TRADE-OFFS? THE IMPACT OF WTO ACCESSION ON INTIMATE PARTNER VIOLENCE IN CAMBODIA

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Abstract—We study the impact of trade-induced changes in labor market conditions on violence within the household. We exploit the local labor demand shocks generated by Cambodia's WTO accession to assess how changes in the employment of women relative to men affected the risk of intimate partner violence. We document that men in districts facing larger tariff reductions experienced a significant decline in paid employment, whereas women in harder-hit districts increased their entry into the labor force. These changes in employment patterns triggered backlash effects by increasing intimate partner violence, without changes in marriage, fertility, psychological distress, or household consumption.

I. Introduction

MANY developing countries have experienced episodes of rapid trade liberalization over the past three decades. A growing body of empirical work shows that greater exposure to import competition adversely affects local labor market outcomes, particularly for workers initially employed in sectors with larger tariff reductions (Kovak, 2013; Dix-Carneiro & Kovak, 2019; Erten, Leight, & Tregenna, 2019).[1] Recent evidence also indicates that these trade-induced job losses increase the incidence of violent crime by lowering the opportunity cost of criminal employment (Dell, Feigenberg, & Teshima, 2019), reducing public goods provision, and increasing income inequality (Dix-Carneiro et al., 2018). Despite this work on generalized violence, as measured by homicide rates, and the evidence that trade-induced job losses vary by gender (Gaddis & Pieters, 2017), no research has examined the violent consequences of trade liberalization within the household, the burden of which falls disproportionately on women.

This paper examines how trade-induced employment changes affect intimate partner violence in Cambodia. Following Topalova (2010), Dix-Carneiro and Kovak (2017, 2019), Erten et al. (2019), and Erten and Leight (2021), we exploit regional variation in import competition stemming from large-scale import tariff reductions to identify labor-demand shocks with heterogeneous effects on local economies across the country. In examining gender-specific effects of trade-induced labor demand shocks, our work is close in spirit to Gaddis and Pieters (2017), who document a narrowing of the gender gap in employment and participation rates in regions of Brazil that experienced larger tariff reductions, and Juhn, Ujhelyi, and Villegas-Sanchez (2014), who observe that the new firms entering the export market in Mexico after NAFTA replaced male blue-collar workers with female workers as new technologies reduced the need for physically demanding skills. Our study complements the evidence from these episodes of trade liberalization in different contexts by assessing whether regional differences in exposure to import competition across a large set of industries and districts in Cambodia differentially affected men's and women's employment and earnings outcomes. Distinct from much prior work, we analyze how these trade-induced employment changes affect the prevalence of intimate partner violence.

The effects of trade-induced employment changes on the risk of experiencing intimate partner violence are a priori ambiguous. On the one hand, narrowing the gender employment gap could increase the bargaining power of women, increase their outside options, and reduce their exposure to domestic violence (Aizer, 2010; Anderberg et al., 2016). On the other hand, reducing the gender employment gap may increase male partners' incentives to use violence or threats of violence as an instrument to regain control over household decision making, to extract financial resources from women whose relative earnings might have improved, or to force women to work longer hours (Eswaran & Malhotra, 2011; Bobonis, Gonzalez-Brenes, & Castro, 2013; Erten & Keskin, 2018, 2021). However, trade shocks may also widen the gender employment gap, which would result in the opposite effects through the same channels.

In this paper, we shed light on the effects of trade-induced employment changes on multiple dimensions of intimate partner violence by exploiting local labor demand shocks brought about by the Cambodian trade liberalization episode. In 2004, Cambodia became a member of the World Trade Organization (WTO), and in doing so, it implemented large-scale unilateral trade liberalization that had heterogeneous effects on districts across the country. Using detailed industrial employment data from the 1998 Census, which are representative at the district level, we construct a measure of exposure to tariff reductions at the level of the local economy. In particular, we use the 1998 industry employment shares as weights to construct a time-varying weighted average of industry-level tariffs at the district level. This measure exploits a combination of variation in the industrial mix across districts together with variation in cross-industry tariff changes. We report two main results.

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[1]For comprehensive literature reviews on the effects of trade on labor market adjustment and inequality in developing countries, see Goldberg and Pavcnik (2007), Harrison, McLaren, and McMillan (2011), and Pavcnik (2017).

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First, men initially employed in districts facing larger tariff reductions experienced a significant decline in paid employment relative to those in districts facing smaller tariff reductions. In contrast, women employed in harder-hit districts increased their entry into the labor force relative to those in less exposed districts. More specifically, women who were previously outside the labor force began to contribute to family income by working additional hours in family-owned enterprises.

Second, we link trade-induced employment changes to the prevalence of intimate partner violence. Our results show that women in districts more exposed to trade liberalization experienced an increase in intimate partner violence in several dimensions. Exploring alternative mechanisms, we find no evidence of differential changes in marriage rates, fertility, psychological distress, or husbands' behavioral indicators in these districts. These findings are consistent with instrumental theories of violence, which predict the use of violence by men as an instrument for controlling household decision making and/or appropriating resources from women in the form of money or time.

In addition, while we cannot completely rule out the income-stress-conflict nexus—that is, the possibility that increased financial stress triggers violent behavior in the household—we also lack the evidence to support this potential channel. Although we estimate a decline in men's earnings without significant changes in women's earnings, we find no evidence of a decline in household consumption per capita. One possibility is that some of the unpaid work of women could have contributed to the production of some consumption items such as food, partially offsetting some of the decline in paid income through trade-induced job losses.[2]

Our finding of women's increased labor force participation differs from many previous studies that have found either no significant differences by gender (McCaig & Pavcnik, 2018; Erten et al., 2019; Dix-Carneiro & Kovak, 2019) or greater employment losses among men than among women without evidence of an increase in women's employment (Gaddis & Pieters, 2017; Autor, Dorn, & Hanson, 2019). One notable exception is Anukriti and Kumler (2019), who find that in India, women's employment increased relative to men's employment for lower castes in districts more exposed to tariff declines, while the opposite effects were found for upper castes.

Apart from providing support for the view that trade-induced job losses among men coincide with an increase in female labor force participation, our analysis indicates that these trade shocks do not necessarily translate into empowerment within the household. In fact, neglecting such potentially large backlash effects from increased intimate partner violence may yield upward-biased estimates of the societal benefits accruing from trade liberalization.

