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AMERICAN JOBS AND THE RISE OF SERVICE OUTSOURCING TO CHINA AND INDIA

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ABSTRACT

We examine the impact on U.S. labor markets of offshore outsourcing in services to China and India. We also consider the reverse flow or 'inshoring' which is the sale of services produced in the United States to unaffiliated buyers in China and India. Using March-to-March matched CPS data for 1996-2006 we examine the impacts on (1) occupation and industry switching, (2) weeks spent unemployed as a share of weeks in the labor force, and (3) earnings. We precisely estimate small positive effects of inshoring and smaller negative effects of offshore outsourcing. The net effect is positive.

To illustrate how small the effects are, suppose that over the next nine years all of inshoring and offshore outsourcing grew at rates experienced during 1996-2005 in business, professional and technical services i.e., in segments where China and India have been particularly strong. Then workers in occupations that are exposed to inshoring and offshore outsourcing (1) would switch 4-digit occupations 2 percent less often, (2) would spend 0.1 percent less time unemployed, and (3) would earn 1.5 percent more. These are not annual changes – they are changes over nine years – and are thus best described as small positive effects.

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Beginning in the mid-1990s, cumulative improvements in information and communications technologies (ICT) facilitated a dramatic expansion of international trade in services. The interaction of these improvements with a politically driven increase in the outward orientation of China and India has had two major consequences for the pattern of world trade. First, educated U.S. workers are competing for the first time ever with educated but low-paid foreign workers. Second, the most entrepreneurial of these educated workers are providing key inputs into the final product so that, as in Antrás (2003) and Antrás and Helpman (2004), these inputs are provided in outsourcing relationships rather than within multinationals. In short, educated American workers are now facing intense competition from the offshore outsourcing of services to China and India.

This development has captured just about everyone's attention. Samuelson (2004) and Blinder (2005) have emphasized its potential dangers while Bhagwati, Panagariya and Srinivasan (2004) and Grossman and Rossi-Hansberg (2008) have pointed to its potential benefits. The U.S. Senate has taken a dim view of those who speak out against the mounting jobs hysteria (e.g., Mankiw and Swagel, 2006) while international organizations have taken a more balanced view of the costs and benefits (e.g., UNCTAD, 2004; OECD, 2006). Even management consultants have been in on the act, seeing the potential to attract business through either over-blown claims (e.g., Forrester Research, 2002) or considered opinion (e.g., Baily and Farrell, 2004). Yet to date there has not been a multivariate econometric assessment of the impact of offshore outsourcing on educated, white-collar U.S. workers. We know nothing about what offshore outsourcing of services to China and India has meant for the incidence of industry and occupation switching, for weeks spent unemployed, or for education- and experience-adjusted earnings.

There is, of course, a large related literature on offshore outsourcing spawned by the seminal work of Feenstra and Hanson (1996, 1999). Much of this literature is concerned with inequality and the relative demand for skills. See Verhoogen (2008) for a recent contribution. See also Amiti and Wei (2005*a*, 2005***b*****,** 2006*a*) for related work on employment and productivity. The closest paper to ours is Amiti and Wei (2006*b*). They regress changes in industry-level U.S. manufacturing employment over the 1992-2000 period on Feenstra-Hanson measures of outsourcing and find modest employment impacts. We depart from the few existing econometric studies in several ways.

First, we focus only on services, only on offshore outsourcing and only on low-wage trading partners. Most of the literature deals with manufacturing and works with the sum of offshore outsourcing and foreign direct investment. The literature also usually looks at all trade rather than just trade with low-wage countries, though see Bernard, Jensen and Schott (2006) and Liu (2007) for exceptions. Offshore outsourcing of services to low-wage countries is where the debate has been most intense and where the econometric analysis has been all but absent. The international trade data we use are international service transactions between unaffiliated parties for the period 1995-2005. The data are produced by the Bureau of Economic Analysis (BEA).

Second, we examine the flip side of offshore outsourcing, namely, the sale of services produced in the United States to unaffiliated parties in low-wage countries. For brevity we refer to this as 'inshoring.'[1] It is inappropriate to look only at the costs of offshore outsourcing to low-wage countries without at the same time looking at the benefits of inshoring to these countries.

Third, we work with March-to-March matched Current Population Survey (CPS) data for 1996-2006. This allows us to control for worker characteristics and exploit longitudinal evidence. Our strategy of combining matched CPS data with trade data has been influenced by

[1] Slaughter (2004) coined the term "insourcing" to refer to foreign direct investment rather than outsourcing. We are unabashedly stealing his term.

Goldberg and Tracy's (2003) excellent work on the effect of exchange rates on wages and job switching. It is the only study we know of that uses matched CPS data to study an international trade issue. We examine four worker outcomes: (1) industry switching, (2) occupation switching, (3) annual changes in weeks spent unemployed as a share of total weeks in the labor force (as in Murphy and Topel, 1987), and (4) changes in earnings.

Turning to our results, we precisely estimate either *small negative effects or zero effects of offshore outsourcing* on all four outcomes. We also precisely estimate *small positive effects or zero effects of inshoring* on each of these outcomes. The positive inshoring effects are either as large as or larger than the negative offshore outsourcing effects so that the net effect is either slightly positive or zero. Since the small effects are precisely estimated we can say with confidence that even if service trade with China and India grows at its current clip, the labor-market implications will be small. In short, we find that the labor-market consequences of inshoring and offshore outsourcing of services to China and India are, to quote the Bard, much ado about nothing.

The remainder of this introduction reviews additional aspects of the related literature. The observation that there has been almost no econometric research on the effect of inshoring and offshore outsourcing on our four outcomes should not be misconstrued to mean that there has been no research at all. Much of the existing research is primarily concerned with counting the number of U.S. workers who are in industries or occupations that are exposed to inshoring and offshore outsourcing in services. This is done by concording the classification systems used to record international trade in services with various industry and occupation classification systems used to record employment data. See Bardhan and Kroll (2003), Garner (2004), Kirkegaard (2004), Blinder (2005), van Welsum and Vickery (2005), and van Welsum and Reif (2006*a*, 2006*b*, 2006*c*, 2006*d*). We use the by-now standard concordance that comes out of these studies to link international trade data with CPS data.

Jensen and Kletzer (2005) provide a very different and novel way of thinking about exposure to international service trade. If U.S. production of a service is concentrated geographically within the United States then it must be that the service is traded across regions within the United States. Jensen and Kletzer argue that it must therefore be tradable internationally. Using this insight, they find that about 28 percent of employment is in tradable occupations. This estimate is very similar to our own estimate of 24 percent.

This paper is related to two strands of research. Research on worker mobility has found that human capital is partly specific to industries and occupations e.g., Jacobson, LaLonde and Sullivan (1993), Neal (1995), and Kambourov and Manovskii (forthcoming). This implies that human capital will be destroyed by industry and occupation switching that is induced by offshore outsourcing. Thus, estimates of the impact of service offshore outsourcing on industry and occupation switching is important for the design of policy. There is also an emerging literature on the effect of trade openness on individual income volatility. See McLaren and Newman (2002), Krabs, Krishna and Maloney (2005) and Bergin, Feenstra and Hanson (2007). We do not examine income volatility though our work has implications for it.

The paper is organized as follows. Section 1 reviews the trends in outsourced international service trade. Section 2 describes our March-to-March CPS matching and the concordance between the BEA international service trade classification and the CPS industry and occupation classifications. Section 3 describes the key variables. Section 4 provides a simple difference-in-difference analysis of the impacts of inshoring and offshore outsourcing. Sections 5-7 present the main econometric results. Section 8 provides a lengthy sensitivity analysis that is entirely responsible for the unusual length of the paper. The less interested reader who skips section 8 will find this paper to be of average length. Section 9 concludes.

1. U.S. International Trade in Services: Inshoring and Offshore Outsourcing

We are interested in the labor-market impacts of the offshore outsourcing of services. The usual data used in studies of service-trade impacts do not distinguish between (1) offshore outsourcing – which is an arm's length transaction between unaffiliated parties and (2) offshoring –which is the sum of affiliated-party and unaffiliated-party trade.[2] See the state-of-the-art studies by Amiti and Wei (2006*b*) and van Welsum and Reif (2006*d*). See also Trefler (2005, forthcoming) and Helpman and Trefler (2006). Service trade data originate from balance-of-payments surveys. To isolate offshore outsourcing in services we use BEA data on international service transactions between unaffiliated parties. See Borga and Mann (2004) for details of the BEA database. As is standard in the offshoring literature, we only consider the BEA category 'other private services.' It is defined as total private-sector services less transportation services, royalties and license fees. We will henceforth reserve the term *offshore outsourcing* for international transactions involving the sale of a foreign-produced service to an unaffiliated U.S. party. Likewise, we will reserve the term *inshoring* for international transactions involving the sale of a U.S.-produced service to an unaffiliated foreign party.[3]

We use trade data for the period 1996-2005 or, when using lags, for 1995-2004. 1995 and

[2] Two parties are unaffiliated if neither has a controlling interest in the other.

[3] It would have been interesting to complement our analysis with a parallel analysis of affiliated-party service trade or even total service trade. Unfortunately, the BEA does not report this information by detailed country and type of service. On the other hand, there have been a number of studies of affiliated-party trade. Harrison and McMillan (2006) provide a thorough examination of the outsourcing of manufacturing jobs by U.S. multinationals. They find that job creation in affiliates located in low-income (high-income) countries substitutes for (complements) job creation in U.S.-based plants. Using *service-sector* data, Feinberg and Keane (2006) find that fast-growing U.S. multinationals tend to expand employment everywhere whereas slow-growing U.S. multinationals tend to contract employment everywhere.

1996 are convenient starting dates because inshoring and offshore outsourcing with China and India were at low levels until then. (Recall that offshore outsourcing of services to India came to prominence during the Y2K scare of the late 1990s.) Given the familiar difficulties of disentangling Mainland Chinese and Hong Kong trade (Feenstra, Lipsey, Deng, Ma, and Mo, 2005), we include Hong Kong in our Chinese trade data.[4] See appendix 1 for additional data details.

Table 1 provides some basic statistics on inshoring and offshore outsourcing. The largest category of U.S. inshoring is business, professional, and technical (BPT) services. This includes ICT services (e.g., call centers and software development) as well as various legal, engineering, management consulting, R&D and advertising services. Column 1 reports the dollar value of these U.S. exports (inshoring) in 2005. Columns 2-4 report the average annual log change in inshoring over the 1996-2005 period for the world, China plus India, and the G8.[5] Columns 7-9 report the corresponding growth rates of offshore outsourcing. China and India's inshoring and offshore outsourcing have been growing at 0.11 and 0.02 log points per year, respectively. See the last row of columns 3 and 8. The slow 0.02 growth of offshore outsourcing will come as a surprise. It is due to declines in telecommunications, China and India's largest offshore outsourcing segment. These declines may be reversed if China's version of third-generation wireless telecommunication standards (TD-SCDMA) is adopted in the future. When attention is focused exclusively on computer and information services, the growth rate of offshore outsourcing rises to a remarkable 0.39 log points a year.

There are two notable numbers in the table that will be used repeatedly below. For Chinese

[4] In the robustness section we include all low-wage countries for which bilateral data are available and find that this does not alter our results. This is to be expected given China and India's dominance.
[5] The G8 includes Canada, France, Germany, Italy, Japan and the United Kingdom. We exclude the United States (the exporter) and Russia (hardly a rich economy) from the G8.

and Indian activities with the highest profile (BPT Services), the growth rates are 0.14 for inshoring and 0.16 for offshore outsourcing. These growth rates will be used for the 'rapid-growth effect' described below.

Since cross-industry variation in growth rates for China and India documented in columns 3 and 8 are one of the sources of sample variation exploited in our regression analysis, it is useful to be reminded of their causes. *First*, ICT-related technological change has been more relevant for some industries (such as computer and information services) than others (such as advertising). *Second*, for political economy reasons entirely exogenous to the U.S. labor market, China and India began liberalizing their economies in the early 1990s.[6] These exogenous liberalizations provided U.S. firms with access to skilled but low-paid workers from China and India and this access had greater cost implications for some industries (such as call centers) than for others (such as R&D). The liberalization also exogenously provided U.S. firms with access to a growing market that was in desperate need of some but not all U.S. services e.g., banking and insurance. It is helpful to separate out these two sources of cross-industry variation because much of the discussion about offshore outsourcing has been focused not on its general, ICT-enabled rise with all countries, but on its specific rise with China and India.

Table 1 allows one to partially disentangle the first source of variation (the pure ICT effect) from the second source (the interaction of ICT with exogenous liberalizations). Columns 4 and 9 of the table document the growth rates of inshoring and offshore outsourcing for the G8. This G8 growth was driven by ICT developments and had nothing to do with Chinese and Indian liberalizations. Indeed, the China-India growth rates in column 3 are not that correlated with the G8 growth rates in column 4. The correlation is only 0.43. Column 5 looks at the difference

[6] In China, market reforms accelerated in 1992 as a result of Deng Xiaoping's famous Southern Tour. In India, market reforms started in July 1991 as a result of a severe balance of payments crisis.

between columns 3 and 4. It shows that there have been large differences between the G8 and China-India in inshoring growth rates. This suggests that a substantial portion of the cross-industry variation in growth rates for China and India had less to do with a pure ICT effect and more to do with how ICT interacted with Chinese and Indian economic liberalizations. As a result, in our regression analysis we will use G8 trade data in various ways as a control and/or instrument for the pure effects of ICT-related technological change.

2. CPS Data and its Link to Trade Data

We match individuals across consecutive March CPS surveys from 1996 to 2006 in order to extract longitudinal information about work histories.[7] We start the matching procedure by extracting the subsample of all civilian adults who were surveyed in March of some year *t*. We then apply Madrian and Lefgren's (2000) two-stage matching algorithm to find a match in the March survey of year *t* +1. In the first or 'naïve' stage, individuals are matched based on three variables: a household identifier, a household number, and an individual line number within a household. If all three variables are the same in two consecutive March surveys then a naïve match is made. In the second stage, a naïve match is discarded if it fails the S|R|A merge criterion i.e., if in the two consecutive March surveys the individual's sex changes, the individual's race changes, or the individual's age changes inappropriately.[8] The naïve and final match rates for each year appear in appendix table A.1. Averaging across all years, the naïve match rate is 67 percent, the S|R|A discard rate is 5 percent, and the final match rate is 64 percent ($0.67\times0.95 = 0.64$). Note that for 2001-2006 we also discard oversamples in the State Children

[7] For international trade economists who are not familiar with the CPS, the most important thing to understand here about the structure of the CPS is that, *very roughly speaking*, an individual is surveyed in March of two consecutive years and then dropped from the CPS. We track (or match) individuals across the two consecutive years.

[8] Following Madrian and Lefgren (2000), an inappropriate age change is less than –1 or more than 3. See Madrian and Lefgren (2000) for more detailed information about the matching algorithm.

Health Insurance Program (SCHIP) extended sample files. Our final match rate is similar to the rates of 62 percent in Goldberg and Tracy (2003) and 67 percent in Madrian and Lefgren (2000).

Since the actual match rate is lower than the match rate of 100 percent that would obtain in the absence of mortality, migration, non-response and recording errors, there is obviously a selection issue associated with using matched CPS data. Neumark and Kawaguchi (2004) partly dispel this selection concern by comparing the estimation results based on matched CPS data to results based on the Survey of Income and Program Participation (SIPP) which follows individuals who move. However, as a precaution, in section 8.3 we model selection as a probit and simultaneously estimate the selection equation together with our labor-market outcome regressions. While we cannot reject selection bias, the coefficients we care about are virtually the same as in OLS specifications.

