



# Did China tire safeguard save U.S. workers?

Sunghoon Chung <sup>a,\*</sup>, Joonhyung Lee <sup>b</sup>, Thomas Osang <sup>c</sup>

<sup>a</sup> Korea Development Institute (KDI), 263 Namsejong-ro, Sejong 30149, South Korea

<sup>b</sup> Department of Economics, Fogelman College of Business and Economics, The University of Memphis, United States

<sup>c</sup> Department of Economics, Southern Methodist University (SMU), United States



## ARTICLE INFO

### Article history:

Received 10 March 2015

Accepted 18 December 2015

Available online 13 January 2016

### JEL classification:

F13

F14

F16

### Keywords:

China tire safeguard

Temporary trade barriers

Trade diversion

Interactive fixed effects

## ABSTRACT

Although temporary trade barriers are perceived as a feasible policy instrument for securing domestic jobs in the presence of increased globalization and economic downturns, no study has assessed whether such temporary barriers have actually saved domestic jobs. To overcome this deficiency, we evaluate the China-specific safeguard case on consumer tires petitioned by the United States. Contrary to claims made by the Obama administration, we find that total employment and average wages in the tire industry were unaffected by the safeguard. Further analysis reveals that this result is not surprising as we find that imports from China are completely diverted to other exporting countries partly due to the strong presence of multinational corporations in the world tire market.

© 2016 Elsevier B.V. All rights reserved.

*“Over a thousand Americans are working today because we stopped a surge in Chinese tires, but we need to do more.”*— President Barack Obama, State of the Union Address, Jan 24th, 2012.

*“The tariffs didn’t have any material impact on our North American business.”*— Keith Price, a spokesman for Goodyear Tire & Rubber Co., Wall Street Journal, Jan 20th, 2012.

## 1. Introduction

While trade barriers have reached historically low levels, a growing number of countries are worried about job losses as a consequence of the trade liberalization. The concern is well epitomized in the recent U.S. trade policy agenda. The Obama administration has filed trade dispute cases with the World Trade Organization (WTO) at a pace twice as fast as that of the previous administration. Moreover, the Interagency Trade Enforcement Center (ITEC) was set up in February 2012 to monitor and investigate unfair trade practices.<sup>1</sup> During the 2012 presidential election, both candidates pledged to take even stronger actions to protect U.S. businesses and workers.<sup>2</sup>

The incentives to secure jobs by raising trade barriers are well explained in the literature. Political economy of trade policy theory explains that higher risk of unemployment makes individuals more protectionist, which induces them to

\* Corresponding author. Tel.: +82 44 550 4278; fax: +82 44 550 4226.

E-mail addresses: [sunghoon.chung@kdi.re.kr](mailto:sunghoon.chung@kdi.re.kr) (S. Chung), [jlee17@memphis.edu](mailto:jlee17@memphis.edu) (J. Lee), [tosang@smu.edu](mailto:tosang@smu.edu) (T. Osang).

<sup>1</sup> See Rapoza (2012) *Forbes*.

<sup>2</sup> Ever-increasing imports from China were discussed as one of the greatest future threats to the national security of the U.S. in the debates for the 2012 presidential election.

demand more protection through voting or union lobbying activity. The politicians who seek re-election then protect industries with high unemployment rates (Wallerstein, 1987; Bradford, 2006; Matschke and Sherlund, 2006). In addition to political economy considerations, there are other economic models that justify protectionism. Costinot (2009) provides a model where the aggregate welfare can improve when highly unemployed industries are protected. Davidson et al. (2012) emphasize fairness or altruistic concern toward displaced workers as another incentive for protection. Bagwell and Staiger (2003) argue that trade policies are preferred to domestic redistributive policies because they *beggar thy neighbor*: While domestic policies come at the expense of domestic residents, trade policies cost foreigners.

Surprisingly, however, the literature so far has ignored to check whether such protective trade policies can actually save domestic jobs. In fact, studies have only focused on the other direction, i.e., how trade liberalization affects employment or wages. Gaston and Trefler (1994) and Trefler (2004), for example, find that import competition due to tariff declines have negative effects on wages in the U.S. and employment in Canada. In recent studies, Autor et al. (2013, 2014) estimate how much the import surge from China costs U.S. manufacturing employees, and find that the greater import competition causes higher unemployment, lower wages, less labor market participation, and greater chance of switching jobs and receiving government transfers. McLaren and Hakobyan (2012) also find a significant adverse effect of import exposure to Mexico on U.S. wage growth for blue-color workers after the implementation of the North American Free Trade Agreement (NAFTA).<sup>3</sup>

The evidences above seem to imply that re-imposing trade barriers would secure domestic jobs. However, most recent protection policies are enacted in the form of antidumping, countervailing duties, or safeguards, which are systematically different in their nature from the trade barriers such as Most-Favored-Nation (MFN) tariff rates and import quotas that have been lowered in recent decades. These policies, often collectively called temporary trade barriers (TTBs), are typically (i) contingent, (ii) temporary, and (iii) discriminatory in that duties are imposed for a limited time to a small set of products from particular countries.<sup>4</sup> Due to these characteristics, there are at least two channels that may divert trade flows and weaken the impact of a TTB on domestic markets. First, the temporary feature of TTBs leaves a room for targeted exporting firms to adjust their sales timing to either before or after the tariff intervention. Second, perhaps more importantly, the discriminatory feature can divert the import of subject products from the targeted country to other exporting countries. Thus, whether (and the degree to which) a TTB can secure domestic jobs remains an unanswered empirical question.

Despite the lack of empirical evidence on whether TTBs actually save domestic jobs, many WTO member countries have already been opting for TTBs, especially in domestic recession phases with high unemployment rates. Knetter and Prusa (2003) link antidumping filings with domestic real GDP growth to find their counter-cyclical relationship during 1980–1998 in the U.S., Canada, Australia, and the European Union. Irwin (2005) extends a similar analysis to the period covering 1947–2002 in the U.S. case, and finds that the unemployment rate is an important determinant of antidumping investigations. More recently, two companion studies by Bown and Crowley (2013, 2014) investigate thirteen emerging and five industrialized economies, respectively, and report evidence that a high unemployment rate is associated with more TTB incidents.

This paper aims to fill up the deficiency in the literature by evaluating a special safeguard case on Chinese tires (China Tire Safeguard or CTS, henceforth) that has received a great deal of public attention among recent TTB cases.<sup>5</sup> Under Section 421 China-specific safeguard, the U.S. imposed higher tariffs on certain Chinese passenger vehicle and light-truck tires for three years from the fourth quarter of 2009 to the third quarter of 2012. The safeguard duties were 35% ad valorem in the first year, 30% in the second, and 25% in the third on top of the MFN duty rates.<sup>6</sup> The case has triggered not only Chinese retaliation on U.S. poultry and automotive parts, but also a serious controversy on the actual effectiveness of the CTS for the U.S. tire industry.<sup>7</sup> Despite such controversy, the CTS has been cited as a paragon of successful trade policy for job security by the Obama administration.

The CTS provides a unique advantageous setting for answering the question of this paper. While the CTS is representative in that it bears all three TTB characteristics described above, one important distinction of the CTS is that the safeguard duties are exogenously determined. In antidumping cases, which are the most pervasive form of TTB, duties are endogenously determined to offset the dumping margin. Even after the duties are in place, they are recalculated over time to adjust the dumping behavior changes of exporting firms.<sup>8</sup> These endogenous tariff changes complicate the evaluation of a tariff imposition effect. Secondly, the change in the total import of subject Chinese tires before and after the safeguard initiation is considerably large in both levels and growth rates.<sup>9</sup> If TTBs have labor market outcomes, this dramatic change should allow us to observe it. Third, contrary to most trade disputes in which the producers filed a claim, the petition for the CTS was filed by the union representing employees known as United Steelworkers. This implies that the petition is indeed intended for employees' benefits and thus labor market effects.<sup>10</sup>

<sup>3</sup> Similar patterns are observed in developing countries, too. See Goldberg and Pavcnik (2005) for Columbia, Menezes-Filho and Muendler (2011) and Kovak (2013) for Brazil, Topalova (2010) for India.

<sup>4</sup> An exception to discriminatory feature is Global safeguard measure, since it is imposed to all countries.

<sup>5</sup> Prusa (2011, p. 55) describes the China Tire Safeguard as "one of the most widely publicized temporary trade barriers during 2005–2009, garnering significant press attention both in the USA and in China."

<sup>6</sup> MFN duty rates are 4% for radial (or radial-ply) tires and 3.4% for other type (bias-ply) of tires.

<sup>7</sup> See also Bussey (2012) in *Wall Street Journal*.

<sup>8</sup> This recalculation process is also called administrative review process. Many studies investigate the implication of the review process on exporting firm's pricing behavior. See, for example, Blonigen and Haynes (2002) and Blonigen and Park (2004).

<sup>9</sup> Detail statistics are provided in Section 3.

<sup>10</sup> Prusa (2011) argues that the last two features are the main reasons of receiving unusual public attention.

Estimating the impact of the China Tire Safeguard brings some challenges that need to be addressed. Above all, the estimates may be confounded by macroeconomic trends. Since the U.S. economy has been in recovery after the great recession of 2008–2009, one may capture a spuriously inflated labor market effects that would have occurred even without tariff changes. A typical identification strategy in this case is the difference-in-differences method, but there is no clear criterion for choosing appropriate control industries in our case. Another important challenge is that unobserved (time-invariant) industry-specific heterogeneity can be interacted with macro trends to affect the industry selection mechanism for exercising the CTS, causing a selection bias. We are particularly worried that nation-wide political concerns may affect each industry's labor market outcome in different magnitudes depending on industry characteristics. To deal with these issues, we employ some of the recently developed empirical models including, notably, the Synthetic Control Method (Abadie et al., 2010) and panel models with interactive fixed effects (Bai, 2009; Moon and Weidner, 2014, 2015).

All our estimates consistently provide a striking result. Contrary to the Obama administration's claim that the safeguard measures had a positive effect on the labor market (see quote above), we find that total employment and wages in the tire industry show no different time trends from those in the synthetic industries.<sup>11</sup> The result is supported by another finding that the substantial drop in Chinese tire imports is completely offset by the increase in imports from other countries. This complete import diversion leaves little room for domestic producers to make an adjustment in their production, which in turn induces no change in the labor market. Thus, our study highlights that the discriminatory feature of TTB plays a crucial role for the negligible labor market effect.

To our best knowledge, there is no study that investigates the effect of a TTB on domestic labor market outcomes. Some papers have looked at the exporting firms' strategic responses to a TTB through price adjustments (Blonigen and Haynes, 2002; Blonigen and Park, 2004), quantity controls (Staiger and Wolak, 1992), or tariff-jumping investment (Blonigen, 2002; Belderbos et al., 2004). These firm behaviors alter the aggregate trade patterns, and these changes in trade patterns have been analyzed in the literature (Prusa and Feenstra, 1997; Bown and Crowley, 2007). Other studies have turned their attention to TTB effects on domestic firms, with particular interests in output (Staiger and Wolak, 1994), markup (Konings and Vandenbussche, 2005), profit (Kitano and Ohashi, 2009), and productivity (Konings and Vandenbussche, 2008; Pierce, 2011).<sup>12</sup> Although these studies may have some indirect implications for labor market outcomes, they are insufficient to draw definite conclusions on employment and wage effects.

