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Investment Climate Reforms and Job Creation in Developing Countries

What Do We Know and What Should We Do?

Aminur Rahman

Abstract

This paper reviews the literature on the role of the investment climate reforms in job creation. It finds that the current landscape of employment and private sector activity in developing countries indicates a number of potential channels through which investment climate reforms can positively affect job creation. However, rigorous empirical evidence is scarce and most of the relevant studies focus on business entry reforms with a few focusing on business taxation and investment promotion activities. Overall, there is evidence of job creation through business entry, tax reforms, and investment promotion activity in developing countries. Almost all of these evidences are from quasi-experimental studies that are significant improvements over conventional cross-country or cross-section panel data analysis. Still, various endogeneity concerns in these studies cannot be ruled out completely. In assessing job effects, future research should provide deeper insights on the gross versus net and short-run versus long-run job effects and general equilibrium effects of various investment climate reforms related to jobs, productivity, competition, and other developmental outcomes. Another critical agenda for future research is to shed light on which investment climate reforms matter most for spurring the employment and productivity growth of firms in developing countries. The World Bank Group, in partnership with development partners and client government countries, can play a significant role in bridging the current knowledge gap by integrating rigorous evaluation as an integral part of project design and implementation, and improving data quality, particularly through its information and communication technologies–led private sector development reform initiatives.

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Aminur Rahman[1]

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1. Introduction

The vision of the World Bank Group (WBG) is to eradicate extreme poverty by reducing the share of the world's population living on less than $1.25 a day to 3 percent by 2030, and to promote shared prosperity by fostering the income growth of the bottom 40 percent in every country. Strategies for job creation have a critical role to play in achieving this vision. Jobs are one of the cornerstones of development, as they are critical to rising living standards, poverty reduction, productivity growth, and social cohesion (World Bank 2013). For most people, particularly in the poorest countries, jobs provide the main source of income. More than two decades of research on poverty dynamics in a range of countries demonstrates that labor-earnings are the largest contributor to poverty reduction (see Ravallion 2009, Baulch 2011, Fields et al. 2003, among others). Getting jobs and starting a business emerge as the two most important reasons for people to move out of poverty in a large set of qualitative studies (Narayan et. al. 2009).

Amid the slow recovery from the global financial crisis, policy makers around the world face the critical challenge of job creation. Some 200 million people, including 75 million under the age of 25 are unemployed, and over the next 15 years an additional 600 million jobs will be needed to absorb a youth bulge in the population, mainly in Asia and Sub-Saharan Arica (WB 2013). The private sector holds the key, as 9 out of 10 jobs are created in the private sector (WB 2013). Consequently, a predictable and business-friendly investment climate stands as an integral component of the policy framework for job creation (World Bank 2013 and IFC 2013). Over the past years, millions of dollars of aid programs have been targeted towards private sector development and investment climate (IC) reforms, often with a focus on small and medium enterprise (SME) development.

An obvious question is what do we know about the impact of these programs on job creation? Answering this question will help in the successful design and implementation of job-creation programs. This paper surveys the literature on the impact of IC reforms on job creation, attempts to identify the knowledge gaps, and offers ideas regarding what WBG, along with the development partners, policy makers, and development practitioners, could do to fulfill these

knowledge gaps.[2] For the purpose of this paper, the scope of IC reforms includes various regulatory and institutional reforms related to business entry, licenses, permits, inspections, fiscal and non-fiscal incentives (e.g. business taxation, property taxation, special zones, etc.), investment policy and promotion agency, trade logistics, bankruptcy reforms, etc., that supposedly affect one or more aspects of the life-cycle of a business, i.e., business creation, operation, and exit.

The paper is organized as follows: Section 2 describes the current landscape of non-farm jobs in developing countries and its potential link to investment climate. Section 3 reviews the literature on the impact of IC reforms on job creation. Section 4 highlights some of the key knowledge gaps and what could be done to bridge those, and Section 5 concludes.

2. Who Creates Jobs in Developing Countries and Why Investment Climate Could Matter

To understand the conceptual link between IC and jobs, we first need to explore the current landscape of jobs and private sector activity in developing countries. The total developing country labor force is approximately three billion people, of which approximately 60 percent are in the informal sector, one-third are working poor (below $2/day), and half of the workforce are in the International Labor Organization (ILO) category of vulnerable employment (ILO 2012).

Cross-country analysis suggests that informal employment is positively correlated with poverty and low-level of development (Figure 1). As the majority of jobs are in the informal sector, so appears to be the majority of private sector businesses in the developing world. According to available data, 60-80 percent of micro, small, and medium enterprises (MSMEs) in different developing countries are informal (IFC 2014), while the number of formal businesses per 1,000 people appears to be the lowest in low income countries (LICs). The majority of the informal firms, which in turn make up the majority of the private sector firms in many developing countries, are typically three-person enterprises or smaller (McKenzie and Bruhn

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[2] This review does not include the literature on the role of labor market policies and programs in job creation. Also the focus here is on non-farm jobs.

2013). For instance, 90 percent of the informal firms in Sri Lanka are single-person enterprises (De Mel et. al. 2013), while 72 percent of all establishments in Rwanda, both formal and informal, employ one person, and 70 percent of Rwandan non-farm household businesses are owner-only businesses[3].

