Globalization and Temporary Worker Employment in Vietnam

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February 2026

Abstract

Many concerns surround the continuing globalization of commerce and employment, including the concern that these processes have led to unstable working conditions, including more use of temporary workers. Despite these public fears, the trade literature to date has found little evidence that either exporting or importing leads to hiring a higher share of temporary workers. We analyze whether increased engagement in international trade has led to changes in the use of temporary workers in Vietnam, a country that has recently rapidly integrated into the world economy. Using data from two six-year balanced panels of the Vietnamese Enterprise Survey, covering 2010-2015 and 2017-2022, we utilize propensity score matching techniques to look for the effect of engaging in international trade on labor force composition in the manufacturing, wholesale and retail trade, and services sectors. We find during both time spans that firms newly engaging in international trade make lower use of temporary workers, both relative to non-traders, and overall, even as they maintain their overall employment and raise their wages.

Keywords: Vietnam, trade, temporary workers

JEL classifications: D22, F16, J23

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[5] We thank Lewis Davis, Huan Ha Thuc, and Yizhao Wang for helpful comments on earlier versions of this paper.

1. Introduction

Globalization has had and continues to have wide-reaching effects. One of the most direct effects has been to transform work for many people around the world, as more companies engage in exporting and importing goods and services, making them more subject to international competition and labor standards. The standard economic argument is that comparative advantage and increased labor competition should lead to rising living standards, reduced consumers' costs and wage equalization across space. But a number of authors (Balsvik et al., 2015; Autor et al., 2014; Biscourp and Kramarz, 2007) have put forth counterarguments that globalization leads to job destruction or less secure working conditions as companies and countries vie to attract foreign capital that looks for the highest return across the globe.

One of the concerns regarding labor market consequences of international trade is whether it leads to expanded use of temporary workers, who can be defined as those workers who do not have a secure ongoing employment contract with the firm in which they work, and do not qualify for social insurance benefits from the firm. The International Labour Organization (2017) estimates, based on World Bank Enterprise Surveys, that exporters hire a larger share of temporary workers, although the temporary-worker-share-gap between exporters and non-exporters depends on the particular industry. On the one hand, hiring temporary workers allows trading firms to reduce labor costs in the face of global competition while keeping their employment flexible in order to buffer against global uncertainty shocks. On the other hand, trading firms tend to pay higher wages, employ better workers, and are more productive than nontrading firms, which also implies that they may need to hire relatively more permanent workers than non-traders so as to ensure higher skill and productivity levels among their workforce. Trading firms may also face higher levels of scrutiny regarding their labor practices from both international buyers and local regulators, leading them to utilize more formal hiring and pay practices. Globalization may also lead to primary-secondary labor market effects where there is sorting by firms across tradable and non-tradable sectors, wherein conditions may improve in one sector but deteriorate in the other sector. Therefore, how globalization influences the hiring practices of firms regarding their use of temporary versus permanent workers is an empirical question concerning how these tradeoffs affect firm choices among both traders and non-traders.

Vietnam provides a particularly interesting case for testing the link between globalization and temporary workers. Vietnam has been put forth as a success story for the power of opening markets to raise standards of living and create the conditions for potential improvements in labor markets. Since Vietnam underwent structural changes in 1986 that moved it towards its current socialist-oriented market economy, it has gone through additional waves of openness: reducing the role of state enterprises; opening up increasingly to trade with other nations; becoming open to foreign direct investment — and

in general encouraging the development of a competitive private sector. In addition to providing additional information on the Vietnamese economy, our research contributes to the literature on the overall effect of globalization on temporary employment, which has generated quite diverse insights and results to date.

Along with these conditions for testing the effect of opening up to global markets, Vietnam has robust and thorough enterprise-level data collection that allows us to observe its formal sector firms over substantial spans of time to see how industries have evolved over time in their use of workers. In particular, we consider whether there have been changes in the use of temporary workers over this period, and contrast their use by firms engaging in international trade ("trading firms") with firms not engaging in international trade ("nontrading firms") during this period. The large sample allows us to first consider what factors are important in leading firms to become exporters. We then use propensity score matching methods in order to better match trading and nontrading firms on observable characteristics in order to mitigate endogeneity and comparing apples-to-oranges issues. Our empirical results suggest that becoming a trading firm is associated with having a lower share of workers that are classified as temporary workers, and is associated with higher total employment and higher wages. Thus, there is little evidence from our analysis that engaging in international trade makes jobs more precarious.

Regarding the subsequent layout of the paper, Section 2 reviews the relevant literature, and Section 3 explains the empirical strategy. Section 4 discusses the data, and Section 5 discusses our results, while Section 6 presents our specification analysis and balancing test for the goodness of matching of the propensity score matching method. Section 7 concludes.

2. Literature Review

Globalization and temporary workers

While engagement in international trade is not the sole way in which one could measure and describe globalization, it provides a useful first cut at thinking about globalization's effects on the labor market. The relationship between trade and employment can be quite complex and dynamic, as it can vary significantly across economies and localities (Autor et al. 2013; Dauth et al. 2017; Dix-Carneiro and Kovak 2019). In this paper, we consider a particular aspect of labor market effects of trade: whether or not the increase in firms' engagement in international trade has had measurable effects on the category of temporary workers, and if so, whether it has increased or decreased their prevalence.

Temporary workers have not been heavily studied in the past, at least in the developing country context, but are a category of increasing interest to researchers, with recent studies showing that temporary workers that are otherwise comparable to permanent workers earn less money (Fauser and Gebel, 2023; Comi and Grasseni, 2012;

Perugini and Pompei, 2017). Other studies have instead considered the larger category of informal workers, which would include all those workers whose employers do not contribute to the country's social security system (which would include those in the informal sector who may not have an employer of record) and find similar precarity of relative earnings compared to permanent workers (cf. Duman, 2023; García, 2017). Hence any increase in this category, holding the overall employment constant, would be evidence of changes to the worse for workers' relative positioning.

A rise in the demand for temporary workers is also seen as a direct outcome of the reallocation friction inherent in this selection effect of increased international competition due to trade leading to labor, especially skilled workers, being reallocated from shrinking and less productive firms to expanding and more productive ones. In this transition, firms find themselves relying more on temporary workers to manage their export orders or fill in the gaps left by departed workers (Houseman, 2001; Ono, 2009). Ono and Sullivan (2013) find evidence that firms tend to use temporary workers to reduce the costs associated with letting go of permanent workers, and they do so when their output is expected to decline. Machikita and Sato (2016) uncover that industries with declining world share of value added lower their permanent worker employment. Yokoyama et al. (2021) find that exposure to trade and the appreciation of the Japanese yen led to a spontaneous decrease in the sale of exporters and hiring of non-regular workers but did not lower regular employment, where non-regular workers are not identical to temporary workers but have more job precarity than regular workers, including less or no opportunity for job advancement. This study also provides evidence that the use of temporary workers is a popular tool for exporters to absorb exogenous shocks and adjust themselves in an uncertain business environment. Tanaka (2013) reaches a stronger positive result that Japanese firms starting to export increase their overall employment, but do not increase their share of non-regular workers.

Openness to trade also means opening closed internal markets to imports, and the employment effects of exposure to import competition have been found to be generally negative. Malgouyres (2017) finds that local employment and total labor income are negatively affected in both manufacturing and non-manufacturing sectors in France as a result of exposure to import competition from China. Colantone et al. (2019) find that import competition has a large negative effect on individual workers' mental health, and it disproportionately hurts specific groups of workers including those with a poor financial condition, temporary contract or blue-collar job.

The literature has also identified uncertainties in macroeconomic conditions as a main driver of the rise in temporary employment. Holmlund and Storrie (2002) find that adverse macroeconomic conditions such as the severe recession in the 1990s in Sweden are behind the rise in temporary work as they make firms more inclined to offer temporary contracts and workers more willing to accept them. Devicienti et al. (2018) find that product

market volatility increases firms' propensity to use temporary contracts. Fernandes and Ferreira (2017) find that firms in financially constrained industries hire a greater proportion of fixed-term or temporary workers relative to less financially vulnerable firms. Rodríguez-Gutiérrez (2006) shows that changes in temporary workers are also a natural part of the business cycle as firms adjust their proportion of temporary workers in efforts to raise their sales or improve their market dynamism.