We begin our study with an overview of the China safeguard and the U.S. tire industry in Section 2. Section 3 describes data and time trends of Chinese tire imports and employment. Section 4 provides the empirical model and discusses the results. Section 5 reports and discusses the results, and Section 6 explores a potential mechanism that has driven our results. Section 7 concludes with policy implications and the direction of future researches.

## 2. Overview of China safeguard and the U.S. tire industry

The U.S. Trade Act of 1974 describes conditions under which tariffs can be applied and which groups can file a petition. Once the petition is filed, the International Trade Commission (USITC) makes a recommendation to the president. The president then makes a decision whether to approve or veto the tariff. Two sections (Section 201 and 421) of the Trade Act of 1974 deal with the use of safeguard tariffs. Under Section 201 (Global Safeguard), USITC determines whether rising imports have been a substantial cause of "serious" injury, or threat thereof, to a U.S. industry. On the other hand, Section 421 (China-specific Safeguard or China Safeguard) applies only to China. China Safeguard was added by the U.S. as a condition to China's joining the WTO in 2001 and expired in 2013. Under Section 421, the USITC determines whether rising imports from China cause or threaten to cause a significant "material" injury to the domestic industry. Total seven China Safeguard cases had been filed, of which two were denied by the USITC and five were approved. Of these five approved cases, the president ruled in favor of only one, which is the tire case.

There are a number of noteworthy differences regarding Global Safeguard vs. China Safeguard. First, the term "serious" vs. "material" implies a significant difference. Simply put, China Safeguard can be applied under weaker conditions than Global Safeguard. For China Safeguard to be applied, rising imports do not have to be the most important cause of injury to the domestic industry, while this has to be the case for Global Safeguard. That is, the imports from China need not be equal to or greater than any other cause. Second, China Safeguard is discriminatory and allows MFN treatment to be violated.<sup>13</sup>

The U.S. tire industry has several noteworthy characteristics. First, tire production is dominated by a few large multinational corporations (MNCs) in both the U.S. and the world. As of 2008, 10 firms produce the subject tires in the U.S., and eight of them are MNCs.<sup>14</sup> Production of the subject tires are so concentrated that five major MNCs (Bridgestone, Continental, Cooper, Goodyear, and Michelin) control about 95% of domestic production and 60% of worldwide production.<sup>15</sup> Except for Continental, seven MNCs have

<sup>11</sup> This finding is in line with Hufbauer and Lowry (2012) who document significant costs of the CTS to save few jobs.

<sup>12</sup> These studies mostly deal only with antidumping cases. Blonigen et al. (2003) provide a comprehensive survey on the literature of antidumping.

<sup>13</sup> There are three other primary areas under the WTO in which exceptions to MFN-treatment for import restrictions are broadly permissible: (1) raising discriminatory trade barriers against unfairly traded goods under antidumping or countervailing duty laws; (2) lowering trade barriers in a discriminatory manner under a reciprocal preferential trade agreement; and (3) lowering trade barriers in a discriminatory manner to developing countries unilaterally, for example, under the Generalized System of Preferences (GSP). For an additional discussion of the China safeguard, see Messerlin (2004) and Bown and Trade (2010).

<sup>14</sup> The 10 U.S. subject tire producers are Bridgestone, Continental, Cooper, Denman, Goodyear, Michelin, Pirelli, Specialty Tires, Toyo, and Yokohama. Eight firms except Denman and Specialty Tires are MNCs.

<sup>15</sup> Data source: Modern tire dealer (<http://www.moderntiredealer.com/stats/default.aspx>).

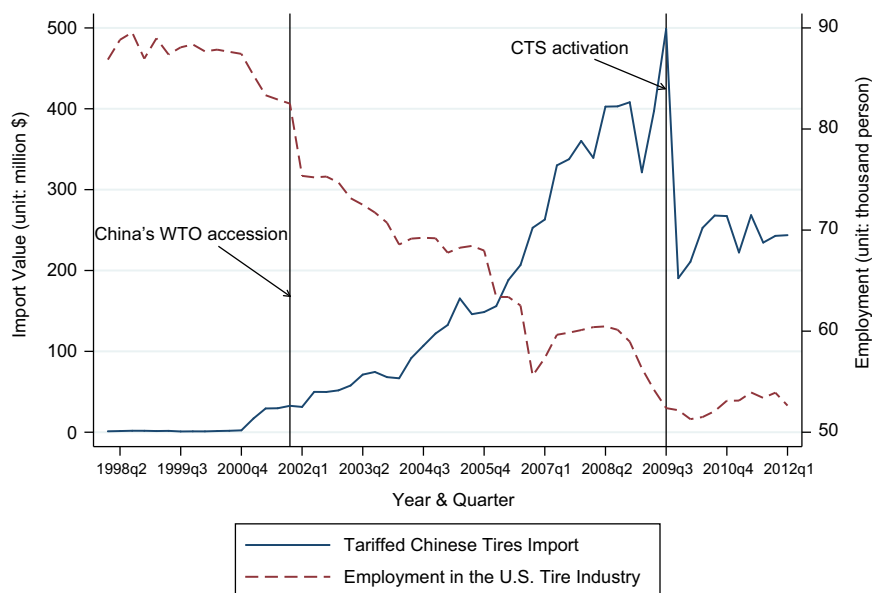


Fig. 1. Trends of subject tire imports and employment in the U.S. tire industry during 1998Q1–2012Q3.

manufacturing facilities in China. Second, the subject tires are known to feature three distinct classes: flagship (high quality), secondary (medium quality), and mass market (low quality). The domestic producers have largely shifted their focus to higher-value tires since 1990s, leaving mass market tire productions to overseas manufacturers.

These characteristics explain why the petition was not welcomed by the U.S. tire producers; the temporary tariff protection may rather hurt the MNCs' global production strategies. Moreover, the CTS would have little influence to the U.S. tire manufacturers that mainly produce high and medium quality tires, unless those tires are well substitutable for low quality Chinese tires.<sup>16</sup>

### 3. Data and descriptive statistics

Our data on quarterly imports are taken from the U.S. International Trade Commission. Import data are available up to Harmonized System (HS) 10-digit, and each 10-digit code is defined as a “product”. Import value is measured by customs value that is exclusive of U.S. import duties, freight, insurance, and other charges. We also define an “industry” as the 6-digit industry in the North American Industry Classification System (NAICS). According to the definition, the tire industry is 326211, “Tire Manufacturing (except Retreading)”, which comprises “establishments primarily engaged in manufacturing tires and inner tubes from natural and synthetic rubber”. This corresponds to 62 tire-related products in the HS 10-digit level (with heading 4006, 4011, 4012, and 4013) among which 10 tire products are subject to the safeguard measures.

Data on employment and wages in U.S. tire industry covering the same time period are from the Bureau of Labor Statistics Quarterly Census of Employment and Wages (QCEW).<sup>17</sup> In fact, Bureau of Labor Statistics provides two different industry-level employment databases, the QCEW and the Current Employment Statistics (CES). We use the QCEW in this paper, because it provides total employment and wages statistics for all 6-digit industries, while the CES contains only part of them.<sup>18</sup> For industry-level characteristics, we use data taken from the Annual Survey of Manufactures.

Fig. 1 plots time trends of the aggregate import value of the 10 tire products subject to the CTS as well as total employment in the U.S. tire industry from 1998Q1 to 2012Q3. The import of Chinese tires starts to surge in 2001, just before China's accession to the WTO. It continues to grow dramatically until the activation of the CTS, except for a slight drop in early 2009 due to the global financial crisis.<sup>19</sup> Specifically, the import increases by 300 times during 10 years from \$5.2 million dollars in 1999 to \$1.56 billion dollars in 2008. In terms of relative size, China alone accounts for a quarter of the U.S. total import of subject tires in 2008, with tire imports from the rest of the world (ROW) at \$4.80 billion dollars in the same year. The value also amounts to 9.2% of gross value added of the U.S. tire industry in 2008, which stood at \$16.98 billion dollars.

<sup>16</sup> Because of these characteristics of the U.S. tire industry, Prusa (2009) predicted that the effect of the CTS would be negligible.

<sup>17</sup> While wages are reported on a quarterly basis, employment data are produced monthly. We construct quarterly employment data by simply averaging of the monthly data.

<sup>18</sup> Both databases have employment data in the tire industry. We checked the discrepancy between the two data, but there was no systematic or significant difference.

<sup>19</sup> As Staiger and Wolak (1994) find, subject tire imports may also fall because of the safeguard investigation started from April in 2009.

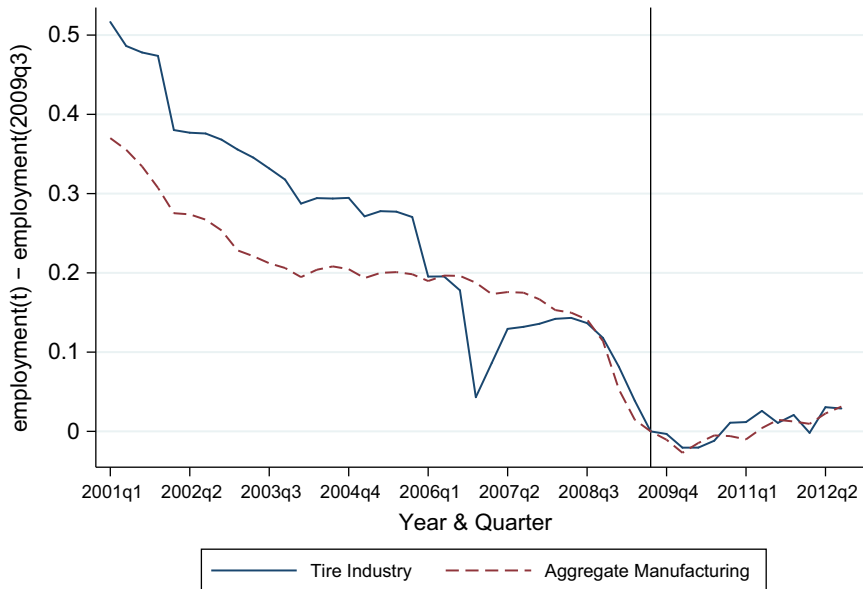


Fig. 2. Employment trends in the tire industry versus aggregate manufacturing during 2001Q1–2012Q3.

The punitive tariffs substantially discourage the rising trend, reducing total imports from China by 62% between 2009Q3 and 2009Q4. A sharp rise between Q2 and Q3 followed by the sharp decline between Q3 and Q4 indicates that some importers in the U.S. bought the subject Chinese tires in advance of the CTS to avoid the higher expected price after 2009Q3. After 2009Q4, tire imports from China are relatively flat, albeit at a much lower level compared to pre-CTS levels.

Interestingly, the trend of employment in the U.S. tire industry stands in sharp contrast to the trend of Chinese tire imports. It starts to fall when the Chinese tire imports start to rise in 2001. In particular, the decline of employment in 2002Q1 coincides with China's WTO accession as documented in [Pierce and Schott \(2012\)](#). Another falloff in 2006Q4 is caused by the strike in the U.S. tire industry and is not relevant to the Chinese tire imports. In terms of growth, employment in the U.S. tire industry falls by 30.5% from 2002Q1 to 2009Q3.