While the significant share of economic activity remains in the informal sector in developing countries (Schneider et al. 2010), what does the landscape of formal sector jobs look like? Ayyagari et al. (2011) provides a comprehensive picture in this regard using the World Bank Enterprise Survey (henceforth WBES) data of 49,370 firms in 104 countries between 2006 and 2010. Their analysis suggests that while small firms (5-19 employees) are the smallest contributors to employment across countries (with approximately 16 percent of employment in the median country with a mean of 20 percent across the sample countries), the employment contribution of the small and medium enterprises (SMEs), with less than 99 employees, is comparable to that of large firms, with 100 or more employees. The SMEs' mean and median employment share are 48 percent and 45 percent, respectively, compared with 52 percent and 56 percent for the large firms. The large firms also appear to have the largest employment share in the United States (Haltiwanger et. al. 2010a, b).

Ayyagari et al. (2011) also find the employment share of small firms tends to be higher in low-income countries (LICs) than high-income countries (HICs). The small firms have the largest share of job creation (sample mean and median are 58 percent and 45 percent, respectively), and the job creation rates are the highest among small firms no more than five years old, while the large firms have the largest share of job losses. This, however, is in sharp contrast with the U.S. experience, where Haltiwanger et al. (2010b) demonstrate that small mature firms have net job losses. However, after controlling for firm age, they do not find any systematic relationship between firm size and growth in the U.S. Based on OECD data on net employment, Haltiwanger et al. (2010a) find that small firms account for higher pace of both job creation and destruction.

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[3] Based on Rwanda Establishment Census, 2011 and Rwanda Integrated Household Survey 2011.

Figure 1: Measures of Informality, Poverty, and Level of Development

36 Countries: Informal Employment and Poverty

38 countries: Informal Employment and GDP per capita
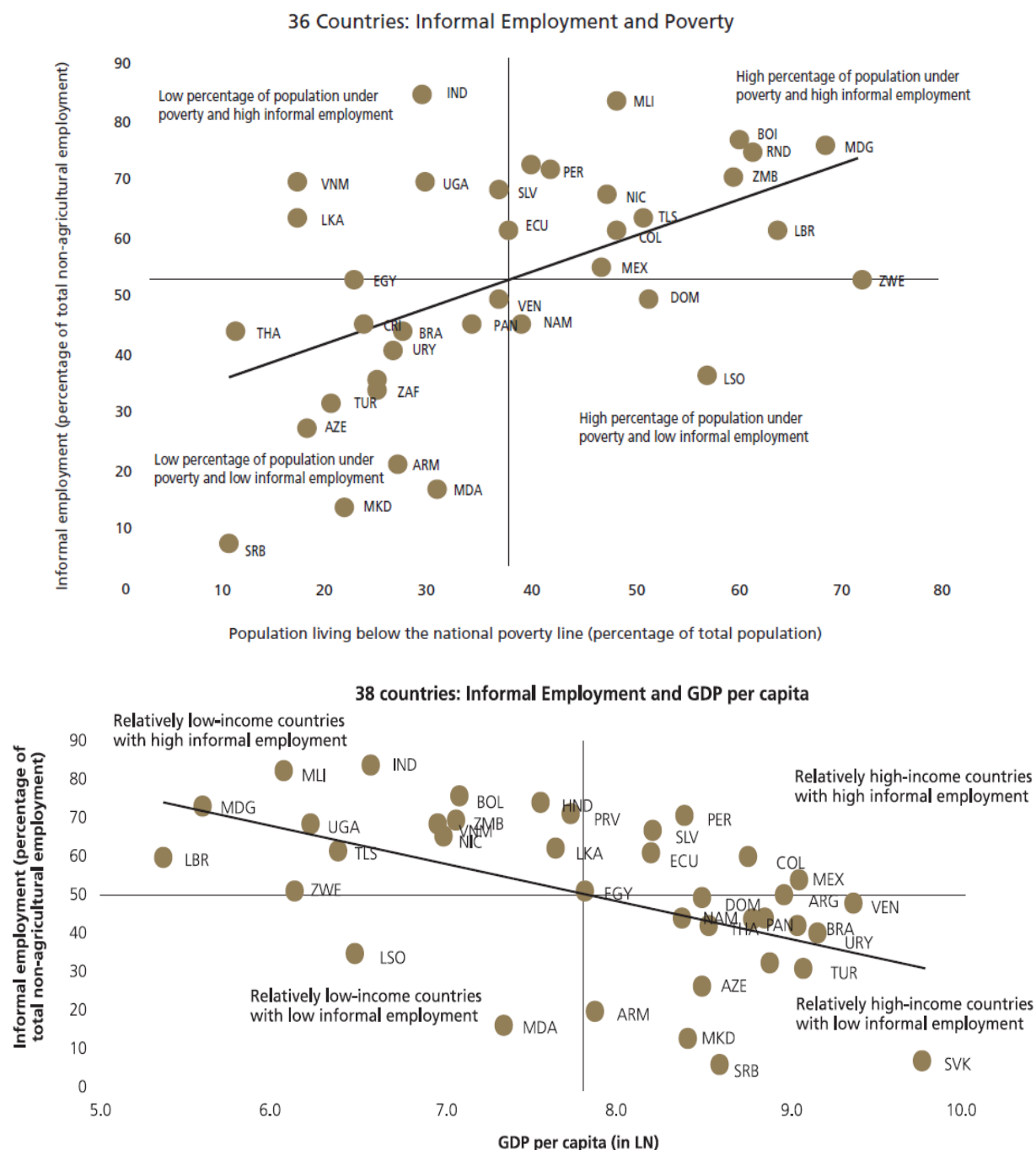
Source: ILO 2011.

