Labour-Market Effects of India's Termination from the United States' GSP*

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April 3, 2025

Abstract

This paper investigates the 2019 termination of India's preferential trading under the United States' Generalized System of Preferences (GSP). It examines the impact of the GSP termination with a triple differences-in-differences specification that controls for both country- and product-level export changes. We calculate estimates of the decrease in exports from India to the United States by sector, deriving a shock index. We then use the shock index as a treatment variable in a second model, utilizing a difference-in-difference approach to measure the effect of the shock on monthly individual-level worker incomes and employment.

Our findings suggest that the GSP termination had a considerable impact on Indian exports to the US; on average exports of affected products dropped by 2 percent in 2019, with exports of transportation manufacturing declining more than 20 percent. These reduced trade flows from India to the U.S. had knock-on economic implications for workers in India that we argue were likely driven by a strong countercyclical response from large firms that resorted to occupational restructuring that saw companies prioritising professionals at the expense of day labourers. These findings have important implications for economic inequality as earners in the top income quintiles arguably capture the largest income gains and demonstrate the greatest employment resilience in the immediate aftermath of the termination.

JEL: F13 | F16 | J31 | M51

KEY WORDS: Generalized System of Preferences, India, United States, U.S.-India trade relations, trade shocks, trade policy, differences-in-differences, occupational restructuring, labour market outcomes, countercyclicality

*Author contributions: C.H. contributed conceptualization, methodology, software, formal analysis, data curation, writing, and visualization. A.S. contributed writing, visualization, supervision, and funding acquisition. This paper benefitted from research carried out with funding from the UK Economic and Social Research Council (ESRC) under grant reference ES/T016248/1. The authors declare no conflict of interest.

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1 Introduction

The last several years have seen a rise in protectionist trade policies. Several studies have documented how the increase in tariffs affects trade flows between countries (Fajgelbaum et al., 2020; Dix-Carneiro and Brian K Kovak, 2017). Despite a body of evidence on trade and labour markets (McCaig and Pavcnik, 2018; Kis-Katos and Sparrow, 2015), there is little attention to the effects of protectionist policies on the labour market in relation to unilateral preferences. This paper addresses this gap and explores the effects of the U.S. termination of India from its generalized system of preferences (GSP) on Indian labour markets in affected sectors.

On 5 June 2019, the U.S. termination of India from preferential trading under its GSP went into effect, three months after the Office of the United States Trade Representative (USTR) announced the termination. Prior to the termination, India had been the largest beneficiary of the GSP, which provided for duty-free trade on 1,921 product lines covering $6.35 billion in exports from India to the United States (Babu, 2020). In affected sectors, the United States represented between 14 and 33 percent of Indian exports within these sectors.

There were many reasons to believe that this move was a surprise for vested parties around the world. Under the GSP, all lower-middle income countries benefit from preferential rates. Although the United States started reviewing India's GSP status in April 2018, there was little reason to suspect that India's access would be terminated, as its Gross National Income per capita was well below the high-income limit set by the World Bank. The USTR press release suggests that the termination was a response to India's trade practices: "India's termination from GSP follows its failure to provide the United States with assurances that it will provide equitable and reasonable access to its markets in numerous sectors" (United States Trade Representative, 2019). That India was ultimately terminated from the GSP must therefore have come as a surprise and could have far-ranging economic consequences first to trade flows and, by extension, households with workers employed in affected industries.

This paper capitalizes on the suddenness of India's GSP termination as a quasi-natural experiment to explore the potential economic consequences of the rise in tariffs. In doing so, it contributes to the small body of empirical literature on the distributional outcomes of the GSP, with no studies on the development impact of the US GSP suspension of 2019 for India. Given the re-election of Donald Trump, this gap should be particularly striking, given the significant dialogue on GSP reform in general, and about trade relations between the US and India in particular. Furthermore, understanding how the GSP affects labour outcomes can inform strategies to balance trade preferences with social equity.

This paper starts by using a triple differences-in-differences model (country-product-year) to estimate the impacts of the termination on India's exports to the United States. It derives a shock index - exports lost as a result of termination- by sector.

The paper then uses the shock index as a treatment variable in a second model, which uses a differences-in-differences approach to measure the effect of the shock on individual-level worker outcomes, such as income and probability of employment.

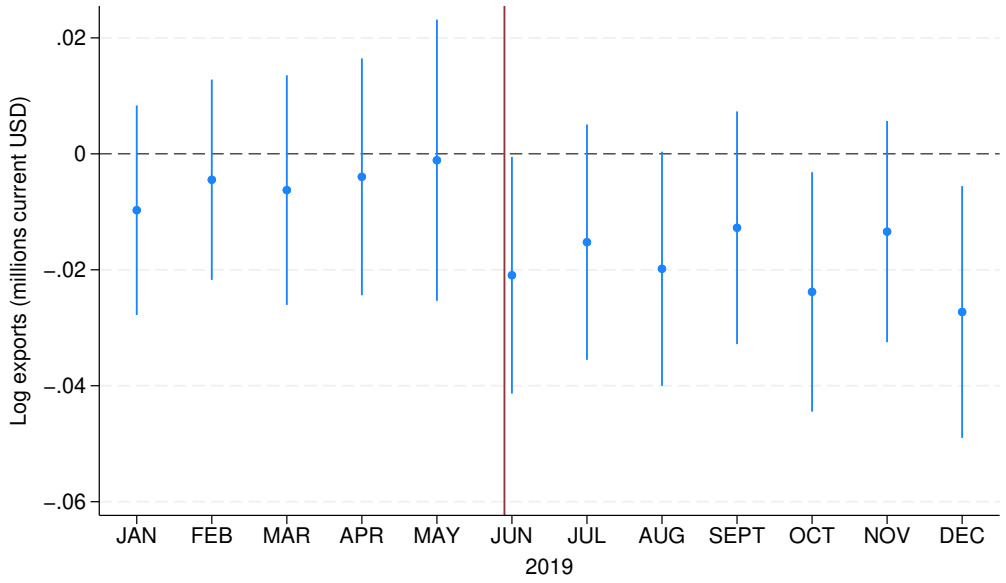
Although we believe the termination of the GSP could arguably have medium- and long-term consequences for India's economy, modelling the effects becomes difficult with the onset of the COVID-19 pandemic just seven months after the termination went into effect. The existing literature on GSP has also shown the benefits of these unilateral preferences in the short-term (Hoekman and Özden, 2005; Agostino, Aiello, and Cardamone, 2007; Cardamone, 2007). For these reasons, our paper focuses on the short-term impacts of the GSP termination. Using a panel dataset of exports we constructed from India's Ministry of Commerce and Industry and data of hundreds of thousands of workers from the Centre for Monitoring Indian Economy (CMIE) Pyramids, we are able to look at the month-by-month effects in detail.

We find that the termination of the GSP had a considerable impact on Indian exports to the US. On average, exports of affected products from India to the United States fall by about 2 percent. There was significant heterogeneity in these effects across sectors. Transportation manufacturing products covered by the GSP, for instance, fell by more than 20 percent. Affected products in foodstuffs, machinery and electrical, pharmaceutical manufacturing, wood and wood products, and manufacturing N.E.C. all experienced reduced trade flows in excess of 4 percent. Analysis of pretrends reveal a stark drop in exports of affected products in the month the termination went into effect (Figure 1).

The development impacts of these changes in export flows can be measured through different channels of improved productivity or competition effects for firms, and labour market participation for households (Winters, McCulloch, and McKay, 2004). However, the causal pathways linking these channels of trade expansion with distributional outcomes of poverty and inclusion are complex (Saha et al., 2022; Autor, Dorn, Hanson, et al., 2025). Further, these distributional consequences can vary widely within countries based on socioeconomic contexts and barriers experienced by different groups in society (Autor, Dorn, and Hanson, 2013; Danzer and Grundke, 2020; Dai, Huang, and Zhang, 2021; Fan, F. Lin, and S. Lin, 2020; Dix-Carneiro and Brian K. Kovak, 2019; Saha, 2025).

The reduced trade flows from India to the US had knock on economic implications for workers in India that were arguably driven by a strong countercyclical response from large firms, which employ a sizeable share of workers in the industrial sectors most affected by the termination. In particular, affected sectors were less likely to let workers go, and were less likely to hire new workers, resulting in a slight increase in net employment. Workers employed in affected sectors saw increases to their wages, a change driven by occupational reshuffling that saw companies prioritising professionals (e.g. support staff and non-industrial technical workers) at the expense of day labourers. We find that the occupational restructuring of affected industries has important implications for economic inequality. Earners in the top income quintiles

Figure 1: Pre-trends: Impact on India's exports in affected sectors



Note: The figure is created using estimates from Equation 1, but replacing g_t with a vector of month indicator variables extending from 1 to 12. The regression uses standard errors robust to heteroskedasticity and which allow for correlation within 6-digit product lines. Country×Product, Country×Year, and Product×Year fixed effects are included. Data are from 2014 to the end of 2019. Units are logged exports in millions of current US dollars. The error bars represent 95 percent confidence intervals.

capture the largest income gains and demonstrate the greatest employment resilience, especially in the first three months after the termination took effect.

In the short-term, the GSP termination serves primarily as a demand shock to affected industries. We therefore hypothesise that this demand shock has implications for sectors that are forwardly linked with, or provide key inputs to, affected sectors. We find the same positive effects on wages and job retention in sectors that are forwardly-linked with affected sectors. In essence, the greater the exposure to the demand shock, whether direct or indirect, the greater the counter-cyclical response observed.

The remainder of this paper is organised as follows. Section 2 reviews recent evidence on the effects of protectionism on trade flows and provides background information on the US termination of India from its GSP. Section 3 describes the data used in this study. Section 4 outlines the methodology and empirical specification used. Section 5 discusses the results and research implications. Section 6 concludes.

2 Background and literature

In this section, we describe the nature of the GSP scheme and explain how the US GSP suspension in 2019 likely led to a strong countercyclical response from firms in India - especially large firms that resorted to occupational restructuring. This paper provides a lens through which to think about the impact of trade policy uncertainty on distributional outcomes. To the best of our knowledge, there is no work that investigates both the trade and development effects of a GSP suspension in the short-term. We fill this gap in the literature by empirically examining the effect of the 2019 US GSP suspension on labour market outcomes in India.

2.1 US GSP suspension for India

The US offers its GSP scheme of trade preferences to approximately one-third of dutiable exports from developing countries, subject to meeting the eligibility criteria required. The USTR assesses the compliance of each GSP beneficiary country with the statutory eligibility criteria.

On 16 May 2019, the US formally announced that it would terminate India's GSP status, effective 5 June 2019 (Babu, 2020), preceded by an informal information release on suspension on 4 March 2019 (United States Trade Representative, 2019). The US highlighted India's high tariffs on products including motorcycles, telecommunications equipment; significant price caps on medical devices; and, India's localization requirements among other reasons for concern.

Consequently, India, one of the largest GSP beneficiaries, was suspended on grounds of not providing equitable and reasonable access to its markets for US goods and services. India immediately lost its preferential tariff access. Prior to the termination, 25 per cent of US GSP imports were from India as of 2018 (Chauhan, 2020). The suspension saw mixed responses in India, as while some Indian trade officials dismissed the impact of the GSP withdrawal as insignificant, others in the Indian government expressed concern specifically for smaller enterprises in India (Chauhan, 2020; Babu, 2020).

2.2 The GSP Scheme and uncertainty

The GSP scheme includes trade preferences offered by developed countries to developing countries on a non-reciprocal basis. The literature has focused on how such unilateral preferences can stimulate exports from developing countries. Much of the evidence has shown that these benefits are likely in the short run (Hoekman and Özden, 2005; Agostino, Aiello, and Cardamone, 2007) than in the long run (Herz and Wagner, 2011; Özden and Reinhardt, 2005).

By its design, GSP preferences clash with the reciprocity and non-discrimination principles of the multilateral trading system, with no binding legal constraints on GSP

implementation (Özden and Reinhardt, 2005). This non-reciprocal nature of GSP schemes implies significant decision-making power resides with the donor country (Grossman and Sykes, 2005; Özden and Reinhardt, 2005; Hakobyan, 2017; Borchert and Di Ubaldo, 2020). GSP related preferences typically need periodic renewal, and can expire, be suspended, or include mechanisms for preference removal based on competitiveness. Consequently, a significant body of literature has shown that these characteristics of the GSP cause considerable uncertainty which is an important drawback of these schemes (Cui and Li, 2023; Handley and Limão, 2017; Caldara et al., 2020; Handley and Limao, 2022; Imbruno, 2019).

