.57)	-0.13 (0.00)	-0.11 (0.14)	-0.11 (0.02)	-0.09 (0.00)	-0.04 (0.20)		
TFP-OIS	0.07 (0.11)	0.04 (0.36)	-0.01 (0.88)	0.08 (0.17)	-0.05 (0.30)	0.04 (0.43)	-0.03 (0.32)	-0.03 (0.54)	-0.10 (0.02)	-0.03 (0.63)	-0.07 (0.09)	0.00 (0.90)	-0.01 (0.86)		

Notes: The number of observations for the employment-matched sample regressions (first seven columns in each row) is 11,000 (rounded), and for the propensity-matched sample regressions (last seven columns in each row) is 8,500 (rounded). Refer to Table 3 for variable definitions. See notes to Table 5 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 D4:** Difference-in-Differences Estimation: Excluding pre-1999 TAA petitioners

	Employment-Matched					Propensity-Matched					Pre-Trend Test SR_PRE - LR_PRE		
	Relative to SR_PRE					Relative to SR_PRE							
	LR_PRE	SR_PRE	SR_POST	LR_POST	LR_POST - SR_POST	LR_PRE	SR_PRE	SR_POST	LR_POST	LR_POST - SR_POST			
<b>Size</b>													
Output	0.04 (0.27)	0.03 (0.21)	-0.18 (0.00)	-0.27 (0.00)	-0.21 (0.00)	-0.30 (0.00)	-0.01 (0.74)	-0.16 (0.00)	-0.24 (0.00)	-0.14 (0.00)	-0.22 (0.00)	-0.03 (0.37)	
Value Added	0.06 (0.15)	0.06 (0.06)	-0.21 (0.00)	-0.32 (0.00)	-0.27 (0.00)	-0.38 (0.00)	0.00 (0.93)	-0.22 (0.00)	-0.28 (0.00)	-0.21 (0.00)	-0.27 (0.00)	-0.03 (0.41)	
Employment	0.04 (0.33)	0.02 (0.33)	-0.21 (0.00)	-0.34 (0.00)	-0.23 (0.00)	-0.36 (0.00)	-0.02 (0.64)	-0.18 (0.00)	-0.33 (0.00)	-0.16 (0.00)	-0.31 (0.00)	-0.03 (0.22)	
Capital	-0.04 (0.38)	-0.03 (0.26)	-0.12 (0.00)	-0.26 (0.00)	-0.09 (0.01)	-0.23 (0.00)	0.01 (0.84)	-0.10 (0.00)	-0.16 (0.00)	-0.08 (0.01)	-0.14 (0.02)	-0.04 (0.38)	
<b>Wage</b>													
Wage Rate	0.00 (0.97)	0.00 (0.94)	0.00 (0.89)	0.01 (0.57)	0.00 (0.94)	0.01 (0.00)	0.00 (0.88)	0.02 (0.14)	0.06 (0.00)	0.00 (0.67)	0.04 (0.00)	0.01 (0.26)	
NPW Wage Rate	-0.01 (0.70)	0.01 (0.73)	-0.04 (0.06)	-0.01 (0.60)	-0.05 (0.03)	-0.02 (0.42)	0.02 (0.40)	-0.02 (0.36)	0.06 (0.02)	-0.04 (0.04)	0.04 (0.15)	0.00 (0.91)	
PW Wage Rate	0.00 (0.86)	-0.01 (0.44)	0.00 (0.92)	0.01 (0.62)	0.01 (0.36)	0.02 (0.25)	-0.01 (0.28)	0.02 (0.24)	0.04 (0.05)	0.00 (0.90)	0.02 (0.25)	0.01 (0.74)	
<b>Factor Intensity</b>													
Capital Intensity	-0.08 (0.03)	-0.06 (0.02)	0.09 (0.00)	0.09 (0.05)	0.15 (0.00)	0.15 (0.00)	0.02 (0.44)	0.07 (0.00)	0.18 (0.00)	0.06 (0.02)	0.17 (0.00)	0.01 (0.95)	
NPW Emp Share	0.00 (0.82)	0.00 (0.70)	0.02 (0.00)	0.03 (0.00)	0.02 (0.00)	0.03 (0.00)	0.00 (0.48)	0.01 (0.01)	0.01 (0.11)	0.02 (0.00)	0.02 (0.02)	0.00 (0.68)	
NPW Wage Share	0.00 (0.75)	0.00 (0.87)	0.01 (0.02)	0.02 (0.05)	0.01 (0.03)	0.02 (0.05)	0.00 (0.81)	0.01 (0.14)	0.02 (0.05)	0.02 (0.02)	0.03 (0.01)	-0.01 (0.74)	
<b>Productivity</b>													
Output per Worker	0.01 (0.74)	0.01 (0.54)	0.04 (0.04)	0.07 (0.02)	0.03 (0.17)	0.06 (0.04)	0.00 (0.81)	0.02 (0.34)	0.09 (0.01)	0.02 (0.41)	0.09 (0.01)	0.01 (0.73)	
VA per Worker	0.03 (0.33)	0.04 (0.13)	0.00 (0.96)	0.03 (0.52)	-0.04 (0.16)	-0.01 (0.74)	0.01 (0.62)	0.01 (0.42)	0.05 (0.30)	-0.05 (0.05)	0.04 (0.43)	0.00 (0.82)	
TFP-Levpet	0.01 (0.77)	0.04 (0.11)	-0.05 (0.10)	-0.04 (0.40)	-0.09 (0.00)	-0.08 (0.00)	0.03 (0.16)	-0.01 (0.79)	-0.06 (0.22)	-0.08 (0.01)	-0.05 (0.00)	-0.01 (0.89)	
TFP-OIS	0.02 (0.51)	0.04 (0.19)	-0.01 (0.71)	0.02 (0.64)	-0.05 (0.12)	-0.02 (0.74)	0.02 (0.53)	-0.01 (0.70)	-0.04 (0.40)	-0.07 (0.05)	-0.03 (0.54)	0.01 (0.62)	

Notes: The number of observations for the employment-matched sample regressions (first seven columns in each row) is 19,300 (rounded), and for the propensity-matched sample regressions (last seven columns in each row) is 16,100 (rounded). Refer to Table 3 for variable definitions. See notes to Table 5 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 D5:** Difference-in-Differences Estimation: Multi-Unit Firms