Despite being the main engine of job creation in developing countries, Ayagarri et. al. (2011) find that small firms have lower productivity growth than large firms, which is in line with a number of previous studies (Banarjee and Duflo 2005, Maksimovic and Phillips 2002, and Bartlesman et. al., 2009). This in turn may explain why job creation does not translate into faster growth in developing countries.

A comprehensive landscape of formal sector jobs in developing countries by Ayyagari et al. (2011), while informative, must be read with a number of caveats in mind, some of which the authors themselves acknowledge. **First** and foremost, this analysis captures only a small segment of the private sector in the developing world, i.e., the formal segment, while the vast majority of the enterprises remain in the informal sector. **Second,** even within the formal sector, as the firm size analyzed is truncated at or above 5 employees, we do not get a picture of the entire formal sector including smaller firms that make up a significant share of the formal private sector. **Third,** as firm age is not one of the stratification criteria for the sample selection, the firm age in the sample may not be representative of the population. **Fourth,** as the data is based on only the surviving firms, the findings can suffer from selection bias resulting from firm-survivorship. The employment contribution from young small firms, which seem to have the highest rate of job creation, may be overestimated as they could also have higher failure rates. Also, without knowing much about the survivorship profile of different categories of firms in developing countries, it is difficult to conjecture the nature of bias in the job creation rates of different categories of firms. **Finally,** the authors defined job creation as the employment change over a two-year period. Thus the job creation effect in this analysis includes not only gross effect, but also short-term, and it is unclear how many of these newly created jobs would be retained by the survivor firms in the medium to long-term. This, in turn, leads us to the analysis of the literature on jobs over the life-cycle of the firms as discussed below.

Does a firm become larger (i.e., create more jobs) as it grow older? While it is a well-established phenomenon in the U.S. that new businesses tend to start small and grow substantially as they age (Dunne et. al. 1989, Davis et. al. 1996), Hsieh and Klenow (2014) demonstrate that this is not the case in India and Mexico. The latter study demonstrates that while in the U.S. the average 40-year old manufacturing plant employs over seven times as many

workers as would a typical five-years or younger plant, the surviving manufacturing plants in India and Mexico roughly double its size over the same life-span. This divergence in plant dynamics could potentially lower aggregate manufacturing productivity on the order of 25 percent in India and Mexico compared to the U.S. over the long term. Such a substantial divergence in aggregate productivity over the long term could translate into dramatic differences in per capita GDP across countries.

Ayyagari et al. (2013), however, argue that the findings of Hsieh and Klenow (2014) on India are driven by informal firms, as the latter study combines both the informal and formal in their analysis. Ayyagari et al. (2013) undertook separate analysis for formal and informal plants, as informal firms tend to be quite different from formal firms (La Porta and Shleifer, 2008). In doing so, they found that the average 40-year-old plant in the formal sector in India is two to four times the size of plants less than five years of age, both when compared contemporaneously and when comparing older and younger firms within the same cohort. The evidence of flat life-cycle appears to be the case only in the informal sector in India, where older firms in the unorganized manufacturing sector tend to employ fewer people than firms younger than 5 years old. Despite the divergence of findings of India between these two studies, the lower bound estimates (i.e., doubling of the size by older plants in the formal sector) of Ayaggari et al. (2013) is in line with that of Hsieh and Klenow (2014).

Using WBES data from 120 developing countries, Ayyagari et al. also find that the upward-sloping firm size-age profile is pervasive across the formal sector in developing countries. They found that the average plant 40 years old and older to be smaller than plants younger than five years in less than 10 percent of the countries studied. While the average middle-aged firm, between 10 and 19 years of age, is only twice the size of the average plant under the age of 5 years, the average plant that is 40 years and older is 4.65 times the size of the average plant under the age of 5 years, in terms of employment. These cross-country estimates, however, could be skewed upward due to survivorship bias in the data as discussed above.

Thus, there are at least two worrisome issues for the policy makers. First, what to do if firm growth tends to occur in the formal sector while a significant share (if not the majority) of the

private sector remains in the informal sector, where there appears to be no growth. Second, even within the formal sector there could be a glaring productivity gap between small and large firms, as illustrated by the "two Mexico" phenomenon of a recent McKinsey Global Institute (MGI) study (Remes and Rubio, 2014). From 1999 to 2009, labor productivity had risen by a respectable 5.8 percent per year in large firms with 500 or more employees. In small firms, with 10 or fewer employees, labor productivity growth had declined at an annual rate of 6.5 percent, while the share of employment, already at a high level, had increased from 39 percent to 42 percent over this period. As Rodrik (2014) argues, this phenomenon is not unique to Mexico but is an increasingly common occurrence around the developing world: the developing economies' low productivity segments, rather than shrinking, in many cases are expanding.