In particular, any unexpected changes to preferences can be detrimental, particularly for smaller exporters whose costs rise suddenly for an uncertain period of time (Hakobyan, 2020). And in general, a trade shock can impinge harder on weak firms, often smaller firms, even in the same sector, and, through them, more on the poor (as workers who are more in abundance in these firms or as owners of micro and small enterprises in developing countries). In the aftermath of the US GSP suspension, evidence shows that Indian exporters faced initial setbacks, with smaller enterprises more affected in sectors such as engineering (Chauhan, 2020).

Poorer households in developing countries receive their income mostly from work in the private sector, as a worker, producer, or trader. Hence, households are affected by the ability of firms and industries to cope with the adverse impacts of import competition and to benefit from export opportunities after trade policy changes and trade shocks. The key factor relates to the ability of firms and industries to improve their business performance, particularly related to competitiveness and productivity, and their resilience to changes (De Loecker and Goldberg, 2014; Bustos, 2011; Fiorini, Sanfilippo, and Sundaram, 2021). We argue that the reduced trade flows from India to the US in the aftermath of the GSP suspension saw a strong countercyclical response from large firms in affected sectors that play a significant role in the Indian economy (Table 1).

A suspension of the GSP can disrupt gains by causing job losses in sectors that are reliant on exports in beneficiary countries. In particular, unilateral trade preferences have shown to benefit low-wage workers (Porto, 2006; Hakobyan, 2017), such that unexpected changes will likely be to the detriment of those in the lowest tail of income distributions and exacerbate income inequality.

Also firms need to innovate to respond to trade shocks (positive and negative) by producing new goods or services or changing the way they produce these goods and services. It is firms that are capable to innovate that have the benefit to respond to trade changes (Lederman, 2011; Bhattacharya, Chakraborty, and Chatterjee, 2022). Importantly, better performing firms are also able to recruit better (higher educated) workers who then earn higher wages, increasing productivity and quality upgrading. We find evidence that suggests the likely occupational reshuffling by large firms was to the advantage of earners in the top income quintiles especially in the first three months after the termination took effect.

Furthermore, better performing businesses have better opportunities to find backward or forward linkages with lead firms in value chains (Antràs and Chor, 2022; Reddy and Sasidharan, 2024; Caliendo and Parro, 2023; Ndubuisi and Owusu, 2022). They can also benefit from agglomeration effects as they seek connections with other firms and institutions close to each other, mainly in urban areas giving them a competitive advantage to benefit from trade. We hypothesize that the shock as a result of the GSP termination presents implications for sectors that are forwardly linked with affected sectors. To capture this, we estimate a secondary shock index that measures exposure to forwardly linked affected sectors.

The overall approach in this paper presents ways to consider different channels through which trade policy uncertainty can affect distributional outcomes. We generate empirical evidence on how an unexpected change in unilateral preferences as a result of uncertainty may specifically affect employment and wage dynamics in affected industries, especially in the short to medium term.

Table 1: Role of large firms in the Indian economy, by sector

Sector	Indicator	Firm group		
		Bottom 50%	*Top 5%*	*Top 1%*
Agriculture	Mandays worked (% of economy)	47.4%	52.6%	23.8%
	Exports (% of economy)	32.5%	67.5%	49.2%
	Mandays worked (median)	3,348	102,554	301,720
Chemical industries	Mandays worked (% of economy)	49.8%	50.2%	24.1%
	Exports (% of economy)	26.7%	73.3%	25.5%
	Mandays worked (median)	9,106	369,298	1,136,054
Foodstuffs	Mandays worked (% of economy)	52.5%	47.5%	26.1%
	Exports (% of economy)	58.7%	41.3%	9.9%
	Mandays worked (median)	8,706	279,938	622,289
Machinery & electrical	Mandays worked (% of economy)	56.8%	43.2%	18.8%
	Exports (% of economy)	43.2%	56.8%	23.5%
	Mandays worked (median)	14,437	388,850	913,250
Metals	Mandays worked (% of economy)	42.3%	57.7%	35.7%
	Exports (% of economy)	20.7%	79.3%	50.8%
	Mandays worked (median)	9,661	373,973	1,382,164
Mineral Products	Mandays worked (% of economy)	43.7%	56.3%	33.2%
	Exports (% of economy)	37.6%	62.4%	0.0%
	Mandays worked (median)	5,444	148,215	436,095
Non-metallic mineral products	Mandays worked (% of economy)	46.6%	53.4%	23.2%
	Exports (% of economy)	54.1%	45.9%	15.3%
	Mandays worked (median)	7,982	254,976	651,025
Other manufacturing	Mandays worked (% of economy)	54.8%	45.2%	20.7%
	Exports (% of economy)	87.7%	12.3%	6.3%
	Mandays worked (median)	13,460	390,754	1,102,686
Pharmaceutical manufacturing	Mandays worked (% of economy)	62.3%	37.7%	15.5%
	Exports (% of economy)	45.1%	54.9%	19.8%
	Mandays worked (median)	42,291	652,478	1,607,329
Textiles, apparel, & leather	Mandays worked (% of economy)	54.8%	45.2%	21.9%
	Exports (% of economy)	52.4%	47.6%	21.0%
	Mandays worked (median)	32,295	614,349	1,508,145
Transportation manufacturing	Mandays worked (% of economy)	58.6%	41.4%	18.7%
	Exports (% of economy)	24.5%	75.5%	32.6%
	Mandays worked (median)	42,952	727,491	1,793,938
Wood and wood products	Mandays worked (% of economy)	39.4%	60.6%	33.6%
	Exports (% of economy)	52.5%	47.5%	18.1%
	Mandays worked (median)	2,527	81,546	308,868

Note: Data come from the Annual Survey of Industries (ASI) and cover the period 2017-2019. Firm group is determined by mandays worked. Mandays worked is calculated by taking the total hours worked of all workers and dividing by eight. Note that, according to the ASI, the largest one percent of firms in the mineral products sector did not report exporting goods.

3 Data

3.1 Trade-related data and worker-level data

This paper uses data from a variety of sources. We source trade data from India's Ministry of Commerce and Industry. Trade data are available by month, by [up to] HS8-digit product, and by country. These data are publicly available, but decentralized across approximately 50,000 dynamically loaded webpages. For this paper, we build a dataset of monthly export data by six-digit product and partnering country.

We collect tariff trade data from the United States Trade Commission. While the United States GSP schedule is also promulgated at the eight-digit level, this entails difficulties matching it with the monthly trade data. Normally, HS codes are only standardized up to the six-digit level and not beyond. To overcome this issue, we follow Frazer and Van Biesebroeck (2010) and collapse India's trade data to the six-digit level and merge it with the tariffs at the six-digit level, taking note of the share of the product lines included in the US' GSP. The share of product lines affected does not necessarily reflect the share of trade within the HS6 line affected. Indeed, in some cases, only a small fraction of the 8-digit products within a 6-digit group are covered by the GSP, but these products represent an outsized share of the volume exchanged within the HS6 product line. For this reason, as we will see in the next section, we define a HS6 product line as "covered by the GSP" if 100 percent of its HS8 products were included in the GSP. By defining it this way, we can be certain that our analysis will indeed correspond to products that are affected by the GSP termination. This definition also means that estimates of trade disruptions likely serve as a lower bound of the true effects of the termination, as the set $p_i = 0$ includes products for which, say, 90 percent of tariff lines within a HS6 code are covered.

We also considered using other definitions to measure sector exposure to the GSP termination. For instance, one may define exposure as the share of HS8 product lines affected within each HS6 code. One could also define it as having at least one HS8 product line affected. Further yet, one could in theory use tariffs themselves as exposure to the termination.

The first two definitions are problematic. Recent research on French exports to Morocco found that only 49 percent of exports had correct product classifications, suggesting it may be easy to slightly shift the product classification to avoid tariffs. It is more difficult to get away with changing codes at the H6 level. Defining exposure as at least one HS8 product line lacks specificity. In theory this could mean that HS6 product lines with only 1 percent of exports affected are grouped together with HS6 product lines with 99 percent of exports affected, introducing a rather noisy righthand side variable.

Using tariff values themselves to measure exposure also poses several challenges in using tariffs values themselves as the tariffs. Many U.S. tariff lines are *ad valorem*, or levied as a share of the final price paid (e.g. in 2018, the most-favored nation rate on

live donkeys was 6.8 percent). However, some tariff lines are levied per unit (e.g. the tariff on live goats was 68 cents per goat). Some tariffs consist of a per-unit cost and an *ad valorem* cost. This poses difficulties of using tariffs changes to directly measure exposure to the GSP termination. For the reasons listed above, we decide to use 100 percent of HS8 product lines affected as our preferred definition of exposure in this paper.

Another significant source of data for this study comes from India's CMIE Consumer Pyramids Household Survey (CPHS), a large-scale, nationally representative survey of India. It reports on employment characteristics, income, expenditure, asset ownership, demographic information and some psycho-social beliefs. Surveyed households are visited every four months, and are asked about the preceding four months, creating a monthly dataset of vital economic indicators of household well-being. Included survey weights allow us to make month-to-month inferences about India as a whole. More information about the CMIE survey data can be found on the CMIE website.

We use ISIC codes to match the estimates of the shock with the CMIE Pyramids data. We first use HS-ISIC correspondence tables to convert HS4 codes in the Ministry of Commerce and Industry trade data to ISIC 4-digit codes. We convert these codes at a relatively high level of disaggregation as these two codes are used for different purposes. HS codes classify goods by their physical appearance, while ISIC codes are defined by use. Matching the two on their 2-digit codes, for instance, would involve making assumptions about the intended use of a large swath of physical goods.

We then collapse the ISIC 4-digit codes into a parsimonious set of 12 sectors that complement the set of sectors identifiable in the CMIE Pyramids data: agriculture; chemical industries; foodstuffs; machinery and electrical; metals, mineral products; non-metalic mineral products; other manufacturing; pharmaceutical manufacturing; textiles, apparel and leather; transportation manufacturing; and wood & wood products.

The trade data from the Ministry of Commerce and Industry unfortunately does not contain information on services exports. We therefore are not able to say what the value of the shock is for different services sectors. While the services sector accounts for the majority of livelihoods in India, we elect to exclude these workers from our analysis, rather than replace the value of the shock with 0 and introduce bias. This choice gives our analysis an inherent focus on industry.

3.2 Limitations of firm-level data in this context

In an ideal world, we would like to include in our analysis firm-level datasets like the Annual Survey of Industries (ASI) or the Orbis database. In practice, there are several difficulties in doing so. Because our period of analysis is abutted by the onset of the shock in the middle of 2019 and the onset of the Covid-19 pandemic in the beginning of 2020, the effects we are looking at are inherently short term, counting

in months rather than years. While the sampling from in ASI is fit for purpose, the annual nature of the data mean that it is difficult to capture sub-annual movements in the data.

Orbis offers quarterly data on gross revenue from sales (but not on employment) for a non-representative subset of Indian firms. But this data too poses its own issues. Revenue data from exports is recognized when the control of goods is transferred from seller to buyer. Transfer of control is defined by International Commercial Terms, or Incoterms, in numerous ways, including 1) Free on Board (FOB), or when the goods are loaded onto the ship; 2) Cost, Insurance and Freight (CIF), or when the goods arrive at the destination port; or 3) when the goods are delivered to the buyer's location. Several months in effect could separate 1 from 3, introducing a lot of noise in the data. Because of the non-representativeness of the data, the limited availability on indicators like employment, and the difficulties of interpreting the timing of sales, we have elected to use the CMIE Pyramids dataset as our main source of information on Indian workers.

4 Methodology

The analysis in this paper proceeds in two steps. In the first step, we use a triple differences-in-differences to estimate the impact of the GSP termination going into effect. We then use the impacts—or "shocks" estimated from this first step as an input into a second model, which applies a differences-in-differences estimator to data on Indian workers to measure the causal impact of the termination on individual-level economic outcomes.

4.1 Measuring India GSP termination trade shock

To estimate trade flows, we use data as far back as January 2014 and as late as December 2019. In the first instance, we are constrained by data availability. Data from the Ministry of Commerce and Industry are only available starting that month. In the second instance, we confront the onset of the Covid-19 pandemic, whose effects on Indian trade were evident even in January 2020 and which so significantly altered India's trading patterns that it is impossible to identify the effects of the GSP termination. For this reason, this study focuses on the short-term effects of a rise in tariffs.

