ABSTRACT

Title of dissertation: ESSAYS ON THE IMPACT OF CONFLICT AND REGULATIONS ON THE PRIVATE SECTOR IN DEVELOPING COUNTRIES

Trang Thu Tran, Doctor of Philosophy, 2013

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My dissertation explores the effect of ethnic conflict, or regulations meant to avoid ethnic strife, on firm employment and productivity in developing countries. The first chapter investigates the impact of the conflict in Cote d'Ivoire that began in 2000, using a census of all registered firms for the years 1998-2003. We use structural estimates of the production function and exploit spatial variations in conflict intensity to derive the cost of conflict on firms in terms of productivity loss. The results indicate that the conflict led to an average 16-23% drop in firm total factor productivity and the decline is 5-10 percentage point larger for foreign firms. These results are consistent with anecdotal evidence of attacks on and looting of foreigners and their businesses during the conflict. We also find evidence to support the hypothesis that firms responded by hiring less foreign workers.

The second chapter studies affirmative action policies in Malaysia, focusing on a specific policy in the private sector. In particular, I examine the impact of a regulatory change which no longer requires foreign-owned manufacturers above a certain

size to reserve 30% equity for (ethnic) Malay shareholders. I set up a theoretical model to show that the original policy results in a range of firms to stay inefficiently small. Removing this equity requirement for foreign firms leads to two effects: (i) foreign firms become less likely to be sized constrained, and (ii) their average size increases relatively to other firms. These predictions are supported by empirical evidence from difference-in-difference estimations, based on firm-level data from the Malaysia Productivity and Investment Climate Survey in 2002 and 2007.

Finally, chapter three examines the relationship between labor standards and market power in imports in a cross-country context. The hypothesis is that since labor standard policies can act as a substitute for import tariffs, all else equal, bigger importers would have lower labor standards. IV estimation with geography-based instruments finds evidence consistent with theory. In general, countries with higher market shares in labor intensive imports tend to have weaker Free Association and Collective Bargaining rights. Moreover, the effect is stronger among GATT members.

ESSAYS ON THE IMPACT OF CONFLICT AND REGULATIONS ON THE PRIVATE SECTOR IN DEVELOPING COUNTRIES

by

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Preface

My dissertation consists of three empirical chapters on the private sector in developing countries. I am particularly interested in the implications of ethnic diversity and ethnicity-based conflict on economic activities. My first two chapters share this common theme. The first chapter, based on coauthored work with Leora Klapper and Christine Richmond, studies the impact of an ethnic conflict on firm productivity and employment in Cote d'Ivoire. The second chapter explores the impact of an affirmative action policy on employment of manufacturing firms in Malaysia, another country with stark economic divides along ethnic lines. Together, these two chapters touch on different aspects of the economic consequences from the political deliberation process to affect resource distribution among ethnic groups. In a sense, both the state-sponsored violence and affirmative action policies studied in the first two chapters can be thought of as a means to alter the "terms of trade" between different ethnic and nationality groups within a country for political gains. In the third chapter, I study whether labor standards policies are used to manipulate the ("classical") terms of trade in a broader cross-country setting.

My work in this dissertation belongs to the economic literature on the private sector in developing countries. Until recently, this literature has remained relatively limited due the lack of firm-level data. Understanding what affects the growth in size and productivity of firms in the private sector is however one of the key questions in development. The private sector, especially the manufacturing sector, has long been considered a crucial element of modern economic growth and a source of various

positive spillovers (Kuznets 1973, Tybout 2000). A vibrant private sector is also instrumental to job creation. According to the World Development Report (World Bank 2013), youth employment in developing countries is widespread and growing. Moreover, the majority of those who are employed work in very small, low wage and low skilled firms and farms. The problem of how to expand employment, especially skilled non-agricultural work, through the expansion of firms is therefore a pressing global concern (Blattman at. el 2013). This dissertation seeks to contribute to this body of literature by studying the interactions between various distortions - caused by either violent conflict or government policies - and economic outcomes both at the firm and cross-country level.

Chapter 1 highlights the asymmetric cost that an ethnic conflict has on different types of firms. We consider the conflict in Cote d'Ivoire which began in 2000 and eventually culminated into a Civil War in 2002. During the conflict, anti-northerners and anti-foreigners sentiments are tolerated and even encouraged by politicians, leading to disproportional increases in violent attacks on and looting of immigrants/foreigners and their businesses. As a result, our analysis focuses on the differential impact of the conflict on foreign and domestic firms. We employ on a unique data set constructed from the census of all registered firms for the years 1998-2003. We use structural estimates of the production function to derive firm-level TFP estimates and exploit spatial variations in conflict intensity to derive the cost of conflict on firms in terms of productivity loss. The results indicate that the conflict led to an average 16-23% drop in firm TFP. Importantly, the decline is 5-10 percentage point larger for firms that are owned by or employing foreigners. Furthermore, we find evidence suggesting

that firms responded by hiring less foreign workers.

While the first chapter looks the impact of ethnic conflict on firms, the second chapter examines the impact of a policy meant to avoid ethnic strife. More specifically, I consider the set of affirmative action policies under the New Economic Policy in Malaysia. These policies were first implemented in 1971 to correct for the long standing ethnic inequality between native Malays and the Chinese minority, and cover a wide range of pro-Malay measures in education, public sector employment and businesses. I focus on one aspect of the policies in the private sector. In particular, I examine the impact of a regulatory change in 2003 which no longer requires foreign-owned manufacturers above a certain size threshold to set aside 30% of their equity for Malays shareholders. I set up a simple theoretical model to show that the original policy results in a distortion that renders a range of firms to stay smaller than their optimal employment size. Removing this equity requirement for foreign firms leads to two effects: (i) foreign firms become less likely to be sized constrained, and (ii) their average size increases relatively to other firms. My empirical analysis is based on two rounds of the Malaysia Productivity and Investment Climate Survey in 2002 and 2007. Results from difference-in-differences estimations find evidence consistent with these the predictions. I find that employment in foreign firms increases significantly by 18-19% and those that are below the size threshold in the first period are also 20% less likely to report as being smaller than optimal.

In the last chapter, I turn to cross-country data to find evidence for whether domestic policies are used to alter the terms of trade between countries. The motivation comes from the terms of trade hypothesis which predicts that all else equal, countries

with higher market power in imports would set higher tariffs due to positive welfare gains from lower import prices. The trade literature has found empirical support for this prediction in the context of non WTO countries. My research asks a related questions, which is whether countries constrained in setting their tariffs would use domestic policies, in particular - labor standards- as a substitute for trade policy. My empirical analysis is based on instrumental variable estimation with geography-based instruments to account for endogeneity of trade and income. I find that in general, countries with higher market shares in labor intensive imports tend to have weaker Free Association and Collective Bargaining rights. Moreover, the effect is stronger among GATT/WTO members than among non-member countries, consistent with theoretical predictions.

Dedication

To my parents.

Acknowledgments

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Chapter 1. Civil Conflict and Firm Performance: Evidence from Cote d'Ivoire

(with Leora Klapper and Christine Richmond)

1 Introduction

More than a quarter of the world's population lives in countries affected by instability and violent conflicts, many recurrent events (World Bank 2011). Previous studies have shown the devastating consequences of war and conflict on various human capital outcomes such as health and education, however, less is known about how the private sector is affected and how firms adapt during such situations, primarily due to a lack of data. Nonetheless, understanding these effects is important for at least two reasons: (i) the private sector is identified as an important engine of growth for developing countries; and (ii) reconstruction policies require the allocation of scarce resources. By understanding the impact of conflict on the private sector, resources can be appropriately allocated to improve the prospects for recovery.

This paper fills a gap in the literature by studying the impact of conflict on firms in the context of Cote d'Ivoire. Cote d'Ivoire presents an interesting case study since it

has a relatively large private sector,[1] with systematic collection of information on registered firms since the 1970s. Economic fluctuations and power struggles characterize the early 1990s, but prior to experiencing its first coup d'tat on Christmas Eve 1999, most observers still considered serious political violence to be unlikely in a country that had enjoyed an uninterrupted 30-year period of political stability. The 1999 coup marks the beginning for an episode of social unrest and eventually a Civil War in 2002. At the center of Cote d'Ivoire's crises were issues of ethnicity and nationality, fueled by political tactics exploiting discontent between its southern populations and the large migrant populations from neighboring countries.

Our analysis makes use of detailed financial and employment data from the census of formal firms for the years 1998-2003. We document that starting in 2000, firms enter at a decreasing rate and exit at an increasing rate. Aggregate employment also decreases. Moreover, average output is 20-40% lower than in the pre-conflict period. These patterns coincide with the surge of violence starting in 2000. However, this period of violence in Cote d'Ivoire is also coupled with major macroeconomic shocks, including large swings in commodity prices and a collapse of foreign aid.[2] As a result, even though these conflict events are largely unanticipated, it is not possible to identify the common macro component of the conflict impact with the data at hand.

The paper focuses on disentangling the non-macro component of the conflict im-

[1] In Cote d'Ivoire, the formal private sector accounts for 60% of GDP (Berthelemy and Bourguignon 1996).

[2] Cocoa prices - Cote d'Ivoire most important export - dropped substantially from 1998-2000 before rebounding in 2002 and 2003 (The Economist 2003). Much of the country's foreign aid was suspended in 2000 as the World Bank, IMF, and EU withdrew support to Cote d'Ivoire in response to the 1999 coup (The Economist 2000).

pact associated with certain firm characteristics. In particular, we test whether firms owned by or employing foreigners were impacted disproportionally. We aim to identify (i) the magnitude and heterogeneity in the costs of conflict associated with firm ownership and employment; (ii) potential transmission mechanisms of these heterogeneous effects; and (iii) how firms respond to conflict intensity. The focus on foreign ownership/employment is motivated by anecdotal evidence which suggests increasing anti-foreigner sentiments during this period led to disproportional attacks and looting targeting foreigners and foreign businesses.

Given the absence of data on the direct costs and losses due to violence and conflict, we propose a 2-step approach to infer the magnitude of these losses from the available data. In the first step, we estimate production functions following Olley and Pakes (1996) and Ackerberg, Caves and Frazer (2006) to correct for endogeneity of input use and firm exit. We then use parameter estimates from the production functions to recover a measure of firm total factor productivity (TFP). Since TFP is essentially a residual that contains any information not captured by observed inputs, output losses that firms incur due to violence and conflict will be reflected in this variable. In the second step, we regress the estimated TFP on measures of foreign ownership, foreign employment and conflict. To test whether foreign firms are impacted differently, we interact foreign ownership/employment with conflict variables, exploiting the fact that the coup in 1999 came as an unanticipated shock so the following civil unrest is exogenous to individual firms. To proxy for conflict, we use both a period dummy and an intensity variable which makes use of the geographical variations in conflict pattern and detailed information on firm location.

The results indicate that foreign owned firms and firms employing foreigners have higher productivity on average but that this advantage over domestic firms was reduced by the conflict. Having at least one foreign employee is associated with an extra 9.7% reduction in firm's measure TFP - or a 9.7% decline in output holding other inputs constant - during the conflict period. Foreign ownership does not appear have an effect until 2003, when it is associated with a 18.8% decline in TFP. In the specifications that rely on conflict intensity, we find an increase of one standard deviation in the conflict rate reduces TFP by 10-11% on average and each additional percentage point increase in the share of foreign employees increases the conflict impact on TFP by about one percentage point. In aggregate, the implied annual impact of conflict on firm's TFP from different specifications ranges from 16-23%. These effects are qualitatively similar when we used a modified Ackerberg, Caves and Frazer's procedure to control for simultaneity between productivity shocks and the choice of foreign employment, along similar line of Van Biesebroeck (2005) and De Loecker (2007). The results are also robust to alternative definitions of industry and different specifications of the production functions to allow for changes in expected investment returns and factor prices over time.

We investigate possible channels of impact, and find that industry concentration and export orientation have no significant impact on TFP, suggesting the conflict had no effect on firms' ability to charge mark-ups. On the other hand, TFP decreases significantly for for firms in import oriented industries, indicating rising cost of imported inputs might be a channel of impact. Finally, we find evidence supporting a simple model of labor adjustment in which firms reduce the shares of foreign workers

at any given relative wage to compensate for their negative effect on productivity.

The contribution of our paper is three-fold. First, we utilize a unique dataset which covers all formal private sector firms in the economy, both before and during the conflict. Hence our analysis is representative of all formal firms and is longitudinal, unlike most existing research on firms and conflict which is often limited by cross section and/or survey data. Second, by measuring the impact on productivity, our methodology enables us to overcome data restrictions to estimate the conflict's economic costs. Since all regressions control for firm and industry-specific time effects and other observable characteristics, our estimated impact is net of any common macroeconomic trends or shocks and likely represents the lower bound impact of the conflict. Finally, the question of an asymmetric impact on foreigners is important since foreign investment plays a crucial role in Cote d'Ivoire[3] and other developing countries. Moreover, if firms are affected because of their identity, it implies the conflict creates an additional distortion to the reallocation of resources, further lowering potential aggregate output.

The remainder of the paper is organized as follows: Section 2 describes Cote d'Ivoire's institutional background in more detail and motivates our hypotheses. Section 3 discusses the related literature on conflict and productivity. Section 4 presents a brief conceptual framework and our empirical strategy. Section 5 describes the data and preliminary analysis and section 6 presents the econometric results, including a discussion on firm's responses in terms of labor composition. Section 7 concludes.

[3] For example, French investment constitutes 25% of total capital invested in Ivorian firms (World Market Research Center 2004) while 55% of tax revenues in Cote d'Ioivre comes from French companies (BBC 2004).

2 Background

Cote d'Ivoire's first coup d'etat on Christmas Eve 1999 marked the beginning of a violent episode and eventually a civil war that divided the country along north and south lines (see figure 1.1 for a summary of events). Initially staged by middle-rank military officers as a response to low wages in the military, the coup came as a complete surprise. A report by the BBC (2000a) noted:

> The people of this city had never experienced a coup d'etat before, ... hundreds of local people were standing frozen on the pavements, staring at the soldiers in complete disbelief.

Before this event, Cote d'Ivoire had been a much more stable country. Despite having some 60 ethno-language groups and 25% of the population made up of immigrants, it managed to maintain economic growth and political stability for more than 30 years after independence. While a policy of "Ivoirite" - which sought to redefine who constitutes "true" Ivorians - had increased social tension during the mid 1990s, the economy was showing signs of improvements after a devaluation package in 1994.

The 1999 coup greatly increased political uncertainty (The Economist 2002). Robert Guei, the coup's leader, initially promised to step down after cleaning up the toxic political environment created by his predecessor's "Ivoirite" policy. Instead, he decided to run for president only a few months later and further enforced the same policy that requires both parents of any presidential candidate to be Ivorian citizens (Kohler 2003). This excluded Alassane Ouatarra, a candidate originally from the North, from running. The general election in October 2000 threw the country into chaos. Lau-

rent Gbagbo, the opposition candidate, won but disputed results set off widespread violent clashes among Guei and Gbagbo's supporters. Legislative elections held later in December also saw unprecedented violence by pro-Gbagbo youth and militias attacked northerners and immigrants for their supposed support for Ouatarra's party (McGovern 2011, Marshall-Fratani 2006).

Gbagbo ran on an anti-French and anti-foreigners political platform. The latent nationalism of his party became state policy and resulted in widened mistrust and deepening ethnic and regional divisions within the country (Marshall-Fratani 2006, Kohler 2003). On September 19, 2002, military troops originating from the North, mutinied and attacked major cities throughout the country, claiming control over the northern region and establishing Bouake (the second largest city in Cote d'Ivoire) as its base. During 2003-2004, violence escalated. Massacres of civilians took place especially in the South West and many presumed northerners in Abidjan neighborhoods were arrested and killed (McGovern 2011). Actions by the international community also provided an excuse for the pro-Gbagbo "Young Patriots" to stage violent riots and attacks against West Africans, French and other foreigners.

While most fighting ended in 2004, the country was effectively split between north and south by a "confidence zone" - set up by international peace keeping forces - between the rebels and government forces (United Nations 2009). The reconciliation process was not abided by and elections originally set for 2005 were pushed back several times until late 2010. The 2005-2010 was described as a "no war, no peace" situation characterized by uncertainty, severe disruptions of services and widespread small armed conflicts (McGovern 2011).

Figure 1.2 maps the conflict rate by department for the period before and after 2000 which shows a clear increase in the overall conflict intensity in the later period. While social tension had increased in the country, the consensus among observers is that violence at the scale witnessed in the 2000 elections and the election outcomes were largely unanticipated before 2000.[4] Thus it seems reasonable to assume that prior to the 1999 coup, the turn of events leading to the election crisis and the civil war in 2002 were unexpected to most individuals and firms. Based on this, we argue that the conflict starting in 2000 was an exogenous shock.

3 Related literature

Our research contributes to the empirical literature on the economic consequences of violence and civil conflicts. Until recently, this literature has largely been dominated by cross-country and cross-region analyses. Not surprisingly, past research often finds civil wars and political crises to have a negative economic cost, particularly in the short run. For example, Cerra and Saxena (2008) estimate that on average, output contracts by 18% immediately following civil wars in a cross section of 190 countries. Abadie and Gardeazabal (2003) study terrorist activities in the Basque country and find a 10 percent gap between GDP per capita in Basque and a comparable "synthetic" region.

Though informative, macroeconomic studies by nature are unable to address het-

[4] The Economist (1998) for example, assesses Mr Gbagbo as "unlikely ever to come to power". An investment guide published in 1999 asserts that "Historically, private investment has not been targeted" and "As the 2000 elections approach, further disturbances are likely but serious violence has not characterized Ivorian political life in the past and is not expected to do so in the foreseeable future" (Africa-Asia Business Forum: http://www.aabf.org/cote_divoire_inv_guide.htm)

erogeneity associated with individual conflicts and the differential impact of conflict on different groups within a country/region. More studies are now adding evidence at the micro level, but due to data constraints, most of this research focuses on analyses at the household level and on human capital outcomes such as health and education attainment.[5] Studies on the links between firms' output and conflicts remain scarce.

Early papers on firm outcomes use stock market returns to get around data limitations. For example, Abadie and Gardeazabal (2003) analyze how the cease-fire declared by the Basque terrorist organization ETA in 1998-1999 affects the returns of firms operating in the Basque Country relative to other firms. They find that the truce announcement led to excess returns of firms operating in the region while its end led to a small negative impact on their returns, suggesting a negative impact of terrorism on expected investment returns. Similarly, Guidolin and La Ferrara (2007) study the market's response by diamond mining firms to the sudden end of the Angolan civil war in 2002. In contrast to Abadie and Gardeazabal, they find that diamond companies with concessions in Angola saw a drop of seven percentage point in market returns compared to otherwise similar firms that held no concessions there. The intuition is that conflicts can be beneficial or harmful to businesses depending on the institutional arrangements where they operate. In Angola, conflicts might have been beneficial to incumbent diamond firms because they helped deter entry and make profitable unofficial dealings easier.

One concern with the above approach is that stock market data are typically not available in countries impacted by civil wars. More recently, Collier and Duponchel

[5] See Blattman and Miguel (2010) for a broader literature survey.

(2010) use survey data to investigate some channels through which conflicts can affect firm performance in the context of Sierra Leone's civil war. They propose that one channel in which wars can have persistent post-conflict effects on firms is through technical regress and loss of workers' skills. The prediction is supported by the result that 5 years after the war ends, firms were more likely to report willingness to pay for staff training in those areas most affected by the conflict, indicating a shortage of skilled labor. A lack of more detailed data prevents Collier and Duponchel from confirming this result since it is not possible to determine the substitutability between labor and capital at the firm level. Moreover, they only have data after the war so they do no have a "control" period. Hence, there are concerns with omitted unobserved factors as high intensity regions might have intrinsic characteristics that make them more prone to conflicts *and* also affect the operating environments of firms. Collier and Duponchel's (2010) use distance to Monrovia (Liberia) to instrument for conflict intensity but distance is a problematic instrument because it can also directly affect output and productivity.

One approach is to find an exogenous event. Ksoll et al. (2010) examine the impact of unexpected violence during the 2007 Kenyan general election on exports by flower firms. The advantage of this paper is a cleaner identification strategy since violence shocks were unexpected and did not equally affect all regions where the firms were located. Thus Ksoll et al. are able to construct appropriate counterfactuals and estimate the reduced form impact of violence on exports more reliably. They find that violence in this period leads to a 38% drop in export volumes. Detailed survey data allow them to further develop and estimate a structural model with endogenous

labor supply. Simulations results show a 16% increase in operating cost if firms were to induce workers to work overtime to compensate for workers' absence caused by fear of violence.

The external validity of these results is unclear. Ksoll et al.'s work is based on a small sample size and it focuses on a highly specialized industry which is likely very different from the rest of the economy. The only paper we are aware of that uses census data is Camacho and Rodriguez (2010). These authors use instrumental variables approaches in models with fixed effects to assess the impact of armed conflicts on the exit of manufacturing firms in Colombia. They find that a one standard deviation increase in the number of attacks (by region) results in a 5.2 percentage point increase in the firms' exit rate. Exit mechanisms however, are left unexplored in the paper.

To the best of our knowledge, our paper is the first to study the evolution of firms' productivity in a conflict context. As such, it also contributes to the growing literature that examines productivity growth in developing countries. A paper closely related to our research is Hallward-Dremeier and Rjikers (2011), who study the relationship between firm survival and productivity before and after the 1997 financial crisis in Indonesia and find an attenuated impact of productivity on firm's exit during the crisis. Among other explanations, Hallward-Dremeier and Rjikers suggest that regime change and political connectedness might be determinants of this attenuation, based on the premise that firms affiliated with the Suharto regime might have been hurt disproportionally and were more productive before the crisis. However, firms might have appeared productive as a result of connectedness or as a result of real technical efficiency. Hallward-Dremeier and Rjikers mention but do not attempt to disentangle

these two possible effects.

4 Methodology

There are various mechanisms through which conflict could impact individual firms. The most obvious channel is through its effect on both factors of production, capital and labor: buildings and machinery might be damaged or stolen, people might die, get injured, migrate out of fear of violence or simply unable to show up at work. Destruction of capital and labor, if on a sufficiently large scale, will likely alter both their marginal product and marginal cost.

In addition, other factors might lead to changes in their relative prices. Uncertainty in conflict situations might reduce the relative attractiveness of domestic investments leading to capital outflows, driving up the cost of capital. Inter-regional migration might either increase or decrease labor supply and consequently prices in a particular location. If there are severe disruptions in any factors of production, firms may find it optimal to change their technology altogether.[6]

Even if capital, labor and technology do not change, conflict likely causes output/efficiency loss due to looting of output, bribe extortion or because businesses have to stop operating when there is a high risk of violence. Further, destruction of infrastructure and supply disruptions of intermediate inputs such as materials, electricity and other utilities can drive up the operating costs. There is anecdotal

[6] These effects have been documented empirically. Imai and Weinstein (2000) argue that reduced private investment by a process of portfolio substitution is a primarily channel through which war affects economic growth and find supporting evidence in cross-country data. Collier and Duponchel (2010) suggest firms in Sierra Leon switched to using inferior technology after the war due to severe shortages of skilled labor.

evidence of these effects in Cote d'Ivoire [7]. Firms' operating costs might also increase because they have to pay for "non-productive" inputs for security purposes to protect their output and investments.[8]

Another channel of impact is through demand effects. Conflict is often associated with reduced consumption due to reduced income. There are also potential changes in preferences. In the context of Cote d'Ivoire for example, anecdotal evidence suggests that some customers might divert their consumption away from foreign owned firms.

All these effects imply that the expected returns of investments may decrease. As a result, firms might reduce investments in both physical and human capital. Moreover, beyond these direct effects, conflict will likely increase uncertainty in terms of how demand will change and how much violence will happen in the future, leading to changes in investment dynamics.[9] Firms with bleak current and future prospects may opt out of the market. Heterogeneity in opportunity costs and all the effects discussed above will determine how the responses differ across firm owners. In fact, foreigners were reportedly leaving Cote d'Ivoire or looking to move their businesses elsewhere after the 1999 coup (BBC 2000b).

Quantifying and disentangling all these transmission channels is difficult, particularly without direct data on output/input losses or prices. We will present evidence

[7] Africa Research Bulletin (2001 and 2003) reports widespread looting and property destruction targeting businesses owned by West Africans and other foreigners after both the 1999 and 2002 coups.

[8] Cross-country enterprise surveys find that the costs of security technology and services represented 13 percent of sales in Senegal, South Africa, Tanzania, and Uganda, and 6 percent in Kenya (World Bank 2011).

[9] Bloom (2009) finds that uncertainty reduces investments because firms take a "wait and see" approach. Further, Bloom et al. (2007) note that the partial irreversibility of investment and fixed costs of hiring and firing influence the timing of actions and response to shocks. They find that firms with adjustment costs is much less responsive to demand shocks than firms not subject to the costs.

in section 5 which suggests destruction of capital and labor is not severe. Our analysis employs a production function framework that uses available data on output and firm's use of capital, labor and material inputs[10] to infer the reduced form net impact of several other mechanisms discussed above. Specifically, we use an estimate of TFP to capture the effect of output loss, "wasted" inputs and demand changes, by modeling their impact as an output distortion. Our estimation procedure does not identify the effects of input prices, and investment and exit dynamics, but allows for them under a number of simplification assumptions. Below, we present a brief conceptual framework of our approach. The sections follow will discuss our empirical strategy in more details.

4.1 Conceptual framework

Assume that firm i at time t produces according to a Cobb-Douglas technology. Without any distortions, for a vector of inputs $X_{it} = (X_{1it}, ..., X_{kit})$ used, the amount of output is: $Y_{it} = e^{A_{it}} F(X_{it}) = e^{A_{it}} X_{1it}^{\alpha_1} ... X_{kit}^{\alpha_k}$, where A_{it} is the technical efficiency term and Y_{it} denotes output. Suppose that because of the conflict, part of the firm's output is stolen and/or it has to halt operations for some fraction of the time, so that the realized output is only a fraction of potential output $\gamma_{it} Y_{it}$. Further assume that not all *observed* inputs are productive because for example, (i) a part of them is used for security purposes, or (ii) they are imperfect substitutes of the inputs normally used. Let λ_{kit} be the proportion of input X_{kit} that is actually used towards production. The net effect of all these factors is equivalent to the firm facing an

[10] It is also possible that firms might fudge their accounting figures to avoid extortion from the government. Without a secondary data source, we cannot identify this effect.

output distortion $e^{\tau_{it}}$, where $\tau_{it} = ln(\gamma_{it}\lambda_{1it}^{\alpha_1}...\lambda_{kit}^{\alpha_k}) < 0$, so that the realized output becomes: $\tilde{Y}_{it} = e^{\tau_{it}}Y_{it} = e^{A_{it}+\tau_{it}}F(X_{it})$.

Increases in the absolute magnitude of τ_{it} reduce the output produced. Therefore given the *observed* (or recorded) inputs, the measured TFP, which we define as $\tilde{Y}_{it}/F(X_{it})$, would be lower than the firm's underlying technical efficiency. Effectively, τ_{it} is a reduced form impact measure that captures the overall effects of output loss and "wasted" inputs through the channels discussed above. Additionally, if output is measured in monetary terms, as in our empirical implementation, changes in the TFP term would also contain price (demand) effects. We expect the absolute value of τ_{it} to increase with the intensity of the conflict. Because firms might be exposed to violence differently, τ_{it} is allowed to be firm-specific.

