Impacts of antidumping policies on Colombian domestic protected industries

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Abstract

While the impacts of antidumping policies on trade have been widely studied, relatively little has been done on the implications for labor markets, even though job protection often motivates protection. This paper fills the gap by focusing on impacts to domestic firm employment and wages. I use synthetic control groups constructed as a weighted average of unaffected industries reflecting similar pre-treatment trends of industries receiving protection to identify the effect of antidumping measures, and use placebo checks to create a finite sample distribution for inference testing. I find that only one of the eight industries receiving antidumping protection experiences around 15% higher employment than matched counterfactual control groups. The remaining seven industries saw little effect in employment relative to their control groups. In terms of average annual wages for workers, one quarter of the protected industries experienced wages 10-20% lower than their control groups. These indicate heterogeneous effects across sectors: for reasons left to future research, very few industries show improvement from antidumping policies, while most see little significant effects.

1 Introduction

Research shows antidumping policies have a chilling effect on trade through a multitude of channels. Numerous associated costs include the possibility of retribution (Egger & Nelson, 2011), distorted allocation of resources, lower aggregate trade (Prusa, 2001), or diverted trade (Prusa, 1997). Despite this wide range of mechanisms, the impact on domestic industries has not received the same amount of attention in economic literature. Antidumping policies can be politically popular because of their protectionist nature. Given that import-competing firms or trade associations initiate antidumping petitions on the basis of leveling the playing field, we need to better understand their domestic impact. This paper examines domestic labor market implications of successful antidumping applications. Specifically, in Colombia, antidumping protections do not support labor markets: out of ten industries receiving antidumping protection between 2004 and 2008, a positive treatment effect can only be found in one industry, which saw higher employment levels. Two other industries continued to experience lower wages and fewer workers even after protection for a statistically significant negative treatment effect.

Table 1 summarizes the results of this paper, which show that antidumping protection is not generally effective at stabilizing employment or average wages. Only one ISIC industry (1721 non-apparel textile manufacturing) experienced a statistically significant increase in employment over 2000 levels. Employment increased on average by 16% more than a comparable control industry. Another textile industry as well as a hardware industry (1711 Preparation and spinning of textiles and 2893 manufacture of cutlery, hand tools and general hardware) experienced a decline in wages relative to counterfactual control groups. Other treated industries have additional treatment effects that are not statistically significant. For those industries, negative treatment effects are more prevalent, suggesting that antidumping protection was unable to effectively stabilize, much less improve upon, the negative effects felt by the workers. The results shown here also strongly suggest heterogeneity across industries. Future work should explore if there is a systematic reason some industries have

Year	ISIC Industry	Employment	Average wage
2004	2520	9.16%	0.25%
2004	2691	-8.49%	-4.10%
2005	2520	-4.62%	-2.62%
2006	1711	-19.47%	-19.96%**
2006	1721	16.18%***	-16.87%
2006	1730	-12.19%	3.71%
2006	1810	-52.52%	5.97%
2006	2899	-5.09%	-3.12%
2008	2893	-4.56%	-8.57%*
2008	2899	-0.21%	-11.21%

Table 1: Average treatment effect over five years after the treatment year (year of petition initiation) for employment (number of workers) and average wages. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

achieved positive results while others have not.

Before turning to the literature and methodology, I first describe the motivations and process of antidumping in Colombia using recent examples. In October 2016, Colombia's steel associations joined with other regional associations in pressing governments at the Ibero-American Summit to not grant China market economy status due to competitive advantages enjoyed by Chinese steel producers. Market economy status, *inter alia*, would make it more difficult to enact temporary trade barriers against China. The producers state "We are united by a common concern: our fears for the loss of jobs and its impact on our communities, the closure of companies, the lack of incentives for investment, financial losses and the destruction of our value chain.... In short, China exports unemployment." The Colombian manufacturing industry shares this sentiment. China, the most frequent target of Colombian antidumping investigations, exports at prices so low that even MFN maximum tariffs have little effect. In 2015, footwear from China, priced as low as $0.50USD faced a maximum 15% tariff. Domestic producers sold shoes for $5.50USD on average. The industry filed an antidumping petition for additional protection from injury. Six months after the *Asociación Colombiana de Industriales del Calzado, el Cuero y sus Manufacturas* - the

Colombian Association of Footwear, Leather, and Leather Products Industries, ACICAM-filed the complaint, the Ministry of Commerce, Industry and Tourism, the government body responsible for antidumping investigations, began their preliminary investigation, issuing a preliminary decision three months later, in September 2015. The investigators determined that they would not place a preliminary dumping remedy on imported Chinese shoes, although they could have done so had they felt it was warranted. The investigation continued until July 2016 when it was terminated without a final determination or duties.

Officially, "injury" is determined in Colombian antidumping investigations through effects of imports on several variables, including employment and wages.[1] In 2016, the Ministry of Commerce, Industry and Tourism, concluded dumping occurred in the aforementioned footwear case; however, dumping did not *cause* injury to the domestic industry, so antidumping measures were not put in place.[2] If causality is in fact established by investigators, attributing lost employment and wages to less-than-fairly priced imports, then we would expect antidumping remedies to remove the source of injury and improve or stabilize labor markets in the affected industries.

There are few existing studies on the domestic effects of antidumping protection. Konings and Vandenbussche (2008), Pierce (2011), and Chung, Lee, and Osang (2016) address the effects on firm productivity and employment.

Konings and Vandenbussche (2008) ask how antidumping protection affects productivity of European firms, and how the effect depends upon firm heterogeneity. Using a difference in differences model, the average firm receiving protection sees a moderate improvement in productivity, measured by total factor productivity as estimated through the (Olley &

[1]Committee on Anti-Dumping Practices (1995, Chapter V Article 13.1.2) states "Effects of imports on trends in the domestic industry with respect to factors such as, *inter alia*, prices, output, market share, profits, utilization of installed capacity, inventories, sales, ability to raise capital or investment, employment and wages."

[2]The various tests to establish causality included the response of prices when Chinese import volumes fluctuated, and what happened when domestically produced shoe prices fluctuated. As imports from China fell, the price of certain types of shoes remained stable. Additionally, domestic producers were able to raise their own prices and increase their market share (El Director de Comercio Exterior, 2016). Therefore, the agency concluded that the price of Chinese shoes were not responsible for the decline of the domestic producers.

Pakes, 1996) methodology. However, the productivity improvement is not sufficient to bring the firm to the same productivity as the average firm in a similar unprotected industry. Furthermore, "frontier firms" experience productivity losses due to protection. Pierce (2011) builds on these results. When productivity is based on revenue (value-added, as it is in Konings and Vandenbussche (2008)), a difference in differences analysis of US data shows moderately increased productivity, similar to Konings and Vandenbussche (2008) at about $3-8\%$ improvement, from protection. If, instead, productivity is quantity-based, we see decreases in productivity from protection, suggesting that protected firms are increasing prices and markups to raise revenue, but reducing output.

More surprising is the one other paper addressing the impact of temporary trade barriers on labor. As with the steel associations above, advocates for antidumping policies frame them as protecting domestic workers. Unfortunately, this seems to have been neglected in the research. The closest paper is Chung et al. (2016): using a China-specific safeguard case initiated by the US in the tire industry, the authors generated a synthetic control group and used a difference in differences model to find that total employment and average wages in the industry were unaffected by the safeguard. Intuitively, the higher tariff on Chinese tires reduced Chinese exports to the US and diverted them elsewhere, opening the door for other countries to export more tires to the US. Thus, the relief from fewer Chinese tire imports was eliminated by third-party countries, essentially leaving the US domestic tire industry no better off relative to a similar synthetic industry.