In addition, studies have also noted the tremendously important role of labor market regulations and institutions, such as employment protection legislation, or the strength of labor unions, in moderating the labor effects of trade liberalization (Ahlquist and Downey, 2023; Lee, 2022; Bentolila et al., 2012; Fialová and Schneider, 2009; MacPhail and Bowles, 2008; Bentolila et al., 1994). However, labor regulations might lead firms to hire fewer permanent and more temporary workers. At the same time, regulations to increase flexibility of employment by allowing firms to hire more workers on temporary contracts without reforming the protection provided to permanent workers might end up generating a worrisome dual tier worker system while strengthening the wage-setting ability for permanent workers (Bentolila et al., 2012). Chen (2018) finds that upward wage rigidity is a driver of the increase in temporary employment in Japan and that a revision to the Dispatch Worker Law of 2009 significantly raised temporary workers' wages and improved market efficiency. This notion is strengthened by Aguirregabiria and Alonso-Borrego (2014), who find a permanent substitution of permanent by temporary workers as a result of a labor market reform in Spain that removed restrictions on fixed term or temporary contracts. On the other hand, while this appears to be the story in more developed economies with higher wages, it may also be that adding regulations making it harder and costlier to hire temporary workers, as has happened in Vietnam, might have the opposite effect, increasing the overall use of permanent workers relative to temporary workers without reducing the overall size of the workforce. This effect has been less studied, but is consistent with two-sector models where encouraging a shift from a low-attachment low-skill economy to a high-attachment high-skill economy could improve overall well-being.

The link between trade and shifts in the distribution of workers by skill has been noted among exporting firms. Brambilla et al. (2012) find that Argentine firms exporting to high-income countries employ more skilled workers than other exporters and domestic firms. Using the data on the Mexican manufacturing sector, Verhoogen (2008) finds that exposure to trade is linked to greater wage inequality among high-skilled and low-skilled workers, as the most productive plants enter trade, produce higher-quality goods than less productive plants, and pay higher wages to retain a higher-quality workforce. Bernard and Jensen (1997) find that employment increases in U.S. exporting plants contribute substantially to the increase in relative demand for skilled labor in manufacturing in the 1980s. Firm-level evidence indicates that larger and more productive firms are more likely to participate in

trade and pay higher wages than smaller domestic-focused firms. Friberg and Sanctuary (2020) find that firms exposed to higher exchange rate risk hire a greater share of skilled labor, and that the effect is especially pronounced among trade-intensive firms and small firms with fewer than 50 workers. Martins (2011) provides evidence that foreign firms attract the best workers by offering large wage increases. Skill level is likely correlated with permanence, although we are unable to observe this in our particular data set and thus cannot corroborate this.

In sum, the optimistic story would be that increased trade, perhaps in concert with a relatively stable macroeconomic environment, and in tandem with changes in the overall labor regulations environment, can lead to an increased number and increased share of permanent workers relative to temporary workers in the workforce. We shall consider whether the evidence for Vietnam supports this story, or is consistent with a less optimistic one.

Vietnam's globalization process and current economic environment

While Vietnam has increasingly opened to international trade starting in 1986, the most striking event over the recent economic history of Vietnam is its WTO entry in 2007. According to the World Bank, Vietnam's exports of goods and services as a percentage of GDP rose from 7 percent in 1986 to 84 percent in 2018.[1] A number of papers have concentrated on the effects of this entry on the Vietnamese economy, including the differential effects on private firms vs. state-owned enterprises, the increases in productivity for private firms, and the higher probability of exit and lower firm profitability subsequent to WTO entry (Baccini et al., 2019). Poverty rates were already dropping substantially before the WTO entry (again with the caveat that these drops were not shared equally across urban and rural areas, and across parts of the country) and commentators were generally crediting increased trade liberalization—in particular the increased volume of exports rather than any negative effects of import substitution (Tran and Heo, 2009; Le et al., 2019)—for these improvements in the earlier period (notably, exports per capita tripled from 1985 to 1990—see Heo and Nguyen, 2009: Table 7), ever since free market reforms began with the regime change in late 1986. The 2001 US-Vietnam Bilateral Trade Agreement also seems to have stimulated wage growth and poverty reductions in Vietnam as well as continuing to attract foreign firms who generally are exporters (McCaig et al., 2023; McCaig, 2011).

The environment for firms in Vietnam appears to be highly competitive, even for the remaining state-owned enterprises. Turnover rates are high in the informal business sector in Vietnam, with entry around 15 percent and exit around 19 percent annually (McCaig and Pavcnik, 2021). That said, overall concentration appears to be quite low in Vietnam based

[1] https://data.worldbank.org/indicator/NE.EXP.GNFS.ZS?locations=VN

on the concentration measures that we have calculated in another paper (Jacobsen and Lee 2025).

Over this period, Vietnam has also regularized its labor code, first enacted in 1994, so as to be more consistent with international standards. This included a significant revision in 2012 that standardized definitions of standard working hours and limited allowable overtime, required employers to contribute to social insurance funds, required severance pay based on length of employment, expanded workplace safety and health standards, and put stricter controls on the employment of minors.[2] Vietnam increased its social insurance requirements as of 2016 so that all firms are supposed to contribute for all workers that work at least one month for them. Additional revisions in 2019 (active as of 2021) also clarified that workers could not continue on an indefinite series of fixed term contracts; after thirty-six months on fixed term, or temporary, contracts they would be considered to have entered into an indefinite-term, or permanent, worker contract.[3] These are important changes in labor regulations that occur during our study period and allow us to see if they affect overall patterns of temporary vs. permanent worker use alongside the effects of trade on worker type use.

While numerous papers have now documented the apparent success of Vietnam's opening up to trade, few papers, for either Vietnam or other countries, have considered, using detailed firm data, what effect the changing trade environment may have on particular categories of workers. In particular, the question is whether trade and export orientation leads to growth in less secure forms of employment or growth in secure employment. For Vietnam, McCaig and Pavcnik (2018) consider the shift of workers from the informal to the formal sector and find reallocation of manufacturing workers into the formal sector. However, they do not consider the types of workers within the formal sector by their level of employment security.

Hence, our paper is the first to explore this story of globalization and temporary versus permanent employment in the case of Vietnam. We first consider the effect of trade on the share of temporary workers, using data from the universe of Vietnamese firms. We explore this over a decade of data, divided into two six-year balanced panels, ensuring the stability and long-term implication of our results. We also consider the implications of our findings both for the Vietnamese economy as a whole and as a story more generally about how temporary employment may be affected by globalization processes.

[2] https://asean.org/wp-content/uploads/2016/08/Labor-Code-No.-10-Year-2012.pdf.
[3] https://www.economica.vn/Content/files/LAW%20&%20REG/Labor%20Code%202019%20ENG.pdf.

3. Empirical Strategy

We want to get a clean measure of the effect of being a trading firm (as in being involved in exporting and importing, where there are few firms who do not do both) on the share of temporary workers out of all employees, as well as seeing if these firms expand their total employment. Since a firm's decision to start trading is likely to be endogenous to other firm characteristics, we need to control for systematic observed differences between traders and non-traders. Propensity score matching (PSM), whereby we estimate propensity to trade and match trading and nontrading firms with similar estimated values, is an appropriate methodology to address this potential endogeneity issue. Using this method, we compare newly trading firms to nontrading firms who are otherwise highly similar to the new traders at the time when trading begins. We first calculate propensity scores, which represent the likelihood to start trading, using selected firm characteristics, and then use them to identify a suitable control group. Using the control group (firms that never trade during the study period and that have the closest propensity scores to the treatment group) as the counterfactuals for the treatment group (firms that started to trade after the first two years), we calculate the treatment effect of starting to trade by taking the difference in the outcome of interest between these two groups, and also see how the difference evolves over time.

We implement the PSM methodology following the example of Tanaka (2013), who applied this technique, including following firms for several years, to his comparison of trading and nontrading Japanese firms in their use of non-regular workers. Specifically, in the first stage, we estimate the probability of starting to trade internationally as a function of total factor productivity (*TFP*), log of capital intensity (*lnKAPINT*), whether a firm is foreign-owned (*FOREIGN*, where =1 is foreign-owned, 0 otherwise), and log of size (measured as number of employees, *lnL*), and log of age (*lnAGE*), using a logit regression as shown below in equation (1):

$$Start\_to\_trade_{it} = F(TFP_{i,t-2}, lnKAPINT_{i,t-2}, FOREIGN_{i,t-2}, lnL_{i,t-2}, lnAGE_{i,t-2}). \quad (1)$$

The left-hand side of equation (1) is the probability of firm i to start engaging in international trade in year t. The estimation includes non-traders and trade-starters, and is run separately by industry sector (manufacturing, wholesale and retail, services). We discuss the definitions of the trading groups in the data section. We calculate *TFP* following the method used in Levinsohn and Petrin (2003). We lag the explanatory variables by two years; all variables are logged except *Foreign*. Our logistic regressions also include year and industry (using 388 4-digit Vietnam Standard Industrial Classification codes) fixed effects. Once we obtain the logistic estimation results, we match trade-starters with non-traders that have a

similar propensity to trade-starters. We use three different matching methods: neighbor with replacement (so the same control unit can be used more than once) for one nearest neighbor and three nearest neighbors, and kernel matching, where all units in the control group are used, but are weighted based on their proximity to the treatment case. Then, we estimate the "average treatment effect on the treated" (ATT) using the matched non-traders as the control group. We present these results in section 5.