So what do all these phenomena—**a high incidence of informality, SME's potential contribution to employment share and job creation, relatively low productivity of the small firms, and a stunted growth profile of the firm in developing countries**—imply for the potential role of investment climate reforms in job creation? Conceptually, IC reforms can affect job creation through at least three channels.

First, reforming barriers to entry can spur the creation of new formal firms and enable existing informal firms to join the formal sector and grow. This, in turn, can lead to more jobs if firms in the formal sector tend to grow, while those in the informal sector tend to remain stagnant throughout their life-cycle. The scope of entry barrier reforms could range from reforming the entry process (such as the process of business registration, tax registration, obtaining necessary entry, and operating permits) to encourage entry through more proactive engagement on behalf of the government in promoting competition and a level playing field, providing incentives, designing prudent investment policy, and engaging in effective investment promotion activities.

Second, reforms relating to the business operating environment which promote an accountable, transparent, efficient, and predictable regulatory regime (implemented by different licensing and inspection authorities and tax and custom administration) can potentially foster firm productivity and profitability by reducing the transaction costs of doing business. This also can lead to more jobs. An improved investment climate that promotes firm expansion could lead

to not only more jobs, but also more high-wage jobs. Evidence from the household data in Ghana and Tanzania suggests that workers in micro/small enterprises (5-10 employees) have essentially the same earnings as the self-employed, while workers in medium and large firms earn between 50 to 70 percent more (Sandefur et al. 2006).

Third, by making the business exit process more efficient and less costly, an improved investment climate can facilitate a more efficient allocation of capital and labor in different sectors. If this efficiency then leads to more productive firms in different sectors, it may have a greater net job creation effect: job losses due to failing firms are outweighed by jobs created by surviving firms.

When designing any potential IC reform program it is important to acknowledge that these three channels are interrelated. For instance, a difficult operating environment and costly exit regime may negatively affect the entry decision of new or existing informal businesses in the formal sector. In such a circumstance, fixing one channel alone may not have the desired effects, such as reforming the entry process may not necessarily lead to firm formalization. An isolated reform could even have an adverse effect. As discussed in the following section, in a poor operating environment, reforming the entry process can potentially lead to a low quality of new entrants, which could then lead to lack of competition, sub-optimal firm size and low level of aggregate productivity in the formal sector, all of which have negative consequences for both the quantity and quality of jobs. Similarly, the realization of the benefits of any IC reforms could be also limited by lack of reforms in other related areas, such as access to finance, infrastructure, macroeconomic policies, etc.

Against this conceptual backdrop, the following section reviews the evidence from the relevant literature on the impact of different channels of IC reforms on job creation.

3. IC Reforms and Job Creation: What Have We Learned?

Rigorously measuring the effect of an investment climate reform or a package of reforms (or for that matter of any policy intervention) on job creation by firms would ideally require some

firms of given characteristics to be randomly exposed while other similar firms remain unexposed to such interventions. This would allow comparison of outcomes before and after the interventions between the exposed and unexposed groups. Such an approach, known as the randomized controlled trial (RCT), has been applied less frequently in the IC space compared to some other fields of development partly because many IC reforms, including reforms of business laws and regulations, occur at the national level or economy-wide, and aim to affect the entire private sector in the country simultaneously.[4] Accordingly, the effects of investment climate tend to be measured through cross-country or cross-sectional empirical analysis, and sometimes through the use of cross-country panel data. Routinely in the evaluation of various programs or projects, a comparison of the situation before and after the reform is used to measure the success or failure of such reform intervention.

A well-known problem with this type of analysis is that it is difficult to infer about the causal effect of any particular IC reform on the outcome of interest, such as job creation. Various uncontrolled or unobserved firm, industry, location, country-specific conditions, and international circumstances, could confound the potential causal effect of any IC reform on job creation. IC reforms might also occur alongside other political reforms or political commitments to private sector development that could make it difficult to distinguish the effect of IC reforms from those other reforms or commitments. Sometimes, the reform intervention itself could be potentially endogenous to the firm's decision-making process rather than being a purely exogenous change. While the use of cross-country or cross-sectional panel data is an improvement over simple cross-country or before-after comparison, the former still suffers from time-variant uncontrolled or unobserved factors. To overcome this problem, while an instrumental variable approach has been used in a number of studies, finding good instruments is not always that easy.

On the other hand, in many circumstances RCT can be built into a reform evaluation effort even when a reform is nationally implemented, for instance by randomly varying the degree of exposures of such reforms to the firms through encouragement design. Similarly, when resource

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[4] See Mckenzie (2010) for a detailed discussion on rigorous evaluation in Finance and Private Sector Development (FPD) space, which also argues that while the economy-wide nature of the PSD or IC reforms could be one reason for the dearth of rigorous evaluation in the PSD space, the mindset of the project designers and implementors also got a lot to do with such dearthness.

constraint affects the exposure or coverage of a reform effort and requires a phased-out approach in terms of coverage, RCT can be introduced by randomly selecting the locations or beneficiaries which would receive the exposure to the reform during the pilot stage. However, under circumstances where it is impossible to implement a RCT, quasi-experimental techniques, such as a regression discontinuity (RD) approach or matching in combination with difference-in-difference (DID) or triple difference (DDD) techniques could be used utilizing unique features of the reform. Some of these features may include any threshold criteria for eligibility to participate in the reform program based on firm size, sectoral, or geographical characteristics, or staggered fashion of implementation of reforms in different location of a country.[5]

A number of RCT and quasi-experimental studies have been undertaken recently to understand the effect of different types of IC reforms on firm performance from which this review draws.