This paper will be similar to Chung et al. (2016) in that it focuses on the impact of antidumping measures on Colombian manufacturing labor markets. By using a relatively small economy, compared to the US and EU in previous studies, it is possible that some diversion effects may be mitigated. Since Colombia is not a major trade partner with larger economies like the US, EU, and China, even though the US and China are major partners for Colombia, it is possible that reducing trade flows from one country may not necessarily result in large increases of goods from other countries. Colombia's economy also provides a unique historical context due to its long civil war. The civil war reduced certainty, slowing economic growth in the country, despite early attempts at liberalization. It was not until the government took a more active role in guaranteeing and promoting security that trade took off. Around 2000, Colombia saw significant growth in exports and imports wioth improved security. Government institutions were in place to transparently record data with this growth in the economy.

In addition to impeding economic growth, the civil war reduced certain labor market frictions. Labor unions during the period I studied were nearly non-existent as unions were caught in the middle of the conflict. In 2006, less than 5% of Colombia's labor force belonged to a union (Bureau of Democracy, Human Rights, and Labor, 2006). By 2013, the number rose to 6.5%, as estimated by the Greater Integrated Household Survey administered by the Colombian government. Violence against union leaders by the government and paramilitary groups contributes to low union membership. While it has since improved, Colombia was considered one of the worst countries in the world for unionists not too long ago- more than half of the worldwide murders of unionists (78 out of 144) in the year 2007 were committed in Colombia (*New ITUC Worldwide Report Reveals Catalogue of Murder, Violence and Intimidation Against Trade Unionists*, September 18, 2007). Despite recent improvements in workers' rights partially due to pressure from the United States before signing a trade agreement and ceasefires between the government and rebel groups, this paper studies the period of time when workers were generally not unionized. Even in instances where unions could form, these typically were concentrated in the agriculture or mining industries, not the manufacturing industry. Hence, wage negotiations can be ignored, unlike in Chung et al. (2016). Since they study the US market, they needed to take into account union wage negotiations that occurred before firms made employment decisions based on output, cost of capital, cost of materials, negotiated wages, and import penetration.

The paper proceeds as follows: Section 2 discusses the methodolgy, describing the synthetic control method and computed statistics, followed by a description of the data in

Section 3. Finally, Section 4 presents results and discusses inference testing with multiple treatments before concluding in Section 5.

2 Methodology

I use quasi-experimental methods to analyze the treatment effect of successful antidumping petitions (defined here as resulting in a dumping remedy being implemented) on employment and wages in Colombian manufacturing industries. Typically, product-level antidumping empirical work identifies treatment effects by constructing control groups of similar product categories to those that received treatment, but did not themselves receive treatment (Blonigen & Park, 2004; Lu, Tao, & Zhang, 2013). The question then becomes how to define similar products. Three of the more common definitions are: (i) products where antidumping petitions were filed but rejected; (ii) products with the same first HS 4 digit classification as an HS 6 digit product that receives treatment; and (iii) products that are "likely" to receive treatment but did not (measured with a propensity score). Because antidumping treatment is endogenous, selection of an "ideal" control group is not clear- each of these control selection methods may be problematic. The first one- choosing similarly classified products- does not guarantee related products experience pre-treatment trends similar to treated products. The second and third control group selection methods take previous trends into account- likely petitioners and failed petitioners should have the same pre-treatment trend as the successful petitioners. However, in the case of Colombia, most failed petitions are successfully refiled shortly after failing, or are filed through another protectionist avenue, such as China-specific safeguards. Antidumping petitions are also concentrated in a few industries. When predicting which products are likely to file petitions, product classifications drive the propensity scores, and the resulting control group is not guaranteed to be a convincing match for the treated group.

To address these concerns, I use the synthetic control procedure from Abadie, Diamond, and Hainmueller (2010). The synthetic control method identifies a weighted average of the potential donor industries that could contribute to a control industry. I exclude industries that either have petitioned for antidumping protection in the past and resulted in no duties, or will petition before 2015 to eliminate potential confounding investigation effects as Staiger and Wolak (1994) find much of the impact occurs during the investigation. The weights in the weighted average minimize the distance between pre-intervention characteristics of the treated and untreated groups, yielding similar pre-treatment trends. Once an appropriate distance-minimizing vector has been determined, the treatment effect is the difference between the outcome in the treated group and the synthetic control group generated by the weights post-treatment. This treatment effect can vary over time, unlike a standard difference in differences approach. The temporary nature of protection means that the effects would be expected to change over time. I use dynamic effects to consider the persistance of antidumping remedies.

The benefit of using synthetic control groups lies in the artificial construction of the control group. By combining a number of industries, the control groups dilute potential unobservable variables that might affect employment, such as other forms of political protection aside from dumping remedies industries may receive over the same time period. Since antidumping protection is given to industries that have experienced harm, one would reasonably expect those industries may be in a period of decline before filing a petition. Thus, the synthetic control group would also experience a similar decline by construction. If antidumping protection is helpful to the harmed industry in terms of employment, then one would expect to see a difference between the treatment and control group. If no difference exists, then the treated group is on a simliar path as before receiving treatment, and there is no apparent effect of protection on the labor market.

The regressions used to fit the control group and determine treatment effects are, as in

Chung et al. (2016):

$$\Delta n_{it} = \beta_0 + \beta_1 \Delta y_{it} + \beta_2 \Delta a_{it} + \beta_3 \Delta m_{it} + \beta_4 \Delta ip_{it} + u_{it} \tag{1}$$

$$\Delta w_{it} = \beta_0 + \beta_1 \Delta y_{it} + \beta_2 \Delta a_{it} + \beta_3 \Delta m_{it} + \beta_4 \Delta ip_{it} + u_{it} \tag{2}$$

where all variables are measured as the log change since 2000, i is an ISIC four digit industry, and t is the year. Equations (1) and (2) state that the changes in employment (number of workers) and wages since 2000 depend on output, inputs, and import competition: the change in gross production in the industry, y_{it}; the change in the industry's total assets (investment), a_{it}; the change in raw materials used, m_{it}; and the change in import penetration, ip_{it}.

The basic method of Abadie et al. (2010) only allows one treatment group. Researchers cope by studying only one affected industry as in Chung et al. (2016), or to aggregating affected industries into a single treated unit. However, heterogeneous effects across industries would be lost with these two methods. Dube and Zipperer (2015) extend Abadie et al. (2010) to permit inference on multiple treated units. Each of their treatment groups corresponds to a state raising their minimum wage. Thus, each treatment unit is thought of as an event, $e = 1, ..., E$ ($E = 1$ corresponds to Abadie et al. (2010)). For each event, a placebo test is conducted as in Abadie et al. (2010). A placebo test finds the "treatment effect" in each non-treated manufacturing industry in Colombia as though it were the recipient of antidumping protection, thereby testing if the actual identified treatment effect was likely to have occurred by chance.