	Employment-Matched							Propensity-Matched							Pre-Trend Test	
	Relative to				Pre-Trend Test			Relative to				Pre-Trend Test				
	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	LR_PRE	SR_PRE
<b>Size</b>																
Output	0.03 (0.41)	0.02 (0.37)	0.17 (0.00)	-0.29 (0.00)	0.15 (0.00)	-0.32 (0.00)	-0.01 (0.77)	0.01 (0.74)	-0.02 (0.35)	-0.14 (0.00)	-0.25 (0.00)	-0.12 (0.00)	-0.23 (0.00)	-0.03 (0.24)		
Value Added	0.07 (0.13)	0.05 (0.09)	-0.22 (0.00)	-0.33 (0.00)	-0.27 (0.00)	-0.39 (0.00)	-0.01 (0.72)	0.03 (0.58)	-0.01 (0.79)	-0.19 (0.00)	-0.28 (0.00)	-0.18 (0.00)	-0.27 (0.00)	-0.04 (0.31)		
Employment	0.04 (0.24)	0.02 (0.46)	-0.20 (0.00)	-0.34 (0.00)	-0.21 (0.00)	-0.35 (0.00)	-0.03 (0.33)	0.01 (0.77)	-0.04 (0.15)	-0.16 (0.00)	-0.32 (0.00)	-0.12 (0.00)	-0.28 (0.00)	-0.05 (0.11)		
Capital	-0.02 (0.70)	-0.03 (0.35)	-0.12 (0.00)	-0.26 (0.00)	-0.09 (0.00)	-0.23 (0.00)	-0.01 (0.78)	0.04 (0.41)	-0.01 (0.69)	-0.10 (0.00)	-0.16 (0.00)	-0.09 (0.01)	-0.15 (0.01)	-0.05 (0.17)		
<b>Wage</b>																
Wage Rate	0.00 (0.79)	0.00 (0.69)	0.00 (0.89)	0.00 (0.92)	0.00 (0.83)	0.01 (0.70)	0.00 (0.92)	0.01 (0.30)	0.02 (0.10)	0.02 (0.15)	0.04 (0.01)	0.00 (0.98)	0.02 (0.00)	0.01 (0.74)		
NPW Wage Rate	-0.01 (0.56)	0.01 (0.66)	-0.02 (0.20)	-0.02 (0.36)	-0.03 (0.07)	-0.03 (0.18)	0.02 (0.22)	0.01 (0.59)	0.01 (0.75)	-0.01 (0.61)	0.04 (0.13)	0.00 (0.42)	0.03 (0.16)	0.00 (0.69)		
PW Wage Rate	0.01 (0.65)	-0.01 (0.29)	0.00 (0.75)	0.00 (0.78)	0.01 (0.49)	0.02 (0.28)	-0.02 (0.09)	0.01 (0.35)	0.01 (0.32)	0.01 (0.28)	0.03 (0.10)	0.00 (0.85)	0.02 (0.30)	0.00 (0.82)		
<b>Factor Intensity</b>																
Capital Intensity	-0.06 (0.06)	-0.04 (0.05)	0.08 (0.00)	0.08 (0.06)	0.12 (0.00)	0.12 (0.00)	0.02 (0.49)	0.03 (0.42)	0.02 (0.27)	0.06 (0.01)	0.16 (0.00)	0.04 (0.21)	0.14 (0.00)	-0.01 (0.87)		
NPW Emp Share	0.00 (0.48)	-0.01 (0.32)	0.02 (0.00)	0.02 (0.01)	0.02 (0.00)	0.03 (0.00)	0.00 (0.93)	0.00 (0.64)	0.00 (0.49)	0.01 (0.02)	0.01 (0.08)	0.01 (0.01)	0.01 (0.03)	0.00 (0.92)		
NPW Wage Share	-0.01 (0.19)	0.00 (0.59)	0.01 (0.01)	0.01 (0.13)	0.02 (0.01)	0.02 (0.08)	0.01 (0.25)	0.00 (0.54)	-0.01 (0.30)	0.01 (0.09)	0.02 (0.04)	0.02 (0.02)	0.03 (0.01)	-0.01 (0.89)		
<b>Productivity</b>																
Output per Worker	-0.01 (0.55)	0.01 (0.70)	0.03 (0.14)	0.04 (0.10)	0.02 (0.29)	0.04 (0.16)	0.02 (0.22)	0.00 (0.94)	0.01 (0.53)	0.02 (0.27)	0.07 (0.02)	0.01 (0.64)	0.06 (0.05)	0.01 (0.60)		
VA per Worker	0.02 (0.45)	0.04 (0.12)	-0.02 (0.46)	0.00 (0.93)	-0.06 (0.03)	-0.03 (0.35)	0.02 (0.45)	0.02 (0.67)	0.03 (0.32)	-0.03 (0.21)	0.04 (0.42)	-0.06 (0.03)	0.01 (0.83)	0.01 (0.65)		
TFP- Levpet	0.01 (0.76)	0.05 (0.05)	-0.06 (0.05)	-0.04 (0.30)	-0.11 (0.00)	-0.10 (0.02)	0.04 (0.07)	-0.01 (0.74)	0.01 (0.72)	-0.07 (0.01)	-0.05 (0.32)	-0.08 (0.01)	-0.06 (0.00)	0.02 (0.37)		
TFP-OIS	0.03 (0.32)	0.05 (0.05)	-0.02 (0.46)	-0.02 (0.73)	-0.07 (0.01)	-0.07 (0.12)	0.02 (0.36)	-0.03 (0.51)	0.01 (0.84)	-0.06 (0.04)	-0.05 (0.35)	-0.07 (0.03)	-0.06 (0.28)	0.04 (0.23)		