3.1 Business Entry Reforms, Firm Formalization, New Firms, and Job Creation

While the majority of private sector jobs and firms in many developing countries are informal, informality itself can have dire consequences in terms of firm growth, productivity, and job creation. A series of high-profile sector studies by the McKinsey Global Institute which compared the operation of formal and informal firms in a number of countries concluded that informality has a negative impact on productivity, accounting for nearly 50 percent of the overall productivity gap between countries, such as Portugal, Turkey, and the United States (Farrell 2004). Using WBES, La Porta and Shleifer (2008) find that formal firms have substantially higher productivity levels than informal firms, while as mentioned before, Ayyagari et. al. (2013) have demonstrated zero growth of employment of informal businesses over its life-cycle vis-à-vis a two- to four-fold increase of employment in the formal sector in India.

Given these apparent substantial benefits to formalization, why does the majority of the private sector in many developing countries remain in the informal sector? The popular

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[5] Good quality instruments still need to be used, however, if there is any potential endogeneity concern of the policy intervention in the difference-in-difference type of setting, and any such endogeneity concerns associated with policy intervention and selection criteria could make the RD design invalid.

paradigm in the IC space, posed by de Soto (1989), is that informal firm owners would like to be formal, but costly regulations and bureaucracy prevent them from doing so, causing a productivity loss for these firms. The de Soto paradigm has motivated the World Bank Group Flagship Report, *Doing Business*, that measures the time, cost, and processes involved in 10 or so dimensions of a business life-cycle involving starting a business, its operation, and ultimately its exit. This paradigm is also empirically apparent in a number of informality surveys of the World Bank. World Bank Group Enterprise Surveys of informal firms in a number of countries have cited as some of the main challenges to formalization are the lack of access to information about how to register a business, the amount of time it takes to register a business, and tax policies (Figure 2).

Figure 2: Main Challenges to Formalization Reported by Informal Firms
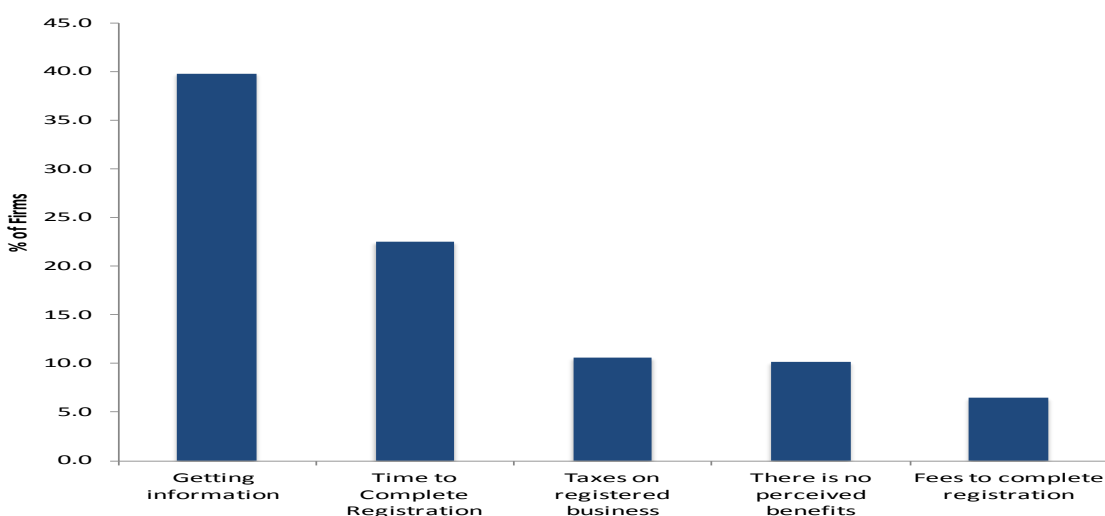
Source: Based on the data from the World Bank Informality Surveys, multiple countries, multiple years
Note: Average based on Informality surveys in: Angola, Argentina, Botswana, Burkina Faso, Cote d' Ivoire, Cameroon, Cape Verde, Democratic Republic of Congo, Guatemala, Madagascar, Mali, Mauritius, Nepal, Peru, and Rwanda.