In the context of Cote d'Ivoire, our conjecture is that as the conflict intensifies, firms owned by foreigners or employing more foreign employees face greater distortions because identity, in particular citizenship and nationality, is an issue at the heart of the conflict. A larger distortion will correspond to a larger observed decline in TFP. Formally, we want to test the following hypotheses:

$$\frac{\partial \tau_{it}}{\partial conflict_{it}} < 0 \tag{1.1}$$

$$|\frac{\partial \tau_{it}}{\partial conflict_{it}}|_{\text{foreign}} > |\frac{\partial \tau_{it}}{\partial conflict_{it}}|_{\text{domestic}} \tag{1.2}$$

where $conflict_{it}$ is a variable that proxies for conflict intensity. Since A_{it} and τ_{it} are unobserved, getting unbiased estimates of TFP first depends on the ability to es-

timate $F(X_{it})$ consistently. If firms anticipate ω_{it} and τ_{it} and incorporate them in their investment, input purchase, and exit decisions then we need a framework that can allow for such possibilities. Therefore, our empirical strategy involves two steps. First, we follow Ackerberg, Caves and Frazer (ACF, 2006) to structurally estimate a production function. After getting consistent estimates of the production function parameters, we can recover a productivity measure. As explained above, this productivity term will measure technical efficiency as well as any residual factors that cannot be controlled for by observable inputs. In the second step, we regress the estimated productivity term on firm ownership, foreign employment and its interaction with time and conflict intensity, and other control variables to test hypotheses (1) and (2).

4.2 Productivity estimation

Assuming the production function is a Cobb-Douglas function in capital K_{it} and labor L_{it}, we estimate its log transformation:

$$y_{it} = \beta_k k_{it} + \beta_l l_{it} + \omega_{it} + \eta_{it} \tag{1.3}$$

where $\omega_{it} = A_{it} + \tau_{it}$, lower case letters denote logs and the error term η_{it} captures unanticipated shocks to output and measurement errors. In this framework, we can estimate the TFP term if β_k and β_l are known. OLS estimation of β_k and β_l, however, runs into an obvious simultaneity problem since high productivity firms may also use more inputs.

ACF (2006) address this endogeneity problem by taking a structural approach in

which an inverted function of the intermediate input demand is used to control for productivity.[11] The underlying structure is based on a model that derives industry equilibrium with heterogeneous firms. In the model, firms maximize a discounted sum of current and future profits, which are governed by exogenous productivity shocks that follow a first-order Markov process. Formally, the productivity process can be described as: $p(\omega_{t+1}|\{\omega_\tau\}_{\tau=0}^{t}, I_t) = p(\omega_{t+1}|\omega_t)$. Hence, current productivity can be written as a function of last period's productivity and a white noise error:

$$\omega_t = E[\omega_{it}|\omega_{it-1}] + \xi_{it} \tag{1.4}$$

There are two crucial timing assumptions in the ACF estimation approach. First, capital is treated as a dynamic input. That is, investment i_{it} takes place one period before the new capital can be used in the next period: $k_{it+1} = (1-\delta)k_{it} + i_{it}$. Second, labor, unlike intermediate inputs, is allowed to be a not fully variable input. It is decided at time $t-b$ $(0 < b < 1)$ when only part of the current productivity shock has been observed by the firm and before intermediate inputs are chosen.[12] Suppose productivity also evolves as a first-order Markov process between the $(t-1, t-b)$ and $(t-b, t)$ sub-periods then l_{it} would be correlated to ω_{it} through ω_{it-b}.

The only fully variable input is intermediate inputs m_{it}. Because it is chosen after both capital and labor, and after current productivity is observed, the material input

[11] It is related to the approaches originated by Olley and Pakes (1996) and Levihnson and Petrin (2003) but includes additional timing assumptions to address collinerarity issues.

[12] In the context of our research, it is a reasonable assumption since formal firms in Cote d'Ivoire are subject to the Labor Code. According to the Bureau of International Affairs (1999), those employed in the formal sector are generally protected against arbitrary discharge from employment.

demand m_{it} can be written as: $m_{it} = m_t(k_{it}, l_{it}, \omega_{it})$. ACF use the insight that m_{it} can be proven to be strictly increasing in ω_{it} under quite general assumptions. Therefore an inverted input function, proxied by a semiparametric function of capital, labor and materials, can be used to control for productivity: $\omega_{it} = m_t^{-1}(k_{it}, m_{it}, l_{it})$. Equation (1.3) then becomes:[13]

$$y_{it} = \beta_k k_{it} + \beta_l l_{it} + m_t^{-1}(k_{it}, m_{it}, l_{it}) + \eta_{it} \tag{1.5}$$

Neither β_k or β_l can be separately identified from the above equation because the function $m_t^{-1}(.)$ also contains k_{it} and l_{it} as its arguments. It is however possible to recover an estimate $\hat{\phi}_{it}$ where $\phi_{it} = y_{it} - \eta_{it}$. This term will prove to be useful in the moment conditions identifying β_k and β_l.

Recall that the timing assumptions above imply that *current* capital and *past* labor are decided before the current productivity is observed. Consequently, they are both uncorrelated with the error term ξ_{it} in equation (1.4). Hence, the two following moment conditions can be used to identify β_k and β_l:

$$E[\xi_{it}. \begin{pmatrix} k_{it} \\ \\ l_{it-1} \end{pmatrix}] = 0 \tag{1.6}$$

To operationalize these two moment conditions, we need to be able to write ξ_{it} as

[13] Note that we do not have m_{it} as an input in this equation thus y_{it} can be interpreted as value-added output. Alternatively, we can estimate a production function in revenue to recover coefficients on intermediate inputs. Since increasing the number of unknown parameters put additional requirements on sample size, we choose to estimate value-added production functions to use our data more efficiently.

a function of the parameters and known quantities. From equations (1.3) and (1.5), we have: $\omega_{it} = \phi_{it} - \beta_k k_{it} - \beta_l l_{it}$. Since current productivity only depends on its last period's value, we can approximate $E[\omega_{it}|\omega_{it-1}]$ as another semipararametric function $g(\omega_{it-1})$. For any set of initial values (β_k^*, β_l^*),[14] ω_{it} and ξ_{it} can be expressed as follow: $\hat{\phi}_{it} - \beta_k^* k_{it} - \beta_l^* l_{it} = g(\hat{\phi}_{it-1} - \beta_k^* k_{it-1} - \beta_l^* l_{it-1}) + \xi_{it}$. This expression can be used to form the sample analog of the moment conditions in equation (1.6) and estimate β_k and β_l. In the empirical estimations, we use third-order polynomials to approximate both $m^{-1}(.)$ and $g(.)$.

In addition to endogeneity of inputs, we face a selection bias problem in unbalanced panel data if the probability of exiting the market at any given productivity shock is correlated with inputs. This is the case if, for example, capital intensive firms are more likely to stay following a bad productivity shock. To account for this problem, we also estimate the production functions following Olley and Pakes (OP, 1996), which involves estimating an exit equation in past capital and investment and using the predicted exit probabilities to control for selection.[15] Nevertheless, ACF is our preferred method for three reasons. First, the ACF method is robust to labor adjustment costs and firm-specific labor and capital price shocks.[16] Second, investment is often lumpy, especially in a developing country context such as Cote d'Ivoire, so

[14] We use OLS estimates as initital values in the estimations.

[15] OP's methodology uses the inverted investment demand instead of intermediate inputs to control for productivity: $\omega_{it} = i_t^{-1}(k_{it}, i_{it})$. Because we only observe firms that survive, the correct expression for the g function which approximates the deterministic portion of ω_{it} should be $g[\omega_{it-1}, Pr(\chi_{it} = 1)]$ where $\chi_{it} = 1$ indicates survival. The selection correction is based on an exit rule, which states that firms exit when productivity falls below a threshold so: $Pr(\chi_{it} = 1|I_{it-1}) = Pr[\omega_{it} \geq \underline{\omega}_{it}(k_{it})|\omega_{it-1}] = \varphi(k_{it-1}, i_{it-1})$. This probability can be estimated using a probit function and the predicted values $\hat{P}_{it}$ will be controlled for in the function g above.

[16] Since the material input demand function includes both capital and labor, any price shocks in capital and labor will be reflected in this demand function

the monotonicity condition between investment and productivity in the OP procedure might not be satisfied. Third, past studies often find negligible impact of controlling for sample selection on estimated TFP. This is the case in our study, as discussed in section 6.1.

Our empirical work assumes that 1) there is a common production function (in value added) for firms in the same 2-digit industry; and that 2) the same functional form holds for the entire sample period. Both assumptions are necessitated by sample size limitations. Assumption 2 is justified by the fact that physical capital was not destroyed to a large extent since the most intense fighting during the civil strife happened outside of the capital city, and the majority of firms (86%) are located in the capital. Moreover, given the short sample period, it is reasonable to assume that firms might not have had the time to change their technology. As discussed above, changes in expected returns and uncertainty during the conflict can affect investment and exit dynamics.

To allow for changes in the material input demand (capital investment and exit responses in the OP approach), we control for year fixed effects, so our estimation procedure is robust to yearly shifts in material (investment) and exit functions. After recovering the input coefficients β_k and β_l, we calculate the log of measured TFP as:

$$\hat{\omega}_{it} = y_{it} - \hat{\beta}_k k_{it} - \hat{\beta}_l l_{it} \tag{1.7}$$

4.3 Heterogeneous impact of conflict

We can think of the estimated TFP from equation (1.7) as containing an underlying technical efficiency index A_{it} and a distortion term τ_{it}, $\hat{\omega}_{it} = A_{it} + \tau_{it}$, which cannot be measured separately unless one is prepared to introduce some additional structure. We test for the hypotheses in (1) and (2) in the regression:

$$\hat{\omega}_{it} = \alpha_0 + \alpha_1 Foreign_{it} + \alpha_2 Foreign_{it} \times after + W_{it}\alpha_3 + \mu_i + \mu_{jt} + \mu_{rt} + \varepsilon_{it} \quad (1.8)$$

where i, j, and r references a firm, an industry and a region respectively, and year is indexed by t.

Here, *after* is a dummy variable denoting the conflict period, which equals one if $t \geq 2000$. $Foreign_{it}$ indicates whether the firm is classified as "foreign". We consider two alternative definitions, namely 1) whether the firm is foreign owned, and 2) whether it employs any foreign employees. These two variables pick up inherent differences in efficiency between foreign and domestic firms, regardless of conflict-induced shocks.

Our variable of interest is thus the interaction term between foreign status and the conflict dummy. A negative and significant α_2 would suggest that foreign firms were impacted disproportionally by the increased conflicts since 2000. Our identification assumption is, conditional on observables, firms have comparable levels and trends in technical efficiency A_{it} so the variations in $\hat{\omega}_{it}$ during the conflict can be solely attributed as the effect of the firm's foreign status on the distortion τ_{it}.

We include a set of fixed effects to control for unobserved heterogeneity that could be confounding with foreign ownership/employment. First, firm fixed effects μ_i capture all time-invariant unobserved effects correlated to foreign status and help mitigate the problem with firms charging different mark-ups which will be reflected in the productivity term (Pavcnik 2002). By including firm-level fixed effects, α_2 is effectively identified from firms that experience a change in their foreign status during the conflict. Second, industry-specific time effects μ_{jt} capture all events during the sample period that affect all firms within a given industry equally. Foreign firms may be representative disproportionally in certain industries and some industries may have been affected more heavily by the conflict or by other macroeconomic shocks. Including these fixed effects avoids incorrectly attributing industry effects to foreign status. Another advantage of including industry-specific time effects is they can help capture the effect of measurement errors in the industry level deflators used in the empirical part (Amiti and Konings 2007). We also include region-specific time fixed effects μ_{rt} to control for shocks overtime that affect productivity in all industries but may vary across regions. This is important if there are spatial correlations in conflict patterns.

Vector W_{it} includes measures of firm size - log of total assets, a dummy indicating whether the firm is above the median employment level and its interaction with time. These measures of firm size address three issues: (i) firm size might affect productivity; and (ii) if financial frictions are present, large firms might weather shocks better than smaller firms and (iii) if firms are richer (have more assets), they are more likely to be looted regardless of identity. We also include a dummy variable indicating whether the firm's age is above the sample median, plus its interaction with time to control

for the possibility that older firms are more productive and are affected differently during the conflict. If foreign ownership and employment are correlated with firm size and age, then not controlling for these variables creates potential bias.

In an alternate specification, we use another set of variables to assess the effects of civil strife:

$$\hat{\omega}_{it} = \delta_1 Foreign_{it} + \delta_2 conflict_{it} + \delta_3 Foreign_{it} \times conflict_{it} + \delta_4 W_{jit} + \mu_i + \mu_{jt} + \mu_{rt} + \varepsilon_{it} \tag{1.9}$$

Here, foreign employment - $Foreign_{it}$ - is measured by two variables, namely (i) the percentage of all foreign employees, and (ii) French West African and other nationalities out of total employment. The $conflict_{it}$ variable measures the number of armed conflicts per 100,000 inhabitants by the department where the firm is located (see section 5.1 and the data appendix). As argued in section 3, we can consider conflict intensity to be exogenous to the individual firms.[17] The interaction between conflict and foreign allows us to see if firms which employ more foreign workers fare worse in situations of violence. Similarly to equation (1.8), we include the same set of fixed effects and size in terms of total assets interacted with time. Testing the hypotheses in (1) and (2) is equivalent to testing $\delta_2 < 0$ and $\delta_3 < 0$.

In all specifications, we cluster the standard errors at the firm level.[18] In the last specification, we use bootstrap standard errors, allowing for clustering, to account for

[17] The total number of firms which changed their locations during the conflict is 30, representing a negligible percentage of all firms in our data.

[18] We also allow for clustering at the geographical level of the conflict variables (following Moulton (1990)) but the resulted standard errors are smaller, possibly due to the unbalanced distribution of firms in our data. Therefore, we report standard errors clustered at the firm level to be conservative.

heteroskedasticity since we also have a generated variable on the right hand side of the regression.

4.4 Endogenous foreign employment choices

A potential problem with above specifications is that foreign employment and ownership might be endogenous since firms might make decisions about employing foreigners/changing ownership after observing their productivity. If having foreign workers during the conflict results in a negative impact on production, we might expect firms to respond by reducing their shares of foreign employees. However, we can observe less hiring of foreigners without a negative effect of foreign employment on production if for example, foreigners were leaving the country, driving up their relative wages. In this section, we outline an empirical test for such adjustment and discuss corrections for the potential bias in the production function estimations above.

Our empirical test is based on a simple model linking wages and labor returns. We assume that the share of foreign employees, denoted as ρ_{it}, has an impact on production in the form of an output distortion so that (log) productivity is the sum of technical efficiency A_{it} and a function $h_t(\rho_{it})$: $\omega_{it} = A_{it} + h_t(\rho_{it})$. The model implies that the optimal choice of ρ_{it} is decreasing with its marginal product, conditional on a relative wage ratio.[19] This prediction gives an empirical specification of the form:

$$\rho_{it} = \gamma_0^i + \gamma_1 \frac{1}{1 - w_{it}^f / w_{it}^d} + \gamma_{2t} + \eta_{it} \tag{1.10}$$

[19] Model set up available upon request.

where w_{it}^f and w_{it}^d denote the wage rates of foreign and domestic workers respectively. Here, γ_{2t} is a dummy variable indicating the conflict period. The hypothesis is $\gamma_{2t} < 0$, that is the share of foreign employees is decreasing with the conflict. We control for firm-level fixed effects to account for any differences in the return of labor at the industry level and other unobserved heterogeneity.

If there is evidence of firms adjusting their labor composition, the production function estimations will need to be adjusted accordingly. The reason is that both the ACF and OP estimation procedures relies on the assumption that the TFP term depends on its past value and an error term that is uncorrelated with current capital stock (see equation (1.6)). If $\omega_{it} = A_{it} + h_t(\rho_{it})$ as in the model setup above and, for example, firms with more foreigners are more capital intensive, then the coefficient estimate on capital in the production function will be biased.[20]

To check whether our results are affected by this bias, we use the ACF approach and include the percentage foreign employees, ρ_{it}, directly in the proxy function for TFP: $\omega_{it} = m_t^{-1}(k_{it}, m_{it}, l_{it}, \rho_{it})$. Intuitively, it means that firms take into account the impact of foreign employment on production and thus the material input demand function is also a function of ρ_{it} in addition to other inputs. We assume productivity still evolves according to a first order Markov process so: $\hat{\omega}_{it} = g(A_{it-1}) + h_t(\rho_{it}) + \xi_{it}$. Effectively we model the impact of foreign employment directly into the production function and not through the unobserved component of productivity. In practice,

[20] To see this, consider equation (1.5) above. The error term η_{it} and consequently ϕ_{it} will contain information on ρ_{it} if it is not included in the proxy function m_t^{-1}. As a results, the error term ξ_{it} calculated using $\hat{\phi}_{it}$ will contain information on ρ_{it}. Therefore, the first moment conditions in (1.6) will not be satisfied if capital is correlated to foreign employment. For a more formal discussion of the problem, see Appendix A.

because this estimation procedure is much more data demanding, we assume that h_t is linear and only allow $h_t(.)$ to change once with a dummy variable indicating the starting of the conflict. The effect of ρ_{it} over time is estimated directly from the production function through $h_t(\rho_{it})$. See Appendix A for more details on the exact estimation procedure.

5 Data

Our analysis is based on firm-level data from the Registrar of Companies for the Modern Enterprise Sectors, collected by Cote d'Ivoire's National Statistics Institute (INS), covering the universe of registered, formal modern enterprises for 1998-2003.[21] The unit of observation is the firm. Almost all firms in Cote d'Ivoire are single-establishment firms. The data set covers manufacturing, agriculture, trade and services firms.

The Registrar of Companies collects information upon incorporation including physical location, sector classification at the 3-digit industry level, and shareholdings for all shareholders. The INS requires all registered companies to submit annual filings with detailed financial and employment information including total wages, employee skill level, taxes paid, sales, and fixed assets, which are reported under the West African accounting system standards, Etats Financiers Normalises du Systeme Comptable Ouest Africain (SYSCOA). These firms, which operate in the formal economy, pay a range of taxes, benefit from bank finance and technical assistance, and

[21] Our panel is shorter than the typical sample length in the TFP literature. However, it is longer than most panels available in Sub-Saharan context. For example, Frazer (2007) uses a 4 year panel while Soderbom et al. (2006) and Van Biesebroeck (2005) both have a panel length of 3.

are characterized as having more educated owners (Goedhuys and Sleuwaegen 2002, OECD 2004).

The structure of the data is an unbalanced panel. We define exit as a permanent exit from the data (We do not consider temporary lapses in reporting as exits). Entry can be defined unambiguously since we know the year when the firm starts operating. The reporting requirement constitutes an important advantage of our data since entry and exit from the data implies real entry and exit, unlike data from other countries which usually require a certain employment or revenue threshold to be included in the data.

Table 1.1 displays information about the composition of the sample. The full sample includes 7010 firms there are missing values and potential outliers. For the production function estimations, we exclude the top and bottom one percent of the distribution of output and inputs. [22] As a result, we are left with 4161 unique firms or 11810 firm-year observations. Table 1.2 shows the distribution of firms by panel length. In general there are a larger number of missing values in the earlier years, but all possible panel length (1-6) are well represented in our data. As mentioned, we estimate separate production functions for 2-digit industries when possible. However, since the sample size is limited for some industries, we also need to pool several industries and estimate the same production function for these industries.[23] Table

[22] The results are very similar if we instead drop observations with abnormal (one-percent tails) year-to-year growth in output and inputs.

[23] We base our pooling on the similarity of the industries i.e. if they have similar capital labor ratios. Nevertheless, we need to drop some industries with a very small number of firms and no obvious similar grouping. These are electric and water utilities, ores and minerals, and petroleum industries. We further exclude financial services and government, education and health services since they are potentially very different from the rest of the industries.

1.B.1 in Appendix B reports the resulting number of firms by industry.

We augment the INS data with population estimates from Cote d'Ivoire's 1998 Census, GIS data on administrative boundaries and conflict data from the Armed Conflict Location and Event Data (ACLED, Raleigh et al 2012). The conflict rate variable is calculated as the number of armed conflicts[24] as reported per year by department, per 100,000 inhabitants. Population data is taken from the 1998 Census. The conflict rate ranges between 0 to 8.04 and the mean conflict rate is 1.28 for all firms in the sample. The average rate of conflicts targeting foreingers increase from 0.63 to 2.38 after 2000. Figure 1.3 maps the distribution of firms in the sample in two periods.[25] It shows that firms' concentration has somewhat changed over time but the majority of firms still concentrated in Abidjan both before and during the crisis.

5.1 The firms

Summary statistics of the variables of interest are reported in Table 1.3, including summary statistics for the sub-sample used to estimate the production functions, which excludes observations whose monetary values are in the first and ninety-ninth percentiles of the full sample. All monetary variables, including sales, value added and material costs are deflated using the corresponding industry deflators taken from the INS, where the industry classification is similar to the US 2-digit SIC classification. The use of industry-specific deflators helps limit the problem of product/input quality

[24] We include five types of events reported by ACLED: Battle-Government regains territory, Battle-No change of territory, Battle-Rebels overtake territory, Riots/Protests and Violence against civilians.

[25] Since the coded location data was collected upon in-corporation, the data might not be accurate if the firm has relocated. When the registered location does not match a firm's physical address, we use the city and department information from the address to recode the firm's location.

or markup differences across industries being incorporated in prices. Investment and capital are deflated using an economy-wide deflator due to a lack of more detailed information. Recent papers have raised concerns that using monetary values instead of physical output amount confounds productivity with mark-ups if there is still considerable heterogeneity in market power within industries (see for example, Foster at el. 2012). In the absence of data on firm-level prices or other information to infer mark-ups, our estimated TFP will have to be interpreted as containing price effects.

Sample means and medians show a typical skewed distribution with mostly small firms and few very large firms.[26] A quick glance at some key characteristics suggest that the quality of our data is reasonable. The average firm size, at 56 employees, is close to estimates from other countries in the region. For example, Soderbom and Teal (2004) estimate the average size of Ghanian manufacturing firms to be 67 employees. The measure of labor productivity, sales per employee, is at around 80,000 USD. This figure is comparable to China and Indonesia (see Bloom et al 2010).

We use information on shareholding, which is available for the sub-period 1999-2003, to construct ownership variables for the firms. We observe nationality and share values held by all shareholders in the firm but not their identity. We define ownership of the firm as the nationality category with the largest total share. We distinguish between Ivorian, French West African, French and other foreign ownership to understand the differences in firm characteristics by ownership. However, in what follows, we only distinguish between Ivorian and foreign ownership since we do not

[26] Compared to the typical distribution of firm size in developed countries however, the formal sector in Cote d'Ivoire exhibits a relatively smaller percentage of small firms and a greater percentage of large firms - similar to findings from other developing countries (Klapper and Richmond 2010).

hold any *a priori* hypotheses on the differences between different types of foreign firms (except for French West African firms, which at any rate constitute a very small share of the sample). Summary statistics of firms by ownership over the years are reported in Table 1.4 and 1.6.

As shown in panels A and C of table 1.4, foreign owned firms in all years represent a significant proportion of all firms in both the original sample and the sample used to estimate TFP. At the same time, there are considerable changes of ownership over the crisis years. By the end of the sample period, the percentage of Ivorian firms has increased by almost 10% compared to the beginning of the sample period, picking up the decline in French ownership. The distribution of ownership in a balanced sample shown in panel B of table 1.4 suggests that part of the changes are caused by entry and exit. This is confirmed in table 1.5 which shows that the majority of changes in ownership happens in 2000. Table 1.6 compares the characteristics of firms by ownership. Given their large percentage and significantly larger size, both in terms of employment and total assets, foreign, and especially French, businesses clearly play an important role in Cote d'Ivoire's economy.

5.2 Firm entry and exit

Figure 1.4 shows aggregate trends in firm's entry, exit and employment growth. Entry and exit rates average over the entire study period at 11.7% and 25.2% respectively. Both entry and exit rates show a clear break after 2000. Exit rate increases from around 23% in 1998-1999 to around 27% in the 2000-2002 period. Entry rate shows an even bigger gap between the two periods, from around 15% in 1998-1999

to 9-10% in the later years. Aggregate employment also contracts from a net job creation of about 10000 workers in 1999[27] to a net loss of about 18000 jobs in 2000 and decreases further in 2002 and 2003.

As shown in figure 1.5, foreign and Ivorian-owned firms have similar entry and exit rates in 1999 but diverging trends of increasing exits among domestic firms and decreasing entry among foreign firms starting in 2000. One possible explanation is that foreign firms weather shocks better than domestic firms and thus are more likely to stay in business. However, because foreigners have higher opportunity costs of establishing businesses in the country given the conflict, they are less likely to enter. Figures 1.6 and 1.7 show that the spike in exit rates since 2000 is also accompanied by an increase in the average size and age of exiting firms. Given the common finding in the literature that larger and older firms are more productive, this trend suggests that more productive firms might be exiting during the conflict.

These figures indicate a highly volatile economy comparing to entry-exit rates of firms in other countries. It is difficult to find census data for other economies with comparable income level but estimates from surveys in other sub-Saharan countries indicate lower annual firm exit rates. For example, Soderbom et al. (2006) find that the attrition rate for a sample of firms from Ghana, Kenya and Tanzania averages 6% between 1993 and 1999. Shiferaw (2009) reports annual exit rates from the manufacturing census in Ethiopia, whose income per capita is about one third the level of Cote d'Ivoire, to be 16% in the 1996-2002 period. In general, coinciding with the

[27] The size of the total workforce in Cote d'Ivoire in the industrial sector is half a million (Bureau of International Affairs 1999).

surge of violence, the formal sector starts to show signs of contraction in 2000 as evidenced by decreased overall employment, increased exit and decreased entry.

5.3 Output

Figure 1.8, panel A plots the simple trend in firms' output - measure as log of value added - over time (the base level in 1998 has been normalized to zero). It shows a sharp decline in average output, which is 38-56% lower in 2000-2003 compared to 1998-1999, the pre-conflict period. In panels B and C, we plot the coefficient estimates γ_{2t} from the following regression:

$$y_{it} = \gamma_0 + \gamma_1 Foreign_{it} + \sum_t \gamma_{2t} Foreign_{it} \times D_t + \mu_i + \mu_{jt} + \varepsilon_{it} \tag{1.11}$$

where y_{it} denotes log value added, $Foreign_{it}$ denotes foreign ownership/employment and D_t are year dummies. The coefficients γ_{2t} tell us the percentage differentials between firms with/without foreign ownership and employment over time, conditional on firm fixed effects μ_i and industry-specific year effects μ_{jt}. The results show that output has been reduced by more than 20% for firms with foreign employees relative to other firms beginning in 2000. The relative decline is the largest (35%) in 2003. Foreign owned firms' output has also decreased more than their Ivorian counterparts but the difference is not significant until 2003.

Figure 1.9 shows the distributions of log capital and log capital/labor ratio for the two periods before and during conflict. It shows that the distribution of capital has shifted somewhat to the left during the conflict but the reduction does not

reflect a situation of severe destruction. The distribution of capital/labor ratios has also decreased but not to a large extent.[28] These observations lend support to our assumption that firms did not change their technology during the conflict.