Each worker in the CPS sample is linked to a trade flow via the worker's occupation and industry of affiliation. We thus need two concordances. One maps the trade categories in table 1 into the Census *industry* classification. The other maps the table 1 trade categories into the Census *occupation* classification. For example, consider a programmer who works for an insurance company. Demand for her services will depend on demand in the insurance industry (the industry link) and on the demand for computer programmers (the occupation link).

The linking of service-trade data to industries and occupations is now commonplace in the offshore outsourcing literature e.g., Bardhan and Kroll (2003), Garner (2004), Blinder (2005), Jensen and Kletzer (2005), Kirkegaard (2005), Mann (2005), van Welsum and Vickery (2005) and van Welsum and Reif (2006*b*). For the sake of non-trade economists we emphasize that we are not breaking new ground here. Appendix tables A.2 and A.3 present our mapping from the table 1 trade categories into industries and occupations, respectively. The mappings are very similar to those reported elsewhere e.g., van Welsum and Vickery (2005) and van Welsum and Reif (2006*b*).[9]

Our CPS sample of matched workers consists of 158,291 private workers aged 18-64. 37,550 of these workers or 24 percent are in occupations that map into service-trade categories. Following the extensive literature cited above, we say that these workers are in occupations that are exposed to inshoring and offshore outsourcing and refer to them as the *occupation-exposed sample*. 24,261 of our 158,291 workers, or 15 percent, are in industries that map into the table 1 service-trade categories i.e., that are exposed to inshoring and offshore outsourcing. We refer to these 24,261 workers as the *industry-exposed sample*. These two samples will be important for what follows. Note that there is overlap between them: 15,183 workers are in both samples.

3. Variable Definitions

Summary statistics for the occupation-exposed sample are presented in table 2. The corresponding statistics for the industry-exposed sample appear in appendix table A.4. We will go through the statistics in detail in order to ensure that the definition of key variables is properly understood. Since each worker appears in two consecutive March surveys we will use t to denote the year of the first March survey. t is what appears in the 'Year' column of table 2.

Industry and Occupation Switching: In both March surveys the worker is asked about her occupation in the longest job held last year ($t-1$ or t). A worker is a 4-digit occupation switcher if she worked in both years and had different occupations in years $t-1$ and t. This raw switching rate is notoriously noisy. We thus filter it as suggested by Moscarini and Thomsson

[9] Welsum is the lead OECD researcher on this topic.

(2006). To be a valid switch, the worker must also have (1) changed her class[10] or (2) looked for a job last year or (3) switched industries.[11] See appendix 2 for details. The generalization to 1- and 2- digit switching and to industry switching is immediate.

Consider column 1 of table 2. It reports 4-digit occupation switching rates for the occupation-exposed sample. For example, the 1996 row reports that 28 percent of workers switched 4-digit occupations between 1995 and 1996. From the bottom panel of the table, switching rates averaged across all years fall to 0.20 at the 2-digit level and 0.17 at the 1-digit level.[12]

Our occupation switching rates at the 1- and 2-digit levels are similar to those in Kambourov and Manovskii (2008) who use 1996 PSID data. They report switching rates of 0.18 at the 2-digit level and 0.16 at the 1-digit level. We cannot compare our 4-digit rates to anything in their work because they use older 3-digit 1970 Census codes while we use 4-digit 2002 Census codes. However, our 4-digit rate of 0.28 is significantly higher than their 3-digit rate of 0.22. Our higher 4-digit rate may have to do with differences between the 1970 and 2002 Census codes. The latter introduced new occupations and industries and also coded existing occupations and industries more finely (especially in services). Thus, there does not appear to be major unexplained differences between our switching rates and those in the more careful, PSID-based study by Kambourov and Manovskii. Finally, while switching rates calculated from the CPS are notoriously too high it is essential to remember that we are not interested in switching levels *per se*. We will be using occupation switching rates in a regression framework with occupation and year fixed effects. Thus, what is potentially problematic for us is the possibility that occupation miscoding varies systematically with inshoring and offshore outsourcing even after controlling

[10] There are three classes of workers: (*i*) private, which includes working in a private for-profit company or being self-employed and incorporated; (*ii*) self-employed but not incorporated; and (*iii*) government employee.
[11] Our results are the same with or without the inclusion of this 'switch-industry' filter.
[12] The upward blip in 2002 results from the reclassification of Census industry and occupation codes that was introduced in 2003. All of our results survive the deletion of 2002 from the sample.

for occupation and year fixed effects. There is little reason to expect such systematic miscoding. Thus, CPS occupation miscoding does not appear to be a major issue in our context. The same holds for industry switching.[13]

Change in Unemployment: Change in unemployment is defined as the change in the number of weeks unemployed as a proportion of total labor force weeks. This definition of unemployment is based on retrospective information from the March CPS and was suggested by Murphy and Topel (1987). Changes in the proportion of labor force weeks spent unemployed appear in column 4 of table 2.

Change in Annual Earnings: Annual earnings are defined as CPI-deflated annual income from wages and salaries. Means for this variable appear in column 7 of table 2.

Worker Characteristic Controls: In the regression analysis we will be including the usual controls for worker characteristics. Sample means for these controls appear in appendix table A.5. Experience is defined as age minus years of schooling minus six. We classify educational attainment into four groups: (1) high-school drop-outs, (2) high-school graduates, (3) college dropouts and (4) college graduates.[14] Over half of both samples are college graduates. We refer collectively to groups (1)-(3) as non-college graduates. We also classify workers into four skill groups: (1) unskilled blue collar, (2) skilled blue collar, (3) less-skilled white collar and (4) skilled white collar. These groups are defined in appendix 3. As shown in appendix table A.5, over 95 percent of our occupation- and industry-exposed samples are white-collar workers. The remaining demographic variables in table A.5 are self-explanatory.

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[13] We are indebted to Gueorgui Kambourov for help with defining occupational switching.

[14] A college graduate has one of the following: a degree from college or occupational/vocational program; an associate degree from college or academic program; a bachelor's degree; a master's degree, a professional school degree, or a doctoral degree.

4. A Simple Difference-of-Difference Analysis

Before presenting our regression-based estimates of the impact of inshoring and offshore outsourcing, we begin with a simple difference-in-differences analysis.[15] We compare the labor market outcomes of service workers who were exposed to inshoring and offshore outsourcing to those who were not. Then we examine whether the difference between the two groups grew over time as inshoring and offshore outsourcing intensified. In particular, we start with the 115,090 matched workers in private service occupations (Census major occupation codes 1-5) and divide this group into those in our occupation-exposed sample (37,550 workers) and the remainder (77,540 workers).

Consider the results for occupation switching in column 2 of table 2. Positive numbers indicate that occupation switching was higher for the exposed sample than for the unexposed sample. However, the differences are not statistically significant (column 3). Further, there is no upward trend over time in column 2. That is, exposed workers did not switch more relative to unexposed workers as inshoring and offshoring intensified.[16]

Turning to the share of labor force weeks spent unemployed, column 5 of table 2 shows that the occupation-exposed sample spent more time unemployed than did the unexposed sample. However, the exposed-unexposed difference is not significant and did not increase over time. The table therefore provides no support for the claim that exposure to inshoring and offshore outsourcing raised the share of weeks in the labor force spent unemployed.

In contrast, table 2 does provide evidence that exposure has lead to lower earnings. From column 8, earnings changes were smaller for the exposed sample than for the unexposed sample

[15] We are grateful to Thomas Lemieux for this and many other suggestions.

[16] Appendix table A.4 provides the corresponding results for industry switching in the industry-exposed sample. For the industry-exposed sample we start with the 78,586 matched workers in private service industries (Census major industry codes 7-12) and divide this group into those in our industry-exposed sample (24,261 workers) and the remainder (54,325 workers).

and this difference has become more pronounced over time. We will see below that this result is driven by occupation switchers.

Section 8.4 revisits this analysis in a regression setting with full controls and finds that none of the table 2 double differences (exposed versus unexposed and 1996-2000 versus 2001-2005) is statistically significant.

The above analysis is more sophisticated than what has appeared in the recent literature on offshore outsourcing. Nevertheless, by academic standards it is too simplistic. First, it does not control for differences in worker characteristics between the exposed and unexposed samples. Appendix table A.5 shows that differences in worker characteristics are important. For example, exposed workers are more likely to be college graduates, skilled white-collar workers, married, male, and white. Second, the above analysis does not control for differences in the degree of exposure: some of the exposed industries have been hard hit while others have been only slightly affected. Third, it does not and can not distinguish between offshore outsourcing from low-wage versus high-wage countries. To deal with these issues we turn to a regression framework.

5. An Econometric Analysis of Switching

5.1. The Econometric Specification

We begin with some notation. Individual i first appears in the sample in March of year $t(i)$ and then appears for the second and last time in March of year $t(i) + 1$. Let $j(i)$ and $o(i)$ be i's 4-digit industry and occupation, respectively, in the longest job held last year i.e., in $t(i) - 1$. We will work with both industry and occupation switching. However, consider industry switching first for the sake of concreteness. Let $Industry\_Switch_{i,t(i)}$ be a binary indicator equal to 1 if i's 4-digit industry of affiliation changes between the longest jobs held in $t(i) - 1$ and $t(i)$. Let

$M^{CI}_{j(i),t(i)-1}$ be U.S. offshore outsourcing to China and India in service industry $j(i)$ in calendar year $t(i) - 1$. Correspondingly, let $X^{CI}_{j(i),t(i)-1}$ be U.S. inshoring from China and India.[17] Let $r(i)$ be i's state of residence in March of year $t(i)$. We estimate a probit of the determinants of switching:

$$\begin{aligned}&\Pr(Industry\_Switch_{i,t(i)}) \\ &\quad = \Phi\left(\theta W_{i,t(i)} + \beta \ln(M^{CI}_{j(i),t(i)-1}) + \gamma \ln(X^{CI}_{j(i),t(i)-1}) + \lambda_{j(i)} + \lambda_{o(i)} + \lambda_{t(i)} + \lambda_{r(i)}\right)\end{aligned} \tag{1}$$

where Φ is the normal cumulative distribution function, $\lambda_{j(i)}$ and $\lambda_{o(i)}$ are industry and occupation fixed effects, $\lambda_{t(i)}$ is a year fixed effect and $\lambda_{r(i)}$ is a state fixed effect. $W_{i,t(i)}$ is a vector of worker characteristics.[18] Note that we are not using a panel: each observation corresponds to a different individual. In what follows we will drop the i arguments of $j(i)$, $o(i)$, $t(i)$ and $r(i)$. Our focus variables are $\ln(M^{CI}_{j,t-1})$ and $\ln(X^{CI}_{j,t-1})$.[19] [20]

An immediate question about the specification is whether inshoring and offshore outsourcing should be entered in levels as we have done or in annual log changes. With annual log changes the sample variation is, somewhat surprisingly, greatest in industries that had consistently low levels of inshoring and offshore outsourcing. This is because small industries tend to have highly variable annual growth rates. For example, the level of offshore outsourcing to China and

[17] Restated, $M^{CI}_{j(i),t(i)-1}$ is unaffiliated-party U.S. imports of service $j(i)$ from China and India and $X^{CI}_{j(i),t(i)-1}$ is unaffiliated-party U.S. exports of service $j(i)$ to China and India.

[18] These are years of experience, experience squared, years of schooling, as well as dummies for sex, race, marital status and veteran status.

[19] The trade data are for the calendar year $t - 1$ while switching occurs sometime during the calendar years $t - 1$ and t. It would also make sense to use trade data for the calendar year t. This is done in section 8.6 where it is shown to slightly improve the precision of our estimates.

[20] While we report probit results, logit results are very similar.

India in the construction-architecture-engineering category is tiny, yet the category had huge annual log changes of between –1.57 and 2.08. Since the sample variation in annual log changes is greatest in industries where levels of inshoring and offshore outsourcing are low, it is not surprising that annual log changes in inshoring and offshore outsourcing are not significant in our switching probits.

The specification in equation (1) – log levels of inshoring and offshore outsourcing together with fixed effects – exploits a very different source of sample variation than does annual changes. An industry that experiences sustained inshoring growth has many years for which inshoring is first well below and then well above the mean level for the industry. Restated, deviations of log levels around the fixed effect are large. The sample variation exploited in equation (1) is therefore driven by sustained growth of inshoring and offshore outsourcing. It is this sustained growth that is most likely to have influenced employer decisions about hiring, firing, and pay and employee decisions about switching.

The usefulness of a specification with log levels and fixed effects stems, then, from the fact that inshoring and offshore outsourcing have been in a prolonged period of sustained growth. Recall that in 1996 there was very little inshoring or offshore outsourcing with China and India. India gained prominence only in the late 1990s when it established itself as a home to cheap programmers who could solve Y2K bugs. Offshore outsourcing of services was not viewed as an essential business practice until the time of the tech bubble. See the counts of media reports in Amiti and Wei (2005*a*). Thus, the big-picture source of sample variation has been the steady rise of inshoring and offshore outsourcing since 1996, not its annual fluctuations. Our equation (1) specification exploits this big-picture sample variation.

5.2. Basic Results

The top panel of table 3 reports the probit estimates of equation (1). For all the probits in this paper, we report marginal probabilities rather than slope coefficients e.g., we report $\partial \Phi / \partial \ln(M^{CI}_{j,t-1})$ rather than β. Consider column 1. It reports the marginal probabilities for the impact of $\ln(M^{CI}_{j,t-1})$ and $\ln(X^{CI}_{j,t-1})$ on industry switching for the industry-exposed sample. A one log point increase in $\ln(M^{CI}_{j,t-1})$ raises the probability of switching by 0.025. Most of the public debate has centered on this offshore outsourcing effect. The often overlooked phenomenon is inshoring i.e., $\ln(X^{CI}_{j,t-1})$. Its marginal probability is –0.031 which indicates that a one log point increase in inshoring reduces the probability of industry switching by 0.031. Before discussing coefficient magnitudes there are a few other features of the table that should be explained. First, coefficients on W_i in equation (1) do not appear in the table, but are reported in appendix table A.6. Second, we do not report the equation (1) fixed effects. Third, since we are examining the effects of industry-level variables on individuals, the standard errors are clustered at the industry level.

Column 2 of table 7 reports the marginal probabilities of 4-digit occupation switching. It differs from column 1 in two subtle but important ways. First, the sample has changed from the industry-exposed sample to the occupation-exposed sample. Second, the trade data are no longer mapped into Census industries. Instead, they are mapped into Census occupations. (Recall that appendix tables A.3 and A.4 report the mappings from the BEA's international trade classification into Census industries and occupations, respectively.)

The remaining columns re-estimate the probits for sub-populations i.e., for college graduates, for all workers excluding college graduates (non-college graduates), for skilled white-collar workers and for less-skilled white-collar workers. For each of these, a column heading of 'Ind.'

means industry switching estimated using the industry-exposed sample and industry-level trade flows. A column heading of 'Occ.' means occupation switching estimated using the occupation-exposed sample and occupation-level trade flows.