To capture the causal impacts of India being terminated from the US GSP, we estimate the following triple differences-in-differences specification, which includes an interaction with sector dummies to capture the effect by sector:

$$\log(x_{i,c,t}+1) = \beta_0 + \sum_{j=1}^{J} \beta_1^j \cdot \text{sector}_j \cdot g_t u_c p_i + \boldsymbol{\beta_2 F_{i,c,t}} + e_{i,c,t} \tag{1}$$

where $log(x_{i,c,t}+1)$ is the log exports of product i from India to country c at time t (month), sector_j is an indicator variable for sector j, g_t is an indicator that takes on the value of 1 when India's stops receiving preferential tariffs from the United States, u_c is a United States dummy variable, p_i is a dummy variable equaling 1 for products that received preferential tariffs when imported to the US from beneficiary developing nations, and $\boldsymbol{F_{i,c,t}}$ is a vector of covariates that consists of country-product, country-month, and product-month fixed effects. Under this formulation, β_1^j identifies the impact of the GSP termination on India's exports in sector j.[1]

When reporting our main results, we choose to estimate Equation 1 using ordinary least squares (OLS). It is increasingly common in the trade literature to utilize Poisson pseudo maximum likelihood (PPML) when estimating models, particularly gravity models. The reason for this is twofold. First, because PPML does not require log-linearization of the gravity model, it includes zero values that would otherwise be

[1]Because the summation in Equation 1 spans the entire set of sectors, one sector will be dropped by the software program due to collinearity. In practice, we create β_1^j from a linear combination of coefficients (in Stata, the *lincom* command does this), that consists of the baseline impact (or the coefficient for the omitted sector) and the sector-specific coefficient.

undefined under the log transformation (Silva and Tenreyro, 2006). Second, because the error term is specified as multiplicative in traditional gravity models, the presence of heteroskedasticity would bias the coefficients of interest (Silva and Tenreyro, 2006).[2] This second issue does not apply to the specification in Equation 1. The triple differences-in-differences estimator depends on an additive error term rather than a multiplicative one. This means that heteroskedasticity affects efficiency but not consistency, which we address using cluster robust standard errors. The first issue remains relevant, however, and we follow Fetzer et al. (2021), Johnson (2020), and others by using the natural log of exports plus one such that zeros are included. Here we make use of the fact that growth can be closely approximated by $\ln(x_{i,c,t}+1) - \ln(x_{i,c,t-1}+1)$. In practical terms, this means that the coefficients in Equation 1 can be interpreted as percent change in exports. Because the issues solved by PPML are not so thorny in our case, we elect to report the OLS estimator in our main results, but we also report the results using PPML in Annex Table A.3.

4.2 Measuring impact of sectoral trade shocks on labour market outcomes

The estimates of β_1^j will comprise a shock index that will be used to estimate individual-level impacts on economic outcomes. If $\hat{\beta}_1^j$ is not statistically different (at the 5 percent level) from 0, then we set its value to 0. To aid in interpretation, we re-scale the shock index so that its absolute value runs from 0 (no shock) to 1 (sector with the most intense shock). We allow the shock index to contain positive and negative values, but scale it such that positive values reflect a greater fall in Indian exports (a tabulation of the index values can be found in Table A.7 in the annex). We then estimate the following specification to identify causal effects of the GSP termination on individual-level economic outcomes:

$$y_{i,j,t} = \alpha_0 + \sum_{m=6}^{12} \alpha_1^m Month_m \cdot \hat{\beta}_1^j + \alpha_2 y_{i,j,t-12} + \alpha_3 ymiss_{i,j,t-12} + \boldsymbol{\alpha_4 C_{i,j,t}} + \epsilon_{i,j,t} \quad (2)$$

where $y_{i,j,t}$ is an outcome of interest (income, employment status, consumption, etc) of individual i associated with sector j (or possibly no sector affiliation) at month t, $Month_m$ is a binary variable that takes a 1 in month m of 2019 and 0 otherwise, $\hat{\beta}_1^j$ represents the shock obtained from Equation 1,

[2] To see this mathematically, start with the base model $T_{ij} = \alpha_0 Y_i^{\alpha_1} Y_j^{\alpha_2} D_{ij}^{\alpha_3} \varepsilon_{ij}$, log-linearize, and take conditional (on matrix of imports, exports, and distance X_{ij}) expectations: $E[\ln T_{ij}|X_{ij}] = \beta_0 + \alpha_1 \ln Y_i + \alpha_2 \ln Y_j + \alpha_3 \ln D_{ij} + E[\ln \varepsilon_{ij}|X_{ij}]$. In this context, it is typical to assume that ε_{ij} is logarithmically distributed. In order for OLS estimation of the coefficients to be unbiased and consistent, we therefore need to have $\ln E[\varepsilon_{ij}|X_{ij}] = 0$. If ε_{ij} has a log-normal distribution with a mean of 1, then $\ln E[\varepsilon_{ij}|X_{ij}]$ will be normally distributed with a mean of 0, satisfying the condition. The issue, however, is that, as shown by the Jensen inequality, we know that $E[\ln \varepsilon_{ij}|X_{ij}] \neq \ln E[\varepsilon_{ij}|X_{ij}]$ in the presence of heteroskedasticity. In fact, because the error term is concave, we can write more specifically that $E[\ln \varepsilon_{ij}|X_{ij}] \leq \ln E[\varepsilon_{ij}|X_{ij}]$.

We estimate Equation 2 for the year 2019, but include the value of the dependent variable one year prior ($y_{i,j,t-12}$) to account for seasonality. In the event this value is missing, we set it equal to -999 otherwise[3] and include the dummy variable $ymiss_{i,j,t-12}$ set to 1 if the lagged variable is missing. $\boldsymbol{C_{i,j,t}}$ is a vector of controls and fixed effects, and includes month fixed effects and sector fixed effects. In this specification, α_1^m identifies the causal impact of the shocks in month m. We do not explore the impacts on individuals after 2019 due to the confounding influence of Covid-19.

Because we are utilizing estimated coefficients in Equation 2 (i.e. $\hat{\beta}_1^j$), we should be confident that the estimates are not due to spurious variation in the standard errors. Our model contains 348,417 country-product fixed effects, 14,273 country-month fixed effects, and 297,101 month-product fixed effects—a total of 659,791 fixed effects. Given our sample size of 5,747,736 observations, we have 5,087,945 degrees of freedom. We therefore feel confident in the structure and precision of the coefficient's standard errors and do not feel the need to bootstrap, which with our model would be extremely computationally expensive. Given the large degrees of freedom, the central limit theorem suggests that the imported coefficients would be, asymptotically speaking, normally distributed. In the annex, we include a histogram (Figure A.3) of the residuals of Equation 1, which depicts a normal distribution, implying the estimated $\hat{\beta}_1^j$ coefficients are also normally distributed. We therefore trust the assumptions underlying hypothesis testing, namely, that the t-statistics are normally distributed.

4.3 Checking the parallel trends assumption

The parallel trends assumption may be tested by running slight variations of the above equations. For the impacts on Indian exports, we test Equation 1 but replace g_t with $\sum_{m=1}^{12} Month_m$. For Equation 2, we examine pretrends by changing the index of the summation to extend from months 1 to 12.

[3]It does not matter what value this is, as changing it only affects the coefficient α_3.

5 Results

5.1 Impacts of the termination on Indian exports

Table 2 contains the results from Equation 1, estimated over the period 2014 to 2019 with Country×Product, Country×Year, and Product×Year fixed effects. Standard errors are clustered at the HS6 product level to avoid spurious results due to autocorrelation (Bertrand, Duflo, and Mullainathan, 2004). The estimates in the table show that the GSP termination caused affected product lines in many sectors to experience reduced trade flows from India to the United States. The sector in which affected products were hit the hardest was *transportation manufacturing*, where affected products saw volumes fall by more than 20 percent, but sectors like *foodstuffs*, *machinery & electrical*, *pharmaceutical manufacturing*, *wood & wood products*, and *other manufacturing*[4] also saw negative impacts. Product lines in highly upstream sectors like agriculture and metals saw increased trade flows as a result of the GSP termination, though we wonder if this effect is driven by increased demand in the US for inputs of goods that were covered by the GSP termination in other sectors (e.g. *foodstuffs*, *machinery & electrical*, and *transportation manufacturing*). Overall, the GSP termination resulted in approximately a 2 percent reduction in exports from India to the United States, which began in the month in which the termination took effect (Figure 1).

Because the GSP termination binary variable p_i in Equation 1 is coded as 1 only if 100 percent of the goods within an HS6 product-line were covered by the GSP, the estimates serve as lower bound for the true impact of the termination. This means that product lines with 100 percent of goods at the HS8 level covered are being compared with product lines with anywhere from 0 to 99 percent coverage. Our approach also means that we cannot study how the effect varies with the magnitude of the tariff change, which also could be a factor influencing the size of the coefficients presented in Table 2.

We believe that the impacts observed in Table 2 are not the results of spurious state dependence on previous periods. Figure 1 provides a test of the parallel trends assumption, which could be violated if the GSP termination was, for instance, largely a symbolic measure whose targeting products were waning in volume between the two countries. The figure shows a clear break in the data in June 2019, the month the termination went into effect. Prior to June 2019, impacts were close to zero and trending upwards, whereas impacts since June 2019 have consistently resulted in estimated 2 percent reduction in India's export of affected products. There does not appear to be an effect on exports to the US in response to the announcement in March, suggesting that the effects starting in June are due primarily to the regime change rather than the announcement.

[4]Includes manufacture of furniture and manufacturing N.E.C.

Table 2: Impacts of India's termination from the United States' GSP on trade flows by sector

	Dependent variable: log exports, $\log(x_{i,c,t}+1)$												
Sector	Agriculture	Chemical industries	Foodstuffs	Machinery & electrical	Metals	Mineral Products	Non-metallic mineral products	Other	Other manufacturing	Pharm. manufacturing	Textiles, apparel, & leather	Transport. manufacturing	Wood and wood products
$\hat{\beta}_1^j$	**0.036*** **	0.000	**-0.059*** **	**-0.053*** **	**0.021*** **	-0.008	0.023	**-0.038*** **	**-0.065*** **	**-0.141*** **	**0.06*** **	**-0.205*** **	**-0.042*** **
se	(0.007)	(0.004)	(0.007)	(0.006)	(0.007)	(0.015)	(0.017)	(0.016)	(0.01)	(0.011)	(0.011)	(0.022)	(0.011)
N	5,747,736												
$R\hat{2}$	0.840												

*Note: Estimates for β_1^j come from Equation 1. The regression uses standard errors robust to heteroskedasticity and which allow for correlation within 6-digit product lines. Country×Product, Country× Year, and Product× Year fixed effects are included. Data are from 2014 to the end of 2019. Units are logged exports in millions of current US dollars. For the sector J dropped due to multicollinearity, I assume $\beta_1^J = 0$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

5.2 Effects of the trade shock on Indian workers

We create a shock index by normalizing the coefficients in Table 2 such that a value of 1 represents the sector in which the GSP termination had the greatest effect (i.e. in *transportation manufacturing*). We then match these sectors to the CMIE Pyramids household survey data, in which individual workers report the sector in which they work. There are two different ways to define the sector of employment as it concerns an individual's exposure to the trade shock. One way is to define one's exposure to the shock as the sector the individual's sector of employment in May 2019—at the onset of the shock. Another way is the sector in which one happens to work in a given month. The first case allows us to explore the effects on workers exposed to the trade shock even as they change sectors or leave the labour force. The second case allows us to study the effects at the extensive margin of the sectors more broadly, looking at the hiring habits of these sectors, the net employment effect, and the effects on wages of workers employed in these sectors.

We choose to look at sector of employment in May 2019 because it gives us the best indication of a worker's exposure to the GSP termination during its onset. Because of the way the CMIE survey was designed, we often do not have information on a worker's sector of employment if they are not gainfully employed. This means that this definition of exposure considers only those that are gainfully employed—that is, those reporting income—in May 2019. Using Equation 2 will compute the impacts on workers employed in May 2019 in affected sectors relative to their pre-shock values and relative to those employed in unaffected sectors, the standard differences-in-differences interpretation.

Table 3 reports the results for Equation 2 with individual, sector, and month fixed effects, as well as a vector of demographic controls. We follow previous analyses of spatial shocks to the Indian economy and cluster our standard errors at the district level (Chodorow-Reich et al., 2020; Chanda and Cook, 2022).[5] Columns (1) and (3)

[5]This previous research did not find a significant difference between using spatial heteroskedastic-

show the impacts of the termination on workers employed in affected sectors in May 2019 relative to the pre-shock period and employed in unaffected sectors. Column (1) shows that workers employed in affected sectors experienced increases in their reported wages. For example, the coefficient in September 2019 suggests that being employed in the most affected sector (i.e. a shock index of 1) in May 2019 resulted in an increase in September wages of INR2,422.69 relative to workers in unaffected sectors. The results in column (3) indicate that these workers were less likely to lose their jobs as a result of the trade shock. Both of these effects persist through the end of 2019, with the employment effect becoming stronger over time.