The observed trends in value added suggest that output of foreign firms has dropped significantly more than non-foreign firms during the conflict. In the next section, we present regression results using structural estimates of TFP in light of the conceptual framework in section 4.1 to get at the causal impact of foreign ownership/employment and conflict on firms' production.

6 Results

6.1 Productivity evolution

Coefficient estimates of the (value-added) production function using the ACF and OP approaches are displayed in Table 1.B.2, Appendix B.[29] Using these estimates, we plot the average firm-level log TFP and labor productivity (sales per employee) in figure 1.10. ACF and OP productivity estimates follow each other closely, suggesting that exit bias does not affect much the estimated TFP. Both estimates suggest that average TFP has dropped substantially in the conflict period. More specifically, productivity decreases by 12% and 18% in 2000 and 2001 respectively before rebounding slightly in 2002. Firm productivity in 2003 remains at 10% lower than the 1998-1999

[28] The statistics for the Kolmogorov-Smirnov tests of the equality of distributions are -0.1041 and -0.1298 for log capital and log capital/labor ratio respectively. The null hypothesis of equal distributions before and after 2000 is rejected at 0.001 significance level for both variables.

[29] Since the coefficient estimates on unskilled labor for group 3 (canned and food preparations, beverages and ice cream, and other food products) and the capital coefficient for group 5 (wood industries) are negative using the ACF and OP approaches respectively, we omit firms in these two groups from our subsequent analyses.

average. Since Cote d'Ivoire's economy is heavily dependent on cocoa and coffee exports, the rebound in 2002 and 2003 might be partly due to a significant increase in their prices in the international markets.

The trend in labor productivity is qualitatively similar. The trends in TFP and labor productivity estimates match the pattern of average value added closely. In fact, a simple regression of firm-level year to year changes in value added on changes in TFP suggests that more that 80% of the changes in value added can be explained by TFP changes. Given this result, TFP appears to be an important channel through which firms' output is impacted by this conflict.

6.2 Heterogeneous effects

The previous section provides some evidence that firm productivity has generally declined since 2000, after the start of Cote d'Ivoire's political turmoil. We now explore whether the productivity decline is heterogeneous with respect to a firm's identity during conflict times. Given that the results using OP and ACF estimates of TFP are very similar, we will only report regression results using the ACF estimates in the main tables.

Table 1.7 reports regression results from the estimating equation (1.8) which investigates the impact of foreign ownership/employment on TFP during the conflict period. Columns 1 and 2 report the results with foreign ownership while columns 3 and 4 report the results with foreign employment. The pre-conflict period is 1998-1999 but the base year is 1999 in columns 1 and 2 because ownership information is missing for 1998. Consistent with our hypothesis, the effect of foreign ownership and

employment during the conflict period is negative in all specifications, but insignificant in columns 1 and 2. This could be due to the fact that most foreign owners are French and other European nationals and violent attacks against them did not intensify until 2003.

Turning to foreign employment, having at least one foreign employee has a significant and larger effect on TFP. This is consistent with the fact that most foreign employees are French West Africans and violence against them started in 2000. The coefficient estimate of the interaction term in column 3 implies that conflict reduces TFP of firms with foreign workers by more than 20% compared to firms with no foreign workers. This effect is identified from within-firm TFP and foreign employment variations, controlling for yearly shocks common among firms within industries and regions. When further controlling for age and size interacted with time in column 4, the estimated impact reduces to 9.7%. Given the functional form of the production function, this TFP impact translates to an annual impact of around 10% of total value added output.[30]

The coefficient estimates on size in terms of assets also have expected signs. In all specifications, total assets have significantly positive effect on TFP, consistent with results elsewhere that larger firms are in general more productive (see, for example, Sorderbom 2006). Conditional on assets however, the results in columns 1 and 3 suggest that size in terms of employment and age have a negative impact on TFP.

[30] Or approximately 6% of GDP since the formal sector's contribution to GDP is estimated to be 60% (Berthelemy and Bourguignon 1996). Due to missing data on value added and industry price indices, we cannot accurately match Berthelemy and Bourguignon's estimates using our data. If we exclude all observations with negative reported value added and summing up nominal value added across firms, the aggregate value added of all firms in our data corresponds to roughly 30% of nominal GDP.

These results seem to be driven by the differential impact of conflict on older and larger firms. When controlling for the interactions of size and age with the conflict dummy in columns 2 and 4, the levels of size and age are no longer significant. Since firm size can affect various factors such as financial constraints and mark-ups, which can be captured in our TFP estimates, it is possible for the estimated TFP of large firms to decrease more during the conflict if these factors are changing differently for small and large firms. There is however no clear theories for why age should drive the magnitude of the the impact of conflict on firms conditional on size. A possible explanation is that older firms are also more "visible" and thus are more likely to be the target of attacks.

In addition to the above before-after specification, we estimate and graph the effect of foreign ownership/employment in each year, $\tilde{\alpha}_{2t}$ where $t = 1998...2003$, using the model:

$$\hat{\omega}_{it} = \tilde{\alpha}_1 Foreign_{it} + \sum_t \tilde{\alpha}_{2t} Foreign_{it} \times D_t + W_{it}\alpha_3 + \mu_i + \mu_{jt} + \mu_{rt} + \varepsilon_{it} \quad (1.12)$$

These graphs permit a visual and statistical test for the year-specific effects of foreign ownership/employment on TFP. In these graphs, the value $\tilde{\alpha}_{2t}$ where $t = 1998/1999$ represents a reference category set to zero (as ownership information is not available in 1998). The results are presented in figure 1.11. The impact pattern of foreign ownership/employment on TFP mirrors the pattern of value added found in figure 1.8. Figure 1.11A shows that the before-after specification above masks the significant effect of foreign ownership in 2003, when anti-French sentiments became prevalent.

The result suggests that having foreign ownership in 2003 reduces firm's TFP by 18.8% on average. Conflict further reduces TFP of firms with foreign workers by 14.3% in 2000 and 17.4% in 2003, respectively, relative to firms which only hired domestic workers. Figure 1.11B also shows an insignificant effect of foreign employment in 1999 compared to the base year, justifying the parallel trend assumption for identification in the before-after specification above.

The results so far provide evidence on how productivity changes with foreign ownership and employment over time, which is suggestive that the turmoil in 2000 and 2003 has a negative impact on foreign firms. However, it is not possible to identify the overall effect of conflict on all firms given the time variation related to conflict will be succumbed into the year fixed effects. To identify this effect, we exploit geographical variations in conflict intensity and estimate equation (1.9). The results are presented in Table 1.8. Conflict intensity is a significant determinant of productivity in all specifications. An increase of one violent attack per 100,000 inhabitants reduces TFP by 10-11%, which translates to a aggregate annual impact of around 13% compared to the counterfactual of no violent attacks.[31] This is comparable to the cross-region results in Abadie and Gardeazabal (2003) but lower than the cross-country estimate of 18% in Cerra and Saxena (2008).

Column 1 presents the results when we control for foreign employment as a dummy variable and interact it with conflict intensity. The coefficient estimate suggests that on average the impact of violent attacks on firms are 41.6% higher in magnitude if

[31] We estimate this aggregate impact by summing up the predicted firm level TFP had the conflict rate variable is zero, and compare it to the observed aggregate TFP.

they have foreign workers. The results are qualitatively similar in column 2 where we control for the interactions between the shares of foreign employees and conflict intensity to allow for the effect of conflict on firms to vary with the degree of foreign employment that they have. When French West Africans and employees of other foreign nationalities are controlled for separately in column 3, only the interaction with French West Africans shows a significant impact. Most likely, this is due to the the small percentage of non-French West African employees in the sample. The results in column 3 suggest that having an extra percent of French West African workers increases the (negative) impact of violent conflict by almost one percentage point. In aggregate, this effect corresponds to a 7% reduction in TFP over the sample period.

6.3 Channels of impact and robustness checks

This section presents results from several tests to disentangle the potential mechanisms through which conflict affects TFP and other robustness checks. Table 1.9 reports results on channels of impact. Specifically, we consider demand effects and effects on operating costs. Table 1.10 includes results from a set of robustness checks in various sub-samples.

As noted above, our TFP measure likely picks up changes in mark-ups (see section 5.1). Hence, the result that TFP of foreign firms are decreasing more than domestic firms might be driven by changes in both efficiency and demand. Since income as a whole for the population was decreasing, it is likely that domestic demand for goods and services produced by the formal sector was also falling. Therefore, firms

that export part of their products might fare better than those depend solely on the domestic market. On the other hand, it is possible that trade in general and exporting firms suffer from instability. We test for which effect is stronger by interacting an indicator of export orientation with foreign employment and the conflict dummy in the before-after regression in equation (1.8). Since we do not have trade data at the firm level, we use aggregate data from the INS to calculate an industry export orientation index before the conflict.[32] The coefficient estimates on the variables of interest are reported in table 1.9, column 1. The interaction between foreign employment and the conflict dummy is still significant and of similar magnitude while the coefficients on export interacting with conflict and foreign employment are insignificant, suggesting exporting has no net effect on all firms.

Another possibility is that firms with different market power would see different changes in profits which is reflected in TFP. To test whether this channel is operating, we control for an indicator that equals one for firms belonging to a highly concentrated industry at the beginning of the sample period (1998). An industry is defined as high concentration when its Herfindahl index - defined as the sum of squared market shares in value added - is larger than 1.4, or approximately in the seventy-fifth percentile of the sample. We then control for this variable in an analogous way as the export orientation variable before. The results are in column 2. The concentration

[32] We calculate the share of exports over total output at two-digit industry level in 1999 since we only have aggregate data from the INS for two years 1999 and 2004. In principle, we can use trade data from other sources such as COMTRADE to calculate export and import shares at the ISIC 4-digit level and for all years. However, this approach does not work because we were unable to match the industry indicator for more than 60% of the firms in our data with the COMTRADE data. Approximately half of the industries (in terms of number of firms) do not export in 1999 so we define export orientation as a dummy variable indicating non-zero exports at the industry level.

dummy interacted with foreign employment and conflict is insignificant, suggesting that productivity loss for foreign firms are present in both high and low concentration industries.

Another channel through which trade may affect firms is if they use imported inputs, which might become more costly with the conflict. Column 3 displays results of whether firms in import oriented industry are impacted differently. The import indicator is calculated in the same way as the export indicator above. The coefficient on its interaction with the conflict dummy is negative and significant, indicating this cost channel might be at play. The coefficient on the interactions with foreign employment and conflict is not significant however. Thus increasing cost of imported inputs does not appear to be a channel through which foreign firms were affected more by the conflict.

As discussed before, conflict might lead to destruction of infrastructure such as roads, causing firms with higher transportation cost to incur disproportionally higher operating cost.[33] Column 4 presents a test of this channel, using the share of transportation cost over total sales at the firm level. The result suggests no significant impact of transportation cost however, even though the signs of the interaction terms are as expected. We also control for lagged instead of current transportation cost to mitigate endogeneity concerns and did not find a different result.

One concern with the previous results is that they could be affected by sample selection bias given we have to drop a larger number of firms from earlier years due

[33] Moreover, this channel may be more important for foreign firms. In Cote d'Ivoire, anecdotal evidence suggests that unrest in the country has led to an increase in the number of checkpoints on the roads, and consequently an increase in the solicitation of bribes at these checkpoints. Furthermore, foreigners are often discriminated and demanded higher bribes.

to missing data. The sample selection issue is driven by that fact that we have to fill in missing sector information for a large number of firms in 1998 and 1999 using data from earlier/later years. As a result, firms that survive only one period are more likely to be excluded. An ideal test would be to drop firms that exit after one period, which would require dropping firms with a panel length of one. However, exit is not known in the last year of data (2003) so such procedure implies a lower likelihood of being included in 2003. To overcome this problem, we reestimate all of the specifications above using a sample where firms' age is larger than one. This test effectively excludes firms that survives one period with the caveat that inferences are only for firms that are age 2 and above. The results are presented in column 1 of table 1.10. The coefficient estimates are largely similar of significance and magnitude as before, suggesting that sample selection is not driving our earlier results. Lastly, columns 2-5 report results from the regressions in equation (1.9) on the sub samples stratified by size and age to account for the possibility that percentage foreign employees might affect firms of different size and age very differently. The coefficients on the interactions between conflict intensity and shares of foreign workers are qualitatively similar but only significant for smaller and older firms.

6.4 Do firms adjust the shares of foreign workers?

Do firms respond to conflict pressure by hiring fewer foreign workers and adjusting their workforce? Table (1.11) presents summary statistics which show that there is significantly less hiring of foreign employees in general during the conflict, consistent with our prediction. Table 1.12 present further econometric evidence from equation

(1.10). In practice, we estimate this equation using the share of French West African employees as ρ_t since they make up the vast majority of foreign workers and they are mostly unskilled workers so the results are less likely to be confounded by skill level.

The results in column 1 and 2 are as predicted. That is, for firms with positive foreign employment, the share of foreign employees is decreasing with the conflict, after controlling for the relative wage of foreigners over Ivorians. To control for the possibility that labor adjustment cost might affect how quickly firms can change the share of foreign workers, we add lagged employment size and its squared terms and interactions with time. The results are reported in column 2. The negative coefficient on the interaction between size and conflict dummy indicates that bigger firms might be slower to adjust but the effect is imprecisely estimated. Coefficient estimates of the wage and conflict variables are qualitatively unchanged, suggesting our prediction holds up after (partially) accounting for adjustment cost. Another important caveat is we assume firms to be maximizing profits in the model and thus have not taken into account the firm's owner's taste for employing Ivorian versus foreign workers.[34]To the extent that larger firms might be more likely to behave as profit maximizers, the results that larger firms are slower to adjust might also be driven by a taste-based effect.

We have found suggestive evidence of firms adjusting the shares of foreign employment downward to mitigate the conflict impact. Tables 1.13 and 1.14 report results from the production function estimations where such adjustment is taken into

[34] Taste could also be changing with the conflict. If it does not change over time then fixed effects can control for such concern.

account. Effectively, the share of foreign workers is modeled as an additional (potentially negative) input in the production function and its effect on productivity is estimated directly in one step. As before, we define the start of the conflict as $t \geq 2000$ and allow percentage foreign employees to have a different before and after effect on production. Since the production function estimations are at the industry level, the effect of foreign employment interacted with the conflict dummy is also allowed to be industry specific. We find negative and significant effects for the interaction term in five industries[35] and no significant impact on other industries. The results for these industries where we find a significant effect are reported in table 1.13. Using these coefficient estimates, the aggregate cost of having foreign workers during the conflict is estimated to be 5.4% of total output .

Next, we run the same production function estimations on sub samples of industries stratified by size to allow technology to be different for small and large firms and to make the percentage foreign employees more comparable across firms of similar size. Because of data limitations, this exercise can be done only for industries with sufficiently numerous observations. The results are reported in table 1.14. Here, we only find evidence of negative impact of foreign employment and conflict for firms in the Transport and communications and Commerce sectors and for larger firms in the Chemical, rubber products and building materials sector. Lack of statistical power might be responsible for the absence of impact in some industries but in general, these results confirm the findings in the earlier sections that foreign firms were impacted

[35] Mechanical and electrical products, Construction and maintenance, Transport and communications, Rental and management of buildings, and Commerce)

more by the conflict.

7 Conclusion

Using firm-level data from Cote d'Ivoire, this paper has examined the heterogeneous impacts of political instability and violent conflict on firms in terms of efficiency loss. Our approach relies on a measure of firm TFP derived from structural estimates of the production function parameters. We argue that under fairly general structural assumptions, this TFP term contains the reduced form impact of several channels through which conflict might have affected firms, including (i) output loss due to looting and extortion, (ii) increasing shares of unproductive inputs for security purpose or due to idle capacity, and (iii) demand changes.

We find that the conflict reduces firm TFP substantially and having foreign ownership/employment magnifies the impact of conflict. The results are qualitatively similar when we control for endogeneity of foreign employment using instrumental variables or using an alternative procedure to estimate production functions. These results suggests that the nature of the conflict, which spurred increasing anti-foreigner sentiments, creates distortions that disproportionally affect foreign firms. In response, we find evidence of firms employing fewer foreigners to mitigate the conflict impact.

Given the importance of foreigners in general and of foreign investment in Cote d'Ivoire, our results suggest that recovery effort would have to entail restoring confidence in the state's commitment and ability to protect the interest of foreign investments and employees. Moreover, increasing hostility and discrimination towards foreigners, as signaled by economic impacts, might further exacerbated the country's

already eroding social cohesion. Blattman and Miguel (2010) argue that if the costs are borne unequally across groups, conflict itself could intensify inequality and social discord, hence further aggravating factors that feed into the risk of conflict reoccurrence. In a society with a history of strong presence of immigrants and long standing social disparity between the north and the south like Cote d'Ivoire, this concern might prove to be important. Just before the Civil War started in 2002, a survey in major cities by PEW Global Attitudes Project found that 78% of the survey respondents rate "ethnic conflict" as a big problem facing the country. Indeed, the civil war that erupted the following year confirms that view.

Our research is also of relevance for other contexts because the type of conflict studied in Cote d'Ivoire - characterized by "low-intensity" but repeated cycle of unrest - has become common in many other developing countries. Understanding how the private sector is affected and adapts in these "no war no peace" situations is critical in understanding the role of instability in underdevelopment in a large part of the world. While this paper focuses on the specific conflict in Cote d'Ivoire and the negative implications it has for foreigners, our approach provides a useful framework to analyze conflicts/shocks in other contexts, which frequently impose asymmetric costs on different groups in the population.

A limitation with our data is that we do not observe directly the type and magnitude of losses that firms incurred due to the conflict. We have used several firm and industry characteristics to investigate potential mechanisms through which the conflict might have affected TFP. We find evidence consistent with a story of increasing cost of imported inputs as a channel of impact and no evidence of demand effects

driving the results. Nevertheless, we cannot rule out any mechanisms with certainty. This is an area for future research.

Figures

Fig. 1.1: Major political and conflict events

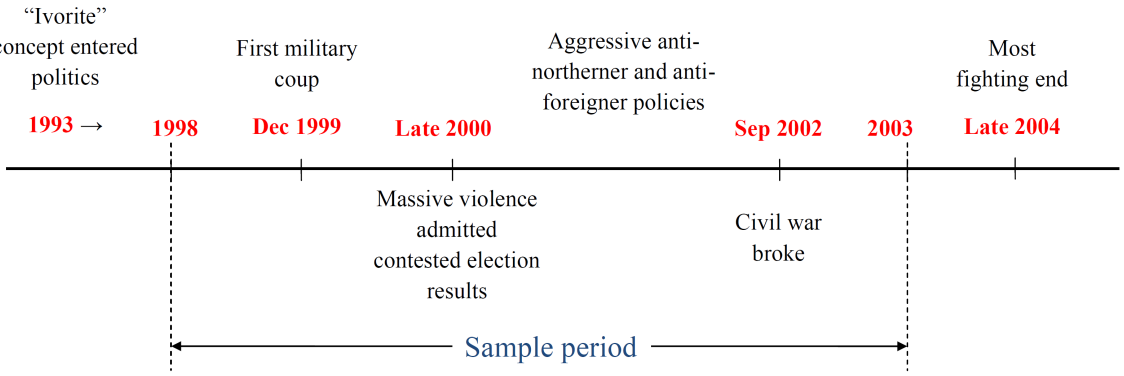
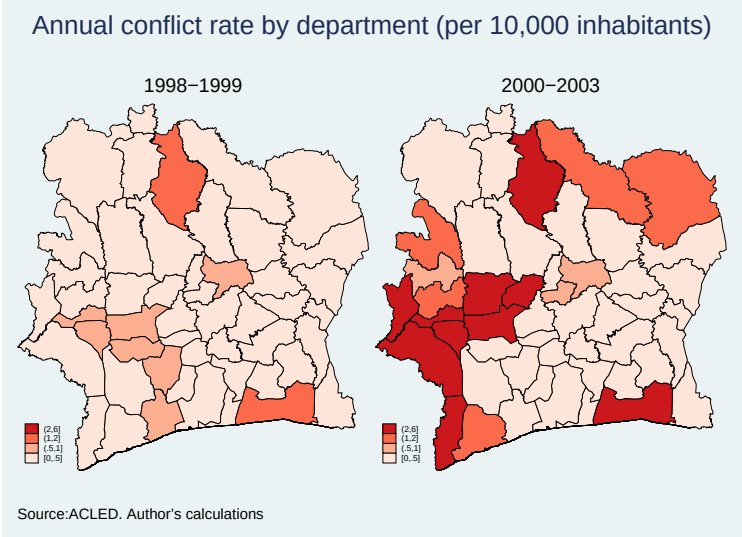


Fig. 1.2: Conflict intensity by department



Note: conflict rate is calculated as the number of armed conflicts, divided by the total population by department

Fig. 1.3: Firm distribution

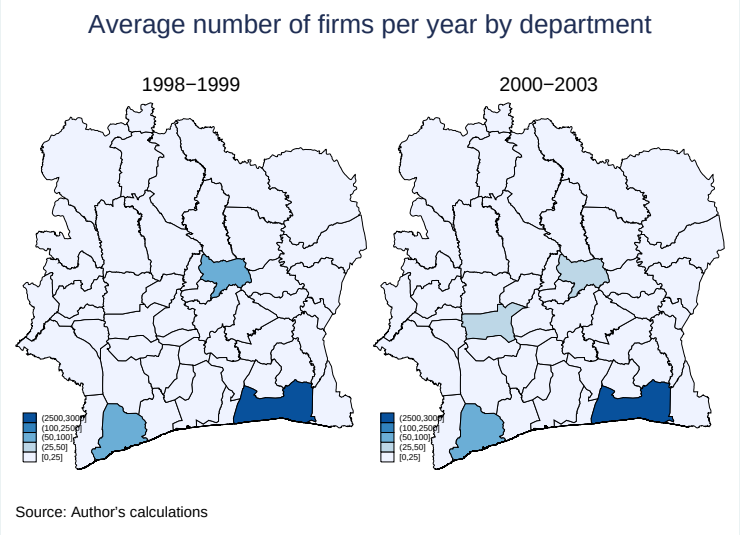


Fig. 1.4: Rate of entering and exiting firms and aggregate net job creation

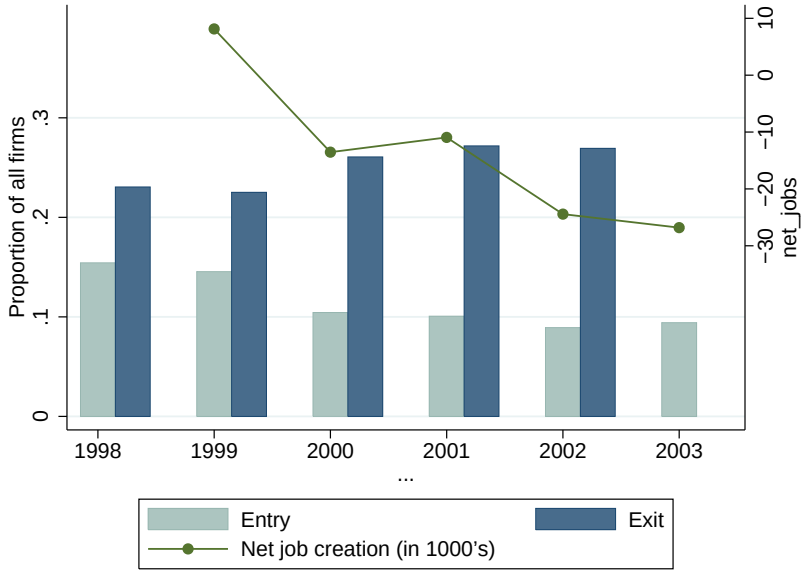


Fig. 1.5: Entry and exit rate by ownership

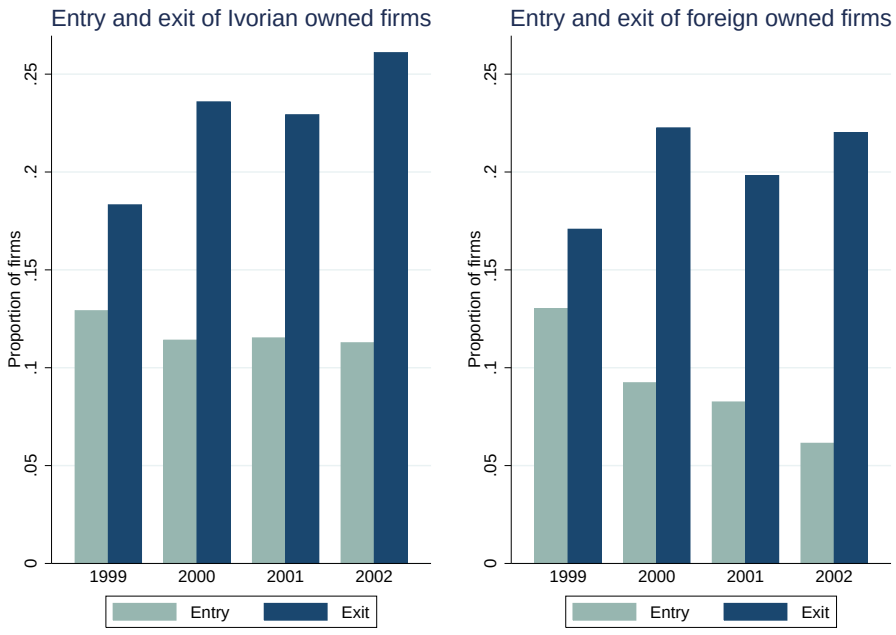


Fig. 1.6: Average size of entering and exiting firms

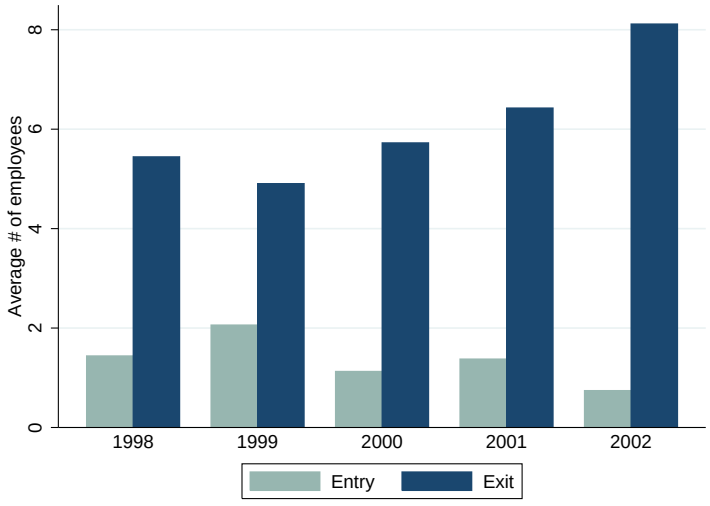


Fig. 1.7: Average age of exiting firms

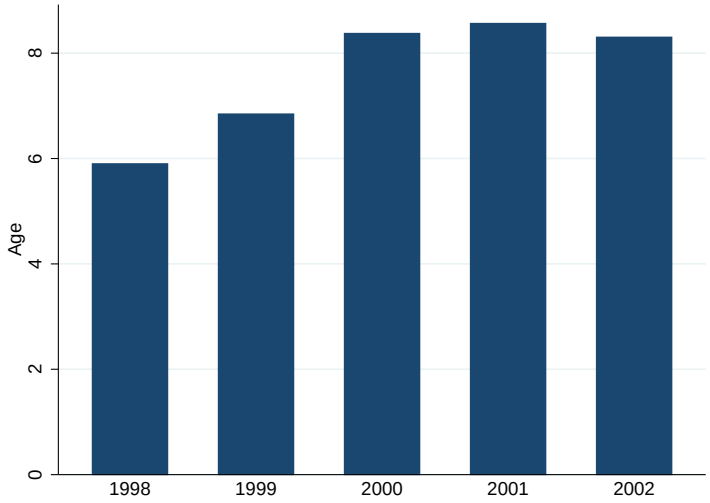
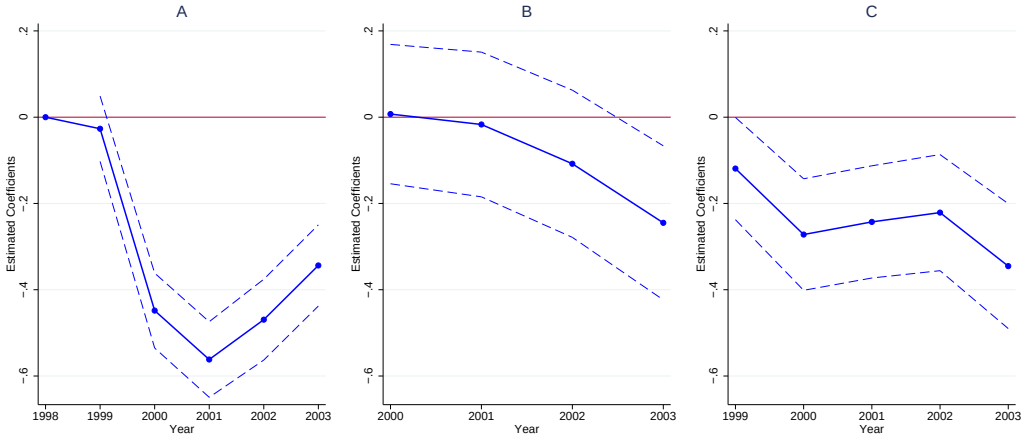
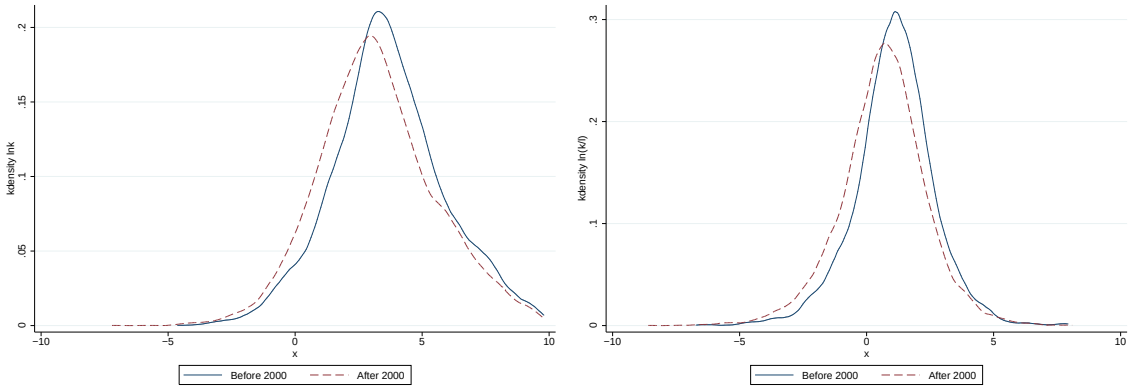