Using the treatment effects from the treatment events and placebo tests, I compute two statistics following Dube and Zipperer (2015) for inference testing. First, I construct an average treatment effect for each treatment event and placebo check. Then, I standardize the treatment effects into elasticities based on the dumping margin: the more severe the margin, the greater the expected treatment effect. To compare across treatment events, the effects should be normalized. Thinking about the treatment effect as an elasticity (a 1%

change in the intensity of treatment is associated with some percentage change in treatment effect) allows the appropriate comparisons.

The first statistic, β_{ie}, measures the percent difference between a treated industry and the synthetic industry, averaged over four years. If a petition is initiated in year t', then β_{ie} is the average treatment effect over the four years following treatment divided by the absolute value of the average synthetic group over that same period.

$$\hat{\beta}_{ie} = \frac{\frac{1}{4}\sum_{t=t'+1}^{t'+4}(Y_{it} - \sum_j w_j^* Y_{jt})}{\left|\frac{1}{4}\sum_{t=t'+1}^{t'+4}\sum_j w_j^* Y_{jt}\right|} \tag{3}$$

where Y_{it} is the outcome variable (either employment or average wage) for industry i in time t, e is the event, w_j^* is the weight of industry j in the synthetic control group. For simplicity, let $i = 1$ represent the treated industry in each event $e = 1, ..., E$. Then, $\hat{\beta}_{1e}$ is the average percent gap between the treated group and the synthetic control group for event e. $\hat{\beta}_{ie}$ when $i \neq 1$ represents the same statistic for placebo checks. If $\hat{\beta}_{1e} < 0$, then the treated industry experienced a decline relative to the synthetic control group; if $\hat{\beta}_{1e} > 0$, then the treated industry experienced growth relative to the synthetic control group. The larger $\left|\hat{\beta}_{1e}\right|$ is, the greater the difference between the synthetic group and the treated group.

To account for heterogeneous treatment intensities, I weight each $\hat{\beta}_{ie}$ by the dumping margin, DM_e for event e (including the placebo tests). The dumping margin indicates how different the price of the import is compared to the price from import-competing firms and is given as a percent. A larger dumping margin means a greater difference in price. Using it to weight $\hat{\beta}_{ie}$ generates an elasticity:

$$\hat{\eta}_{ie} = \frac{\hat{\beta}_{ie}}{DM_e} \tag{4}$$

If η_{1e} falls within the tails of the distribution of placebo tests, then the null hypothesis of zero treatment effect for that event can be rejected. If η_{1e} falls in the middle of the distribution, then there is not sufficient evidence to reject the null hypothesis. Thus, something needs to be said about the unknown underlying distribution of η.

While the exact distribution is unknown and the asymptotic distribution is not necessarily normal, Dube and Zipperer (2015) exploit ranking percentiles to generate a uniform distribution for hypothesis testing. For each event, let $\hat{F}_e$ be the empirical cumulative distribution function of elasticities $\hat{\eta}_{ie}$. The percentile rank statistic is

$$p_{1e} = \hat{F}_e(\eta_{1e}) = \frac{r_{1e}}{(N_e + 1)} \tag{5}$$

where r_{1e} is the ranked position of the elasticity of the treated industry, and N_e is the number of industries included in the donor pool for event e. Hence, I take the unknown distribution of η and consider a *ranking* of the observed values. If the ranked position is in the tails, then the null hypothesis is rejected. In other words, if $p_{1e} < 0.025$ or $p_{1e} > 0.975$, I reject the null hypothesis of $\eta_{1e} = 0$ at the five percent significance level.

While the above procedure performs inference testing on single applications of antidumping policies, I am also interested in the sharp null hypothesis of zero effect everywhere. Theoretically, if the sharp null holds, the percentile ranks p_{1e} should have a uniform distribution over the interval from 0 to 1. Actual percentile ranks that significantly differ from the theoretical distribution provide evidence against the sharp null hypothesis. This may happen if there is heterogeneity of the treatment effects- for example, if one group of industries experiences negative effects while another group experiences positive effects of treatment. It may instead indicate that all industries experience the same, non-zero treatment effect. Heterogeneous effects in this case can still be tested for by centering the distribution of percentile ranks around the mean and repeating the test of the equivalence of the empirical distribution and the theoretical distribution.

The synthetic control method has been applied to only a handful of international trade papers before. In addition to Chung et al. (2016), Billmeier and Nannicini (2013) use the method to assess the effect of economic liberalization on GDP trajectories, finding a generally positive impact, although in more recent liberalizations in Africa, the impact was negligent. Hosny (2012) applies the synthetic control method to estimate the impact of Algeria waiting 7 years before joining the Greater Arab Free Trade Area, and finds Algeria's trade with other Arab countries would have benefitted had it not delayed joining the FTA. In each of these cases, the synthetic control method tests the aggregate effects of one instance of endogenous treatment. The additional application of Dube and Zipperer (2015) here allows for a broader analysis of effects of international policy in Colombia.

3 Data

3.1 Sources

The data comes from a variety of sources. Chad Bown's Global Temporary Trade Database collects information about antidumping cases, including the initiation of antidumping cases, the outcomes (affirmative- duty, affirmative- no duty due to unestablished causality, negative, and withdrawn), the products involved in cases, and the petitioning firms. The products, given in HS codes, were matched to their corresponding ISIC Revision 3 four-digit industry. Notably, comparing the product industries to the industry where the petitioning firm operates revealed that not all firms consider their primary industry the same as that of the products identified in cases.[3]

The remaining Colombia-specific data was collected by the Colombian government and accessed either through the *Departamento Administrativo Nacional de Estadística* (DANE) or the *Banco de la República*. DANE conducts annual surveys of firms in the manufacturing sector (the *Encuesta Anual Manufacturera*, EAM). The EAM covers firms with more than

[3] For example the products in Case 32, conventional tires (for cars, buses, or trucks) fall under the ISIC industry "Manufacture of rubber tires and tubes; retreading and rebuilding of rubber tires" (code D2511). However, the petitioning firm, Indústria Colombiana de Lllantas, identifies its principle operating industry as "Sale of motor vehicle parts and accessories" (G5030). To account for this discrepancy, I consider Case 32 as affecting both industries and add variables across the two where applicable.

10 employees or with production levels above a certain fluctuating threshold. In 2007, this threshold was 120 million Colombian pesos (approximately $\$60,000$ in 2010 USD), while in 2006, it was 121.7 million pesos. The data is collected at the firm level, but publicly released at the industry level for privacy concerns. The data, available from 1995 through 2014, is reported at the four-digit ISIC (Colombian aggregation) level or the three digit ISIC level, with additional sub-categories, such as the region of the country or size of the firms.[4] The ISIC has also undergone multiple revisions from 1995 to 2014. Most of the data I obtained is reported in ISIC Revision 2 industries through 1999, switching to ISIC Revision 3 through 2012, then finally to ISIC Revision 4 (skipping Revision 3.1). To avoid problems associated with concording industries through the years, I only use data from 2000 to 2012 with the ISIC Revision 3 industry codes. Thus, the manufacturing industry corresponds to ISIC Revision 3 two digit codes D15 through D37 (manufacturing). The EAM survey provides detailed accounts of the industries: from the number of firms within the 4-digit ISIC code, the number of employees (can be disaggregated into paid or unpaid, permanent or temporary, type of contract, or even the number of employees that are related to the owners), the compensation of employees (wages and benefits), gross production, intermediate goods used (including energy, land, materials, etc), value added, net investment, and total assets. Monetary values are measured in thousands of current-priced pesos. I deflate the values using a manufacturing industry specific producer price index compiled by DANE.