There are potentially other analysis methods that could be used instead of PSM, but they have their own drawbacks. While by definition we cannot match on unobservables, which can be driving the movement into trade, one could imagine using an IV approach. The issue with an IV approach is choosing a good IV. Given that there is no obvious IV to use here, the choice of a bad IV can be worse than not using one at all, and we present the PSM method as an alternative way of simulating experimental data, recognizing its inherent limitations. However, there are still ways to consider whether PSM is a reasonable approach, so in order to assess the goodness of matching of our PSM method, we perform specification analyses and balancing tests. We present these results in section 6.

4. Data

We use firm-level data from the Vietnamese Enterprise Survey (VES) by the General Statistics Office in Vietnam. The VES is an annual survey that spans the entire universe of Vietnamese firms. The VES includes labor, export, and financial information, among other features, although it does not include detailed data on the composition of the labor force so we cannot, for instance, observe what share of temporary workers are skilled or unskilled. We focus in particular on manufacturing, wholesale and retail, and services industries, where trade-starters are more likely to be found; some data are also shown for transportation and construction.

We use two balanced panels of six consecutive years each (2010-2015 and 2017-2022) of the data for which international trade information is available; we refer to these two panels as the 2012 cohort and the 2019 cohort, based on the year in which the new traders begin to export and/or import. We do not use the 2016 data, which does not include information on whether a firm is an exporter or an importer. These time spans allow us both to define trade-starters and to then follow them for several years after they begin to trade; having two separate panels allows us to reset the measure of similarity between trader-starters and non-traders, and also to see whether our results are similar over a longer period where there are also labor law changes occurring that may make the second period look different from the first. In each panel, we define a firm as a trade-starter if: 1) it did not trade in the first two years; 2) it started trading in the third year; and 3) it continued trading through the last year in the panel. Non-traders are those that did not trade at all during the six years, and always-traders are those that traded in all the years. Others are all other firms that do

not fall into those three categories. Those firms may have traded in the early years and quit in the later years, or may have traded intermittently during the sample period. For our PSM analysis, we use only the trader-starters and the non-traders.

Table 1 shows the number of firms in each cohort by trading type (non-traders, trader-starters, always-traders, and others) and industrial sector. In both cohorts, manufacturing has the most trader-starters and always-traders, as expected, but 33 percent of all trade-starters across the two panels are found in wholesale and retail trade and services industries, though the percentage drops significantly by the second panel. Hence, we show full analyses in our following tables for all three of these areas, in part to contrast the results across the three sectors.

Our main dependent variable of interest is the share of temporary workers in the firm, which we have to estimate, given the lack of a standard definition of a temporary worker. According to the 2013 Vietnam Labour Force Survey (General Statistics Office, 2014), 38.7 percent of all workers are employed without a labor contract in Vietnam. The VES survey includes the number of employees for whom the employer does not contribute to social security, along with the number of regular employees for whom the employer does contribute to social security. We define the former category as temporary workers. As Table 2 reports, in the VES, the share of temporary workers out of all workers decreased from 38 percent in 2010 to 29 percent in 2022.[4] Our estimation of temporary workers from the VES is similar but not identical to the Labour Force Survey calculation of workers employed without a labour contract; we have 34.0 percent for 2013. In our data, there is a clear decline in the use of temporary workers in manufacturing over this period which appears to drive the overall decrease in temporary workers over this time frame, as the other sectors actually increase their use of temporary workers over this time. There is some evidence that the newer regulations that took effect in 2021 regarding temporary workers not being able to continue indefinitely on fixed term contracts have reduced the 2022 percentage relative to the 2019 numbers.

Table 3 shows the pairwise correlations in the firm-level data between the share of temporary workers and other firm characteristics, including profit, total factor productivity, capital intensity, age of firm, whether the firm is foreign owned, and the size of the firm, measured by total number of employees. We do not use profit, directly available from the VES, as an explanatory variable in our main results, but we use it instead of TFP in our matching equation to see if it changes our results; the results are reported for the 2012

[4] The Law on Social Insurance (Law No. 58/2014/QH13) that requires all employers to contribute to the social insurance for employees who work more than one month became effective on January 1, 2016. It repeals the Law on Social Insurance (Law No. 71/2006/QH11) of 2006. See the page on the law at the International Labour Organization (https://natlex.ilo.org/dyn/natlex2/r/natlex/fe/details?p3_isn=99775) for more detail. However, this change does not appear to have created a break in the time series.

cohort in Appendix Table 1. Capital intensity is calculated as the value of fixed assets divided by the total number of employees, where value of fixed assets is in millions of Vietnamese Dong. Age is the data year minus the year a firm is established, which is not calculatable consistently for the 2019 cohort, so is excluded from the correlations and from the subsequent logit regression. There is a statistically significant negative correlation between temporary worker share and all the other firm-level variables. The finding that small firms utilize proportionately more temporary workers is consistent with the findings for Japan. In comparing the two periods, the negative relationship between temporary worker share and foreign ownership increases by the second period and the relationship to total factor productivity lessens but stays negative.

Table 4 shows the descriptive statistics of the firm labor force variables that we are tracking. The first variable is the percentage of the temporary workers out of all workers at the firm level. For both cohorts, the mean temporary worker share is highest among non-traders, followed by others, trader-starters, and always-traders. The second variable is the total wage bill divided by the number of employees, or the wage per worker by firm. The mean wage per employee is the highest for trade-starters and firms that trade consistently, followed distantly by other firms and non-traders. The third variable, employment per firm, is substantially higher for the always-traders, followed by trader-starters. Therefore, traders, both trader-starters and always-traders, offer the largest share of regular (i.e., non-temporary) jobs and higher wages and are larger firms. We control for the employment size difference effect on propensity to start trading in our regressions so that we are not comparing apples to oranges, but we also track the changes in employment size after firms start to trade to see how it affects their workforce size along with their proportion of temporary workers. But it is also the case that if we simply compared trading firms (both always traders and trader-starters) to non-traders, that it would be decisive that non-traders utilize a higher proportion of temporary workers in their workforces, as well as being smaller and paying lower wages.

Table 5 provides the descriptive statistics for the model's explanatory variables. In our sample, TFP varies substantially across time and sector but is rising over time except for in wholesale and retail. Capital intensity is greatest in transportation, followed by wholesale and retail trade and manufacturing. Foreign ownership is much higher in manufacturing than in the other sectors, and is lowest in transportation. Employment per firm is highest in manufacturing, as is the average age of a firm.

5. Estimation Results

Main results

Table 6 reports the results of the logistic regression specified in equation (1), including calculated average marginal effects in the lower panel. For the 2012 cohort, for

manufacturing, as shown in Panel A, Column (1), a 10 percent increase in capital intensity is associated with 4.2 percentage points higher chance of becoming a trader. Foreign ownership leads to 1.6 percentage points higher chance of becoming a trader. Lastly, a 10 percent increase in employment is associated with 12.8 percentage points higher chance of becoming a trader. Wholesale and retail trade also has positive relationships with capital intensity and size, but at a smaller scale. For services, TFP has a negative relationship to starting to trade, and smaller positive effects of foreign ownership and size.

For the 2019 cohort, as shown in Panel B, for manufacturing (Column 1), the positive effects of capital intensity, foreign ownership, and employment size on probability of becoming a trader become larger than in the 2012 cohort, and TFP becomes statistically significant. For wholesale and retail trade capital intensity and foreign ownership also increase in impact, while employment size is still significant but drops in impact. For services, the effect of TFP becomes insignificant, while the other coefficients are significant and have increased impact. The overall fit of the regressions, measured by pseudo-R-squared, improves for the later cohort.

Table 7 shows the relative effect of becoming a trader on firm employment as the first application of our matching technique. In Table 7 we use the "Average Effect of the Treatment on the Treated", or ATT, in order to judge the effect of the treatment, in this case, of becoming a trader relative to being a non-trader. In Panel A, for the 2012 cohort, for all three sectors, the ATTs are positive and statistically significant at one, two, and three years after starting to trade using all three matching methods, though the "nearest neighbor" method for the services sector is borderline statistically significant. In Panel B, for the 2019 cohort, this pattern holds up for manufacturing, for wholesale and retail trade, and for the "three nearest neighbors" and "kernel matching" methods for services, but the effect becomes insignificant for the "one nearest neighbor" method. Overall, this is strong evidence that becoming a trader is associated with higher average labor force size than for firms that remain as non-traders.

We now see if the effect of becoming a trader leads to the firm recomposing their employment to increase or decrease relative use of temporary workers, even as they have higher total employment. The effect of trading on temporary worker share of employment is shown in Table 8. In Panel A, for the 2012 cohort, for all manufacturing and for wholesale and retail trade, the ATTs are negative and statistically significant at one, two, and three years after starting to trade using all three matching methods, and the share drops for the treatment group from t+1 to t+3. For services, the kernel method yields similar patterns and there is a decline in use of temporary workers from t+1 to t+3, though the "nearest neighbor" method for the services sector does not yield statistically significant ATTs. Hence the overall evidence is that the new traders are less likely to use temporary workers than comparable non-traders.