Columns (2), (4), and (5), however, look at workers employed in affected sectors. Column (2) paints a less clear picture for worker wages in affected sectors overall. There is slight evidence of an increase in worker wages from June to September, as all of the coefficients are positive and some are close to significant, but only the coefficient on August is significant at the 5 percent level. There is stronger evidence of a negative wage effect in the final quarter of 2019, though again only one of these coefficients (November) is significant. Column (4) shows that workers were less likely to be hired in affected sectors compared to before the termination. This effect becomes particularly pronounced in the final quarter of 2019. Column (5) shows the net employment effect of the termination. There are some signs of an initial positive effect on net employment with significant coefficients in August and September, but there otherwise the results do not otherwise suggest a change.

India's National Sample Survey Office defines employment as having worked 30 days in the past 365 days. Because we are using monthly data and interested in the short-term effects of the shock, we think of employment in terms of whether the respondent reported wage or salary income in a given month. As a result, the sample size in columns (1) and (3) are substantially smaller than one would expect given the 2019 Period Labour Force Survey's estimated employment rate of just under 50 percent.

ity and autocorrelation consistent (SHAC) standard errors and those clustered at the district level, so we opt for the less computationally intensive specification (i.e. the latter)

Table 3: Impacts of GSP termination wage income and dimensions of employment, by month

	(1) Monthly wage (INR)	(2) Monthly wage (INR)	(3) Probability of retaining job	(4) Probability of gaining job	(5) Probability of employment
June 2019 X May 2019 sector shock	**2409.220*** **		**0.067*** **		
	(360.832)		(0.017)		
July 2019 X May 2019 sector shock	**1846.736*** **		**0.060*** **		
	(374.364)		(0.023)		
August 2019 X May 2019 sector shock	**2531.713*** **		**0.099*** **		
	(435.879)		(0.027)		
September 2019 X May 2019 sector shock	**2422.685*** **		**0.113*** **		
	(588.683)		(0.029)		
October 2019 X May 2019 sector shock	**2944.685*** **		**0.127*** **		
	(641.692)		(0.029)		
November 2019 X May 2019 sector shock	**1961.840*** **		**0.129*** **		
	(693.725)		(0.031)		
December 2019 X May 2019 sector shock	**1971.918*** **		**0.151*** **		
	(729.659)		(0.036)		
June 2019 X Sector shock		121.331		**-0.208*** **	-0.011
		(637.497)		(0.080)	(0.028)
July 2019 X Sector shock		824.503		-0.138	0.024
		(685.683)		(0.093)	(0.039)
August 2019 X Sector shock		**1672.661** **		**-0.208*** **	**0.079***
		(656.872)		(0.079)	(0.041)
September 2019 X Sector shock		810.819		**-0.255*** **	**0.084***
		(810.767)		(0.086)	(0.046)
October 2019 X Sector shock		-1425.050		**-0.421*** **	0.006
		(1114.549)		(0.088)	(0.048)
November 2019 X Sector shock		**-2074.492** **		**-0.422*** **	0.016
		(976.627)		(0.090)	(0.049)
December 2019 X Sector shock		-1500.597		**-0.435*** **	0.029
		(1095.985)		(0.098)	(0.055)
Observations	382,812	2,546,454	382,820	2,161,229	2,546,476

*Note: Estimates come from Equation 2. The regression uses standard errors robust to heteroskedasticity and which allow for correlation within districts. All regressions do not consider those working in services sectors, utilities, or screen production, as these sectors could not be well matched with the trade data. Regressions use individual, sector, and month fixed effects, and control for urban status, gender, religion, relation to the household head, household size, literacy, education, and age. All columns use data from 2019 only, but control for values from one year prior to control for cyclicality. As some 2018 values are missing, mean values for 2018 are used and an indicator variable reflecting this missing data is included. Columns 1 and 3 consider only those who report income the month before the termination went into effect (i.e. May 2019) in industrial sectors. Column 4 only considers those not reporting income in May 2019. Columns 2 and 5 consider the whole sample. Columns 2, 4, and 5 use the same controls and fixed effects as columns 1 and 3, except that the sector fixed effect reflect the sector in which the individual works in a given month. The sector shock independent variables capture the estimates for β_1^j and are normalized on a scale from -1 to 1. Wage outcomes are in current INR, while employment outcomes are binary variables with 1 indicating employment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Table 4 and Table 5 explore more deeply the impacts on those employed in May 2019. Table 4 reports impacts on wages and probability of retaining employment for individuals employed in May 2019. For ease of interpretation, linear combinations of coefficients are shown.[6] The results suggest that earners in the top two quintiles working in affected sectors were significantly more likely to retain their jobs. They also report higher wages, likely related to their increased likelihood of remaining employed. The same is not true of workers in the bottom three quintiles, for whom there is not robust evidence of changes to wages or employment in the first three months after the shock. In the context of the findings in columns (1) and (3) of Table 3, the results here suggest that companies in affected sectors were more likely to retain staff in the months following the shock, but primarily those with higher compensation.

The story begins to change toward the end of 2019. In fact it is people working in the lower quintiles that are more likely to retain their jobs and report higher incomes. In fact, in the final two months, the highest-earners in affected sectors are no more likely to be employed or report higher wages than similar workers in other sectors.

Table 5 considers the impacts by occupation for those employed in affected sectors in May 2019. Once again, linear combinations of coefficients are shown for ease of interpretation. The table shows that despite aggregate positive effects on job retention, *day labourers* in affected sectors are no more likely to retain their job following the GSP termination. *Day labourers*, which account for 6.6 percent of employment in affected sectors and 62 percent of workers in the bottom quintile, appear to be the workers that lose out on the countercyclical actions by firms to retain workers essential to their operation.

Interestingly, *industrial workers*, for the most part, do not see an increase in their likelihood of retain their job. On the other hand, workers like *non-industrial technical workers* and *support staff*, who are not directly involved with production but who nonetheless support the operation of businesses, are more likely to retain their jobs as a result of the GSP termination. This effect is particularly strong for *non-industrial technical workers*, who also experience a perhaps related increase in wages.

Table 6 displays similar results to Table 5, except for the entire sample and those not employed in May 2019 respectively. On the whole, these results show that, while affected sectors were less likely to hire and more likely to retain workers on the whole, the GSP termination did not result in significant changes to the net composition of

[6]When interacting $\sum_{m=6}^{12} \alpha_1^m Month_m \cdot \beta_1^j$ in equation 2 with wage quintile and occupation, the statistical software will drop one quintile/occupation to avoid multicollinearity. This creates a base quintile/occupation and then quintile/occupation specific coefficients. The overall impact in on a given quintile or occupation is given by the coefficient on the base value plus the quintile/occupation specific coefficient—except for the base value. By linear combination of coefficients we mean that we sum the coefficients ($\hat{\theta} = c_1\hat{\beta}_1 + c_2\hat{\beta}_2$), use the original variance-covariance matrix to create new standard errors for the summed coefficient, and rerun statistical inference tests. What we are reporting is the summed coefficient, the new standard errors, and the resulting p-values.

Table 4: Impacts of GSP termination by wage income quintile

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Monthly wage (INR)					**Probability of retaining job**				
	Q1	*Q2*	*Q3*	*Q4*	*Q5*	*Q1*	*Q2*	*Q3*	*Q4*	*Q5*
June 2019	-26.30	112.18	270.26	**1,699.54*** **	**10,159.36*** **	0.017	-0.026	-0.019	0.040	**0.312*** **
	(283.24)	(282.60)	(339.07)	(609.98)	(1,431.81)	(0.029)	(0.018)	(0.018)	(0.026)	(0.047)
July 2019	**605.74***	246.68	156.84	**2,259.42*** **	**6,077.22*** **	0.076	-0.007	-0.015	**0.071***	**0.167*** **
	(332.41)	(422.54)	(421.02)	(801.24)	(1,396.29)	(0.046)	(0.029)	(0.025)	(0.039)	(0.054)
August 2019	481.20	**897.10***	637.70	**3,021.77*** **	**7,554.57*** **	0.058	0.024	0.033	**0.140*** **	**0.243*** **
	(412.59)	(492.79)	(515.96)	(914.87)	(1,467.54)	(0.058)	(0.040)	(0.029)	(0.049)	(0.054)
September 2019	**1,399.38*** **	1,010.27	813.17	**3,046.27*** **	**5,419.44*** **	**0.098***	**0.079***	**0.079** **	**0.145*** **	**0.172** **
	(489.03)	(655.22)	(750.10)	(1,116.67)	(2,030.95)	(0.057)	(0.047)	(0.033)	(0.048)	(0.067)
October 2019	**1,414.40** **	**1,876.61*** **	626.03	**3,572.13*** **	**6,873.77*** **	**0.110***	**0.107** **	**0.066** **	**0.171*** **	**0.191*** **
	(555.01)	(611.64)	(882.02)	(1,163.69)	(1,964.15)	(0.061)	(0.043)	(0.030)	(0.052)	(0.071)
November 2019	**1,290.26** **	**1,761.54** **	858.52	**2,742.58** **	2,407.81	**0.151** **	**0.129** **	**0.101*** **	**0.158*** **	**0.124***
	(586.18)	(683.80)	(922.69)	(1,163.63)	(2,037.83)	(0.063)	(0.053)	(0.035)	(0.052)	(0.066)
December 2019	**1,429.39** **	972.24	733.33	**2,557.46***	2,816.48	**0.266*** **	**0.133** **	**0.102** **	**0.181*** **	0.093
	(626.08)	(888.13)	(1,060.28)	(1,329.99)	(1,935.00)	(0.065)	(0.067)	(0.042)	(0.057)	(0.074)
Observations			385,121					385,121		

*Note: Results come from estimating Equation 2 with an interaction term for wage quintile (as of May 2019). Estimates for columns 2-5 and 7-10 are linear combinations of coefficients. The regressions use standard errors robust to heteroskedasticity and which allow for correlation within districts. All regressions do not consider those working in services sectors, utilities, or screen production, as these sectors could not be well matched with the trade data. Columns 1 and 2 consider only those who report income the month before the termination went into effect (i.e. May 2019) in industrial sectors. Regressions use individual, sector, and month fixed effects, and control for urban status, gender, religion, relation to the household head, household size, literacy, education, and age. All columns use data from 2019 only, but control for values from one year prior to control for cyclicality. As some 2018 values are missing, mean values for 2018 are used and an indicator variable reflecting this missing data is included. Column 4 only considers those not reporting income in May 2019. Columns 2 and 5 consider the whole sample. The sector shock independent variables are normalized on a scale from -1 to 1. Wage outcomes are in current INR, while employment outcomes are binary variables with 1 indicating employment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

occupations in affected sectors. The main exception to this statement is support staff, who were hired at higher rates in affected sectors, resulting in net employment gains. Non-industrial technical workers also experienced net employment increases in the months of August and September, but these figures are only significant at the 10 percent level. While net changes were not observed in many professions, occupations like managers and home-based workers saw wage increases as a result of the GSP termination. Short-term staff like day labourers and self-employed non-professionals saw an increase in wages, but only in the final quarter of 2019.