Comparison between informal and formal firms demonstrates that the informal firms are worse off in terms productivity, profitability, and access to credit facilities. Regulatory constraints around business entry process are key factors preventing the formalization of informal firms. Therefore, development policy makers and practitioners have an obvious interest in finding ways to encourage informal firms to formalize by redesigning the business entry process. Making it easier to formally register a business has received enormous attention from

policy makers around the world. A number of investment climate programs in different countries have focused on simplifying and streamlining business regulations to address informality and private sector development. All these in turn have led to a surge of business entry reforms around the world in recent years. According to the *Doing Business* database, 368 ease-of-doing business reforms were approved and implemented in 149 economies between 2003 and 2012. As a result of these reforms, the worldwide average time to start a business has fallen from 50 days to 30, while the cost of starting a business is one-third of what it was in 2003. A simple before-after comparison demonstrates a sizeable increase in the number of registered businesses after the business entry reform across countries, ranging from about 8 percent in Spain to more than 80 percent in Bangladesh (Figure 3). This finding is broadly consistent with Klapper and Love (2010), which using panel data on the number of new firm registrations in 91 countries finds that the costs, days and procedures required to start a business are important predictors of the number of new firm registrations[6].

Figure 3: Business Registration Reforms and Increase in Registered Businesses
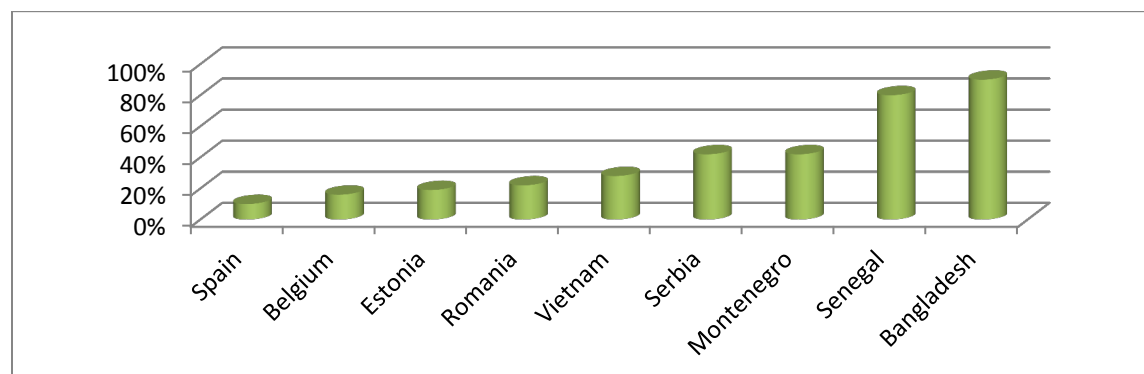
Source: Based on the information from World Bank Group Doing Business Reports, 2009 and 2010.

A question of obvious interest, then, is whether the increase in formal businesses is due to a significant share of informal firms moving into the formal sector or to the creation of completely new businesses. In either case, how has this growth affected job creation? Bruhn and McKenzie (2013) survey the literature on rigorous evidence of business entry reform, on

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[6] Klapper and Love (2010) find that small reforms, in general less than a 40 percent reduction in procedures or 50 to 60 percent reduction in costs and days, do not have a significant effect on new registrations. There are also important synergies in multiple reforms of two or more business environment indicators. Finally, countries with relatively weaker business environments prior to reforms require relatively larger reforms in order to impact the number of newly registered firms.

firm formalization, new business creation, and the effect of formalization on firm performance. They concluded that most of these reforms have limited if any success in moving firms from informal to the formal sector. For instance, survey findings show that access to registration information and costs associated with registration is a key bottleneck for formalization (Figure 2). An obvious policy response, therefore, is to reform the business entry system to reduce the time and costs of registration and reach out to the informal firms with information about how to register business and the potential benefits of such registration. However, a number of rigorous evaluations in Sri Lanka, Bangladesh, and Brazil find that providing information and waiving registration costs has no effect on encouraging firm formalization or job creation (Andrade et al. 2013, de Mel et al. 2013, and de Giorgi and Rahman 2013). While Alcazar et al. (2010) find that subsidizing the cost of obtaining a municipal license to informal firms in Peru led to 10–12 percent of informal firms obtaining a municipal license, a study by Mullainathan and Schnable (2010) on municipal licensing reform in Peru suggests that such uptake could be one-time or temporary, as many newly formalized firms decide not to renew their licenses. Moreover, Alcazar et al. (2010) find no effect of obtaining a municipal license on firm profits or revenues, while the effect on job creation is not analyzed. Beyond IC reforms, the studies that demonstrate the effect of other incentives or factors affecting formalization, such as being closer to the tax office (McKenzie and Sakho 2010), cash grants (De Mel et al. 2013), stricter enforcement (Andrade et al. 2013), do not analyze job effects due to formalization. But the former two studies find some positive effects on profits and sales, albeit often driven by the results of few firms.