II. The Context

A. *The Details of Cambodia's WTO Accession*

After Cambodia gained its independence from France in 1953, the new government focused on building an industrial base by investing in infrastructure projects and building factories (Delvert, 1963). These early attempts proved fruitful, increasing the number of small and medium-sized factories from 650 in 1965 to 3,700 in 1968 and increasing state-owned enterprises (SOEs) from 0 to 57 (Ear, 1995). In 1969, Cambodia experimented with a brief period of sharp tariff cuts, which had detrimental effects on the survival of small and some medium-sized firms. This policy was reversed completely by the Khmer Rouge regime that came to power in 1975. This period witnessed one of the most devastating civil wars in world history.[3] The Khmer Rouge regime, which adopted a Maoist ideology, prohibited all international trade flows except those with a few allied communist countries. After the liberation of Cambodia from the Khmer Rouge regime, the new government aimed to eradicate hunger because the civil conflict had taken a large toll on the economy. The SOEs were subsidized to provide basic subsistence goods, and Cambodia received immediate food relief from Vietnam and Soviet-bloc countries. The state monopoly on foreign trade was abolished in 1987, allowing for active engagement of private firms in trade.

However, the important turning point in trade relations began in the post-conflict reconstruction period. Following the fall of the Soviet Union and the subsequent signing of the Paris Peace Accords in 1991, Cambodia began implementing economic reforms for rehabilitation and reconstruction (Thayer, 1998). In 1993, the Kingdom of Cambodia was established, and the newly elected government sought to promote industrial development by engaging in international trade. In particular, the government recognized the potential of the textile industry in world markets and aimed to improve access to the world garment market. It also sought to improve the domestic investment climate by liberalizing goods and service trade. Finally, another goal of the government was to improve access to low-cost drugs through the WTO Declaration on the easing of patents for the least developed countries (Siphana, 2005).

The major trade policy change occurred with the accession of Cambodia to WTO on October 13, 2004. Cambodia became the first least developed country to join the WTO, along with Nepal. During this period, Cambodia reduced its import tariff bands from twelve to four and limited the

[2] Another possibility is that local prices decline in more exposed regions after trade liberalization, offsetting partially the decline in men's purchasing power. Moreover, it might be difficult to detect expenditure changes due to large recall bias in these types of surveys. Hence, these results should be interpreted with caution.

[3] The Cambodian civil war began in 1970, with one side supported by China and the other by the United States, and it was part of the larger Cold War context in which Cambodia was somewhat divided initially between those supporting the Soviet-bloc/Chinese side and those supporting the U.S. side (Chhair & Ung, 2016).

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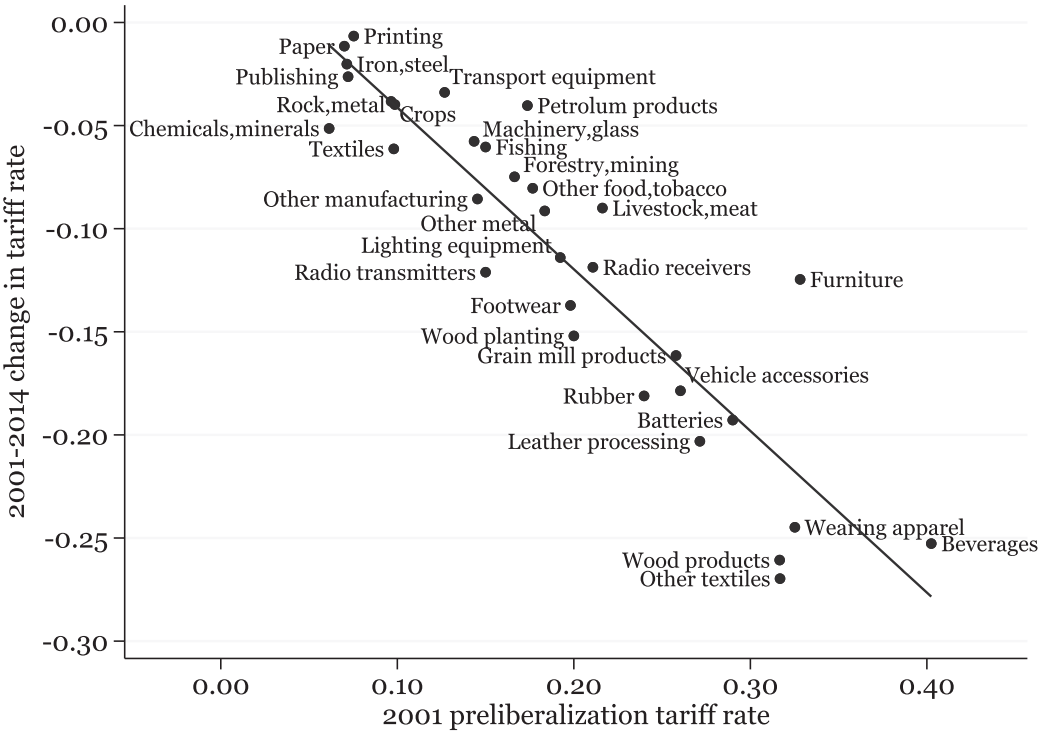
FIGURE 1.—TARIFF REDUCTIONS AND PRE-LIBERALIZATION TARIFF RATES BY SECTOR



This graph shows the total reduction in tariffs between 2001 and 2014 observed by subsector relative to the pre-liberalization tariff rate observed in 2001. Correlation: −0.912; regression coefficient: −0.780; standard error: 0.063; *t*: −12.35. Tariff data are obtained from the WITS–TRAINS database.

tariffs on most imports to 0%, 7%, 15%, or 35%. In the final accession package, Cambodia applied an overall average 16.5% bound tariff rate (Siphana, 2005). The average nominal tariffs fell from nearly 18% in 2001 to 8% in 2014 (appendix figure A1). The Customs Reform and Modernization Program, implemented during the accession process, targeted the share of bound zero-rated tariff lines and was intended to harmonize tariff schedules by reducing the average tariff rates and the number of tariff categories.

We measure trade protection by the import tariff rates (including ad valorem equivalents) imposed by Cambodia. The tariff data are provided by the World Integrated Trade Solution–Trade Analysis Information System (WITS–TRAINS) database. We use the tariff rates reported at the three-digit International Standard Industrial Classification (ISIC) level. We match these detailed tariff data to the industry classification in the 1998 Cambodian Census by constructing a simple average of tariffs within industries using the concordance available in the industry documentation of the 1998 Census. Appendix table A1 provides this concordance table showing the match between the census industry categories and ISIC Revision 3 codes.

B. Exogeneity of Tariff Changes to Industry Performance

Our empirical analysis uses variation in tariff rates across industries and over time to estimate the causal effects of trade liberalization. This estimation strategy assumes that tariff changes are exogenous to the performance of industries subject to tariff cuts, as well as the districts of Cambodia where those industries were concentrated. If political economy considerations dominate such that policymakers were to impose smaller tariff cuts for better-performing industries that lobby for such policies, these endogenous responses would render the exogeneity assumptions invalid.