There are a large number of marginal probabilities to examine. It is therefore worth starting with some broad conclusions. First, the marginal probabilities for $\ln(M^{CI}_{j,t-1})$ are always positive and the marginal probabilities for $\ln(X^{CI}_{j,t-1})$ are always negative. This means that the incidence of switching is raised by offshore outsourcing and lowered by inshoring. For most specifications the two marginal probabilities are statistically significant and in all but one case they are jointly significant. See the row labeled '*p*-value for joint sig.' Second, the marginal probability for $\ln(M^{CI}_{j,t-1})$ is the same for all worker types: it is about 0.025 for industry switching and 0.015 for occupation switching. In contrast, the marginal probability for $\ln(X^{CI}_{j,t-1})$ is about twice as large for college graduates and skilled white-collar workers as it is for non-college graduates and less-skilled white-collar workers.[21] *This means that non-college workers and less-skilled white-collar workers are taking a hit from offshore outsourcing without getting the benefits from inshoring.*

5.3. Coefficient Magnitudes

Assessing whether the coefficients are large or small can be subjective so we will use several criteria. The median industry in table 1 experienced annual changes in inshoring and offshore outsourcing of about 0.10 log points. Multiplying the marginal probabilities in table 3 by 0.10 yields small impacts. Another way of thinking about magnitudes is to ask what would happen if inshoring and offshore outsourcing trends over the nine-year span of 1996-2005 continued for

[21] To see this either compare the industry switching columns 3 and 7 with 5 and 9 or compare the occupation switching columns 4 and 8 with 6 and 10.

another nine years. From table 1, inshoring would increase by 0.99 log points and offshore outsourcing would increase by 0.18 log points.[22] From column 1 of table 3, this implies that offshore outsourcing raised the incidence of switching by a tiny 0.005 (= 0.025 × 0.18) while inshoring reduced the incidence by a much larger –0.031 (= –0.031 × 0.99). Applying this analysis to all columns in table 3, it is immediately obvious that the inshoring effect dominates the offshore outsourcing effect so that the net impact of inshoring and offshore outsourcing is to reduce the incidence of switching.

A very different way of assessing coefficient magnitudes is what is best described as a fear-mongering experiment. It will be used repeatedly in what follows. From table 1, the growth of offshore outsourcing has been spectacular in business, professional and technical services (BPT). If all inshoring and offshore outsourcing grew over the next nine years as it did for BPT during 1996-2005, then from table 1 offshore outsourcing would increase by 1.45 log points (≈ 0.16 × 9) and inshoring would increase by 1.23 log points (≈ 0.14 × 9). We emphasize that this is a fear-mongering exercise because it extrapolates based on the most rapidly growing major segment of service trade. Such rapid growth would result in a 0.002 *reduction* in the incidence of industry switching: 0.002 = (0.025 × 1.45) – (0.031 × 1.23). This result is reported in the row labeled 'Rapid-Growth Effect.' (We find this to be a more neutral term than the 'fear-mongering effect.') Looking across columns, the rapid-growth effect is almost always negative. The two exceptions are for non-college and less-skilled white-collar industry switchers. Thus, even a fear-mongering exercise yields overall reductions in switching.

Since the rapid-growth effects are not statistically significant, we can take fear-mongering the extra mile by looking at the upper bound of the 95 percent confidence intervals for the rapid-growth effect. This gives us an upper limit on the worst that could happen to American workers

[22] Multiply by 9 the numbers for China and India in the 'total' row of table 1 i.e., 0.99 = 0.11×9 and 0.18 = 0.02×9.

under the already extreme rapid-growth scenario: industry switching would rise by 0.032 and occupation switching would rise by 0.007. These strike us as small increases in switching for a worst-case-plus scenario that stretches over nine years.

5.4. Isolating the Low-Wage Effect

As discussed earlier, there are two reasons for the growth of inshoring and offshore outsourcing with China and India. The first is technological change in ICT which allowed all countries to engage in inshoring and offshore outsourcing. The second is economic liberalization in China and India which allowed workers in these low-wage countries to compete with skilled American workers. It is the latter aspect – low-wage competition for skilled white-collar jobs – that has caught the public's attention. It is therefore of interest to isolate it. Let $M^{G8}_{j,t-1}$ be U.S. offshore outsourcing to G8 countries in industry *j* in year $t - 1$ and let $X^{G8}_{j,t-1}$ be the corresponding U.S. inshoring from the G8. In the lower panel of table 3, we re-estimate equation (1) after replacing our trade measures with $\ln\left(M^{CI}_{j,t-1} / M^{G8}_{j,t-1}\right)$ and $\ln\left(X^{CI}_{j,t-1} / X^{G8}_{j,t-1}\right)$. The rationale for dividing by G8 flows is that technological change in ICT led to an expansion of inshoring and offshore outsourcing to *all* countries so that $\ln\left(M^{CI}_{j,t-1} / M^{G8}_{j,t-1}\right)$ captures that component of offshore outsourcing that is unique to China and India i.e., that is driven primarily by endowments of low-paid skilled labor.

To evaluate this rationale for using the G8 as a control, we constructed a rudimentary measure of the ICT costs of outsourcing, ICT_{jt}, which builds on work by Bartel, Lach and Sicherman (2005) and McKinsey Global Institute (2003). See appendix 4 for details. As shown in appendix figure A.1, ICT_{jt} is highly correlated with $\ln(M^{CI}_{j,t-1})$ ($t = 7.40$) and $\ln(X^{CI}_{j,t-1})$ ($t = 6.31$). On the other hand, ICT_{jt} is uncorrelated with $\ln\left(M^{CI}_{j,t-1} / M^{G8}_{j,t-1}\right)$ (t = 1.82) and $\ln\left(X^{CI}_{j,t-1} / X^{G8}_{j,t-1}\right)$ (t = 0.69). This supports our claim that $\ln\left(M^{CI}_{j,t-1} / M^{G8}_{j,t-1}\right)$ and $\ln\left(X^{CI}_{j,t-1} / X^{G8}_{j,t-1}\right)$ purges pure ICT effects and captures what is unique about China and India, namely, low wages for skilled workers.

From the bottom panel of table 3, the coefficients on inshoring and offshore outsourcing have the expected signs in all but one case and are almost all statistically significant. The magnitudes have changed somewhat and this is best summarized by the rapid-growth effect row. Little has changed for the occupation-exposed sample, but the rapid-growth effect for the industry-exposed sample is more negative than in the upper panel. There is thus more consistency now between the industry and occupation rapid-growth effects. More importantly, the big picture remains the same: a small or zero reduction in switching due to inshoring and offshore outsourcing.

5.5. Endogeneity

A final issue is the endogeneity of the service trade flows in the upper panel of table 3. It is possible that technological change in ICT is driving changes in trade flows *and* having independent effects on switching. If so then the trade variables are spuriously picking up the effects of ICT technology change. Since we know from section 5.4 and appendix 4 that developments in ICT are uncorrelated with $\ln\left(M^{CI}_{j,t-1} / M^{G8}_{j,t-1}\right)$ and $\ln\left(X^{CI}_{j,t-1} / X^{G8}_{j,t-1}\right)$, we can use these as instruments for $\ln(M^{CI}_{j,t-1})$ and $\ln(X^{CI}_{j,t-1})$. This leaves us with a just-identified model. The first-stage regressions appear in appendix A.7. The second-stage probits appear in columns 1 and 2 of table 4. We use the two-stage IV procedure described in Wooldridge (2001, p. 474). The 'Exogeneity' rows of table 4 indicate that we cannot reject endogeneity. For industry switching, the OLS and IV marginal probabilities of inshoring and offshore outsourcing are nevertheless almost identical. For occupation switching, the IV results are larger than the OLS results, but the inshoring marginal probability remains much larger than the offshore outsourcing marginal probability so that the rapid-growth effect for IV (-0.019) and OLS (-0.023) are very similar. In short, both the OLS and IV estimates imply that inshoring and offshore outsourcing had no net effect on industry switching (the industry-exposed sample), but did reduce occupation switching (the occupation-exposed sample).

5.6. Sensitivity Analysis

Our results will turn out to be insensitive to a large variety of specification changes. These include broadening the list of low-wage countries beyond just China and India (table 7), deleting the technology-bubble years (table 8), correcting for CPS sample selection (table 9), including all private sector workers rather than just those exposed to offshore outsourcing and inshoring (table 10), using different definitions of switching (table 11) and more. We defer this discussion until section 8.

6. An Econometric Analysis of Unemployment

We next turn to impacts on weeks spent unemployed as a share of total weeks in the labor force. This measure of unemployment was proposed by Murphy and Topel (1987). We estimate the following linear regression:

$$\Delta\left(\frac{unemployed\ weeks_{it}}{weeks\ in\ labor\ force_{it}}\right) = \theta\left(\Delta W_{it}\right) + \beta \ln(M^{CI}_{j,t-1}) + \gamma \ln(X^{CI}_{j,t-1}) + \lambda_j + \lambda_o + \lambda_t + \lambda_r + \varepsilon_{it} \qquad (2)$$

where the dependent variable is the change between $t-1$ and t in the share of labor force weeks spent unemployed. Since we are working in changes, we first difference W_{it}. This leaves only experience squared in ΔW_{it}.[23]

Since we will be arguing that the estimates of β and γ in equation (2) are small, we will need a benchmark for coefficient magnitudes. Let σ_U, σ_X and σ_M be the standard deviations of the dependent variable, $\ln(M^{CI}_{j,t-1})$ and $\ln(X^{CI}_{j,t-1})$, respectively. One modest benchmark is to require that one-standard-deviation changes in inshoring or offshore outsourcing change the dependent variable by at least $\sigma_U/10$. This implies coefficients for inshoring of at least 0.01 in absolute value and coefficients for offshore outsourcing of at least 0.008.[24] An alternative modest benchmark would be an effect that reduced the share of time spent unemployed by at least 0.01 over nine years (i.e., over 1996-2005). This implies coefficients on inshoring of at least 0.008 in absolute value and coefficients on offshore outsourcing of at least 0.007. Summarizing, both modest benchmarks lead one to expect coefficients of at least 0.007 in absolute value. Any coefficient less than this would have to be considered tiny relative to all the hype surrounding offshore outsourcing.

A quick perusal of table 5 reveals that every coefficient is less than 0.007 in absolute value. Further, even though statistical significance is often very low, every coefficient has a 1 percent confidence interval that lies strictly within the interval (–0.007, 0.007). *Thus, we have precisely estimated very small effects of inshoring and offshore outsourcing on the share of labor-force weeks spent unemployed*.

The rapid-growth effects are mostly negative and, where statistically significant, always negative. This means that the net of effect of inshoring and offshore outsourcing has been to

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[23] The only time-varying variables in W_{it} are experience and experience squared. The change in experience is always 1 and so ends up in the intercept.

[24] $\sigma_U = 0.11$, $\sigma_X = 0.97$ and $\sigma_M = 1.43$ so that $\sigma_U / \sigma_X = 0.11$ and $\sigma_U / \sigma_M = 0.08$. These last two numbers are then divided by 10.

reduce the share of labor-force weeks spent unemployed. Further, the rapid growth effects are always less than 0.007 in absolute value and so are small as judged by our benchmarks.

The IV results appear in columns 3 and 4 of table 4 and do not alter these conclusions. For the industry sample, the IV and OLS coefficients are the same size but the IV coefficients are much more significant. For the occupation sample, the coefficients are insignificant for both IV and OLS. To conclude, inshoring and offshore outsourcing combined likely reduced time spent unemployed and the net effect is precisely estimated to be small.

7. An Econometric Analysis of Earnings

There is by now a very large empirical literature analyzing the impact of trade openness on wage levels and the distribution of income e.g. Borjas, Freeman and Katz (1992), Lawrence and Slaughter (1993), Gaston and Trefler (1994, 1997), and Feenstra and Hanson (1996, 1999, 2002). All of these are limited to manufacturing. For the effect of exchange rates on both manufacturing and services see Goldberg and Tracy (2003). We estimate the effect of service inshoring and offshore outsourcing on earnings using the following linear regression:

$$\Delta \ln(earnings_{it}) = \theta \Delta W_{it} + \beta \ln(M^{CI}_{j,t-1}) + \gamma \ln(X^{CI}_{j,t-1}) + \lambda_j + \lambda_o + \lambda_t + \lambda_r + \varepsilon_{it} \qquad (3)$$

Table 6 reports the results. Consider column 1 first. It reports the results for the industry-exposed sample using industry-level imports. The coefficient on offshore outsourcing is small and implies that even if $\ln(M^{CI}_{j,t-1})$ grew for nine-years at 16 percent a year, as in the rapid-growth effect, earnings would only fall by 0.01 percent (= –0.0001×0.016×9). This is tiny. The corresponding number for $\ln(X^{CI}_{j,t-1})$ is an earnings rise of 0.73 percent (= 0.006×0.014×9).

When the results are broken down by education we find small but precisely estimated effects for college graduates. For non-college graduates we obtain a surprising reversal of signs. This is possibly explained by selection. To explore, we distinguish between switchers and non-switchers. Column 7 reports results for workers who did not switch industries. For these workers, inshoring had a small effect on earnings, offshore outsourcing had no effect and the rapid-growth effect was small (a nine-year earnings increase of 0.7 percent, t = 3.28). Column 8 reports results for workers who switched from one exposed industry to another. Now the expected sign pattern appears but the effects are small and insignificant. Column 9 reports results for workers who switched from an exposed industry to an unexposed industry. There is no effect of either inshoring or offshore outsourcing for these workers.[25]

These results for switchers versus non-switchers are interesting, but do not explain why non-college graduates have an unexpected sign on the coefficient for offshore outsourcing. Column 10 points to the role of young workers. Workers between the ages of 18 and 24 are the source of the unexpected sign.

Returning to the big picture, all the table 6 estimated coefficients are small and have sufficiently small standard errors that we can have some confidence in claiming that inshoring and offshore outsourcing have had either zero or small positive effects on earnings, selection notwithstanding.

[25] Appendix table A.8 presents a transition matrix that tracks where switchers go and what this means for their earnings. 30 percent of workers in the industry-exposed sample switch their 4-digit industries. 10 of these 30 percentage points involve workers who move to other exposed industries. These workers see their earnings *rise* by 0.01 log points in the year of transition. This is likely due to selection: at least some of the workers who move from one exposed industry to another do so because of better prospects in the new industry. Another 9 of these 30 percentage points involve workers who move to unexposed service industries. They experience an earnings loss of 0.11 log points. Finally, 4 of these 30 percent involve workers who switch to wholesale and retail trade and take an earnings hit of 0.15 log points. Somewhat similar numbers hold for occupation switchers in the occupation-exposed sample. See appendix table A.8 for details.

8. Sensitivity to Alternative Specifications

We have precisely estimated small effects of inshoring and offshore outsourcing on occupation switching, industry switching, weeks unemployed as a share of labor force weeks, and earnings. The remainder of this paper shows that our results are not sensitive to (1) the inclusion of other low-wage countries, (2) the exclusion of the years in which the technology bubble collapsed (2000-2001), (3) modeling the selection of workers into our March-to-March matched sample, (4) the inclusion of all private service workers in the analysis, not just exposed private service workers, (5) alternative definitions of our dependent variables (1-digit switching, transitions from employment to unemployment, hourly wages), (6) the use of contemporaneous trade flows rather than lagged trade flows, and (7) exclusion of all industries except business, professional and technical services. The results appear in tables 7-13 and are quite stable across specifications. In the few instances where an estimate becomes significant where previously it was not (at the 1 percent level) or becomes insignificant where previously it was significant, this is flagged in boldface in tables 7-13. Since our results are similar for each of these alternative specifications reported below, there will be no new important insights and a reader who already believes that our results are robust can jump straight to the conclusions.