Table 5: Impacts on wage income and employment by occupation held in May 2019

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Day Labourer		**Support staff**		**Professionals**		**Self-employed non-professionals**	
	Monthly wage (INR)	*Prob. of retaining job*	*Monthly wage (INR)*	*Prob. of retaining job*	*Monthly wage (INR)*	*Prob. of retaining job*	*Monthly wage (INR)*	*Prob. of retaining job*
June 2019 X May 2019 shock	**-2331.148*** **	0.004	**1842.117***	0.012	276.277	0.001	**3798.013*** **	**0.062*** **
	(636.457)	(0.007)	(1091.487)	(0.010)	(2438.334)	(0.008)	(1144.145)	(0.018)
July 2019 X May 2019 shock	**-2543.618*** **	-0.008	**3172.184*** **	**0.025***	2120.963	0.014	**4169.039*** **	**0.162*** **
	(667.838)	(0.011)	(1169.640)	(0.014)	(2647.568)	(0.012)	(1322.628)	(0.034)
August 2019 X May 2019 shock	**-1532.699***	-0.007	2289.368	0.018	1359.526	0.011	**6500.844*** **	**0.269*** **
	(851.939)	(0.016)	(1432.877)	(0.018)	(2971.799)	(0.016)	(1533.318)	(0.054)
September 2019 X May 2019 shock	**2412.531*** **	-0.013	-1913.419	0.024	-980.451	0.020	1709.061	**0.197***
	(893.509)	(0.017)	(1571.625)	(0.020)	(3952.264)	(0.018)	(2186.038)	(0.103)
October 2019 X May 2019 shock	**1755.353** **	-0.040	-1014.683	0.059	2078.893	0.051	3350.919	**0.201***
	(866.469)	(0.034)	(1468.726)	(0.036)	(3812.481)	(0.035)	(2312.291)	(0.103)
November 2019 X May 2019 shock	**2500.416*** **	-0.024	**-4065.966*** **	0.041	114.376	0.036	1625.193	0.063
	(833.429)	(0.031)	(1489.418)	(0.033)	(4020.578)	(0.032)	(2771.707)	(0.150)
December 2019 X May 2019 shock	**1956.979** **	-0.017	**-3934.062** **	0.033	1369.124	0.033	**7968.263** **	0.188
	(818.772)	(0.030)	(1571.775)	(0.033)	(4236.043)	(0.031)	(3587.436)	(0.179)

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Industrial worker		**Non-industrial technical worker**		**Home-based worker**		**Manager**	
	Monthly wage (INR)	*Prob. of retaining job*	*Monthly wage (INR)*	*Prob. of retaining job*	*Monthly wage (INR)*	*Prob. of retaining job*	*Monthly wage (INR)*	*Prob. of retaining job*
June 2019 X May 2019 shock	**2023.411*** **	-0.008	**4459.092*** **	**0.017***	**4199.218*** **	-0.046	1253.721	-0.021
	(712.817)	(0.008)	(1677.441)	(0.010)	(1286.244)	(0.031)	(6904.643)	(0.016)
July 2019 X May 2019 shock	**2334.909*** **	0.010	**4781.562*** **	**0.032** **	**8989.753*** **	**0.200***	1830.846	-0.007
	(742.945)	(0.011)	(1853.615)	(0.014)	(1809.197)	(0.108)	(6515.057)	(0.018)
August 2019 X May 2019 shock	**1860.048** **	0.011	3081.131	**0.038** **	**7119.972*** **	0.142	2434.427	-0.022
	(913.735)	(0.016)	(2092.977)	(0.019)	(1980.435)	(0.115)	(13412.057)	(0.022)
September 2019 X May 2019 shock	-1353.650	0.016	-1426.179	**0.044** **	3830.854	0.207	-8816.974	-0.007
	(958.788)	(0.017)	(2718.530)	(0.021)	(2437.408)	(0.154)	(18683.026)	(0.025)
October 2019 X May 2019 shock	-1424.681	0.044	-3465.283	**0.070***	**4770.926***	**0.283***	-1.54e+04	0.025
	(945.287)	(0.034)	(2577.521)	(0.037)	(2571.573)	(0.167)	(20160.961)	(0.039)
November 2019 X May 2019 shock	**-2594.632*** **	0.026	-2930.678	**0.066***	-1813.751	-0.094	-4402.901	0.005
	(920.021)	(0.031)	(3378.259)	(0.035)	(2007.722)	(0.078)	(12338.589)	(0.034)
December 2019 X May 2019 shock	**-2405.998** **	0.019	-2847.175	0.019	3166.844	0.330	-1.38e+04	0.009
	(946.475)	(0.031)	(3011.238)	(0.052)	(2570.967)	(0.273)	(14206.680)	(0.034)

*Note: Observations: 385,121. Results come from estimating Equation 2 with an interaction term for occupation (as of May 2019). Estimates for columns 3-16 are linear combinations of coefficients. The regressions use standard errors robust to heteroskedasticity and which allow for correlation within districts. All regressions do not consider those working in services sectors, utilities, or screen production, as these sectors could not be well matched with the trade data. Regressions use individual, sector, and month fixed effects, and control for urban status, gender, religion, relation to the household head, household size, literacy, education, and age. All columns use data from 2019 only, but control for values from one year prior to control for cyclicality. As some 2018 values are missing, mean values for 2018 are used and an indicator variable reflecting this missing data is included. Column 4 only considers those not reporting income in May 2019. Columns 2 and 5 consider the whole sample. The sector shock independent variables are normalized on a scale from -1 to 1. Wage outcomes are in current INR, while employment outcomes are binary variables with 1 indicating employment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Table 6: Impacts on overall wages, hiring, and net employment by occupation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Day Labourer			**Support Staff**			**Professionals**			**Self-employed non-professionals**		
	Monthly wage (INR)	*Prob. of being hired*	*Net employ.*	*Monthly wage (INR)*	*Prob. of being hired*	*Net employ.*	*Monthly wage (INR)*	*Prob. of being hired*	*Net employ.*	*Monthly wage (INR)*	*Prob. of being hired*	*Net employ.*
June '19 X Shock	-66.40	-0.01	-0.01	472.73	**0.02****	**0.02****	175.87	-0.00	-0.00	424.18	0.02	0.02
	(392.92)	(0.02)	(0.02)	(666.44)	(0.01)	(0.01)	(941.54)	(0.00)	(0.00)	(894.90)	(0.05)	(0.05)
July '19 X Shock	237.59	-0.02	-0.02	1,163.61	**0.03*****	**0.03*****	1,150.91	0.00	0.00	472.97	0.03	0.03
	(555.50)	(0.02)	(0.02)	(907.74)	(0.01)	(0.01)	(1,308.50)	(0.01)	(0.01)	(1,345.34)	(0.07)	(0.07)
Aug '19 X Shock	184.44	-0.02	-0.02	1,361.27	**0.03*****	**0.03*****	440.12	0.00	0.00	744.64	0.04	0.04
	(928.70)	(0.03)	(0.03)	(946.25)	(0.01)	(0.01)	(1,507.21)	(0.01)	(0.01)	(1,748.89)	(0.10)	(0.10)
Sept '19 X Shock	1,641.12	-0.01	-0.01	**1,801.89***	**0.04*****	**0.04*****	403.40	0.00	0.00	982.67	0.06	0.06
	(1,188.67)	(0.04)	(0.04)	(989.60)	(0.01)	(0.01)	(1,677.29)	(0.01)	(0.01)	(1,907.52)	(0.11)	(0.11)
Oct '19 X Shock	**2,074.55***	-0.02	-0.02	2,738.75	**0.05*****	**0.05*****	1,324.46	-0.00	-0.00	1,408.78	0.10	0.10
	(1,247.47)	(0.04)	(0.04)	(1,731.08)	(0.02)	(0.02)	(2,224.15)	(0.01)	(0.01)	(2,035.41)	(0.12)	(0.12)
Nov '19 X Shock	2,244.27	-0.02	-0.02	664.46	**0.06*****	**0.06*****	-910.24	-0.01	-0.01	2,355.16	0.18	0.18
	(1,395.00)	(0.04)	(0.04)	(1,027.61)	(0.02)	(0.02)	(2,348.74)	(0.01)	(0.01)	(2,259.66)	(0.13)	(0.13)
Dec '19 X Shock	**2,269.89***	-0.02	-0.02	161.61	**0.05****	**0.05****	-47.96	-0.00	-0.00	**3,945.09****	**0.36*****	**0.36*****
	(1,194.24)	(0.03)	(0.03)	(1,056.43)	(0.02)	(0.02)	(2,719.01)	(0.01)	(0.01)	(1,985.78)	(0.10)	(0.10)
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
	Industrial Workers			**Non-industrial Technical Workers**			**Home-based Workers**			**Managers**		
	Monthly wage (INR)	*Prob. of being hired*	*Net employ.*	*Monthly wage (INR)*	*Prob. of being hired*	*Net employ.*	*Monthly wage (INR)*	*Prob. of being hired*	*Net employ.*	*Monthly wage (INR)*	*Prob. of being hired*	*Net employ.*
June '19 X Shock	-36.93	**-0.01***	**-0.01***	100.36	0.01	0.01	244.03	-0.06	-0.06	**5,677.50*****	0.02	0.02
	(245.67)	(0.00)	(0.00)	(572.18)	(0.01)	(0.01)	(545.84)	(0.05)	(0.05)	(1,822.47)	(0.01)	(0.01)
July '19 X Shock	-190.14	-0.00	-0.00	255.95	0.02	0.02	**3,014.69****	0.03	0.03	**5,340.47****	0.02	0.02
	(305.02)	(0.00)	(0.00)	(740.75)	(0.02)	(0.02)	(1,473.96)	(0.11)	(0.11)	(2,604.00)	(0.02)	(0.02)
Aug '19 X Shock	-102.53	-0.00	-0.00	237.25	**0.03***	**0.03***	**3,433.80****	0.05	0.05	**6,734.33***	0.04	0.04
	(385.19)	(0.00)	(0.00)	(997.88)	(0.02)	(0.02)	(1,698.36)	(0.12)	(0.12)	(3,879.70)	(0.03)	(0.03)
Sept '19 X Shock	13.54	-0.00	-0.00	243.62	**0.03***	**0.03***	**4,774.09****	0.15	0.15	5,969.80	0.07	0.07
	(496.05)	(0.00)	(0.00)	(1,101.70)	(0.02)	(0.02)	(2,012.82)	(0.17)	(0.17)	(3,800.42)	(0.05)	(0.05)
Oct '19 X Shock	11.83	-0.00	-0.00	145.66	0.02	0.02	**5,147.48****	0.14	0.14	**7,631.61***	0.09	0.09
	(510.09)	(0.00)	(0.00)	(1,280.46)	(0.02)	(0.02)	(2,088.12)	(0.17)	(0.17)	(4,621.20)	(0.06)	(0.06)
Nov '19 X Shock	-175.92	-0.00	-0.00	886.05	0.02	0.02	**3,667.69****	0.01	0.01	3,234.41	0.13	0.13
	(535.26)	(0.01)	(0.01)	(1,453.13)	(0.02)	(0.02)	(1,596.99)	(0.10)	(0.10)	(4,082.90)	(0.10)	(0.10)
Dec '19 X Shock	-116.09	-0.00	-0.00	862.23	-0.02	-0.02	**3,600.85****	-0.02	-0.02	1,406.04	0.13	0.13
	(646.65)	(0.01)	(0.01)	(1,592.99)	(0.04)	(0.04)	(1,680.34)	(0.11)	(0.11)	(4,241.43)	(0.11)	(0.11)
Observations	2,546,454	2,161,229	2,546,454	2,546,454	2,161,229	2,546,454	2,546,454	2,161,229	2,546,454	2,546,454	2,161,229	2,546,454

*Note: Results come from estimating Equation 2 with an interaction term for occupation. Estimates for columns 4-24 are linear combinations of coefficients. The regressions use standard errors robust to heteroskedasticity and which allow for correlation within districts. All regressions do not consider those working in services sectors, utilities, or screen production, as these sectors could not be well matched with the trade data. Regressions use individual, sector, and month fixed effects, and control for urban status, gender, religion, relation to the household head, household size, literacy, education, and age. All columns use data from 2019 only, but control for values from one year prior to control for cyclicality. As some 2018 values are missing, mean values for 2018 are used and an indicator variable reflecting this missing data is included. The sector shock independent variables are normalized on a scale from -1 to 1. Wage outcomes are in current INR, while employment outcomes are binary variables with 1 indicating employment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

5.3 Effects on linked sectors

Equation 1 to a certain extent accounts for general equilibrium effects of the termination on trade flows. For example, if the GSP termination causes a change in the metals sector that is mediated by changes in tariff lines in the machinery and electrical sector, that change is nonetheless reflected in the reduction in trade flows in the metals sector. In our analysis of labour market impacts, however, the interpretation becomes more difficult. It is best to examine this concretely. When estimating the impacts on individuals working in, say, the metals sector, Equation 2 measures exposure to the GSP termination as the causal effect on Indian exports to the United States. But we see above that a highly linked sector (transportation manufacturing) was deeply affected. It is possible that workers in the metals sector are affected not only by changes in exports in the metals sector but also changes in exports from linked sectors. Changes in exports from linked sectors represent a demand shock to these sectors. In the transportation manufacturing sector, we find that the GSP termination caused a 20 percent decrease in exports to the United States. Perhaps this decrease caused the transporation manufacturing sector to demand fewer inputs from the metals sector.[7] It is relationships such as these that we want to investigate in this section.