In the context of Cambodian trade liberalization, the threats to identification resulting from the potential endogeneity of tariff cuts are limited for several reasons. First, the private sector played a limited role in affecting relative tariff declines because the main driver of liberalization policies was the post-conflict government, which was eager to demonstrate its willingness to open up and access world markets (Siphana, 2005). Second, the differential tariff reductions across industries were primarily the outcome of Cambodia's WTO negotiations, during which the government committed to four categories of tariffs (0%, 7%, 15%, and 35%), excluding the possibility of discretionary changes to the tariff structure. Following previous studies (Goldberg & Pavcnik, 2005; Kovak, 2013; Erten et al., 2019), we examine the relationship between initial tariff levels and subsequent tariff liberalizations at the industry level. Figure 1 shows that the industries with the highest tariff levels prior to liberalization experienced the greatest tariff reductions. The correlation between the pre-liberalization tariff rate and the change in the tariff rate is very high (−0.91). Moreover, in subsequent sections, we also provide evidence that districts

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exposed to different levels of tariff reduction during this time period do not demonstrate differential trends in employment outcomes observed prior to the trade liberalization episode.

In appendix figure A2, we examine whether the sectors that experienced the largest tariff declines disproportionately employed women or men or, alternatively, unskilled or skilled workers. In panel A, we plot the total reduction in tariffs between 2001 and 2014 observed by subsector relative to the pre-liberalization female share of employment observed in 1998. We observe a weak correlation of −0.26, and the regression coefficient is not significant at conventional levels. While sectors such as wearing apparel and other textiles had a high share of female employment and also experienced large tariff cuts, there are other sectors, such as wood products and beverages, where the female share of employment is not that high even though these sectors also experienced large tariff declines.[4] Hence, while there is a weak correlation between sectoral tariff declines and female employment share, it is difficult to conclude that the sectors that experienced the largest tariff declines disproportionately employed women. In panel B, we plot the total reduction in tariffs between 2001 and 2014 observed by subsector relative to the pre-liberalization skilled share of employment observed in 1998.[5] The correlation coefficient is −0.003, and the regression coefficient is −0.005 and insignificant. Hence, we conclude that we do not observe a significant relationship between skill intensity and sectoral tariff changes in this time period.

III. Methods

A. Data

To capture labor market outcomes, we use the Cambodian censuses conducted in 1998 and 2008 by the Cambodian National Institute of Statistics.[6] Throughout the analysis, local labor markets are defined as districts. Each district consists of a number of economically integrated contiguous neighborhoods with fairly similar productive and geographic features. We use the census data for two main purposes. First, by using the 1998 Census, we use information on employment status and industry of employment to calculate the industrial distribution of labor in each district. Because Cambodia began its trade liberalization as it joined the WTO in 2004, the 1998 Census data allow us to capture the pre-liberalization industrial composition at the district level. We restrict the sample to individuals aged 15 to 64 to focus on the working-age population. The industry classification in the census consists of forty industries. Second, we use the 2008 Census to represent the post-liberalization period and combine it with the pre-liberalization 1998 Census to estimate the effects of trade liberalization on local labor market outcomes.

Panel A of table 1 provides summary statistics for individuals aged 15 to 64 from the 1998 and 2008 Censuses. On average, men are more educated than women. Close to 60% of individuals are married. Approximately 79% of men are employed, while 74% of women are employed. A slightly higher share of women are unemployed than men; however, the incidence of not being in the labor force (NILF) is significantly higher among women (23%) compared to men (18%). Finally, while 16% of men are working in paid employment, only 8% of women are earning wages for their work. Most women are employed in unpaid work (46%), which includes working without compensation in a family farm or enterprise. In contrast, only 18% of men work as unpaid family workers. Self-employment is also common, as 45% of men and 20% of women work as self-employed workers.

As the census does not report wage information, we use four rounds of an annual household survey, the Cambodian Socioeconomic Survey (CSES), conducted in 1999, 2003, 2009, and 2014, to examine the effects of the WTO accession on wage changes and to conduct a placebo test for the employment effects.

Finally, we use three rounds of Cambodia's Demographic and Health Survey (DHS) conducted in 2000, 2005, and 2014. These are nationally representative household surveys that contain information on women's employment outcomes, their experience of domestic violence, demographics, marriage market outcomes, and other indicators. The surveys targeted women between 15 and 59 years old, and an additional domestic violence module was administered to one-third of the households.[7] One woman per household was randomly selected for the interviews. No one else was present in the room during the interviews, and the respondents were informed that their answers would be kept confidential to minimize reporting bias. The empirical analysis focuses on a balanced panel of 145 districts, for which data are available in every survey year of analysis.

Panel B of table 1 presents summary statistics for major indicators of ever-married women using the DHS surveys. The DHS data include several binary variables on whether a woman experienced various forms of physical, sexual, or psychological violence from her intimate partner. We use the incidence of experiencing different forms of intimate partner violence over the past twelve months as the main outcome variables. For example, physical violence is an indicator variable that takes a value of 1 if the respondent

[4] Notably, textiles had a very high share of female employment while experiencing only modest declines in tariffs. Similarly, sectors such as batteries, rubber, vehicle accessories, grain mill products, and wood planting experienced modest tariff declines, while these sectors' female employment shares are rather low.

[5] Skilled workers are defined as those who completed secondary school or higher.

[6] The Cambodian Census data sets are available for only these two years.

[7] The DHS final reports indicate that one-third of households were randomly selected to be included in the domestic violence module (National Institute of Statistics of Cambodia, 2015). In addition, they report that special weights were constructed to adjust for the selection of one woman per household and to ensure that the domestic violence sample was nationally representative.

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TABLE 1.—SUMMARY STATISTICS

A: Census data, individuals 15–64 years old	Men (1) Mean (S.D.)	Women (2) Mean (S.D.)	Difference (3) Est. (S.E.)
Years of schooling	5.51 (3.69)	3.88 (3.55)	1.63*** (0.01)
Completed less than primary school	0.50 (0.50)	0.69 (0.46)	−0.18*** (0.00)
Completed primary school	0.42 (0.49)	0.28 (0.45)	0.14*** (0.00)
Completed secondary school	0.06 (0.24)	0.03 (0.17)	0.03*** (0.00)
Completed university	0.02 (0.13)	0.01 (0.08)	0.01*** (0.00)
Married	0.61 (0.49)	0.60 (0.49)	0.01*** (0.00)
Employment	0.79 (0.41)	0.74 (0.44)	0.05*** (0.00)
Unemployment	0.02 (0.15)	0.03 (0.17)	0.00*** (0.00)
Not in labor force (NILF)	0.18 (0.39)	0.23 (0.42)	−0.04*** (0.00)
Paid employment	0.16 (0.37)	0.08 (0.28)	0.08*** (0.00)
Unpaid employment	0.18 (0.38)	0.46 (0.50)	−0.28*** (0.00)
Self-employment	0.45 (0.50)	0.20 (0.40)	0.25*** (0.00)
Observations	576,898	653,071	1,229,969