The activation of the CTS seems to not only stop further decline in employment (with some lags) but also prompt a slight recovery thereafter. As the Obama administration claims, total employment increases from 45,855 in 2009Q3 to 46,812 in 2011Q4, an increase of about one thousand workers. However, the employment trend around 2009 may be confounded by an economic recovery from the global financial crisis. This problem looms larger when we compare the (log of) employment trend in the tire industry to the one in aggregate manufacturing industry as shown in [Fig. 2](#). Employment in each period is rescaled by subtracting 2009Q3 value for comparison. The two trends are quite similar to each other around 2009 and afterward. Thus, the time-series data alone do not allow us to identify the safeguard effect on employment in the U.S. tire industry.

## 4. Empirical model

### 4.1. A conceptual framework

We conceptually sketch how domestic labor market can be affected by foreign competition to propose an empirical model for identifying the safeguard effect. If the labor market for an industry were competitive, domestic employment and wages would be simultaneously determined by its supply and demand elasticities. In reality, however, industries in the U.S. are likely to face non-competitive labor market. One main reason is the presence of labor union. As a matter of fact, we observe a union's bargaining in the tire industry expressed as a strike in 2006.<sup>20</sup> In a typical bargaining structure, the union members and employers bargain over wages leaving firms to set their employment level unilaterally. Therefore, the negotiated wages tend to be higher than the competitive rate of wages (or outside reservation wages) which in turn reduces the demand for labor ([Abowd and Lemieux, 1993](#); [Revenga, 1997](#)).<sup>21</sup>

<sup>20</sup> Non-competitive wages and employment can also be driven by efficiency wages or fair wages, but the directions to which wages and employment adjust would be the same in any case.

<sup>21</sup> This bargaining structure is called the monopoly union model (also known as the labor demand model or the right-to-manage model). We prefer this model to the one called the strongly efficient contract model where the parties bargain over both wages and employment, because firms in the U.S. usually do not have the duty to bargain directly over the level of employment. See [Abowd and Lemieux \(1993\)](#) and references therein for more discussions about the two models and empirical evidences that support the monopoly union model.

That said, we benchmark the empirical model by [Reventa \(1997\)](#) where non-competitive wages are first negotiated between labor unions and employers under foreign competition and then firms choose their employment level according to their own labor demand curves. Foreign competition can affect domestic employment and wages through two channels in the model. First, an increase in import competition would shrink the demand for domestic product and thereby decrease both labor demand and the competitive component of wages. Second, it would trim down the size of rents available to the industry and hence reduce the rent component of wages. In the end, the negotiated wages would decline by both channels, while firms' employment would be determined conditional on the negotiated wages and output demand. Specifically, consider the following reduced-form employment and wage equations:

$$n_{it} = \alpha_1 q_{it} + \alpha_2 k_{it} + \alpha_3 m_{it} + \alpha_4 r_{it} + \alpha_5 w_{it} + \gamma_{it}^n D_{it} + \mu_i^n \lambda_t^n + \epsilon_{it}^n \quad (1)$$

and

$$w_{it} = \beta_1 q_{it} + \beta_2 k_{it} + \beta_3 m_{it} + \beta_4 r_{it} + \gamma_{it}^w D_{it} + \mu_i^w \lambda_t^w + \epsilon_{it}^w \quad (2)$$

where  $n_{it}$  and  $w_{it}$  are log of employment and log of average weekly wages realized in industry  $i$  at time  $t$ , respectively. Without the presence of labor union and foreign competition, output ( $q_{it}$ ), cost of capital ( $k_{it}$ ), and cost of materials ( $m_{it}$ ) all in logarithm would alone determine the level of employment and wages. However, as explained above, the collective bargaining under foreign competition necessitates adding a measure of import competition in both equations and additionally the negotiated wages in the employment equation. We proxy the magnitude of import competition at industry level by the (log of) import penetration ratio ( $r_{it}$ ) that is equal to the ratio of import to market size (= output + import – export).<sup>22</sup>

When the government intervenes in product markets by imposing punitive tariffs to certain imported goods, its impact on domestic employment and wages is supposed to be captured by the coefficient  $\gamma_{it}^n$  and  $\gamma_{it}^w$ . Thus,  $D_{it}$  is a treatment assignment indicator that is one if industry  $i$  is protected by the safeguard action at time  $t$ , and zero otherwise. Note that  $\gamma_{it}$ 's in employment and wage equations vary fully over time and across industries to give us a complete set of heterogeneous safeguard effects on all industries in all post-intervention periods. The time-varying safeguard effect reflects the declining schedule of the CTS by 5% annually, but it could also mean that the responses of industries may come with some lags or simply be transitory.

Each equation above contains the term for unobservables ( $\mu_i \lambda_t$ ) as well as the error term ( $\epsilon_{it}$ ) with different superscripts. While the error term is assumed to be a white noise as usual, the term for unobservables needs more explanation. The unobservables are made up of a vector of interactive fixed effects of which dimension is unknown. They essentially capture the effects of an unknown number of common factors ( $\lambda_t$ ) with heterogeneous factor loadings ( $\mu_i$ ) that may be jointly correlated with the treatment assignment.<sup>23</sup> This specification is particularly useful in our case study, because several economy-wide shocks (i.e., common factors) that occurred during the sample period are likely to have heterogeneous impacts on domestic employment and wages across industries depending on industry-specific characteristics (i.e., factor loadings). For example, we have no rationale to assume that the financial crisis in 2008 would affect all industries by an equal magnitude. Similarly, not all industries must be equally affected by China's accession to the WTO in 2001 or its currency manipulation over recent years. An even more relevant scenario is that, as described in the Introduction, nation-wide political concerns over job security against foreign competitors can affect each industry's labor market outcome in different magnitudes depending on industry characteristics.

The interactive fixed effects,  $\mu_i \lambda_t$ , in our model allow these macro-shocks to interact with industry-specific time-constant characteristics so that the impact of each shock can differ across industries. If the selection into petition filings for protection and subsequent government interventions are based on these interactive unobservables, failing to control for such interactive effects would lead to biased estimates for the treatment effects.

#### 4.2. Estimation strategy

A common approach to identify the treatment effect is the Difference-In-Differences (DID) design. In a conventional DID model, the treatment (tire) industry is compared with some control industries that have not experienced any trade policy change. An important assumption here is that the treatment industry would have followed the same trend as control industries if the policy had not changed. Therefore, the DID model requires a proper selection of a control group to satisfy the common trend assumption.

In our case study, however, there is no clear criterion which industries should be chosen as the control group. One choice may be a group of all manufacturing industries that filed no petition (hence no protection) during the sample period, but those industries may be too heterogeneous in their characteristics to have the same time trend in outcome variables. Alternatively, a group of manufacturing industries that did file petitions but failed to be accepted can be considered in the sense that the group would face more or less similar circumstances to the tire industry. Another possible control group consists of all industries (other than the tire industry) under the same NAICS 3-digit code, i.e., 326 Plastics and Rubber Product Manufacturing since they are classified within the same 3-digit code based on the similarity of industry characteristics. However, neither of these groups are convincing to satisfy the common trend assumption.<sup>24</sup>

<sup>22</sup> Non-competitive market structure may also induce rigidities in employment and wages over time, because the terms and conditions of contract may hold at least for a few years. We can include lagged employment and wages in Eqs. (1) and (2), respectively, to account for such rigidities.

<sup>23</sup> Obviously, our model specification is more general than the conventional configuration of panel fixed effects. By letting  $\mu_i^w = [\mu_i^w \ 1]$  and  $\lambda_t^w = [\lambda_t^w]$ , the vector of interactive fixed effects reduces to the conventional two factors panel model with industry-specific effect and time effect.

<sup>24</sup> Another problem in the conventional DID method occurs if the number of controls are small, since it leads to an over-rejection of the null hypotheses of zero effect. Indeed, the suggested control groups above, except the group of manufacturing industries that filed no petition, have less than 30

The Synthetic Control Method (SCM), designed by [Abadie and Gardeazabal \(2003\)](#) and [Abadie et al. \(2010\)](#), is advantageous to deal with the present problem. They provide a method to estimate the missing counterfactual for a single treated observation. The estimated missing counterfactual is given by the outcome of a single “synthetic” control, defined as a weighted combination of potential controls. The SCM chooses the weights using a data-driven approach; the weights are chosen such that the synthetic control fits the treated observation in the pre-intervention period. The estimated missing counterfactual for the treated observation is then the outcome of this synthetic control in the post-intervention period. Thus, the SCM avoids the arbitrary definition of the control group required under DID. Instead, with SCM, the researcher needs only to define the group of potential controls, some of which may end up receiving zero weight.

There are two more advantages of using the synthetic control method in our analysis. First, the SCM can estimate the time-varying heterogeneous effect of the CTS, while a standard DID or fixed effects panel estimation can only provide an estimate for the time-invariant average treatment effect. Second, it allows the dimension of the vector of interactive fixed effects to be arbitrarily unknown. As already mentioned, this advantage is important to obtain consistent estimates for the safeguard effect, given that the interactions of unobservable (time-varying) macro-shocks and (time-invariant) industry-specific characteristics are potentially related to the industry selection mechanism for trade remedies.

However, the SCM has two notable caveats. First, we need all observables to be time-invariant in the model in order for the SCM to work, i.e., the estimation equations should look like the following:

$$n_{it} = X_i^n \alpha + \gamma_{it}^n D_{it} + \mu_i^n \lambda_t^n + \epsilon_{it}^n \quad (3)$$

$$w_{it} = X_i^w \beta + \gamma_{it}^w D_{it} + \mu_i^w \lambda_t^w + \epsilon_{it}^w. \quad (4)$$

where  $X_i^n$  and  $X_i^w$  are the vectors of all observables in Eqs. (1) and (2) that are restricted to be constant over time. Thus,  $X_i$ 's should be interpreted as the pre-intervention industry characteristics to predict the post-intervention values of outcome variables. Although this requirement may appear restrictive, the SCM can instead have any (or combination of) available pre-intervention outcome variables in  $X_i$ , that is,  $X_i$  can include all values of dependent variables in the pre-intervention period as predictors. These lagged values can explain the time trend of dependent variables and therefore account for rigidities in employment and wages in the pre-intervention period. Moreover, the problem of time-constant restriction on predictors would be minimized by the extent to which each lagged values represent the industry characteristics at that period.

The second caveat needs a more detailed explanation and will be described in the following subsection.

### 4.3. Implementation

Our sample period in the synthetic control analysis ranges from 2001Q1 when the employment level in the U.S. tire industry starts plunging to 2012Q3 when the CTS ends. Hence, the pre-intervention period spans 35 quarters from 2001Q1 to 2009Q3, and 12 quarters from 2009Q4 to 2012Q3 are the post-intervention period.<sup>25</sup> Without loss of generality, let the tire industry be industry 1 among observable industries. For all  $I-1$  potential control industries, a vector of weights,  $\omega = [\omega_2, \omega_3, \dots, \omega_I]$  such that  $0 \leq \omega_i \leq 1$  for  $i = 2, \dots, I$  and  $\sum_{i=2}^I \omega_i = 1$ , is assigned such that

$$\sum_{i=2}^I \omega_i^* y_{it} = y_{1t}, \quad \forall t \leq 2009Q3 \quad \text{and} \quad \sum_{i=2}^I \omega_i^* X_i = X_1. \quad (5)$$

Here, the pre-intervention outcomes and the vector of predictors,  $(y_{it}, X_i)$ , are either  $(n_{it}, X_i^n)$  or  $(w_{it}, X_i^w)$  for all  $i \in I$ . Eq. (5) implies that one can obtain the exact solution for  $\omega^*$  only if  $(\{y_{1t}\}_{t \leq 2009Q3}, X_1)$  belongs to the convex hull of  $[(\{y_{2t}\}_{t \leq 2009Q3}, X_2), \dots, (\{y_{It}\}_{t \leq 2009Q3}, X_I)]$ . If it is not the case, the SCM finds the optimal weights that minimize the distance between variables in the left- and right-hand sides of Eq. (5), but the fit may be poor especially when the distance is far. To avoid such problem, we choose all NAICS 6-digit manufacturing industries that filed no petition during the sample period as our potential control industries in the baseline analysis. This selection gives us 145 control industries.