Notes: The number of observations for the employment-matched sample regressions (first seven columns in each row) is 19,245, and for the propensity-matched sample regressions (last seven columns in each row) is 16,200 (rounded). Refer to Table 3 for variable definitions. See notes to Table 5 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 D6:** Difference-in-Differences Estimation: Single-Unit 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_PRE	LR_POST	SR_POST - LR_POST - SR_PRE	LR_PRE	SR_PRE	SR_POST - LR_POST - SR_PRE	LR_POST	SR_POST - LR_POST - SR_PRE	LR_PRE	SR_PRE	SR_POST - LR_POST - SR_PRE
<b>Size</b>													
Output	0.24 (0.01)	0.14 (0.03)	-0.27 (0.00)	-0.25 (0.04)	-0.41 (0.00)	-0.39 (0.00)	0.18 (0.06)	0.10 (0.09)	-0.40 (0.00)	-0.49 (0.01)	-0.50 (0.00)	-0.59 (0.00)	-0.08 (0.22)
Value Added	0.26 (0.02)	0.14 (0.11)	-0.39 (0.00)	-0.26 (0.08)	-0.53 (0.00)	-0.40 (0.00)	0.21 (0.06)	0.10 (0.19)	-0.55 (0.00)	-0.59 (0.02)	-0.65 (0.00)	-0.69 (0.00)	-0.11 (0.16)
Employment	0.14 (0.06)	0.13 (0.01)	-0.38 (0.00)	-0.56 (0.00)	-0.51 (0.00)	-0.69 (0.00)	0.20 (0.01)	0.12 (0.01)	-0.46 (0.00)	-0.66 (0.00)	-0.58 (0.00)	-0.78 (0.00)	-0.08 (0.15)
Capital	-0.03 (0.76)	0.03 (0.51)	-0.04 (0.44)	-0.21 (0.02)	-0.07 (0.25)	-0.24 (0.01)	0.09 (0.28)	0.08 (0.11)	-0.11 (0.03)	-0.30 (0.01)	-0.19 (0.01)	-0.38 (0.00)	-0.01 (0.82)
<b>Wage</b>													
Wage Rate	0.03 (0.44)	0.01 (0.66)	0.01 (0.80)	0.08 (0.16)	0.07 (0.85)	0.07 (0.00)	0.00 (0.98)	0.03 (0.34)	0.02 (0.59)	0.15 (0.01)	-0.01 (0.79)	0.12 (0.00)	0.03 (0.18)
NPW Wage Rate	0.06 (0.38)	0.05 (0.40)	-0.11 (0.07)	-0.04 (0.66)	-0.16 (0.01)	-0.09 (0.25)	0.05 (0.44)	0.05 (0.27)	-0.10 (0.04)	0.09 (0.24)	-0.15 (0.01)	0.04 (0.63)	0.00 (0.95)
PW Wage Rate	0.03 (0.49)	0.02 (0.53)	0.03 (0.42)	0.05 (0.44)	0.01 (0.82)	0.03 (0.67)	0.02 (0.70)	0.04 (0.29)	0.04 (0.41)	0.06 (0.42)	0.00 (0.94)	0.02 (0.81)	0.02 (0.58)
<b>Factor Intensity</b>													
Capital Intensity	-0.17 (0.14)	-0.09 (0.11)	0.34 (0.00)	0.36 (0.01)	0.43 (0.00)	0.45 (0.00)	-0.10 (0.29)	-0.04 (0.46)	0.35 (0.00)	0.36 (0.02)	0.39 (0.00)	0.40 (0.10)	0.06 (0.37)
NPW Emp Share	-0.01 (0.56)	-0.03 (0.09)	0.05 (0.01)	0.09 (0.02)	0.08 (0.00)	0.12 (0.00)	-0.02 (0.45)	-0.02 (0.11)	0.05 (0.00)	0.08 (0.07)	0.07 (0.00)	0.10 (0.02)	0.00 (0.58)
NPW Wage Share	-0.01 (0.76)	-0.03 (0.14)	0.03 (0.17)	0.06 (0.08)	0.06 (0.01)	0.09 (0.01)	-0.02 (0.50)	-0.02 (0.13)	0.03 (0.12)	0.08 (0.06)	0.05 (0.01)	0.10 (0.02)	0.00 (0.70)
<b>Productivity</b>													
Output per Worker	0.10 (0.14)	0.01 (0.80)	0.11 (0.06)	0.32 (0.00)	0.10 (0.11)	0.31 (0.00)	-0.01 (0.85)	-0.02 (0.68)	0.06 (0.34)	0.17 (0.19)	0.08 (0.24)	0.19 (0.16)	-0.01 (0.89)
VA per Worker	0.12 (0.17)	0.01 (0.84)	0.00 (0.98)	0.31 (0.02)	-0.01 (0.83)	0.30 (0.02)	0.01 (0.92)	-0.03 (0.65)	-0.09 (0.29)	0.07 (0.70)	-0.06 (0.46)	0.10 (0.60)	-0.04 (0.56)
TFP- Levpet	0.20 (0.06)	0.06 (0.49)	-0.05 (0.64)	0.16 (0.29)	-0.11 (0.26)	0.10 (0.00)	-0.02 (0.84)	-0.10 (0.13)	-0.26 (0.01)	-0.21 (0.28)	-0.16 (0.12)	-0.11 (0.00)	-0.08 (0.26)
TFP-OIS	0.08 (0.35)	-0.04 (0.56)	-0.08 (0.42)	0.17 (0.23)	-0.04 (0.68)	0.21 (0.11)	0.06 (0.55)	-0.07 (0.29)	-0.20 (0.04)	-0.19 (0.34)	-0.13 (0.17)	-0.12 (0.55)	-0.13 (0.09)

Notes: The number of observations for the employment-matched sample regressions (first seven columns in each row) is 2,600 (rounded), and for the propensity-matched sample regressions (last seven columns in each row) is 2,800. Refer to Table 3 for variable definitions. See notes to Table 5 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 D7: Difference-in-Differences Estimation: Balanced Panel of Firms**

	Employment-Matched						Propensity-Matched						Pre-Trend Test					
	Relative to			Pre-Trend			Relative to			Pre-Trend								
	LR_PRE	SR_PRE	SR_POST - SR_PRE	LR_POST - SR_POST - SR_PRE	LR_POST - SR_POST - SR_PRE	LR_POST - SR_POST - SR_PRE	LR_PRE	SR_PRE	SR_POST - SR_PRE	LR_POST - SR_POST - SR_PRE	LR_POST - SR_POST - SR_PRE	LR_POST - SR_POST - SR_PRE		LR_PRE	SR_PRE	SR_POST - SR_PRE	LR_POST - SR_POST - SR_PRE	LR_POST - SR_POST - SR_PRE
<b>Size</b>																		
Output	-0.02 (0.78)	-0.03 (0.46)	-0.16 (0.00)	-0.29 (0.00)	-0.13 (0.00)	-0.26 (0.00)	-0.01 (0.75)	0.01 (0.88)	-0.15 (0.00)	-0.24 (0.00)	-0.16 (0.00)	-0.01 (0.00)	0.02 (0.88)	-0.07 (0.00)	-0.25 (0.00)	-0.16 (0.00)	-0.02 (0.00)	-0.01 (0.76)
Value Added	0.00 (0.98)	0.00 (0.96)	-0.19 (0.00)	-0.32 (0.00)	-0.19 (0.00)	-0.32 (0.00)	0.00 (0.93)	0.04 (0.39)	-0.17 (0.00)	-0.26 (0.00)	-0.21 (0.00)	0.11 (0.00)	0.04 (0.39)	-0.30 (0.00)	-0.21 (0.00)	-0.21 (0.00)	-0.02 (0.22)	-0.07 (0.22)
Employment	0.02 (0.69)	0.00 (0.95)	-0.17 (0.00)	-0.33 (0.00)	-0.17 (0.00)	-0.33 (0.00)	-0.02 (0.57)	-0.01 (0.80)	-0.14 (0.00)	-0.32 (0.00)	-0.13 (0.00)	0.03 (0.61)	-0.01 (0.80)	-0.31 (0.00)	-0.13 (0.00)	-0.13 (0.00)	-0.04 (0.34)	-0.04 (0.34)
Capital	-0.05 (0.44)	-0.07 (0.09)	-0.10 (0.01)	-0.23 (0.00)	-0.03 (0.58)	-0.16 (0.02)	-0.02 (0.72)	-0.08 (0.07)	-0.15 (0.00)	-0.20 (0.00)	-0.07 (0.23)	-0.11 (0.19)	-0.08 (0.07)	-0.12 (0.11)	-0.07 (0.23)	-0.07 (0.23)	0.03 (0.71)	0.03 (0.71)
<b>Wage</b>																		
Wage Rate	0.01 (0.34)	0.01 (0.62)	0.00 (0.72)	0.00 (0.78)	-0.01 (0.90)	-0.01 (0.90)	0.00 (0.44)	0.02 (0.27)	0.00 (0.79)	0.03 (0.11)	-0.02 (0.18)	0.01 (0.62)	0.02 (0.27)	0.00 (0.79)	-0.02 (0.18)	0.01 (0.18)	0.01 (0.64)	0.01 (0.64)
NPW Wage Rate	0.01 (0.86)	0.01 (0.58)	-0.01 (0.77)	-0.01 (0.65)	-0.02 (0.34)	-0.02 (0.29)	0.00 (0.70)	-0.03 (0.82)	-0.03 (0.27)	0.02 (0.45)	-0.02 (0.38)	-0.03 (0.41)	-0.01 (0.82)	-0.03 (0.27)	-0.02 (0.38)	-0.02 (0.38)	0.02 (0.42)	0.02 (0.42)
PW Wage Rate	-0.01 (0.70)	-0.02 (0.18)	-0.02 (0.19)	0.00 (0.88)	0.00 (0.98)	0.02 (0.30)	-0.01 (0.34)	0.02 (0.11)	0.01 (0.45)	0.03 (0.17)	-0.01 (0.40)	0.02 (0.38)	0.02 (0.11)	0.01 (0.45)	-0.01 (0.40)	-0.01 (0.40)	0.00 (0.70)	0.00 (0.70)
<b>Factor Intensity</b>																		
Capital Intensity	-0.07 (0.15)	-0.07 (0.02)	0.07 (0.03)	0.10 (0.05)	0.14 (0.00)	0.17 (0.00)	0.00 (0.93)	-0.07 (0.03)	-0.01 (0.73)	0.12 (0.00)	0.06 (0.11)	-0.14 (0.02)	-0.07 (0.03)	-0.01 (0.73)	0.19 (0.00)	0.06 (0.11)	0.07 (0.18)	0.07 (0.18)
NPW Emp Share	0.00 (0.81)	0.00 (0.84)	0.02 (0.00)	0.02 (0.03)	0.02 (0.01)	0.02 (0.03)	0.00 (0.59)	0.00 (0.93)	0.01 (0.12)	0.02 (0.12)	0.01 (0.16)	0.00 (0.94)	0.00 (0.93)	0.01 (0.12)	0.02 (0.12)	0.01 (0.16)	0.00 (0.86)	0.00 (0.86)
NPW Wage Share	0.01 (0.57)	0.01 (0.38)	0.03 (0.00)	0.02 (0.06)	0.02 (0.04)	0.01 (0.28)	0.00 (0.89)	-0.01 (0.24)	0.00 (0.57)	0.02 (0.11)	0.03 (0.16)	-0.01 (0.47)	-0.01 (0.24)	0.00 (0.57)	0.03 (0.16)	0.03 (0.16)	0.00 (0.96)	0.00 (0.96)
<b>Productivity</b>																		
Output per Worker	-0.04 (0.19)	-0.03 (0.26)	0.01 (0.62)	0.04 (0.19)	0.04 (0.14)	0.07 (0.04)	0.01 (0.59)	0.02 (0.53)	-0.01 (0.73)	0.07 (0.04)	-0.03 (0.41)	-0.01 (0.82)	0.02 (0.53)	-0.01 (0.73)	0.05 (0.14)	-0.03 (0.41)	0.03 (0.42)	0.03 (0.42)
VA per Worker	-0.02 (0.54)	0.00 (0.89)	-0.02 (0.54)	0.01 (0.90)	-0.02 (0.45)	0.01 (0.98)	0.02 (0.30)	0.05 (0.16)	-0.02 (0.46)	0.06 (0.26)	-0.08 (0.05)	0.08 (0.15)	0.05 (0.16)	-0.03 (0.46)	0.01 (0.92)	-0.08 (0.05)	0.01 (0.53)	-0.03 (0.53)
TFP- Levpet	0.00 (0.97)	0.05 (0.25)	-0.07 (0.11)	-0.05 (0.36)	-0.12 (0.01)	-0.10 (0.00)	0.05 (0.13)	0.07 (0.09)	-0.03 (0.47)	-0.02 (0.73)	-0.10 (0.03)	0.08 (0.13)	0.07 (0.09)	-0.03 (0.47)	-0.09 (0.00)	-0.10 (0.03)	-0.01 (0.84)	-0.01 (0.84)
TFP-OLS	-0.03 (0.52)	-0.01 (0.86)	-0.03 (0.41)	-0.01 (0.92)	-0.02 (0.55)	0.00 (0.97)	0.02 (0.44)	0.00 (1.00)	-0.01 (0.03)	-0.07 (0.22)	-0.10 (0.04)	0.01 (0.83)	0.00 (1.00)	-0.10 (0.03)	-0.07 (0.22)	-0.10 (0.04)	-0.01 (0.73)	-0.01 (0.73)