B: DHS ever-married sample, women 15–59 years old	Mean	S.D.	Obs.
Women's employment	0.81	0.40	33,509
Physical violence	0.11	0.32	7,776
Injury	0.05	0.23	7,777
Sexual violence	0.03	0.17	7,776
Psychological violence	0.16	0.37	7,773
Decision-making index	0.04	0.93	7,522
Married	0.88	0.32	33,593
Divorced, widowed, or separated	0.12	0.32	33,593
Has children	0.92	0.27	33,593
Has young children (under 5 years of age)	0.48	0.50	33,593
Number of children	3.22	2.36	33,593
Number of young children (under 5 years of age)	0.62	0.72	33,593
Husband is jealous when respondent talks to other men	0.21	0.40	7,730
Husband accuses respondent of unfaithfulness	0.12	0.33	7,740

The table presents the means, standard deviations, and number of observations for demographics, labor market outcomes, women's empowerment, and husband's indicators. The sample in panel A includes the working-age population of 15- to 64-year-old individuals from the 1998 and 2008 Cambodian Census. The sample in panel B includes ever-married women from the 2000, 2005, and 2014 Demographic and Health Surveys of Cambodia.

reported experiencing one of the following violent acts from her partner in the last twelve months: slapping or throwing an object that would hurt; pushing, shoving, or pulling hair; hitting with the partner's fist or in a way that hurts; kicking, pushing on the ground, or beating; and choking or burning.[8] In addition, following Erten and Keskin (2018) and Anderson (2008), we show the robustness of our results constructing indices by averaging the z-scores of the underlying domestic violence indicators over the past twelve months.[9] Higher values indicate higher levels of violence. Following a similar method, we create a decision-making index, which is the average of the z-scores from the following indicators: having decision-making power to decide on her own health care, her own friends, or whether to make large household purchases.[10]

We observe that approximately 11% of women report experiencing physical violence over the last twelve months and 5% of them incurred injuries from such violence. Approximately 3% of women report experiencing sexual violence and 16% report experiencing psychological violence over the last twelve months. Compared to other low-income countries, the prevalence of intimate partner violence does not appear particularly high. The World Health Organization reports that the incidence of intimate partner violence in the last twelve months was highest among the least developed

[8]Other measures are similarly defined. Injury is an indicator variable that takes a value of 1 if the respondent reported experiencing one of the following injuries because of violent behavior from her partner in the last twelve months: bruises, light injuries, and severe injuries due to violent acts by one's partner. Sexual violence is an indicator variable that takes a value of 1 if the respondent reported experiencing one of the following violent acts from her partner in the last twelve months: forced into unwanted sex or forced into other unwanted sexual acts. Psychological violence is an indicator variable that takes a value of 1 if the respondent reported experiencing one of the following acts from her partner in the last twelve months: insulting, humiliating, and scaring or threatening.

[9]We standardize each indicator and calculate the simple average of these z-scores to create indices. The physical violence index is constructed by averaging the z-scores of five indicators for the male partner: slapping or throwing an object that would hurt; pushing, shoving, or pulling hair; hitting with the partner's fist or in a way that hurts; kicking, pushing on the ground, or beating; and choking or burning. The injury index is the average of the z-scores from the following indicators: bruises, light injuries, and severe injuries due to violent acts by one's partner. The sexual violence index is the average of z-scores from the following indicators: forced into unwanted sex and forced into other unwanted sexual acts. The psychological violence index is the average of the z-scores from the following indicators: insulting, humiliating, and scaring or threatening.

[10]We use this index instead of an indicator variable to capture the decision-making power of women because an indicator variable that takes a value of 1 when the respondent reports having power to make any of these decisions will have a mean of 99% in our context.

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countries, with 22% of women experiencing physical and/or sexual violence from their partners (World Health Organization, 2021). However, for low- and middle-income countries in the Western Pacific region (where Cambodia is located), the incidence is 8%. In contrast, in Cambodia, the same incidence of physical and/or sexual violence over the last twelve months for the most recent 2014 DHS data is 11%. Although Cambodia emerged from a very violent period of civil conflict, it does not seem to be an outlier in terms of intimate partner violence prevalence; it actually appears less violent compared to the least developed countries and only slightly more violent compared to low- and middle-income countries in its region.

B. *Identification*

Following Cambodia's WTO accession, the level of import tariffs varied significantly across industries and over time. There was also substantial heterogeneity in the industrial composition of Cambodian districts prior to trade liberalization. Consequently, based on their initial industrial composition of employment at the time of the reform, some districts were more exposed to tariff declines than were others. Building on a large body of empirical work (Topalova, 2010; Dix-Carneiro & Kovak, 2019; Erten et al., 2019), our identification strategy relies on this relative exposure to isolate the causal effect of trade liberalization.

In particular, following Hasan, Mitra, and Ural (2007), Kovak (2013), and Erten et al. (2019), our measure of regional exposure to trade liberalization for district d in year t, $Tariff_{dt}$, is constructed by interacting the national ad valorem applied tariff rate faced by industry i in year t, $Tariff_{it}$ with the share of tradable employment in industry i and district d in 1998, $Empshare_{id}^{1998}$, as reported in the 1998 Census data.[11] This includes forty traded industries represented in the data set.

$$Tariff_{dt} = \sum_{i} Empshare_{id}^{1998} \times Tariff_{it}. \tag{1}$$

In cases where we have two rounds of data, we use the following reduced-form specification to compare outcomes of interest for workers located in districts exposed to larger versus smaller tariff reductions:

$$y_{jdt} = \alpha + \beta Tariff_{dt} + \theta X_{jdt} + \sigma z_{dt} + \mu_t + \gamma_d + \lambda \mu_t \Delta y_{d,pre} + \epsilon_{jdt}, \tag{2}$$

where y_{jdt} denotes outcomes for individual j in district d in year t, $Tariff_{dt}$ is the district tariff in district d in year t, X_{jdt} is a vector of worker characteristics, including age, indicator variables for four educational categories (completed less than primary school, completed primary school, completed secondary school, and completed university), years of schooling, and an indicator for whether an individual lives in a rural area. To capture changes in external trade barriers that occurred during the WTO accession, we include z_{dt}, a weighted average of external tariff barriers faced by Cambodia's exports in key partner countries.[12] The specification also includes year fixed effects (μ_t), district fixed effects (γ_d), and $\Delta y_{d,pre}$, the pre-liberalization change in the outcome from 1996 to 1999.[13] We cluster standard errors at the district level to account for serial correlation in outcomes within districts. The main parameter of interest is the coefficient on district tariffs, with a positive coefficient implying that a decrease in district tariffs is associated with a decline in the probability of the outcome tested.