8.1. Beyond China and India: Expanding the Set of Low-Wage Countries

When it comes to U.S. trade in services, China and India are by far the major low-wage trading partners. The BEA also publishes bilateral service trade data for all countries that have significant service trade with the United States. Among low-wage countries, data are available for China, India, Indonesia, Malaysia, the Philippines and Thailand. We therefore redefine $M^{CI}_{j,t-1}$ and $X^{CI}_{j,t-1}$ to be service trade with all six of these low-wage countries. Table 7 reports the results of re-estimating equations (1)-(3) with this redefinition. Columns 1-3 correspond to column 1 of tables 3, 5 and 6, respectively. Columns 4-6 correspond to column 2 of tables 3, 5 and 6, respectively. From the bold-faced numbers, significance changes slightly in only three cases. All three have the right sign. Two become significant (columns 3 and 4) and one becomes insignificant (column 6). In all cases there is virtually no change in coefficient magnitudes.

8.2. Sensitivity to the Technology Bubble

NASDAQ began its precipitous decline in March 2000 and continued to decline until mid-2002. As noted by Mann (2003), early research on offshore outsourcing was unable to disentangle the effects of offshore outsourcing from the effects of the bursting bubble. We now have a longer data series with more post-bubble data. Nevertheless, to eliminate the effects of the bubble we delete all data for the years 2000 and 2001. Table 8 reports the results. The one change is an earnings coefficient that becomes insignificant. (See the boldfaced estimate.) However, our main conclusions are unchanged.

8.3. Sample Selection Resulting from Unmatched Workers

To be in our matched sample a worker must remain in the same dwelling from March of year t to March of year $t + 1$. Since offshore outsourcing may encourage workers to move in search of jobs, our sample may not be randomly chosen and our estimates may be tainted by sample selection bias. See Neumark and Kawaguchi (2004) and Goldberg and Tracy (2003). In this section we use maximum likelihood to simultaneously estimate two equations, a selection equation and a second-stage equation (switching, unemployment or earnings). Our specification of the selection equation borrows from the migration literature which shows that mobility is strongly tied to family characteristics that have been excluded from our second-stage equations. These are family size, number of children, home ownership and whether the individual has a recent history of moving as proxied by whether the individual lived in the same house last year.[26] These instruments are drawn from responses in the first of the two March surveys.

The estimates appear in table 9. Estimates of the second-stage equations appear in the top panel while estimates of the selection equation appear in the bottom panel. The Wald test for selection is reported in the row labeled 'Wald test of indep. Eqns.' and indicates that in almost every case selection bias cannot be rejected.[27] Selection affects the fixed effects and the worker-characteristic coefficients, but does not affect our estimates of inshoring and offshore outsourcing. The latter estimates do not change in any economically significant way.

8.4. Exploiting Differences between Exposed and Unexposed Workers

In section 4, we characterized industries with a binary variable indicating whether or not the industry was exposed to inshoring and offshore outsourcing. We then compared the labor-force outcomes of exposed and unexposed workers. In sections 5-7, we characterized exposed industries with continuous measures of exposure ($\ln(M^{CI}_{j,t-1})$ and $\ln(X^{CI}_{j,t-1})$). We then dropped unexposed workers from the analysis and regressed the outcomes of exposed workers on $\ln(M^{CI}_{j,t-1})$ and $\ln(X^{CI}_{j,t-1})$. In this section we combine the two approaches by including unexposed workers in our regression analysis.

[26] In the first of the two March surveys the individual is asked if he or she lived in the same house last year. The correlation of this response with whether the individual is matched across March surveys is 0.14. This is a small correlation and our results are unchanged when this variable is removed from the instrument set.

[27] The Wald test is calculated by comparing the results in table 9 with the results of fitting the second-stage equation without a selection correction.

To this end we introduce an exposure dummy for whether the individual is in the exposed sample (D = 1) or the unexposed sample (D = 0). We also introduce a period dummy for whether or not the year is in the second half of our sample where offshore outsourcing was at far higher levels. The period dummy equals 1 in 2001-2006 and 0 in 1996-2000. To obtain a double difference we interact the exposure and period dummies. We expect the interaction to be negative for earnings, which means that the earnings of exposed workers relative to unexposed workers have declined in recent years as offshore outsourcing intensified. Likewise, we expect the interaction term to be positive for switching and unemployment. The results appear in table 10 where the interactions are statistically insignificant everywhere. See the rows labeled 'Diff-of-Diff'.[28] Thus, including workers in unexposed industries does not affect our conclusions.

8.5. Alternative Dependent Variables

So far, all of our switching results were based on 4-digit industry switching. In columns 1 and 4 of table 11 we report results for 1-digit switching. We expect these estimates to be smaller and indeed they are about half the size of the estimates for 4-digit switching. Interestingly, the inshoring coefficient for the industry-exposed sample now becomes significant. Thus, even at the 1-digit switching level it remains true that the growth of inshoring and offshore outsourcing has been associated with small but statistically significant (t = –5.84) reductions in industry switching.

So far, we have defined changes in unemployment in terms of changes in the proportion of labor-force hours spent unemployed. An alternative definition is a binary indicator of

[28] The estimates for $\ln(M^{CI}_{j,t-1})$ and $\ln(X^{CI}_{j,t-1})$ do not change much from our baselines: the rapid growth effect continues to imply small but positive impacts of inshoring and offshore outsourcing. There is the issue of what to do about the trade flows of unexposed workers since by definition these workers have no trade in their industries and occupations. We therefore set the inshoring and offshore outsourcing variables to zero for unexposed workers. (It does not matter that it is zero because the fixed effects absorb any time-invariant constant.)

transitioning from employment to unemployment. If the worker is employed in the March reference week of year *t* and becomes unemployed in the March reference week of year *t* + 1, we assign 1 to the binary indicator of unemployment. If the worker is employed in the March reference week of year *t* and the March reference week of year *t* + 1, we assign 0 to the binary indicator of unemployment. A worker who is unemployed in the March reference week of year *t* is dropped from the sample regardless of employment status in *t* + 1. (Unemployed workers in *t* are dropped only in this subsection.) Columns 2 and 5 of table 11 present estimated marginal probabilities of a probit for transitioning to unemployment. The only major change from our previous results is that inshoring now significantly reduces transitions into unemployment for both the industry- and occupation-exposed samples. However, the coefficients remain economically small.

We also re-estimated our earnings regressions using the change in real hourly wages as the dependent variable. Hourly wages are defined as real annual earnings divided by hours worked last year. Hours worked last year is weeks worked last year times hours worked each week. The inshoring coefficient in column 3′ is no longer significant (the industry-exposed sample). However, all of the coefficients remain as small as in our baseline specification.

8.6. Contemporaneous Rather than Lagged Trade Variables

The dependent variables typically involve changes between year $t-1$ and *t*. To reduce issues of endogeneity, we lagged the trade variables so that $\ln(M^{CI}_{j,t-1})$ and $\ln(X^{CI}_{j,t-1})$ were used in place of $\ln(M^{CI}_{jt})$ and $\ln(X^{CI}_{jt})$. In table 12, we re-estimate our equations (1)-(3) using contemporaneous trade variables rather than lagged ones. As is apparent from the table, this tends to raise *t*-statistics without altering coefficient magnitudes.

8.7. Business, Professional and Technical (BPT) Services

Much of the press about offshore outsourcing focuses on BPT services to the exclusion of the other service categories in table 1 such as financial and insurance services. That is, the press focuses on services for which U.S. comparative advantage is relatively weak. Table 13 presents estimates of our equations (1)-(3) when only the eight BPT service subcategories are included in the analysis. One might expect this to increase the coefficients on offshore outsourcing and decrease the coefficients on inshoring. This does not happen. Indeed, for industry and occupation switching the reverse happens. Consequently, the net effect of inshoring and offshore outsourcing on switching is now more favorable to American workers. This can be seen from the fear-mongering, rapid-growth effect which implies a large reduction in switching: –0.035 ($t = -11.45$) for industry switching and –0.065 ($t = -1.94$) for occupation switching.

8.7. Conclusions from Sensitivity Analysis

As is apparent from tables 7-13, our main conclusions survive the many alternative specifications presented in this section.

9. Conclusions

The rise of service offshore outsourcing to China and India has brought with it something new – for the first time ever, educated U.S. workers are competing with educated but low-paid foreign workers. Despite the public concern about this development, there has been almost no econometric work on the subject. Most previous studies have simply counted the number of workers who are in industries or occupations that are exposed to offshore outsourcing. Many (but not all) of these studies then conclude from the resulting large counts that bad things must be happening to American workers. We came up with similar large counts using CPS data, but then went on to ask about actual as opposed to conjectured effects on U.S. labor markets. In so doing we emphasized that one cannot ignore the reverse flow (inshoring), which is the sale of services produced in the United States to unaffiliated buyers in China and India.

Using March-to-March matched CPS data for 1996-2006 we examined the impacts of inshoring and offshore outsourcing on (1) occupation and industry switching, (2) weeks spent unemployed as a share of weeks in the labor force, and (3) earnings. We *precisely* estimated small positive effects of inshoring and smaller negative effects of offshore outsourcing. The net effect of inshoring and offshore outsourcing was positive. We quantified this net effect using something of a fear-mongering experiment which we called the rapid-growth effect. Suppose that over the next nine years inshoring and offshore outsourcing continued to grow at rates experienced during 1996-2005 in business, professional and technical services i.e., in segments where China and India have been particularly strong. Then for workers in occupations that are exposed to offshore outsourcing (*i*) 4-digit occupational switching would *decline* by 2 percent, (*ii*) the share of weeks spent unemployed would *fall* by 0.1 percent and (*iii*) earnings would *rise* by 1.5 percent. That is, American workers on average would benefit. Of course, these numbers are not annual changes – they are changes over nine years – and thus represent small benefits. The important take away is that this extreme, fear-mongering exercise does not produce adverse impacts on U.S. workers.

There are some darker spots in the U.S. labor market experience with offshore outsourcing. For workers in *industries* exposed to offshore outsourcing, the effects tend to be smaller (i.e. less positive). The effects tend to be negative for workers without a college degree or who work in less-skilled, white-collar jobs. As with all labor-market impacts of international trade, there are winners and losers and, in the current U.S., the losers are less educated. We do not want to minimize the effect on losers. The loss of one's job can be enormously damaging both financially and psychologically.

These darker spots should not be allowed to obscure the big picture. All of these effects are remarkably small given the hype associated with offshore outsourcing. Further, they were estimated with sufficient precision that even the upper bounds of 95 percent confidence intervals involved small effects. The estimates were also shown to hold across a wide variety of alternative specifications (section 8). There can thus be only one way of describing the hype surrounding the labor-market impacts of inshoring and offshore outsourcing: Much Ado About Nothing.

Appendix 1. Trade Data

All data are from the 'other private services' category of the BEA database. We exclude (*i*) Installation, maintenance, and repair of equipment, (*ii*) Education, and (*iii*) Other because these categories are difficult to concord into industries and occupations. In this we follow van Welsum and Vickery (2005).

In 2003, the CPS updated its industry and occupation classifications from 1990 Census codes to 2002 Census codes. To ensure that codes are consistent over our entire sample we converted the 1990 Census codes into 2002 Census codes. We then linked 2002 Census industry or occupation codes with BEA types of service trade. In order to do this as accurately as possible we used (*i*) the 2002 NAICS manual for detailed industry definitions and the 2000 SOC manual for detailed occupation definitions, and (*ii*) Borga and Mann (2004) and U.S. Department of Commerce (1998) for detailed information about the coverage of each type of trade in services.

Our measures of inshoring and offshore outsourcing in services come from published BEA data on U.S. international services cross-border trade and sales through affiliates. Data for early years are sporadically missing. This could either be because values of less than 0.5 million dollars are suppressed or because of disclosure concerns. The two likely go hand in hand: even a quick look through the data for each industry shows that when data are missing in a year there are usually neighboring years with data and these data involve very small values of trade. We therefore used linear interpolation to fill in missing data. However, none of our main results change when we restrict ourselves to non-imputed data.

Appendix 2. Switching

Responses to questions about industry and occupation in the longest job held last year are known to be frequently miscoded. This leads to overestimation of switching. We therefore clean up the raw switching data using the yearly equivalent of the criteria in Moscarini and Thomsson (2006). Specifically, a switch is valid only if at least one of the following three events occurred. (1) The class of worker changed. (2) There was job search during the period.[29] (3) For an industry (occupation) switch the occupation (industry) changed. Note that in most cases, criterion (3) was satisfied only when either (1) or (2) were satisfied. That is, criterion (3) does not have much bite. On a separate issue, see appendix 1 for a discussion of 2003 changes in Census definitions of occupations and industries.

Appendix 3. Skills

Skills are defined by 1-digit Census major occupations. (1) *Unskilled blue-collar workers*: farming, fishing, and forestry occupations; construction and extraction occupations; production occupations; transportation and material moving occupations. (2) *Skilled blue-collar workers*: installation, maintenance, and repair occupations. (3) *Less-skilled white-collar workers*: service occupations; sales and related occupations; office and administrative support occupations. (4) *Skilled white-collar workers*: management, business and financial occupations; professional and related occupations.

Appendix 4. The Relationship of ICT to Inshoring and Offshore Outsourcing

We have argued that $\ln\left(M_{jt}^{CI}\right)$ and $\ln\left(X_{jt}^{CI}\right)$ are determined in part by technical change in ICT

[29] In the variable coding of LOOKED, a worker looked for a job last year if she worked last year (_WORKYN=1), was a part-year worker (1<=WKSLYR<=51) and looked for work last year (LKEDPY=0).

and in part by political developments in China and India that led to an opening up of these economies. We also argued that the technical change aspects could be controlled for by $\ln\left(M_{jt}^{G8}\right)$ and $\ln\left(X_{jt}^{G8}\right)$. The argument was that if trends in inshoring and offshore outsourcing were exclusively driven by technical change then they might be expected to affect all trading partners equally. In particular, we argued that trends in $\ln\left(M_{jt}^{CI} / M_{jt}^{G8}\right)$ and $\ln\left(X_{jt}^{CI} / X_{jt}^{G8}\right)$ were driven primarily by political developments in China and India. To assess this claim we introduce a rudimentary measure of the importance of ICT change in industry *j*. For service industry *j* let $ICT\_Intensity_j$ be investment in ICT equipment and software divided by total new equipment and software investment. This measure was suggested in U.S. Department of Commerce (2003) and is used by Bartel, Lach and Sicherman (2005) in their examination of outsourcing and technological change. We extracted the data for $ICT\_Intensity_j$ from the BEA 1997 capital flow table. Let $Telecom\_Cost_t$ be a time trend that captures technical change over time. We obtain almost identical results whether we use a time trend or the annual leasing cost of a 2 Mbps fiber cable between New York and Mumbai (McKinsey Global Institute, 2003). Our measure of ICT in industry *j* in year *t* is:

$$ICT_{jt} = ICT\_Intensity_j \times Telecom\_Cost_t \ .$$

The top panels of figure A.1 plot $\ln\left(M_{jt}^{CI}\right)$ and $\ln\left(X_{jt}^{CI}\right)$ against ICT_{jt} . Each point is an industry (see table 1) in one of the 10 years 1996-2005. From the graphs, the correlations are highly significant: $t = 7.40$ for $\ln\left(M_{jt}^{CI}\right)$ and $t = 6.31$ for $\ln\left(X_{jt}^{CI}\right)$. The bottom panels of figure A.1 plot $\ln\left(M_{jt}^{CI} / M_{jt}^{G8}\right)$ and $\ln\left(X_{jt}^{CI} / X_{jt}^{G8}\right)$ against ICT_{jt}. From the plots it is apparent that there is no relationship: $t = 1.82$ for $\ln\left(M_{jt}^{CI} / M_{jt}^{G8}\right)$ and $t = 0.69$ for $\ln\left(X_{jt}^{CI} / X_{jt}^{G8}\right)$. This supports our claim that the ratio of China-India activity relative to G8 activity is orthogonal to ICT technical change. The ratio is instead driven by political economy developments unique to China and India.