Notes: The number of observations for the employment-matched sample regressions (first seven columns in each row) is 9,000 (rounded), and for the propensity-matched sample regressions (last seven columns in each row) is 7,000 (rounded). Refer to Table 3 for variable definitions. See notes to Table 5 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 D8:** Difference-in-Differences Estimation: Offshoring Event Prior to 2002

	Employment-Matched						Propensity-Matched						Pre-Trend Test			
	Relative to			Pre-Trend			Relative to			Pre-Trend			Test			
	LR_PRE	SR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	
<b>Size</b>																
Output	0.07 (0.22)	0.04 (0.28)	-0.20 (0.00)	-0.29 (0.00)	-0.24 (0.00)	-0.33 (0.00)	0.03 (0.52)	0.00 (0.93)	-0.19 (0.00)	-0.28 (0.00)	-0.19 (0.00)	-0.28 (0.00)	-0.03 (0.38)	-0.28 (0.00)	-0.03 (0.38)	
Value Added	0.08 (0.21)	0.04 (0.36)	-0.27 (0.00)	-0.34 (0.00)	-0.31 (0.00)	-0.38 (0.00)	0.06 (0.34)	0.02 (0.67)	-0.26 (0.00)	-0.32 (0.00)	-0.28 (0.00)	-0.34 (0.00)	-0.04 (0.36)	-0.34 (0.00)	-0.04 (0.36)	
Employment	0.06 (0.24)	0.03 (0.36)	-0.22 (0.00)	-0.33 (0.00)	-0.25 (0.00)	-0.36 (0.00)	0.02 (0.63)	-0.02 (0.57)	-0.20 (0.00)	-0.34 (0.00)	-0.18 (0.00)	-0.32 (0.00)	-0.04 (0.29)	-0.32 (0.00)	-0.04 (0.29)	
Capital	-0.06 (0.34)	-0.06 (0.07)	-0.14 (0.00)	-0.28 (0.00)	-0.08 (0.06)	-0.22 (0.00)	0.00 (0.99)	-0.03 (0.44)	-0.11 (0.00)	-0.19 (0.00)	-0.08 (0.02)	-0.16 (0.01)	-0.03 (0.61)	-0.16 (0.01)	-0.03 (0.61)	
<b>Wage</b>																
Wage Rate	0.00 (0.99)	0.00 (0.85)	0.00 (0.91)	-0.01 (0.52)	0.00 (0.76)	-0.01 (0.00)	0.00 (0.80)	0.01 (0.51)	0.01 (0.41)	0.03 (0.12)	0.00 (0.81)	0.02 (0.00)	0.01 (0.71)	0.02 (0.00)	0.01 (0.71)	
NPW Wage Rate	-0.01 (0.66)	0.01 (0.59)	-0.02 (0.34)	-0.04 (0.17)	-0.03 (0.10)	-0.05 (0.04)	-0.01 (0.82)	-0.02 (0.35)	-0.02 (0.38)	0.02 (0.50)	0.00 (0.98)	0.04 (0.12)	-0.01 (0.49)	0.04 (0.12)	-0.01 (0.49)	
PW Wage Rate	0.00 (0.98)	-0.01 (0.44)	-0.02 (0.26)	-0.01 (0.75)	-0.01 (0.66)	0.00 (0.82)	0.01 (0.40)	0.02 (0.18)	0.01 (0.72)	0.02 (0.44)	-0.01 (0.34)	0.00 (0.84)	0.01 (0.73)	0.00 (0.84)	0.01 (0.73)	
<b>Factor Intensity</b>																
Capital Intensity	-0.12 (0.01)	-0.09 (0.00)	0.08 (0.01)	0.06 (0.28)	0.17 (0.00)	0.15 (0.00)	-0.02 (0.59)	-0.01 (0.72)	0.08 (0.00)	0.14 (0.00)	0.09 (0.01)	0.15 (0.00)	0.01 (0.68)	0.00 (0.00)	0.01 (0.68)	
NPW Emp Share	0.00 (0.95)	0.00 (0.66)	0.02 (0.00)	0.02 (0.04)	0.02 (0.00)	0.02 (0.02)	0.00 (0.87)	0.00 (0.95)	0.02 (0.00)	0.02 (0.13)	0.02 (0.01)	0.02 (0.12)	0.00 (0.78)	0.02 (0.01)	0.00 (0.78)	
NPW Wage Share	0.00 (0.96)	0.00 (0.67)	0.03 (0.00)	0.01 (0.16)	0.03 (0.00)	0.01 (0.29)	0.00 (0.79)	-0.01 (0.16)	0.02 (0.02)	0.02 (0.07)	0.03 (0.00)	0.03 (0.01)	-0.01 (0.40)	0.03 (0.01)	-0.01 (0.40)	
<b>Productivity</b>																
Output per Worker	0.01 (0.81)	0.01 (0.68)	0.02 (0.37)	0.05 (0.15)	0.01 (0.63)	0.04 (0.26)	0.01 (0.73)	0.01 (0.52)	0.00 (0.87)	0.06 (0.13)	-0.01 (0.70)	0.05 (0.27)	0.00 (0.91)	0.05 (0.27)	0.00 (0.91)	
VA per Worker	0.02 (0.64)	0.01 (0.77)	-0.05 (0.18)	0.00 (0.95)	-0.06 (0.10)	-0.01 (0.79)	0.04 (0.42)	0.03 (0.29)	-0.06 (0.07)	0.02 (0.72)	-0.09 (0.01)	-0.01 (0.78)	-0.01 (0.93)	-0.01 (0.78)	-0.01 (0.93)	
TFP- Levpet	0.07 (0.09)	0.05 (0.18)	-0.07 (0.06)	-0.03 (0.59)	-0.12 (0.00)	-0.08 (0.00)	-0.01 (0.72)	0.01 (0.78)	-0.12 (0.00)	-0.06 (0.28)	-0.13 (0.00)	-0.07 (0.00)	0.02 (0.41)	-0.07 (0.00)	0.02 (0.41)	
TFP-OIS	0.04 (0.38)	0.03 (0.38)	-0.07 (0.08)	-0.03 (0.58)	-0.10 (0.01)	-0.06 (0.26)	-0.02 (0.58)	0.02 (0.65)	-0.09 (0.02)	-0.10 (0.07)	-0.11 (0.01)	-0.12 (0.04)	0.04 (0.18)	-0.12 (0.04)	0.04 (0.18)	