In cases where we have more than two rounds of data, we modify equation (2) by replacing the pre-liberalization change in the outcome ($\Delta y_{d,pre}$) with district-specific linear time trends ($\delta_d t$) to better control for unobserved time trends specific to each district over time:

$$y_{jdt} = \alpha + \beta Tariff_{dt} + \theta X_{jdt} + \sigma z_{dt} + \mu_t + \gamma_d + \delta_d t + \epsilon_{jdt}, \tag{3}$$

where $\delta_d t$ denotes district-specific linear time trends and all other variables are defined as in equation (2).

The inclusion of year fixed effects in equations (2) and (3) controls for any macroeconomic shocks at the national level that coincide with trade liberalization. District fixed effects control for any time-invariant heterogeneity across districts. District-level linear trends account for changes in time trends specific to each district across years. Individual-level worker demographic characteristics control for differences in demographic composition across districts and over time that could influence outcomes and produce spurious correlations with district tariffs. Hence, this specification compares outcomes for individuals with the same observable characteristics who are exposed to different local trade shocks due to their initial regions of residence.

[11] We follow Kovak (2013) in calculating the industry weights based on the traded sector because, as Kovak (2013) has shown, assigning a zero weight to the nontradable sector greatly overstates the labor market impact of trade liberalization by implicitly assuming no price changes in the nontradable sector. Instead, as Kovak (2013) proposed, we exclude the nontradable sector from the industry weights, implicitly assuming a proportional decline in nontradable sector prices in the local labor market. However, we acknowledge that the measure proposed by Kovak also includes the cost share of labor in the calculation of industry weights; as we do not have this information for Cambodia, our measure is more in line with Hasan et al. (2007) and Erten et al. (2019). Because the earliest tariff data are available in 2001, we use these data when calculating district tariffs in 1998 and 2000. For other survey years, we use the tariff data for that year (e.g., for 2008 DHS, we use the tariff data available for that year). There are also a few years missing in the post-2001 period in the tariff data. In these cases, we take the average of tariffs one year before and one year after the missing year (e.g., for 2009 CSES, we take the average of tariffs in 2008 and 2010).

[12] Using import tariff data from the WITS–TRAINS database, we construct a weighted average of import tariffs that the top five export partners (the United States, Hong Kong, Germany, United Kingdom, and Singapore) imposed on Cambodian goods.

[13] We calculate these pre-liberalization changes in outcome variables using the 1996 and 1999 CSES data and interact them with a time dummy to control for potential unobserved pretrends in outcome variables.

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TABLE 2.—TRADE LIBERALIZATION AND LABOR MARKET OUTCOMES

A: Employment Status Outcomes			
	Employment (1)	Unemployment (2)	NILF (3)
I. Men			
District tariff	−0.008	0.004	0.005
	(0.007)	(0.003)	(0.006)
N	576,898	576,898	576,898
II. Women			
District tariff	−0.036***	0.008**	0.029***
	(0.012)	(0.003)	(0.010)
N	653,071	653,071	653,071
III. Test of coefficient equality between women and men			
p-value	0.009	0.112	0.021
B: Type of Employment Outcomes			
	Paid employment (1)	Unpaid employment (2)	Self-employment (3)
I. Men			
District tariff	0.013**	0.006	−0.028***
	(0.006)	(0.006)	(0.006)
N	576,898	576,898	576,898
II. Women			
District tariff	−0.002	−0.024	0.005
	(0.013)	(0.015)	(0.007)
N	653,071	653,071	653,071
III. Test of coefficient equality between women and men			
p-value	0.143	0.004	0.000

Data are from the 1998 and 2008 Cambodian Census. In panel A, the dependent variables are indicator variables for being employed, unemployed, and not in the labor force (NILF). In panel B, the dependent variables are indicator variables for paid employment (i.e., working for pay), unpaid employment (i.e., working as an unpaid family worker), and self-employment (i.e., working as an own-account worker). The independent variable is the district tariff variable constructed using employment subsector weights as measured in 1998 and industry-specific tariffs over time. All specifications are estimated conditional on district fixed effects, year fixed effects, pre-liberalization change in the outcome variable, a weighted average of external tariff barriers faced on Cambodia's exports to key partner countries, and individual-level covariates, including age, indicator variables for four educational categories (completed less than primary school, completed primary school, completed secondary school, and completed university), years of schooling, and an indicator variable for living in a rural area. Robust standard errors in parentheses are clustered at the district level.

IV. Effects of Cambodia's WTO Accession

A. *Labor Market Outcomes*

We begin by examining the effects of Cambodia's trade liberalization during its WTO accession on labor market outcomes. In table 2, we report the results of estimating equation (2) for employment status outcomes in panel A and employment type outcomes in panel B. In each panel, we divide the sample by gender to examine gender-specific responses to the trade shock. In panel A, we observe no evidence of a significant impact of trade liberalization on the probability of men's employment, unemployment, or not being in the labor force. Since total employment changes might mask underlying margins of adjustment by type of employment, panel B reports results by type of employment. The positive and significant coefficient estimate in the first row of column 1 of panel B indicates that male workers located in districts that faced greater reductions in import tariffs experienced larger declines in the probability of paid employment relative to those in districts exposed to smaller tariff reductions. The magnitude of the coefficient (0.013) implies that workers in a district exposed to a 1 standard deviation decline in tariff rates experienced a 0.04 percentage point decline in the probability of working for pay relative to workers in districts not exposed to any declines in tariffs. Columns 2 and 3 indicate that men located in regions more exposed to import competition increased their self-employment, without experiencing significant changes in their unpaid employment. These results imply that in harder-hit districts, men's probability of working for pay declines, while their probability of self-employment increases, resulting in a null effect for total employment, compared to less affected districts.[14]

In the second row of panel A in table 2, we examine the impact of trade liberalization on women's labor market outcomes. In column 1, we observe that female workers located in districts that experienced larger declines in import tariffs experienced an increase in their probability of employment relative to women in districts exposed to smaller tariff reductions. The magnitude suggests an increase in the probability of employment of 0.1 percentage points for a district facing a 1 standard deviation decline in tariff rates. Moreover, the estimates in columns 2 and 3 indicate that women in regions more exposed to import competition experienced a decline in their probability of not participating in the labor market and of being unemployed. The bottom section of panel A shows that the differences between men and women are statistically significant for employment and not being in the labor force. Moreover, the estimates in panel B indicate an imprecisely estimated increase in employment of women as unpaid family workers in family-owned enterprises.