Note that in order for the SCM to provide an unbiased estimator of the counterfactual  $y_{1t}$  for  $t \geq 2009Q4$ , the optimal weights,  $\omega^*$ , should also satisfy that  $\sum_{i=2}^I \omega_i^* \mu_i = \mu_1$ , i.e.,  $\mu_1$  must lie in the convex hull of  $(\mu_2, \mu_3, \dots, \mu_I)$ . This is the second caveat of the SCM mentioned above, because we do not know whether the convex hull covers  $\mu_1$  since  $\mu_i$ 's are not observed. [Abadie et al. \(2010\)](#) show that, for a sufficiently long pre-intervention period with modest model assumptions, a synthetic control fits  $(\{y_{1t}\}_{t \leq 2009Q3}, X_1)$  only if it fits  $X_1$  and  $\mu_1$  so that  $\sum_{i=2}^I \omega_i^* \mu_i = \mu_1$  holds approximately. Given that the tire industry is the only one that receives a China Safeguard, however, it may have unobserved abnormal characteristics: factor loadings for the tire industry may lie outside the convex hull of controls, which leads a biased estimate.<sup>26</sup> [Gobillon and Magnac \(in press\)](#) address this issue in their Monte Carlo study and show that the panel model with interactive fixed effects suggested by [Bai \(2009\)](#) outperforms the SCM in such case. Hence, we will employ Bai's model in our robustness check.

(footnote continued)

industries. According to [Bertrand et al. \(2004\)](#), we need at least about 40 control industries (with one treatment industry) in order to avoid the over-rejection problem.

<sup>25</sup> The extension of the pre-intervention period, for example to 1998Q1, does not change our finding at all. The result is available upon request.

<sup>26</sup> We are grateful to a referee for this point and alternative suggestions.

**Table 1**  
Predictors of employment and wages.

Predictors	Tire industry	Average <sup>a</sup>	Synthetic industry for	
			Employment	Wages
$\Delta$ Employment ( $\Delta n$ )	-0.517	-0.290	-0.511	-
Wages ( $w$ )	7.014	6.956	7.059	-
$\Delta$ Wages ( $\Delta w$ )	0.106	0.174	-	0.109
Output ( $q$ )	8.307	7.620	8.385	8.377
Cost of capital ( $k$ )	5.397	3.940	4.623	4.819
Cost of material inputs ( $m$ )	7.869	6.915	7.750	7.783
Import penetration ratio ( $r$ )	-0.801	-2.102	-1.849	-1.410

Notes: All variables are log transformed. Growth rates for employment and wages are calculated as the % change from 2001Q1 to 2009Q3. Output, cost of capital, cost of material inputs, import penetration ratio, and wages (only in employment equation) are 2008 values. Six lagged values of employment and wages are included as predictors for the trends of post-intervention employment and wages, respectively, but are not reported here to save space.

<sup>a</sup> The simple average of all potential control industries.

**Table 2**  
6-Digit manufacturing industries constituting synthetic industries.

Employment			Wages		
NAICS	Industry title	Weight	NAICS	Industry title	Weight
<b>Treatment Industry</b>					
326211	Tire Manufacturing	n/a	326211	Tire Manufacturing	n/a
<b>Control Industries</b>					
311611	Animal (except Poultry) Slaughtering	0.121	311611	Animal (except Poultry) Slaughtering	0.072
313210	Broadwoven Fabric Mills	0.233	312111	Soft Drink Manufacturing	0.100
325120	Industrial Gas Manufacturing	0.225	312140	Distilleries	0.201
333996	Fluid Power Pump & Motor Manufacturing	0.050	324110	Petroleum Refineries	0.099
334112	Computer Storage Device Manufacturing	0.073	325411	Medicinal & Botanical Manufacturing	0.026
334290	Other Communications Equipment Manufacturing	0.015	336330	Motor Vehicle Steering & Suspension Components	0.332
336120	Heavy Duty Truck Manufacturing	0.021	336991	Motorcycle, Bicycle, & Parts Manufacturing	0.129
336411	Aircraft Manufacturing	0.220	337121	Upholstered Household Furniture Manufacturing	0.040
336510	Railroad Rolling Stock Manufacturing	0.042			

Notes: The table only shows industries with strictly positive weights for constructing each synthetic industry.

That said, the estimated treatment effect on the tire industry is given by

$$\hat{\gamma}_{1t} = y_{1t} - \sum_{i=2}^I \omega_i^* y_{it}, \quad \forall t \geq 2009Q4 \quad (6)$$

where  $\hat{\gamma}_{1t}$  stands for either employment or wages. Finally, as time-invariant pre-intervention predictors in  $X_i$ 's, we include the 2008 values of total domestic shipments, cost of capital, cost of materials, and import penetration ratio (additionally, wages for the employment equation) as well as the average growth rate of dependent variables over the pre-intervention period. All values of outcome variables in this pre-intervention period could also be added in  $X_i$ 's as predictors. However, even with only a few selective values, we can provide almost the same but more efficient estimates for the treatment effects. Therefore, we include six lagged values of employment and wages in 2001Q1, 2002Q4, 2004Q3, 2005Q4, 2007Q3, and 2009Q2 in the estimation equations as additional predictors.

## 5. Estimation results

### 5.1. Main finding

After the synthetic industries for employment and wages are constructed, their industry characteristics are compared to those of the tire industry as well as those of simple averages of all potential control industries in Table 1. Numbers in the table indicate that the two synthetic industries are closer to the tire industry than the simple averages in both growth rates and industry characteristics. In particular, the linear time trends of dependent variables (i.e., employment and wages) from 2001Q1 to 2009Q3 are almost identical between the tire industry and each synthetic industry. This provides a strong support for the common trend assumption. Table 2 reports the list of control industries that construct the two synthetic industries. Since employment and wages do not exhibit the same time trend, we expect the optimal weights for each synthetic industry to differ, which turns out to



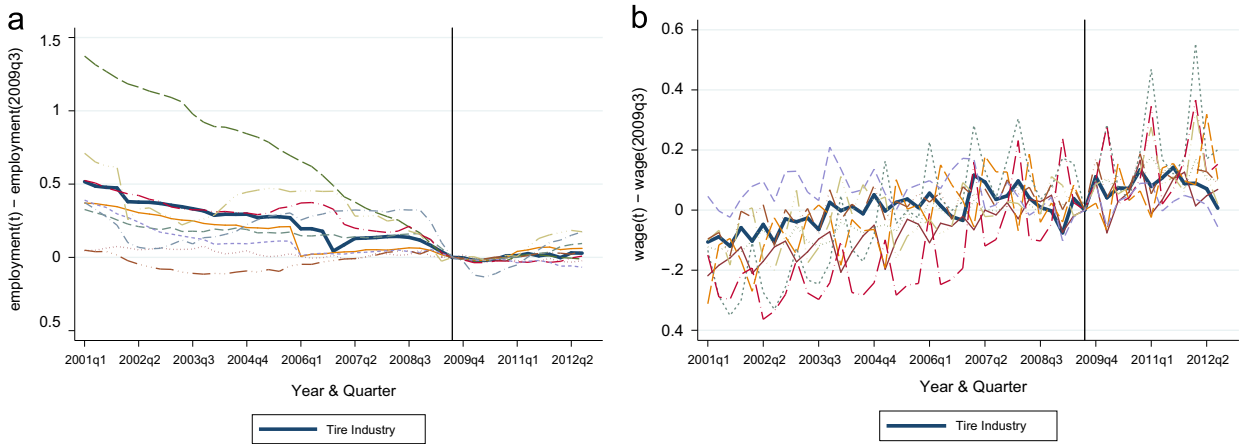


Fig. 3. Trends in the U.S. tire versus control industries during 2001Q1–2012Q3. (a) Employment and (b) Wages.

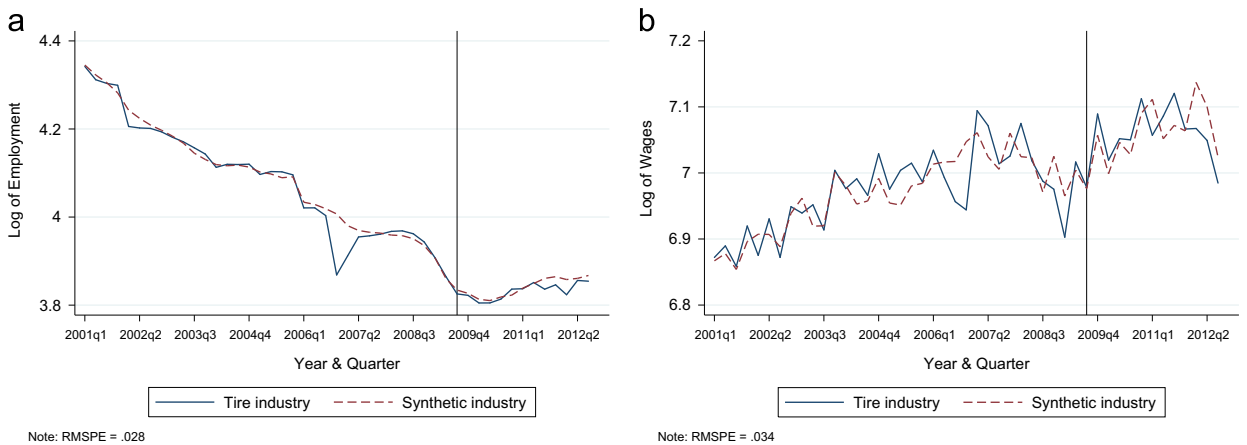


Fig. 4. Trends in the U.S. tire vs. synthetic industry during 2001Q1–2012Q3. (a) Employment and (b) Wages.

be true. This again supports the advantage of the synthetic industries over the equally weighted average of all controls for both employment and wages.

Fig. 3 shows the (log of) employment and wage trends in the tire versus all positive weighted control industries. Values are rescaled in the same manner as in Fig. 2. Certainly, we can see that the trends in the control industries encompass the trend in the tire industry for both employment and wages. This suggests that the convex hull requirement of the SCM would be satisfied at least approximately and the synthetic control industries would be able to replicate the missing counterfactual.

Fig. 4 compares the trends of employment and wages in the U.S. tire industry with those of the synthetic industries. In general, the synthetic industries mimic employment and wage trends of the tire industry quite well in the pre-intervention period. An exception is around 2006 due to the strike in the U.S. tire industry. The Root Mean Squared Prediction Error (RMSPE) shown at the bottom of each figure measures the sum of discrepancies between outcomes in the tire and in synthetic industries for the pre-intervention period. It will be used later as a criterion for whether a synthetic industry is constructed well enough to mimic the tire industry. For the post-intervention period, we see no significant differences between the tire industry and the synthetic industries for both employment and wages.