Notes: The number of observations for the employment-matched sample regressions (first seven columns in each row) is 11,400 (rounded), and for the propensity-matched sample regressions (last seven columns in each row) is 11,300 (rounded). Refer to Table 3 for variable definitions. See notes to Table 5 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 D9:** Difference-in-Differences Estimation: Offshoring Event After 2001

	Employment-Matched						Propensity-Matched						Pre-Trend Test		
	Relative to			Relative to			Relative to			Relative to			Pre-Trend Test		
	LR_PRE	SR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	SR_POST - SR_PRE	LR_PRE	SR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE	SR_POST - SR_PRE	LR_POST - LR_PRE
<b>Size</b>															
Output	0.08 (0.09)	0.07 (0.07)	-0.13 (0.00)	-0.28 (0.00)	-0.20 (0.00)	-0.35 (0.00)	0.03 (0.64)	-0.02 (0.68)	-0.11 (0.01)	-0.25 (0.01)	-0.09 (0.06)	-0.23 (0.02)	-0.05 (0.29)		
Value Added	0.13 (0.02)	0.12 (0.01)	-0.16 (0.00)	-0.32 (0.00)	-0.28 (0.00)	-0.44 (0.00)	0.02 (0.78)	-0.02 (0.64)	-0.18 (0.00)	-0.31 (0.00)	-0.16 (0.01)	-0.29 (0.00)	-0.04 (0.36)		
Employment	0.11 (0.02)	0.07 (0.02)	-0.18 (0.00)	-0.34 (0.00)	-0.25 (0.00)	-0.41 (0.00)	0.05 (0.38)	-0.01 (0.82)	-0.16 (0.00)	-0.35 (0.00)	-0.15 (0.00)	-0.34 (0.00)	-0.06 (0.14)		
Capital	0.10 (0.08)	0.08 (0.03)	-0.08 (0.06)	-0.20 (0.00)	-0.16 (0.00)	-0.28 (0.00)	0.12 (0.06)	0.05 (0.27)	-0.07 (0.05)	-0.16 (0.05)	-0.12 (0.02)	-0.21 (0.02)	-0.07 (0.08)		
<b>Wage</b>															
Wage Rate	-0.01 (0.65)	-0.01 (0.39)	-0.01 (0.72)	0.01 (0.58)	0.00 (0.67)	0.02 (0.00)	0.02 (0.20)	0.03 (0.02)	0.03 (0.10)	0.08 (0.00)	0.00 (0.62)	0.05 (0.00)	0.01 (0.47)		
NPW Wage Rate	-0.02 (0.48)	-0.02 (0.40)	-0.04 (0.10)	0.00 (0.90)	-0.02 (0.36)	0.02 (0.50)	0.05 (0.17)	0.07 (0.01)	-0.01 (0.67)	0.10 (0.02)	-0.08 (0.01)	0.03 (0.41)	0.02 (0.64)		
PW Wage Rate	0.01 (0.67)	-0.01 (0.51)	0.01 (0.63)	0.01 (0.79)	0.02 (0.22)	0.02 (0.43)	0.02 (0.39)	0.01 (0.58)	0.03 (0.09)	0.06 (0.05)	0.02 (0.20)	0.05 (0.08)	-0.01 (0.56)		
<b>Factor Intensity</b>															
Capital Intensity	-0.01 (0.86)	0.01 (0.75)	0.11 (0.00)	0.14 (0.02)	0.10 (0.01)	0.13 (0.00)	0.08 (0.14)	0.06 (0.14)	0.09 (0.02)	0.19 (0.01)	0.03 (0.40)	0.13 (0.00)	-0.02 (0.52)		
NPW Emp Share	0.00 (0.73)	0.00 (0.83)	0.02 (0.02)	0.03 (0.03)	0.02 (0.02)	0.03 (0.02)	-0.01 (0.23)	-0.01 (0.08)	0.02 (0.09)	0.03 (0.04)	0.03 (0.00)	0.04 (0.00)	0.00 (0.77)		
NPW Wage Share	-0.01 (0.17)	-0.01 (0.41)	0.00 (0.78)	0.02 (0.12)	0.01 (0.31)	0.03 (0.04)	-0.01 (0.31)	-0.01 (0.52)	0.01 (0.46)	0.04 (0.02)	0.02 (0.24)	0.05 (0.01)	0.00 (0.49)		
<b>Productivity</b>															
Output per Worker	-0.02 (0.49)	-0.01 (0.75)	0.06 (0.03)	0.06 (0.16)	0.07 (0.02)	0.07 (0.10)	-0.02 (0.61)	-0.01 (0.80)	0.05 (0.05)	0.10 (0.05)	0.06 (0.04)	0.11 (0.04)	0.01 (0.64)		
VA per Worker	0.02 (0.55)	0.05 (0.20)	0.02 (0.55)	0.02 (0.78)	-0.03 (0.49)	-0.03 (0.61)	-0.03 (0.60)	-0.01 (0.73)	-0.02 (0.62)	0.04 (0.61)	-0.01 (0.90)	0.05 (0.45)	0.02 (0.71)		
TFP- Levpet	-0.04 (0.39)	0.04 (0.27)	-0.04 (0.33)	-0.03 (0.60)	-0.08 (0.05)	-0.07 (0.00)	-0.02 (0.77)	-0.04 (0.40)	-0.06 (0.14)	-0.08 (0.34)	-0.02 (0.58)	-0.04 (0.00)	-0.02 (0.63)		
TFP-OIS	0.06 (0.19)	0.12 (0.00)	0.06 (0.11)	0.08 (0.24)	-0.06 (0.15)	-0.04 (0.51)	-0.02 (0.75)	-0.04 (0.32)	-0.06 (0.19)	0.02 (0.81)	-0.02 (0.81)	0.06 (0.40)	-0.02 (0.55)		