Fig. 1.8: Trend in average value added over time



A: Average value added over time, where 1998's level has been normalized to 0.
B: Impact of foreign ownership on value added over time, relative to 1999.
C: Impact of having foreign employees on value added, relative to 1998.
Coefficients from regression (1.11) are plotted in B and C. The dotted lines represent 95% CI, where all s.e are clustered at the firm level

Fig. 1.9: Distribution of capital and capital/labor ratio before-after 2000



Note: Log capital and log (capital/labor) are plotted in the first and second panels respectively

Fig. 1.10: Trend in un-weighted aggregate TFP index

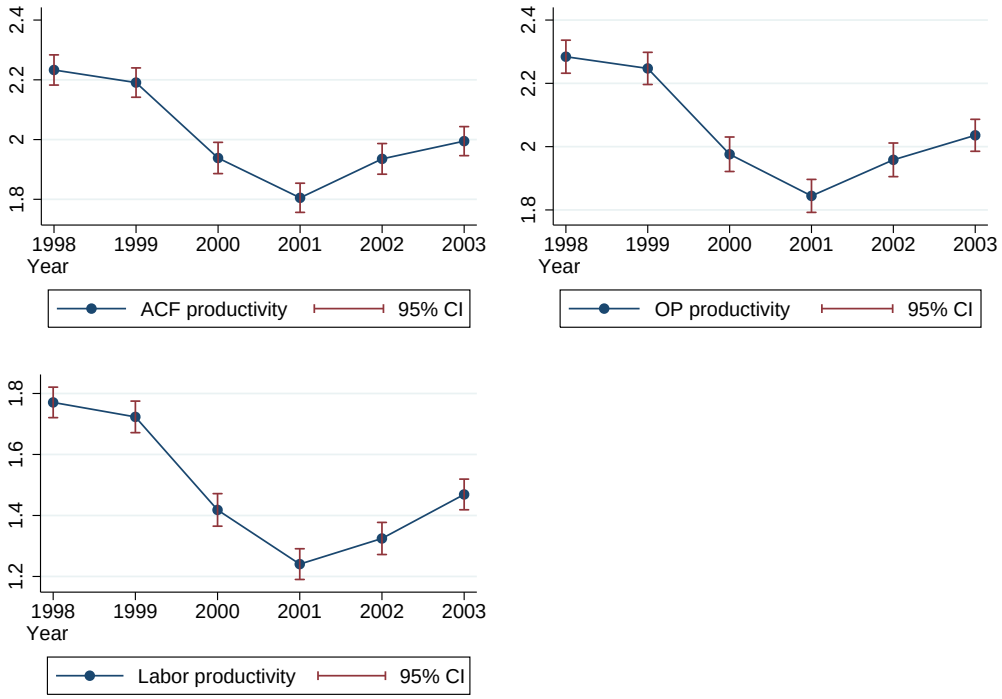
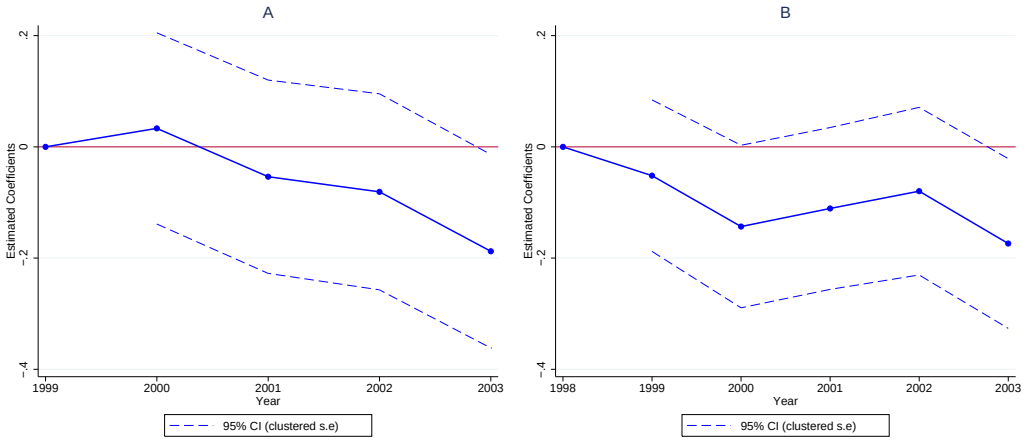


Fig. 1.11: Yearly effects of foreign ownership/employment on TFP



A: Impact of foreign ownership on TFP over time, where 1999's level has been normalized to 0.
B: Impact of foreign employment on TFP over time, where 1998's level has been normalized to 0.
Coefficients from regression (4) are plotted in A and B. The dotted lines represent 95% CI, where all s.e are clustered at the firm level

Tables

Tab. 1.1: Panel information

Year	Number of firms with missing Capital	Labor	Value-added	No of firms with valid values for K, L, value added
1998	345	331	922	1,879
1999	294	482	889	1,789
2000	274	462	693	2,078
2001	277	451	656	2,037
2002	224	358	608	1,941
2003	140	278	585	2,086
Total				11,810

Tab. 1.2: Panel length

Number of years in data	Number of firms
1	1,733
2	1,728
3	1,953
4	1,816
5	2,180
6	2,400

Tab. 1.3: Summary statistics

Variable	Obs	Median	Mean	Std. Dev.	Min	Max
Foreign ownership/employment:						
Foreign ownership	10811	1.00	0.53	0.50	0.000	1.00
% foreign employees	16348	1.94	15.96	23.76	0.000	100.00
% foreign employees (if>0)	8473	25.00	30.78	25.15	0.021	100.00
% French West Africans	16348	0.00	12.62	21.27	0.000	100.00
% other foreign employees	16348	0.00	3.33	10.27	0.000	100.00
Production function variables (full sample):						
Value added	17317	31.39	266.06	840.93	-410.79	9538.21
Capital	18437	23.73	402.90	1505.21	0.000	18026.10
Skilled labor	16409	3.00	19.63	88.52	0.000	2923.00
Unskilled labor	16409	4.00	44.07	245.50	0.000	10395.00
Total employees	16409	10.00	63.70	289.77	0.000	11019.00
Investment	18345	2.00	75.71	276.36	0.000	3348.71
Materials	17468	86.78	1037.25	3613.16	0.000	42397.08
Production function variables (after cleaning):*						
Value added	11810	61.40	331.06	862.26	0.004	9525.39
Capital	11810	32.23	384.53	1310.81	0.001	17965.25
Skilled labor	11810	4.00	16.59	54.06	0.000	1650.00
Unskilled labor	11810	6.00	38.96	145.81	0.000	5443.00
Investment	11716	4.63	77.41	262.10	0.000	3312.97
Materials	11699	154.31	1199.72	3722.53	0.000	42397.08
Conflict variables (by department):						
Conflict rate, overall	18771	0.67	1.28	1.02	0.000	8.04
Conflict rate, FWA	18771	0.99	1.43	1.60	0.000	10.95
Conflict rate, foreigners	18771	5.06	11.35	16.43	0.000	330.03
Other firm characteristics:						
Total assets	16939	146.27	2318.00	12802.64	0.008	455305.38
Sales	16044	247.41	1834.04	5743.94	1.820	60890.87
Sales per employee	14503	23.44	77.41	325.69	0.007	14201.23
Age of the firm in years	18436	6.00	10.32	11.34	1.000	104.00

Notes:
1. Monetary values are in constant 000's 1996 USD.
2. *The clean sample includes obs used in the production function estimations, where all obs in the one-percent tails of any monetary input/output variables are dropped.
3. Conflict rate is the number of armed conflicts divided by total population by department

Tab. 1.4: Percentage of firms by ownership

Year	Ivorian	FWA	French	Foreign - other	Total
A: All obs with ownership information					
1999	45.05	3.59	29.77	21.59	1283
2000	45.63	3.40	29.97	20.99	2382
2001	42.71	3.50	31.52	22.28	2002
2002	45.23	3.83	28.36	22.59	2116
2003	53.10	3.50	22.95	20.44	3028
Total	47.04	3.55	27.95	21.46	10811
B: Balanced sample with ownership information					
1999	43.69	3.47	29.80	23.03	547
2000	39.44	3.28	37.74	19.55	885
2001	34.74	3.13	39.42	22.72	832
2002	35.66	3.37	38.55	22.41	830
2003	40.36	3.10	34.40	22.14	1224
Total	38.61	3.24	36.27	21.89	4318
C: TFP sample with ownership information					
1999	42.18	2.83	31.27	23.72	742
2000	45.66	3.32	32.06	18.97	1566
2001	41.67	3.38	32.96	22.00	1332
2002	42.98	3.51	31.25	22.26	1424
2003	52.70	3.43	24.76	19.11	2072
Total	46.06	3.35	29.86	20.73	7136

Tab. 1.5: Transition rate in/out of foreign status

	Year (t)				
	1999	2000	2001	2002	2003
Foreign employment:					
Total share of switchers	0.118	0.136	0.147	0.133	0.128
Zero in $t-1$, positive in t	0.058	0.065	0.076	0.071	0.071
Positve in $t-1$, zero in t	0.060	0.071	0.070	0.062	0.057
Foreign ownership:					
Total share of switchers		0.398	0.068	0.051	0.065
Zero in $t-1$, positive in t		0.174	0.034	0.025	0.027
Positve in $t-1$, zero in t		0.224	0.034	0.026	0.037

Tab. 1.6: Firm characteristics differentials between foreign owned and Ivorian firms

Variable	Means Ivorian	Relative differences* FWA	French	Foreign-other	N	R^2
Number of firms	4615	346	2733	2110	9804	
Characteristics:						
Log of total assets	5.669	0.667***	0.619***	0.217*	9804	0.202
Employment	86.343	22.591*	25.306*	-1.735	9417	0.13
Percentage skilled labor	44.12	5.434**	3.721***	-5.819***	9230	0.088
Percentage Ivorian workers	85.798	-9.925***	-9.861***	-10.324***	9273	0.153
Percentage FWA workers	11.629	7.513***	6.962***	5.773***	9273	0.161
Percentage foreign workers	13.905	9.689***	9.831***	9.821***	9273	0.156
Log of wage	14.39	0.417***	0.439***	0.016	9377	0.072
Log of staff cost	17.116	0.461***	0.609***	-0.057	9775	0.441
Log (VA/L)	2.234	0.359***	0.409***	0.11	8369	0.116
Log(K/L)	1.343	0.261**	0.088	-0.132	9146	0.107
Investment/lagged assets	0.1	0.004	-0.003	0.006	6712	0.038
Receivables/sales	0.257	0.031*	0.039***	-0.016	9224	0.103
Payables/sales	0.763	0.01	-0.034	0.014	8853	0.069

Notes:

$*$ Coefficients from OLS regressions, controlling for year, industry & size FEs

(in equations without ln(total assets)/employment on the LHS)

Tab. 1.7: TFP and foreign ownership/employment over time - full sample

Dependent variable: $ln(TFP_{it})$	ACF estimates			
	(1)	(2)	(3)	(4)
Foreign ownership	0.0426	0.0418		
	(0.0655)	(0.0642)		
Foreign ownership × after	-0.0744	-0.0716		
	(0.0808)	(0.0788)		
Foreign employment			0.0961*	0.0170
			(0.0505)	(0.0506)
Foreign employment × after			-0.203***	-0.0971**
			(0.0483)	(0.0487)
Ln(total asset)	0.259***	0.296***	0.214***	0.245***
	(0.0915)	(0.0909)	(0.0712)	(0.0711)
Ln(total asset) squared	0.00296	-0.00103	0.00624	0.00267
	(0.00921)	(0.00919)	(0.00731)	(0.00728)
Size above median	-0.272***	-0.00437	-0.201***	-0.0824
	(0.0487)	(0.0898)	(0.0385)	(0.0525)
Age above median	-0.0427	0.0423	-0.0868**	0.0390
	(0.0523)	(0.0709)	(0.0419)	(0.0472)
Size × after		-0.304***		-0.179***
		(0.0881)		(0.0503)
Age × after		-0.108*		-0.228***
		(0.0645)		(0.0468)
Firms FE	Yes	Yes	Yes	Yes
Industry×year FE	Yes	Yes	Yes	Yes
Region×year FE	Yes	Yes	Yes	Yes
Constant	0.661	0.894**	0.824***	0.666***
	(0.404)	(0.363)	(0.187)	(0.188)
Observations	6344	6344	10204	10204
R-squared	0.140	0.145	0.132	0.139
Number of firm_id	3106	3106	4084	4084

Notes:

1. after indicates the conflict period 2000-2003

2. Clustered standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Tab. 1.8: Impact of conflict and foreign employment on TFP - full sample

Dependent variable: $ln(TFP_{it})$	ACF estimates		
	(1)	(2)	(3)
Conflict rate	-0.107*	-0.116**	-0.118**
	(0.0551)	(0.0547)	(0.0547)
Foreign employment	-0.00727		
	(0.0489)		
Foreign employment × conflict rate	-0.0463**		
	(0.0191)		
Percentage foreign employees		0.000100	
		(0.000953)	
% foreign employees × conflict rate		-0.000638*	
		(0.000387)	
% FWA employees			0.000842
			(0.00105)
% other foreign employees			-0.00253
			(0.00172)
% FWA employees × conflict rate			-0.000936**
			(0.000439)
% other foreign employees * conflict rate			0.000501
			(0.000799)
Firms FE	Yes	Yes	Yes
Industry × year FE	Yes	Yes	Yes
Region × year FE	Yes	Yes	Yes
Total assets × year FE	Yes	Yes	Yes
Constant	0.300*	0.295*	0.290*
	(0.170)	(0.168)	(0.169)
Observations	10287	10287	10287
R-squared	0.133	0.132	0.132
Number of firm_id	4107	4107	4107

Clustered standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Tab. 1.9: Channels

Dependent variable: $ln(TFP)_{it}$	Demand		Cost	
	Exporting	Concentration	Importing	Transport
	(1)	(2)	(3)	(4)
Foreign employment×after	-0.117**	-0.101**	-0.105*	-0.0979*
	(0.0578)	(0.0508)	(0.0558)	(0.0513)
Export oriented×after	-0.445			
	(0.288)			
Export oriented×foreign emp	0.0472			
× after	(0.0731)			
High concentration×after		-0.324		
		(0.300)		
High concentration×foreign emp		0.0265		
× after		(0.103)		
Import oriented×after			-0.565**	
			(0.230)	
Import oriented×foreign emp			0.0446	
× after			(0.0722)	
Transportation cost×after				-0.770
				(0.559)
Transportation cost×foreign emp				-0.329
× after				(0.661)

Notes: specifications1-4 include controls as in columns 2 and 4 of Table 1.7

1. Clustered standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.
2. Export and import orientation are industry indicators of positive export/import in 1999.
3. High concentration indicates industries with above median Herfindahl index in 1998.
4. Transportation cost: transportation cost/sales, measured at the firm level.

Tab. 1.10: Robustness checks in sub-samples

	Age>1	Sample: below median		Sample: above median	
		Size	Age	Size	Age
		(2)	(3)	(4)	(5)
Spec: before-after					
Foreign ownership×after	-0.0457				
	(0.0758)				
Foreign emp×after	-0.0965*				
	(0.0502)				
Observations	9415				
Spec: conflict intensity					
% FWA emp	-0.000883**	-0.00152**	-0.000582	-0.000551	-0.000968**
×conflict rate	(0.000427)	(0.000686)	(0.000793)	(0.000588)	(0.000491)
% other foreign emp	0.000368	7.84e-05	0.000621	0.00123	-0.000234
×conflict rate	(0.000812)	(0.00125)	(0.000926)	(0.00104)	(0.00119)
Observations	9500	4899	4958	5388	5412

Clustered standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Tab. 1.11: Trends in skill and nationality composition

	Mean	Difference relative to 1998				
	1998	1999	2000	2001	2002	2003
% FWA employees	14.87	-0.918**	-1.782***	-2.667***	-3.924***	-4.267***
% other foreign emp	3.65	-0.334	0.32	0.0363	-0.279	-1.659***
% skilled employees	40.91	-0.369	0.166	1.438	2.281**	3.575***

Clustered standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Tab. 1.12: Labor adjustments

Dependent variable	Share of foreigners over all employees		
	(1)	(2)	(3)
After 2000	-0.0178***	-0.0179***	-0.0143***
	(0.00403)	(0.00402)	(0.00502)
Wage ratio (firm level)		-0.000974***	-0.00122***
		(0.000299)	(0.000375)
Size (lagged employment/1000)			-0.00862
			(0.0416)
Size × after			-0.0291
			(0.0211)
Size^2			-0.00141
			(0.00480)
Size^2 × after			0.00273
			(0.00261)
Constant	0.283***	0.287***	0.287***
	(0.00250)	(0.00281)	(0.00529)
Firm FE	Yes	Yes	Yes
Observations	4412	4412	2764
R-squared	0.010	0.014	0.019
Number of firms	2035	2035	1381

Notes: all regressions are run on a sample of firms with wage data

1. Clustered standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.
2. Foreign implies French West African (FWA) in all specifications.
3. Wage ratio: calculated as average $\text{wage}_{\text{Ivorian}}/(\text{wage}_{\text{Ivorian}}\text{-wage}_{\text{foreign}})$.

Tab. 1.13: Effect of foreign employment on productivity (endogenous foreign employment)

Industry group	Mec. & electrical products	Construction & maintenance	Transport & communications	Rental & mgmt. of buildings	Commerce
% foreign employees	0.00905	0.00558*	0.0129**	0.00752**	0.00184**
% foreign emp×after	-0.00612**	-0.00494**	-0.00814*	-0.00400**	-0.00241***
After 2000	-0.405	-0.523	-0.102	-0.339	-0.281***

Clustered bootstrap standard errors *** p<0.01, ** p<0.05, * p<0.1. Foreign denotes % foreign employees.

Tab. 1.14: Effect of foreign employment on productivity (endogenous foreign employment), by size

Industry group	6		10		11		13		14	
Size	small	large	small	large	small	large	small	large	small	large
% foreign employees	0.0117	0.00975	0.0172	-0.00158	0.0219***	0.00912*	-0.000523	-0.00221	0.00214	0.00267*
% foreign emp×after	-0.0233	-0.0340*	-0.0111	-0.00410	-0.0116*	-0.0131**	-0.00111	0.00145	-0.00234**	-0.00264**
After 2000	0.305	0.306	-0.237	-1.030	0.463	0.206	-0.342**	-0.187	-0.307**	-0.299***

Notes:

1. Clustered bootstrap standard errors *** p<0.01, ** p<0.05, * p<0.1. Foreign denotes % foreign employees.
2. Size is measured in terms of employment. Small: below sample median. Large: above sample median (by industry)
3. Industry groups: 6 - Chemicals, rubber products, glass and building materials, 10- Construction and maintenance, 11- Transport and communications, 13- Other services, 14 - Commerce

Chapter 2. The impact of affirmative action and equity regulations on manufacturing firms in Malaysia

1 Introduction

During Malaysia's most recent general election in May 2013, criticisms against the country's affirmative action policies became a leading attack argument from the opposition party (The Economist 2013). This decades-old set of official ethnic discrimination was first implemented in 1971 under the New Economic Policy (NEP) to address ethnic tension between the Bumiputera (or "sons of the soil," implying ethnic Malays and other indigenous groups) and the Chinese. Initially introduced as a temporary measure, most of the NEP policies have stayed on until this day even as it is increasingly considered as a hindrance to the country's efficiency as well as the source of resentment by Chinese and Indian groups.

Widely regarded as one of the most comprehensive packages of affirmative action in the world, the NEP policies include measures favoring Bumiputera in a wide range of areas. In the public sector, large shares of university admissions and job quotas are reserved for Bumiputera. In the private sector, companies had to ensure that at least 30% of their shares as well as employment were held by this group. Government

contracts also explicitly favor Bumiputera businesses (The Economist 2003, Jomo 2004). While affirmative action policies are also prevalent in many other parts of the world, comprehensive pro-Bumiputera policies in the private sector is a unique feature of Malaysia's NEP.

There has been a relatively large body of analyses on the effects of NEP establishing its effect in increasing the education level and asset ownership of the Malays (Ong 2012). However, rigorous research of its impact on the private sector is scant. In this paper, I attempt to fill a gap in the literature by studying the impact of the NEP policies on businesses. I focus on the manufacturing sector, which experiences a regulatory change that relaxes the rule imposing equity restrictions on foreign-owned firms. This rule, which previously required all firms above a certain size threshold to reserve 30% equity for Bumiputera, was fully abolished for all foreign manufacturing projects since 2003.

I set up a simple theoretical model to show that the original policy results in a distortion that renders some firms to stay below their optimal employment size. Moreover, the policy change leads to two effects: (i) foreign firms are less likely to be sized constrained, and (ii) their average size increases relatively to other firms. My empirical analysis uses data from two rounds of the Malaysia Productivity and Investment Climate Survey in 2002 and 2007. I test for the above predictions in a reduced-form difference-in-difference framework. More specifically, I compare the outcomes of foreign and domestically owned firms between the first and the second survey rounds, which correspond to before and after the policy change.

My empirical results are consistent with the theoretical predictions. I find that the

policy significantly increases employment in foreign firms by 18-19%. Foreign firms that are below the size threshold in the first period are also 20% less likely to report that they are operating below the optimal level of employment. Further estimates with alternative definitions of foreign ownership and sample selection suggest that the results are robust. Moreover, I check for whether my results could be caused by another policy change in this period which relaxes regulations on expatriate employment and find evidence suggesting that this not the case.

The rest of the paper is organized as follows. Section 2 describes the institutional background, emphasizing the NEP policies in the manufacturing sector and the policy change in 2003. Section 3 presents a brief review of the literature. In section 4, I set up a theoretical model to motivate my hypotheses about the impact of this policy change on firms' employment size. Section 5 introduces the data and empirical framework used to test these hypotheses. Section 6 discusses the empirical results. Section 7 concludes.

2 Institutional background

Malaysia is a multi-racial country with a history of strong ethnic disparities and severe marginalization of the Bumiputera in economic activities. The ethnic composition in Malaysia is roughly 60% Bumiputera, 25% Chinese and the rest Indian and other ethnic groups. However, at the end of the British rules in 1957, Bumiputera accounted for only 2.5% of corporate assets against over 30% for the Chinese (The Economist 2003). Inter-ethnic tension between the Bumiputera and the Chinese escalated and eventually erupted into street riots on May 13th 1969. This single event

led to 200 deaths and prompted the government to respond with one of the most comprehensive packages of affirmative action in the world.

The set of affirmative action policies were introduced in 1971 through the New Economic Policy (NEP). The rhetoric focus of the NEP was both "poverty eradication regardless of race" and "restructuring society to eliminate the identification of race with economic function". In practice however, it had grown to emphasize and be associated mainly with the second objective (Jomo 2004). Overall, the NEP intended to pursue (a) correction of income imbalances existing between Bumiputera and other ethnic groups; (b) restructuring of the employment pattern; (c) restructuring of the inter-ethnic ownership of share capital in limited companies; and (d) the creation of a Bumiputera commercial and industrial community. The NEP set a specific target of 30 per cent ownership and control of the corporate sector by Malays and Malay interests by 1990. In addition, it also proposed that the Malays, who represent half of the population, proportionately participate in the commercial and industrial sectors which should lead to realization of a Bumiputera commercial and industrial community. With regard to employment, it expected private sector companies to accommodate at least 30 per cent of managers/senior management from among Bumiputeras (Onozawa, 1991).

While the NEP officially ended in 1990, most of its policies of favoring the Bumipuera still remains in effect through Malaysia's subsequent National Development Policy (NDP) for 1991-2000 and the National Vision Policy (NVP) implemented from 2001. Below, I describe in more details the Bumiputera equity and employment quotas in the manufacturing sector. Next, I present a discussion on the criticisms of these

policies and the change in equity requirements that will be analyzed in this paper.