These findings could suggest an added worker effect—a well-established pattern observed in studies of labor market adjustment (Lundberg, 1985; Cullen & Gruber, 2000; Stephens, 2002; Gong, 2010)—in which a reduction in the employment probability of men induces more women to participate in the labor market by actively seeking work and taking up new employment opportunities to compensate for the income loss experienced by their husbands.

The identification assumption for the main specification requires that the reduction in district tariffs in this period is orthogonal to other trends observed at the district level. This assumption would be violated if the reduction in tariffs were designed to protect districts with relatively weaker local economies ex ante. We previously mentioned that tariff liberalization during WTO accession was intended to harmonize tariffs such that industries with the highest tariff levels ex ante faced the largest declines in tariffs. In appendix table A2, we provide further evidence on pretrends using data from the 1999, 2003, 2009, and 2014 rounds of the CSES. In panel A, focusing on the pre-accession period preceding the WTO accession in 2004, we find no evidence of a

[14] Note that the sum of coefficients in panel B may not be equal to the coefficient of employment reported in column 1 of panel A since the pre-liberalization change in the outcome variable differs for each regression in panel B given the different outcomes being estimated. However, if this term is replaced by the pre-liberalization change in total employment, the sum of coefficients in panel B is equal to the employment coefficient in column 1 of panel A.

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TABLE 3.—TRADE LIBERALIZATION AND INTIMATE PARTNER VIOLENCE

	Women's employment (1)	Women's employment (2)	Physical violence (3)	Injury (4)	Sexual violence (5)	Psychological violence (6)	Decision-making index (7)
District tariff	−0.057***	−0.041*	−0.035**	−0.015*	−0.017**	−0.060***	0.119**
	(0.015)	(0.024)	(0.016)	(0.009)	(0.008)	(0.021)	(0.055)
N	33,476	7,748	7,745	7,747	7,745	7,742	7,496

Data are from the 2000, 2005, and 2014 Cambodia DHS. Dependent variables include an indicator variable for whether the respondent is employed; indicator variables that take a value of 1 if the respondent experienced physical violence, was physically injured, experienced sexual violence, or experienced psychological violence; and a *z*-score index for having decision-making power. In all specifications, the independent variable is the district tariff variable constructed using employment subsector weights as measured in 1998 and industry-specific tariffs over time. All specifications are estimated conditional on district fixed effects, year fixed effects, district-specific linear time trends, a weighted average of external tariff barriers faced on Cambodia's exports to key partner countries, and individual-level covariates, including age, indicator variables for three educational categories (completed primary school, completed secondary school, and completed higher education), years of schooling, indicator variables for literacy level, and an indicator variable for living in a rural area. Robust standard errors in parentheses are clustered at the district level.

significant change in the probability of men's or women's employment.[15] In contrast, in the post-accession period, the estimates in panel B reveal a significant increase in the probability of women's employment and no significant change in the probability of men's employment for districts that were more exposed to tariff declines, consistent with the results reported in table 2.

In appendix table A3, we examine whether exposure to tariff reductions had a significant impact on the monthly earnings of men and women. In panel A, relying on the sample of individuals who reported earning a monthly income, we find a significant decline in the monthly earnings of men, whereas we find no evidence of a significant impact on women's earnings. In panel B, we impute wages using observed predetermined characteristics of workers, such as age, educational categories, marriage status, rural indicator, and gender (Rubin, 1987; Schenker & Taylor, 1996). We again observe significant declines in earnings only for men. This result is in line with the finding that men experience a significant decline in paid employment, while we find no evidence of such a change for women, as reported in table 2.[16]

In appendix table A4, we further investigate whether exposure to tariff reductions had a significant effect on household consumption per capita. We construct a consistent measure of household expenditures across several categories over the last twelve months using the 1999, 2009, and 2014 rounds of the CSES data. In the first three columns of appendix table A4, we observe positive but imprecisely estimated coefficients for total consumption, food consumption, and nonfood consumption. The remaining columns present estimates for different categories of nonfood consumption, including clothing, domestic workers' salaries and furniture expenditures, personal care, personal effects, and medical expenditures.[17] We observe mostly imprecisely estimated coefficients for these nonfood expenditure categories, except for clothing expenditures, which has a positive coefficient that is marginally significant. Overall, we find weak evidence of effects of trade liberalization on household consumption measures in Cambodia. Although it is plausible that trade-induced financial stress triggers more violent behaviors from men toward their female partners, we do not find robust evidence to support this potential channel in this context.

Altogether, these findings indicate that men initially employed in districts that faced greater tariff reductions experienced a greater loss in paid employment. The paid employment loss among men and the associated decline in their earnings motivated women to enter the labor market by working predominantly as unpaid family workers.

B. Intimate Partner Violence Outcomes

The differential labor market effects of trade liberalization by gender that we documented in the previous section have a priori ambiguous effects on women's empowerment within the household. On the one hand, an increase in women's employment probability relative to her husband could improve her bargaining power within the household by increasing her access to resources and improving her outside options. Consequently, women's economic empowerment may result in a decline in their exposure to intimate partner violence (Aizer, 2010; Hidrobo & Fernald, 2013; Anderberg et al., 2016). On the other hand, the greater probability of a woman's employment relative to her partner may also increase the partner's incentives to use violence or threats of violence as an instrument to regain control over household decision making, to extract financial resources from women whose relative earnings might have improved, or to force women to work longer hours (Bloch & Rao, 2002; Bobonis et al., 2013; Erten & Keskin, 2018, 2021, forthcoming). Moreover, an increase in women's bargaining power through better employment opportunities may trigger backlash from their partners, who may prefer that their wives do not work (Field et al., 2021).

In columns 1 and 2 of table 3, we estimate the effects of tariff reductions on women's probability of employment using the full DHS sample and the sample to which the

[15] In particular, we conduct a placebo check by assigning the same tariffs as in table 2 to the pre-liberalization CSES data. That is, we assign the 1998 district tariff measure to the 1999 household survey and the 2008 district tariff measure to the 2003 household survey.

[16] This change in earnings could be partly explained by the effects of increased international competition on taste-based discrimination; in particular, increased competition with foreign firms may put pressure on domestic firms to decrease gender pay differentials by reducing taste-based discrimination (Black & Brainerd, 2004).

[17] Note that the time horizons over which consumption measures are reported do not always overlap with the time horizons over which domestic violence incidence is reported. Hence, these estimates should be interpreted with caution.