In Table 8 Panel B, for the 2019 cohort, for manufacturing, there is less evidence supporting any immediate difference at time t+1 between new traders and non-traders for all three sectors, but then at times t+2 and t+3 the ATTs become negative and statistically significant. Wholesale and retail trade actually shows a positive statistically significant effect of starting to trade at time t+1 in the three nearest neighbors and kernel results, but the effect then switches to negative and statistically significant at times t+2 and t+3. Services similarly show a positive effect (kernel) at time t+1, switching to a negative statistically significant effect at times t+2 and t+3. Hence, for both firm cohorts, there is strong evidence that starting to engage in trade is associated with lower use of temporary workers relative to non-traders.

In Table 9, we see if these reductions in temporary worker use by the trading firms occur along with a change in average wages. In all times after starting to trade, for the 2012 cohort, the manufacturing and wholesale and retail trade starter firms have higher wages than the non-traders. The results are in similar direction, though slightly weaker for services firms, as only the kernel method delivers fully statistically significant results. These patterns hold up for the 2019 cohort (with the exception of the one nearest neighbor at time t+2 for wholesale and retail trade, which is statistically insignificant).

Having now shown that the treatment group has higher employment, lower use of temporary workers, and higher wages than the control group at each point after beginning to trade, the final question we address with our data is whether these three variables change in magnitude for the treatment group over the three years following their beginning to trade. Table 10 shows that the differences in trade starters' total employment and temporary worker share between t+1 and t+3 are generally statistically insignificant, with the only exceptions being a drop in both employment and temporary worker share in wholesale and retail trade for the 2012 cohort. Table 10 also shows that for manufacturing firms in both cohorts and for wholesale and retail firms in the 2012 cohort, the mean nominal wage of the treated group three years after starting to trade is higher than one year after. For services in both cohorts and for wholesale and retail in the 2019 cohort, the mean nominal wage is not statistically significantly higher. Wages in the VES are nominal wages; real wages calculated using Vietnam's consumer price index over these time periods also indicates growth between t+1 and t+3 for workers in both the treatment and control groups. Hence workers both overall but particularly in the treatment group experience real wage growth over these time periods.

Robustness tests

Just as our results do not in general appear to be driven by the choice of matching method, our results do not appear to be heavily dependent either on the variables used in the matching process, or on our use of both importing and exporting firms in our definition

of trading firms. In Appendix Table A1, we redo our analyses for the 2012 cohort using the two-year lagged log of profit instead of TFP, and, in Appendix Table A2, we redo our analyses for the 2012 cohort using only firms newly engaged in exporting (rather than possibly importing); we are unable to do this for the 2019 cohort because the survey questionnaires for the later panel combine exporting and importing into one question. The appendix tables only report the results for three nearest neighbors matching, which shows the best results in the balancing test in Section 6 below.

Profit was also statistically significantly negatively correlated with use of temporary workers, but was less related to the use than the other variables, as reported in Table 3 (with a correlation of -.03 for both cohorts). Given its weaker relationship, we chose to exclude it from our main analyses, but decided to see if its use in the logistic regression specification used to create the matches made any difference. As shown in Table A1, use of profit instead of TFP as an explanatory variable for the matching process yields the same patterns of higher levels of employment, less use of temporary workers, and higher wages in the treatment group as does our simpler matching model, though the control sample ends up using firms with higher average employment size and wages for the match.

Table A2 shows that, in the 2012 cohort, for those firms that are exporters only (thus a smaller sample in the treatment group), for their employment levels, the ATTs are still positive and statistically significant at one, two, and three years after starting to export using all three matching methods for both the manufacturing and the wholesale and retail trade sectors; for the services sector, with only three treatment cases, only the kernel method yields statistically significant results. Regarding use of temporary workers, in manufacturing the ATTs are negative and statistically significant throughout, but there is no downward trend in the share of temporary workers. For wholesale and retail trade and for services, the neighbors method does not yield statistically significant differences between exporters and non-exporters, while the kernel method does yield negative statistically significant differences. In addition, these sectors exhibit a downward trend in the use of temporary workers. So these results are in general confirmatory of the results found for all firms engaging in international trade in 2012.

One caveat for interpreting our results (for the 2019 cohort) is that we define temporary workers here as those who do not receive social insurance contribution, and under the 2014 Law on Social Insurance, employers need to contribute to social insurance for employees on labor contracts of one month or longer starting from 2016. As such, the effects of starting to trade we observe here are on the growth of the type of workers on contracts of less than one month. We cannot generalize our results to include workers on other short-term contracts of duration of, for example, two or three months. Notably, the overall pattern of results is not that different between our two cohorts and may in part reflect a level of noncompliance with this regulation. It may certainly be that this way of defining

temporary workers will be of less use going forward and that worker categories may need redefinition for future studies that are examine the effects of trade and other phenomena on the precarity of work.

6. Specification Analysis and Balancing Tests

To test the goodness of matching of the PSM method, we perform two tests using the three nearest neighbors matching results. We first perform a specification analysis, which follows Mueser et al. (2007). In this test, we calculate the averages of the main outcome (share of temporary workers) over time between the control group (non-traders) and the treatment group (trade-starters) before and after matching. Panels A and B of Figure 1 show the means across these groups for the 2012 cohort and 2019 cohort respectively. According to these figures, the treatment and control groups become more comparable after matching for both cohorts, as the gap between them narrows post-matching compared to pre-matching for both cohorts while the overall trends stay steady.

In addition to checking how PSM improves comparability between the two groups in terms of the outcome, we performed a balancing test to verify that there is not significant variation in firm characteristics between the control and treatment groups (Rosenbaum and Rubin, 1985). This test checks whether average differences are equal before and after matching for each of the variables used to calculate the propensity score. Results of the goodness of matching tests are reported for the three nearest neighbors method as this method results in the highest percentage reduction in bias among the three matching methods we employ in this paper. The results for the two cohorts are presented in Table 11. As shown in the two panels, PSM has reduced bias for most of the characteristics except for TFP for the 2012 cohort. However, the p-value for the average difference in terms of TFP between the two group after matching for this cohort remains large, which validates the goodness of PSM. In addition, for most variables, the p-values of average differences go from being significant pre-matching to insignificant post-matching. Overall, the results of this balancing test demonstrate that we were able to eliminate the differences in observable characteristics in trade-starters and non-traders.

The results of these two tests confirm that PSM method has helped us minimize bias and endogeneity in estimating the effect of starting to trade by identifying comparable control and treatment groups and taking the difference between them. While we prefer the three nearest neighbors method over the other two matching methods, based on these results, the similarity of the results across the three methods indicates that our results are not heavily dependent on the matching method used.

7. Conclusions and Directions for Further Research

Our results for Vietnam's recent experience support the general pattern that the firms that start to engage in international trade have a lower share of temporary workers in their employment. This is true both in absolute size and relative to nontrading firms that have similar characteristics to the trade-starters.

In some situations where one sector is doing better than another in terms of quality of employment, we might be concerned about what happens to workers who may be displaced from that sector. Primary and secondary labor market systems can lead to greater inequality as well as less good outcomes for workers in the secondary sector. But that phenomenon does not seem to be consistent with our findings since employment is higher for the trade-starters or new traders, including for those in the services sector (which tends to be the most likely area in which one would find less formal employment). New traders are a small part of the overall labor market, so they are not having a big effect on the margin. But the optimistic view is that the increased numbers of trading firms has increased the number of good jobs available; indeed, the firms that have already been trading for a while employ even larger labor forces. Of course, this virtuous tale may not persist forever, as there could be a downturn in trade or reduced gains from trade in later periods beyond year three. On the other hand, if firms and employees learn by doing and benefit from higher earnings by developing additional efficiency, the virtuous circle could continue as workers and exporter firms become more productive through this process over time.

One question is of course whether similar patterns would be found in other countries, particularly in other countries that are continuing to develop their labor regulations and formal labor market. Perhaps other countries do not have an overall regulatory system that supports firms entering into trade to be "virtuous" in the sense of using fewer temporary workers and continuing to pay high wages and social insurance. More case studies are needed to see if the patterns we find are also found in either other similar countries like Thailand, or in very different countries like those in Latin America and Africa. We hope others will follow our lead and look more closely at the use of temporary workers in other country settings.

There are also a number of interesting questions about temporary worker use that we could not explore given the limitations of our particular data set. These include whether there are gender, age, and other demographic differences in who engages in temporary work in both the trading and nontrading firms, and whether these demographic patterns have changed over time. It would also be interesting to continue to follow our firms from the later cohort over a longer time period to see how they continue to evolve, as well as to add an additional cohort that is fully past COVID and has more fully engaged with recent changes in the regulations surrounding use of temporary labor.

As we move into a world where labor may become more precarious due to the increase in developed countries of “gig work” and concerns about the effects of AI on labor utilization and overall employment, it behooves us to consider in multiple contexts the fundamental question of whether expansion of markets leads to better opportunities for workers. Without nimble labor markets and supportive governments, displacement of labor from contracting sectors may not automatically result in higher employment in expanding ones. Vietnam continues to stand out as an impressive example of how to convert from a largely agricultural country to one that has become an increasingly world-market oriented economy, and an economy that provides improved employment opportunities to a large share of its workforce.