Finally note that if we include ICT_{jt} directly into our regressions it has no effect on our estimates.

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Table 1. U.S. Unaffiliated Cross-Border Imports and Exports of Other Private Services, Average Annual Log Changes, 1996-2005

	U.S. Exports (Inshoring)					U.S. Imports (Offshore Outsourcing)				
	2005	Average Annual Log Change				2005	Average Annual Log Change			
	World	World	China & India	G8	CI - G8	World	World	China & India	G8	CI - G8
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Business, professional, and technical services	33,637	0.08	**0.14**	0.09	0.04	13,490	0.10	**0.16**	0.10	0.06
Computer and information service	6,039	0.09	0.07	0.11	-0.04	2,469	0.20	0.39	0.22	0.18
Legal services	4,306	0.09	0.07	0.08	-0.01	914	0.04	0.01	0.05	-0.04
Construction, architecture and engineering	4,080	0.02	0.13	0.08	0.06	422	-0.01	0.13	0.00	0.13
Industrial engineering	2,327	0.11	0.01	0.11	-0.10	174	-0.01	0.10	0.00	0.10
Management consulting and public relations	2,219	0.05	0.12	0.02	0.10	1,694	0.14	0.22	0.12	0.09
Research, development and testing services	1,295	0.07	0.12	0.09	0.03	2,317	0.20	0.33	0.24	0.09
Advertising	606	0.01	-0.03	0.00	-0.02	1,005	0.00	0.04	-0.02	0.06
Other BPT services	12,765	0.13	0.19	0.12	0.07	4,495	0.10	0.06	0.07	-0.01
Financial services	29,281	0.14	0.11	0.14	-0.03	6,549	0.09	0.07	0.08	-0.01
Insurance	6,831	0.16	0.17	0.15	0.02	28,482	0.18	-0.08	0.11	-0.19
Telecommunications	4,724	0.04	-0.02	0.06	-0.07	4,658	-0.06	-0.10	-0.01	-0.09
Total	74,473	0.10	0.11	0.11	0.00	53,179	0.10	0.02	0.09	-0.07

Notes: Columns 1 and 6 are in millions of dollars. All other columns are average annual log changes, 1996-2005. The two numbers in bold are used for the 'rapid-growth effect' described in section 5.3.

Table 2. Summary Statistics and a Simple Differencing Approach

	Occupation-Exposed Sample (*N* = 37,550)								
	4-Digit Occupation Switching			Change in Unemployment			Change in Earnings		
	Exposed	Exposed - Unexposed		Exposed	Exposed - Unexposed		Exposed	Exposed - Unexposed	
	Mean	Mean	*t*	Mean	Mean	*t*	Mean	Mean	*t*
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1996	0.28	-0.006	-0.67	-0.003	0.002	0.60	0.047	-0.002	-0.09
1997	0.28	0.002	0.20	-0.003	0.002	0.76	0.047	-0.019	-1.07
1998	0.30	0.019	2.01	-0.002	0.003	1.30	0.057	-0.052	-3.17
1999	0.31	0.023	2.47	-0.001	0.002	0.79	0.014	-0.035	-2.18
2000	0.31	0.020	2.09	-0.002	0.001	0.40	0.042	-0.031	-1.83
2001	0.30	0.006	0.73	0.006	0.002	1.09	0.036	-0.030	-2.00
2002	0.36	0.011	1.16	0.005	0.003	1.41	-0.005	-0.043	-2.75
2003	0.31	0.000	-0.01	-0.003	0.000	-0.15	-0.017	-0.048	-3.35
2004	0.30	0.002	0.24	-0.002	0.003	1.21	-0.018	-0.052	-3.43
2005	0.31	0.013	1.51	-0.003	0.003	1.10	-0.002	-0.054	-3.64
All Years: *n*-digit occupation switching									
4-digit	0.31	0.009	3.03						
2-digit	0.20	-0.020	-7.79						
1-digit	0.17	-0.021	-8.40						

Notes: 'Year' is the year of the individual's first March survey. Columns 1, 4, and 7 report means for the occupation-exposed sample. Columns 2, 5, and 8 report the difference of means for the exposed sample less the unexposed sample. The unexposed sample consists of private workers in service occupations that are not exposed to inshoring and offshore outsourcing. See section 4 for details. Columns 3, 6, and 9 report *t*-statistics for the differences. The top panel reports occupation switching at the 4-digit level. The bottom panel reports occupation switching at other levels.

Table 3. Probits for 4-digit Industry and Occupation Switching

	All Workers		College Graduates		Non-College Graduates		Skilled White Collar		Less-Skilled White Collar	
	Ind.	Occ.	Ind.	Occ.	Ind.	Occ.	Ind.	Occ.	Ind.	Occ.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\ln(M^{CI})$	0.025	0.013	0.025	0.012	0.025	0.018	0.023	0.015	0.020	0.011
	(7.01)	(2.15)	(5.93)	(1.36)	(2.53)	(3.97)	(7.59)	(2.12)	(2.43)	(4.51)
$\ln(X^{CI})$	-0.031	-0.033	-0.037	-0.036	-0.021	-0.027	-0.037	-0.039	-0.016	-0.020
	(-1.95)	(-3.34)	(-2.36)	(-2.89)	(-1.17)	(-3.17)	(-2.45)	(-4.27)	(-1.01)	(-0.60)
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.000	0.000	0.000	0.008	0.000	0.000	0.000	0.000	0.000	0.550
Rapid-growth effect	-0.002	-0.023	-0.009	-0.027	0.011	-0.008	-0.012	-0.026	0.009	-0.010
	(-0.14)	(-1.44)	(-0.45)	(-1.41)	(1.23)	(-0.52)	(-0.65)	(-1.70)	(0.96)	(-1.20)
Pseudo R^2	0.047	0.028	0.046	0.026	0.061	0.040	0.046	0.028	0.052	0.037
N	24,261	37,550	13,499	18,679	10,762	18,871	13,848	18,946	9,119	18,604

	(1')	(2')	(3')	(4')	(5')	(6')	(7')	(8')	(9')	(10')
$\ln(M^{CI}/M^{G8})$	0.015	0.021	0.018	0.017	0.009	0.028	0.014	0.021	0.004	0.018
	(10.28)	(5.84)	(4.87)	(2.90)	(3.40)	(7.33)	(5.29)	(5.82)	(1.38)	(5.01)
$\ln(X^{CI}/X^{G8})$	-0.033	-0.045	-0.046	-0.047	-0.008	-0.037	-0.041	-0.050	0.005	-0.039
	(-3.06)	(-3.64)	(-5.37)	(-2.70)	(-0.57)	(-3.47)	(-4.55)	(-3.32)	(0.41)	(-1.54)
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.000	0.000	0.001	0.000	0.000	0.000	0.000	0.000	0.249	0.120
Rapid-Growth Effect	-0.019	-0.025	-0.030	-0.033	0.004	-0.005	-0.030	-0.032	0.012	-0.022
	(-1.25)	(-1.60)	(-1.93)	(-1.39)	(0.30)	(-0.37)	(-2.02)	(-1.56)	(0.84)	(-1.19)
Pseudo R^2	0.046	0.028	0.046	0.026	0.060	0.041	0.045	0.027	0.051	0.037
N	24,261	37,550	13,499	18,679	10,762	18,871	13,848	18,946	9,119	18,604

Notes: Each column presents two separate probits, the top panel with M^{CI} and X^{CI} and the bottom panel with M^{CI}/M^{G8} and X^{CI}/X^{G8}. Each probit includes worker characteristics (experience, experience squared, years of schooling and dummies for sex, race, marital status and veteran status) as well as fixed effects for 2-digit industries, 2-digit occupations, year and state. In odd-numberd columns the dependent variable is 4-digit *industry* switching and the sample is the *industry-exposed* sample. In even-numberd columns the dependent variable is 4-digit *occupation* switching and the sample is the *occupation-exposed* sample. Marginal probabilities are reported. *t*-statistics adjusted for clustering at the industry level (odd columns) or the occupation level (even columns) are in parentheses. '*p*-value for joint sig.' is the test for the joint significance of the two trade variables. A number less than 0.01 indicates significance. 'Rapid-growth effect' is 1.45 times the marginal probability of the offshore outsourcing variable plus 1.23 times the marginal probability of the inshoring variable.

Table 4. IV Estimation with M^{CI}/M^{G8} and X^{CI}/X^{G8} as Instruments

	Switching		Change in unemployment		Change in earnings	
	Ind.	Occ.	Ind.	Occ.	Ind.	Occ.
	(1)	(2)	(3)	(4)	(5)	(6)
$\ln(M^{CI})$	0.027	0.033	0.0007	-0.0009	-0.0028	-0.0003
	(7.90)	(6.49)	(1.96)	(-1.12)	(-2.50)	(-0.05)
$\ln(X^{CI})$	-0.031	-0.054	-0.0018	0.0008	0.0068	0.0133
	(-2.20)	(-4.68)	(-10.10)	(0.99)	(14.52)	(2.18)
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.000	0.000	0.000	0.440	0.002	0.128
Rapid-growth effect	0.001	-0.019	-0.001	0.000	0.004	0.016
Exogeneity (χ^2 or F)	6.07	9.35	7.88	8.86	6.82	0.82
R^2 or pseudo-R^2	0.05	0.03	0.01	0.01	0.01	0.01
N	24,261	37,550	24,261	37,550	24,261	37,550
Estimation Method	2SIV	2SIV	2SLS	2SLS	2SLS	2SLS

Notes: 'Ind.' refers to the industry-exposed sample and 'Occ.' refers to the occupation-exposed sample. Marginal probabilities of probits or OLS coefficients are reported. *t*-statistics are in parentheses and are clustered at the industry level (for the industry sample) or the occupation level (for the occupation sample). Columns 1-2 correspond to columns 1-2 of table 3. Columns 3-4 correspond to columns 1-2 of table 5. Columns 5-6 correspond to columns 1-2 of table 6. See the notes to those tables for additional information about the specification.

Table 5. Change in Unemployed Weeks as a Share of Weeks in the Labor Force

	All Workers		College Graduates		Non-College Graduates		Skilled White Collar		Less-Skilled White Collar	
	Ind.	Occ.	Ind.	Occ.	Ind.	Occ.	Ind.	Occ.	Ind.	Occ.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\ln(M^{CI})$	0.0003	0.0006	-0.0009	-0.0002	0.0019	0.0016	0.0001	0.0002	0.0028	0.0029
	(0.62)	(0.82)	(-1.57)	(-0.14)	(1.28)	(1.00)	(0.13)	(0.21)	(6.65)	(2.01)
$\ln(X^{CI})$	-0.0019	-0.0013	-0.0008	-0.0012	-0.0039	-0.0012	-0.0021	-0.0010	-0.0039	-0.0038
	(-7.08)	(-2.57)	(-2.28)	(-1.43)	(-14.18)	(-1.55)	(-7.25)	(-1.02)	(-5.34)	(-3.87)
Change in worker charact. and f.e.s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p -value for joint sig.	0.012	0.088	0.047	0.217	0.000	0.308	0.008	0.283	0.016	0.294
Rapid-growth effect	-0.002	-0.001	-0.002	-0.002	-0.002	0.001	-0.003	-0.001	-0.001	-0.001
	(-1.98)	(-0.69)	(-3.35)	(-1.07)	(-0.82)	(0.37)	(-2.82)	(-1.65)	(-1.42)	(-0.17)
R^2	0.006	0.004	0.009	0.006	0.010	0.007	0.009	0.006	0.010	0.005
N	24,261	37,550	13,499	18,679	10,762	18,871	13,848	18,946	9,119	18,604
	(1')	(2')	(3')	(4')	(5')	(6')	(7')	(8')	(9')	(10')
$\ln(M^{CI}/M^{G8})$	0.0001	-0.0006	-0.0015	-0.0015	0.0021	0.0002	-0.0003	-0.0005	0.0016	-0.0009
	(0.38)	(-1.08)	(-2.66)	(-0.91)	(5.62)	(0.15)	(-0.72)	(-0.72)	(1.51)	(-1.52)
$\ln(X^{CI}/X^{G8})$	-0.0017	0.0007	0.0010	0.0008	-0.0061	0.0007	-0.0009	0.0012	-0.0051	0.0003
	(-9.95)	(0.93)	(4.17)	(0.73)	(-8.14)	(0.70)	(-2.60)	(0.79)	(-1.85)	(0.46)
Change in worker charact. and f.e.s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p -value for joint sig.	0.000	0.464	0.030	0.650	0.010	0.790	0.142	0.707	0.286	0.370
Rapid-Growth Effect	-0.002	0.000	-0.001	-0.001	-0.004	0.001	-0.002	0.001	-0.004	-0.001
	(-8.38)	(-0.09)	(-0.99)	(-0.56)	(-5.78)	(0.47)	(-2.36)	(0.46)	(-1.97)	(-0.64)
R^2	0.006	0.004	0.009	0.006	0.011	0.007	0.009	0.006	0.010	0.005
N	24,261	37,550	13,499	18,679	10,762	18,871	13,848	18,946	9,119	18,604

Notes : The dependent variable is the change in unemployed weeks as a share of total weeks in the labor force. Each column presents two separate OLS regressions, the top panel with M^{CI} and X^{CI} and the bottom panel with M^{CI}/M^{G8} and X^{CI}/X^{G8}. The regressions include the change in time-varying worker characteristics (change in experience squared) as well as fixed effects for 2-digit industries, 2-digit occupations, year and state. The sample alternates between columns. The industry-exposed sample is in odd-numberd columns and the occupation-exposed sample is in even-numberd columns. *t* -statistics adjusted for clustering at the industry level (odd columns) or occupation level (even columns) are in parentheses. '*p* -value for joint sig.' is the joint test for the significance of the two trade variables. A number less than 0.01 indicates significance. 'Rapid-growth effect' is 1.45 times the coefficient on the offshore outsourcing variable plus 1.23 times the coefficient on the inshoring variable.