Notes: The number of observations for the employment-matched sample regressions (first seven columns in each row) is 11,000 (rounded), and for the propensity-matched sample regressions (last seven columns in each row) is 7,900 (rounded). Refer to Table 3 for variable definitions. See notes to Table 5 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 D10:** Panel Difference-in-Differences Analysis: Including all ASM-CMF firms

					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
<b>Size</b>							
Output	0.13 (0.00)	0.15 (0.00)	-0.10 (0.00)	-0.22 (0.00)	-0.25 (0.00)	-0.37 (0.00)	0.02 (0.29)
Value Added	0.16 (0.00)	0.17 (0.00)	-0.15 (0.00)	-0.26 (0.00)	-0.32 (0.00)	-0.43 (0.00)	0.01 (0.64)
Employment	0.14 (0.00)	0.13 (0.00)	-0.17 (0.00)	-0.34 (0.00)	-0.30 (0.00)	-0.47 (0.00)	-0.01 (0.70)
Capital	0.06 (0.01)	0.12 (0.00)	0.03 (0.18)	-0.08 (0.02)	-0.09 (0.00)	-0.20 (0.00)	0.06 (0.02)
<b>Wage</b>							
Wage Rate	-0.02 (0.00)	-0.01 (0.05)	0.00 (0.98)	0.01 (0.46)	0.01 (0.14)	0.02 (0.00)	0.01 (0.40)
NPW Wage Rate	-0.01 (0.29)	-0.02 (0.08)	-0.03 (0.01)	0.00 (0.85)	-0.01 (0.29)	0.02 (0.33)	-0.01 (0.64)
PW Wage Rate	-0.01 (0.41)	-0.01 (0.45)	0.00 (0.96)	0.00 (0.70)	0.01 (0.57)	0.01 (0.38)	0.00 (0.95)
<b>Factor Intensity</b>							
Capital Intensity	-0.08 (0.00)	-0.02 (0.30)	0.20 (0.00)	0.26 (0.00)	0.22 (0.00)	0.28 (0.00)	0.06 (0.00)
NPW Emp Share	-0.01 (0.00)	-0.01 (0.05)	0.01 (0.00)	0.01 (0.12)	0.02 (0.00)	0.02 (0.01)	0.00 (0.27)
NPW Wage Share	-0.01 (0.00)	-0.01 (0.01)	0.01 (0.03)	0.01 (0.18)	0.02 (0.00)	0.02 (0.01)	0.00 (0.22)
<b>Productivity</b>							
Output per Worker	-0.01 (0.35)	0.02 (0.14)	0.07 (0.00)	0.12 (0.00)	0.05 (0.00)	0.10 (0.00)	0.03 (0.02)
VA per Worker	0.02 (0.19)	0.04 (0.01)	0.02 (0.36)	0.08 (0.00)	-0.02 (0.30)	0.04 (0.10)	0.02 (0.27)
TFP-Levpet	0.05 (0.01)	0.03 (0.10)	-0.06 (0.04)	-0.03 (0.37)	-0.09 (0.00)	-0.06 (0.00)	-0.02 (0.39)
TFP-OLS	-0.05 (0.02)	0.08 (0.00)	0.14 (0.00)	0.21 (0.00)	0.06 (0.01)	0.13 (0.00)	0.13 (0.00)

Notes: The number of observations is 725,600 (rounded). In this table, we run panel regressions with one-digit industry-year and firm fixed effects. Refer to Table 3 for variable definitions. See notes to Table 5 for explanation of column titles. The figures in parenthesis are p-values based on standard errors clustered by 4-digit industry codes.

**Table D11:** Alternative Propensity Model Estimates

Variable	Coeff
3-year Employment Growth	0.01
3-year Wage Growth	-0.007**
Output per Worker	0.012**
NPW Emp Share	-0.009**
Capital Intensity	0.012**
Constant	-0.077**
Observations	136,296
R-sq	0.06

Notes: Dependent variable is a dummy=1 if the firm offshored in any year in the sample period. Refer to Table 3 for variable definitions. Number of observations is 16,296. \*\* denotes significance at 1% level and \* at 5% level.

**Table D12:** Difference-in-Differences Estimation - Alternative Propensity Score Matching

					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
<b>Size Measures</b>							
Output	-0.019 (0.624)	0.016 (0.529)	-0.12 (0.000)	-0.239 (0.000)	-0.144 (0.000)	-0.255 (0.000)	0.035 (0.228)
Value Added	0.035 (0.424)	0.061 (0.055)	-0.159 (0.000)	-0.295 (0.000)	-0.22 (0.000)	-0.356 (0.000)	0.026 (0.412)
Employment	-0.011 (0.741)	0.011 (0.624)	-0.193 (0.000)	-0.359 (0.000)	-0.204 (0.000)	-0.37 (0.000)	0.022 (0.392)
Capital	0.019 (0.660)	-0.019 (0.484)	-0.052 (0.073)	-0.171 (0.003)	-0.033 (0.288)	-0.152 (0.009)	-0.038 (0.239)
<b>Wage Measures</b>							
Wage Rate	-0.011 (0.384)	-0.008 (0.412)	0.007 (0.503)	0.037 (0.015)	0.015 (0.145)	0.045 (0.002)	0.003 (0.731)
NPW Wage Rate	0.011 (0.674)	-0.006 (0.757)	-0.009 (0.674)	0.053 (0.072)	-0.003 (0.870)	0.059 (0.030)	-0.017 (0.374)
PW Wage Rate	-0.028 (0.048)	-0.015 (0.187)	0.001 (0.992)	0.009 (0.603)	0.016 (0.185)	0.024 (0.131)	0.013 (0.238)
<b>Factor Intensity</b>							
Capital Intensity	0.031 (0.379)	-0.03 (0.190)	0.141 (0.000)	0.188 (0.000)	0.171 (0.000)	0.218 (0.000)	-0.061 (0.017)
NPW Emp Share	-0.006 (0.379)	-0.003 (0.610)	0.016 (0.003)	0.021 (0.016)	0.019 (0.001)	0.024 (0.008)	0.003 (0.510)
NPW Wage Share	0.001 (0.992)	-0.002 (0.734)	0.016 (0.008)	0.03 (0.002)	0.018 (0.007)	0.032 (0.001)	-0.003 (0.761)
<b>Productivity</b>							
Output per Worker	0.047 (0.114)	0.05 (0.059)	0.034 (0.219)	0.063 (0.129)	-0.016 (0.562)	0.013 (0.735)	0.003 (0.876)
VA per Worker	-0.007 (0.741)	0.004 (0.803)	0.065 (0.000)	0.119 (0.000)	0.061 (0.001)	0.115 (0.000)	0.011 (0.462)
TFP- Levpet	-0.012 (0.734)	0.025 (0.384)	-0.043 (0.177)	0.009 (0.841)	-0.068 (0.025)	-0.016 (0.712)	0.037 (0.130)
TFP- OLS	-0.031 (0.384)	0.023 (0.424)	0.017 (0.589)	0.064 (0.177)	-0.006 (0.856)	0.041 (0.348)	0.054 (0.029)