More generally, we are curious whether sectors that are forwardly linked with affected sectors might exhibit differential effects than those without such linkages as a result of demand-shock reverberations. To measure this so-called *secondary shock*, we create another shock index that measures exposure to forwardly linked affected sectors (e.g. the metals sector's exposure to a reduction in demand for machinery and electrical products). To measure forward linkages, we use the Asian Development Bank's (ADB) input-output tables for India in 2018. In particular, measure intensity of the forward linkages by looking at the inputs a given sector provides for other sectors. To compare this measure across sectors, we standardize it, considering the monetary value of inputs needed in a given sector to produce US$100 of value added in another sector. The secondary shock index consists of the sum of these linkages weighted by the value of the original shock value:

$$\text{Secondary shock of sector i} = \Psi_i = \sum_{j \neq i} \lambda_{ij} \times \hat{\beta}_1^j \tag{3}$$

where λ_{ij} is the value of inputs of sector i needed to produce US$100 of value added in sector j, and $\hat{\beta}_1^j$ is value of the shock index for sector j. Because the ADB's input-output table does not allow us to identify the pharmaceutical industry, we replace its

[7] Equation 2 captures the effect of the GSP termination on employment outcomes, regardless of whether the sector of employment is linked to other affected sectors or not. While in the long run the GSP termination may serve as a recalibrator of factor prices, employment incentives on the labour supply side, and investment choices that affect production, in the short term it is fundamentally a demand shock that may have knock-on effects throughout the economy.

value for the shock index with the mean. We explore if secondary shocks are present by estimating a revised version of Equation 2, which now includes the secondary shock interaction term:

$$y_{i,j,t} = \alpha_0 + \sum_{m=6}^{12} \alpha_1^m Month_m \cdot \hat{\beta}_1^j \cdot \Psi_j + \alpha_2 y_{i,j,t-12} + \alpha_3 ymiss_{i,j,t-12} + \boldsymbol{\alpha_4 C_{i,j,t}} + \epsilon_{i,j,t} \quad (4)$$

Table 7 and Table 8 contain estimates for Equation 4. The results show that the positive impacts on wages and job retention observed in Table 3 are primarily concentrated in sectors that were simultaneously affected directly by the shock and had forward linkages with other affected sectors (columns 1 and 2 of Table 7. For example, in August 2019, relative to their pre-shock values and to workers sectors that experienced a direct shock only, workers in sectors that experienced both the largest direct shock and largest indirect shock reported wage increases of INR21,135 (Table 7 row 17).[8] Interestingly, sectors with strong direct shocks but weak indirect shocks saw positive impacts to job creation in the last quarter of 2019, but sectors that experienced both dimensions of shocks were significantly less likely to hire (Table 8 column 2). There were not consistent impacts on wages for workers overall (column 5). The main takeaway from this exercise appears to be that the greater the exposure to the GSP termination, the stronger the response by Indian firms to cope with the short-term effects of the shock.

[8]Note that the total effect of the shock in this example is given by summing rows 3 and 17, and the total effect of the secondary shock is given by summing rows 10 and 17.

Table 7: Effects of shocks from forwardly linked sectors

	(1) **Monthly wage (INR)**	(2) **Probability of retaining job**
(1) June '19 X May '19 Sector Shock	-185.115	**0.020***
	(260.273)	(0.011)
(2) July '19 X May '19 Sector Shock	166.387	0.041
	(479.606)	(0.026)
(3) August '19 X May '19 Sector Shock	172.373	0.023
	(554.399)	(0.030)
(4) September '19 X May '19 Sector Shock	803.108	0.023
	(702.730)	(0.029)
(5) October '19 X May '19 Sector Shock	750.965	0.033
	(802.356)	(0.034)
(6) November '19 X May '19 Sector Shock	126.626	0.034
	(803.419)	(0.035)
(7) December '19 X May '19 Sector Shock	558.352	0.046
	(830.985)	(0.038)
(8) June '19 X May '19 secondary Shock	1144.211	-0.018
	(716.287)	(0.016)
(9) July '19 X May '19 secondary Shock	**2070.518****	0.047
	(835.226)	(0.029)
(10) August '19 X May '19 secondary Shock	**2758.142*****	**0.119*****
	(975.252)	(0.038)
(11) September '19 X May '19 secondary Shock	**2802.256*****	**0.153*****
	(1074.206)	(0.038)
(12) October '19 X May '19 secondary Shock	**2348.401****	**0.137*****
	(1144.646)	(0.040)
(13) November '19 X May '19 secondary Shock	**2268.963***	**0.157*****
	(1222.424)	(0.042)
(14) December '19 X May '19 secondary Shock	**2602.501***	**0.170*****
	(1369.470)	(0.044)
(15) June '19 X May '19 Shock X May '19 Secon. Shock	**15084.384*****	0.097
	(3866.388)	(0.095)
(16) July '19 X May '19 Shock X May '19 Secon. Shock	**15630.652*****	**0.279****
	(4178.981)	(0.137)
(17) August '19 X May '19 Shock X May '19 Secon. Shock	**21135.570*****	**0.818*****
	(4504.395)	(0.174)
(18) September '19 X May '19 Shock X May '19 Secon. Shock	**18526.928*****	**1.012*****
	(5126.709)	(0.176)
(19) October '19 X May '19 Shock X May '19 Secon. Shock	**18616.876*****	**0.955*****
	(5294.059)	(0.179)
(20) November '19 X May '19 Shock X May '19 Secon. Shock	**16964.978*****	**1.051*****
	(5711.519)	(0.189)
(21) December '19 X May '19 Shock X May '19 Secon. Shock	**16982.603****	**1.147*****
	(6825.865)	(0.214)
Observations	382,812	382,820

*Note: Results come from estimating Equation 4. The regressions use standard errors robust to heteroskedasticity and which allow for correlation within districts. All regressions do not consider those working in services sectors, utilities, or screen production, as these sectors could not be well matched with the trade data. Regressions use individual, sector, and month fixed effects, and control for urban status, gender, religion, relation to the household head, household size, literacy, education, and age. All columns use data from 2019 only, but control for values from one year prior to control for cyclicality. As some 2018 values are missing, mean values for 2018 are used and an indicator variable reflecting this missing data is included. Columns 1 and 2 only consider those reporting income in May 2019. The sector shock independent variables are normalized on a scale from -1 to 1. Wage outcomes are in current INR, while employment outcomes are binary variables with 1 indicating employment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Table 8: Effects of shocks from forwardly linked sectors

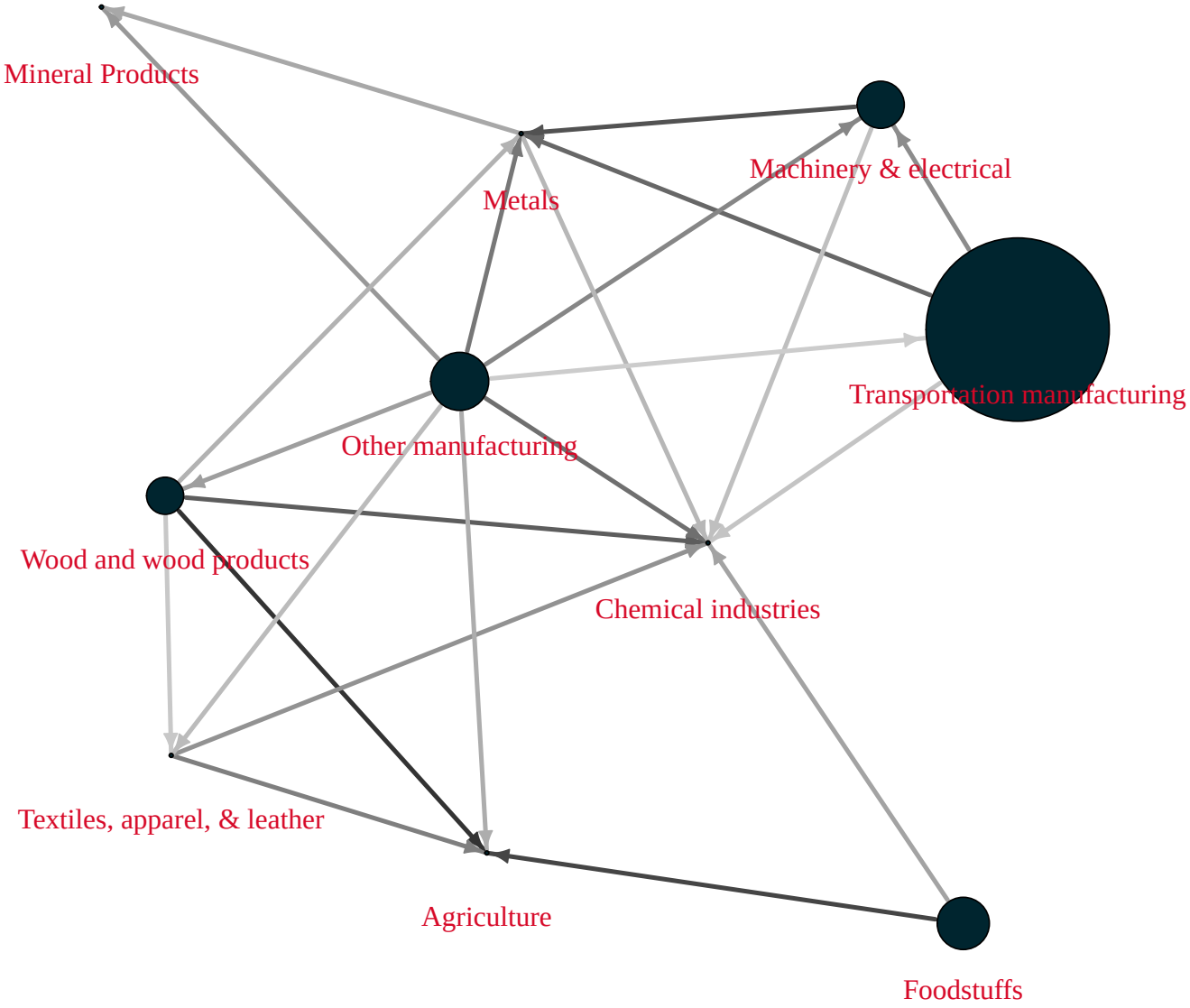
	(1) Monthly wage (INR)	(2) Probability of gaining job	(3) Probability of employment
(1) June '19 X Sector Shock	-51.508	0.002	-0.006
	(278.917)	(0.041)	(0.009)
(2) July '19 X Sector Shock	-37.695	0.015	-0.009
	(369.721)	(0.041)	(0.012)
(3) August '19 X Sector Shock	-221.357	0.035	-0.021
	(510.089)	(0.040)	(0.021)
(4) September '19 X Sector Shock	143.354	0.048	-0.017
	(593.311)	(0.038)	(0.022)
(5) October '19 X Sector Shock	747.601	**0.078****	-0.004
	(672.604)	(0.037)	(0.022)
(6) November '19 X Sector Shock	363.732	**0.092****	0.005
	(666.847)	(0.036)	(0.022)
(7) December '19 X Sector Shock	807.898	**0.122*****	**0.035***
	(649.205)	(0.034)	(0.018)
(8) June '19 X Secondary Shock	**1568.329****	-0.007	0.005
	(731.337)	(0.034)	(0.004)
(9) July '19 X Secondary Shock	**2254.683*****	0.009	0.005
	(741.869)	(0.032)	(0.005)
(10) August '19 X Secondary Shock	**2648.229*****	0.013	0.007
	(720.199)	(0.031)	(0.006)
(11) September '19 X Secondary Shock	**3154.747*****	0.014	0.007
	(730.185)	(0.030)	(0.006)
(12) October '19 X Secondary Shock	**4005.034*****	0.010	0.009
	(741.758)	(0.030)	(0.006)
(13) November '19 X Secondary Shock	**3825.227*****	0.009	0.009
	(826.078)	(0.030)	(0.008)
(14) December '19 X Secondary Shock	**3889.228*****	-0.001	0.005
	(998.445)	(0.031)	(0.009)
(15) June '19 X Shock X Secon. Shock	**8814.756***	-0.295	0.011
	(4492.053)	(0.229)	(0.058)
(16) July '19 X Shock X Secon. Shock	**13955.521*****	-0.107	0.087
	(4604.722)	(0.238)	(0.081)
(17) August '19 X Shock X Secon. Shock	**18404.027*****	-0.193	**0.230*****
	(4497.238)	(0.218)	(0.085)
(18) September '19 X Shock X Secon. Shock	**18761.499*****	-0.271	**0.236****
	(4654.711)	(0.215)	(0.098)
(19) October '19 X Shock X Secon. Shock	**17671.704*****	**-0.602*****	0.057
	(4901.712)	(0.220)	(0.099)
(20) November '19 X Shock X Secon. Shock	**16233.498*****	**-0.629*****	0.065
	(5310.126)	(0.220)	(0.105)
(21) December '19 X Shock X Secon. Shock	**17143.521*****	**-0.744*****	0.013
	(6313.958)	(0.234)	(0.114)
	2,546,454	2,161,229	2,546,476

*Note: Results come from estimating Equation 2 with an interaction term for secondary shock index. The regressions use standard errors robust to heteroskedasticity and which allow for correlation within districts. All regressions do not consider those working in services sectors, utilities, or screen production, as these sectors could not be well matched with the trade data. Regressions use individual, sector, and month fixed effects, and control for urban status, gender, religion, relation to the household head, household size, literacy, education, and age. All columns use data from 2019 only, but control for values from one year prior to control for cyclicality. As some 2018 values are missing, mean values for 2018 are used and an indicator variable reflecting this missing data is included. Column 2 only considers those not reporting income in May 2019. Columns 1 and 3 consider the whole sample. The sector shock independent variables are normalized on a scale from -1 to 1. Wage outcomes are in current INR, while employment outcomes are binary variables with 1 indicating employment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Figure 2 visualizes the main findings in this paper, illustrating the demand shock and how it might pass through the Indian economy. The nodes of the diagram are weighted by the estimated reduction in exports caused by the GSP termination. Arrows indicate linkages between sectors stemming from input-output tables. The direction of the arrows depicts the direction the demand shock might travel, that is, from downstream of the supply chain to upstream. For instance, there is an arrow from transportation manufacturing to metals. The shade of the arrows conveys the strength of the linkage, with darker shades indicating stronger linkages. The diagram supported by the results in this paper shows that the GSP termination had wide-ranging impacts on India's labour market.