Table 6. Change in Log Earnings

								Switchers to:		
	All Workers		College Grads		Non-College Grads		Non-Switchers	Exposed Industries	Non-Exposed Industries	Age <25
	Ind.	Occ.	Ind.	Occ.	Ind.	Occ.	Ind.	Ind.	Ind.	Ind.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\ln(M^{CI})$	-0.0001	-0.002	-0.008	-0.014	0.016	0.025	0.001	-0.010	0.006	0.050
	(-0.04)	(-0.38)	(-7.54)	(-2.06)	(4.00)	(5.36)	(0.64)	(-1.20)	(0.63)	(6.10)
$\ln(X^{CI})$	0.006	0.015	0.008	0.028	-0.003	-0.011	0.005	0.009	-0.007	-0.043
	(2.13)	(2.83)	(5.87)	(3.29)	(-0.58)	(-1.54)	(5.00)	(0.48)	(-1.13)	(-3.19)
Change in worker charact. and f.e.s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.146	0.059	0.003	0.018	0.003	0.002	0.033	0.450	0.189	0.009
Rapid-growth effect	0.007	0.015	-0.002	0.014	0.020	0.023	0.007	-0.004	0.000	0.020
	(1.43)	(1.99)	(-0.61)	(1.04)	(1.66)	(2.81)	(3.28)	(-0.28)	(-0.02)	(0.87)
R^2	0.010	0.010	0.016	0.015	0.014	0.013	0.009	0.066	0.034	0.062
N	24,261	37,550	13,499	18,679	10,762	18,871	16,903	2,472	4,886	1,557
	(1')	(2')	(3')	(4')	(5')	(6')	(7')	(8')	(9')	(10')
$\ln(M^{CI}/M^{G8})$	-0.0004	0.001	-0.006	-0.011	0.008	0.021	0.001	-0.013	-0.003	0.017
	(-0.35)	(0.24)	(-8.88)	(-1.55)	(2.73)	(10.86)	(0.80)	(-2.30)	(-0.53)	(0.63)
$\ln(X^{CI}/X^{G8})$	0.0064	0.011	0.007	0.026	0.004	-0.012	0.007	0.021	-0.016	0.013
	(10.24)	(2.44)	(3.02)	(1.68)	(1.03)	(-1.28)	(3.35)	(2.04)	(-1.18)	(2.05)
Change in worker charact. and f.e.s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.001	0.109	0.007	0.156	0.114	0.000	0.017	0.132	0.530	0.058
Rapid-Growth Effect	0.007	0.015	0.000	0.017	0.017	0.016	0.010	0.008	-0.025	0.041
	(7.35)	(1.87)	(-0.13)	(0.82)	(1.91)	(1.43)	(5.49)	(0.56)	(-1.25)	(1.20)
R^2	0.010	0.010	0.016	0.015	0.010	0.013	0.009	0.066	0.034	0.060
N	24,261	37,550	13,499	18,679	10,762	18,871	16,903	2,472	4,886	1,557

Notes: The dependent variable is the change in log earnings. Each column presents two separate OLS regressions, the top panel with M^{C} and X^{CI} and the bottom panel with M^{CI}/M^{G8} and X^{CI}/X^{G8}. The regressions include the change in time-varying worker characteristics (change in experience squared) as well as fixed effects for 2-digit industries, 2-digit occupations, year and state. The 'Ind.' columns use the industry-exposed sample and the 'Occ.' columns use the occupation-exposed sample. *t*-statistics adjusted for clustering at the industry level ('Ind.' columns) or the occupation level ('Occ.' columns) are in parentheses. '*p*-value for joint sig.' is the joint test for the significance of the two trade variables. A number less than 0.01 indicates significance. 'Rapid-growth effect' is 1.45 times the coefficient on the offshore outsourcing variable plus 1.23 times the coefficient on the inshoring variable.

Table 7. Robustness:
Inshoring and Service Offshoring from All Available Low-Wage Countries

	Industry-Exposed Sample			Occupation-Exposed Sample		
Dependent variable	Industry switching	Change in unemploy-ment	Change in earnings	Occupation switching	Change in unemploy-ment	Change in earnings
	(1)	(2)	(3)	(4)	(5)	(6)
$\ln(M^{CI})$	0.030	0.0005	-0.0022	**0.019**	0.0009	-0.0031
	(5.29)	(0.55)	(-1.22)	**(2.94)**	(0.98)	(-0.47)
$\ln(X^{CI})$	-0.029	-0.0019	**0.0062**	-0.039	-0.0017	**0.0155**
	(-2.56)	(-8.74)	**(2.72)**	(-3.65)	(-2.44)	**(2.05)**
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.00	0.00	0.14	0.00	0.11	0.18
Rapid-growth effect	.008	-.002	.004	-.020	-.001	.015
	(0.59)	(-1.51)	(1.21)	(-1.21)	(-0.70)	(1.36)
R^2 or pseudo-R^2	0.05	0.01	0.01	0.03	0.00	0.01
N	24,261	24,261	24,261	37,550	37,550	37,550
	(1')	(2')	(3')	(4')	(5')	(6')
$\ln(M^{CI}/M^{G8})$	0.015	0.0002	-0.0019	0.029	-0.0010	0.0017
	(9.55)	(0.43)	(-0.95)	(7.85)	(-1.69)	(0.34)
$\ln(X^{CI}/X^{G8})$	-0.021	-0.0012	0.0041	-0.039	0.0012	0.0031
	(-3.38)	(-5.95)	(6.71)	(-3.88)	(1.46)	(1.13)
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.00	0.02	0.01	0.00	0.20	0.41
Rapid-Growth Effect	-.003	-.001	.002	-.007	.000	.006
	(-0.32)	(-2.27)	(0.75)	(-0.70)	(-0.05)	(0.93)
R^2 or pseudo-R^2	0.05	0.01	0.01	0.03	0.00	0.01
N	24,261	24,261	24,261	37,550	37,550	37,550

Notes: In this table the service trade variables have been redefined to include not just China and India, but all other low-wage countries for which data are available. Marginal probabilities from probits are reported in columns 1 and 4. OLS estimates are reported in the remaining columns. *t*-statistics are in parentheses and are clustered at the industry level (for the industry sample) or the occupation level (for the occupation sample). Columns 1 and 4 correspond to columns 1-2 of table 3. Columns 2 and 5 correspond to columns 1-2 of table 5. Columns 3 and 6 correspond to columns 1-2 of table 6. See the notes to those tables for additional information. A bolded estimate means that its statistical significance has changed from that in the baseline specifications of tables 3, 5, or 6. In particular, the coefficient has gone from significant to insignificant or from insignificant to significant.

Table 8. Robustness: Deleting the Technology-Bubble Years (2000-2001)

	Industry-Exposed Sample			Occupation-Exposed Sample		
Dependent variable	Industry switching	Change in unemploy-ment	Change in earnings	Occupation switching	Change in unemploy-ment	Change in earnings
	(1)	(2)	(3)	(4)	(5)	(6)
$\ln(M^{CI})$	0.019	-0.0005	-0.0018	0.010	0.0004	0.0001
	(4.73)	(-0.61)	(-1.26)	(1.42)	(0.42)	(0.02)
$\ln(X^{CI})$	-0.027	-0.0017	0.0073	-0.030	-0.0009	**0.0115**
	(-1.75)	(-6.87)	(1.94)	(-3.06)	(-1.54)	**(1.83)**
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.00	0.01	0.26	0.00	0.29	0.23
Rapid-growth effect	-.005	-.003	.007	-.023	.000	.014
	(-0.33)	(-2.29)	(1.41)	(-1.27)	(-0.42)	(1.45)
R^2 or pseudo-R^2	0.05	0.01	0.01	0.03	0.01	0.01
N	19,178	19,178	19,178	30,446	30,446	30,446
	(1')	(2')	(3')	(4')	(5')	(6')
$\ln(M^{CI}/M^{G8})$	0.010	-0.0005	-0.0015	0.020	-0.0008	0.0038
	(4.57)	(-1.85)	(-1.92)	(4.53)	(-0.96)	(0.70)
$\ln(X^{CI}/X^{G8})$	-0.027	-0.0013	0.0093	-0.046	0.0004	0.0062
	(-2.63)	(-6.26)	(7.50)	(-4.21)	(0.65)	(1.56)
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.00	0.00	0.01	0.00	0.61	0.34
Rapid-Growth Effect	-.018	-.002	.009	-.028	-.001	.013
	(-1.20)	(-8.47)	(7.73)	(-1.67)	(-0.56)	(1.24)
R^2 or pseudo-R^2	0.05	0.01	0.01	0.03	0.01	0.01
N	19,178	19,178	19,178	30,446	30,446	30,446

Notes: In this table the years 2000 and 2001 are omitted from the analysis. The table is identical in structure to table 7. See the table 7 notes.

Table 9. Robustness: Correcting for CPS Sample Selection

	Industry-Exposed Sample						Occupation-Exposed Sample					
Dependent variable	**Industry switching**		**Change in unemployment**		**Change in log earnings**		**Occupation switching**		**Change in unemployment**		**Change in log earnings**	
Second-Stage Equation												
$\ln(M^{CI})$	0.025		0.0006		-0.0004		**0.012**		0.0007		-0.0009	
	(6.85)		(0.61)		(-0.28)		**(3.27)**		(0.66)		(-0.29)	
$\ln(X^{CI})$	-0.030		-0.0023		0.0058		-0.032		-0.0017		**0.0147**	
	(-1.93)		(-7.13)		(2.20)		(-2.90)		(-1.15)		**(1.90)**	
$\ln(M^{CI}/M^{G8})$		0.015		0.0005		-0.0012		0.020		-0.0002		0.0013
		(11.88)		(1.26)		(-1.35)		(5.89)		(-0.18)		(0.50)
$\ln(X^{CI}/X^{G8})$		-0.032		-0.0024		0.0077		-0.043		0.0000		0.0109
		(-2.92)		(-3.36)		(12.32)		(-2.65)		(-0.02)		(1.19)
Worker Charact. and f.e.s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	38,348	38,348	38,348	38,348	38,348	38,348	58,633	58,633	58,633	58,633	58,633	58,633
uncensored *N*	24,949	24,949	24,968	24,968	24,339	24,339	38,259	38,259	38,260	38,260	37,682	37,682
log likelihood	-37,074	-37,081	-5,859	-5,859	-50,211	-50,210	-57,342	-57,341	-8,147	-8,149	-75,181	-75,186
Wald test of indep. Eqns	2.82	2.50	4.43	4.31	11.77	11.63	26.98	28.62	5.79	5.71	9.31	9.22
Selection Equation												
Excluded Regressors												
Family size	-0.056	-0.056	-0.053	-0.053	-0.052	-0.052	-0.044	-0.044	-0.041	-0.041	-0.038	-0.038
	(-7.50)	(-7.41)	(-12.62)	(-12.68)	(-8.57)	(-8.59)	(-5.07)	(-5.07)	(-8.59)	(-8.58)	(-6.60)	(-6.59)
Number of children	0.013	0.013	0.009	0.009	0.006	0.006	0.009	0.009	0.005	0.005	0.001	0.001
	(3.26)	(3.27)	(6.46)	(6.49)	(4.92)	(4.94)	(1.03)	(1.03)	(1.25)	(1.25)	(0.29)	(0.30)
House owner	0.618	0.618	0.623	0.623	0.599	0.599	0.606	0.606	0.604	0.604	0.591	0.591
	(20.76)	(20.61)	(20.57)	(20.56)	(15.43)	(15.42)	(22.37)	(22.35)	(31.65)	(31.65)	(29.66)	(29.68)
Same house last year	0.038	0.038	0.035	0.035	0.029	0.029	0.079	0.079	0.074	0.075	0.072	0.072
	(2.23)	(2.23)	(5.81)	(5.77)	(2.28)	(2.27)	(3.12)	(3.14)	(5.30)	(5.33)	(4.42)	(4.45)
Other Regressors												
Experience	0.032	0.032	0.032	0.033	0.031	0.031	0.029	0.029	0.030	0.030	0.029	0.029
	(8.79)	(8.78)	(14.39)	(14.39)	(11.87)	(12.07)	(12.75)	(12.70)	(21.13)	(21.20)	(25.87)	(25.78)
Experience squared	-0.0003	-0.0004	-0.0004	-0.0004	-0.0004	-0.0004	-0.0003	-0.0003	-0.0003	-0.0003	-0.0003	-0.0003
	(-3.86)	(-3.89)	(-4.69)	(-4.70)	(-4.50)	(-4.52)	(-5.10)	(-5.10)	(-6.55)	(-6.57)	(-6.68)	(-6.68)
Years of schooling	0.031	0.031	0.031	0.031	0.024	0.024	0.034	0.034	0.033	0.033	0.029	0.029
	(6.79)	(6.86)	(20.64)	(20.90)	(7.89)	(7.83)	(7.61)	(7.49)	(15.68)	(15.44)	(12.87)	(12.68)
Married	0.180	0.180	0.172	0.172	0.175	0.175	0.154	0.154	0.149	0.149	0.142	0.142
	(6.79)	(12.94)	(11.22)	(11.24)	(10.84)	(10.81)	(11.91)	(11.90)	(18.56)	(18.54)	(11.92)	(11.91)
Male	0.010	0.009	0.014	0.014	-0.019	-0.019	-0.020	-0.020	-0.015	-0.015	-0.034	-0.034
	(0.86)	(0.83)	(0.96)	(0.95)	(-1.23)	(-1.25)	(-1.46)	(-1.52)	(-1.56)	(-1.58)	(-2.99)	(-3.02)
White	0.087	0.087	0.087	0.086	0.089	0.088	0.085	0.085	0.082	0.082	0.080	0.080
	(3.90)	(3.94)	(8.07)	(8.22)	(5.95)	(5.93)	(3.74)	(3.73)	(6.89)	(6.88)	(6.29)	(6.30)
Veteran	-0.037	-0.037	-0.038	-0.038	-0.034	-0.033	-0.016	-0.016	-0.011	-0.012	-0.015	-0.015
	(-3.06)	(-3.10)	(-3.40)	(-3.41)	(-2.24)	(-2.22)	(-0.66)	(-0.69)	(-0.95)	(-1.00)	(-0.81)	(-0.85)
$\ln(M^{CI})$	-0.006		-0.004		-0.006		0.018		0.02		0.023	
	(-0.75)		(-1.30)		(-1.07)		(1.71)		(3.96)		(4.00)	
$\ln(X^{CI})$	0.015		0.012		0.012		-0.003		-0.004		-0.004	
	(1.25)		(3.18)		(3.70)		(-0.19)		(-0.77)		(-0.62)	
$\ln(M^{CI}/M^{G8})$		-0.002		-0.002		0.000				0.009		0.014
		(-0.92)		(-0.78)		(0.08)				(3.61)		(3.25)
$\ln(X^{CI}/X^{G8})$		0.024		0.020		0.019				-0.003		0.000
		(3.63)		(4.42)		(11.22)				(-0.54)		(0.02)

Notes: In this table, second-stage equations are simultaneously estimated together with a selection equation. The latter is a probit for whether a worker entering the sample in period t is matched in period $t+1$. For the selection equation, coefficients are reported. For the second-stage equations, marginal probabilities are reported in columns 1 and 4 and coefficients are reported in the remaining columns. t-statistics are in parentheses and are clustered at the industry level (for the industry sample) or the occupation level (for the occupation sample). Columns 1 and 4 correspond to columns 1-2 of table 3. Columns 2 and 5 correspond to columns 1-2 of table 5. Columns 3 and 6 correspond to columns 1-2 of table 6. See the notes to those tables for additional information about the second-stage equations. A bolded estimate means that its statistical significance has changed from that in the baseline specifications of tables 3, 5, or 6. In particular, the coefficient has gone from significant to insignificant or from insignificant to significant.