Colombian import data was collected from the Comtrade database and product values concorded into ISIC Revision 3 industries in constant Colombian pesos to match the domestic data. Then, import penetration is defined as the ratio of imports to domestic production plus imports in each industry.

[4]The Colombian aggregation of the ISIC industries is generally similar to the international ISIC version, but some of the more important Colombian industries, such as the production of coffee, have their own four-digit industry. Since I also use data reported in the ISIC international aggregation and multiple Colombian industries may map into a single international industry, but not vice versa, I use the international ISIC codes. Where needed, I concord the Colombian industries into the appropriate international industry classifications.

Table 2: Colombian Antidumping Cases Initiated Between January 1, 2004 and December 31, 2008, Resulting in a Duty

Case Number	**Country Named**	**Product Description**	**Product Industry**	**Initiation Date**	**Final Date**	**Duty**
46	CHN	Ceramic Tableware	D2691, J6599, J6719	2/2004	11/2004	Diff btwn US$0.84/kg and price
47	CHN	Porcelain Tableware	D2520, D2691	2/2004	11/2004	Missing
49	CHN	Balls (except Golf, Table Tennis, or Inflatable)	D2520, D3693	5/2005	3/2006	Diff btwn US$0.46/kg and price
50	CHN	Chain Links (polished or galvanized)	D2899	6/2006	4/2007	Diff btwn US $1.82/kg and price
51	CHN	Socks	D1730, D1810	7/2006	5/2007	Diff btwn US$0.79/pair and price
52	CHN	Textiles- Blended Fabrics	D1711	7/2006	7/2007	Diff btwn US $0.75/sq meter and price
55	CHN	Textiles- Curtains	D1721	7/2006	7/2007	Diff btwn US$6.74/kg and price
56	CHN	Textiles- Bed Linen	D1721	7/2006	10/2007	Diff btwn US$6.64/unit and price
57	CHN	Textiles- Table Linen	D1711, D1721	7/2006	10/2007	Diff btwn US$5.29/unit and price
62	CHN	Textiles- Towels	D1711	7/2006	5/2007	Diff btwn US$5.13/kg and price
65	CHN	Bolts	D2893, D2899, G5143, G5234, J6599	6/2008	2/2009	Diff btwn US$1.31/kg and price
67	CHN	Staples in Strips	D2899	6/2008	4/2009	Diff btwn US$1.66/kg and price
70	CHN	Hoes/ Digging Bars/ Picks	D2893	9/2008	6/2009	Diff btwn US$1.32/kg and price

3.2 Antidumping Cases

Antidumping cases will be used as the events e defined in Section 2. Over the course of the years 2004-2008, Colombian groups initiated 26 antidumping cases, half of which resulted in price undertakings, where the firm exporting to Colombia pays the difference between the price they charge and a base price. I restrict my attention to the 13 cases that resulted in measures. In generating control groups to compare treatment groups (industries with a case resulting in duties), I exclude from the potential donor pool any industry that petitioned for protection, had the legal body determine imports were being sold at less-than-fair-value, but ultimately determined that the dumping prices were not harming the domestic Colombian industry. I exclude these industries because of potential effects from the *threat* of antidumping protection. As shown in Prusa (2001) and Staiger and Wolak (1994), much of the effects on trade of dumping policies occur during the initial phase. I want to focus on the longer term effects, so I only consider industries where a final price undertaking was put in place. Table 2 summarizes the included antidumping cases.

Colombian industries file petitions in bursts. Of the four years where petitions were successful, 2004, 2005, 2006 and 2008, each year corresponds to a different industry: ceramic and porcelain tableware; inflatable plastic balls; textiles; and nuts, hoes, and picks ("other" metal tools industry), respectively. In the case of textiles and metal tools, manufacturing associations called *gremios* organized the petitions along with a few major firms, which is likely why, especially for textiles, many petitions for different related products were all filed around the same time.

4 Results

4.1 Impacts on Industries with Successful Petitions

To determine the impact on employment and wages in industries with successful antidumping petitions (ie, those that resulted in remedies), I use the synthetic control method as described previously. The employment variable used in the synthetic control method is defined as

$$\log\left(\frac{Employment_t}{Employment_{2000}}\right)$$

where $Employment_t$ is the number of employees within the industry in the year t and $Employment_{2000}$ is the number of employees in the base year 2000. Thus, I am measuring the employment growth of these industries. Similarly, the wage per person variable is defined as

$$\log\left(\frac{(TotalWages_t)/(Employment_t)}{(TotalWages_{2000})/(Employment_{2000})}\right)$$

I obtained similar results using data on total wages or benefits paid instead of average wages. Because of their similarities, those results are omitted.

Figures 1 and 2 show the results of the synthetic control method analysis of employment and average wages. Vertical lines indicate the treatment year (initiation of an ultimately successful petition). Solid lines represent the treated industry in each event, dashed lines represent the synthetic control group (constructed as weighted averages of other industries), and dash-dotted lines show the manufacturing sector average. Post treatment, the difference between the treated average and the synthetic average is the treatment effect, which can change through time. Figures 3 and 4 plot the treatment effects. In terms of employment, the graphs show heterogeneous effects of antidumping policies. Some industries experience an increase in employment, then a fall relative to their control group; other industries experience an initial decline then an increase in employment; still others experience only a decline or only an increase in employment. Some of these changes are small, on the order of less than

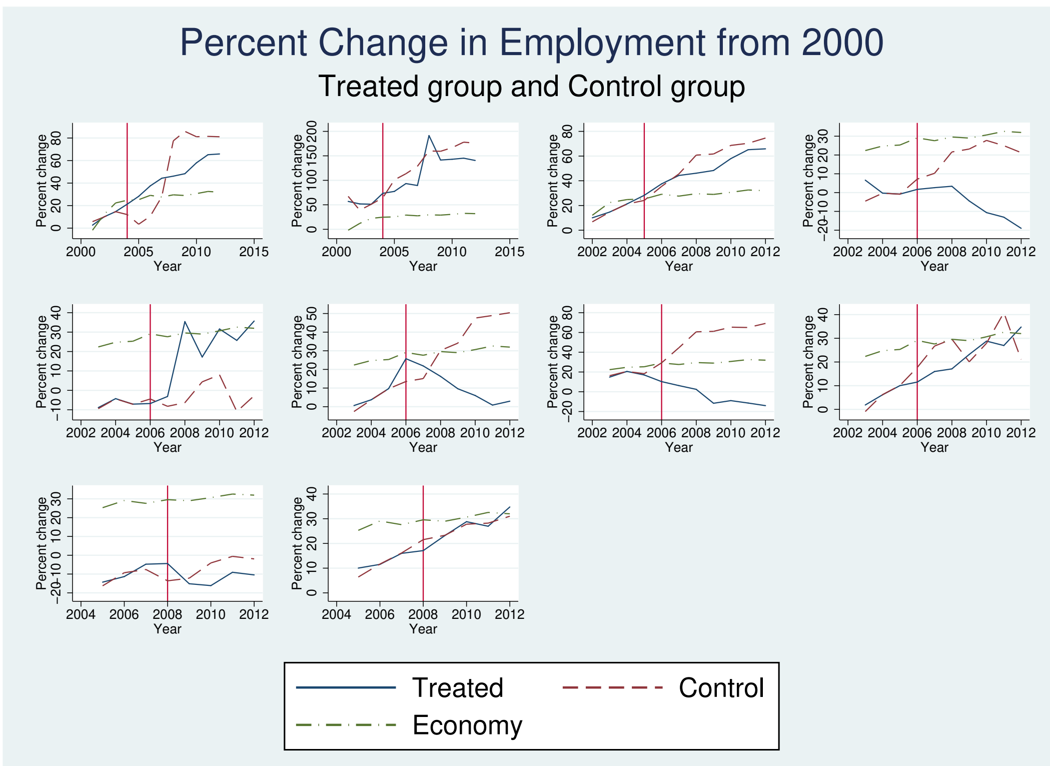

Figure 1: Comparison of the Treated Group and Control Group on Employment. Vertical lines indicate treatment year. Vertical axis measures the percent change in employment from 2000 for the Treated group, Control group, and the aggregate change in all of the manufacturing sector.