To infer the significance of the treatment effects formally, the SCM suggests a set of placebo tests. A placebo test can be performed by setting one of the control industries as the treated industry and all other industries (including the tire industry) as untreated industries. Specifically, we set the tire industry to be one of the control industries, and treat industry 2 as the treatment industry. Then, we follow the same SCM procedure described above to estimate  $\hat{\gamma}_{2t}$  for  $t \geq 2009Q4$  using the rest of industries 1 and 3 through 146 as control industries. This procedure is repeated for  $i = 3, \dots, 146$  with replacement. Since there are no control industries that are protected during the sample period, their treatment effects,  $\hat{\gamma}_{it}$  for  $i = 2, \dots, 146$ , are expected to be zero. Hence, if the tire industry was affected by the safeguard measures, we should be able to observe significantly different  $\hat{\gamma}'_{it}$ s from all other  $\hat{\gamma}'_{it}$ s.

The results of two sets of placebo tests for employment and wages are displayed in Fig. 5. Because some industries have poor synthetic industries with high RMSPEs, we show the estimated treatment effects for industries whose RMSPE is less than 0.014 for employment and 0.027 for wages. The vertical axis shows the estimated treatment effects of the tire and placebo industries over the sample period. All of them are close to zero before the activation of CTS, with exception of 2006 in the case of the tire industry. In particular, the treatment effects in the tire industry after the CTS are well bounded by

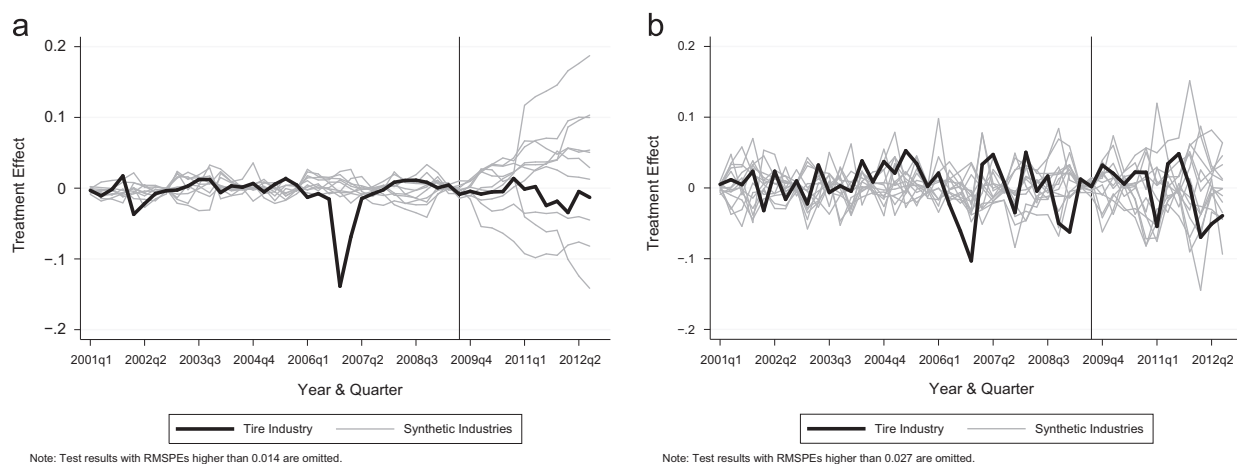


Fig. 5. Placebo tests for the CTS effect on labor market outcomes. (a) Employment and (b) Wages.

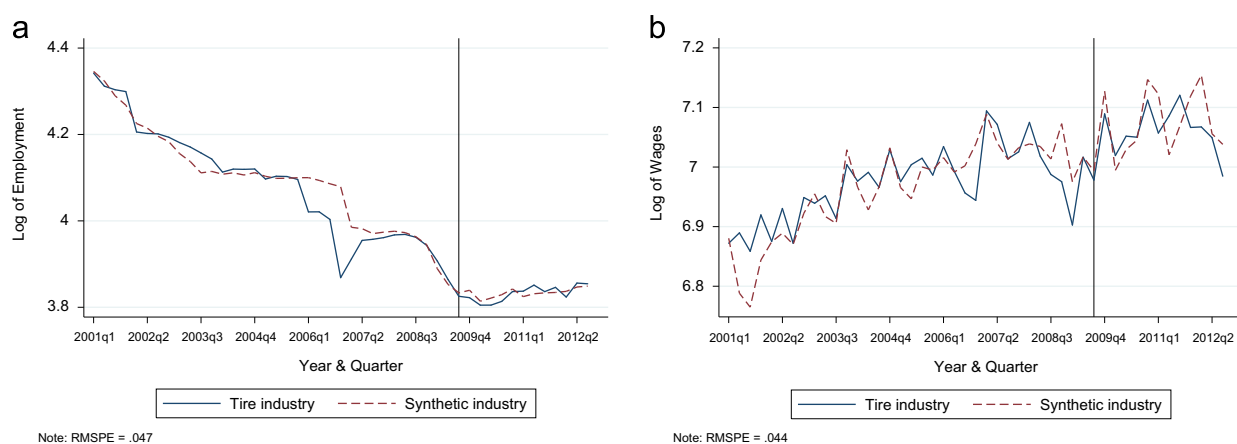


Fig. 6. SCM estimation results using TTB rejected industries as potential controls. (a) Employment and (b) Wages.

other placebo treatment effects. This confirms that neither employment nor wages in the tire industry are significantly affected by the safeguard measures.

## 5.2. Robustness check

We conduct a couple of robustness checks for our findings in the baseline analysis. First of all, since the choice of the potential control group might be critical to obtain the results, we use two alternative control groups: (i) a group of industries that filed a form of TTBs at least once during the sample period, but failed to be protected in the end, and (ii) a group of six-digit industries under NAICS 326 Rubber and Plastic Product Manufacturing that are free of any TTB case during the sample period (including filed, but rejected ones). The former group includes 24 potential control industries. Using the same model and predictors, the SCM estimation results are presented in Fig. 6. Clearly, we confirm no CTS effect on employment and wages in the U.S. tire industry. The latter group comprising 14 potential controls does not change the results either. Furthermore, our findings still hold when the period of the tire industry strike (i.e., 2006Q4) is dropped from our sample period and when employment and wages are measured in levels instead of log transforms.<sup>27</sup>

Next step is to employ alternative estimators to check whether such different estimation methods would produce the same findings as the SCM. We first estimate the treatment effects using panel models with Interactive Fixed Effects (IFE) suggested by Bai (2009) and extended by Moon and Weidner (2015). As already mentioned, Bai's IFE procedure has shown to outperform the SCM when the unobserved industry-specific factor loadings for the treated units are outside the convex hull of those for untreated units (Gobillon and Magnac, in press). Bai's procedure, however, assumes that the number of factors is known, which is not the case in practice. Moon and Weidner (2015) show that the IFE procedure with larger number of factors than the true number can still provide consistent estimates under certain conditions. Moon and Weidner

<sup>27</sup> We do not report every estimation and placebo test result here to save space. All results are available upon request.

**Table 3**  
Estimated effects of the CTS using Bai's panel models with interactive fixed effects.

# of Factors	R=2	R=3	R=4	R=5	R=6	R=7	R=8	R=9	R=10
<b>Panel A: employment</b>									
$\hat{\tau}_1$ (Year 1)	-0.008 (0.008)	0.011 (0.012)	0.008 (0.012)	0.013 (0.013)	0.013 (0.015)	0.013 (0.016)	0.002 (0.015)	-0.005 (0.018)	-0.015 (0.022)
$\hat{\tau}_2$ (Year 2)	-0.012 (0.012)	0.011 (0.016)	0.006 (0.016)	0.016 (0.017)	0.015 (0.019)	0.014 (0.021)	0.006 (0.022)	-0.003 (0.033)	0.012 (0.040)
$\hat{\tau}_3$ (Year 3)	-0.012 (0.009)	0.013 (0.015)	0.008 (0.016)	0.023 (0.020)	0.020 (0.024)	0.019 (0.024)	0.009 (0.025)	-0.004 (0.041)	0.020 (0.055)
<b>Panel B: wages</b>									
$\hat{\tau}_1$ (Year 1)	-0.038** (0.014)	0.015 (0.024)	0.012 (0.023)	-0.002 (0.016)	0.005 (0.017)	0.027 (0.021)	0.024 (0.023)	0.023 (0.023)	0.030 (0.025)
$\hat{\tau}_2$ (Year 2)	-0.033 (0.020)	0.025 (0.031)	0.023 (0.030)	-0.007 (0.018)	0.003 (0.023)	0.020 (0.025)	0.017 (0.028)	0.020 (0.029)	0.031 (0.030)
$\hat{\tau}_3$ (Year 3)	-0.083** (0.020)	-0.016 (0.031)	-0.017 (0.030)	-0.057* (0.023)	-0.036 (0.029)	-0.020 (0.032)	-0.022 (0.034)	-0.019 (0.035)	-0.009 (0.037)

Notes: The sample covers 146 industries ( $N$ ) and 46 quarter periods ( $T-1$ ) from 2001Q2 to 2012Q3, which gives sample size of 6716. All estimates are corrected for the heteroskedasticity biases in both industry and time dimensions and the dynamic bias as suggested in Bai (2009) and Moon and Weidner (2014). Bandwidth for the dynamic bias correction is set to 4 for both employment and wages. Heteroskedasticity-robust standard errors are estimated and the corresponding  $t$ -values are reported in parentheses.

\* Significance at the 1% and the 5% levels, respectively.

\*\* Significance at the 1% and the 5% levels, respectively.

(2014) also extend Bai (2009) to a dynamic model that can incorporate the lagged dependent variable as an explanatory variable. See Bai (2009) and Moon and Weidner (2014, 2015) for a full account.

Our model specification closely follows the empirical application in Moon and Weidner (2015). We let the treatment effect vary each year to account for a possible dynamic (or temporary) effect of the CTS, as we did in the SCM. We also incorporate a dynamic feature of labor market in our model by including a lagged dependent variable. Thus, the model we estimate is

$$y_{it} = \alpha y_{it-1} + X_{it}\beta + \sum_{k=1}^3 \tau_k D_{k,it} + \mu_i \lambda_t + \epsilon_{it} \quad (7)$$

where  $y$  is either (log of) employment or wages, and  $X_{it}$  includes the same explanatory variables as in Eqs. (1) and (2), respectively, but those explanatory variables are now allowed to vary over time.  $D_{k,it}$  ( $k = 1, 2, 3$ ) are annual dummies after the CTS activation:  $D_{1,it}$  equals one if  $i$  is treated and  $2009Q3 \leq t < 2010Q3$ , and zero otherwise.  $D_{2,it}$  equals one for the treated unit(s) in the second year of activation (i.e.,  $2010Q3 \leq t < 2011Q3$ ), and  $D_{3,it}$  is one in the last year for the same  $i$ . This specification corresponds to the changes in safeguard duties which dropped by 5% every year after its activation. Hence, the annual dummies can capture different magnitudes of the policy impact in each year.

Following the practices of Gobillon and Magnac (in press) and Moon and Weidner (2015), we report the IFE estimation results with various factor dimensions in Table 3.<sup>28</sup> In each column, the number of factors (including the known ones) varies from 2 to 10 so that it is large enough to cover all relevant factors. All estimates are corrected for the heteroskedastic biases both in industry and time dimension as in Bai (2009). We also correct for the dynamic bias pointed out in Moon and Weidner (2014) with bandwidth set to 4.<sup>29</sup> Heteroskedasticity-robust standard errors are reported in parentheses.