Table 10. Robustness: All Workers in Private Services

	Industry-Exposed and Unexposed Sample			Occupation-Exposed and Unexposed Sample		
Dependent variable	Industry switch	Change in unemployment	Change in earnings	Occupation switch	Change in unemployment	Change in earnings
	(1)	(2)	(3)	(4)	(5)	(6)
$\ln(M^{CI})$	**0.017**	-0.0005	-0.0001	0.014	0.0004	-0.0022
	(2.10)	(-0.90)	(-0.12)	(1.89)	(0.50)	(-0.56)
$\ln(X^{CI})$	-0.027	**-0.0012**	0.0071	-0.030	-0.0006	**0.0044**
	(-1.43)	**(-1.38)**	(3.17)	(-3.63)	(-1.01)	**(0.80)**
Exposure dummy	0.076	0.0141	-0.0517	0.07	0.0032	-0.0269
	(0.91)	(2.45)	(-3.96)	(1.72)	(1.25)	(-1.17)
Diff-of-Diff	0.040	0.0004	-0.0130	0.01	-0.0007	-0.0031
	(2.47)	(0.20)	(-1.28)	(0.41)	(-0.43)	(-0.33)
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.00	0.05	0.00	0.00	0.03	0.03
Rapid-growth effect	-.008	-.002	.009	-.016	.000	.002
	(-0.46)	(-3.05)	(2.94)	(-1.32)	(-0.11)	(0.23)
R^2 or pseudo-R^2	0.04	0.00	0.01	0.04	0.00	0.01
N	78,586	78,586	78,586	115,090	115,090	115,090
	(1')	(2')	(3')	(4')	(5')	(6')
$\ln(M^{CI}/M^{G8})$	**0.008**	-0.0006	0.0016	0.021	-0.0008	0.0008
	(1.55)	(-1.67)	(0.97)	(4.50)	(-1.19)	(0.23)
$\ln(X^{CI}/X^{G8})$	-0.030	**-0.0012**	0.0079	-0.046	0.0013	0.0007
	(-3.04)	**(-1.62)**	(5.46)	(-3.92)	(1.80)	(0.15)
Exposure dummy	-0.019	0.0024	0.0019	-0.056	0.0027	-0.0105
	(-0.45)	(0.39)	(0.20)	(-2.04)	(1.13)	(-0.36)
Diff-of-Diff	0.033	-0.0004	-0.0095	0.001	-0.0008	-0.0020
	(2.20)	(-0.18)	(-0.95)	(0.06)	(-0.52)	(-0.24)
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.00	0.01	0.00	0.00	0.04	0.15
Rapid-growth effect	-.025	-.002	.012	-.026	.000	.002
	(-1.37)	(-3.57)	(4.08)	(-2.16)	(0.53)	(0.22)
R^2 or pseudo-R^2	0.04	0.00	0.01	0.04	0.00	0.01
N	78,586	78,586	78,586	115,090	115,090	115,090

Notes: In this table we add to our sample those private service workers in *unexposed* industries (columns 1-3) or *unexposed* occupations (columns 4-6). 'Exposure dummy' is a dummy for whether the worker is exposed (=1) or not (=0) to inshoring or offshore outsourcing. 'Diff-of-Diff' is the interaction of the exposure dummy with a dummy for whether the year is in the 2001-2006 period (= 1) or not (= 0). For unexposed workers service trade is by definition 0 so that the inshoring and offshore outsourcing variables are set to 0. The table is identical in structure to table 7. See the table 7 notes.

Table 11. Robustness: Alternative Dependent Variables

	Industry-Exposed Sample			Occupation-Exposed Sample		
Dependent variable	1-digit industry switching	Transition into unemployment	Change in 'synthetic' wages	1-digit occupation switching	Transition into unemployment	Change in 'synthetic' wages
	(1)	(2)	(3)	(4)	(5)	(6)
$\ln(M^{CI})$	**0.012**	0.0013	0.0037	-0.004	0.0029	-0.0010
	(1.74)	(1.39)	(2.49)	(-1.11)	(2.46)	(-0.20)
$\ln(X^{CI})$	**-0.025**	-0.0016	-0.0015	**-0.016**	**-0.0041**	0.0151
	(-3.18)	(-3.32)	(-0.49)	**(-2.12)**	**(-3.70)**	(3.72)
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.00	0.00	0.14	0.11	0.00	0.02
Rapid-growth effect	-.013	.000	.003	-.025	-.001	.017
	(-5.84)	(-0.02)	(1.14)	(-2.00)	(-0.70)	(1.78)
R^2 or pseudo-R^2	0.05	0.05	0.01	0.03	0.04	0.01
N	24,261	23,876	24,261	37,550	37,017	37,550
	(1')	(2')	(3')	(4')	(5')	(6')
$\ln(M^{CI}/M^{G8})$	**0.001**	0.0005	0.0009	**0.004**	0.0017	-0.0006
	(0.46)	(0.89)	(1.57)	**(1.10)**	(1.08)	(-0.12)
$\ln(X^{CI}/X^{G8})$	-0.028	-0.0030	**0.0010**	**-0.021**	-0.0025	0.0131
	(-3.43)	(-2.78)	**(0.52)**	**(-2.28)**	(-1.85)	(2.40)
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.00	0.00	0.41	0.03	0.18	0.09
Rapid-Growth Effect	-.033	-.003	.003	-.020	-.001	.015
	(-4.70)	(-4.65)	(0.90)	(-1.56)	(-0.33)	(1.42)
R^2 or pseudo-R^2	0.05	0.05	0.01	0.03	0.04	0.01
N	24,261	23,876	24,261	37,550	37,017	37,550

Notes: Column 1 (2) is a probit for 1-digit industry (occupation) switching. This differs from the 4-digit switching reported in all other tables. Column 2 is a probit for a binary dependent variable which equals 1 if the worker went from being employed in March of year t to being unemployed in March of year $t + 1$. Column 3 is a linear regression of the growth in hourly wages where hourly wages is defined as real annual earnings divided by hours worked last year (weeks worked last year times hours worked each week). Marginal probabilities from probits are reported in columns 1, 2, 4 and 5. OLS estimates are reported in the remaining columns. The table is identical in structure to table 7. See the table 7 notes.

Table 12. Robustness: Contemporaneous Inshoring and Offshore Outsourcing Variables

	Industry-Exposed Sample			Occupation-Exposed Sample		
Dependent variable	Industry switching	Change in unemploy-ment	Change in earnings	Occupation switching	Change in unemploy-ment	Change in earnings
	(1)	(2)	(3)	(4)	(5)	(6)
$\ln(M^{CI})$	0.027	-0.0001	0.0002	**0.016**	0.0004	-0.0018
	(11.45)	(-0.08)	(0.24)	**(2.65)**	(0.43)	(-0.52)
$\ln(X^{CI})$	**-0.034**	-0.0019	0.0085	-0.040	-0.0009	0.0135
	(-2.91)	(-4.29)	(1.88)	(-3.37)	(-1.67)	(2.77)
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.00	0.01	0.26	0.00	0.30	0.07
Rapid-growth effect	-0.002	-0.002	0.011	-0.026	-0.001	0.014
	(-0.13)	(-1.99)	(1.71)	(-1.66)	(-0.45)	(2.11)
R^2 or pseudo-R^2	0.05	0.01	0.01	0.03	0.00	0.01
N	24,261	24,261	24,261	37,550	37,550	37,550
	(1')	(2')	(3')	(4')	(5')	(6')
$\ln(M^{CI}/M^{G8})$	0.015	-0.0001	-0.0005	0.021	0.0000	0.0015
	(11.40)	(-0.20)	(-0.60)	(4.33)	(-0.06)	(0.46)
$\ln(X^{CI}/X^{G8})$	-0.031	**-0.0020**	0.0088	-0.046	0.0001	**0.0054**
	(-3.38)	**(-2.33)**	(7.32)	(-3.46)	(0.17)	**(2.80)**
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.00	0.00	0.01	0.00	0.98	0.06
Rapid-Growth Effect	-0.016	-0.003	0.010	-0.026	0.000	0.009
	(-1.20)	(-13.74)	(9.13)	(-1.87)	(0.07)	(1.70)
R^2 or pseudo-R^2	0.05	0.01	0.01	0.03	0.00	0.01
N	24,261	24,261	24,261	37,550	37,550	37,550

Notes: In this table we use imports and exports for year t rather than year t - 1. The table is identical in structure to table 7. See the table 7 notes.

Table 13. Robustness: Only Business, Professional and Technical Services

	Industry-Exposed Sample			Occupation-Exposed Sample		
Dependent variable	Industry switching	Change in unemploy-ment	Change in earnings	Occupation switching	Change in unemploy-ment	Change in earnings
	(1)	(2)	(3)	(4)	(5)	(6)
$\ln(M^{CI})$	0.021	-0.0006	0.0006	0.000	0.0014	-0.0024
	(3.00)	(-1.06)	(0.28)	(-0.01)	(1.38)	(-0.51)
$\ln(X^{CI})$	**-0.052**	-0.0026	**0.0109**	-0.053	-0.0012	0.0211
	(-9.14)	(-5.98)	**(4.66)**	(-3.46)	(-1.45)	(2.65)
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.00	0.00	0.00	0.00	0.17	0.03
Rapid-growth effect	-.035	-.004	.014	-.065	.001	.023
	(-11.45)	(-10.55)	(26.08)	(-1.94)	(0.29)	(3.31)
R^2 or pseudo-R^2	0.06	0.01	0.01	0.03	0.00	0.01
N	11,927	11,927	11,927	32,481	32,481	32,481
	(1')	(2')	(3')	(4')	(5')	(6')
$\ln(M^{CI}/M^{G8})$	**-0.017**	0.0005	0.0009	**0.022**	-0.0001	0.0047
	(-2.17)	(0.39)	(0.24)	**(1.25)**	(-0.05)	(0.50)
$\ln(X^{CI}/X^{G8})$	-0.042	-0.0016	**0.0049**	-0.054	0.0007	0.0170
	(-19.40)	(-4.42)	**(1.89)**	(-2.81)	(0.68)	(1.91)
Worker characteristics and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value for joint sig.	0.00	0.06	0.03	0.00	0.77	0.02
Rapid-Growth Effect	-.076	-.001	.007	-.034	.001	.028
	(-8.40)	(-0.76)	(2.78)	(-0.73)	(0.19)	(1.15)
R^2 or pseudo-R^2	0.06	0.01	0.01	0.03	0.00	0.01
N	11,927	11,927	11,927	32,481	32,481	32,481

Notes: In this table we only include those service trade categories that feed into Business, Professional, and Technical (BPT) services. See table 1 for a list of these 8 categories. The table is identical in structure to table 7. See the table 7 notes.

Table A.1. Matching Rates for CPS Data

Year	Naïve Match	Valid Match	Final Match
1996	71%	95%	67%
1997	70%	95%	67%
1998	70%	96%	67%
1999	69%	96%	66%
2000	75%	97%	73%
2001	64%	94%	60%
2002	65%	92%	60%
2003	65%	94%	61%
2004	57%	95%	54%
2005	59%	94%	55%
average	67%	95%	64%

Notes: 'Naïve Match' is the proportion of all civilian adults in March of the indicated year who can be matched to an individual in March of the subsequent year. The naïve match is based on a household identifier, a household number, and an individual line number within a household. 'Valid Match' is the percentage of naïve matches that survive the S|R|A (sex, race, age) merge criterion. 'Final Match' is the final match rate and equals (naïve match)x(valid match).

Table A.2. Concordance between Census Industry Codes and NAICS/BEA Codes

2002 Census Codes	2002 Census Categories	2002 NAICS Codes	BEA 'Other Private Service' Codes
7470	Advertising and related services	5418	advertising
7290	Architectural, engineering, and related services	5413	construction, architecture, engineering services
6490	Software publishing	5112	computer and information services
6675	Internet publishing and broadcasting	5161	computer and information services
6692	Internet service providers	5181	computer and information services
6695	Data processing, hosting, and related services	5182	computer and information services
6780	Other information services	5191 exc. 51912	computer and information services
7380	Computer systems design and related services	5415	computer and information services
6870	Banking and related activities	521, 52211,52219	finance
6880	Savings institutions, including credit unions	52212, 52213	finance
6890	Non-depository credit and related activities	5222, 5223	finance
6970	Securities, commodities, funds, trusts, and other financial investments	523, 525	finance
7370	Specialized design services	5414	industrial engineering
6990	Insurance carriers and related activities	524	insurance
7270	Legal services	5411	legal services
7390	Management, scientific, and technical consulting services	5416	management, consulting, public relation services
7280	Accounting, tax preparation, bookkeeping, and payroll services	5412	other business, professional and technical services
7490	Other professional, scientific, and technical services	5419 exc. 54194	other business, professional and technical services
7590	Business support services	5614	other business, professional and technical services
7780	Other administrative and other support services	5611, 5612, 5619	other business, professional and technical services
7460	Scientific research and development services	5417	research, development and testing services
6680	Wired telecommunications carriers	5171	telecommunication
6690	Other telecommunications services	517 exc. 5171, 5175	telecommunication

Table A.3. Concordance between Census Occupation Codes and NAICS/BEA Codes

2002 Census Codes	2002 Census Categories	2002 SOC Codes	BEA 'Other Private Service' Codes
0040	Advertising and promotions managers	11-2011	advertising
4800	Advertising sales agents	41-3011	advertising
0300	Engineering managers	11-9041	construction, architecture, engineering services
1300	Architects, except naval	17-1010	construction, architecture, engineering services
1310	Surveyors, cartographers, and photogrammetrists	17-1020	construction, architecture, engineering services
1360	Civil engineers	17-2051	construction, architecture, engineering services
1560	Surveying and mapping technicians	17-3031	construction, architecture, engineering services
0110	Computer and information systems managers	11-3021	computer and information services
1000	Computer scientists and systems analysts	15-10XX	computer and information services
1010	Computer programmers	15-1021	computer and information services
1020	Computer software engineers	15-1030	computer and information services
1040	Computer support specialists	15-1041	computer and information services
1060	Database administrators	15-1061	computer and information services
1100	Network and computer systems administrators	15-1071	computer and information services
1110	Network systems and data communications analysts	15-1081	computer and information services
1400	Computer hardware engineers	17-2061	computer and information services
5800	Computer operators	43-9011	computer and information services
5810	Data entry keyers	43-9021	computer and information services
5830	Desktop publishers	43-9031	computer and information services
5920	Statistical assistants	43-9111	computer and information services
0120	Financial managers	11-3031	finance
0830	Credit analysts	13-2041	finance
0840	Financial analysts	13-2051	finance
0850	Personal financial advisors	13-2052	finance
0900	Financial examiners	13-2061	finance
0910	Loan counselors and officers	13-2070	finance
0950	Financial specialists, all other	13-2099	finance
4820	Securities, commodities, and financial services sales agents	41-3031	finance
5200	Brokerage clerks	43-4011	finance
5230	Credit authorizers, checkers, and clerks	43-4041	finance
5330	Loan interviewers and clerks	43-4131	finance
5340	New accounts clerks	43-4141	finance
1350	Chemical engineers	17-2041	industrial engineering
1410	Electrical and electronic engineers	17-2070	industrial engineering
1430	Industrial engineers, including health and safety	17-2110	industrial engineering
1460	Mechanical engineers	17-2141	industrial engineering
1550	Engineering technicians, except drafters	17-3020	industrial engineering
0540	Claims adjusters, appraisers, examiners, and investigators	13-1030	insurance
0860	Insurance underwriters	13-2053	insurance
1200	Actuaries	15-2011	insurance
4810	Insurance sales agents	41-3021	insurance
5840	Insurance claims and policy processing clerks	43-9041	insurance
2140	Paralegals and legal assistants	23-2011	legal services
2150	Miscellaneous legal support workers	23-2090	legal services
0010	Chief executives	11-1011	management, consulting, public relation services
0020	General and operations managers	11-1021	management, consulting, public relation services
0050	Marketing and sales managers	11-2020	management, consulting, public relation services
0060	Public relations managers	11-2031	management, consulting, public relation services
0100	Administrative services managers	11-3011	management, consulting, public relation services
0130	Human resources managers	11-3040	management, consulting, public relation services
0140	Industrial production managers	11-3051	management, consulting, public relation services
0150	Purchasing managers	11-3061	management, consulting, public relation services
0160	Transportation, storage, and distribution managers	11-3071	management, consulting, public relation services
0600	Cost estimators	13-1051	management, consulting, public relation services
0710	Management analysts	13-1111	management, consulting, public relation services
0730	Other business operations specialists	13-11XX	management, consulting, public relation services
1220	Operations research analysts	15-2031	management, consulting, public relation services