2.1 Affirmative action and policy instruments in manufacturing

The NEP comprises a wide range of policies in education, public sector employment, business and asset ownership. My focus in this paper is the rule in the manufacturing sector that 30% equity and employment of private firms have to be reserved for Bumiputera. The main policy instruments used to enforce this requirement are implemented under the Industrial Coordination Act (ICA) 1975. The ICA primarily aims to direct growth in the manufacturing sector through its approval of manufacturing licenses. This license is essential for firms above a certain size threshold to operate legally or to obtain any formal government incentives, government contracts and to participate in exports.

For private manufacturing firms, the latest ICA revision requires that all manufacturers with size above 75 employees or with shareholders' funds exceeding RM 2.5 million to apply to the Ministry of International Trade and Industry (MITI) for a license (MIDA 2012). The approval of this license as well as any expansion and diversification projects is conditional on whether the NEP guidelines of minimum 30% Bumiputera participation have been met in shareholders' equity, employment share, Board of Directors and appointment of Malay distributors (Searle 1999).

2.2 Regulatory changes in the sample period

The effects of this equity policy are controversial. On the one hand, it is widely considered to have successfully created a Malay urban middle-class in the span of a

little over a generation (Ong 2012). Even the most conservative government estimates put the share of business equity owned by Bumiputera in 2004 at 18.7% (Centre for Public Policy Studies 2005). On the other hand, many criticisms are directed towards the inefficiencies that the policy creates. What the policy meant is that large companies are required to restructure their ownership to ensure Bumiputera's participation either through individual involvement or trust agencies. As a result, the policy's most deleterious effects was probably due to the fact that it introduced so much uncertainty into businesses planning that it was a disincentive against which all forms of incentive appeared unattractive (Searle 1999, Gomez and Jomo, 1999). Many Chinese companies are believed to have chosen to stay small and private, rather than growing to the point of having to comply with the NEP's requirements. Critics also argue that the policy has acted as a barrier to foreign investment and that only the Malay elite has benefited from it (The Economist 2003, 2009). An early estimate puts the amount of capital flight due to the ICA's pro-Bumiputera requirements at 12 billion USD from 1975 to 1984 (Lee 2002).

The policy proves to be increasingly unsustainable given competition for foreign investments from neighboring countries. As a result, to attract foreign investment, since June 2003, Malaysias government fully liberalized equity holdings in all foreign manufacturing projects (The Economist Intelligence Unit 2004). The new equity policy also applies to companies that operated prior to the changes but only recently became large enough, that is, exceeded shareholders' funds of RM 2.5m or those that engage 75 or more full-time employees, and are thus required to be licensed.

In this paper, I aim to test empirically whether this regulatory change has any

impact on firm size in terms of employment. The reason to focus on firm size in terms of employment as opposed to equity is due to data availability. My sample covers only two years of data and the percentage of firms that started business during the sample period is negligible (see Data section). Given raising equity is a rare event for firms, it is infeasible to analyze the impact on equity using the available data.

3 Literature review

While many analyses of Malaysia's NEP policies exist, the majority contains descriptive statistics documenting trends in different outcomes for Bumiputera and other ethnic groups over time. The only two existing rigorous studies that I am aware of focus on the effects in education, employment and wages. A recent paper by Ong (2012) looks at the NEP impact in public education, and suggests that affirmative action in secondary education did not affect the earnings of Malay secondary school graduates relative to their Chinese counterparts. Conversely, since there is capacity constraint in tertiary education, quotas reserved under the NEP led to an increase in the Malay-Chinese wage gap among tertiary graduates. In an earlier study, Fang and Norman (2006) document the widening Chinese/Malay (positive) wage gap since the NEP adoption using household data from the Malaysian Family Life Survey. To explain this gap, they introduce a model with two features: (i) there is imperfect information about skills in the labor market, and (ii) exclusion to public jobs creates better incentives for the minority group to invest in private sector skills. As a result of discrimination policy, the minority group is able to overcome informational free-riding problem. If this positive effect outweighs the direct effect in the public sector,

it is possible for them to have higher wage in equilibrium.

My paper contributes to understanding of the NEP's impact in the private sector. I specifically consider a policy change that aims to address one of the NEP's criticisms. As a study on firm size distortions, my paper is also related to the recent literature on misallocation of resources and its consequences for aggregate productivity. This literature has shown theoretically and empirically how distortions in the labor or capital market can reduce aggregate output through allocating resources away from high productivity to low productivity firms. For example, Hsieh and Klenow (2009), one of the most prominent papers in this literature, documents that misallocation can account for a large proportion of the productivity gap between China, India and the US.

A paper more closely related to my research is Garicano, LeLarge and Van Reenen (2013). They examine a specific set of labor laws in France as the source of distortion rather than using a theoretical distortion term as in most of the rest of the literature. The paper shows that these regulations, which impose significant labor costs on firms that exceed a certain size, lead to firms inefficiently staying below this size. Structural estimations using census data suggests the cost due to this size distortion can range from 1-5% of GDP, depending on wage flexibility. Similarly, Braguinsky, Lee and Regaterio (2011) show that Portugal's strong labor protection laws have contributed substantially to the continuing shift of the Portuguese firm size distribution to the left. Their calibration exercises indicate that large growth effects could arise if the distortions were lessened or abolished altogether.

A limitation of my work compared to these papers is a much smaller sample size

since I use survey data instead of census data. Moreover, my analysis cannot consider general equilibrium effects. On the other hand, I am able to observe a policy change during my study period, and this allows me to examine how firm size can be affected by actual changes in the extent of the policy distortions.

4 Model and hypotheses

In this section, I present a simple theoretical model to demonstrate the impact of the policy change on firm size. Assume that the firm produces output Y following a Cobb-Douglas function in capital K, labor L and productivity A drawn from some $f(A)$ distribution. Let $\pi(A)$ then be the profit function of the firm:

$$\pi(A) = AK^{\alpha}L^{\beta} - (r+\bar{\gamma})K - (w+\bar{\tau})L, \text{ with } \begin{cases} \bar{\gamma} = 0, \bar{\tau} = 0 \text{ if } L \leq \bar{L} \\ \bar{\gamma} = \gamma, \bar{\tau} = \tau \text{ if } L > \bar{L} \end{cases}$$

where r is the rental rate of capital and w is the wage rate. When employment passes a certain size threshold $\bar{L}$, the firm incurs additional costs in terms of both labor and capital. This assumption follows the regulation that firms have to reserve 30% equity and employment to Bumiputera once their size gets past 75 full-time employees.[1] If search cost for Bumiputera shareholders and employees are higher on average then these additional costs, γ and τ, are positive.[2] In other words, firms above the size

[1] I model these costs as variable costs only. There might also be fixed costs given the firm has to restructure but adding fixed costs does not change the comparative statistics in this set up.

[2] This is likely the case since Bumiputera employees are in general less skilled than Chinese and foreign workers. In addition to search cost for Bumiputera shareholders, there might also be disutility cost of diluting shares. Searle (1999, p.44) for example, claims that the ICA was vigorously opposed by both Chinese and foreign business, who "loath to transfer 30 percent equity to Malays".

threshold $\bar{L}$ incur higher per unit capital and labor costs.[3]

The first order conditions for the profit maximization problem of the firms with respect to labor and capital are:

$$\alpha \frac{Y}{K} = r + \bar{\gamma}$$

$$\beta \frac{Y}{L} = w + \bar{\tau}$$

From these FOCs, the optimal labor choice for the firm is given by:

$$L^{1-\alpha-\beta} = A(r+\bar{\gamma})^{-\alpha}(w+\bar{\tau})^{\alpha-1}\frac{\alpha^{\alpha}}{\beta^{\alpha-1}} \tag{2.1}$$

$$\Leftrightarrow A = L^{1-\alpha-\beta}(r+\bar{\gamma})^{\alpha}(w+\bar{\tau})^{1-\alpha}\delta \tag{2.2}$$

where $\delta = \beta^{\alpha-1}/\alpha^{\alpha}$.

From equation (2.1), it is clear that the firm's optimal size is increasing in its productivity A. Moreover, equation (2.2) shows that the values of A that make a firm unconstrained in choosing its size (in other words, when the firm can choose its optimal L according to the FOCs) are:

$$\begin{cases} A \leq A^1 = \delta \bar{L}^{1-\alpha-\beta} r^{\alpha} w^{1-\alpha} \\ A > A^2 = \delta \bar{L}^{1-\alpha-\beta}(r+\gamma)^{\alpha}(w+\tau)^{1-\alpha} \end{cases}$$

[3] This assumption of higher total capital cost for firms above $\bar{L}$ depends partly on the assumption that r does not change with size. It might be violated for very large firms if they are able to borrow sufficiently more cheaply, for example, because of investment incentives or because of market power. Nevertheless, it is likely to hold for firms close to the size threshold.

where $A^1 < A^2$ given the assumptions that $\gamma, \tau > 0$ and $\alpha, \beta \in (0, 1)$.

For all productivity draws between (A^1, A^2), the firm is constrained in its size. At $A = A^1$, the optimal size is $L = \bar{L}$. Since optimal size is increasing in A, it follows that all firms whose $A \in (A^1, A^2)$ also choose $L = \bar{L}$. At $A = A^2$, the firm is indifferent between staying below and going above the size threshold since profits are equal under those two choices.

In summary, firms with productivity below A^1 optimally stays under $\bar{L}$. Firms with productivity above A^2 can also choose their labor optimally above $\bar{L}$. For firms whose productivity are in between A^1 and A^2, they are size constrained and choose to hire exactly $\bar{L}$ workers. Intuitively, these are the firms with productivity values such that it is too costly to hire more labor than the threshold and subject to the additional costs. However, their productivity is high enough so that they would like to have size larger than $\bar{L}$ if not for the distortion caused by the size-dependent regulation.

Now suppose the policy changes so that a subset of firms no longer have to satisfy the 30% Bumiputera equity condition. In other words, there is no additional capital cost, i.e. $\bar{\gamma} = 0$ when $L > \bar{L}$, when these firms exceeds the threshold $\bar{L}$ (additional labor cost still present since the 30% Bumiputera employment condition is still in place). As a result, the upper bound of A such that a firm is no longer size constrained becomes smaller for a subset of firms. Let this threshold be $\tilde{A}^2$ then:

$$\tilde{A}^2 = \delta \bar{L}^{1-\alpha-\beta} r'^{\alpha} (w' + \tau')^{1-\alpha} < A^2 = \delta \bar{L}^{1-\alpha-\beta} (r' + \gamma')^{\alpha} (w' + \tau')^{1-\alpha}$$

Note that there is a new subscript on wages and capital rental rates as well as their

additional costs in the above expression. In a general equilibrium model, changes in firms' labor demand can change the equilibrium prices of labor and capital. For simplicity, I am only using a partial equilibrium model and do not model these effects but allow for prices to be different with the policy change.

Denote the set of firms that are subject to the policy change as F. If all firms have the same support of the productivity draw A then the proportion of firms $\in F$ that are constrained to be too small is smaller under the new policy change :

$$Pr(L < L^{optimal})|_{\in F} = \int_{A^1}^{\tilde{A}^2} f(A)dA < Pr(L < L^{optimal})|_{\notin F} = \int_{A^1}^{A^2} f(A)dA$$

We can also predict how the the average firm size changes with the policy given the new productivity threshold. The average size for firms not affected by the regulation change is then[4]:

$$\begin{aligned} E(L)|_{\notin F} &= E(L)|_{A<A^1} Pr(A < A^1) + \bar{L} Pr(A^1 < A < A^2) + E(L)|_{A>A^2} Pr(A > A^2) \\ &= E(L)|_{A<A^1} Pr(A < A^1) + \bar{L}[Pr(A^1 < A < \tilde{A}^2) + Pr(\tilde{A}^2 < A < A^2)] \\ &+ E(L)|_{A>A^2} Pr(A > A^2) \end{aligned}$$

where $E(L)$ is the average firm size in any range of A given equation (2.1).

[4] The expression below ignores the lower threshold of A which results in zero profits. In the non-trivial case where this threshold is below A^1, the results do not change since the threshold would be the same regardless of whether the firm is subjected to the regulation change or not.

The average size for firms that are affected by the regulation change is:

$$\begin{aligned} E(L)|_{\in F} &= E(L)|_{A<A^1} Pr(A < A^1) + \bar{L} Pr(A^1 < A < A^2) + E(L)|_{A>A^2} Pr(A > A^2) \\ &= E(L)|_{A<A^1} Pr(A < A^1) + \bar{L} Pr(A^1 < A < \tilde{A}^2) + E(L)|_{A>\tilde{A}^2} Pr(\tilde{A}^2 < A < A^2) \\ &+ E(L)|_{A>A^2} Pr(A > A^2) \end{aligned}$$

From the above two equations, it clearly follows that foreign firms have higher size on average. Intuitively, the average size of foreign firm has to be higher since there is a range of productivity where foreign firms can choose size above $\bar{L}$ optimally while other firms cannot..

In summary, these predictions form the following hypotheses, which will be tested empirically:

Hypothesis 1: *The average size of foreign firms increases after the regulation change.*

Hypothesis 2: *The relative probability of foreign firms being constrained to be too small decreases after the regulation change.*

5 Empirical tests

5.1 Data

My analysis is based on two rounds of data from the Malaysia Productivity and Investment Climate Survey (PICS) I and II. The first and second rounds are conducted in 2002 and 2007 respectively, thus covering firms both before and after the regulation change in 2003. These surveys are part of the World Bank's Enterprise Survey series

and are jointly conducted with the Government of Malaysia. The surveys include firms in both manufacturing and business support services sectors. However, I only use data for manufacturing firms, which are subject to regulations by the ICA.

The two rounds share a similar sampling method and the same sets of questionnaires. The sampling frame comes from the Central Register of Establishments maintained by the Department of Statistics. Samples are drawn following a single-stage stratified sampling method. Within each sector, firms are stratified by location and industry, and are drawn proportionally according to the total sample size.[5] They are representative of the whole economy but only establishments with employment size above 10 are included for the manufacturing sector (World Bank 2009).

The data cover identification information such as firm location, year starting operation, legal status as well as detailed ethnic and nationality composition of shareholders. Firms' economic activities are defined according to Divisions (2-digit codes) under the Malaysia Standard Industrial Classification (MSIC) 2000, which is identical to the ISIC Rev. 3.1 up to the 4-digit level. The final sample size for manufacturing firms is 1115 and 902 observations in the first and second survey round respectively, of which 976 are panel observations. Table 2.1 presents the distribution of firms by industry in both rounds. Figure 2.2 displays the distribution of firms by age group. It shows that the presence of newly established firms is negligible this data set. As explained above, this limitation is partly the reason for my analysis to focus on employment instead of equity as an outcome.

[5] The PICS sampling unit is establishment. I use firm and establishment interchangeably from here onwards.

The firm questionnaire includes questions on firms' opinions about investment climate that are standard in the Enterprise Survey series as well as recall balance sheet information on employment, capital, investment, etc. Balance sheet data are available for 6 years, 1999-2001 and 2004-2006.

Ethnic and nationality of shareholders are only available for 2 years (2001 and 2006). Table 2.3 shows the firm-level average percentages of shares held by each group of shareholders by survey round. Since these are my main variables of interest, my analysis makes use of only these two years of data. Table 2.4 shows the pooled summary statistics of firm characteristics for 2001 and 2006. About 28% of firms in the sample can be classified as foreign (defined as 30% or more shares being held by foreigners). Roughly half have either more than 75 permanent workers or shareholders' equity above 2.5mil RM and are thus subject to the licensing requirement under the ICA. Given these percentages, my sample has relatively good balance of firms in the "treated" and "control" groups (defined below).

5.2 Model and estimation strategy

I test Hypotheses 1 and 2 using a difference-in-difference (DID) approach. Given the timing of the policy, the post "treatment" period is the second wave of the survey. I define the "treated" group as firms that are foreign-owned, as they are the ones that are subjected to the regulatory change. Domestic firms then constitute the "control" group.

The above definition is dictated by data availability. Ideally, one would want to define the treated and control groups more precisely according to the firm's size in

2001. The ideal treated and control groups would be foreign firms that are just below the size threshold in 2001, and domestic firms of the same size, respectively. However, this categorization is not feasible due to the small sample size. Figure 2.1 displays the distribution of firms in both sample periods, showing that the percentage of firms close to the size threshold is relatively small. For example, only 11% of firms in the panel sample have between 50-75 employees in 2001, which results in a total of only 100 panel observations.

Alternatively, since only foreign firms in the manufacturing sector are affected by the policy change, foreign firms in the services sector can also serve as the control group. This construction of control and treatment groups is likewise infeasible however since I only have data for services firms in the second period. In sum, I use the full sample of manufacturing firms. I will also consider the sub-sample of firms which were below the size threshold in 2001. Intuitively, these are the firms where we should observe a larger impact of the policy change. Table 2.5 presents summary statistics of the firms by their treatment status, both before and after the policy change. Foreign firms are generally larger both in terms of employment and assets, which suggest that the DID analysis needs to control for these differences.

Hypothesis 1 is tested in the following linear regression:

$$ln(emp)_{it} = \alpha_{i0} + \alpha_1 Foreign_{it} + \alpha_2 after + \alpha_3 Foreign_{it} \times after + \alpha_4 Xit + \varepsilon_{it} \quad (2.3)$$

where $ln(emp)_{it}$ is log of the total number of all permanent employees at firm i in year

t.[6] $Foreign_{it}$ is an indicator denoting if at least 30% shares of the firms are owned by foreigners,[7] and $after$ denotes the second survey round. Controlling for the period dummy takes into account the effect of any other factors that could affect all firms to the same extent. I also control for $Foreign_{it}$ given that foreign firms are often different from domestic firms in various characteristics, including size. In addition, I include in X_{it} a set of city fixed effects (FE), industry FE and their interactions with time. Since foreign firms can be disproportionally represented in certain locations and industries, controlling for these FE ensures that my results are not due to confounding effects of other policies that are location or industry-specific and constant throughout the study period. Finally, I include firm FE to control for productivity and other time-invariant unobservables that could be correlated to foreign ownership and affect firm size at the same time. Because the panel sample is significantly smaller than the pooled sample, I also run equation (2.3) without fixed effects and control for firm age in those specifications to test for the prediction in the full sample.

The coefficient of interest is α_3. Testing Hypothesis 1 is equivalent to testing $\alpha_3 > 0$, that is, if foreign firms are becoming larger on average in the second period. My identification assumption is that conditional on firm FE and other controls included in X_{it}, there are no omitted effects impacting employment of foreign firms and domestic firms differently over time so the DID results can be attributed to the policy change that affects only foreign firms.

[6] This measure is taken from the section on "Labor and human resources" in the firm questionnaire. I do not include temporary workers since part-time employment is included in the number of temporary workers and the size threshold in the ICA regulation is with respect to full-time employees only.

[7] I will consider other indicators of "foreign" in the robustness checks.

For testing Hypothesis 2, the question used to construct my dependent variable comes from the questionnaire's section on labor relations. More specifically, the questions asks: *"Given your current level of output, if you were free to choose without restrictions your current level of employment what percentage of the current level would you choose?"*. I construct a categorical variable which takes the value of 1,2 and 3 if the firm response is more than 100 percent, equal to 100 percent, and less than 100 percent respectively. I test for Hypothesis 2 in the following generalized ordered logit specification:

$$Pr(y_i > k) = g(\delta_k + Z_i\beta_k) = \frac{exp(\delta_k + Z_i\beta_k)}{1 + exp(\delta_k + Z_i\beta_k)} \tag{2.4}$$

where

$$Z_i\beta_k = \beta_{k1}Foreign_{it} + \beta_{k2}after + \beta_{k3}Foreign_{it} \times after + \beta_{k4}X_{it}$$

and $k = 1, 2, .., M-1$. M denotes the number of categories of the dependent variable ($M = 3$ in this case). I consider both the general case where the slope β's are allowed to differ across k and the more parsimonious model where β's are restricted to be the same (the ordered logit).

As in the previous regression, I include the set of firm characteristics and fixed effects in X to control for possible correlations of foreign ownership with other factors affecting firm size. The coefficient of interest is β_{k3} where $k = 1$. However, since coefficient estimates in this model are often hard to interpret,[8] I will report marginal

[8] Hypothesis 2 states that the probability of foreign firms constrained to be too small decreases

effect estimates. Hypothesis 2 implies a negative marginal effect of the interaction term $Foreign_{it} \times after$ on the outcome $y_i = 1$.

6 Results and discussion

6.1 Main results

Table 2.6 presents results from equation (2.3).[9] Column (1) show the results in the pooled sample with the full set of time-varying city and industry FE. The results show that foreign firms are larger on average. The coefficient on the interaction term between foreign and the period dummy indicates their size increases by 16.3 percent after the regulation change but it is not significant. Column (2) shows the results in the panel sample with firm FE. The effect of being foreign owned becomes negative but insignificant when firm FE are controlled for. The policy change effect in this specification is smaller than in column (1) and is also insignificant. When I further control for the set of location and industry year FE in column (3), the coefficient on the interaction term is of similar magnitude as in column (1) and becomes significant at 5%. The results indicate that the change in equity policy has the effect of increasing size of foreign firms by 18.7 percent on average. The positive effect on employment is as predicted by Hypothesis 1.

Columns (3)-(6) display the results with the same specifications as in columns (1)-

in the period after the regulation change. Since the probability of being "too small" is $1 - Pr(y_i > 1) = 1/(1 + e^{Z_i\beta_1})$, which decreases in Z_i if $\beta_1 > 0$, this implies $\beta_{13} > 0$.

[9] All standard errors are clustered at the firm level. The results are from a sample dropping the top and bottom 5% tails employment. I also drop publicly listed firms from the sample since they are not subject to the same regulation change. The results are however insensitive to whether they are included or not.

(3) but in a sub-sample where I only include firms that are below the size threshold, that is having less than 75 employees, in 2001. The results are qualitatively similar as in the full sample. The coefficient on the interaction between foreign and after in column (6) becomes more imprecisely estimated than in column (3) since the sample size reduces. However, its magnitude also increases, which should be the case since the policy should have an effect mostly on firms that were below the size threshold in the initial period.

Before going into the regression results on Hypothesis 2, I first check for changes in the proportions of firms with different size constraints in table 2.7. The results suggest that the number of foreign firms reporting being smaller than optimal has decreased in the second period but no similar changes are observed for other firms. Next, table 2.8 presents formal results with marginal effect estimates for each outcome of the dependent variable from equation (2.4). I report results from both the ordered logit and the generalized ordered logit in the upper and lower panels respectively.[10] The marginal effect estimates of $Foreign_{it} \times after$ on the first outcome (i.e. on the probability of firm size being "too small") have the same expected sign in both models and across different specifications. However, its estimated marginal effects on the other two outcomes are different in signs, magnitude and significance level in the ordered logit model compared to the generalized logit model. Results from Wald tests for the restrictions on equality of coefficients in the ordered logit model, i.e. $\beta_k = \beta$ for all k in equation (2.4), suggest that the ordered logit is too restrictive.[11]

[10] Note that unlike in the previous results from equation (2.3), my results in this table only control for state instead of city FE. The reason is that the results do not converge in some of the specifications. Results are otherwise insensitive to whether I control for city or state FE.

[11] The Wald tests are performed on each variable in the model iteratively. The tests fail for a

Therefore, my preferred model is the generalized model where the coefficients β_k are allowed to be different across k. In the discussion below, I focus on the results from this model.

Column (1) reports the full sample results when I control for the full set of location and industry-specific year FE.[12] The results suggest that foreign firms are 7% less likely to report being too small in the second round but the marginal effects are imprecisely estimated. Next, columns (2) shows estimation results from the sub-sample of firms whose size is below the 75 employee threshold in 2001. In this sample, the estimated impact of being foreign owned after the policy change is statistically as well as economically significant. Given the marginal effect estimate on "foreign," the results suggest that the policy has reduced the probability of foreign firms being smaller than the optimal size by around 20%. Moreover, there is no evidence that there is an impact on the probability of firms being "too large." As a result, the probability of firms being of optimal size also increases by about 20%.

Lastly, column (3) reports estimation results from the same specification as in column (2) but in the sample where firms were already above the size threshold in 2001. Since the policy change should not have an effect on these firms, we would expect the interaction between foreign ownership and the period dummy to have no effect on firms' size constraints. The results support this prediction.

In general, the results from the generalized ordered logit support the prediction

range of industry and location FE. A global LR test also rejects the null hypothesis that the slopes are the same for all k at 0.01 level of significance (The test statistics for the samples in columns 1, 2, and 3 are chi2(39) = 106.49, 78.68 and 78.78 respectively). LR test results however do not take into account clustering.

[12] Results are also similar when I do not control for these FE. For brevity, I only include estimations with the full set of FE in this table.

in Hypothesis 2. Foreign firms not only are more likely to increase in size but they are also significantly less likely to report as being constrained to be too small. On the other hand, there is no evidence that the probability of firms being larger than optimal has decreased. Taken together, the results suggest that the change in equity policy has helped reduce the size distortion on foreign firms.

6.2 Robustness checks

The previous results are based on the definition of foreign firms as firms owned by 30 percent or more by foreigners. This is the standard definition of foreign ownership in the literature. However, the rationale also rests in the fact the for firms that already have less than 30 percent foreign ownership, i.e. they already have more than 70 percent domestic shareholders, restructuring is less likely to be an issue. Nevertheless, I test for whether the results are different if I define all firms with any positive foreign ownership as being "treated". The resulted coefficient and marginal effect estimates on the interaction term from equations (2.3) and (2.4) are given in column (1) of table 2.9.These results correspond to the specifications with the full set of controls, that is, they correspond to column (3) and (2) in tables 2.6 and 2.8 respectively. The coefficient and marginal effect estimates on our variable of interest in both equations (2.3) and (2.4) are statistically significant and of comparable magnitude as in the estimations using the previous definition of foreign ownership.

Another concern with the previous results is that firms, in particular non-Bumiputera domestic firms, might switch to acquire foreign ownership since only foreign-owned firms are given the policy exemption. If there are other factors that affect employment

at these firms differently then I might incorrectly attribute the effects of these changes to the equity policy. To account for this possibility, ideally I would want to model the decisions of firms to change ownership. However, I do not have a good instrument that would affect firm's ownership but not it's employment. I choose a simpler approach to check the robustness of my previous results. I define the "treated" group as having foreign ownership in *both* rounds of the survey.

The results when 30 percent shares is used to define foreign ownership are reported in column (2) of table 2.9. For the regression on log employment, the coefficient estimate on foreign interacted with the period dummy is slightly smaller but still significant. The ordered logit results on firm size constraint are almost unchanged. The results suggest that firms that are at least 30% owned by foreigners are 18% less likely to employment below optimal level and the marginal effects are precisely estimated at 1% level. Next, column (3) shows the estimates when I define the "treated" group as having any positive shares held by foreigners in both survey rounds. The impact of foreign ownership on firm size constraint now becomes slightly smaller but still significant at 1% level. In general, my results are robust to these alternative definitions of foreign ownership.

Finally, I check for whether the results are sensitive to dropping firms which switched industries between the first and second rounds of the survey. Since firms might switch to another industry, for example to take advantage of industry-specific policies, if foreign firms are more likely to switch then my results might be spurious. The results in column (4) are essentially the same as in the full sample, suggesting that this is not the case.

6.3 Alternative explanations

The previous regressions have attempted to control for confounding factors that could be correlated with foreign ownership and affect firm size. However, it is still possible that there are other policies during the same period that target foreign firms and affect their size differently over time. To check for this possibility, I examined Country Reports by the The Economist Intelligent Unit in 2002-2006 for other policy changes during this period.