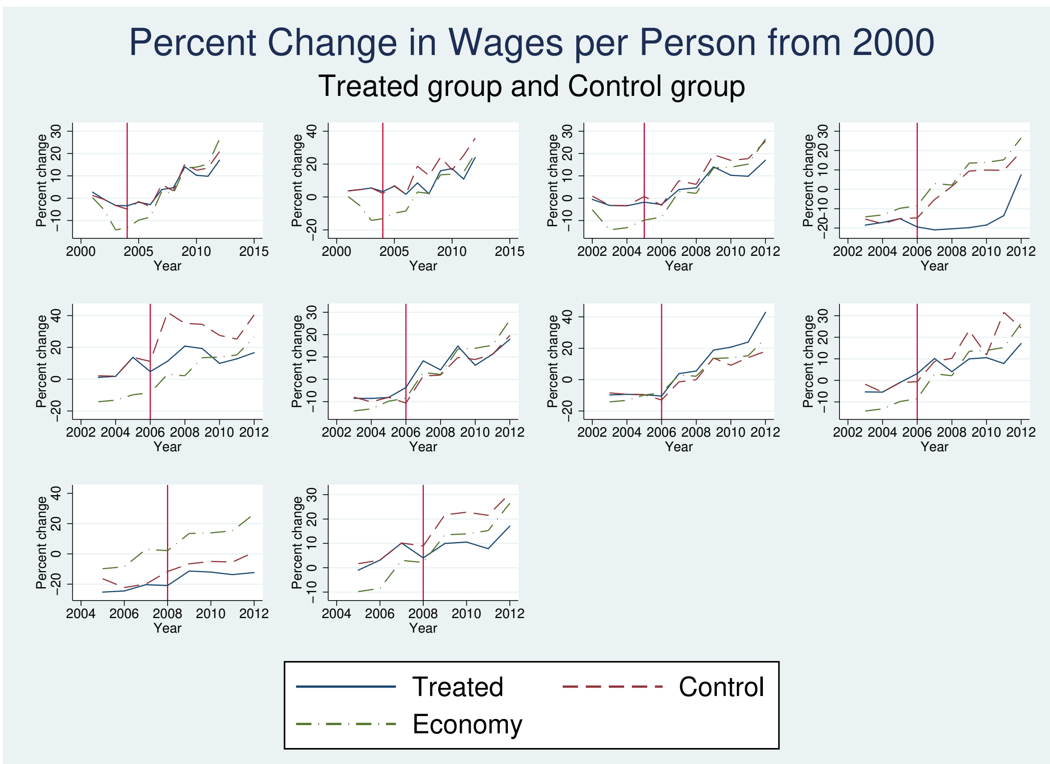

Figure 2: Comparison of the Treated Group and Control Group on per person wages. Vertical lines indicate treatment year. Vertical axis measures the percent change in average wages from 2000 for the Treated group, Control group, and the aggregate change in all of the manufacturing sector.

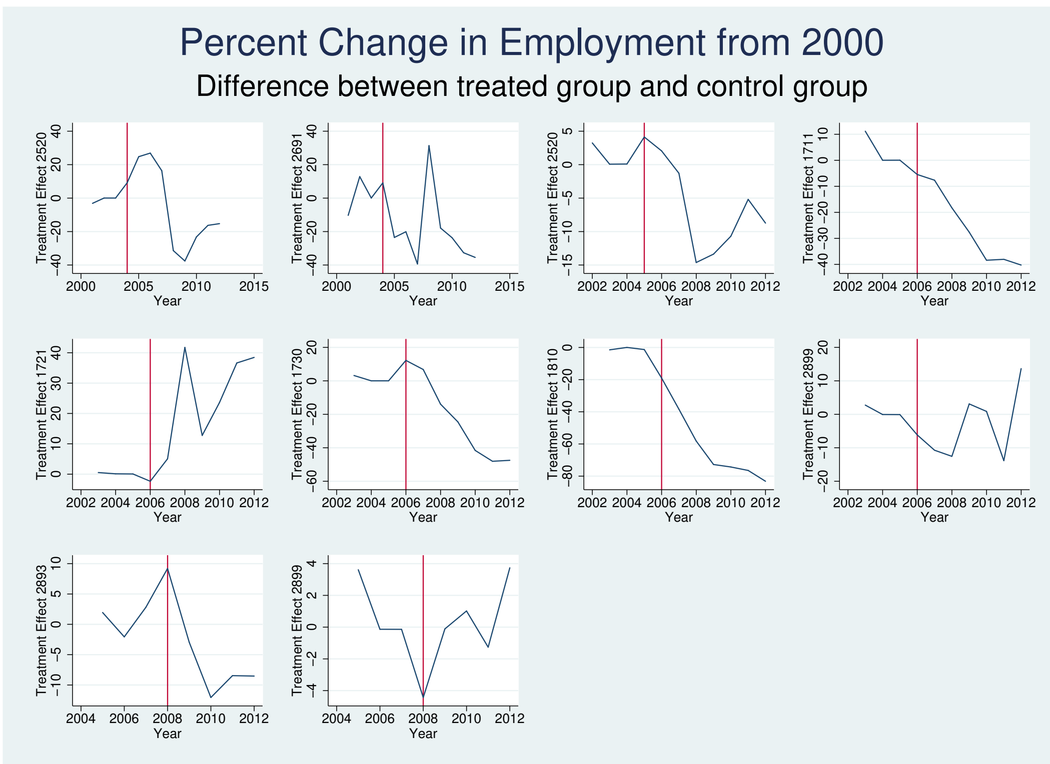

Figure 3: Treatment Effect on Employment. Vertical lines indicate treatment year. Vertical axis measures the difference in the percent change in employment from 2000 between the treated group and the synthetic control group.