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Table 1. The Number of Firms by Trader Type and Industry

A. 2012 Cohort

	Non traders	Trade starters	Always traders	Others	Total
Manufacturing	21,488	231	3,962	7,573	33,254
Wholesale and Retail	47,135	182	504	13,415	61,236
Services	35,867	29	87	2,551	38,534
Transportation	6,063	4	7	258	6,332
Construction	21,643	32	51	1,240	22,966
Total	132,196	478	4,611	25,037	162,322

B. 2019 Cohort

	Non traders	Trade starters	Always traders	Others	Total
Manufacturing	21,789	251	3,123	6,584	31,747
Wholesale and Retail	50,554	36	103	2,976	53,669
Services	28,837	11	42	1,179	30,069
Transportation	6,964	3	2	161	7,130
Construction	21,924	2	9	337	22,272
Total	130,068	303	3,279	11,237	144,887

Notes: The 2012 cohort uses the 2010-2015 data to define firm types, and the 2019 cohort uses the 2017-2022 data to define firm types. In the 2012 cohort, trader-starters engage in exports or imports in 2012-2015 and have no trade in 2010 and 2011. Trade-starters in the 2019 cohort are defined in similar fashion. Because our data are balanced panels, all years have the same statistics---the table shows one year's number of observations for each cohort.

Table 2. The Share of Temporary Workers by Industry and Year

Year	Manufacturing	Wholesale and Retail	Services	Transportation	Construction	Total
2010	24.2%	53.3%	37.7%	42.1%	73.8%	38.3%
2011	22.2%	49.4%	33.6%	39.9%	75.2%	36.1%
2012	20.0%	51.0%	35.7%	37.9%	76.3%	35.3%
2013	19.0%	50.2%	34.9%	38.4%	76.4%	34.0%
2014	17.8%	49.7%	34.5%	39.3%	75.6%	32.5%
2015	17.0%	44.1%	32.7%	35.5%	73.5%	30.6%
2017	19.5%	63.3%	48.2%	71.4%	81.4%	34.5%
2018	17.4%	59.9%	44.9%	58.1%	79.2%	30.6%
2019	16.6%	70.1%	50.5%	61.7%	85.4%	31.7%
2021	19.5%	99.9%	53.0%	73.5%	83.8%	99.2%
2022	13.4%	67.2%	49.2%	67.6%	81.6%	28.6%

Notes: The temporary worker share is the number of temporary employees divided by all employees. The year 2016 is excluded in the analysis because no trade information is available. Statistics for 2020 are unavailable because the VES omitted the number of temporary workers in the questionnaire. Statistics for 2021 are unreliable due to the COVID pandemic disruption.

Table 3. Correlations Between Firm Characteristics

A. 2012 Cohort

	Temporary worker share	Profit	TFP	Capital Intensity	Age	Foreign Ownership
Profit	-0.0322*					
TFP	-0.2764*	0.0230*				
Capital intensity	-0.0541*	0.0240*	0.2427*			
Age	-0.1822*	0.0278*	0.1905*	0.0236*		
Foreign Ownership	-0.2887*	0.0332*	0.2110*	0.0742*	0.0454*	
Employment	-0.1310*	0.1764*	0.1498*	0.0116*	0.1289*	0.1815*

B. 2019 Cohort

	Temporary worker share	Profit	TFP	Capital intensity	Foreign ownership
Profit	-0.0279*				
TFP	-0.0673*	0.0032			
Capital intensity	-0.0307*	-0.0296*	0.0300*		
Foreign Ownership	-0.4707*	0.0371*	-0.0608*	0.0289*	
Employment	-0.1863*	0.4340*	0.0024	0.001	0.2380*

Notes: * indicates that the p-value is less than 0.05. For the 2019 cohort, Age is omitted because age information is limited.

Table 4. Descriptive Statistics of Labor Variables

	A. 2012 Cohort			B. 2019 Cohort		
Temporary worker share	Mean	SD	N	Mean	SD	N
Non-traders	0.704	0.296	132,195	0.910	0.254	130,040
Trade-starters	0.348	0.373	478	0.199	0.291	303
Always-traders	0.184	0.258	4,611	0.101	0.191	3,278
Others	0.506	0.308	25,036	0.529	0.446	11,236
Total	0.658	0.317	162,320	0.860	0.315	144,857
Wage per employee	Mean	SD	N	Mean	SD	N
Non-traders	57.4	896.8	132,041	66.6	43.7	113,607
Trade-starters	78.3	91.4	478	109.4	66.8	297
Always-traders	75.7	68.7	4,611	124.8	60.7	3,219
Others	66.8	448.8	25,025	93.7	58.7	10,440
Total	59.5	828.4	162,155	70.4	47.1	127,563
Employment	Mean	SD	N	Mean	SD	N
Non-traders	28.4	131.2	147,770	16.5	68.5	130,040
Trade-starters	343.8	841.5	266	295.7	760.5	303
Always-traders	715.9	2184.6	3,074	744.2	2282.6	3,278
Others	138.6	478.7	11,213	149.6	612.2	11,236
Total	49.5	364.0	162,323	43.8	406.3	144,857

Notes: Temporary worker share is the proportion of employees who do not receive social security benefits out of all workers by firm. Wage per employee is the total wage bill (in millions Vietnamese dong) divided by the number of total employees by firm. Wage per employee has been trimmed at the top 1 percent in order to remove outliers.

Table 5. Descriptive Statistics of Explanatory Variables

	A. 2012 Cohort			B. 2019 Cohort		
Manufacturing	Mean	Std. dev.	Obs	Mean	Std. dev.	Obs
TFP (t-2)	0.706	0.157	31,094	0.959	1.092	24,445
lnKAPINT (t-2)	4.279	1.369	32,161	5.189	1.596	25,628
FOREIGN (t-2)	0.133	0.339	33,254	0.159	0.366	31,739
lnL (t-2)	3.170	1.564	33,251	2.996	1.645	31,739
lnAGE (t-2)	1.764	0.705	33,111	1.836	0.906	15,184
Wholesale and Retail	Mean	Std. dev.	Obs	Mean	Std. dev.	Obs
TFP (t-2)	0.732	0.176	55,482	0.696	0.196	30,554
lnKAPINT (t-2)	4.362	1.162	58,011	5.034	1.408	32,100
FOREIGN (t-2)	0.009	0.096	61,236	0.004	0.060	53,659
lnL (t-2)	1.965	0.938	61,235	1.667	0.881	53,659
lnAGE (t-2)	1.577	0.654	61,012	1.699	0.849	12,047
Services	Mean	Std. dev.	Obs	Mean	Std. dev.	Obs
TFP (t-2)	0.651	0.244	29,587	1.457	0.784	15,303
lnKAPINT (t-2)	4.179	1.854	35,167	5.107	1.881	19,228
FOREIGN (t-2)	0.043	0.203	38,534	0.014	0.117	30,061
lnL (t-2)	2.208	1.194	38,531	2.051	1.123	30,061
lnAGE (t-2)	1.557	0.669	38,358	1.727	0.859	11,172

Notes: *KAPINT* is capital intensity, total fixed assets divided by the total number of employees. *FOREIGN* indicates whether a firm is foreign-owned or not. *L* is the total number of employees. All variables are lagged by two years and are logged except for *Foreign*. *Age* is available for only a subset of firms in the 2019 cohort in the VES.

Table 6. Logit Estimation Results

	A. 2012 Cohort			B. 2019 Cohort		
Dependent: Starting to trade in 2012 for the 2012 cohort and 2019 for the 2019 cohort						
Coefficients						
	(1)	(2)	(3)	(1)	(2)	(3)
	Manufacturing	Wholesale and retail	Service	Manufacturing	Wholesale and retail	Service
TFP (t-2)	1.124	-0.157	-9.086**	0.520**	0.399*	-0.310
	(0.878)	(0.747)	(3.757)	(0.229)	(0.233)	(0.873)
ln KAPINT (t-2)	0.331***	0.226***	-0.117	0.603***	0.618***	0.895***
	(0.059)	(0.069)	(0.145)	(0.071)	(0.132)	(0.321)
FOREIGN (t-2)	1.297***	0.830*	1.903***	3.216***	7.358***	3.014***
	(0.259)	(0.503)	(0.525)	(0.220)	(1.477)	(1.162)
ln L (t-2)	1.018***	0.886***	0.558***	0.935***	0.660***	0.892***
	(0.065)	(0.078)	(0.176)	(0.070)	(0.165)	(0.334)
ln AGE (t-2)	0.013	0.153	0.294			
	(0.112)	(0.128)	(0.359)			
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R2	0.300	0.169	0.335	0.466	0.238	0.434
Observations	15,204	33,502	13,834	8,862	12,329	1,165
Corresponding Marginal Effects						
	(1)	(2)	(3)	(1)	(2)	(3)
TFP (t-2)	0.014	-0.001	-0.016	0.008	0.001	-0.002
ln KAPINT (t-2)	0.042	0.011	-0.002	0.049	0.017	0.018
FOREIGN (t-2)	0.016	0.004	0.003	0.093	0.014	0.053
ln L (t-2)	0.128	0.044	0.010	0.143	0.015	0.052
ln AGE (t-2)	0.002	0.008	0.005			

Notes: Data used in this analysis include trade-starters and nontrading firms only. Starting to trade is defined as beginning to export or import in 2012 for the 2012 cohort and in 2019 for the 2019 cohort. Age is dropped for the 2019 cohort due to its small number of observations. The marginal effects of logged variables are in response to a 10% change in each variable.