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domestic violence module was administered. The coefficient estimates are similar in magnitude, and they corroborate the results from the census in that women in harder-hit districts experienced an increase in their probability of employment relative to women in less affected districts.[18]

In the remaining columns of table 3, we examine the effects of trade liberalization on the risk of experiencing intimate partner violence. In column 3, we find that women in more exposed districts faced increased physical violence from their partners. The magnitude suggests an increase in the probability of experiencing physical violence of 0.1 percentage points for a district facing a 1 standard deviation decline in tariff rates.[19] The estimate in column 4 also indicates that these women experienced increases in the physical injuries they suffer from such violence, including bruises, broken bones, and other physical injuries, which is plausibly a more objective measure of physical violence. The remaining estimates show that women in harder-hit regions experienced increased sexual and psychological violence as well as reduced decision-making power. In appendix table A5, we show that these results are robust to using z-score indices to measure intimate partner violence.[20]

Moreover, we explored whether exposure to tariff reductions generated heterogeneous effects by education and age. In panel A of appendix table A6, we interact the district tariff measure with an indicator variable of lower education that takes a value of 1 if the respondent has fewer than the median years of schooling (which is three years), controlling for lower education and district tariff. We observe no evidence of significant heterogeneous treatment effects for women's employment and only one indicator for intimate partner violence (injury) is marginally significant with a coefficient that is very close to null. We also find a small decline in decision-making power for this group. In panel B, we interact the district tariff measure with an indicator variable of being younger than the median age of 30, controlling for being younger and district tariff. The interaction coefficients are all very close to 0, and only an indicator of intimate partner violence (sexual violence) is marginally significant. Hence, we conclude that there is not much evidence of significant heterogeneity in treatment effects by education or age in the Cambodian trade liberalization context.

To summarize, this evidence is consistent with instrumental theories of violence, which predict that an increase in women's relative employment opportunities creates incentives for men to use violence or other controlling behavior as an instrument for regaining control over household decision making and/or appropriating women's income. In this case, such extraction could take the form of women's working hours, given the increase we observe in unpaid work of women in family enterprises. Because we estimate a similar impact on more objective measures of violence that reflect violence-related injuries, reporting bias is unlikely to explain our results in this context.

Finally, certain individuals who were initially living in districts that experienced larger tariff reductions may have relocated to less affected districts. Such interregional migration could potentially affect our results, particularly if men and women are now physically separated while seeking new employment opportunities. Using the census data, we test whether there is a significant difference in the probability of in-migration into harder-hit districts compared to less affected regions after trade liberalization began. Appendix table A7 presents the results. In panel A, we observe no evidence of a significant impact of trade liberalization on migrating into more affected districts. This result holds for the entire sample as well as for men and women. In panel B, we find no evidence of a differential in-migration effect for less educated individuals compared to more educated individuals. In panel C, we find that younger individuals have a lower probability of migrating into harder-hit districts compared to less affected regions. However, as shown in appendix table A6, we find no evidence of differential employment or domestic violence effects for the same age group using the DHS data. Hence, there is little reason to believe that differential migration could explain our results.

C. *Alternative Channels*

Although our findings indicate a change in the risk of experiencing intimate partner violence that is largely driven by a trade-induced exogenous shock to labor markets, in this section, we explore other potential channels through which import competition could generate changes in the domestic violence that women experienced. First, as reported in a recent study by Autor et al. (2019), labor demand shocks driven by international competition may reduce the probability of marriage for young adults and change their fertility profiles, which can in turn affect intrahousehold dynamics. In panel A of table 4, we examine whether exposure to import competition had any significant impacts on women's

[18] We cannot examine the labor market outcomes for men because the DHS does not contain the corresponding employment questions for men.

[19] Relatedly, the incidence of physical violence in an average district declined by 7.4 percentage points from 2000 to 2014. We estimate that the trade-induced increase in the incidence of physical violence for the mean district is 0.3 percentage points. This estimate implies that districts exposed to average tariff reduction experienced a 4% (0.3/7.4) slower decline in the incidence of physical violence.

[20] The results in table 3 demonstrate that in more exposed districts women are more likely to work on average and women are more likely to experience intimate partner violence on average. To explore whether these are the same women and whether violence is used as a tool to force women to work, we followed a straightforward exercise by creating new outcomes that are the interaction of the violence measures and indicators for being employed and not being employed. Since these are mutually exclusive, they sum to the total effect on the various violence measures and provide further insight into how working and intimate partner violence are related. Our findings indicate that the bulk of the estimated effects on intimate partner violence measures are driven by working women as the coefficients for the interaction of violence measures and being employed are highly significant and large, while the coefficients for the interaction of violence measures and not being employed are generally small and insignificant for most outcomes. However, these differences are statistically significant only for physical violence and the decision-making index. This provides suggestive evidence that intimate partner violence is used as a tool to force women to work, particularly as unpaid workers in family enterprises.

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TABLE 4.—ALTERNATIVE CHANNELS

	A: Women's marital status		B: Fertility outcomes			
	Married	Divorced widowed separated	Has children	Has young children	Number of children	Number of young children
	(1)	(2)	(3)	(4)	(5)	(6)
District tariff	−0.021	0.015	0.005	−0.057	−0.003	−0.065
	(0.021)	(0.022)	(0.021)	(0.037)	(0.091)	(0.043)
N	7,771	7,771	5,882	5,882	5,882	5,882
	C: Psychological problems		D: Husband's behavior			
	Men	Women	Husband is jealous when respondent talks to other men	Husband accuses respondent of unfaithfulness		
	(1)	(2)	(3)	(4)		
District tariff	−0.001	0.001	−0.007	−0.005		
	(0.001)	(0.001)	(0.037)	(0.024)		
N	47,171	52,985	7,722	7,732		

Data are from the 2000, 2005, and 2014 Cambodia DHS for all panels except panel C, where the data are from the 2003, 2009, and 2014 CSES. Dependent variables in panel A are indicator variables for whether the respondent is married or whether she is divorced, widowed, or separated. Dependent variables in panel B are indicator variables for whether the respondent has children, whether the respondent has children under the age of 5, the number of children the respondent has, and the number of children under the age of 5 that the respondent has. Dependent variables in panel C are indicator variables for whether the respondent reports that he or she experienced psychological or emotional difficulties or has become extremely upset within the last month. Dependent variables in panel D are indicator variables for whether the respondent's husband is jealous when the respondent talks to other men and whether the respondent's husband accuses the respondent of unfaithfulness. All panels report reduced-form regression results using the district tariff variable as an explanatory variable. All specifications are estimated conditional on district fixed effects, year fixed effects, district-specific linear time trends, a weighted average of external tariff barriers faced on Cambodia's exports to key partner countries, and individual-level covariates, including age, indicator variables for three educational categories (completed primary school, completed secondary school, and completed higher education), years of schooling, indicator variables for literacy level, and an indicator variable for living in a rural area. Robust standard errors in parentheses are clustered at the district level.