In the first column ( $R=2$ ), the vector of interactive fixed effects includes only additive factor and factor loading, i.e.,  $\mu_i = [\mu_{1i} \ 1]$  and  $\lambda_t = [1 \ \lambda_{1t}]$ . Thus, Eq. (7) reduces to a standard DID model with industry-specific effect and time effect (with lagged dependent variable). The estimation results indicate that the CTS did not affect employment, but its impact on wages in the tire industry was negatively significant and became larger in the third year despite the gradual decline in the safeguard duties. All significant effects disappear in the second column, once the model adds the linear time trend ( $\mu_{2i}t$ ) as another known factor, which is also known as a random growth model. Yet, our concern about heteroskedastic industry responses to unobserved common shocks has not been accounted for. From the third to the last column, we increase the number of unknown factors to control for such effects. The estimation results with factor number greater than 3 clearly show that there are no significant effect of the CTS activation on both employment and wages in the U.S. tire industry, even temporarily. Not only that, estimates are mostly stable throughout the columns, which is consistent with the theoretical prediction in Moon and Weidner (2015).

Bai's procedure is not the only approach to estimate a linear IFE panel model. Another frequently used approach is the Common Correlated Effect (CCE) estimator by Pesaran (2006). The CCE estimator controls for biasing effects of the unobserved common factors by including cross-sectional averages of dependent and independent variables as regressors. This estimator does not require the number of factors to be known contrary to Bai's estimator, but instead it restricts the data generating

<sup>28</sup> All estimations were implemented using matlab code written by Moon and Weidner (2014, 2015). We are grateful to them for sharing the code.

<sup>29</sup> Different values of bandwidth had only negligible effects on the bias correction and did not change statistical significance of estimates. We also estimated the model without the lagged dependent variable. The result did not change our finding qualitatively.

**Table 4**  
Alternative estimates for the China tire safeguard effect.

Treatment effect	(1) CCEP		(2) PSM-Matched	
	Employment	Wages	Employment	Wages
$\hat{\tau}_1$ (Year 1)	0.003 (0.002)	0.060 (0.058)	-0.004 (-0.121)	0.029 (1.529)
$\hat{\tau}_2$ (Year 2)	0.006 (0.011)	0.071 (0.063)	0.012 (0.229)	0.053 (2.084)
$\hat{\tau}_3$ (Year 3)	0.008 (0.020)	0.009 (0.007)	0.055 (1.032)	-0.007 (-0.459)
Lagged dependent variable	Yes	Yes	No	No
Industry & time fixed effects	Yes	Yes	Yes	Yes
Industry-specific linear time trends	Yes	Yes	Yes	Yes
Observations	6716	6716	11,750	10,340

Notes: Column (1) shows CCEP estimates by Pesaran (2006). The sample covers 146 industries ( $N$ ) and 46 quarter periods ( $T - 1$ ). Standard errors based on non-parametric variance estimator of Eq. (69) in Pesaran (2006) with equal cross-sectional weights ( $\omega_i = N^{-1}$ ) are reported in parentheses. Column (2) presents panel within estimator with matched sample using propensity score matching. The sample includes matched industries (311611, 312130, 321219, 322110, 334210, and 336111 for employment and 322110, 324110, 327310, and 336111 for wages) and 47 quarter periods ( $T$ ). Robust standard errors are clustered at industry level in parentheses.

processes of observables in a way that they can control for the factors. We refer to Pesaran (2006) for more discussion of the estimation procedure and present the CCE Pooled (CCEP) estimates of Eq. (7) in the first column of Table 4.<sup>30</sup> The two known factors are included in the regression. Standard errors based on non-parametric variance estimator of Eq. (69) in Pesaran (2006) with equal cross-sectional weights (i.e.,  $\omega_i = N^{-1}$ ) are reported in parentheses. Clearly, there is no significant CTS effect, both statistically and economically, on domestic employment and wages in the tire industry.

The third estimator we consider is the fixed effects panel estimator with matched sample using the Propensity Score Matching (PSM) method. Just like we match industry characteristics in 2008 between the treatment and control groups for the SCM case, here we match observables in 2008 (i.e., from 2008Q1 to 2008Q4) and choose the 10 nearest neighbors to the tire industry with replacement, based on the propensity scores.<sup>31</sup> Then, we run a frequency weighted regression on the explanatory variables with additive fixed effects as well as industry-specific linear time trend. Lagged dependent variable is not included in the regression, but we allow an arbitrary serial correlation of standard error. Consequently, our sample size becomes 11,750 for employment equation and 10,340 for wages equation. Column (2) in Table 4 indicates that the treatment effects are not significant for both employment and wages.

As a final note, the industry that we have analyzed (NAICS 326211) experienced more than one policy change. While Passenger Car and Light Truck Tires under NAICS 326211 were subject to China Safeguard from the 3rd quarter of 2009 for 3 years, Off-the-road Tires imported from China were subject to anti-dumping (AD) duties from the 3rd quarter of 2008 and still effective. Since our treatment group (NAICS 326211) is contaminated by the AD duties, ignoring that it might cast doubts on our empirical results. However, the domestic production of off-the-road tires is less than 5%, whereas that of passenger car and light truck tires is about 80% of the total production in the tire industry.<sup>32</sup> Hence, even if AD duties might have affected the employment and wages of the U.S. tire industry, its impacts would not be economically significant. Moreover, if one looks at the employment and wages trend around 2008 in Fig. 4, the AD duties do not seem to matter for the domestic employment and wages.<sup>33</sup>

## 6. Potential mechanism

Our evidence regarding the CTS raises the question of why there is no effect. In this section, we provide a potential mechanism through which the CTS had only a negligible impact on employment and wages in the U.S. tire industry.

<sup>30</sup> Pesaran (2006) proposes two kinds of CCE estimators: CCE mean group estimator and CCE pooled estimator, and we apply the latter to our analysis. Everaert and Groote (2016) document that the CCEP estimator is quite useful for cross-sectional dependent dynamic panel models when  $T$  is not too small as in our case. Note that we did not include the cross-sectional averages of  $D_{k,it}$  for  $k = 1, 2, 3$  in our estimation reported in column (1), because they have little variations across industries; the estimation including the averages, in fact, provides too small values of coefficient estimates and standard errors (basically zero), but still did not change our finding qualitatively.

<sup>31</sup> The selected control industries in terms of NAICS code are 311611, 312130, 321219, 322110, 334210, and 336111 for the employment equation and 322110, 324110, 327310, and 336111 for the wage equation.

<sup>32</sup> Authors calculate the ratio using disaggregated production data from 2008 Annual Survey of Manufacturers. This ratio is similar to the report of Modern Tire Dealer in 2008. In terms of imports proportion, off-the-road tires were only 9% out of the total Chinese new tire imports in 2008, while passenger car and light truck tires account for about 75%.

<sup>33</sup> In fact, we attempt to investigate passenger car tires only. While the employment and wages data on passenger car tires are not available, the annual shipment data is available from Annual Survey of Manufacturers. We calculate the annual ratio of passenger car tire production to the total tire production and multiply this ratio by the employment data, assuming that the shipment ratio is proportional to the employment ratio. If there had been a change in employment of passenger car tires manufacturing, the shipment must have been reflected. SCM results and fixed effects panel regression results using this weighted data produce the exactly same message, i.e., no impact of CTS on domestic labor market. Estimation results are available upon request.

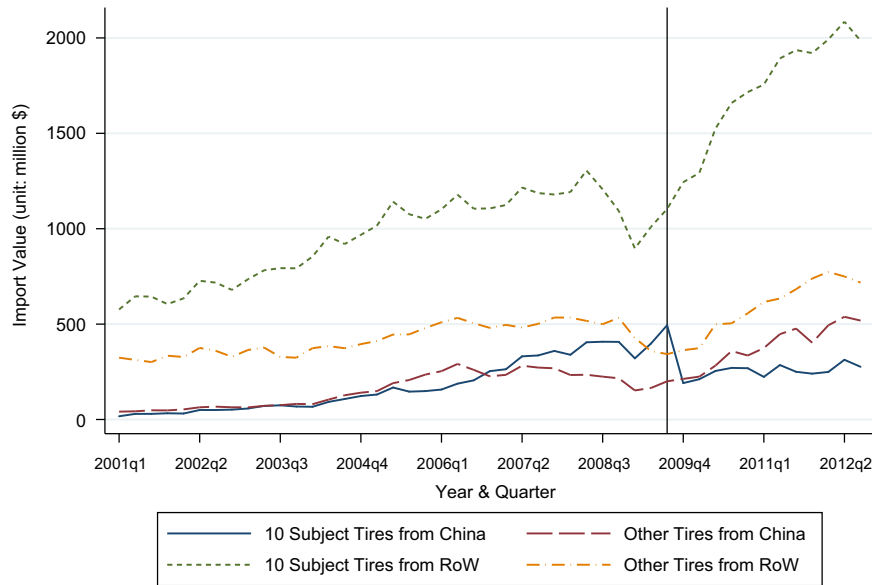


Fig. 7. Trend in the U.S. tire import during 2001Q1–2012Q3.

Specifically, we focus on the discriminatory nature of TTB as the key driving factor: when the punitive tariff is imposed on a certain set of products made in only one or a few countries, imports may be diverted to other non-tariffed countries that produce the same products. As Prusa and Feenstra (1997) argue, if this import diversion is complete in the sense that the decrease in import from the target countries is offset by the increase in import from non-target countries, domestic producers have little room for any adjustment.<sup>34</sup> In our case, we indeed find a complete import diversion in terms of import value as well as volume (i.e., quantity). Obviously, however, not every TTB would induce the complete diversion as in our case, and therefore we need to understand what determines the degree to which import is diverted. Although answering this question is beyond the scope of our study, we provide some theoretical and anecdotal evidence that MNCs play an important role for the complete diversion at the end of this section.

### 6.1. Trade diversion

To formally assess how total imports of the subject tires from China and the RoW are affected by the CTS, we again exploit a random growth model used in the previous section. As shown in Fig. 7, the subject tire imports were more rapidly increasing than the control tire imports before the CTS. This means that the safeguard measures might be imposed to tire products with high import growth rates. The random growth model deals with this selection bias.

In our DID design for the tariff effect on the subject tire imports, a natural control group would comprise the other 52 tire-related products that have not experienced any tariff change during the sample period. However, 13 products among 52 are subject to anti-dumping duties as noted in section 5.2. Also, some tire products are not imported for many years or their import values are highly volatile. After dropping such products from the control group, we have 33 control tire-related products versus 10 treated products.<sup>35</sup> Given these 43 product units in our sample, clustering standard errors at the product level is reasonably safe to avoid the over-rejection problem discussed in Bertrand et al. (2004). We confine our sample period from 2006Q4 to 2012Q3 so that three years before and after the treatment can be compared, though extending the sample period does not change our results qualitatively.