Table 7. Causal Effect of Starting to Trade on Employment

A. 2012 Cohort						
Manufacturing		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	4.620	3.778	0.842	0.130	6.49
	t+2	4.639	3.704	0.935	0.136	6.90
	t+3	4.613	3.673	0.940	0.140	6.74
Three nearest neighbors	t+1	4.620	3.823	0.797	0.104	7.67
	t+2	4.639	3.722	0.917	0.107	8.55
	t+3	4.613	3.631	0.982	0.112	8.76
Kernel matching	t+1	4.620	3.079	1.541	0.085	18.17
	t+2	4.639	3.001	1.638	0.087	18.88
	t+3	4.613	2.940	1.673	0.092	18.25
Wholesale and retail		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	3.384	2.580	0.804	0.140	5.73
	t+2	3.340	2.361	0.979	0.144	6.81
	t+3	3.280	2.336	0.944	0.148	6.37
Three nearest neighbors	t+1	3.384	2.568	0.816	0.115	7.08
	t+2	3.340	2.461	0.879	0.118	7.42
	t+3	3.280	2.416	0.864	0.124	6.97
Kernel matching	t+1	3.384	1.831	1.553	0.102	15.30
	t+2	3.340	1.779	1.561	0.104	15.00
	t+3	3.280	1.739	1.541	0.109	14.08
Services		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	4.488	3.415	1.074	0.539	1.99
	t+2	4.446	3.415	1.031	0.548	1.88
	t+3	4.355	3.341	1.014	0.559	1.82
Three nearest neighbors	t+1	4.488	3.362	1.126	0.426	2.65
	t+2	4.446	3.424	1.022	0.449	2.27
	t+3	4.355	3.270	1.085	0.447	2.43
Kernel matching	t+1	4.488	2.441	2.047	0.370	5.53
	t+2	4.446	2.382	2.064	0.395	5.23
	t+3	4.355	2.338	2.017	0.392	5.14

Table 7 (contd.). Causal Effect of Starting to Trade on Employment

B. 2019 Cohort						
Manufacturing		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	4.856	3.450	1.406	0.188	7.48
	t+2	4.806	3.359	1.447	0.190	7.61
	t+3	4.778	3.215	1.564	0.199	7.85
Three nearest neighbors	t+1	4.856	3.779	1.077	0.145	7.40
	t+2	4.806	3.661	1.145	0.146	7.83
	t+3	4.778	3.459	1.319	0.150	8.79
Kernel matching	t+1	4.856	3.354	1.419	0.107	13.28
	t+2	4.806	3.230	1.491	0.107	13.94
	t+3	4.778	3.069	1.626	0.107	15.17
Wholesale and retail		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	3.137	2.279	0.858	0.321	2.68
	t+2	3.110	2.153	0.957	0.327	2.92
	t+3	3.156	2.085	1.071	0.327	3.27
Three nearest neighbors	t+1	3.137	2.230	0.907	0.260	3.49
	t+2	3.110	2.061	1.048	0.269	3.89
	t+3	3.156	1.984	1.172	0.270	4.34
Kernel matching	t+1	3.137	1.695	1.442	0.229	6.28
	t+2	3.110	1.667	1.443	0.239	6.02
	t+3	3.156	1.626	1.530	0.234	6.54
Services		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	4.026	3.839	0.186	0.619	0.30
	t+2	4.123	3.680	0.443	0.660	0.67
	t+3	4.145	3.666	0.479	0.666	0.72
Three nearest neighbors	t+1	4.026	3.137	0.889	0.389	2.28
	t+2	4.123	3.196	0.926	0.376	2.46
	t+3	4.145	3.081	1.065	0.408	2.61
Kernel matching	t+1	4.026	2.129	1.897	0.202	9.38
	t+2	4.123	2.033	2.090	0.184	11.36
	t+3	4.145	2.023	2.122	0.207	10.26

Notes: The post-event periods t+1, t+2, and t+3 correspond to 2013, 2014, and 2015 for the 2012 cohort and 2020, 2021, and 2022 for the 2019 cohort, respectively. For manufacturing, wholesale and retail, and services, the numbers of treated firms are 224, 172, and 27 in the 2012 cohort and 222, 31, and 9 in the 2019 cohort, respectively.

Table 8. Causal Effect of Starting to Trade on Temporary Worker Share

A. 2012 Cohort						
Manufacturing		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	0.307	0.495	-0.188	0.034	-5.62
	t+2	0.311	0.491	-0.179	0.034	-5.22
	t+3	0.293	0.485	-0.192	0.032	-5.92
Three nearest neighbors	t+1	0.307	0.485	-0.178	0.026	-6.81
	t+2	0.311	0.501	-0.190	0.027	-7.02
	t+3	0.293	0.516	-0.223	0.026	-8.66
Kernel matching	t+1	0.307	0.569	-0.262	0.021	-12.47
	t+2	0.311	0.593	-0.282	0.022	-12.87
	t+3	0.293	0.600	-0.307	0.021	-14.75
Wholesale and retail		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	0.423	0.618	-0.195	0.032	-6.08
	t+2	0.437	0.645	-0.208	0.032	-6.51
	t+3	0.355	0.624	-0.270	0.033	-8.28
Three nearest neighbors	t+1	0.423	0.629	-0.206	0.025	-8.11
	t+2	0.437	0.653	-0.215	0.026	-8.37
	t+3	0.355	0.634	-0.279	0.026	-10.58
Kernel matching	t+1	0.423	0.708	-0.285	0.021	-13.37
	t+2	0.437	0.737	-0.300	0.022	-13.84
	t+3	0.355	0.713	-0.359	0.022	-16.12
Services		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	0.310	0.480	-0.170	0.107	-1.59
	t+2	0.318	0.441	-0.123	0.100	-1.23
	t+3	0.305	0.450	-0.145	0.093	-1.56
Three nearest neighbors	t+1	0.310	0.439	-0.129	0.083	-1.55
	t+2	0.318	0.476	-0.158	0.079	-2.01
	t+3	0.305	0.420	-0.114	0.075	-1.52
Kernel matching	t+1	0.310	0.604	-0.294	0.072	-4.08
	t+2	0.318	0.607	-0.289	0.067	-4.29
	t+3	0.305	0.583	-0.278	0.064	-4.33

Table 8 (contd.). Causal Effect of Starting to Trade on Temporary Worker Share

B. 2019 Cohort						
Manufacturing		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	0.149	0.149	0.000	0.012	-0.04
	t+2	0.163	0.491	-0.328	0.049	-6.72
	t+3	0.118	0.477	-0.360	0.049	-7.33
Three nearest neighbors	t+1	0.149	0.148	0.001	0.010	0.11
	t+2	0.163	0.480	-0.317	0.035	-9.00
	t+3	0.118	0.463	-0.345	0.034	-10.09
Kernel matching	t+1	0.147	0.137	0.010	0.012	0.79
	t+2	0.163	0.548	-0.385	0.023	-16.63
	t+3	0.121	0.527	-0.407	0.022	-18.68
Wholesale and retail		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	0.166	0.109	0.057	0.046	1.23
	t+2	0.142	0.788	-0.645	0.085	-7.58
	t+3	0.167	0.823	-0.656	0.081	-8.14
Three nearest neighbors	t+1	0.166	0.084	0.082	0.031	2.66
	t+2	0.142	0.800	-0.657	0.066	-9.92
	t+3	0.167	0.830	-0.664	0.063	-10.46
Kernel matching	t+1	0.166	0.096	0.070	0.028	2.51
	t+2	0.142	0.864	-0.722	0.055	-13.19
	t+3	0.167	0.834	-0.667	0.054	-12.46
Services		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	0.173	0.184	-0.011	0.029	-0.40
	t+2	0.027	0.348	-0.321	0.165	-1.95
	t+3	0.121	0.479	-0.358	0.195	-1.84
Three nearest neighbors	t+1	0.173	0.166	0.007	0.027	0.26
	t+2	0.027	0.510	-0.483	0.092	-5.25
	t+3	0.121	0.556	-0.435	0.133	-3.28
Kernel matching	t+1	0.173	0.105	0.067	0.013	5.10
	t+2	0.027	0.813	-0.786	0.018	-44.28
	t+3	0.121	0.821	-0.700	0.099	-7.05

Notes: Since there are no data in 2020 for the number of workers who received social insurance contributions, we impute the 2020 data by multiplying the total number of workers by the ratio of social insurance payment expense to total labor-related expense for each firm. The post-event periods t+1, t+2, and t+3 correspond to 2013, 2014, and 2015 for the 2012 cohort and 2020, 2021, and 2022 for the 2019 cohort, respectively. For manufacturing, wholesale and retail, and services, the numbers of treated firms are 224, 172, and 27 in the 2012 cohort and 222, 31, and 9 in the 2019 cohort, respectively.