One potential confounding policy change is the relaxation of rules on employing expatriates in manufacturing, also implemented in June 2003. Faced with labor shortages in certain sectors, the government relaxed its stance on employing highly skilled foreigners and importing foreign laborers (The Economist Intelligent Unit 2004). While this policy could potentially impact all firms, foreign firms might particularly benefit from it if they are more likely to employ skilled workers.

In table 2.10, I check whether there is evidence that foreign firms employ more foreign workers after the policy change. The percentage of skilled foreign workers in foreign firms actually decreases on average while increasing for domestically owned firms. The simple DID calculation in means is -3.17%. Similarly, the DID in percentage unskilled foreign workers is also negative at -2.97%.

To formally check for the impact of the policy change with respect to expatriate employment, I rerun the regressions in equations (2.3) and (2.4) controlling additionally for the firm's percentage of skilled labor in 2001, interacted with the period dummy. If foreign firms are more likely to benefit because they are more likely to need

skilled foreign employees then controlling for this interaction term would separate out the impact of this labor policy change from the equity policy. The results with all location and industry FE are reported in table 2.11. Columns (1) and (2) show the results on log employment and on firm size constraints respectively. While the coefficient estimates on the percentage skilled labor interaction suggests that there might be a positive effect of the labor policy on firm employment, the coefficient estimate on the interaction with foreign ownership are still significant and of similar magnitude as before. The impact of foreign ownership on firm size constraints are also similar to the results not controlling for skilled labor. Therefore, it is unlikely that my results above are confounded with the change in expatriate employment rules.

7 Conclusion

By many standards, affirmative action in Malaysia could be considered a success as it managed to lead to a rapid advancement of the Bumiputera, its designated beneficiary group, in various areas such as education, public sector employment, and asset ownership. In addition, Malaysia was able to avoid of the kind of widespread violence associated with affirmative action found in India and elsewhere (Sowell 2004). However, the myriad of government interventions under the NEP policies have also been widely criticized for creating efficiencies and discouraging both Chinese and foreign investments. Nevertheless, there has not been much formal empirical evidence on the impact the NEP on businesses.

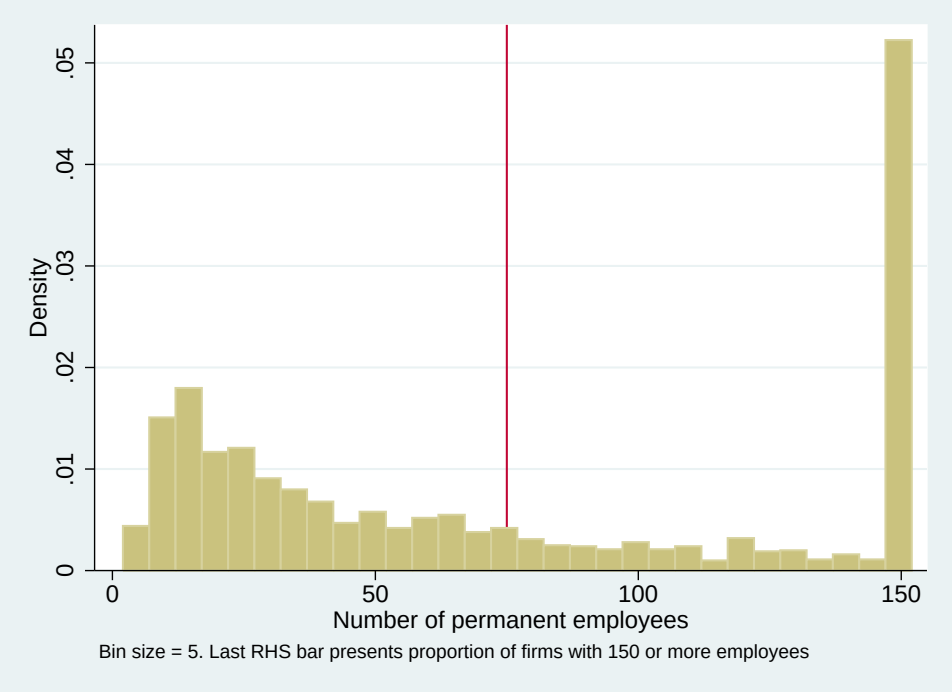
In this paper, I study one aspect of the pro-Bumiputera policies in the private sector. More specifically, I examine the impact of a policy change which no longer

requires foreign-owned manufacturers to set aside 30% of their equity for Malays shareholders once their size passes a certain threshold. I develop a partial equilibrium model to guide empirical predictions on the impact of this change on firm size in term of employment and use data from the Malaysia Productivity and Investment Climate Surveys in 2002 and 2007 to test these predictions. My model shows that there is a range of productivity values where firms would find it too costly to operate above the size threshold and therefore stay below their otherwise optimal size. Given data limitations, I do not analyze the general equilibrium effects of the policy change. Since labor demand changes can affect equilibrium wages, it is not clear how firm size will change for all firms along the whole productivity distribution. In my model, removing the restrictions for foreign firms means that they are less likely to be size constrained. Consequently, the average size of foreign firms also increases accordingly. DID regressions results find evidence supporting these predictions.

Despite the limitations mentioned above, my results indicate that the equity restriction has an effect as a size distortion on firms and relaxing it allows firms to increase their employment to a more efficient size. In recent years, Malaysia has implemented further similar reforms in the service sector as well as with publicly listed companies. Evaluating the impacts of these reforms, particularly their consequences on aggregate productivity, is an interesting and important area for future research.

Figures

Fig. 2.1: Firm size distribution



Tables

Tab. 2.1: Sample distribution by industry

Industry	PICS round 1	PICS round 2	Total	Panel obs (non-switchers)	No. of switchers
1. Food Processing	207	243	450	276	3
2. Textiles	30	40	70	28	3
3. Garments	102	90	192	94	4
4. Wood & Wood Products	4	28	32	2	4
5. Chemicals & Chemical Products	33	82	115	40	5
6. Rubber and Plastics	249	281	530	242	11
7. Machinery and Equipment	87	93	180	76	9
8. Office, Accounting & Comp. Machine	0	10	10	0	0
9. Electrical Machinery & Apparatus	0	27	27	0	3
10. Electronics (Equip. & Components)	75	84	159	64	6
11. Motor Vehicles and Parts	38	35	73	32	4
12. Furniture	77	102	179	64	6
Total	902	1,115	2,017	918	58

Tab. 2.2: Number of firms by age group

Age group	Number of firms in round 1	Number of firms in round 2	Total	% firms in round 1	% firms in round 2
1 (entering)	2	2	4	0.22	0.18
2 to 5	81	38	119	8.97	3.45
6 to 10	214	154	368	23.70	13.96
10+	605	907	1,512	67.00	82.23

Tab. 2.3: Firm ownership by round

Round	1	2	Total
Percentage shares held by foreigners	23.97	22.99	23.43
Percentage shares held by Bumiputera	11.45	10.58	10.96
Percentage shares held by Chinese	57.76	59.60	58.80
Percentage shares held by other domestic owners	6.07	6.80	6.48

Tab. 2.4: Summary statistics

Variable	Obs	Mean	S.d	Min	Max
Foreign (foreign shares $\geq$ 30%)	2017	0.28	0.45	0.00	1.00
Age	2003	17.73	11.27	1.00	215.00
Number of permanent workers	2002	180.67	428.90	2.00	6709.00
% firms with $\geq$ 75 workers	2002	41.76	49.33	0.00	100.00
Equity	1938	9215.80	66695.15	-149.61	2103755.00
% firms with equity $\geq$ 2.5 mil RM	1938	31.32	46.39	0.00	100.00
% firms above emp/equity threshold	1964	50.51	50.01	0.00	100.00
Total assets	1919	32922.76	94217.77	11.78	1158128.00
% of total export sales	1968	35.89	40.37	0.00	100.00
Sales	1961	51746.53	156822.30	4.71	1733783.00

Monetary values are in thd RM and deflated using PPI index (2000 as base).
The percentage of firms above equity threshold are calculated using nominal equity

Tab. 2.5: Summary statistics for treated and control groups

		Obs		Mean		Difference	p-val
		Domestic	**Foreign**	**Domestic**	**Foreign**		
BEFORE							
	Age	644	258	17.404	15.616	-1.787	0.044
	Number of permanent workers	640	257	118.517	292.432	173.915	0.000
	% firms with more than 75 workers	640	257	31.563	63.813	32.251	0.000
	Equity	602	240	8681.205	13390.956	4709.751	0.445
	% firms with equity above 2.5 mil RM	602	240	20.100	46.667	26.567	0.000
	% firms above emp OR equity threshold	612	248	39.052	75.000	35.948	0.000
	Total assets	595	234	21680.810	58925.082	37244.272	0.000
	% of total export sales	616	252	25.995	63.044	37.049	0.000
	Sales	622	248	25528.236	88577.441	63049.205	0.000
AFTER							
	Age	811	290	19.518	18.459	-1.059	0.134
	Number of permanent workers	809	296	113.244	402.301	289.057	0.000
	% firms with more than 75 workers	809	296	33.869	66.216	32.347	0.000
	Equity	800	296	6892.285	13197.573	6305.288	0.084
	% firms with equity above 2.5 mil RM	800	296	23.500	62.838	39.338	0.000
	% firms above emp OR equity threshold	804	300	40.920	79.333	38.413	0.000
	Total assets	796	294	18884.228	72987.662	54103.434	0.000
	% of total export sales	805	295	26.831	58.105	31.274	0.000
	Sales	802	289	36621.611	118542.003	81920.391	0.000

Tab. 2.6: Impact on firm size

Dependent var: ln(employment)	(1)	(2) Full sample	(3)	(4)	(5) Size in 2001 < 75	(6)
age	0.00357			-0.00639**		
	(0.00266)			(0.00325)		
Foreign	0.381***	-0.178	-0.268**	0.0785	-0.242**	-0.342***
	(0.0929)	(0.119)	(0.120)	(0.0889)	(0.121)	(0.122)
after		0.0965***			0.158***	
		(0.0302)			(0.0329)	
Foreign × after	0.163	0.0351	0.187**	0.0456	0.154	0.271*
	(0.116)	(0.0691)	(0.0857)	(0.157)	(0.113)	(0.138)
Firm FE		Yes	Yes		Yes	Yes
Industry year FE	Yes		Yes	Yes		Yes
City year FE	Yes		Yes	Yes		Yes
Constant	3.252***	4.047***	4.443***	3.126***	3.337***	2.764***
	(0.214)	(0.0317)	(0.433)	(0.154)	(0.0224)	(0.237)
Observations	1672	836	836	713	500	500
R-squared	0.273	0.046	0.380	0.320	0.142	0.573
Number of firms		455	455		271	271

Foreign is a dummy variable denoting 30% or more foreign ownership
Clustered standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Tab. 2.7: Reported size constraints in the panel sample

	Size constraint	Round 1	Round 2	Total
Foreign owned firms				
	Too small	46	30	76
	Optimal	81	82	163
	Too large	42	47	89
All firms				
	Too small	133	112	245
	Optimal	228	254	482
	Too large	127	122	249

Tab. 2.8: Size constraint probabilities (marginal effects reported)

Sample:	(1) All firms			(2) Size in 2001 < 75			(3) Size in 2001 >75		
Dependent variable: size	Too small	Optimal	Too large	Too small	Optimal	Too large	Too small	Optimal	Too large
Ordered logit model									
age	0.0020**	-0.000049	-0.0020**	0.00098	-0.000083	-0.00089	0.0026**	0.000044	-0.0027**
	(0.00093)	(0.000099)	(0.00090)	(0.0015)	(0.00015)	(0.0014)	(0.0013)	(0.00017)	(0.0013)
Foreign	0.027	-0.0015	-0.025	0.018	-0.0020	-0.016	0.036	-0.00053	-0.035
	(0.032)	(0.0032)	(0.029)	(0.053)	(0.0077)	(0.045)	(0.045)	(0.0031)	(0.042)
Foreign × after	-0.045	-0.0033	0.049	-0.088	-0.015	0.10	-0.041	-0.0038	0.044
	(0.036)	(0.0067)	(0.042)	(0.056)	(0.028)	(0.083)	(0.047)	(0.0085)	(0.055)
State year FE		Yes			Yes			Yes	
Industry year FE		Yes			Yes			Yes	
Observations	1884	1884	1884	787	787	787	1107	1107	1107
Generalized ordered logit model									
age	0.0018	0.00020	-0.0020**	0.0013	-0.00086	-0.00047	0.0021	0.00083	-0.0029**
	(0.0012)	(0.0010)	(0.00091)	(0.0019)	(0.0019)	(0.00099)	(0.0015)	(0.0014)	(0.0012)
Foreign	0.036	-0.021	-0.014	0.042	-0.045	0.0035	0.045	-0.025	-0.020
	(0.036)	(0.040)	(0.035)	(0.061)	(0.058)	(0.033)	(0.049)	(0.056)	(0.048)
Foreign × after	-0.070*	0.047	0.023	-0.21***	0.23***	-0.022	-0.044	0.0081	0.036
	(0.041)	(0.053)	(0.049)	(0.045)	(0.060)	(0.049)	(0.053)	(0.067)	(0.062)
State year FE		Yes			Yes			Yes	
Industry year FE		Yes			Yes			Yes	
Observations	1884	1884	1884	787	787	787	1107	1107	1107

Dependent variable = (1, 2, 3) if firms' self reported size is too small, optimal and too large.
The generalized ordered logit model is estimated using the gologit2 command in Stata (Williams 2006).
Marginal effects reported. Clustered standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Tab. 2.9: Robustness checks

Eqn	Dependent variable	Alternative definitions of foreign (1)	(2)	(3)	Same industry (4)
(2.3)	ln(employment)	0.198**	0.149*	0.158**	0.176**
		(0.0829)	(0.0833)	(0.0780)	(0.0874)
	Sample size	846	846	846	788
(2.4)	Size too small	-0.175***	-0.176***	-0.158***	-0.217***
		(0.0528)	(0.0655)	(0.0582)	(0.0490)
	Size optimal	0.219***	0.195**	0.151*	0.247***
		(0.0579)	(0.0804)	(0.0782)	(0.0630)
	Size too large	-0.0439	-0.0187	0.00689	-0.0304
	Sample size	802	802	802	763

Eqn (2.3): coefficient estimates on (foreign×after), from the spec in column (3) of Table 2.6
Eqn (2.4): Marginal effects on (foreign×after), from the spec in column (2) of Tables 2.8
Column (1): foreign is defined as having positive shares by foreigners
Column (2): foreign is defined as having 30 or more shares by foreigners both 2001 & 2006
Column (3): foreign is defined as having positive shares by foreigners in both 2001 & 2006
Column (4): firms that switched industries between 2 rounds are dropped from the sample

Tab. 2.10: Percentage of permanent workers of foreign nationals

		Before	After
Skilled	Domestic	10.94	12.23
	Foreign	10.61	8.73
	DID	-3.17	
Unskilled	Domestic	16.62	27.95
	Foreign	15.23	23.58
	DID	-2.97	

Tab. 2.11: Alternative explanations

	(1)	(2)		
		Firm size		
Dependent variable	ln(emp)	Too small	Optimal	Too large
age		0.00154	-0.00122	-0.000319
		(0.00190)	(0.00184)	(0.000966)
Foreign	-0.249**	0.0427	-0.0468	0.00416
	(0.119)	(0.0614)	(0.0586)	(0.0322)
Foreign× after	0.189**	-0.199***	0.214***	-0.0155
	(0.0851)	(0.0480)	(0.0629)	(0.0509)
% skilled labor in 2001×after	0.207*	-0.145	0.290***	-0.145***
	(0.110)	(0.0973)	(0.0926)	(0.0538)
Constant	4.641***			
	(0.401)			
Firm FE	Yes			
Industry year FE	Yes			
City year FE	Yes			
Industry year FE			Yes	
State year FE			Yes	
Observations	836	787	787	787
R-squared	0.388			
Number of firms	455			

Clustered standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Column (1): linear regression on log employment with firm FE

Column (2): generalized ordered logit regression, mar. eff. reported, sample: size in 2001 < 75

Chapter 3. Labor Standards, Market Power and the Terms of Trade Incentive: An Empirical Analysis

1 Introduction

Trade negotiators have long demanded GATT/WTO agreements to incorporate domestic policies such as labor standards in the so-called "social clauses". One of the major arguments put forward as a justification for the international harmonization of standards is the well worn the-race-to-the-bottom hypothesis. There are several explanations for this hypothesis. The first line of argument aims at developing countries. It posits that lax labor standards give these countries an unfair advantage in exports. This in turn exerts a downward pressure on standards in rich countries at the fear of loss in competitiveness. However, this concern is not supported by trade economists, who argue that differences are the source of gains from trade. Thus if some countries are different in their preferences about labor standards, it is a reason against, not for harmonization. Empirical work also fails to find conclusive evidence about the impact of labor standards on export performance and competitive advantage. Labor standards after all have two opposite effects on competitiveness. One is on labor cost and the other is on productivity. Accounting for productivity, Rodrik

(1996) finds a range of measures of labor standards to still have significant effect on labor cost but they are not important determinants of comparative advantage in labor intensive exports. On the contrary, Mah (1997) finds the rights to nondiscrimination and freedom of association to be strongly negatively correlated with export performance for developing countries.

A more convincing argument concerns the distributional consequences of standard setting. Brown et al. (1996) explain that for a small country, the costs of imposing a labor standard on an import-competing sector would be borne solely by domestic producers if the country acts unilaterally. But if all countries coordinate then the world price rises and part of the costs is shifted to consumers. The country is actually worse off because of the price increase but it can still be politically optimal when the government gives a higher weight on producers than on consumers.

These concerns have motivated a sizable empirical literature on whether trade actually affects labor standards. These studies often rely on cross-country data. Trade or export openness, defined as (Import + Export)/GDP or Export/GDP, is often used as the independent variable to estimate the impact of exposure to trade on labor standards. Evidence of causal relationships is rare because most studies neglect the problem of reverse causality between trade and standard as well as the unobserved characteristics that can affect both variables. Among the few recent studies that address this issue, the evidence is mixed. For example, Edmonds & Pavcnik (2006) fail to find any significant effect of openness on child labor while Neumayer & De Soysa (2006) suggest that openness significantly improves the rights to Free Association and Collective Bargaining (FACB). Nonetheless, there is generally

no evidence for the negative impact of trade openness on labor standards.

In this paper, I present a perspective different from this dominant focus in the current literature. I argue that by focusing on openness, previous studies have failed to explore another channel through which trade could affect labor standards. My reasoning stems from a less widely recognized explanation of the-race-to-the-bottom possibility. It concerns large importers that possess some market power. Essentially, it is a second-best argument that if countries cannot set import tariffs freely, lower labor standards could work as a substitute to gain terms of trade advantages or to achieve domestic political objectives. Unlike the ambiguous predictions of the impact of openness on standards, both political economy and terms of trade theories predict that the larger the size of the importer, the larger its incentive to lower labor standards.

It is this prediction that the paper aims to test. To my knowledge, the only other study that investigates a similar hypothesis is Ederington & Minier (2003), which concerns environmental standard stringency in different US manufacturing sectors. This paper differs from their study in three aspects. Firstly, my estimation framework is in a cross-country rather than industry wise setting. Secondly, I consider a different measure of market power. And finally, to address endogeneity problems, Ederington & Minier (2003) use simultaneous equations estimation while I rely on the Instrumental Variables method.

My results offer some first evidence, albeit weak that market shares in import affect countries' labor standards adversely and the effect is stronger among GATT members than among non-member countries.

The rest of the paper is organized as follow. Section 2 provides theoretical discussions for the links between a country's terms of trade and its domestic policies, including labor standard policies and explains the reasoning of the paper's central hypothesis. Section 3 describes the estimation strategy and data. Section 4 presents estimation results and section 5 concludes.

2 Theory

The central hypothesis of this paper is founded on the terms of trade theory of protection. The intuition is simple. Analogous to the case of a monopoly whereby restricting output can raise prices, a large enough country could induce foreign exporters to lower their export prices by imposing tariffs and thereby reducing its import demand. It is beneficial for the country to impose a "small" tariff on its imports as long as the gains from this terms of trade effect are more than offset the losses due to its market distortion effect. All else equal, the extent to which the country can gain from its tariff increases with its ability to influence its terms of trade by reducing import demand. If the government's objective is to choose tariff levels to maximize national income, it is shown that the optimal tariff is equal to the inverse export supply elasticity. This relationship holds in a two good- two factor setting as well as a many good- many country and intra-industry trade setting (see for example, Broda et al. (2008) for theoretical derivations).

A direct application of this theory is Bagwell & Staiger (1999) theory of GATT formation. They argue that countries seek multilateral agreements to avoid the ineffi-

ciency caused by unilateral tariff setting[1] and many GATT's features can be explained as to implement efficient agreements. The optimal tariff argument has sometimes been seen as a theoretical derivation without much applicability. For example, Krugman (1997, p113) asserts that it "plays almost no role in real world dispute over trade policy" while Rose (2004) finds no evidence that the WTO has any effects on trade liberalization. However, recent empirical studies show that the optimal tariff motive might actually play a role in trade policies. Bagwell & Staiger (2006) provide evidence that WTO accession after the Uruguay round leads to greater tariff reductions in products with higher import volumes. Broda et al. (2008) find a typical non-WTO member in their sample sets tariffs 9% higher in goods with high market power relative to those with low market power. A natural question that follows is what if a country is a GATT/WTO member? Would a country being constrained in tariff setting use other policies such as environmental or labor regulations that have similar effects on import demand to regain some of its terms of trade advantages?

Second-best models offer an answer to this question. In theory, domestic policies that affect domestic supply/demand in an open economy are also trade distorting. Copeland (1991) argues that when there are limits to the feasibility of cooperation in trade negotiations, governments have incentives to substitute negotiable barriers such as tariffs towards other nonnegotiable means of protection. Even though these other instruments are more costly than direct protection, it is still optimal to negotiate then "cheat" because at some point, the benefits from reduced protection outweigh

[1] Intuitively, unilateral optimal tariff setting is inefficient because gains from terms of trade improvement are essentially international cost shifting. As part of the cost of tariff distortion is borne by foreign exporters in the form of price reductions, countries would set their tariffs higher than the globally optimal level (Bagwell & Staiger 2006).

the costs of using less efficient instruments.

More specific explanations of how domestic regulations work as instruments for trade policies can be found in Krutilla (1991). He derives the optimal Pigouvian taxes associated with negative environmental externalities for a large open economy. In the case of negative production externality, the optimal tax equals the standard Pigouvian tax plus terms reflecting the terms of trade impact of taxation and tariff revenue collection. The first-best solution involves a combination of the standard Pigouvian tax and the standard optimal tariff, which varies inversely with export supply elasticity. However, if an importer is constrained to set tariff lower than the optimal level, the terms of trade effect (negative) dominates the tariff revenue effect (positive) and the optimal environmental tax is lower than the standard Pigouvian tax. Intuitively, setting lower tax increases the country's excess supply and decreases its import volume. The loss from setting lower tax is then counterbalanced by the gain from the decrease in the volume and cost of imports.

If the standard optimal tariff is negatively related to the country's export supply elasticity and second-best environmental tax is set to regain some terms of trade advantages, it follows that the discrepancy between the optimal taxes in the first-best and second-best solutions is also negatively related to the country's export supply elasticity. In other words, the optimal Pigouvian tax for an importer decreases with its market power. As market power is positively related to market share, the optimal tax should also decrease with market share. Empirical evidence by Ederington & Minier (2003) supports this prediction. By examining US data on manufacturing industries, they find that the stringency of environmental policy has a positive impact

on net import levels while net import levels have a negative impact on environmental stringency.

The case of labor standards is completely analogous to imposing environmental taxes to correct negative production externalities. Bagwell & Staiger (2001) prove theoretically that lowering labor standards is a policy substitute for import tariffs. Intuitively, it reduces labor cost for import competing producers. Domestic production expands while import demand decreases as a consequence. If the country has market power, this demand reduction will lead to a decrease in world prices. In summary, theory predicts a possibility for countries to use labor standards as a second means of protection. As market power rises with market share, we can expect a negative relationship between market shares in imports and labor standards across countries.[2]

However, if big importers seek to lower their labor and environmental standards to improve their terms of trade, we can expect big exporters to do just the opposite. For example, Brown et al. (1996) show in a 2x2 model that if labor standards are labor using then imposing labor standard is equivalent to contracting labor supply. Consequently, world wages and the price of the labor intensive exports increase. Therefore, exporters of labor intensive goods, as a group have an incentive to overprotect labor. Chau & Kanbur (2006) draws a similar conclusion in a model with two big exporters and a competitive fringe. The competitive fringe will choose low standards while big exporters take into account the terms of trade effects and raise their standards to

[2] Bagwell & Staiger (2006) offer a decomposition of the income effect when a country imposes tariffs on imports. From this decomposition, it is shown explicitly that the unilateral optimal tariff level is further away from the efficient level, the larger the country's import volume. If the same argument is applied to the second-best model, the optimal level of domestic policy distortion should vary positively with import volumes.

higher than the efficient level: the bigger the exporter, the higher its ability to alter terms of trade and hence bigger incentive to raise standards. Then if a country has market power in both import and export markets, does the terms of trade motive in the export market eliminate the other motive in the import market? Broda et al. (2008) summarize theoretical studies which show that it is not the case. Market power in exports might create another motive for the use of import tariffs or domestic policies but all else equal, countries with bigger market power in imports still have a bigger incentive to impose higher tariffs or setting lower standards. In addition, the terms of trade incentive in exports might not be strong in reality because unlike tariffs, countries are less likely to be constrained in imposing export taxes. The WTO, for example, does not regulate export taxes. For these reasons, it seems more likely for domestic policies to play a role in influencing terms of trade in import sectors rather than in export sectors.

Note that protection can also be explained by political economy theory and the predictions still hold in the same direction. For example, Grossman & Helpman (1995) incorporate the notion of political contribution to show that optimal tariff is still positively correlated to the country's inverse export supply elasticity even if the government places no weight on social welfare. Bagwell & Staiger (2001, 2006) models are also robust to political economy considerations.

Finally, as Krugman (1997, p114) puts it, free trade negotiations "are a game scored according to mercantilist rules, in which an increase in exports – no matter how expensive to produce in terms of other opportunities foregone – is a victory, and an increase in imports – no matter how many resources it releases for other uses – is

a defeat". From this point of view, the argument to use domestic policies to influence terms of trade for import seems even more convincing than in the case for exports.

3 Estimation framework

Ideally, the hypothesis that market power in imports affects labor standards can be tested in a structural model relating export supply elasticities and industry-specific labor standards. However, the lack of industry-specific labor standard data does not allow for this approach. The empirical strategy of this paper is thus to find whether cross-country evidence supports the hypothesis of a negative relationship between labor standards and importer sizes. As a measure for the size of the importing country, I use its market share in world import of labor intensive goods.[3] Only labor intensive goods is included because if a big country imports non-labor intensive goods, it is likely to export labor intensive goods therefore hurting itself by reducing labor standards. Thus, market share of total import would not be a good measure of how the terms of trade incentive affects country's labor standard choice.