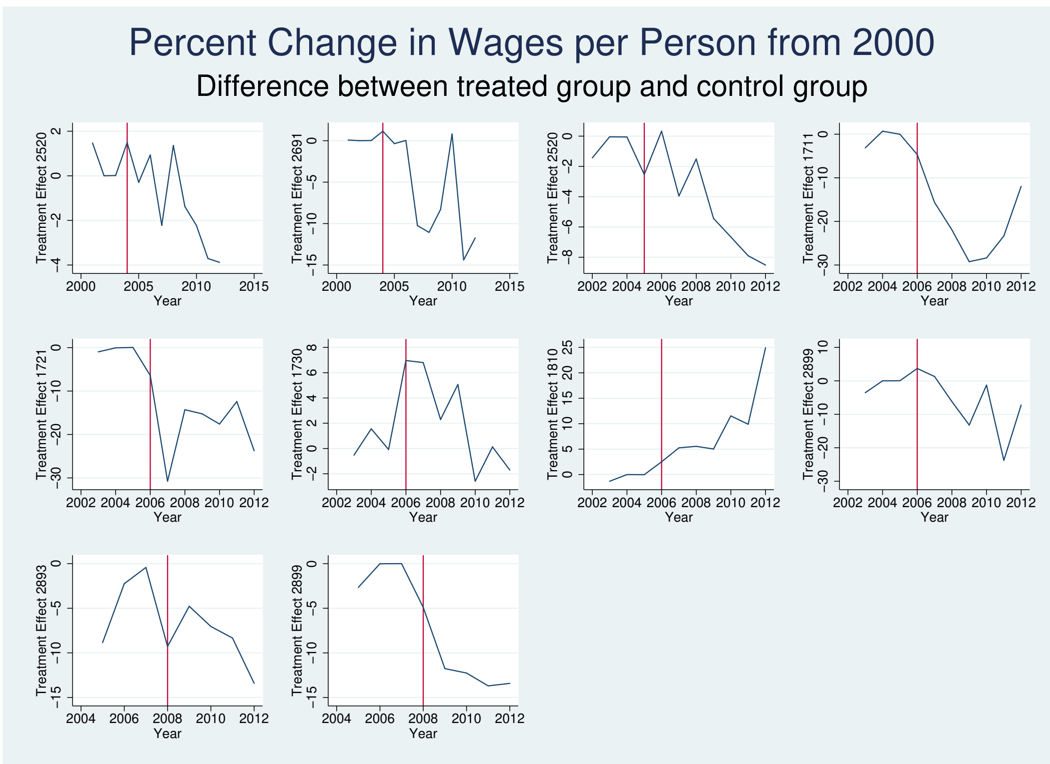

Figure 4: Treatment Effect on Wages Per Person. Vertical lines indicate treatment year. Vertical axis measures the difference in the percent change in average wages from 2000 between the treated group and the synthetic control group.

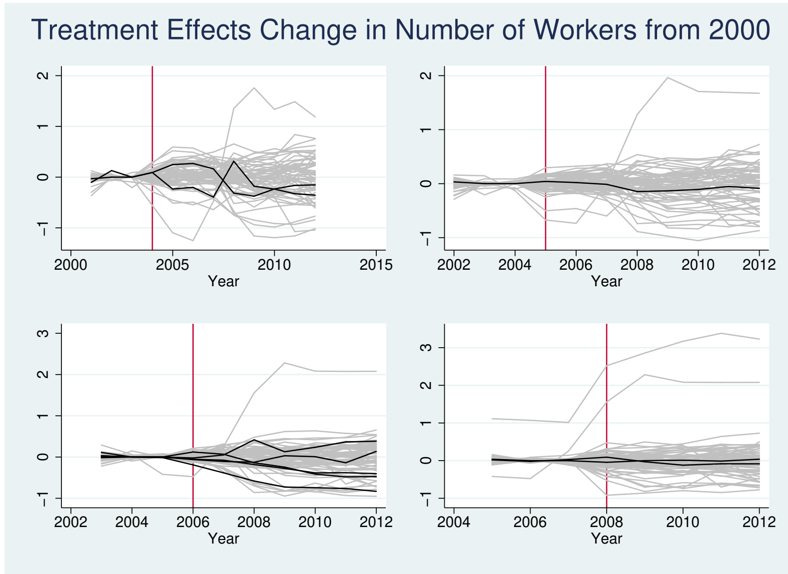

Figure 5: Robustness Check: Compares the resulting treatment effect when each ISIC four digit industry is considered individually as receiving the treatment. Each gray line is the treatment effect of a four digit industry, and the black line is the treatment effect of the industries receiving antidumping protection. Vertical lines indicate the treated year.

5% change since 2000, whereas others are large, on the order of 80% change since 2000. There is a comparable lack of clear pattern in the wage data as well, though significantly less dramatic than employment. Without further testing, it is impossible to say with certainty if there is an effect of antidumping policies on employment and wages, much less determine a positive or negative effect. Before turning to inference testing, consider a robustness check: the placebo test.

Conducting the placebo check by pretending as though each manufacturing industry received antidumping protection- the standard robustness check of the synthetic control literature- shows that the treatment effects in industries that did receive protection could have been random. Figures 5 and 6 plot the calculated treatment effects (black lines) with approximately 60 placebo treatment effects (gray lines) per graph. The treatment effects usually lie within the bulk of the placebo treatment effects. Even when considering a subset of placebo industries that are similar to the treated industries (such as industries with similar import demand elasticities or industries with the best pre-treatment fits with the synthetic control), I still find a lack of robustness of the treatment effect results (graphs omitted).

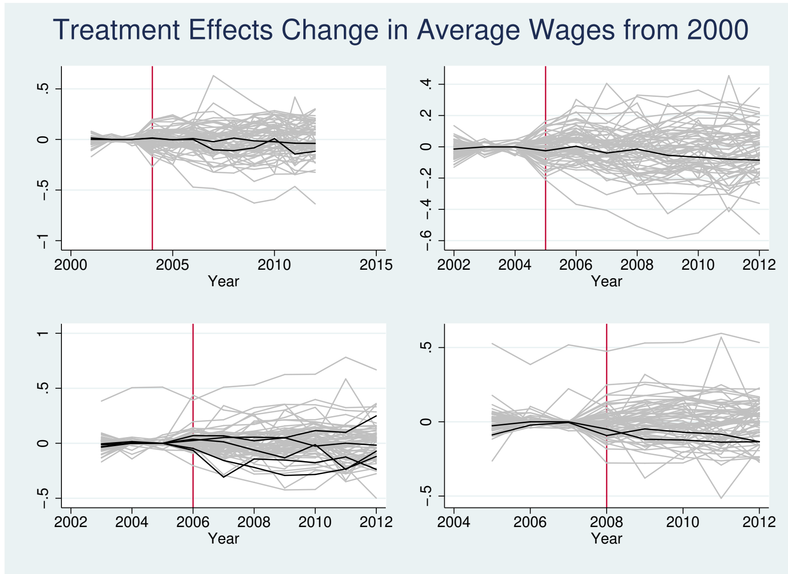

Figure 6: Robustness Check: Compares the resulting treatment effect when each ISIC four digit industry is considered individually as receiving the treatment. Each gray line is the treatment effect of a four digit industry, and the black line is the treatment effect of the industries receiving antidumping protection. Vertical lines indicate the treated year.