Table 9. Causal Effect of Starting to Trade on Wages

A. 2012 Cohort						
Manufacturing		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	4.030	3.674	0.356	0.058	6.14
	t+2	4.058	3.608	0.450	0.079	5.69
	t+3	4.235	3.907	0.328	0.058	5.65
Three nearest neighbors	t+1	4.030	3.693	0.337	0.044	7.65
	t+2	4.058	3.632	0.426	0.060	7.10
	t+3	4.235	3.888	0.347	0.046	7.52
Kernel matching	t+1	4.030	3.649	0.381	0.035	11.03
	t+2	4.058	3.449	0.609	0.046	13.23
	t+3	4.235	3.870	0.365	0.035	10.58
Wholesale and retail		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	4.261	3.812	0.449	0.057	7.91
	t+2	4.031	3.493	0.538	0.125	4.32
	t+3	4.526	3.993	0.533	0.063	8.47
Three nearest neighbors	t+1	4.261	3.809	0.452	0.046	9.75
	t+2	4.031	3.418	0.613	0.103	5.94
	t+3	4.526	4.050	0.477	0.051	9.42
Kernel matching	t+1	4.261	3.767	0.493	0.040	12.19
	t+2	4.031	3.329	0.702	0.089	7.89
	t+3	4.526	3.968	0.558	0.045	12.48
Services		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	4.547	4.320	0.227	0.202	1.12
	t+2	4.555	3.848	0.707	0.279	2.53
	t+3	4.668	4.383	0.285	0.183	1.56
Three nearest neighbors	t+1	4.547	4.231	0.315	0.170	1.85
	t+2	4.555	3.800	0.755	0.240	3.15
	t+3	4.668	4.411	0.257	0.163	1.58
Kernel matching	t+1	4.547	3.941	0.606	0.152	3.99
	t+2	4.555	3.522	1.033	0.198	5.23
	t+3	4.668	4.150	0.518	0.137	3.78

Table 9 (contd.). Causal Effect of Starting to Trade on Wages

A. 2019 Cohort						
Manufacturing		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	4.610	4.364	0.247	0.070	3.50
	t+2	4.732	4.456	0.276	0.076	3.61
	t+3	4.831	4.558	0.273	0.079	3.44
Three nearest neighbors	t+1	4.610	4.437	0.174	0.057	3.05
	t+2	4.732	4.381	0.351	0.075	4.70
	t+3	4.831	4.547	0.284	0.060	4.73
Kernel matching	t+1	4.612	4.271	0.341	0.055	6.18
	t+2	4.730	4.320	0.409	0.068	6.03
	t+3	4.820	4.485	0.335	0.054	6.25
Wholesale and retail		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	4.655	4.221	0.435	0.161	2.71
	t+2	4.719	4.279	0.440	0.243	1.81
	t+3	4.762	4.173	0.590	0.163	3.61
Three nearest neighbors	t+1	4.655	4.100	0.556	0.152	3.65
	t+2	4.719	4.207	0.512	0.156	3.27
	t+3	4.762	4.298	0.464	0.124	3.73
Kernel matching	t+1	4.655	4.071	0.584	0.140	4.17
	t+2	4.719	4.010	0.709	0.131	5.42
	t+3	4.762	4.321	0.441	0.107	4.12
Services		(1)	(2)	(3)	(4)	(5)
Matching Method	Year	Treated	Control	ATT	S.E.	t-Value
One nearest neighbor	t+1	4.947	4.343	0.604	0.326	1.85
	t+2	5.122	4.667	0.455	0.383	1.19
	t+3	5.143	4.758	0.384	0.304	1.26
Three nearest neighbors	t+1	4.947	4.825	0.122	0.253	0.48
	t+2	5.122	4.489	0.633	0.303	2.09
	t+3	5.143	4.586	0.557	0.273	2.04
Kernel matching	t+1	4.947	4.071	0.876	0.206	4.25
	t+2	5.122	4.066	1.056	0.262	4.03
	t+3	5.143	4.388	0.755	0.254	2.97

Notes: The post-event periods t+1, t+2, and t+3 correspond to 2013, 2014, and 2015 for the 2012 cohort and 2020, 2021, and 2022 for the 2019 cohort, respectively. For manufacturing, wholesale and retail, and services, the numbers of treated firms are 224, 172, and 27 in the 2012 cohort and 222, 31, and 9 in the 2019 cohort, respectively

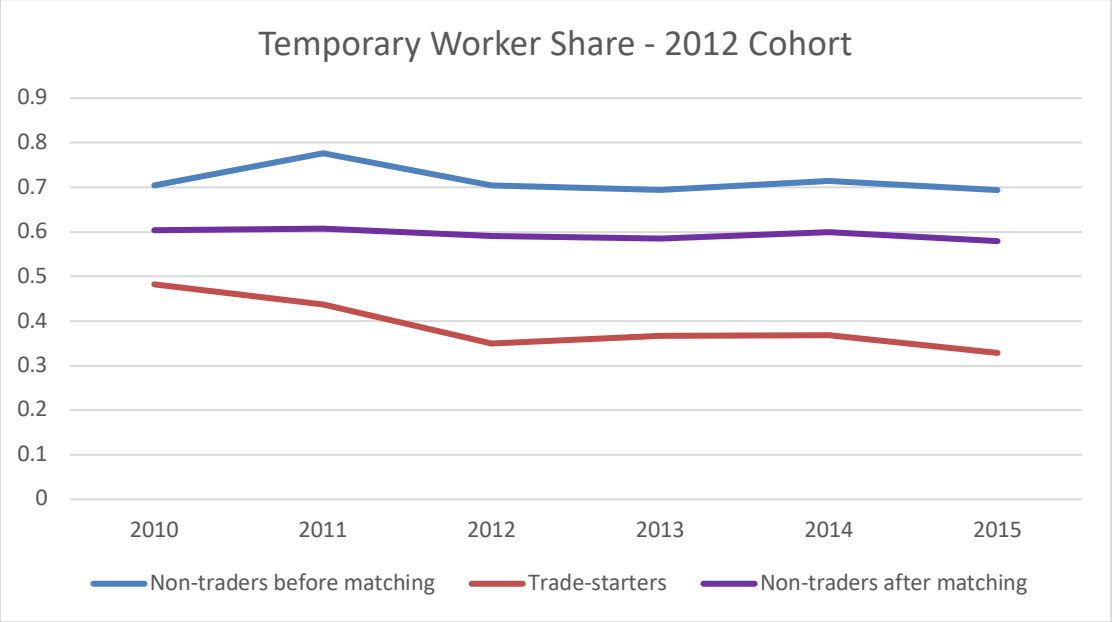
Table 10. The mean difference between t+1 and t+3 values for trade starters

	A. 2012 Cohort		B. 2019 Cohort	
Employment	Mean difference	P-value	Mean difference	P-value
Manufacturing	-0.007	0.879	-0.077	0.079
Wholesale and retail	-0.104	0.009	0.019	0.772
Servies	-0.133	0.215	0.119	0.595
Temporary Worker Share	Mean difference	P-value	Mean difference	P-value
Manufacturing	-0.014	0.377	-0.029	0.107
Wholesale and retail	-0.068	0.002	0.001	0.989
Services	-0.005	0.934	-0.037	0.739
Wage per worker	Mean difference	P-value	Mean difference	P-value
Manufacturing	0.205	0.000	0.220	0.000
Wholesale and retail	0.266	0.000	0.106	0.164
Services	0.122	0.216	0.195	0.614