Notes: The number of observations for each regression (row) is 18,949. Refer to Table 3 for variable definitions. See notes to Table 5 for explanation of column titles. All specifications include firm and event-year effects; p-values in parenthesis.

**Table D13:** Difference-in-Differences Estimation: Pseudo-Firms using LBD

					<b>Relative to SR_PRE</b>		<b>Pre-trend Test</b>
	LR_PRE	SR_PRE	SR_POST	LR_POST	SR_POST- SR_PRE	LR_POST - SR_PRE	SR_PRE- LR_PRE
<b>Panel A: Employment-Matched DID</b>							
Employment	0.180	0.030	-0.120	-0.200	-0.150	-0.230	-0.150
	(0.000)	(0.220)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Payroll	0.100	-0.010	-0.190	-0.260	-0.180	-0.250	-0.110
	(0.002)	(0.690)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Average Wage	-0.100	-0.040	-0.070	-0.060	-0.030	-0.020	0.060
	(0.000)	(0.020)	(0.001)	(0.080)	(0.230)	(0.620)	(0.000)
<b>Panel B: Propensity Score-Matched DID</b>							
Employment	0.130	-0.010	-0.200	-0.330	-0.190	-0.320	-0.140
	(0.000)	(0.550)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Payroll	0.110	-0.010	-0.180	-0.280	-0.170	-0.270	-0.120
	(0.000)	(0.700)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Average Wage	-0.020	0.000	0.010	0.050	0.010	0.050	0.020
	(0.240)	(0.720)	(0.380)	(0.003)	(0.560)	(0.000)	(0.020)

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 in Panel A is about 24,800 (rounded), and for the propensity-matched sample regressions in Panel B is 27,300 (rounded). Refer Table 3 for variable definitions. See notes to Table 5 for explanation of column titles. All specifications include firm fixed effects and event-period effects; the figures in parenthesis are p-values based on standard errors clustered by industry-size/industry-propensity score cells.

**Table D14: Heterogeneity of Offshoring Effects by Initial Firm Characteristics: Propensity Score-Matched DID**

Sample Split by:	Short-run Effects (SR_POST - SR_PRE)						Long-run Effects (LR_POST - SR_PRE)							
	Output	Emp	Wage	Capital Intensity	NPW	Output per Worker	TFP-Levpet	Output	Emp	Wage	Capital Intensity	NPW	Output per Worker	TFP-Levpet
<b>Initial Firm Employment</b>	Low	-0.24 (0.000)	-0.28 (0.000)	0.00 (0.980)	0.14 (0.040)	0.05 (0.000)	0.03 (0.520)	-0.32 (0.020)	-0.4 (0.000)	0.07 (0.110)	0.13 (0.220)	0.06 (0.010)	0.07 (0.410)	-0.07 (0.590)
	High	-0.11 (0.020)	-0.10 (0.020)	-0.01 (0.580)	0.04 (0.150)	0.01 (0.070)	-0.01 (0.790)	-0.09 (0.030)	-0.23 (0.000)	-0.28 (0.000)	0 (0.620)	0.13 (0.010)	0.02 (0.070)	0.05 (0.260)
<b>Initial PW Wage Rate</b>	Low	-0.21 (0.000)	-0.31 (0.000)	0.08 (0.000)	0.19 (0.000)	0.04 (0.000)	0.1 (0.010)	-0.37 (0.000)	-0.49 (0.000)	0.12 (0.000)	0.26 (0.000)	0.04 (0.030)	0.12 (0.050)	-0.04 (0.630)
	High	-0.17 (0.000)	-0.14 (0.000)	-0.06 (0.000)	0.01 (0.700)	0.02 (0.120)	-0.03 (0.320)	-0.14 (0.000)	-0.29 (0.000)	-0.33 (0.000)	-0.03 (0.160)	0.10 (0.060)	0.02 (0.250)	0.04 (0.470)
<b>Initial Capital Intensity</b>	Low	-0.27 (0.000)	-0.36 (0.000)	0.04 (0.120)	0.21 (0.000)	0.05 (0.000)	0.1 (0.020)	-0.4 (0.000)	-0.54 (0.000)	0.12 (0.000)	0.36 (0.000)	0.06 (0.000)	0.15 (0.020)	-0.08 (0.380)
	High	-0.15 (0.010)	-0.13 (0.010)	-0.03 (0.110)	-0.02 (0.680)	0.02 (0.020)	-0.02 (0.460)	-0.10 (0.020)	-0.19 (0.020)	-0.24 (0.000)	0.00 (0.780)	-0.02 (0.810)	0.03 (0.010)	0.05 (0.330)
<b>Initial Output per Worker</b>	Low	-0.21 (0.000)	-0.31 (0.000)	0.02 (0.310)	0.17 (0.000)	0.03 (0.050)	0.1 (0.000)	-0.27 (0.020)	-0.52 (0.000)	0.07 (0.040)	0.29 (0.000)	0.04 (0.080)	0.25 (0.000)	0.04 (0.710)
	High	-0.16 (0.000)	-0.11 (0.020)	-0.02 (0.310)	-0.01 (0.790)	0.02 (0.080)	-0.06 (0.100)	-0.14 (0.010)	-0.34 (0.000)	-0.28 (0.000)	0.00 (0.950)	0.1 (0.130)	0.02 (0.080)	-0.05 (0.350)
<b>Initial Export Share</b>	Non-exporter	-0.24 (0.000)	-0.29 (0.000)	-0.01 (0.640)	0.17 (0.000)	0.03 (0.000)	0.05 (0.220)	-0.34 (0.000)	-0.45 (0.000)	0.06 (0.060)	0.23 (0.010)	0.05 (0.010)	0.11 (0.100)	-0.07 (0.490)
	High	-0.14 (0.010)	-0.14 (0.000)	0.00 (0.850)	0.02 (0.540)	0.02 (0.020)	0.00 (0.940)	-0.09 (0.080)	-0.25 (0.000)	-0.3 (0.860)	0.00 (0.070)	0.02 (0.080)	0.06 (0.260)	-0.05 (0.550)

Notes: The table presents results similar to Table 6 with samples split into the top and bottom terciles of initial firm characteristics in column 1, except for export share where the split is by "Non-exporters" vs "above median (by export share) exporters". All specifications include firm fixed effects and event-period effects; the figures in parenthesis are p-values based on standard errors clustered by industry-size/industry-propensity score cells.