[3] The only other cross-country study that concerns the impact of market power on labor standards (child labor) is Shelburne (2001) where country's GNP is used as a measure of its economic to capture the notion of market power. However, market share is a more direct and thus a better indicator of a country's market power than national income even if the category of goods is broad. Bagwell & Staiger (2006) find that export supply elasticities have a much stronger (positive) correlation with market shares in imports than with GDP.

3.1 Estimated equations

I estimate the following cross-country equation:

$$LS_i = \beta_0 + \beta_1 ln(Mshare_i) + \beta_2 ln(Eshare_i) + \beta_3 ln(income_i) + \beta_4 (ln(income_i))^2 + \delta X_i + \varepsilon_i \tag{3.1}$$

- LS_i is country i's labor standard
- $Mshare_i$ is country i's share of world import in labor-intensive goods
- $Eshare_i$ is country i's share of world export in labor-intensive goods
- $income_i$ is the real GDP per capita for country i
- X_i is a vector of other country characteristics that affect labor standards and are potentially correlated to $Mshare_i$ and $Eshare_i$
- ε_i is the error term
- the variables are in natural log form to avoid the impact of outliers

Previous work has considered either net imports or import levels in examining the relationship between market shares and industry protection (Ederington & Minier 2003, Trefler 1993). However, both approaches have important caveats in this study's setting. Using net imports rather than imports ignores the fact that the relationship between market power in import and net import levels are less straightforward. Countries with similar net import levels but different import levels could have com-

pletely different market power.[4] Using only import level is also problematic since imports and exports are often highly correlated. Estimated coefficient on import alone could therefore be biased if governments actually use labor standards to affect exports. Even if it is not the case, bigger export shares could still counteract governments' incentive to lower standards because it could harm their export terms of trade. Therefore, I include both import and export levels, normalized by the world trade value to account for these issues. Nevertheless, as argued earlier, countries are less likely to be constrained in setting export taxes. The main coefficient of interest thus remains β_1.

If labor standard is a normal good, increases in income would improve labor standards. Since trade is likely to have an impact on income as the trade & growth literature indicates, excluding income would bias the coefficients on trade variables. I control for this by using both the natural log of GDP per capita and its squared values to avoid bias estimates if labor standards vary nonlinearly with income. Including both income terms also yields a better modeling fit as the results later reveal.

This paper hypothesizes that when countries are constrained by multilateral trade agreements in their ability to set optimal tariffs, domestic policies are used as a secondary means of protection. It follows that the main countries of interest would be members of GATT/WTO. Certainly, the argument does not exclude other countries from using this means since they could also be constrained by other regional or bilateral agreements. However, the smaller coverage of those agreements implies

[4] The correlation coefficients between net imports and import shares vary from 0.5 to 0.6 in different samples in this study.

market shares would affect GATT/WTO members more than non-member countries. I examine this prediction in several ways.

Firstly, equation (3.1) is also estimated with GATT/WTO members only to compare the estimated β_1 in two samples. Secondly, I consider the impact of a dummy variable on GATT/WTO membership denoted by D_i and its interactions with import and export shares in the following specifications:

$$\begin{aligned} LS_i =& \beta_0 + \beta_1 ln(Mshare_i) + \beta_2 ln(Eshare_i) + \beta_3 ln(income_i) + \beta_4 (ln(income_i))^2 \\ &+ \gamma_1 D_i + \delta X_i + \varepsilon_i \end{aligned} \tag{3.2}$$

$$\begin{aligned} LS_i =& \beta_0 + \beta_1 ln(Mshare_i) + \beta_2 ln(Eshare_i) + \beta_3 ln(income_i) + \beta_4 (ln(income_i))^2 \\ &+ \gamma_1 D_i + \gamma_2 D_i \times ln(Mshare_i) + \gamma_3 D_i \times ln(Eshare_i) + \delta X_i + \varepsilon_i \end{aligned} \tag{3.3}$$

Theory also suggests a more direct relationship between tariffs and labor standards: conditional on market power in import, higher tariffs will be associated with lower labor standards. I use average duty as a proxy for tariffs and estimate equations (3.2) and (3.3) where D_i is replaced by average duty.

3.2 Instrumental Variables estimation

Import shares and export shares are endogenous. If big importing countries lower labor standards to improve their terms of trade, that is because lower standards induce lower import levels. The converse is true for exports. Other arguments for the

negative impact of standards on exports include its negative effect on capital formation & Foreign Direct Investment. Some empirical studies find evidence supporting this prediction. For example, Kucera (2006) finds labor standards to have a negative impact on labor intensive exports. In addition, if there are unobserved country characteristics that affect both trade and labor standards then OLS estimations would suffer from omission bias.

To address the problem of endogeneity, I follow the trade and growth literature to instrument for trade based on geography with the assumption that geography does not affect labor standards through channels other than trade (Frankel & Romer 1999, Frankel & Rose 2005, Edmonds & Pavcnik 2006). But instead of using trade openness (Import + Export/GDP) as the dependent variable, I regress the natural log of bilateral export and import shares on geography. More specifically, the natural log of import (export) of country i from (to) country j, divided by world import value, is regressed on the log of distance between two countries, the log of population of country j,[5] the log of the product of the two countries' areas [6] and indicators for whether the countries share a common language, common border and are landlocked.[7] The bilateral import equation is:

$$ln(import_{ij}/world\ import) = \alpha_0 + \alpha_1 ln(distance_{ij}) + \alpha_2 ln(population_j) + \alpha_3 commlang_{ij} + \alpha_4 commborder_{ij} + \alpha_5 ln(area_i \times area_j) + \alpha_6 landlocked_{ij} + \eta_{ij} \quad (3.4)$$

[5] The standard gravity also considers population of country i but I follow Frankel & Rose (2005) to use only country j's population. Doing this has the advantage that if a country's population affects labor standards independently, the exclusion restrictions of the instruments are still satisfied.

[6] to capture the notion that country i and country j's areas should affect trade equivalently

[7] Similarly, to capture that whether country i or j is landlocked has the same effect on trade: 0- both are not landlocked, 1- one country is landlocked and 2- both are landlocked

The exponential of the fitted values are then summed up across trading partners to construct the values of total import and export shares of a country. To avoid outlier problems, I take the natural log of these constructed values again to use as an instrument for the log of import shares. R^2 of these two gravity equations are not as high as in the standard gravity model result but actual and constructed log of import/export shares from the model are highly correlated (the correlation coefficients between the constructed and actual import and export shares are 0.79 and 0.87 respectively). Since import and export shares are highly correlated after being instrumented by the same variables in the gravity model, using both of them in one equation causes collinearity problems. I use the natural log of the average import and export shares of other countries in the sample as two other instruments that could potentially drive import and export shares of a country in opposite directions.

A concern with our constructed instruments is if geography affects labor standard outcomes independently then the exclusion restriction of the IV estimation is violated. To control for this possibility, I consider a range of variables that could affect labor standards and are potentially correlated to a country's geographical characteristics. Edmonds & Pavcnik (2006) identify several such factors. Firstly, the growth literature suggests that a country's geography, including latitude, could be correlated to its quality of institutions. Democracy, as a dimension of quality of institutions, has been identified as an important determinant of labor standards in previous empirical studies such as Rodrik (1999). Thus, latitude and a measure of democracy[8] are included in

[8] I use the polity variable from the Polity IV project. Other similar measures exist such as the Freedom House index of civil right liberties and political freedom. However, these indices are highly correlated with income so they cause collinearity problems in my regressions.

all of the above specifications. Secondly, countries in different regions of the world have different historical and cultural characteristics that might influence their labor standard policies. These unobserved characteristics are potentially correlated with geography. I control for this possibility by including dummy variables to indicate whether a country belongs to East Asia & Pacific, South Asia, Eastern Europe & Central Asia, Middle East & North Africa, Sub-Saharan Africa, Latin America & Caribbean, North America and Western Europe. Other controlled variables are also included as a robustness check and are discussed in the results section in more details.

Income per capita also suffers the same problems of reverse causality and unobserved heterogeneity. To instrument for income and its non-linear term, I use the two-step IV method (Wooldridge 2002). First, the gravity model is used to construct predicted values for trade openness. The procedure is the same as in Frankel & Rose (2005) and Edmonds & Pavcnik (2006). In the next step, log of per capita income is regressed on the constructed trade openness, lagged capital per worker in natural log and X_i. The predicted values and its squares are used as instruments for *ln(income)* and *(ln(income))*^{2} in the final regression. However, it is likely that lagged capital per worker is not truly exogenous due to unobserved heterogeneity. As noted by Edmonds & Pavcnik (2004), it is very difficult to find strictly exogenous instruments for income in the cross country context. Therefore, the IV estimation only aims to minimize bias and assess its extent by comparing IV results against OLS results.

Another endogenous variable is GATT/WTO membership - D_i - because of the selection bias problem. Countries that choose to join GATT/WTO could have unobserved characteristics that make them differ systematically from other countries.

In addition, to gain accession, countries are often required to implement a range of policy reforms that could include some provisions for labor policies. To avoid the selection bias issue, a linear probability equation is included to instrument for D_i. The instrument used is the average membership of other countries in the region that a country belongs to, in order to take advantage of the spatial correlation in membership. Intuitively, this instrument takes into account the relationships among countries in the same region. Those countries often have closer links among each other, including trade. Thus if more countries in the region join GATT/WTO, the non-member countries could have more incentives to join as the previously trade links in the region are now diverted to outside countries. Obviously, IV estimation cannot resolve the endogeneity problem completely but can only avoid selection bias. However, if GATT/WTO accession has any implications for labor standards, it is likely to be of the positive direction. If this paper still finds the negative impact of import shares on standards for GATT/WTO members, it is possible to suggest that the impact of the terms of trade motive on standards is as predicted and it is stronger than the impact of accession requirements.

The same argument goes for average duty. Unobservable country characteristics that affect both labor standards and trade policies can result in endogeneity problems. However, it is unclear in which direction the results would be biased. Therefore, I choose to treat average duty as exogenous, mainly to compare the results when GATT/WTO membership is controlled for.

It is harder to find instruments for the interaction terms in equation (3.3) since both variables in each term are potentially endogenous. When both are treated

as endogenous, the instruments become so weak that none of the variable in the final stage equation is significant. Consequently, D_i is treated as exogenous in this specification. The interaction terms are instrumented by the interactions of D_i with the excluded instruments for import and export shares mentioned above.[9] Both the IV estimations for (3.2) & (3.3) again can be viewed as a robustness check to assess the extent of OLS bias rather than a complete solution for endogeneity.

Another concern with GATT/WTO membership is how to define D_i because the eventual purpose of D_i is not to say if a country is a member but to capture whether the country is constrained in its ability to set tariffs. Membership does not perfectly reflect this constraint. Piermartini & Teh (2005) argue that before the creation of the WTO in 1995, GATT did not require significant reductions in trade barriers for developing countries acceding. Moreover, transition periods for tariff reductions are often allowed. On the other hand, many countries liberalize beforehand to facilitate accession. If $D_i$is simply defined as one if a country is a member at the time considered, the results could be misleading but the direction of bias is ambiguous. The problem is further complicated by the fact that the data in this study are averaged over the 1993-1997 period (as explained in the next section). Some arbitrary choices are considered but they produce very similar results. Section 4 presents the results with a rather "balanced" definition of membership: $D_i = 1$ if the country is a GATT member (if it joined before 1995).

[9] In notations, if X is endogenous and Z & W are excluded instruments for X: the excluded instrument set for (X; X*D) is (Z;W; Z*D;W*D)

3.3 Data

The dependent variable is a measure of the Freedom of Association and Collective Bargaining (FACB), one of the four core labor standards. It is an index of trade union rights in the mid 90s (1993-1997) constructed by Kucera (2004) based on violations of FACB rights recorded in the International Confederation of Free Trade Unions' *Annual Survey of Violations of Trade union Rights*, the US State Department's *Country Reports on Human Rights Practice*s and the ILO's *Reports of the Committee on Freedom of Association*. The resulting score is rescaled to run from 0 to 10 with 10 being the "least violations observed" and 0 being the "most violations observed" or when unionization is prohibited. Both the unweighted and weighted versions of the variable are used in this study. However, only the results using the weighted index are reported since they produce similar estimates but the weighted index yields more conservative results.

The FACB index is suitable for the purpose of this study for a number of reasons. Firstly, its evaluation criteria come mostly from observations in the formal manufacturing sector which is relevant as import/export sectors. Secondly, FACB rights are a process-related standard which is more likely to subject to government's determination than other outcome standards such as actual wages or hours of work. It is however an index measuring outcomes at the same time. Therefore it is less subject to criticisms about using the number of ILO convention ratifications or country written laws which do not always measure true labor standard outcomes. Several other measures related to FACB rights also exist such as the Freedom House index of civil

right liberties and the OECD index of trade union rights but to date this is the richest variable and covers a wide range of countries. It is also technically convenient since it is a continuous variable and the problem of zero observations could be resolved by Tobit estimation.

Nevertheless, the FACB index also has several shortcomings. It is subject to bias because countries are different in their reporting levels. Latin American trade unions, for example are more likely to file complaints with the ILO Committee on Freedom of Association. This bias could be addressed by including regional dummies (Kucera 2004, p9). However, a more important concern is the extent to which FACB rights capture production cost. Kucera (2004) asserts that although FACB rights do not impose an immediate labor cost, stronger FACB rights are expected to be associated with higher cost at least due to the impact of unionization. Empirical studies such as Rodrik (1996, 1999) find that after accounting for productivity; labor standard measured in terms of democracy (the FH civil right liberties index, which is a part of the FACB index) still has a large and statistically significant effect on labor cost. Yet, it is worth keeping in mind that this cannot ensure that the labor cost effect is strong enough for the government to consider FACB policies as an alternative protection means. As summed up by Brown (2001), labor standards are at most a secondary determinant of wages in low income countries.

Bilateral trade data come primarily from the NBER data on World Trade Flows (Feenstra 2000). However, because trade values of transition countries are not reported separately in this dataset, data for transition countries from Feenstra et al. (2005) is combined to obtain as many countries that match with the countries reported

in the FACB data as possible. Following Edmonds & Pavcnik (2006), I consider items from the BEA (Bureau of Economic Analysis) manufacturing industry categories 5 - Apparel and other textiles products, 6 – Leather and Leather products & 34 – other manufacturing and the corresponding SITC rv.2 categories as labor-intensive manufacturing goods. Edmonds and Pavcnik (2006) also treat category 4 – Food and kindred products as labor intensive but it is more controversial as food processing is considered capital intensive in Kucera (2006). Import and export values[10] in these categories are averaged over 1993-1997 for each country. The sums of imports and exports are then divided by total world imports across three BEA categories to calculate the market share of labor intensive goods.

Data sources for other variables can be found in the Appendix A. Appendix B lists the countries in the sample with their weighted FACB scores, log of export shares and log of import shares in ascending order.

4 Empirical results

4.1 Main results

Table 3.1 reports OLS results. The results are relatively consistent with the theoretical predictions. Import shares have a negative and significant impact on FACB rights. The coefficients on export shares are sometime negative but very close to zero and insignificant except for specification (3). GATT membership and average duty also have a negative effect on labor standards even though they are not significant.

[10] Import and export volumes are reported in Feenstra et al. (2005) but not completed. Feenstra (2000) only reports value data. Therefore the option of using trade volumes is excluded.

The estimated effects of income are on the other hand, unexpected. Neumayer & De Soysa (2006) argue that income should not affect a process-related standard such as FACB rights and find evidence supporting this argument. Similarly, Botero et al. (2004) find that income does not have a significant impact on labor union power. But both income and income squared are highly significant in my results. The signs are also hard to interpret. They seem to suggest a U-shaped relationship between FACB rights and income: FACB rights worsen with the initial income increases and improve at later stages of development.

Other covariates include Polity (measuring democracy) and latitude. Democracy has a positive and significant effect, which is consistent with findings from previous studies. Countries in higher latitude appear to have better FACB rights.[11] The specifications in Table1 exclude regional indicators. I choose to report the more parsimonious models because when these variables are included, OLS results stay relatively constant. Standard errors of all coefficients increase considerably but magnitudes of the coefficient estimates are similar, suggesting that excluding regional indicators do not bias results.

OLS results are potentially biased. Table 3.2 reports results for the same specifications in Table 3.1 when import and export shares are instrumented. The equations are estimated by LIML to make use of the test for weak instruments.

When trade shares are instrumented, the coefficients on import shares (β_1) still

[11] The estimated effect of latitude is rather different from past studies. When I include latitude as the distance from the equator as in Frankel & Romer (1999) and Edmonds & Pavcnik (2006), standard errors for all coefficients are very large. But if the "true" latitude is included, the modeling fit improves significantly. The same applies for richer models presented below. This observation suggests a systematic difference between countries in the north and in the south.

have expected sign but become insignificant. OLS estimations treat the market share variables as exogenous but if lowering labor standards reduces import then estimates of β_1 will be biased upwards. However, the IV point estimates are in general comparable with OLS estimates and their differences are not statistically significant.

The instruments perform reasonably well except for specification (3). All specifications pass the tests of underidentification and the overidentification test at 1% level. The Shea partial R^2 are high in general. I also use the minimum eigenvalue statistics to test for the presence of weak instruments. Except for specification (3), the specifications pass the test of weak instruments at 10% or 15% level (IV bias within 10% or 15% of OLS bias, all tests are performed at 5% level of significance). It is also interesting to look at the point estimates in specification (3). In both OLS and IV results, the point estimates are significantly different from other specifications. Since instruments for trade shares perform poorly in this equation and import shares become more positive while export shares become more negative, it seems to suggest that in fact they have the opposite signs, which is indicated by other specifications.

As we argue earlier, income, GATT/WTO membership and average duty are unlikely to be exogenous. The IV results in Table 2 can thus still be biased. Table 3 reports the results when more regressors are treated as endogenous. Regional indicators are included as they provide better modeling fit and have a large effect on the estimation results. All estimates of β_1 are larger than the estimates without IV for income. Surprisingly, when more variables are instrumented for, standard errors decrease rather than increase. Efficiency of the IV estimates appears to improve as a result of using more instruments for income, GATT membership and the interaction

terms. For IV with more than 2 endogenous regressors, there are no critical values of the weak instrument tests. However, Shea partial R^2 increases for all specifications.

Most results fit well with our theoretical predictions. The coefficient estimates on import shares and export shares have expected signs across all specifications. Moreover, import shares appear to have larger and significant impact on standards while the impact of export shares is smaller and in general, not significant. In addition, when equation (1) is estimated for GATT members only, the estimates of import share coefficient are larger than the full sample estimates. Similarly, the coefficient on GATT membership (γ_2) in column (3) has negative sign. It is thus likely that GATT member countries are more constrained in tariff settings and are more likely to use labor standards as an instrument for protection. Import shares and its interaction with average duty in specification (5) are not significant but it could be a result of low efficiency due to small sample size. However, the problem with using average duty is it could be endogenous. Average GATT membership of neighboring countries works well as an instrument for GATT but it has no explanatory power for average duty. I have not been able to find an instrument for average duty so the result in column (4) could be biased.

The estimation results of equation (4) are also problematic for several reasons. Firstly, the indicator of GATT membership is dropped due to collinearity. As a result, the effect of GATT membership at levels will be carried out to the affect the interaction estimates. For example, if this level effect is positive and large then the effect of the interaction term might appear to be positive even if it is truly negative. The negative sign of GATT membership in column (3) might lead us to think that

it is not an issue. However, the coefficient estimate of the interaction term between GATT and import share is positive and significant in column (4). This is the opposite of what theory predicts as it implies that being a GATT member would reduce the impact of a country's import share on labor standard. While its significance seems to be caused by outlier problems as it becomes insignificant when China – a big non GATT importer with zero FACB score – is removed, the result is still at odds with the negative sign of γ_2 found in specifications (3). The second problem is the null hypothesis of underidentified in these two specifications is only rejected at 5% level. Therefore, one needs to interpret the results in columns (4) with caution because weak instruments might cause finite sample bias.

Beside our main variables of interest, the coefficient estimates for other variables are fairly stable across all specifications in Table 3.3. They also have the same signs as in the OLS estimates.

4.2 Sensitivity analysis

Table 3.4 & 3.5 present some of the results when different robustness checks are performed. The first check is to consider the impact of outliers. It is a dilemma to decide if countries such as China and USA should belong to the sample. On one hand, including these unusually large importers could bias estimates. On the other hand, the purpose of this paper is to examine the impact of market power on labor standards, which applies exactly to those large countries. To minimize the impacts of outliers, import and export shares have been used in the natural log form in the main regressions. However, to examine whether the results were not driven solely because of

one or two observations in the sample, all the specifications are re-estimated without USA and/or China. The results show that excluding China does not change the results significantly but the USA greatly dominates the results. In all specifications, when the USA is excluded, the coefficient estimates on import shares are smaller and generally insignificant. Nevertheless, they remain negative while the export share coefficients stay positive and smaller in absolute value.

As explained earlier, I use both import and export shares in my regression to account for the fact that using net import could be misleading about the real market power of a country. However, I also estimate equations (3.1)-(3.3) with net import shares to see if the prediction is still robust to using a different proxy of market power. Table 5 presents the results when the average import share of other countries is used as a simple instrument for a country's net import. Income is instrumented using lagged capital per worker and lagged investment share of GDP (hence the smaller sample size). Note that log of income squared is not included because in instruments are not strong enough as in the main regressions. The results also confirm a negative impact of net imports on labor standards, particularly in the sample of GATT members only. Our theoretical prediction thus holds when different regressors and different instruments are used. Another interesting finding from Table 5 is that latitude has extremely small and insignificant effect. In fact, the results are almost unchanged when it is excluded. This observation suggests that latitude is actually correlated to our geography-based instruments because when it is excluded from the main regressions, the results change significantly.

In previous empirical work, it is suggested that legal origin and the political ori-

entation of the country's chief executive and congress could affect labor standards. For example Botero et al. (2004)'s work on the regulation of labor find common law to have a negative impact while more leftist/centrist governments have a positive impact on labor standards. If these variables are not correlated to geography however, omitting them does not cause any bias.

As a sensitivity check, I include dummy variables for British, French, German, Scandinavian and Socialist legal system in all specifications of Table 3.1, 3.2 & 3.3. The coefficient estimates on import shares change in some cases but are still negative and significant at the same levels of significance. My results are thus robust to the presence of legal origin indicators.

I include next a variable calculated as the percentage of the years from 1975-1995 in which the chief executive and the majority party in congress of a country have leftist or centrist political orientation. The variable has expected positive sign but is not significant in general. Import shares are still negative but become insignificant in all but specification (5) of Table 3.3. Moreover, the coefficient estimates are systematically smaller in absolute values (although a Hausman tests cannot the hypothesis that they are equal). However, when it is included in specifications (1) & (2) in Table 3.5, the net import variable remains significant and its magnitudes almost do not change. It is thus likely that political orientation is correlated to countries' geographical characteristics.

Finally, Neumayer & De Soysa (2006) suggest that the FACB variable could be systematically underreported for countries with smaller shares of the manufacturing sector. To control for this issue, I include manufacturing share of GDP in all specifica-

tions. Including this variable could be a problem because it results in smaller sample size and 1st stage F statistics. However, the coefficient estimates of import shares are still significant and similar in magnitude. On the other hand, the manufacturing share variable is not significant and also not always positive as predicted.

In summary, the study's results are not robust to all different specifications. Nevertheless, it is encouraging that the coefficient estimates on import shares always remain negative and larger than the export share coefficients.

5 Conclusions

In this paper, I have attempted to investigate empirically the prediction that countries use labor standards as a substitute for import tariffs to improve their terms of trade. More specifically, I use cross country data on FACB rights and trade in labor intensive goods to test this simple hypothesis: all else equal, a country's market share in world import should affect its labor standard negatively. The gravity model is used to construct geography-based instruments for trade to account for endogeneity of trade and income variables. The paper does not aim to quantify the impact of import market shares on labor standards. Rather, it seeks to separate out the noises caused by endogeneity to find whether a causal link exists between these two variables.

The paper finds IV estimates on the import share coefficient to be systematically higher than OLS estimates. This is an indication that unobserved heterogeneity might have biased OLS results. Although the estimated impact of import shares is not consistent and significant across all different specifications, it is always negative as

theory predicts. It suggests the cross country data offer some evidence that domestic policies such as labor standard policies could in reality serve as a secondary means of protection.

One implication of the results is they suggest the need to prevent countries from using distorting domestic policies that undermine the effectiveness of trade agreement. However, it is not an argument for harmonization of standards. As Bagwell & Staiger (2001) argue, demands for harmonization originate from a misunderstanding of the cause of the race-to-the-bottom. The authors point out that if the terms of trade incentive is the issue at hand, rebalancing market access would be an efficient solution. My results also cast doubts about empirical studies on the impact of labor standards on export performance that treat standards as exogenous. Since exports and imports are highly correlated, a negative association between export performance and labor standards does not necessarily mean that higher standards harm exports. OLS results could be overestimated by the fact that big importing countries deliberately lower their standards to improve terms of trade. Nonetheless, the results should be treated with great caution. An important reason is that the dependent variable used is not a perfect measure of labor standards. Labor standards have many dimensions to it and theory does not provide any suggestions for which dimension governments would choose. Therefore, unlike the case of tariffs in which higher tariffs is an unmistakable sign of protection, it is much harder to conclude that bigger importing countries choose worse FACB rights as a mean of protection.

The study could be improved in several ways. Firstly, the current country coverage in the two datasets is small due to the way trade for transition countries is reported

in Feenstra (2000). Using a bigger and more consistent dataset should benefit the results. Secondly, although I have considered the impact of GATT membership, my approach has not captured the fact that GATT/WTO members can still apply tariffs on non-member countries. The effect of membership might therefore be better estimated if I consider trade among members only. Finally, cross country studies are in most cases prone to unobserved heterogeneity problems, which call for the need of panel estimations. Structural estimations with industry-level data can also help substantiate evidence found in cross country data.

Tab. 3.1: The impact of import shares on FACB rights OLS results

Dependent variable	FACB rights weighted index				
	(1)	(2)	(3)	(4)	(5)
ln(import share)	-0.509	-0.492	-0.18	-0.848	-0.699
	[0.253]**	[0.261]*	[0.409]	[0.231]***	[0.282]**
ln(export share)	-0.072	-0.084	-0.255	0.081	0.075
	[0.136]	[0.139]	[0.179]	[0.151]	[0.148]
ln(income)	-9.286	-9.282	-10.169	-11.658	-8.853
	[3.187]***	[3.189]***	[3.557]***	[4.269]***	[5.465]
(ln(income))2	0.606	0.605	0.659	0.757	0.596
	[0.196]***	[0.196]***	[0.219]***	[0.249]***	[0.317]*
Polity	0.184	0.182	0.184	0.157	0.161
	[0.056]***	[0.057]***	[0.058]***	[0.051]***	[0.051]***
Latitude	0.024	0.025	0.025	0.023	0.023
	[0.009]***	[0.009]***	[0.009]***	[0.009]**	[0.009]**
GATT membership		0.222	-2.119		
		[0.789]	[2.971]		
ln(import shares)*GATT			-0.461		
			[0.547]		
ln(export shares)*GATT			0.28		
			[0.289]		
Average import duty				-0.026	-0.142
				[0.028]	[0.139]
ln(import shares)*duty					-0.017
					[0.020]
Constant	35.333	35.278	40.411	43.029	31.841
	[12.723]***	[12.728]***	[15.011]***	[18.265]**	[22.860]
Observations	96	96	96	74	74
R-squared	0.42	0.42	0.43	0.54	0.55

Heteroskedasticity-robust OLS. Robust standard errors in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%.