Figure 2: Transmission of trade shocks on Indian workers

Nodes are weighted by reduction of trade flows as a result of the GSP termination. Edges reflect backward linkages, with bolder edges indicating stronger links. For visualization purposes, only the strong links are drawn.



5.4 Robustness checks

Estimating models using PPML is quickly becoming the standard in the trade literature using gravity models. As our model is motivated by differences-in-differences and not a gravity model, we estimate Equation 1 using OLS. We follow a long literature of using a scaled log transformation so that zero values are included (Fetzer et al., 2021; Johnson, 2020). As a robustness check, however, we estimate Equation 1 using PPML and include these results in Table A.3. These results are remarkably similar to the OLS estimates shown in Table 2. The PPML estimates convey an more profound impacts than the OLS estimates, suggesting affected products in multiple sectors experienced double-digit percent losses.

For robustness, we also present the results of Equation 2 estimated using PPML in the annex. These results, shown in Table A.3, are qualitatively very similar to those shown in Table 3. Workers employed in affected sectors in May 2019 experienced a wage premium compared to workers in unaffected sectors, a result that lasted through the end of 2019. This went hand in hand with increased likelihood in job retention, reflecting the same countercyclical results observed in other specifications. Effects at the sectoral level indicate some evidence that the sector increased wages in the months following a shock, but these estimates become negative in the final quarter of 2019. They also suggest that affected sectors were less likely to hire workers, although these estimates are not as high and seem to result, if anything, in a net positive effect on net employment.

Moreover, we test the robustness of the first stage estimates by varying the start year of the trade panel dataset. The results included in the body of this paper use data from January 2014 through December 2019. Table A.2 includes results that start the sample at the beginning of 2015, 2016, and 2017. We do not report the results for 2018 because we feel it is important to have at least one pre-termination period that was not affected by the numerous tariffs levied by the U.S in 2018. Estimates in Table A.2 show that the results in Table 2 are robust to the base year, with the same sectors affected in proportional magnitudes regardless of the year.

Our data permit us to explore several GSP-related shocks, including the EU GSP graduations of 2014 and 2017 involving India. These inquiries are the focus of separate research by the authors, but are available in the supplementary appendix.

6 Conclusion

This paper capitalized on the relatively swift termination of India's access to the United States GSP to measure Indian exports to the United States and, therewith, its effects on India's labour market. We show that the termination resulted in a reduction in affected exports of two percent, an effect that lasts for at least seven months following the termination. We find considerable heterogeneity in the sectors affected, with transport manufacturing and pharmaceutical manufacturing experiencing reductions of more than 14 percent, while covered products in some upstream sectors saw increased flows. The sudden changes in exports resulted in a surprisingly countercyclical response in the labour force.

Using data from India's annual survey of industries, we demonstrate that the most impacted sectors are characterized by large, export-driven firms that employ significant shares of workers in their respective sectors. We therefore surmise that these firms responded to the shock with a strategic shift that prioritized some occupations over others to solve the acute problem posed by the GSP termination. The occupational reshuffling resulted in increased wages and employment in more skilled occupations, particularly those not involved directly in production.

With protectionist policies becoming a more common policy utilized by the United States for political, geopolitical, and economic objectives, the impacts termination of India from the United States GSP measured in this paper are becoming increasingly relevant for developing countries across the world. Entering his second term, President Donald Trump has already vowed to raise tariffs on countries like China and Mexico. This paper's conclusions suggest that the labour markets in these countries may experience distributional effects as exporting firms attempt to grapple with the tariffs. That firm responses may disadvantage certain occupations highlights this as an area warranting greater attention from both academic and policy audiences.

ACKNOWLEDGEMENTS: The authors would like to thank Mattia Di Ubaldo for reviewer comments. We also thank members of the Centre for Inclusive Trade Policy (CITP) at the University of Sussex for their valuable feedback.

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A. Annex

Table A.1: Impacts of India's termination from the United States' GSP on trade flows by sector: PPML vs OLS

	Dependent variable: log exports, $\log(x_{i,c,t}+1)$												
Sector	Agriculture	Chemical industries	Foodstuffs	Machinery & electrical	Metals	Mineral Products	Non-metallic mineral products	Other	Other manufacturing	Pharm. manufacturing	Textiles, apparel, & leather	Transport. manufacturing	Wood and wood products
$\hat{\beta}^j_{1,PPML}$	**0.404*** **	**0.231*** **	**-0.234** **	**-0.214*** **	0.005	-0.252	-0.094	**-0.375*** **	-0.109	**-0.769*** **	**-0.123*** **	**-0.495*** **	-0.044
se	(0.085)	(0.06)	(0.098)	(0.047)	(0.058)	(0.223)	(0.068)	(0.121)	(0.098)	(0.195)	(0.045)	(0.128)	(0.107)
N	5,432,104												
Pseudo R^2	0.7872												
$\hat{\beta}^j_{1,OLS}$	**0.036*** **	0.000	**-0.059*** **	**-0.053*** **	**0.021*** **	-0.008	0.023	**-0.038*** **	**-0.065*** **	**-0.141*** **	**0.060*** **	**-0.205*** **	**-0.042*** **
se	(0.007)	(0.004)	(0.007)	(0.006)	(0.007)	(0.015)	(0.017)	(0.016)	(0.01)	(0.011)	(0.011)	(0.022)	(0.011)
N	5,747,736												
R^2	0.840												

*Note: Estimates for β^j_1 come from Equation 1, estimated using PPML. The regression uses standard errors robust to heteroskedasticity and which allow for correlation within 6-digit product lines. Country×Product, Country× Year, and Product× Year fixed effects are included. Data are from 2014 to the end of 2019. Units are logged exports in millions of current US dollars. For the sector J dropped due to multicollinearity, I assume $\beta^J_1 = 0$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Table A.2: Impacts of India's termination from the United States' GSP on trade flows by sector: Changing base year

	Dependent variable: log exports, $\log(x_{i,c,t}+1)$												
Sector	**Agriculture**	**Chemical industries**	**Foodstuffs**	**Machinery & electrical**	**Metals**	**Mineral Products**	**Non-metallic mineral products**	**Other**	**Other manufacturing**	**Pharmaceutical manufacturing**	**Textiles, apparel, & leather**	**Transportation manufacturing**	**Wood and wood products**
$\hat{\beta}^j_{1,2014}$	**0.036*** **	0.000	**-0.059*** **	**-0.053*** **	**0.021*** **	-0.008	0.023	**-0.038*** **	**-0.065*** **	**-0.141*** **	**0.060*** **	**-0.205*** **	**-0.042*** **
se	(0.007)	(0.004)	(0.007)	(0.006)	(0.007)	(0.015)	(0.017)	(0.016)	(0.01)	(0.011)	(0.011)	(0.022)	(0.011)
N	5,747,736												
R^2	0.840												
$\hat{\beta}^j_{1,2015}$	**0.037*** **	-0.001	**-0.051*** **	**-0.049*** **	0.019	-0.011	0.026	**-0.041** **	**-0.061*** **	**-0.152*** **	**0.050*** **	**-0.175*** **	**-0.034*** **
se	**(0.007)**	(0.004)	**(0.006)**	**(0.006)**	(0.007)	(0.016)	(0.016)	**(0.016)**	**(0.009)**	**(0.012)**	**(0.011)**	**(0.021)**	**(0.01)**
N	4,864,057												
R^2	0.849												
$\hat{\beta}^j_{1,2016}$	**0.032*** **	-0.005	**-0.039*** **	**-0.044*** **	**0.011***	-0.022	0.023	**-0.045*** **	**-0.052*** **	**-0.167*** **	**0.053*** **	**-0.129*** **	**-0.017** **
se	**(0.006)**	(0.004)	**(0.005)**	**(0.006)**	**(0.006)**	(0.016)	(0.015)	**(0.015)**	**(0.008)**	**(0.013)**	**(0.011)**	**(0.018)**	**(0.009)**
N	3,984,860												
R^2	0.857												
$\hat{\beta}^j_{1,2017}$	**0.033*** **	-0.008**	**-0.026*** **	**-0.036*** **	**0.018*** **	-0.023	0.018	**-0.05*** **	**-0.04*** **	**-0.193*** **	**0.047*** **	**-0.086*** **	**-0.002**
se	(0.005)	(0.004)	(0.004)	(0.005)	(0.006)	(0.016)	(0.013)	(0.015)	(0.007)	(0.015)	(0.009)	(0.016)	(0.007)
N	3,078,818												
R^2	0.867												

*Note: Estimates for β^j_1 come from Equation 1, estimated using OLS. The regression uses standard errors robust to heteroskedasticity and which allow for correlation within 6-digit product lines. Country×Product, Country× Year, and Product× Year fixed effects are included. Data are from 2014 to the end of 2019. Units are logged exports in millions of current US dollars. For the sector J dropped due to multicollinearity, I assume $\beta^J_1 = 0$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Table A.3: Impacts of GSP termination wage income and dimensions of employment by month, estimated with Poisson Pseudo Maximum Likelihood

	(1) Monthly wage (INR)	(2) Monthly wage (INR)	(3) Probability of retaining job	(4) Probability of gaining job	(5) Probability of employment
June 2019 X May 2019 sector shock	**2145.877*** **		**0.053*** **		
	(343.976)		(0.016)		
July 2019 X May 2019 sector shock	**1340.351*** **		0.030		
	(390.128)		(0.022)		
August 2019 X May 2019 sector shock	**1956.357*** **		**0.071*** **		
	(442.603)		(0.026)		
September 2019 X May 2019 sector shock	**1568.635*** **		**0.080*** **		
	(547.844)		(0.027)		
October 2019 X May 2019 sector shock	**2110.245*** **		**0.087*** **		
	(603.518)		(0.027)		
November 2019 X May 2019 sector shock	**1550.330** **		**0.090*** **		
	(615.379)		(0.028)		
December 2019 X May 2019 sector shock	**1312.426** **		**0.102*** **		
	(635.218)		(0.03)		
June 2019 X Sector shock		-23.681		**-0.084*** **	-0.008
		(388.307)		(0.032)	(0.017)
July 2019 X Sector shock		381.694		-0.049	0.012
		(409.762)		(0.038)	(0.023)
August 2019 X Sector shock		**988.401*** **		**-0.078** **	**0.049** **
		(373.49)		(0.032)	(0.023)
September 2019 X Sector shock		402.778		**-0.100*** **	**0.051***
		(474.384)		(0.036)	(0.027)
October 2019 X Sector shock		**-1109.117***		**-0.187*** **	-0.001
		(634.911)		(0.036)	(0.027)
November 2019 X Sector shock		**-1385.023** **		**-0.188*** **	0.004
		(535.92)		(0.037)	(0.028)
December 2019 X Sector shock		**-1083.656***		**-0.198*** **	0.003
		(610.681)		(0.042)	(0.032)
Observations	382,812	2,546,454	382,820	2,161,229	2,546,476