Studies that find limited or no impact of business entry reforms on formalization often mention other potential constraints, such as tax burden, land titling and registration, and lack of benefits of formalization among possible explanations for the limited impact. In contrast to the generally limited impact of reforms on formalization, Fajnzylber et al. (2011) find that a 1996 business tax reduction and simplification program, "SIMPLES," in Brazil had a significant positive impact on firm formalization and performance. The benefits, however, were limited to a short time period around the introduction of this reform. In November 1996, the Brazilian government implemented a new simplified tax system for micro (up to R$120,000) and small firms (up to R$720,000). The new national system consolidated several federal taxes and social security contributions into a single monthly payment, from 3 percent to 5 percent of gross

revenues for microenterprises, and from 5.4 percent to 7 percent of revenues for small firms. In total, SIMPLES permitted an overall reduction of up to 8 percent in the tax burden faced by eligible firms, and also allowed substituting a fixed, and relatively low percentage of total revenues for the standard payroll contribution. This led to a substantial reduction in labor costs and hence created a strong incentive to hire new employees and legalize already existing labor relationships. The motivation behind the reductions in direct and indirect taxes achieved through SIMPLES was to enable small, unskilled labor-intensive firms to compete more effectively with larger enterprises for which high tax burdens are more manageable due to scale economies.

To estimate the casual effect of SIMPLES, Fajnzylber et al. (2011) employs two strategies: RD and DID (using ineligible firms as control group)[7]. The RD approach regards SIMPLES as an exogenous event for the small interval of firms who were born about the time of the introduction of SIMPLES. RD exploits the sample immediately around the introduction of SIMPLES to identify the effect of formality on firm performance for those firms that formalize because of a change in circumstances such as the introduction of SIMPLES. Using an extensive Brazilian micro-enterprise survey, the study finds that for RD and DID approaches respectively, SIMPLES led to an 11.6 percent and 7.1 percent increase in licensing rates, an increase of 7.5 percent and 6.4 percent in firms registered as formal legal entities, an increase of micro-firm registration of 6.3 percent and 5.7 percent, an increase in tax registration of 7.2 percent and 2.8 percent (not significant in DID), an increase in tax payments of 3.1 percent and 4.6 percent, and an increase in social security contributions of 4.3 percent (in RD only). The effect of SIMPLES on formality is often roughly twice as large in firms with employees. Older, better educated, male entrepreneurs are more likely to be formal. So are those who entered voluntarily and, especially, because of family tradition. Retail trade, transportation, restaurants, and lodging have the highest formality rates, while manufacturing, construction and personal services have the lowest. The firms that have obtained licenses have 55–57 percent more revenues and 45–49 percent higher profits than non-licensed firms. If only firms with employees are considered, the

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[7] SIMPLES explicitly excluded from program eligibility all activities that by law require the employment of professionals with regulated occupations. Examples of ineligible activities include the manufacturing of chemical products, machinery and equipment, as well education and health services. Incorporated companies were also not eligible for SIMPLES, nor were firms with government or foreign ownership, and firms operating in some selected sectors including financial services, real estate, private security, warehousing, and the manufacture of tobacco and beverages. In case of DID, the endogenous formality indicators are instrumented by period of SIMPLES introduction, age and gender of the entrepreneur and their interactions and these instruments are weighted by a RD weighting scheme that receives higher weights nearer the SIMPLES introduction window to improve the quality of instruments.

increase in revenues and employment are 60–70 percent and 10-40 percent, respectively. The avenue of increased firm performance appears not to be access to credit or to larger clients but rather the likelihood of having a fixed location, which permits an expansion of capital and employment, and a large increases in labor contracted formally. This suggests that the reductions in social security payments for hired labor were central— the lower amounts made firms more willing to register workers, and hence made them less concerned about detection. It also appears that firms born under SIMPLES adopted production technologies and lines of business that were more permanent, capital intensive, and of a larger scale, as measured by number of employees.
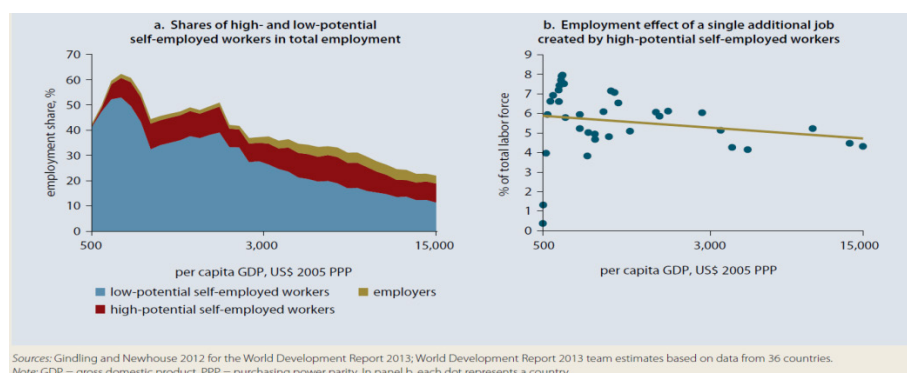
Two important points, however, are worth mentioning. Under the RD scheme, the effects are local average treatment effect (LATE), implying that the effects are only valid for the segment of micro-firms that change their formality status because of SIMPLES. The inference about the impact of formality is only valid in a small interval around the date of SIMPLES introduction. The data also suggest that there is decay overtime in the impact of SIMPLES. The other important concern is whether the treatment can be viewed as truly exogenous. If firms anticipated the introduction of the program and thus waited to formalize until after the program had been introduced, then the observed effect of SIMPLES would suffer from selection bias. However, simple statistical analysis of the entry patterns of different cohorts of firms before and after SIMPLES conducted in the paper do not indicate the possibility for such selection bias.