In the model, the treatment effect,  $\tau_j$ , is assumed to be heterogeneous across products but constant over time. Let the import value (or volume) of product  $j$  at time  $t$  (from either China or RoW),  $y_{jt}$ , be given by

$$y_{jt} = \exp(\delta_j + \lambda_t + \rho_j t + \tau_j D_{jt}) \epsilon_{jt} \quad (8)$$

where  $\delta_j$  and  $\lambda_t$  are product and time fixed effects, respectively,  $\rho_j t$  captures the product-specific (linear) growth rate, and  $\epsilon_{jt}$  is the idiosyncratic shock with zero mean. A typical estimation approach is to transform Eq. (8) into log-linear form to obtain the fixed effects (FE) estimator. However, Santos Silva and Tenreyro (2006) argue that the log-linear transformation can cause a bias due to

<sup>34</sup> Konings and Vandebussche (2005) empirically support this argument by showing that domestic firms do not change their mark-up when they experience a strong import diversion after their industry is protected by antidumping action.

<sup>35</sup> As emphasized in the main analysis, there is no clear criterion for selecting control unit. Our finding in this section is at least robust to the inclusion of the volatile products in the control group.

**Table 5**  
Impact of the U.S. tariffs on tire import flows.

Dep. variable	Import value	Quantity	Unit value
<b>Panel A: import from China</b>			
$\hat{\tau}$	−0.962** (0.110)	−0.709** (0.242)	0.126 (0.210)
% change	−62.03	−52.23	10.95
Observations	1032	1032	847
R <sup>2</sup>	0.973	0.948	0.665
<b>Panel B: import from RoW</b>			
$\hat{\tau}$	0.157** (0.059)	0.328** (0.105)	0.008 (0.229)
% change	16.84	38.09	−1.80
Observations	1032	1032	1026
R <sup>2</sup>	0.994	0.982	0.731
<b>Panel C: total import</b>			
$\hat{\tau}$	−0.082 (0.054)	0.007 (0.151)	0.201 (0.270)
% change	−8.01	−0.44	17.85
Observations	1032	1032	1027
R <sup>2</sup>	0.992	0.973	0.768

Notes: The sample includes 43 products with 24 quarter periods. All specifications include product-specific fixed effect and linear time trend, and time dummies. Robust standard errors for coefficients are clustered at product level in parentheses. Calculation of percentage changes is based on Kennedy (1981).  
\*\* The significance at the 1% level.

heteroskedasticity or zero trade values, and suggest a Poisson pseudo-maximum likelihood (PPML) estimator with the dependent variable in levels. Hence, we follow the PPML estimation method, although the FE estimates are not qualitatively different.

Estimation results are provided in the first two columns in Table 5. Since we have some zero trade values, the sample size is less than 1032 (= 43 × 24). Panel A shows the average treatment effect (ATE) on the subject Chinese tire imports, which is also called the trade destruction effect by Bown and Crowley (2007). Trade destruction is both statistically and economically significant: the estimates show that safeguard measures reduced subject tire imports from China by around 62% more than non-subject Chinese tire product imports in total value and 52% in quantity.

Panel B shows trade diversion effect by estimating the ATE on the subject tire import from the RoW. Trade diversion is also significant, with around a 17% increase in total value and a 38% increase in quantity. These increases are substantial, given that the total import value of subject tires from the RoW in the pre-intervention period are, on average, three-times that of China. To examine whether the trade diversion was actually complete, we estimate the ATE on the total U.S. import (including China) of subject tires (see Panel C). Statistically and economically insignificant estimates in Panel C imply that the total U.S. tire imports, whether they are measured by value or volume, are not affected by the CTS. Thus, we find that trade destruction is completely offset by trade diversion.

We look at how import unit values from China and the RoW change with the tariff in the last column. As Trefler (2004) notes, changes in unit values within an HS 10 digit are likely to reflect changes in prices. We use the same setup as Eq. (8) with import unit values as the dependent variable instead. The unit value is defined as the ratio of customs value to total quantity imported. Hence, it is the value prior to the import duty. The unit value of a tire product from the RoW is the weighted average of each country's product unit value with its import share being used as the weight. Panel A of the table estimates the ATE in unit values of the subject Chinese tire products. The estimated effect is statistically insignificant. This implies that the safeguard measures are mostly passed through, and it is consistent with the fact that the import destruction effect was substantial. Moreover, the estimation results for the RoW case in Panel B are also insignificant. These results together imply that the reduction in tire imports from China is completely offset by a rise in RoW tire imports at the pre-TTB unit price.

## 6.2. The role of MNCs

The potential mechanism described above implies that the labor market effect of a TTB would crucially depend on the degree to which an import diversion occurs. Although the existing literature has not provided a rigorous explanation for the degree of diversion, we can expect that factors such as the level of protection, industry structure, and substitutability between foreign and domestic goods would affect the magnitude of import diversion. In the CTS case, low substitutability between Chinese and the U.S. tires might stimulate the import diversion from China to other countries who produce similar quality tires. Also, as Konings et al. (2001) argue, high concentration of the subject tire market might increase the strategic rivalry which in turn offsets the effects of the safeguard measures.<sup>36</sup>

<sup>36</sup> Konings et al. (2001, p. 2945) discuss a couple of possible reasons why the import diversions in the European Union are generally weaker than in the U.S. The reasons include lower duty level, lower market concentration, higher uncertainty in decision making process, and more tariff-jumping FDI.

**Table 6**

Top 10 subject tire exporting countries to the U.S. by export percentage growth.

Country	Export to the U.S. (million\$)		Net Increase	% Growth
	Before CTS	After CTS		
Thailand	418	1457	1038.58	248.33
Indonesia	489	1220	731.08	149.53
Mexico	764	1544	780.16	102.18
South Korea	1941	3876	1935.49	99.73
U.K.	103	190	86.58	84.02
Taiwan	396	604	207.68	52.46
Germany	580	788	208.49	35.96
Canada	3481	4589	1107.73	31.82
Costa Rica	256	327	70.92	27.72
Brazil	672	840	167.77	24.96

Note: The total import volumes are calculated for 12 quarters before and after the CTS activation ranging from 2006Q4 to 2012Q3. Countries with export greater than hundred million dollars before the CTS activation are only listed.

In our view, however, a more crucial reason for the 'complete' diversion is that the world market for subject tire productions is dominated by MNCs. If there were no MNCs and the tires were produced entirely by local exporters, trade diversion would induce the U.S. importers to look for new exporters from other countries. Certainly, the frictions in replacing trade partners make trade diversion costly. Not only that, even if trade partners are replaced, the (new) local exporters might not be able to fully meet the domestic demand because of their physical capacity constraints (Ahn and McQuoid, 2013; Blum et al., 2013) or credit constraints (Chaney, 2013; Manova, 2013). On the other hand, MNCs who have multiple production facilities across countries can substantially reduce such frictions, since they can not only reallocate tire productions along their horizontal production chains to circumvent capacity constraint, but also use internal capital markets linked with their parent firms to mobilize additional funds in case of liquidity constraint.

Due to the lack of adequate data, we cannot formally test the hypothesis that trade diversion tends to be stronger in the prevalence of MNCs. However, anecdotal evidence combined with the U.S. import data corroborates our argument. Table 6 lists the top 10 subject tire exporting countries to the U.S. in order of export percentage growth. All of these countries have manufacturing facilities of the world's major tire MNCs. For example, Thailand, the highest ranked country in the table, has production facilities of large MNCs such as Bridgestone, Goodyear, Michelin, Sumitomo, and Yokohama. The Japanese business magazine, *Nikkei*, reports that Thailand has become a key export base for these MNCs after the CTS activation.<sup>37</sup> Indonesia has the subject tire plants of Bridgestone, Goodyear, and Sumitomo. Particularly, Bridgestone in Indonesia has expanded its production capacity to meet increased demands in 2010.<sup>38</sup>

South Korea has benefited the most, in terms of the dollar value of the net increase. There are two major MNCs headquartered in South Korea (Hankook and Kumho) which also have plants in China. These two MNCs shifted large shares of their productions from China to South Korea and other countries to circumvent the safeguard measures. Especially, Hankook Tire Co., the biggest foreign tire producers in China and the world's fastest-growing tire company, clearly reports that "the [America] regional headquarters diversified production sources to circumvent the additional 35 percent safeguard tariff on Chinese-made tires that was imposed from the fourth quarter of 2009." (Hankook Tire Annual Report *Hankook Tire (2010, p. 44)*).

In the case of Taiwan, *Asia Times (2011)* reports that Bridgestone Taiwan, which in the past did not export tires to the U.S., began to export one million tires to the U.S. in 2009 in response to the tariff imposed on China. Furthermore, Cooper, headquartered in Ohio, did not start sourcing tires from its U.S. plants to replace the Chinese imports. Instead, the company switched to its partners in Taiwan and South Korea to supply the U.S. market. These pieces of evidence altogether support that the discriminatory tariff induced MNCs to switch productions from China to other countries.

Finally, it is noteworthy to compare our findings to another safeguard protection case, the tariff on imports of heavy-weight motorcycles from Japan between 1983 and 1987. This case is often heralded as a great success of safeguard protection.<sup>39</sup> While the Japan safeguard is similar to the CTS in its temporary and discriminatory nature, there is a major difference between them: The major motorcycle companies at the time were not MNCs. Had Japanese or American (i.e., Harley-Davidson) firms been MNCs in the 1980s with plants outside the U.S. and Japan, our analysis suggests that the impact of the safeguard would have been much weaker.

<sup>37</sup> Article source: <http://www.thetruthaboutcars.com/2010/07/trade-war-watch-15-thai-tires-trump-chinese/>. The original article is available at [http://www.nikkei.com/article/DGXNASDD210AG\\_R20C10A7MM8000/](http://www.nikkei.com/article/DGXNASDD210AG_R20C10A7MM8000/)

<sup>38</sup> Article source: <http://www.bridgestone.com/corporate/news/2010051401.html>

<sup>39</sup> There is some controversy on whether the safeguard protection actually saved Harley-Davidson, the only heavyweight motorcycles maker in the U.S. at the time, but the safeguard surely gave some breathing room to Harley-Davidson on the brink of bankruptcy. See Feenstra (2004, Chapter 7) and Kitano and Ohashi (2009).

## 7. Concluding remarks

Two branches in the trade literature independently document that trade adjustment costs due to the globalization are significant and that TTBs have been progressively used across countries during periods of high unemployment rates. Our interpretation of these two phenomena is that temporary trade barriers are perceived as a feasible policy instrument for securing domestic jobs in the presence of increased globalization. Recent U.S. foreign trade policies are also in line with this interpretation. Particularly, during the recent presidential election in 2012, both candidates pledged stronger protection policies against China to save domestic jobs while citing the China-specific safeguard case on consumer tires as a successful example. This paper formally asks whether the CTS actually saved domestic jobs. Using the synthetic control method to estimate the impact of the CTS, we find that the U.S. tire industry experienced no gains in both employment and wages.

The negligible labor market effects are not surprising as further analysis reveals that imports from China were completely diverted to other exporting countries leaving the U.S. production unchanged. We also provide a potential reason for the complete import diversion. Since the world tire industry is dominated by a small number of multinational corporations with their own production and financial networks, the reallocation of production across countries is relatively frictionless. Since MNCs would diversify subject tire production to countries with a comparative advantage in producing similar quality tires, countries such as Thailand, Indonesia, South Korea, Mexico, and Taiwan became the predominant beneficiaries of the discriminatory tariff policy, but not the U.S. Although we provide anecdotal evidence for the crucial role that MNCs played in making the complete trade diversion possible, a more systematic analysis with adequate data is left for future work.