4.2 Inference Testing

Following the methods discussed previously in Section 2, this section exploits rankings of the treated industries relative to the placebo industries to perform inference testing across all uses of antidumping policies. Figures 7 and 8 show the distributions of the placebo elasticities for employment and wage effects compared to normal distributions with the same mean and variance. Clearly, the placebo elasticity distributions are not normal. Thus, when conducting inference tests, I use the exact placebo distribution instead of properties of large samples. Using treatment effects for the actual treated industries in each event and the placebo industries, I calculate the average treatment effect β_{1e} and elasticity η_{1e} statistics described previously. Then, I calculate and rank the percentile. These are all reported in Table 3. Five of ten events have a negative β_{1e} for both employment and average wages. Both the number of workers and average wages continued to decline beyond the synthetic control group for these industries after the treatment occurred, perhaps indicating a decrease in demand for labor. No events experienced a positive treatment effect in both number of

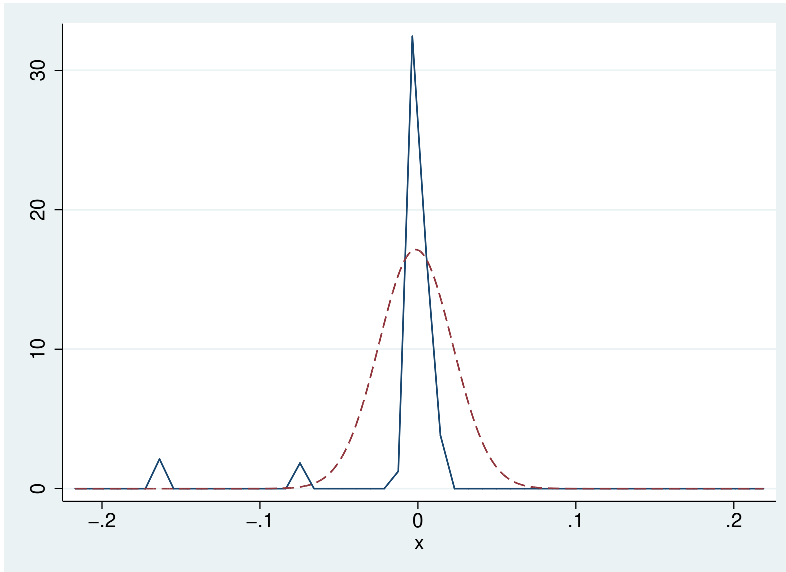

Figure 7: Employment Distribution of Placebo Elasticities

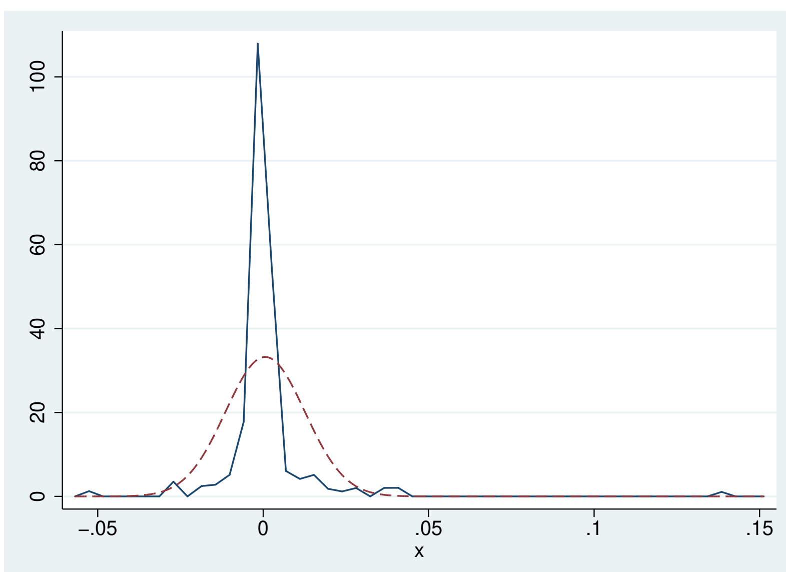

Figure 8: Average Wage Distribution of Placebo Elasticities

	ISIC	Employment			Average Wage		
Year	Industry	β	η	Rank	β	η	Rank
2004	2520	0.3749	0.0009	0.6719	-0.0374	-0.0001	0.5469
2004	2691	-0.0920	-0.0002	0.3281	-0.5190	-0.0013	0.3594
2005	2520	-0.1073	-0.0003	0.3810	-0.3373	-0.0010	0.3175
2006	1711	-1.1320	-0.0016	0.1642	-7.1051	-0.0100	0.0149**
2006	1721	23.6301	0.0332	1.0000***	-0.5242	-0.0007	0.2388
2006	1730	-0.5410	-0.0008	0.2388	0.5098	0.0007	0.7612
2006	1810	-1.0728	-0.0015	0.1791	1.2335	0.0017	0.9104
2006	2899	-0.1705	-0.0002	0.3731	-0.3393	-0.0005	0.3433
2008	2893	-1.7678	-0.0109	0.1250	-2.2352	-0.0138	0.0312*
2008	2899	0.0260	0.0002	0.5312	-0.5035	-0.0031	0.2656
Mean		1.9148	0.0019	0.3992	-0.9858	-0.0028	0.3789

Table 3: Calculated treatment effects, elasticities of treatment effects, and ranking of elasticities among all placebo elasticities. $^{***}p < 0.005$ or $p > 0.995$, $^{**}p < 0.025$ or $p > 0.975$, $^{*}p < 0.05$ or $p > 0.95$.

workers and average wages, which means in the other five events, either the employment or the average wages fell. In terms of elasticity, once the treatment effect is normalized by the dumping margin, we see that most of the effects are small. The largest effect occurred in the employment of the non-apparel textile manufacturing industry (ISIC 1721). A 1% increase in the dumping margin is associated with a 3.3% decline in employment in this industry on average after a price undertaking occurs.

The ranked elasticities, presented in the "Rank" columns of Table 3, describe the relative position of each treatment elasticity compared to 62 placebo industries. If $p_{1e} < 0.025$ or $p_{1e} > 0.975$, I reject the null hypothesis of $\eta_{1e} = 0$ at the five percent significance level. I repeat this for the ten percent significance as well, with critical values of 0.05 and 0.95. Considering the number of placebo industries in the ranking, in order to be significant at the 5% level, an event must be ranked as either one of the highest or lowest elasticities. The 10% significance level allows more leeway. Only three events are in the tails of the placebo distributions at the 10% significance level. Two of these are on the lower end of the distribution, ie, they are significant negative treatment effects. One is in the upper tail of the distribu-

	Employment		Average Wage	
Year	90% CI	95% CI	90% CI	95% CI
2004	(-0.0103, 0.0061)	(-0.0233, 0.0116)	(-0.0279, 0.0065)	(-0.1376, 0.0178)
2005	(-0.0141, 0.0033)	(-0.0775, 0.0041)	(-0.0246, 0.0052)	(-5.5444, 0.0196)
2006	(-0.0048, 0.0033)	(-0.0108, 0.0054)	(-0.0066, 0.0015)	(-0.0161, 0.0024)
2008	(-0.0467, 0.0058)	(-0.1071, 0.1420)	(-0.0453, -0.0173)	(-0.0613, -0.0092)

Table 4: In order to be significant at the 95% level, an elasticity must fall outside of the computed 95% confidence interval. Confidence intervals are calculated based on the placebo elasticity distributions.

tion, ie, positive treatment effects. Overall, with only 15% of the possible event-outcome combinations being significant at the ten percent level, it seems that antidumping policies are not effective at stabilizing employment and wages- the affected industries are relatively similar to their respective artificially created industries before and after the treatment.

One potential criticism is that with relatively "few" placebo industries (62 manufacturing industries), if one of those placebo industries has a large treatment effect, it would be difficult for any of the treated industries to beat it in the ranking, thereby driving the lack of significant results. In other words, the lack of significant results may be due to not having enough observations. Table 4 addresses this concern with confidence intervals of elasticities at the 90% and 95% levels for each year. In 2004, in order for the percentile rank of an employment effect elasticity to be significant at the 90% level, the elasticity would need to be either less than -0.0103 or greater than 0.0061. More often than not the computed elasticities are not borderline cases that are close to the bounds of the confidence interval; 50% of the elasticities are in the two middle quartiles of the percentile ranking, between ranks of 0.25 and 0.75. Therefore, it seems unlikely that the elasticities would become significant even after including additional placebo industries.