*Note: Coefficients come from Equation 2, estimated using Poisson Pseudo Maximum Likelihood (PPML). The regression uses standard errors robust to heteroskedasticity and which allow for correlation within districts. All regressions do not consider those working in services sectors, utilities, or screen production, as these sectors could not be well matched with the trade data. Regressions use individual, sector, and month fixed effects, and control for urban status, gender, religion, relation to the household head, household size, literacy, education, and age. All columns use data from 2019 only, but control for values from one year prior to control for cyclicality. As some 2018 values are missing, mean values for 2018 are used and an indicator variable reflecting this missing data is included. Columns 1 and 3 consider only those who report income the month before the termination went into effect (i.e. May 2019) in industrial sectors. Column 4 only considers those not reporting income in May 2019. Columns 2 and 5 consider the whole sample. Columns 2, 4, and 5 use the same controls and fixed effects as columns 1 and 3, except that the sector fixed effect reflect the sector in which the individual works in a given month. The sector shock independent variables capture the estimates for β_1^j and are normalized on a scale from -1 to 1. Wage outcomes are in current INR, while employment outcomes are binary variables with 1 indicating employment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Table A.4: Coverage of US Beneficiary Development Nation GSP by Sector

Sector	Share of HS8 product lines within a given HS6 code that are included in the US' Beneficiary Developing Nation GSP	The share of HS6 codes for which 100 percent of HS8 productlines are included in the US GSP
Agriculture	28.6%	16.4%
Chemical Industries	50.2%	36.0%
Foodstuffs	35.3%	18.5%
Machinery & Electrical Manufacturing	37.0%	27.7%
Metals	46.8%	38.0%
Mineral Products	13.0%	7.1%
Non-metallic Mineral Products	51.1%	39.2%
Other	17.7%	10.7%
Other manufacturing	38.8%	27.0%
Pharmaceutical Manufacturing	5.7%	4.0%
Textiles, Apparel, and Leather	8.4%	2.9%
Transportation Manufacturing	38.0%	19.3%
Wood and Wood Products	37.0%	17.2%
Total	34.7%	24.3%

Table A.5: US share in Indian exports by sector

Year	Sector	Exports to ROW (millions USD)	Exports to USA (millions USD)	US share in Indian exports
2018	Pharmaceutical manufacturing	10,368	5,139	33.1%
2018	Wood and wood products	157	65	29.1%
2018	Other manufacturing	27,654	11,282	29.0%
2018	Textiles, Apparel, & Leather	30,154	9,151	23.3%
2018	Non-metallic mineral products	2,539	662	20.7%
2018	Transportation manufacturing	23,809	4,566	16.1%
2018	Machinery & electrical	23,111	4,317	15.7%
2018	Foodstuffs	23,147	3,789	14.1%
2018	Metals	21,989	3,083	12.3%
2018	Other	6,953	917	11.7%
2018	Chemical industries	80,633	7,135	8.1%
2018	Agriculture	9,724	791	7.5%
2018	Mineral Products	5,071	142	2.7%

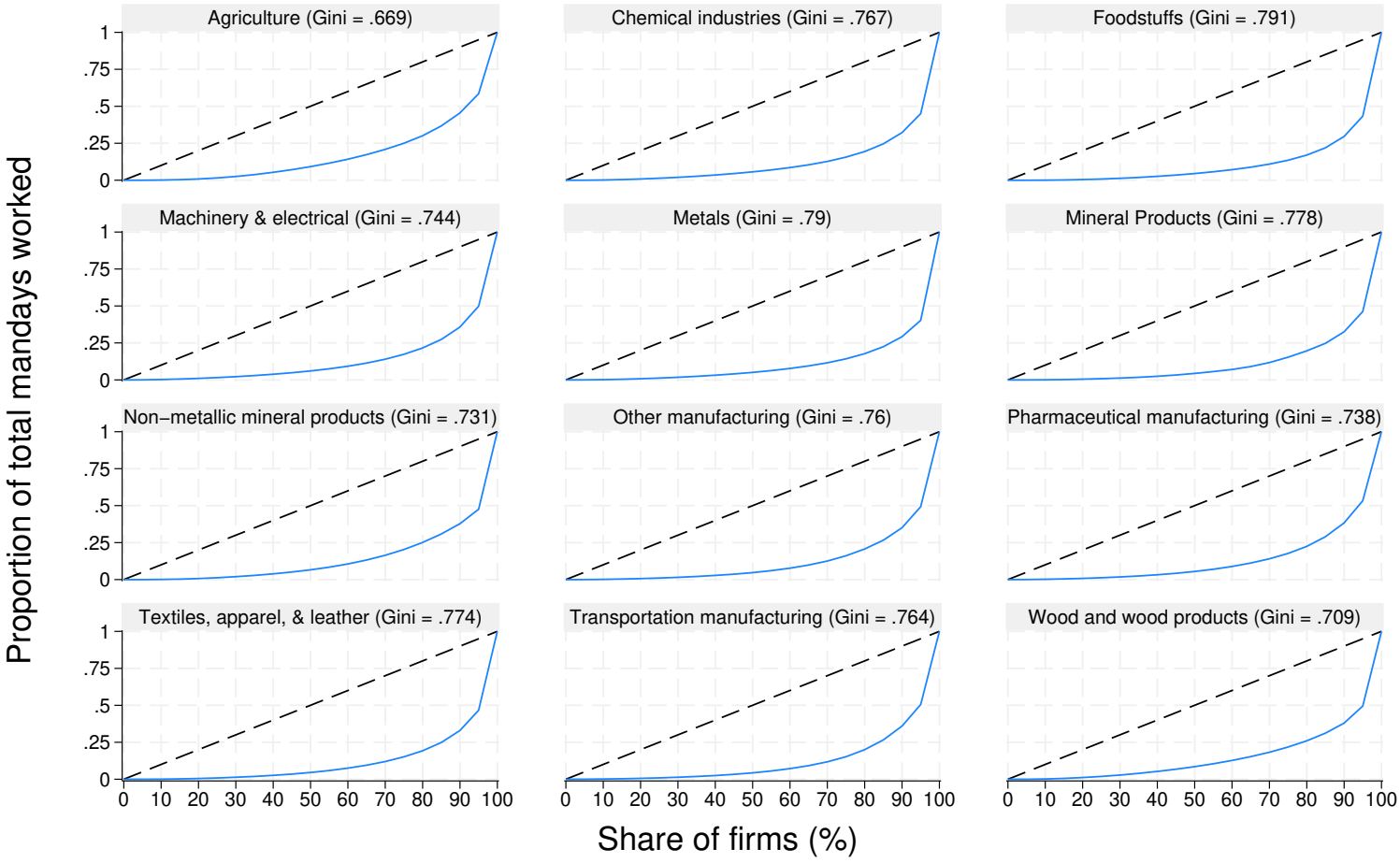
Table A.6: Share of employment in May 2019 by affected sector (%)

Occupation	Foodstuff	Machinery & Electrical	Other manufacturing	Pharm. Manufacturing	Trans. Manufacturing	Wood & Wood Products	Total
Day labourer	10.8	1.2	12.7	1.4	2.8	52.2	6.6
Support staff	18.8	5.3	4.6	23.3	10.4	0.0	10.14
Professionals	2.0	6.0	0.3	9.7	10.2	0.0	4.57
Domestic/unoccupied	0.3	0.2	0.7	2.1	0.2	0.0	0.42
Industrial worker	29.4	83.1	33.3	50.0	62.2	0.0	53.44
Non-industrial technical workers	5.5	2.1	9.4	7.3	3.6	0.0	5.06
Home-based worker	14.7	0.0	18.9	0.0	0.1	0.0	7.83
Manager	0.8	0.8	0.5	2.8	1.3	0.0	0.87
Other	0.6	0.5	1.1	2.8	0.4	47.8	0.9
Total	100	100	100	100	100	100	100

Table A.7: Shock index and demand-driven secondary shock form forwardly-linked sectors

	Secondary shock											
Shock	0.00	0.02	0.03	0.03	0.05	0.05	0.05	0.22	0.27	0.28	0.68	1.00
-0.29					Textiles, Apparel, & Leather							
-0.17												Agric.
-0.10											Metals	
0.00		Non-metalic Mineral Products				Mineral Products			Chemical Industries			
0.19												
0.20							Wood & Wood Products					
0.26										Machinery & Electrical Manufacturing		
0.28	Food stuffs											
0.32			Other Manufacturing									
0.69								Pharm. Manufacturing				
1.00				Trans. Manufacturing								

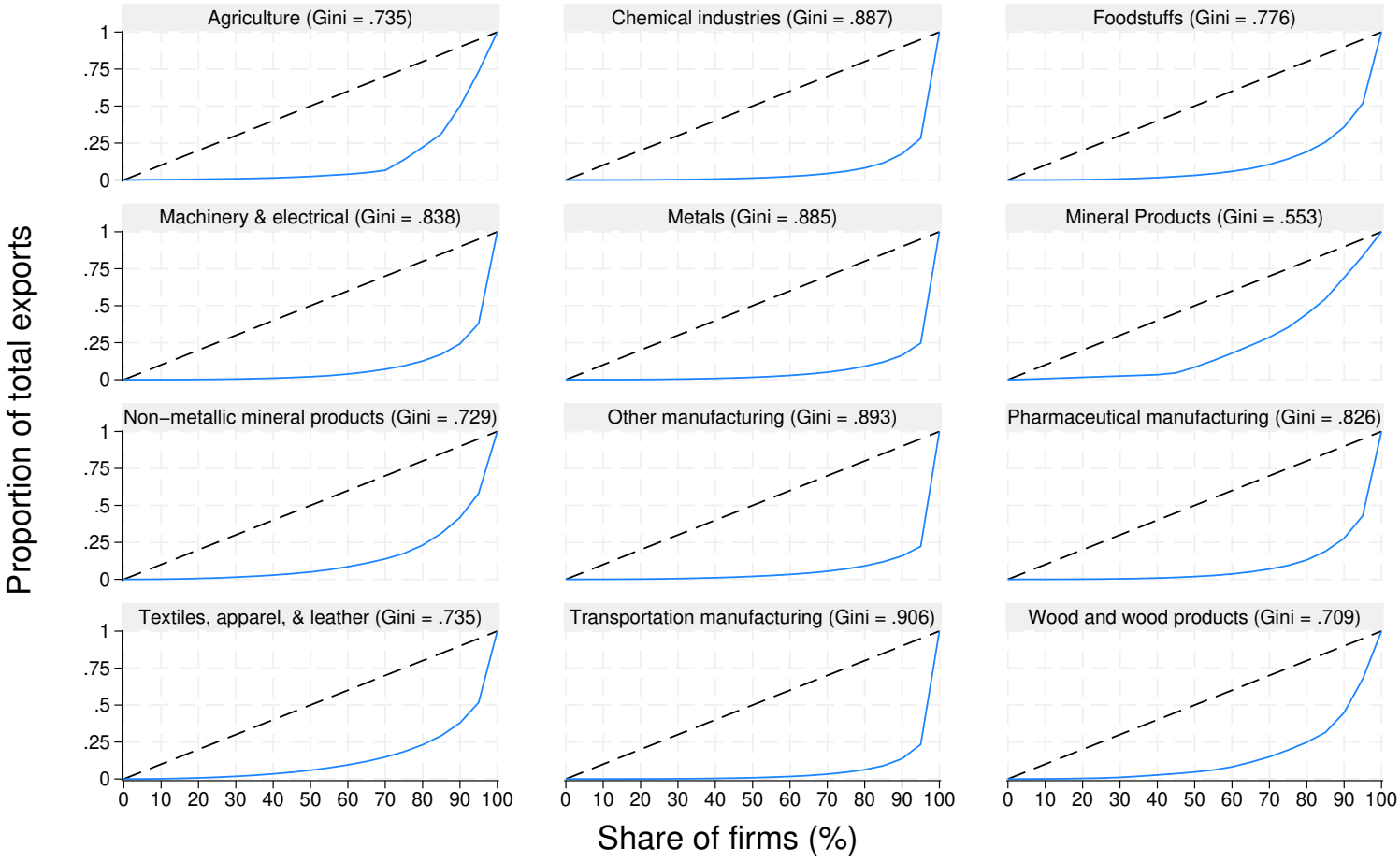
Figure A.1: Large firms account for a outsized share of employment in many of India's industrial sectors



Source: Annual Survey of Industries

Note: Lorenz curve created using data of firms from 2016-18.

Figure A.2: Large firms account for a outsized share of exports in many of India's industrial sectors



Source: Annual Survey of Industries

Note: Lorenz curve created using the subset of firms that exported goods during the period 2016-18.

Figure A.3: Histogram of residual value from Equation 1