Our study predicts that other TTBs that bear similar characteristics to the CTS should have little impact on domestic labor markets in industries where MNCs are major players. This prediction is particularly important given the remarkable trend in recent years toward the proliferation of massively networked MNCs. Hence, negligible TTB effect should be more pronounced in the future and, accordingly, an optimal trade policy design must take the presence of MNCs into consideration.

## Acknowledgments

We deeply thank James Lake and Daniel Millimet for their constant feedback and discussions. We also thank the journal editor, Theo Eicher, and two anonymous referees for their excellent comments on the earlier version of the paper. We are grateful to Andrew Bernard, Meredith Crowley, Maggie Chen, Robert Feinberg, Marc Muendler, Thomas Prusa, Frederic Warzynski as well as conference and seminar participants at the Midwest International Trade Conference (Spring 2013), the 9th EITI Conference, KDI, Korea University, Seoul National University, and Southern Methodist University for helpful comments. All errors are ours.

## Appendix A. Supplementary data

Supplementary data associated with this paper can be found in the online version at <http://dx.doi.org/10.1016/j.euroecorev.2015.12.009>.

## References

- Abadie, A., Diamond, A., Hainmueller, J., 2010. Synthetic control methods for comparative case studies: estimating the effect of California's tobacco control program. *J. Am. Stat. Assoc.* 105, 493–505.
- Abadie, A., Gardeazabal, J., 2003. The economic costs of conflict: a case study of the Basque country. *Am. Econ. Rev.* 93, 113–132.
- Abowd, J.A., Lemieux, T., 1993. The effects of product market competition on collective bargaining agreements: the case of foreign competition in Canada. *Q. J. Econ.* 108, 983–1014.
- Ahn, J., McQuoid, A.F., 2013. Capacity Constrained Exporters: Micro Evidence and Macro Implications. Working Paper.
- Asia Times (2011). US Drivers Pay Steep Price for China Tire Tariff, Retrieved from ([http://www.atimes.com/atimes/China\\_Business/MI10Cb01.html](http://www.atimes.com/atimes/China_Business/MI10Cb01.html)) (September 10).
- Autor, D.H., Dorn, D., Hanson, G.H., 2013. The China syndrome: local labor market effects of import competition in the united states. *Am. Econ. Rev.* 103, 2121–2168.
- Autor, D.H., Dorn, D., Hanson, G.H., Song, J., 2014. Trade adjustment: worker-level evidence. *Q. J. Econ.* 129, 1799–1860.
- Bagwell, K., Staiger, R.W., 2003. Protection and the business cycle. *Adv. Econ. Anal. Policy*, 3.
- Bai, J., 2009. Panel data models with interactive fixed effects. *Econometrica* 77, 1229–1279.
- Belderbos, R., Vandenbussche, H., Veugelers, R., 2004. Antidumping duties, undertakings, and foreign direct investment in the EU. *Eur. Econ. Rev.* 48, 429–453.
- Bertrand, M., Duflo, E., Mullainathan, S., 2004. How much should we trust differences-in-differences estimates? *Q. J. Econ.* 119, 249–275.
- Blonigen, B.A., 2002. Tariff-jumping antidumping duties. *J. Int. Econ.* 57, 31–49.
- Blonigen, B.A., Haynes, S.E., 2002. Antidumping investigations and the pass-through of antidumping duties and exchange rates. *Am. Econ. Rev.* 92, 1044–1061.
- Blonigen, B.A., Park, J.-H., 2004. Dynamic pricing in the presence of antidumping policy: theory and Evidence. *Am. Econ. Rev.* 94, 134–154.
- Blonigen, B.A., Prusa, T.J., 2003. Antidumping. In: Choi, E.K., Harrigan, J. (Eds.), *Handbook of International*, Blackwell Publishing Ltd, Oxford, UK, pp. 251–284.
- Blum, B.S., Claro, S., Horstmann, I.J., 2013. Occasional and perennial exporters. *J. Int. Econ.* 90, 65–74.



- Bown, C.P., 2010. China's WTO entry: antidumping, safeguards, and dispute settlement. In: Feenstra, R.C., Wei, S.-J. (Eds.), *China's Growing Role in World Trade*, University of Chicago Press, Chicago, pp. 281–337.
- Bown, C.P., Crowley, M.A., 2007. Trade deflection and trade depression. *J. Int. Econ.* 72, 176–201.
- Bown, C.P., Crowley, M.A., 2013. Import protection, business cycles, and exchange rates: evidence from the great recession. *J. Int. Econ.* 90, 50–64.
- Bown, C.P., Crowley, M.A., 2014. Emerging economies, trade policy, and macroeconomic shocks. *J. Dev. Econ.* 111, 261–273.
- Bradford, S., 2006. Protection and unemployment. *J. Int. Econ.* 69, 257–271.
- Bussey, J., 2012. Get-tough policy on Chinese tires falls flat. *Wall Street J.* Retrieved from <http://online.wsj.com/article/SB10001424052970204301404577171130489514146.html> (January 20).
- Chaney, T., 2013. Liquidity Constrained Exporters NBER Working Paper 19170.
- Costinot, A., 2009. Jobs, jobs, jobs: a new perspective on protectionism. *J. Eur. Econ. Assoc.* 7, 1011–1041.
- Davidson, C., Matusz, S.J., Nelson, D., 2012. A behavioral model of unemployment, Sociotropic concerns, and the political economy of trade policy. *Econ. Polit.* 24, 72–94.
- Everaert, Gerdie, Tom De Groote, Tom De., 2016. Common correlated effects estimation of dynamic panels with cross-sectional dependence. *Economet. Rev.* 35 (3), 28–63. <http://dx.doi.org/10.1080/07474938.2014.966635>.
- Feenstra, R.C., 2004. *Advanced International Trade: Theory and Evidence*. MIT Press, Princeton.
- Gaston, N., Trefler, D., 1994. Protection, trade, and wages: evidence from U.S. manufacturing. *Ind. Labor Relat. Rev.* 47, 574–593.
- Gobillon, L., Magnac, T., Regional policy evaluation: interactive fixed effects and synthetic controls. *Rev. Econ. Stat.*, [http://dx.doi.org/10.1162/REST\\_a\\_00537](http://dx.doi.org/10.1162/REST_a_00537), in press.
- Goldberg, P.K., Pavcnik, N., 2005. Trade, wages, and the political economy of trade protection: evidence from the Colombian trade reforms. *J. Int. Econ.* 66, 75–105.
- Hankook Tire (2010). *Hankook Tire Annual Report 2010*, Hankook Tire Corporation.
- Hufbauer, G.C., Lowry, S., 2012. US Tire Tariffs: Saving Few Jobs at High Cost. Policy Brief 12-9. Peterson Institute for International Economics, Washington, D.C.
- Irwin, D.A., 2005. The rise of us anti-dumping activity in historical perspective. *World Econ.* 28, 651–668.
- Kennedy, P.E., 1981. Estimation with correctly interpreted dummy variables in semilogarithmic equations. *Am. Econ. Rev.* 71, 801.
- Kitano, T., Ohashi, H., 2009. Did US safeguards resuscitate Harley-Davidson in the 1980s? *J. Int. Econ.* 79, 186–197.
- Knetter, M.M., Prusa, T.J., 2003. Macroeconomic factors and antidumping filings: evidence from four countries. *J. Int. Econ.* 61, 1–17.
- Konings, J., Vandenbussche, H., 2005. Antidumping protection and markups of domestic firms. *J. Int. Econ.* 65, 151–165.
- Konings, J., Vandenbussche, H., 2008. Heterogeneous responses of firms to trade protection. *J. Int. Econ.* 76, 371–383.
- Konings, J., Vandenbussche, H., Springael, L., 2001. Import diversion under European antidumping policy. *J. Ind. Compet. Trade* 1, 283–299.
- Kovak, B.K., 2013. Regional effects of trade reform: what is the correct measure of liberalization?. *Am. Econ. Rev.* 103, 1960–1976.
- Manova, K., 2013. Credit constraints heterogeneous firms, and international trade. *Rev. Econ. Stud.* 80, 711–744.
- Matschke, X., Sherlund, S.M., 2006. Do labor issues matter in the determination of U.S. trade policy? An empirical reevaluation. *Am. Econ. Rev.* 96, 405–421.
- McLaren, J., Hakobyan, S., 2012. Looking for Local Labor Market Effects of NAFTA. Working Paper.
- Menezes-Filho, N.A., Muendler, M.-A., 2011. Labor Reallocation in Response to Trade Reform. NBER Working Paper 17372.
- Messerlin, P.A., 2004. China in the World Trade Organization: antidumping and safeguards. *World Bank Econ. Rev.* 18, 105–130.
- Moon, H.R., Weidner, M., 2014. Dynamic Linear Panel Regression Models with Interactive Fixed Effects. CEMMAP Working Paper 47/14.
- Moon, H.R., Weidner, M., 2015. Linear regression for panel with unknown number of factors as interactive fixed effects. *Econometrica* 83, 1543–1579.
- Pesaran, M.H., 2006. Estimation and inference in large heterogeneous panels with a multifactor error structure. *Econometrica* 74, 967–1012.
- Pierce, J.R., 2011. Plant-level responses to antidumping duties: evidence from U.S. manufacturers. *J. Int. Econ.* 85, 222–233.
- Pierce, J.R., Schott, P.K., 2012. The Surprisingly Swift Decline of U.S. Manufacturing Employment. NBER Working Paper 18655.
- Prusa, T.J., 1997. The trade effects of U.S. antidumping actions. In: Feenstra, R.C. (Ed.), *The Effects of U.S. Trade Protection and Promotion Policies*, University of Chicago Press, Chicago, pp. 191–214.
- Prusa, T.J., 2009. Estimated Economic Effects of the Proposed Import Tariff on Passenger Vehicle and Light Truck Tires from China. Technical Report. Retrieved from <http://www.cbe.csueastbay.edu/~alima/courses/4700/Articles/PrusaTireTariffAnalysis.pdf>.
- Prusa, T.J., 2011. USA: evolving trends in temporary trade barriers. In: C.P. Bown (Ed.), *The Great Recession and Import Protection: The Role of Temporary Trade Barriers*. Centre for Economic Policy Research, London, pp. 53–83.
- Rapoza, K., 2012. Obama's half-truth On China tire tariffs, *Forbes*. Retrieved from <http://www.forbes.com/sites/kenrapoza/2012/01/25/obamas-half-truth-on-china-tire-tariffs/> (January 25).
- Revenge, A.L., 1997. Employment and wage effects of trade liberalization: the case of Mexican manufacturing. *J. Labor Econ.* 15, S20–S43.
- Santos Silva, J.M.C., Tenreyro, S., 2006. The log of gravity. *Rev. Econ. Stat.* 88, 641–658.
- Staiger, R.W., Wolak, F.A., 1992. The effect of domestic antidumping law in the presence of foreign monopoly. *J. Int. Econ.* 32, 265–287.
- Staiger, R.W., Wolak, F.A., 1992. Measuring industry-specific protection: antidumping in the United States. *Brookings Pap. Econ. Act. Microecon.* 1994, 51–118.
- Topalova, P., 2010. Factor immobility and regional impacts of trade liberalization: evidence on poverty from India. *Am. Econ. J. Appl. Econ.* 2, 1–41.
- Trefler, D., 2004. The long and short of the Canada–U.S. free trade agreement. *Am. Econ. Rev.* 94, 870–895.
- Wallerstein, M., 1987. Unemployment collective bargaining, and the demand for protection. *Am. J. Polit. Sci.* 31, 729–752.