**Table D15: Pre-Offshoring Premia of TAA-Certified Offshoring Firms – Compustat Sample**

Variable	Definition	(1)	(2)	(3)
<b>Size Measures</b>				
Employment	Log (Employment)	2.419 (0.000)	2.263 (0.000)	
Total Revenue	Log (Total Revenue)	2.438 (0.000)	2.413 (0.000)	
Capital	Log (Property, Plant & Equipment, Net)	2.440 (0.000)	2.471 (0.000)	
Total Assets	Log (Total Assets)	1.896 (0.000)	2.229 (0.000)	
<b>Factor Intensity Measures</b>				
Capital Intensity	Log (Capital/Employment)	0.021 (0.742)	0.208 (0.000)	
<b>Productivity/Profitability Measures</b>				
Labor Productivity	Log (Total Revenue/Employment)	0.019 (0.601)	0.150 (0.000)	0.057 (0.162)
EBIT/Total Assets	EBIT/Total Assets (Winsorized, 1%, both tails)	0.146 (0.000)	0.167 (0.000)	-0.062 (0.000)
Net Income/Equity	Net Income/Equity (Winsorized, 1%, both tails)	0.096 (0.019)	0.153 (0.002)	0.069 (0.165)
<b>Market Value-related Measures</b>				
Market Value	Log annual average of month-end market values	1.610 (0.000)	2.033 (0.000)	0.473 (0.000)
Tobin's Q	Market value of liabilities/Book value of liabilities	-0.181 (0.026)	-0.172 (0.035)	0.148 (0.096)
<b>Industry diversification</b>				
NSIC	Total number of primary and secondary SIC segment codes	1.492 (0.000)	1.103 (0.000)	0.486 (0.004)
NNAICS	Total number of primary and secondary NAICS segment codes	1.501 (0.000)	1.113 (0.000)	0.476 (0.008)
SIC2 Fixed Effects		No	Yes	Yes
Employment Controls		No	No	Yes

Notes: The reported figures are the coefficient on a dummy that equals one for firms that offshored, in the year before offshoring; 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-year fixed effects and hence captures the mean difference between offshorers and all other firms within the same industry. The third column includes log employment as an independent variable, thus illustrating how offshorers compare to firms of similar sizes in the same industry. The number of observations for all of the statistics is 123,322, except for market value and Tobin's Q for which number of observations is about 83,900. The data source is the TAA data linked to the Compustat and CRSP datasets. More details on the variable definitions are provided in the Data Appendix Section A.6.

**Table D16:** Pre-Offshoring Premia within TAA-certified categories – Compustat Sample

Variable	“Offshorers” vs “Import Competing”			“Production Shift” vs “Company Imports”		
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Size Measures</b>						
Employment	0.695 (0.000)	0.605 (0.067)		-0.185 (0.454)	-0.217 (0.716)	
Total Revenue	0.611 (0.000)	0.611 (0.085)		-0.161 (0.539)	-0.182 (0.793)	
Capital	0.318 (0.043)	0.289 (0.449)		-0.253 (0.398)	-0.653 (0.392)	
Total Assets	0.657 (0.000)	0.644 (0.074)		-0.121 (0.661)	-0.477 (0.526)	
<b>Factor Intensity Measures</b>						
Capital Intensity	-0.377 (0.000)	-0.317 (0.076)		-0.068 (0.601)	-0.436 (0.136)	
<b>Productivity/Profitability Measures</b>						
Labor Productivity	-0.084 (0.054)	0.006 (0.949)	0.029 (0.284)	0.024 (0.754)	0.035 (0.825)	0.048 (0.736)
EBIT/Total Assets	-0.012 (0.238)	0.017 (0.508)	0.015 (0.271)	0.012 (0.497)	0.065 (0.083)	0.074 (0.097)
Net Income/Equity	-0.04 (0.473)	-0.003 (0.989)	0.01 (0.881)	0.048 (0.531)	0.008 (0.973)	0.045 (0.860)
<b>Market Value-related Measures</b>						
Market Value	0.635 (0.001)	0.434 (0.399)	1.136 (0.000)	0.28 (0.445)	-0.139 (0.878)	0.059 (0.905)
Tobin’s Q	0.197 (0.022)	0.08 (0.706)	0.141 (0.006)	0.176 (0.294)	0.367 (0.282)	0.403 (0.246)
<b>Industry diversification</b>						
NSIC	0.552 (0.009)	0.435 (0.353)	0.651 (0.000)	-0.465 (0.220)	-0.318 (0.620)	-0.169 (0.810)
NNAICS	0.614 (0.005)	0.369 (0.419)	0.656 (0.000)	-0.197 (0.380)	-0.11 (0.775)	-0.026 (0.945)
SIC2 Fixed Effects	No	Yes	Yes	No	Yes	Yes
Employment Controls	No	No	Yes	No	Yes	Yes

Notes: In columns 1 to 3, the reported figures are the coefficient on a dummy that equals one for firms that were classified as “Offshorers” (which aggregates sub-categories “Company Imports” and “Production Shift”, while the excluded (dummy=0) sample consists of firms classified as “Import competing” (which aggregates sub-categories “Aggregate Imports” and “Customer Imports”, in the year before the TAA layoff event. In columns 4 to 6, the reported figures are the coefficient on a dummy that equals one for firms that were classified as “Production shifts”, while the excluded (dummy=0) sample consists of firms classified as “Company Imports”, in the year before the TAA layoff event. The figures in parenthesis are p-values. Columns 1 and 4 (OLS) capture the mean difference between offshorers and all other firms, while the columns 2 and 5 (Industry FE) includes 3-digit SIC industry-year fixed effects and hence captures the mean difference between offshorers and all other firms within the same industry. The third column includes log employment as an independent variable, thus illustrating how offshorers compare to firms of similar sizes in the same industry. The number of observations for all of the statistics is 123,322, except for market value and Tobin’s Q for which number of observations is about 83,900. The data source is the TAA data linked to the Compustat and CRSP datasets. Refer to Appendix Table D15 for variable descriptions; more details on the variable definitions are provided in the Data Appendix Section A.6.

**Table D17:** Propensity to Offshore: Analysis using Compustat Data

	(1)	(2)	(3)	(4)
Employment	1.406 (0.000)		1.442 (0.000)	2.241 (0.024)
Capital Intensity	0.178 (0.314)		0.119 (0.534)	-0.081 (0.809)
Output per worker		-0.043 (0.771)	0.250 (0.175)	0.167 (0.529)
EBIT/Total Assets		2.367 (0.000)	-0.263 (0.566)	0.029 (0.935)
Net Income/Equity		0.174 (0.021)	0.050 (0.489)	0.030 (0.711)
Tobin's q		-0.034 (0.727)	0.155 (0.119)	0.102 (0.242)
Fixed Effects	Ind-Year	Ind-Year	Ind-Year	Ind-Year-Size
R-squared	0.099	0.097	0.100	0.388

Notes: Dependent variable is a dummy=1 if the firm offshored in any year in the sample period. Refer to Table 3 for definitions of the control variables. Number of observations is 83,836. p-values based on standard errors clustered at 3 digit industry-level are in parentheses.

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