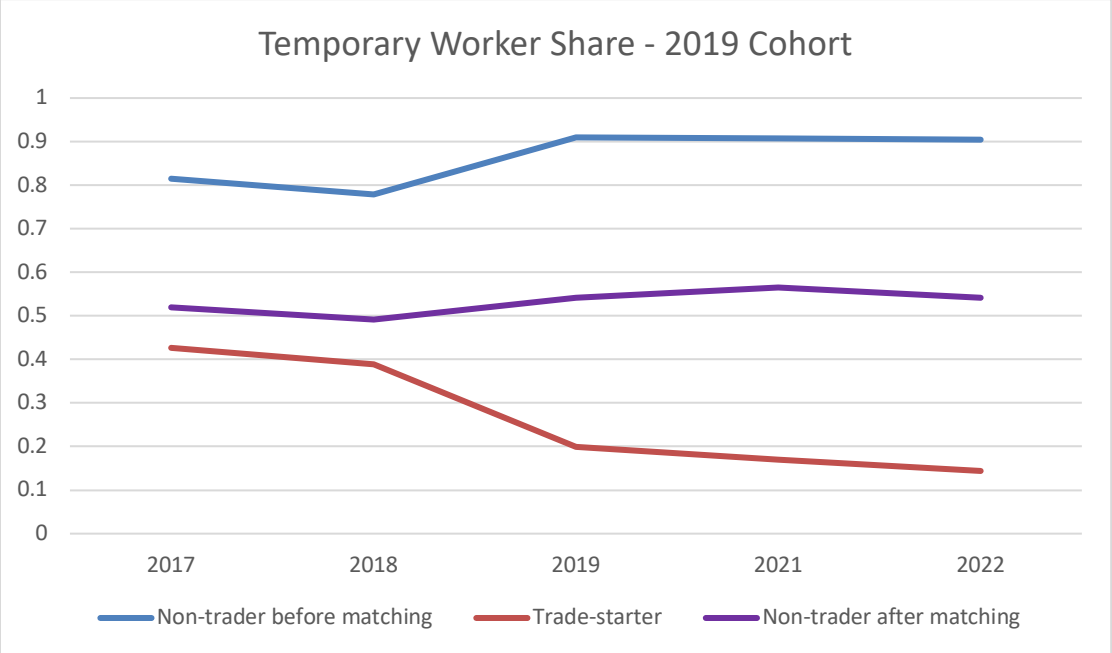
Notes: The p-values are calculated based on the null hypothesis that the mean difference is zero against the alternative hypothesis that the mean difference is not zero. Tables 7-9 present the mean values used in this table.

Figure 1. Share of Temporary Workers among Trader-Starters and Non-Traders before and after Matching

Panel A. 2012 Cohort



Panel B. 2019 Cohort



Note: The matching method used is the three nearest neighbors method. We use the temporary worker share among trade-starters before matching only because there is a very small difference between the pre-matching and post-matching means for this group.

Table 11. Goodness of Matching Test Results

Variables		Averages: Trade-starters	Averages: Non-traders	Averages: % Bias	Averages: % Reduction in Bias	T-tests: T	T-tests: p>t
TFP	Before Matching	0.291	0.293	-2.8		-1.02	0.309
	After Matching	0.291	0.296	-7	-150.2	-1.28	0.202
lnKAPINT	Before Matching	4.269	4.152	7.1		2.77	0.006
	After Matching	4.269	4.217	3.2	55.3	0.79	0.429
lnL	Before Matching	3.873	2.389	111.7		48.08	0.000
	After Matching	3.873	3.943	-5.2	95.3	-1.22	0.223
FOREIGN	Before Matching	0.141	0.012	50		42.59	0.000
	After Matching	0.141	0.110	12.1	75.9	2.45	0.014

B. 2019 Cohort Variables		Trade-starters	Non-traders	% Bias	% Reduction in Bias	T	p>t
TFP	Before Matching	0.785	0.946	-20.9		-3.37	0.001
	After Matching	0.785	0.792	-0.9	95.7	-0.11	0.912
l*nKAPINT*	Before Matching	5.843	5.041	48.1		8.27	0.000
	After Matching	5.843	5.953	-6.6	86.3	-0.74	0.461
l*nL*	Before Matching	4.054	2.410	115.3		48.08	0.000
	After Matching	4.054	4.244	-13.3	88.4	-1.22	0.187
FOREIGN	Before Matching	0.448	0.005	124.4		42.59	0.000
	After Matching	0.448	0.403	12.7	89.8	2.45	0.301

Note: The goodness of matching tests reported here were performed using the three nearest neighbors method.

Table A1. Causal Effect of Starting to Trade on Employment, Temporary Worker Share, and Wage, Controlling for Profit (2012 Cohort)

Employment		(1)	(2)	(3)	(4)	(5)
Industry	Year	Treated	Control	ATT	S.E.	t-Value
Manufacturing	t+1	4.764	3.978	0.786	0.124	6.34
	t+2	4.726	3.873	0.853	0.130	6.56
	t+3	4.714	3.812	0.902	0.135	6.69
Wholesale and Retail	t+1	3.393	2.817	0.576	0.132	4.35
	t+2	3.359	2.794	0.564	0.134	4.20
	t+3	3.319	2.678	0.641	0.142	4.51
Services	t+1	4.880	4.047	0.833	0.518	1.61
	t+2	4.724	3.977	0.747	0.568	1.32
	t+3	4.722	4.257	0.464	0.573	0.81
Temporary Worker Share		(1)	(2)	(3)	(4)	(5)
Industry	Year	Treated	Control	ATT	S.E.	t-Value
Manufacturing	t+1	0.314	0.492	-0.178	0.033	-5.32
	t+2	0.310	0.513	-0.203	0.034	-6.04
	t+3	0.287	0.490	-0.203	0.033	-6.21
Wholesale and Retail	t+1	0.404	0.579	-0.176	0.029	-6.08
	t+2	0.435	0.612	-0.177	0.029	-6.08
	t+3	0.344	0.570	-0.226	0.030	-7.43
Services	t+1	0.271	0.260	0.011	0.091	0.12
	t+2	0.276	0.284	-0.008	0.091	-0.09
	t+3	0.321	0.308	0.013	0.086	0.16
Wage		(1)	(2)	(3)	(4)	(5)
Industry	Year	Treated	Control	ATT	S.E.	t-Value
Manufacturing	t+1	4.009	3.710	0.300	0.056	5.34
	t+2	4.132	3.692	0.440	0.064	6.85
	t+3	4.246	3.891	0.354	0.058	6.14
Wholesale and Retail	t+1	4.294	3.856	0.439	0.051	8.58
	t+2	4.095	3.579	0.516	0.109	4.72
	t+3	4.558	4.054	0.504	0.058	8.70
Services	t+1	4.661	4.378	0.283	0.254	1.11
	t+2	4.893	4.134	0.759	0.280	2.71
	t+3	4.659	4.630	0.029	0.192	0.15

Notes: The post-event periods t+1, t+2, and t+3 correspond to 2013, 2014, and 2015. The three nearest neighbors method is used for matching. For manufacturing, wholesale and retail, and services, the numbers of treated firms are 145, 125, and 18, respectively.

Table A2. Causal Effect of Starting to Export on Employment, Temporary Worker Share, and Wage (2012 Cohort)

Employment		(1)	(2)	(3)	(4)	(5)
Industry	Year	Treated	Control	ATT	S.E.	t-Value
Manufacturing	t+1	4.827	4.080	0.747	0.122	6.14
	t+2	4.851	3.981	0.870	0.124	7.02
	t+3	4.827	3.969	0.857	0.128	6.69
Wholesale and Retail	t+1	3.852	2.942	0.910	0.311	2.93
	t+2	3.846	2.829	1.017	0.320	3.17
	t+3	3.803	2.811	0.992	0.332	2.99
Services	t+1	4.332	2.994	1.338	0.680	1.97
	t+2	4.249	3.486	0.763	0.672	1.14
	t+3	4.310	3.210	1.100	0.653	1.68
Temporary Worker Share		(1)	(2)	(3)	(4)	(5)
Industry	Year	Treated	Control	ATT	S.E.	t-Value
Manufacturing	t+1	0.262	0.414	-0.152	0.027	-5.73
	t+2	0.257	0.433	-0.176	0.027	-6.60
	t+3	0.278	0.447	-0.169	0.027	-6.33
Wholesale and Retail	t+1	0.403	0.590	-0.187	0.068	-2.75
	t+2	0.440	0.588	-0.148	0.067	-2.20
	t+3	0.385	0.534	-0.149	0.068	-2.20
Services	t+1	0.311	0.473	-0.162	0.167	-0.97
	t+2	0.278	0.473	-0.194	0.135	-1.44
	t+3	0.305	0.416	-0.111	0.141	-0.79
Wage		(1)	(2)	(3)	(4)	(5)
Industry	Year	Treated	Control	ATT	S.E.	t-Value
Manufacturing	t+1	4.105	3.832	0.273	0.051	5.35
	t+2	4.140	3.815	0.325	0.063	5.18
	t+3	4.260	4.073	0.188	0.042	4.42
Wholesale and Retail	t+1	4.366	3.936	0.430	0.103	4.20
	t+2	4.293	3.728	0.565	0.209	2.70
	t+3	4.415	4.092	0.323	0.115	2.81
Services	t+1	4.780	4.167	0.613	0.380	1.62
	t+2	4.793	3.974	0.819	0.334	2.45
	t+3	4.598	4.261	0.337	0.263	1.28

Notes: The post-event periods t+1, t+2, and t+3 correspond to 2013, 2014, and 2015. The three nearest neighbors method is used for matching. For manufacturing, wholesale and retail, and services, the numbers of treated firms are 202, 35, and 8, respectively.