Interestingly, the average weighted treatment effect of employment and wages (the elasticities) are both close to the 40th percentile ranking, as seen in the bottom row of Table 3, indicating that on average, the treatment effects are close to the placebo treatment effects (where no treatment actually occurred). This could potentially be due to either having no

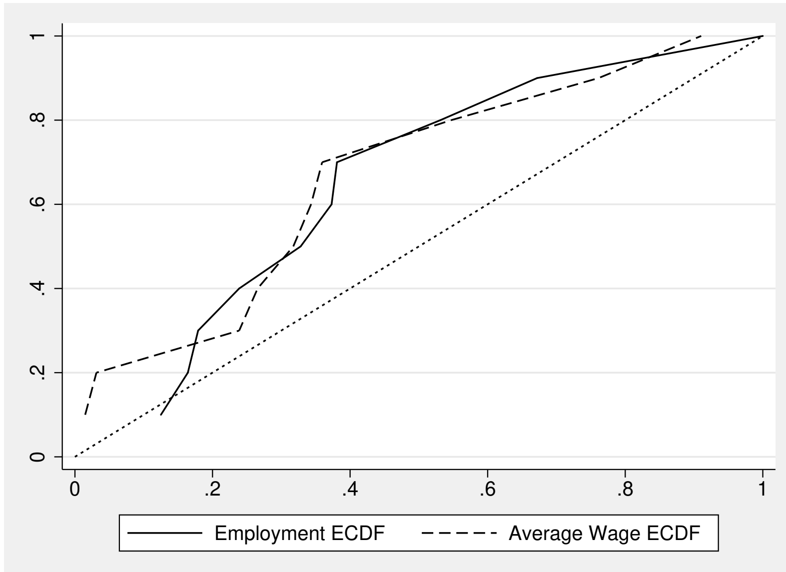

Figure 9: Empirical CDF of Treated industries percentile rank. Test of sharp null that $\eta_e = 0$.

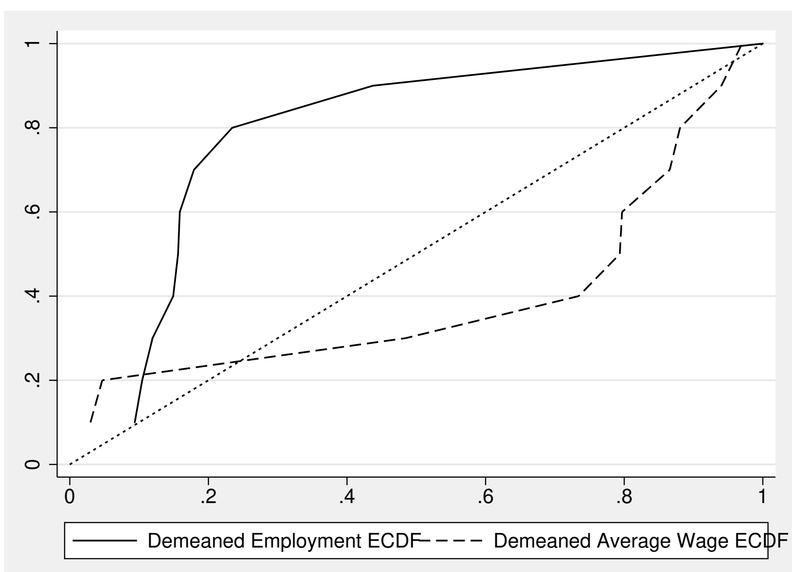

Figure 10: Empirical CDF of Treated industries percentile rank, demeaned. Test of the null $\eta_e = \bar{\eta}$.

treatment effect in all events, or having positive and negative treatment effects that average out to be close to zero. The inference tests that follow will help determine which of these is more accurate.

First, consider the sharp null hypothesis for both changes in employment and wages that the treatment effect elasticity η_e is zero, ie, antidumping policies have no effect. Figure 9 plots the empirical CDFs of employment and average wage against the theoretical percentile rank CDF- the uniform CDF. Visually, both empirical CDFs are the most concentrated around the middle of the distribution. A one-sample Kolmogorov-Smirnov test for each outcome of interest verifies that the sharp null of no effect should be rejected in both cases. The p-values are both less than 0.002. Thus, there is evidence that the treatment effect elasticity is not constant at zero.

Next, I test for heterogeneity in treatment effects. That is, I test to see if the treatment effect elasticity is constant at a value different from zero, or the sharp null hypothesis $\eta_e = \bar{\eta}$ where $\bar{\eta} \neq 0$ is the mean value of the treatment elasticities. If the sharp null is rejected, then there is heterogeneity in the treatment effect response. To test for heterogeneity, I demean the treatment elasticities as follows.

$$\eta_{1e}^{demeaned} = \eta_{1e} - \bar{\eta}_{1e} \text{ where } \bar{\eta}_{1e} = \frac{1}{E}\sum_{e=1}^{E}\eta_{1e}$$

Then, I recalculate the percentile ranks using the original placebo elasticities and the new demeaned treatment elasticities. Finally, I plot the empirical CDFs of the demeaned rankings with the theoretical uniform CDF in Figure 10. An associated one-sample Kolmogorov-Smirnov test rejects the sharp null hypothesis in both outcomes with p-values less than 0.001. The demeaned empirical CDFs are significantly different from the theoretical distribution. Thus, there is evidence that the treatment elasticities are heterogeneous and not all equal to the average treatment elasticity.

These results show that more often than not, antidumping policies are not an effective policy to stabilize employment in Colombian manufacturing industries. However, there is heterogeneity in the results. Antidumping protection seems to have helped stabilized employment in only one out of ten industries studied. Wages were not significantly stabilized in any of the industries, but continued to decline beyond a synthetic benchmark in two of the ten industries. However overall, there is not enough evidence to suggest antidumping policies have zero effect as a whole. The effects depend instead on the industry receiving protection.

5 Conclusion

Overall, the impacts from antidumping duties vary across industries and measures. By taking an approach that is typically applied in policy evaluation studies, and now emerging more in international trade, I quantify the domestic impacts from antidumping protection. In some instances, protected industries are better off than a synthetic control, and in others, they are worse off. When antidumping duties help, they increase employment and wages per person by up to 40% over the control group, artificially constructed to represent the pre-treatment affected industry. Despite these magnitudes, there is not strong evidence that the results are robust. Similar results can be obtained by conducting the same analysis on other manufacturing industries in the Colombian economy. However, while there is not a systematic strong effect of antidumping policies for all industries, there do seem to be heterogeneous effects. This begs the question of why. Why do some industries benefit from protection while others continue to decline afterwards? Is it possible that these industries use their brief period of protection to invest in better technology? Or are there fewer import substitutes available, so by taxing the dumping importers, the domestic firms are able to grow? Future research that can address these questions can move economic agents closer to using the discriminatory, distortionary policy in more efficient ways, or determine when the policy would not be beneficial at all.

A companion paper to this one seeks to explore those questions further. In particular, it examines the effects on Colombian imports of products subject to antidumping investigations.

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