Table A.3. Concordance between Census Occupation Codes and NAICS/BEA Codes (*continued*)

2002 Census Codes	2002 Census Categories	2002 SOC Codes	BEA 'Other Private Service' Codes
0800	Accountants and auditors	13-2011	other business, professional and technical services
0820	Budget analysts	13-2031	other business, professional and technical services
0940	Tax preparers	13-2082	other business, professional and technical services
3320	Diagnostic related technologists and technicians	29-2030	other business, professional and technical services
4940	Telemarketers	41-9041	other business, professional and technical services
5000	First-line supervisors/managers of office & admin. support	43-1011	other business, professional and technical services
5010	Switchboard operators, including answering service	43-2011	other business, professional and technical services
5020	Telephone operators	43-2021	other business, professional and technical services
5030	Communications equipment operators, all other	43-2099	other business, professional and technical services
5100	Bill and account collectors	43-3011	other business, professional and technical services
5110	Billing and posting clerks and machine operators	43-3021	other business, professional and technical services
5120	Bookkeeping, accounting, and auditing clerks	43-3031	other business, professional and technical services
5140	Payroll and timekeeping clerks	43-3051	other business, professional and technical services
5150	Procurement clerks	43-3061	other business, professional and technical services
5160	Tellers	43-3071	other business, professional and technical services
5210	Correspondence clerks	43-4021	other business, professional and technical services
5240	Customer service representatives	43-4051	other business, professional and technical services
5260	File Clerks	43-4071	other business, professional and technical services
5310	Interviewers, except eligibility and loan	43-4111	other business, professional and technical services
5350	Order clerks	43-4151	other business, professional and technical services
5400	Receptionists and information clerks	43-4171	other business, professional and technical services
5410	Reservation and transportation ticket agents and travel clerks	43-4181	other business, professional and technical services
5420	Information and record clerks, all other	43-4199	other business, professional and technical services
5600	Production, planning, and expediting clerks	43-5061	other business, professional and technical services
5700	Secretaries and administrative assistants	43-6010	other business, professional and technical services
5820	Word processors and typists	43-9022	other business, professional and technical services
1320	Aerospace engineers	17-2011	research, development and testing services
1330	Agricultural engineers	17-2021	research, development and testing services
1440	Marine engineers and naval architects	17-2121	research, development and testing services
1450	Materials engineers	17-2131	research, development and testing services
1600	Agricultural and food scientists	19-1010	research, development and testing services
1610	Biological scientists	19-1020	research, development and testing services
1640	Conservation scientists and foresters	19-1030	research, development and testing services
1720	Chemists and materials scientists	19-2030	research, development and testing services
1740	Environmental scientists and geoscientists	19-2040	research, development and testing services
1760	Physical scientists, all other	19-2099	research, development and testing services
1900	Agricultural and food science technicians	19-4011	research, development and testing services
1910	Biological technicians	19-4021	research, development and testing services
1920	Chemical technicians	19-4031	research, development and testing services

Table A.4. Summary Statistics and a Simple Differencing Approach for the Industry-Exposed Sample

	Industry-Exposed Sample (N = 24,261)								
	Industry Switching			Change in Unemployment			Change in Earnings		
	Exposed	Exposed - Unexposed		Exposed	Exposed - Unexposed		Exposed	Exposed - Unexposed	
	Mean	Mean	*t*	Mean	Mean	*t*	Mean	Mean	*t*
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1996	0.27	-0.011	-0.91	-0.005	0.000	-0.07	0.054	-0.020	-0.95
1997	0.26	-0.005	-0.43	0.002	0.009	2.58	0.023	-0.065	-2.96
1998	0.28	0.011	0.99	-0.003	0.004	1.38	0.042	-0.087	-4.32
1999	0.30	0.024	2.12	-0.001	0.006	2.06	0.023	-0.034	-1.67
2000	0.28	-0.007	-0.59	-0.002	0.003	1.05	0.018	-0.084	-4.08
2001	0.30	0.022	2.09	0.005	0.007	2.43	0.017	-0.076	-4.19
2002	0.38	0.069	6.43	0.007	0.009	3.21	-0.031	-0.107	-5.74
2003	0.31	0.023	2.18	-0.003	0.003	0.89	-0.018	-0.053	-2.89
2004	0.31	0.030	2.68	-0.002	0.003	0.96	-0.039	-0.082	-4.24
2005	0.31	0.025	2.28	-0.005	0.000	0.04	0.004	-0.047	-2.46
All Years: *n* -digit industry switching									
4-digit	0.30	0.020	5.67						
2-digit	0.25	0.020	6.05						
1-digit	0.23	0.022	6.89						

Notes : This table is the industry-exposed counterpart to table 2. The table is identical in structure to table 2 so see the table 2 notes.

Table A.5. Sample Statistics

	Industry-Exposed Sample (N = 24,261)			Occupation-Exposed Sample (N = 37,550)		
	Mean	Std. Dev.	Difference	Mean	Std. Dev.	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Industry Switch						
4-digit industry switch	0.30	0.46	0.020 *			
2-digit industry switch	0.25	0.43	0.020 *			
1-digit industry switch	0.23	0.42	0.022 *			
Occupation Switch						
4-digit occupation switch				0.31	0.46	0.009 *
2-digit occupation switch				0.20	0.40	-0.020 *
1-digit occupation switch				0.17	0.38	-0.021 *
Employment and Earnings						
change in unemployment	0.00	0.11	0.004 *	0.00	0.11	0.002
log annual earnings	10.39	0.90	0.619 *	10.23	0.87	0.310 *
change in annual earnings	0.01	0.74	-0.066 *	0.02	0.70	-0.040 *
experience	20.04	10.83	-0.449 *	20.79	11.00	0.289 *
Education						
schooling	14.48	2.20	0.940 *	14.14	2.06	0.461 *
high-school dropout	0.02	0.14	-0.081 *	0.02	0.13	-0.066 *
high-school graduate	0.22	0.42	-0.072 *	0.26	0.44	-0.027 *
college dropout	0.20	0.40	0.005	0.22	0.42	0.018 *
college graduate	0.56	0.50	0.147 *	0.50	0.50	0.075 *
Skills						
skilled white-collar	0.57	0.49	0.138 *	0.50	0.50	0.057 *
less-skilled white-collar	0.38	0.48	-0.105 *	0.50	0.50	-0.057 *
skilled blue-collar	0.03	0.17	-0.002	0.00	0.00	0.000
unskilled blue-collar	0.02	0.15	-0.031 *	0.00	0.00	0.000
Other Demographics						
married	0.68	0.47	0.084 *	0.68	0.47	0.066 *
male	0.46	0.50	0.105 *	0.38	0.48	-0.072 *
white	0.88	0.32	0.033 *	0.88	0.32	0.014 *
veteran	0.09	0.28	0.025 *	0.07	0.26	-0.009 *

Notes: Columns 1 and 2 report means and standard deviations for the industry-exposed sample. Column 3 is the difference between private service workers aged 18-64 in exposed less unexposed industries. See section 4 for a discussion. An asterisk (*) indicates that the difference is statistically significant at the 1% level. Columns 4-6 are the corresponding statistics for the occupation-exposed sample.

Table A.6. Worker Characteristic Coefficients

	Industry-Exposed Sample			Occupation-Exposed Sample		
	Industry switch	Change in unemployment	Change in earnings	Occupation switch	Change in unemployment	Change in earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Experience	-0.0067			-0.0076		
	(-4.19)			(-3.99)		
Experience^2	0.000082			0.000092		
	(2.84)			(3.72)		
Schooling	-0.0091			-0.0076		
	(-3.40)			(-1.89)		
Married	-0.0398			-0.0387		
	(-14.36)			(-8.00)		
Male	0.0092			0.0093		
	(5.34)			(0.74)		
White	-0.0602			-0.0632		
	(-5.33)			(-7.69)		
Veteran	0.0302			0.0515		
	(1.55)			(3.24)		
ΔExperience^2		0.000074	-0.0013		0.000045	-0.0011
		(3.86)	(-4.62)		(4.26)	(-15.85)

Notes: This table reports the coefficients on the worker characteristic controls. Columns 1 and 4 go with columns 1 and 2 of table 3, respectively. Columns 2 and 5 go with columns 1 and 2 of table 5, respectively. Columns 3 and 6 go with columns 1 and 2 of table 6, respectively. Columns 1 and 4 report marginal probabilities while the remaining columns report OLS coefficients. *t*-statistics are clustered at the industry or the occupation levels.

Table A.7. First-Stage IV Estimates

	Industry-Exposed Sample		Occupation-Exposed Sample	
	$\ln(M^{CI})$	$\ln(X^{CI})$	$\ln(M^{CI})$	$\ln(X^{CI})$
	(1)	(2)	(3)	(4)
$\ln(M^{CI}/M^{G8})$	0.92	0.32	0.77	0.08
	(10.38)	(6.92)	(11.03)	(1.09)
$\ln(X^{CI}/X^{G8})$	-0.28	0.82	0.03	0.85
	(-3.90)	(8.57)	(0.26)	(7.64)
R^2	0.87	0.80	0.87	0.74
N	24,261	24,261	37,550	37,550

Notes: This table reports the first-stage results for the IV estimates presented in table 4. t statistics are in parentheses and are clustered at the industry level (for the industry sample) or the occupation level (for the occupation sample). All the explanatory variables in the second stage are also included in the first stage, but are not reported here.

Table A.8. Switchers: Receiving Industry and Change in Annual Earnings

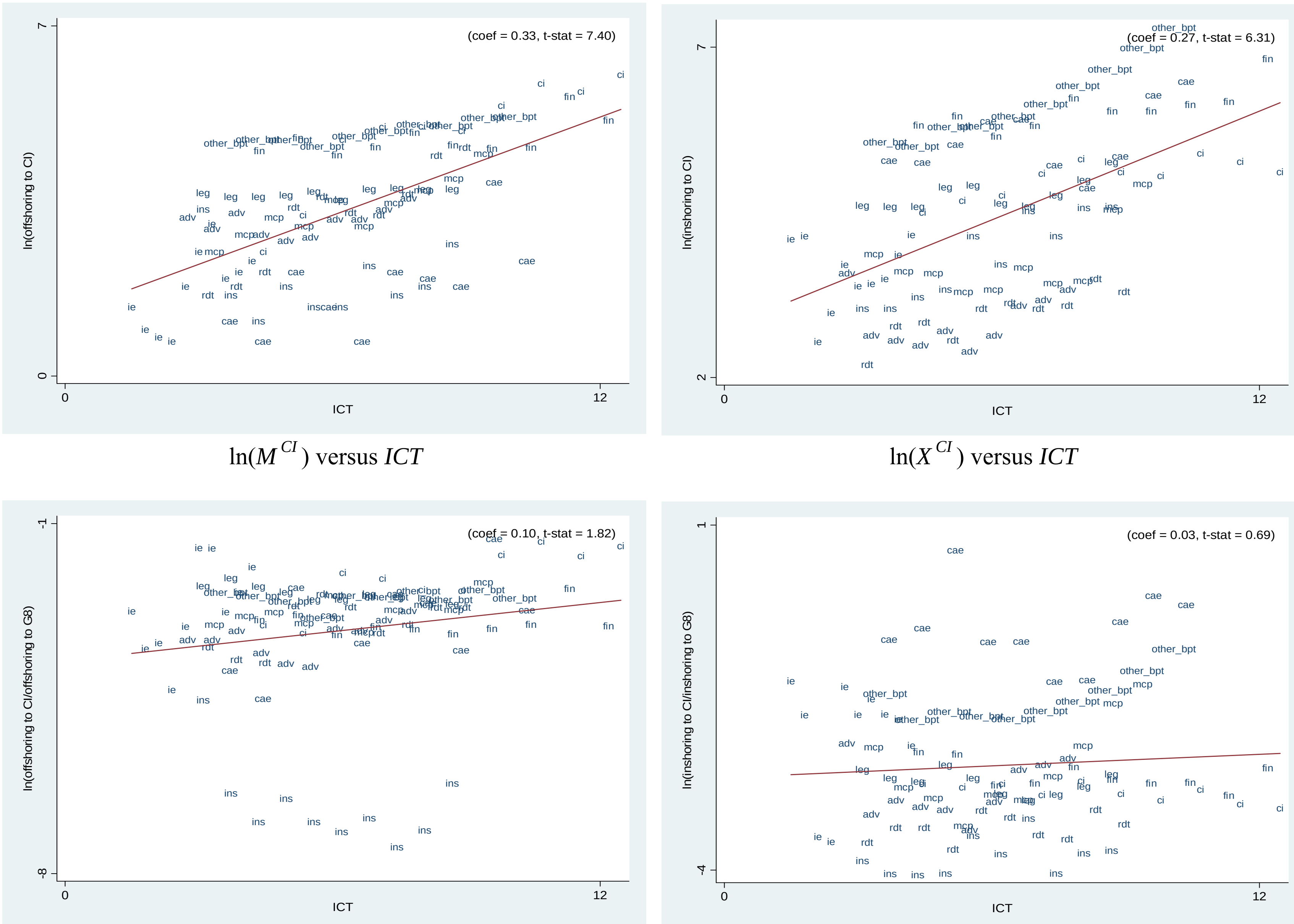
Industry-Exposed Sample

	Stayers	**Switchers: Receiving Industry**				
		Exposed Private-Sector Services	Non-Exposed Private-Sector Services	Wholesale and Retail	Manufac-turing	Other
Number of Switchers	16903	2472	2115	1021	869	881
(%)	(70%)	(10%)	(9%)	(4%)	(4%)	(3%)
Change in Annual Earnings	3%	1%	-11%	-15%	1%	0%

Occupation-Exposed Sample

	Stayers	**Switchers: Receiving Occupation**			
		Exposed Private-Sector Services	Non-Exposed Private-Sector Services	Production	Other
Number of Switchers	26022	5459	5,082	346	641
(%)	(69%)	(15%)	(14%)	(1%)	(1%)
Change in Annual Earnings	3%	4%	-7%	-10%	0%

Figure A.1. Inshoring and Offshore Outsourcing Plotted Againt ICT



$\ln(M^{CI})$ versus ICT $\quad$ $\ln(X^{CI})$ versus ICT

$\ln(M^{CI} / M^{G8})$ versus ICT $\quad$ $\ln(X^{CI} / X^{G8})$ versus ICT

Notes: See appendix 4.