Tab. 3.2: IV results treating import and export shares as endogenous

Dependent variable	FACB rights weighted index				
	(1)	(2)	(3)	(4)	(5)
ln(import share)	-0.536	-0.578	3.933	-0.683	-0.719
	[0.615]	[0.719]	[3.005]	[0.581]	[0.515]
ln(export share)	-0.197	-0.168	-2.748	-0.103	-0.023
	[0.402]	[0.472]	[1.630]*	[0.376]	[0.306]
ln(income)	-8.576	-8.759	-14.308	-10.831	-9.396
	[3.372]**	[3.461]**	[5.645]**	[3.785]***	[4.797]*
(ln(income))2	0.572	0.583	0.892	0.706	0.627
	[0.206]***	[0.214]***	[0.331]***	[0.226]***	[0.283]**
Polity	0.193	0.191	0.173	0.164	0.165
	[0.040]***	[0.042]***	[0.060]***	[0.041]***	[0.040]***
Latitude	0.027	0.027	0.03	0.024	0.024
	[0.010]***	[0.010]***	[0.014]**	[0.010]**	[0.009]**
GATT membership		0.284	-21.529		
		[0.970]	[15.677]		
ln(import shares)*GATT			-4.572		
			[3.036]		
ln(export shares)*GATT			2.836		
			[1.702]*		
Average import duty				-0.027	-0.102
				[0.033]	[0.121]
ln(import shares)*duty					-0.011
					[0.018]
ln(export shares)*duty					
First stage Shea partial R^2					
ln(import share)	0.18	0.147	0.075	0.248	0.375
ln(export share)	0.165	0.135	0.071	0.242	0.348
Min eigenvalue stat	3.964	3.119	1.349	4.674	7.802
Constant	31.193	31.503	77.987	40.43	33.876
	[12.727]**	[12.814]**	[35.898]**	[15.323]***	[19.102]*
Observations	96	96	96	74	74

FIML estimations. Standard errors in brackets, * significant at 10%; ** at 5%; *** at 1%.
The minimum eigenvalue statistic is the statistic for Stock & Yogo (2004) weak instrument test.
H0: instruments are weak. For IV LIML with 2 endogenous regressors and 4 excluded instruments,
the critical values at 5% level of significance for 15% and 10% bias are 3.39 and 4.72 respectively.

Tab. 3.3: IV results treating trade shares, income & GATT membership as endogenous

Dependent variable	FACB rights weighted index				
	(1)	(2)	(3)	(4)	(5)
ln(import share)	-0.826	-1.494	-1.184	-2.006	-0.706
	[0.483]*	[0.791]*	[0.555]**	[0.638]***	[0.494]
ln(export share)	0.261	0.758	0.491	0.742	0.072
	[0.335]	[0.571]	[0.357]	[0.341]**	[0.312]
ln(income)	-19.441	-20.2	-22.053	-22.35	-16.218
	[7.134]***	[6.710]***	[7.284]***	[6.854]***	[6.364]**
(ln(income))2	1.178	1.247	1.348	1.385	1.021
	[0.413]***	[0.395]***	[0.419]***	[0.402]***	[0.380]***
Polity	0.264	0.266	0.281	0.266	0.175
	[0.072]***	[0.070]***	[0.074]***	[0.067]***	[0.058]***
Latitude	0.013	0.018	0.014	0.02	0.002
	[0.014]	[0.018]	[0.015]	[0.016]	[0.015]
Regional dummies	Yes	Yes	Yes	Yes	Yes
GATT dummy			-2.124		
			[1.216]*		
ln(import shares)*GATT				0.489	
				[0.207]**	
ln(export shares)*GATT				0.028	
				[0.320]	
Average import duties					-0.039
					[0.033]
Constant	78.745	76.358	88.326	83.826	64.133
	[30.896]**	[28.230]***	[31.540]***	[27.498]***	[25.578]**
First stage Shea partial $\boldsymbol{R}^2$					
ln(import share)	0.313	0.131	0.257	0.263	0.31
ln(export share)	0.268	0.104	0.218	0.51	0.285
ln(income)	0.331	0.331	0.304	0.304	0.586
(ln(income))2	0.342	0.327	0.312	0.304	0.573
GATT			0.556		
ln(import share)*GATT				0.844	
ln(export share)*GATT				0.577	
Observations	91	84	91	91	71
R-squared	0.41	0.34	0.39	0.36	0.59

IV GMM estimations with small sample adjustments. Standard errors in brackets, * significant at 10%; **at 5%; *** at 1%. Column (2): results from the sample with GATT members only

Tab. 3.4: Sensitivity analysis 1

	Excluding China or the USA				GATT excl. USA
	(1)	(2)	(3)	(4)	(5)
ln(import share)	-1.275	-2.024	-0.567	-1.026	-0.735
	[0.574]**	[0.693]***	[0.502]	[0.845]	[0.886]
ln(export share)	0.572	0.572	0.049	0.11	0.193
	[0.427]	[0.416]	[0.361]	[0.513]	[0.687]
ln(income)	-22.692	-21.365	-21.673	-21.373	-20.32
	[7.402]***	[7.032]***	[7.610]***	[7.325]***	[7.143]***
(ln(income))2	1.383	1.345	1.322	1.334	1.244
	[0.425]***	[0.412]***	[0.442]***	[0.430]***	[0.422]***
Polity	0.284	0.265	0.236	0.232	0.236
	[0.074]***	[0.067]***	[0.073]***	[0.075]***	[0.077]***
Latitude	0.014	0.024	0.012	0.019	0.012
	[0.015]	[0.018]	[0.015]	[0.018]	[0.018]
Regional dummies	Yes	Yes	Yes	Yes	Yes
GATT dummy	-2.401				
	[1.813]				
ln(import shares)*GATT		0.289		0.052	
		[0.406]		[0.473]	
ln(export shares)*GATT		0.333		0.246	
		[0.629]		[0.690]	
Constant	90.98	76.94	87.985	81.829	81.613
	[32.327]***	[29.478]**	[32.680]***	[30.018]***	[29.212]***
F statistics on 1st stage equations					
ln(import share) equation	66.33	65.43	63.57	55.38	59.75
ln(export share) equation	49.91	49.49	37.65	38.45	32.7
ln(income)	31.75	55.26	25.06	55.15	36.18
(ln(income))2	33.15	53.58	26.15	53.17	36.2
GATT	53.9				
ln(import share)*GATT		238.46		225.46	
ln(export share)*GATT		43.78		32.47	
Observations	90	90	90	89	83
R-squared	0.34	0.32	0.41	0.4	0.4

IV GMM & 2SLS heteroskedasticity-robust estimation. Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%. Column (1) & (2): China is excluded. Column (3): the USA is excluded. Column (4): China and the USA are excluded. Column (5): GATT members excluding the USA. When the USA is excluded, 2SLS is used instead of GMM due to the singleton dummy problem (regional dummy for North America)

Tab. 3.5: Sensitivity analysis 2

	IV for net import share		
	(1)	(2)	(3)
Net import share	-0.552	-0.533	-0.428
	[0.290]*	[0.267]*	[0.175]**
ln(income)	0.587	0.59	0.42
	[0.581]	[0.588]	[0.586]
Polity	0.231	0.229	0.19
	[0.066]***	[0.066]***	[0.068]***
Latitude	-0.0002	0.00005	0.01
	[0.019]	[0.019]	[0.020]
ln(net import share)*GATT		-0.014	
		[0.151]	
Constant	0.678	0.586	2.218
	[5.868]	[6.111]	[5.986]
F statistics on 1st stage equations			
Net import share	7.68	6.27	17.54
ln(income)	34.06	32.34	52.72
ln(net import share)*GATT		84.8	
Observations	88	88	81
R-squared	0.37	0.4	0.4

IV GMM with small sample adjustments. Robust standard errors in brackets,
* significant at 10%; ** significant at 5%; *** significant at 1%.
Column (3): GATT members only.

APPENDIX

1. Chapter 1

A Empirical model with an endogenous productivity process

This sections sets up a simple model of the firm's profit maximizing problem to illustrate endogeneity issues with foreign employment and explains an estimating procedure based on ACF (2006) using materials to proxy for productivity to correct for this problem. We omit the issue of attrition bias for simplicity, but it can be reintroduced with an additional step that predicts exits using past investment (at the expense of being more data intensive).

Assume the firm's production is given by function of capital, labor, material inputs and an efficiency index:

$$y_{it} = e^{A_{it}} f(k_{it}, l_{it}, m_{it})$$

Let $e^{\tau_{it}}$ be a time-varying parameter that reflects an output distortion faced by the firm so that the firm's realized output is only $e^{\tau_{it}} y_{it}$ where $\tau_{it} < 0$.[1] Under our hypothesis that this distortion is increasing over time during the conflict for firms employing foreigners, denoting some measure of foreign employment as ρ_{it}, we can write τ_{it} as a time indexed function: $\tau_{it} = h_t(\rho_{it})$.

We maintain the assumptions that (i) the "real" productivity index A_{it} follows a first order Markov process so that $A_{it} = E(A_{it}|A_{it-1}) + \xi_{it}$, and (ii) the timing of investment is such that it takes one full period to form working capital so current level of capital stock is uncorrelated to the current productivity shocks. Allowing for input prices to change with whether firms employ foreigners and normalizing output price, the firm's profit in any period t can be written as: [2]

$$\pi(k_{it}, l_{it}, m_{it}, \rho_{it}, A_{it}) = e^{\omega_{it}} f(k_{it}, l_{it}, m_{it}) - c(k_{it}, l_{it}, m_{it}, \rho_{it})$$

where $\omega_{it} = A_{it} + h_t(\rho_{it})$

It can be shown that under general assumptions, the material input demand is a functions of the state variables A_{it} and k_{it} and it is strictly increasing in the productivity parameter. We follow ACF (2006) to assume that labor decisions are made after the productivity shocks are completely realized but before decisions on purchasing materials so that the firm solves

[1] As discussed in the Methodology section, this distortion could arise if some firms (i) had their output expropriated, stolen, taxed more heavily or (ii) were forced to close operations more often.

[2] In the empirical estimation, we only have value and not quantity data thus the distinction is irrelevant. Consequently, the differences in estimated productivity can also reflect how firms' product demand over time were affected differently

a sequential problem and chooses labor composition first.[3] Then:

$$m_{it} = m_t(k_{it}, A_{it}, l_{it}, \rho_{it})$$

Because of strict monotonicity, we can invert the material input demand function to get

$$A_{it} = m_t^{-1}(k_{it}, m_{it}, l_{it}, \rho_{it}) \tag{A-1}$$

Assuming a Cobb-Douglas functional form, we can rewrite the (log) value added production function as:

$$y_{it} = \beta_0 + \beta_k k_{it} + \beta_l l_{it} + A_{it} + h_t(\rho_{it}) + \varepsilon_{it} \tag{A-2}$$

$$= \beta_0 + \phi_t(k_{it}, m_{it}, l_{it}, \rho_{it}) + \varepsilon_{it} \tag{A-3}$$

In our implementation, we approximate for ϕ_t semi-parametrically by a third order polynomial. There is a time index t in m_t and ϕ_t because of our hypothesis that the impact of being a foreign firm changes overtime with conflict intensity. Accordingly, the empirical estimation of the above equation has to allow for this possibility. Due to data constraints, we allow ϕ_t to change only once by including a dummy variable indicating the start of the conflict and its interactions with all other terms in the function ϕ_t. It is consistent with assuming that $h_t(\rho_{it}) = h^1(\rho_{it})$ before the conflict and $h_t(\rho_{it}) = h^2(\rho_{it})$ during the conflict.

The first step in the estimation involves estimating equation (A-3) to get predicted values for ϕ_t. If we have some initial values for β_k, β_l and $h_t(\rho_{it})$ then we can get a predicted value of the productivity term:

$$\tilde{A}_{it} = \hat{\phi}_t - \beta_k^* k_{it} - \beta_l^* l_{it} - h_t^*(\rho_{it})$$

Based on the assumption of the productivity process, we can get an estimate of the unexpected shocks ξ_{it} in the productivity process by regressing $\tilde{A}_{it}$ on a function of $\tilde{A}_{it-1}$:

$$\hat{\xi}_{it} = \hat{\phi}_t - \beta_k^* k_{it} - \beta_l^* l_{it} - h_t^*(\rho_{it}) - g(\hat{\phi}_{t-1} - \beta_k^* k_{it-1} - \beta_l^* l_{it-1} - h_{t-1}^*(\rho_{it-1}))$$

We approximate for g by a second order polynomial and use the following moment conditions to estimate β_k, β_l and the parameters in $h_t(\rho_{it})$:

$$E(\xi_{it}|k_{it}, l_{it-1}, \rho_{it-1}, m_{it-1}, k_{it-1}) = 0 \tag{A-4}$$

From equations (A-3) and (A-4), it can be seen that if ρ_{it} is not included in the proxy function ϕ_t, the estimated $\hat{\xi}_{it}$ would still contain information on ρ_{it}. Therefore if the choices of foreign employees are correlated with labor or capital then we will get biased estimates using these moment conditions.

If $h_t(\rho_{it})$ contains a large number of parameters, it would require more moment conditions for identification and a large sample size to get precise estimates. Given our data constraints,

[3] What this means is labor does not have to be a perfectly variable input as needed in the OP estimation procedure. Thus we can accommodate the case where labor and its composition (in terms of foreign/domestic workers) are serially correlated. Moreover, this estimation procedure is consistent with firm-specific capital and labor price shocks since they will simply be reflected in the material input demand function. This is an important advantage since in the context of a developing country like Cote d'Ivoire, it is likely that there are frictions in the labor and capital markets which result in different input prices across firms.

we assume that

$$h_t(\rho_{it}) = \gamma_0\rho_{it} + \gamma_1\rho_{it} \times after + \gamma_2 after + \eta_{it}$$

where *after* is a dummy indicating the conflict period and η_{it} is a random noise unknown to the firm.

B Data appendix

B.1 Construction of variables used in TFP estimation

For the production function estimation, we need measures of output, inputs and investment. Except for labor input, all other variables are taken from the balance sheet information and measured in monetary values.

Labor: is measured as the total number of permanent employees. The employment information from the data allows us to distinguish between technical and unskilled employees and thus the ability to estimate separate coefficients on skilled and unskilled labor in the production function.

Capital and *investment:* Capital is proxied for by total fixed assets in book values in US dollars and investment is total acquisition of tangible and intangible fixed assets. With this definition, there are 10719 firm-year observations, or 70% of the sample, reporting positive investment. Both capital stock and investment are observed every year in the data. Therefore, we can also calculate investment or capital using the perpetually inventory method: $I_{it} = K_{it+1} - (1-\delta)K_{it}$ where δ is the depreciation rate. De Loecker (2007) argues that this method is preferred when reported investment is not accurate. However, the lack of data on depreciation rates at the industry level makes this method also prone to measurement errors. Moreover, using a 15% depreciation rate for all firms, the number of firm-year observations with positive investment account for less than one third of the sample. Therefore the reported investment will be used in the estimation.

Output: we estimate the production function using the reported value added as a measure of output (implicitly imposing a separability assumption in the production functions).

Materials: are defined as total cost of intermediate inputs and other good purchases. Since both value added and materials are reported in the data, we reestimate value added - gross output less intermediate inputs and other purchases - as a robustness check as inaccuracies (in terms of reporting errors) are common. The two measures are highly correlated thus only results using reported value-added are included in the paper.

Industry classification and industry-specific deflators: Due to a revision in the industry classification in 1998, many firms in the data in 1998 and 1999 have missing sector information. To fill in this information, one can use sector information available in later years. This procedure implies a bias that firms that exited before 1999 are less likely to have sector information. To deal with this problem, we augmented the data with sector information from earlier years[4]. Because of inconsistent sector categorization, we reconcile the sector variable by using a harmonized classification that groups some industries from the old and new systems. Consequently, some industry deflators need to be recalculated. We calculate

[4] We have identification and employment data for firms prior to 1998, but financial information was not available. This procedure still implies that firms that entered and exited in 1998 are more likely to have missing sector information. However, the TFP estimation is not affected by this because we need at least 2 periods of data for each firm

the new deflators by adding the monetary values (production, value-added or intermediate goods) of the newly formed industries both in real and nominal terms and take the ratios of these two terms.

B.2 Constructing ownership variables

Recall that we define ownership by the nationality of the firm's largest shareholder. Since for more than 95% of the sample, the largest total share of shareholders of the same nationality is also larger than 50%, this definition almost coincides with ownership definition often found elsewhere in the corporate finance literature. Because the data were entered manually, there are significant number of typos which resulted in duplicate entries and inflated values (in cases when decimal places were entered wrongly). We fix the typos when possible using the rule that individual shares have to sum up to approximately 100 (i.e. dropping the duplicated entries and divide the values by an appropriate number) and drop the firms when there are no obvious way to fix the values. Another caveat with using this information is that data are not available in 1998 and are missing for a large number of firms in later years.

B.3 Conflict data

We use the Armed Conflict Location and Event Data (ACLED, Raleigh 2010)[5] to construct the conflict rate variables. This database tracks politically driven events in unstable and warring states therefore does not include crime violence. The database was compiled from various sources including news articles, books and humanitarian workers accounts of conflict events with the exact date, and by longitude and attitude. The longitude and latitude information allows us to merge the conflict data with GIS data on Cote d'Ivoire administrative boundaries to locate any conflict in the respective administrative unit.

[5] Available at http://www.acleddata.com/

Tab. 1.B.1: Industries and industry groupings

Industry	Group	Description	Median ln(K/L)	Proportion of firms with foreign		Number of firms by	
				Ownership	Employment	Industry	Group
2	1	Agriculture for industry and export	0.71	0.55	0.91	224	338
3		Forestry and logging	1.47	0.55	0.91	58	
4		Fishery products	1.13	0.42	0.77	56	
6	2	Grain and flour products	0.95	0.35	0.56	235	272
9		Oilseed industry	1.71	0.10	0.84	37	
7	3	Canned and food preparations	1.79	0.59	0.77	53	169
8		Beverages andice cream	2.58	0.47	0.89	27	
10		Other food products	2.13	0.55	0.66	89	
11	4	Textiles	2.14	0.53	0.84	103	177
12		Leather andfootwear	1.36	0.85	0.70	74	
13	5	Wood industries	1.36	0.74	0.84	411	411
15	6	Chemicals	1.83	0.67	0.77	190	476
16		Rubber Products	2.25	0.70	0.85	225	
17		Building materials, glass	1.75	0.81	0.84	61	
19	7	Transport equipment	0.28	0.71	0.72	186	186
20	8	Mechanical and electrical-products	1.37	0.61	0.69	388	388
21	9	Other manufacturing	1.22	0.48	0.68	394	394
23	10	Construction and Maintenance	0.56	0.52	0.56	884	884
24	11	Transport and communications	1.58	0.49	0.64	808	808
25	12	Rental and management of buildings	2.10	0.61	0.66	261	261
26	13	Other services	1.00	0.48	0.49	1961	1961
27	14	Commerce	0.91	0.54	0.47	4919	4919

Tab. 1.B.2: Production function coefficient estimates

	OP			ACF		
Ind. group	capital	skilled L	unskilled L	capital	skilled L	unskilled L
1	0.339**	0.277***	0.228***	0.195**	0.336***	0.255***
2	0.345**	0.292***	0.245***	0.218**	0.319***	0.291***
3	0.331	0.550***	-0.0242	0.314	0.0168	-0.159
4	0.435**	0.350***	0.238***	0.292	0.297*	0.354**
5	-0.159	0.318***	0.336***	0.128	0.276***	0.268**
6	0.306*	0.398***	0.188***	0.328*	0.468***	0.205***
7	0.280***	0.482***	0.430***	0.258***	0.418***	0.399***
8	0.295***	0.670***	0.275***	0.206*	0.607***	0.205***
9	0.260	0.469***	0.273***	0.253**	0.277**	0.229*
10	0.279***	0.401***	0.268***	0.296***	0.442***	0.253***
11	0.319***	0.369***	0.216***	0.285***	0.116	0.128*
12	0.176	0.523***	0.348***	0.216**	0.732***	0.326***
13	0.279***	0.535***	0.240***	0.303***	0.556***	0.226***
14	0.144***	0.498***	0.301***	0.174***	0.505***	0.248***

Clustered bootstrap standard errors *** p<0.01, ** p<0.05, * p<0.1

Tab. 1.B.3: Correlation coefficients of productivity estimates

	Labor productivity	OP TFP	ACF TFP
Labor productivity	1		
OP TFP	0.9515*	1	
ACF TFP	0.8341*	0.8064*	1

Note: * indicates significance at 5% level

3. Chapter 3

A Data sources

All data are averaged over 1993-1997 unless otherwise indicated

FACB indexes: Kucera (2004)

Import shares: Sum of bilateral imports for each country by trading partners over BEA categories 5, 6 & 34 and corresponding SITC rv.2 categories of manufacturing goods, divided by world imports in the same categories. Source: Feenstra (2000) and Feenstra et al. (2005)

Export shares: Sum of bilateral exports for each country by trading partners over BEA categories 5, 6 & 34 and corresponding SITC rv.2 categories of manufacturing goods, divided by world imports in the same categories. Source: Feenstra (2000) and Feenstra et al. (2005)

Trade openness: (Import + Export)/GDP. Source: Rose (2004)

Income: real GDP per capita, Laspeyres index. Source: Penn World Table 6.2

Capital per worker: capital per worker in 1987. Source: Easterly (2001)

Lagged investment share of real GDP: average investment share of real GDP in 1988-1992. Source: Penn World Table 6.2

Manufacturing shares of GDP: WDI 2006

Latitude: Easterly (2001) and CIA World Factbook

Regional and legal system dummies: Easterly (2001)

GATT/WTO accession year: Rose (2004)

Average import duties: WDI 2006

Geographical variables in the gravity model: bilateral distance, landlocked status, indicators for common borders, common language, population and area from Rose (2004) with minor correction for landlocked status from CIA World Factbook

Polity: democracy score with 10 denotes complete democracy and -10 denotes complete autocracy. Source: Polity IV project

Political orientation: the percentage of years from 1975-1997 that both a country's chief executive and congress have leftist or centrist political orientation. Source: World Bank (2002)

B Main variables by country

Tab. 3.B.1: FACB rights, log of import shares and export shares

Country	FACB index, weighted	ln(import share)	ln(export share)
Burkina Faso	8.5	-10.4363	-6.85817

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Guyana	8.65	-10.3189	-5.98627
Sierra Leone	7.59	-10.3059	-3.9293
Malawi	5.04	-9.91508	-6.52246
Ethiopia	2.33	-9.65952	-5.56763
Uganda	6.09	-9.54604	-9.83084
Zambia	2.48	-9.42275	-4.80014
Niger	6.99	-9.28533	-8.84714
Nepal	6.39	-9.09526	-2.52597
Nicaragua	6.09	-9.04672	-4.94253
Senegal	5.94	-8.97973	-6.49674
Guinea	6.99	-8.91365	-3.84405
Cameroon	3.08	-8.88171	-6.18412
Trinidad and Tobago	9.55	-8.84813	-5.57048
Yemen, Rep.	5.04	-8.73544	-11.0235
Haiti	6.24	-8.71713	-3.88105
Cambodia	5.94	-8.65763	-3.72211
Gambia, The	8.2	-8.63347	-4.02159
Tanzania	6.69	-8.55177	-4.59167
Cote d'Ivoire	5.34	-8.54332	-3.4185
Madagascar	8.35	-8.49322	-4.71916
Ghana	7.89	-8.30694	-3.26277
Fiji	5.19	-8.27619	-3.71571
Zimbabwe	4.44	-8.20688	-3.36344
Qatar	0	-8.19287	-4.30758
Congo, Dem. Rep.	2.93	-8.17086	-2.18648
Kenya	4.44	-8.12745	-4.36145
Myanmar	0	-8.06712	-3.29406
Togo	5.49	-8.0504	-6.06289
Bolivia	1.43	-8.04639	-3.79815
Iceland	9.1	-8.01655	-5.32802
Benin	8.2	-7.99274	-7.75148
Jordan	6.69	-7.93398	-5.1221
Bahrain	7.14	-7.84758	-3.94552
Pakistan	2.78	-7.74636	0.235094
Nigeria	2.03	-7.66908	-3.73814
Oman	7.89	-7.61193	-3.88787
Syrian Arab Republic	0	-7.52933	-3.43153
Peru	2.03	-7.42222	-2.30999
Uruguay	8.65	-7.33667	-2.06223
El Salvador	2.78	-7.33574	-3.63977
Cyprus	8.65	-7.18904	-3.26931
Guatemala	2.48	-7.16785	-3.9333
Jamaica	8.5	-6.92371	-2.69305
Honduras	3.08	-6.90578	-5.31506
Slovak Republic	8.2	-6.89516	-1.83001

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Mauritius	6.39	-6.82163	-1.66409
Costa Rica	2.56	-6.78281	-2.81705
Venezuela	6.92	-6.72368	-3.82183
Egypt, Arab Rep.	4.59	-6.509	-1.76039
Sri Lanka	6.09	-6.45551	-0.91852
Bangladesh	1.73	-6.43112	-0.64388
Slovenia	8.05	-6.41742	-1.56819
Kuwait	3.53	-6.35786	-4.65665
New Zealand	9.1	-6.32491	-2.2891
Morocco	3.68	-6.09125	-1.26668
Dominican Republic	4.29	-5.93031	-1.1087
Chile	5.86	-5.89334	-3.08479
Argentina	2.56	-5.88916	-1.23684
Philippines	1.95	-5.80551	-0.82027
South Africa	5.49	-5.78599	-1.07915
Panama	5.19	-5.758	-4.88085
Romania	4.29	-5.74612	-0.77385
Czech Republic	7.29	-5.68626	-0.97703
Indonesia	0.98	-5.63746	0.636776
Hungary	6.84	-5.59152	-0.77992
Finland	9.55	-5.56723	-1.90217
Tunisia	6.54	-5.5542	-0.55362
Brazil	3.83	-5.41491	-0.26886
Ireland	10	-5.2632	-0.91601
Norway	8.65	-5.16862	-2.50782
Malaysia	2.18	-5.15152	-0.3505
Greece	9.1	-5.14219	-0.49933
Thailand	5.04	-4.99264	0.585193
Poland	8.2	-4.95522	-0.16127
India	5.34	-4.90649	0.936832
Denmark	8.2	-4.82393	-0.52395
Portugal	10	-4.79274	0.462574
Sweden	9.55	-4.64942	-1.10462
Australia	7.44	-4.61334	-1.17086
Mexico	2.63	-4.48318	0.143281
Korea, Rep.	2.93	-4.46174	1.28028
Israel	6.69	-4.43892	0.308848
Austria	10	-4.34253	0.1374
Singapore	8.2	-4.31986	-0.20714
Spain	8.05	-4.16351	0.272603
Canada	8.65	-3.94635	-0.33924
Switzerland	9.55	-3.7672	0.567749
Netherlands	9.55	-3.61606	0.799526
Italy	9.55	-3.40865	2.279344
China	0	-3.11063	2.392195

Continued on next page

France	8.95	-2.97081	1.338422
United Kingdom	4.14	-2.89247	1.338834
Japan	6.39	-2.66267	0.974143
Germany	9.47	-2.27114	1.583193
United States	4.74	-1.66689	1.615339

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