marital status. The estimates reported in columns 1 and 2 show no evidence of a significant effect on the probability of being married or on the probability of being divorced, widowed, or separated. Similarly, in panel B, we find no evidence that trade liberalization had a significant impact on the probability of having children, the probability of having children under the age of 5, the number of children in total, or the number of children under the age of 5.[21]

Another plausible mechanism through which trade-induced employment changes could increase intimate partner violence is the deterioration of individuals' psychological well-being. For instance, losing a job can lead to significant distress in men, which may trigger angry outbursts, resulting in violent episodes. Using data from the 2003, 2009, and 2014 CSES, we investigate whether exposure to larger tariff reductions had a significant impact on the prevalence of psychological problems. Our findings presented in panel C indicate no evidence of a significant change in psychological problems experienced by men or women.[22]

Finally, another concern is that the increased employment of women could lead to more interactions with men outside the home, which may in turn make their husbands upset and jealous. In panel D, we examine whether exposure to import competition had a significant effect on husbands' behavioral indicators. The estimates in columns 1 and 2 in the second row of table 4 show no evidence of a significant impact on the probability that the husband would become jealous when the respondent talked to other men or on the probability that the husband would accuse the respondent of unfaithfulness.

D. Robustness

We estimate several alternative specifications to examine the robustness of our results. In appendix tables A8, A9, and A10, we use the log of the district tariff instead of the level as the explanatory variable. We find that the estimates are consistent in sign and magnitude. In the next set of results, we explore whether the estimates are robust to excluding industries that were outliers in terms of tariff declines. In particular, in appendix tables A11, A12, and A13, we reconstruct an alternative measure of district tariffs excluding industries that experienced the highest tariff declines; these industries include beverages, wood products, and other textiles. Similarly, in appendix tables A14, A15, and A16, we reconstruct another alternative measure of district tariffs excluding industries that experienced the lowest tariff declines, which include the printing, paper, publishing, and iron and steel industries. The results in both cases are consistent with our main findings. Finally, we calculate the district tariff measure excluding the five sectors that appear to deviate from the linear relationship between the initial tariff level and the size of the tariff reduction. These off-diagonal industries include petroleum products, furniture, other textiles, wood products, and radio transmitters. The results reported in appendix tables A17, A18, and A19 are consistent with our main findings.

Moreover, we conduct two robustness checks to examine the variation driving our shift-share design. First, in

[21] We estimate these regressions using a sample of women who are in their childbearing ages for the entire period between 2004 and 2014. Specifically, women must be born in or before 1989 to be 15 or older in 2004, and they must be born in or after 1964 to be 50 or younger in 2014. We thank an anonymous referee for this point.

[22] To capture the incidence of psychological problems, we construct a dummy variable that takes a value of 1 if the respondent reported that he or she experienced psychological or emotional difficulties or had become extremely upset within the last month.

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appendix table A20, we follow Goldsmith-Pinkham, Sorkin, and Swift (2020) to decompose our shift-share estimator for demonstrating the variation that the estimator is exploiting. Panel A presents the results for women's employment, and panel B presents the results for physical violence using the baseline results in table 3. In panels A.I and B.I, we observe that the correlation between Rotemberg weights (α_k) and tariff rates (g_k) is fairly low. This finding is similar to the canonical setting of estimating the inverse elasticity of labor supply discussed by Goldsmith-Pinkham et al. (2020). In contrast, the elasticities have a higher correlation with the variation in industry shares across locations ($var(z_{lk})$). Panels A.II and B.II show that the top five instruments account for over 85% (0.867/1.052) of the positive weight in the estimator. These top five instruments are manufacturing of wearing apparel, fishing, manufacture of furniture, manufacture of meat products, and farming of animals.[23] Furthermore, we visually inspect the dispersion in point estimates across instruments in appendix figure A3. This figure shows the heterogeneity in the instruments' estimates (β_k) and the relationship to the first-stage F-statistic. Panel A presents this relationship for women's employment, while panel B presents this relationship for physical violence. We observe that there is not a great deal of dispersion in point estimates across instruments. In addition, the high-weight industries are all very close to the overall point estimate. We observe that while there are negative weights, these industries form a small share of the overall weight.

Second, as Adao, Kolesár, and Morales (2019) show, the regression residuals in specifications involving shift-share designs are likely to be correlated across regions with similar initial sectoral composition. To address this potential problem, following Campante, Chor, and Li (2019) and Dai, Huang, and Zhang (2021), we construct an alternative clustering scheme by creating a similarity index capturing the degree of similarity in initial industry employment composition across districts. In particular, we calculate the similarity index for each district relative to all other districts in its initial employment share. We construct the similarity index as follows:

$$SimilarityIndex_{ab} = \sum_i min\{Empshare_{ai}, Empshare_{bi}\}, \quad (4)$$

where $Empshare_{ai}(Empshare_{bi})$ is the initial employment share of industry i in district $a(b)$. The similarity index ranges from 0 to 1, with higher values indicating a more similar industry employment composition. Appendix table A21 presents the results of our main regressions from table 3 using alternative methods of clustering. In the first row, we replicate the same results as in table 3 by clustering standard errors at the district level. In the second row, we estimate standard errors by two-way clustering at the district and the district with the highest similarity index. We observe that the estimates remain statistically significant under this alternative clustering scheme.

[23] However, it should be noted that we have only 40 industries in the Cambodian Census data, which is considerably fewer than the approximately 400 industries in the examples used for demonstration in Goldsmith-Pinkham et al. (2020). A better comparison might examine the share of the top instrument in the total, which accounts for 54% of the positive weight in the estimator. The latter proportion is very similar to the share of top five instruments in positive weight for the China shock example in Goldsmith-Pinkham et al. (2020).

V. Conclusion

In this paper, we study the effects of Cambodia's WTO accession on the risk of intimate partner violence. Our findings indicate that men in districts more exposed to tariff reductions experienced a decline in paid employment, whereas women in these districts increased their entry into the labor force. Although previous studies have shown that trade-induced changes in labor market conditions in disproportionately affected regions leads to more violent street crime, they did not examine whether these trade shocks can also increase the prevalence of domestic violence. Indeed, our analysis shows that women suffer from an increased risk of intimate partner violence in response to such trade shocks.

Our findings have broader implications for the distributional consequences of trade policy. To the extent that exposure to trade shocks brings about changes in male-female employment gaps, these trade-induced employment changes are likely to have significant effects on intrahousehold bargaining dynamics. In many contexts where exposure to import competition resulted in greater job losses for men than women, such increases in the relative employment of women are likely to generate important changes in the prevalence of intimate partner violence.

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