Overall, the limited impact of business entry reforms on firm formalization and job creation suggests that regulatory reform to address informality often fails to appreciate the heterogeneity of the informal sector. Not all the informal firms are necessarily highly aspirational entrepreneurs who remain in the informal sector because of the regulatory maze imposed on formalized firms. They are subsistence-entrepreneurs who operate in the informal sector for survival in the absence of job opportunities in the formal sector as the dualistic paradigm suggests (Harris and Todaro 1979). Hence, regulatory simplification will not necessarily induce them to formalize. Consequently, rather than informality being a cause of low productivity, it could very well be a consequence, in which less productive firms simply remain informal. For instance, De Mel et al. (2010) find that between one-quarter and one-third of the self-employed appear to share characteristics that give them the potential to be owners of

somewhat larger firms, while the remaining two-thirds to three-quarters look much more like wage earners. This is consistent with cross-country analysis of the World Bank (2013), which illustrates that the share of self-employed who demonstrate the attributes of successful entrepreneurs, defined as those who employ others and are not living in poverty) in total employment is small and relatively stable across countries at different levels of development. The share of self-employed who resemble the attributes of low-potential entrepreneurs (that is, the ones without paid employees and whose characteristics are closer to wage workers than employers in the formal sector), on the other hand, initially increases and then declines with GDP per capita (Figure 4a). If each of the high-potential entrepreneurs were to create a single additional job, total employment would increase substantially—even more so in low-income countries (Figure 4b). As a share of the work-age population, such additional job creation would amount to 8 percent in Kenya, for instance.

Thus it remains a critical policy question to be rigorously evaluated if and what type of IC reform(s) would induce this job effect by moving the high-potential entrepreneurs from informal to the formal sector.

Figure 4: High versus Low Potential Informal Entrepreneurs and the Employment Effect of High Potential Entrepreneurs

Sources: Gindling and Newhouse 2012 for the World Development Report 2013; World Development Report 2013 team estimates based on data from 36 countries.
Note: GDP = gross domestic product. PPP = purchasing power parity. In panel b, each dot represents a country.

Besides formalization, business entry reforms could also lead to job creation by enabling creation of more new businesses. A number of quasi-experimental studies have examined the impact of business entry reforms on firm registration and job creation, exploiting the cross-time and cross-municipality variation in implementing these reforms. A majority of

these studies, however, cannot distinguish if the increase in registered businesses is due to informal firms becoming formal or to the creation of new firms. Mexico introduced a one-stop-shop (OSS) for business registration in some of the most populous and economically developed municipalities starting in 2002, reducing the number of days to register a business from 30 days to 1 day. Bruhn (2011), using individual employment status from the Mexican Labor Market Survey, found that reform has increased the number of registered businesses by 5 percent and employment by as much as 2.8 percent. In addition, by encouraging competition, the reform seems to benefit consumers at the expense of the incumbent businesses. The competition from new registered firms decreases the prices by 0.6 percent and revenues of incumbent firms by 3.2 percent. Using the administrative data from the Mexican Social Security Institute, Kaplan et al. (2011) find that the same reform increased the number of registered businesses by 5 percent and employment by 7–8 percent.

A follow-up study by Bruhn (2013) divides informal business owners into those who share characteristics with formal business owners and those who have characteristics similar to wage earners. The study finds that wage earner types are less likely to register their businesses due to the reform, but more likely to become wage workers due to the job opportunities created by the reform. On the other hand, informal business owners with the characteristics of formal business owners are more likely to register, but only in municipalities with high pre-reform constraints to formal entrepreneurship. The effects are relatively small, and most informal business owners remain informal even after the reform.[8]

The quality of new entrants due to entry reform has a significant bearing on overall productivity growth and creative destruction driven by competition in the formal sector. Utilizing cross-counrty and cross-industry interactions in a DID setting, using a comprehensive database of European firms Klapper et al. (2006) find that costly business entry regulations hamper the creation of new firms especially in naturally high-entry industries based on a comprehensive database of European firms for 1998-1999. Costly regulations also force new entrants to be

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[8] Similar OSS reforms in Colombia have brought down the time to register a business from 55 days to less than 9 days and lowered the registration fees by 30 percent. Cardenas and Rozo (2007) use administrative data from chambers of commerce in six major cities to show that the Colombian reform led to a 5 percent increase in registered businesses. They did not analyze the job effect, however. Also, locational characteristics of business entry reforms might matter as Bruhn and Mckenzie (2013) find that this type of reform does not have any effect in less populous and more remote municipalities in Brazil.

larger and cause incumbent firms in naturally high-entry industries to grow slower by providing a form of protection to incumbents.

However, Branstetter et al. (2010) find that while OSS reform in Portugal increased firm creation by 17 percent and employment by 22 percent, the improvements were mostly in small firms owned by less educated entrepreneurs operating in low-tech sectors such as agriculture, construction, and retail trade. These are less likely to survive beyond their first two years. As noted in the previous section, most of the rapid job creation by small firms is likely to be short term in nature.

While most of the above studies analyze the effect of business entry liberalization, Bertrand and Kramarz (2002) in a natural experiment setting estimate the employment effect of entry restrictions that limit competition in the French retail sector. At the end of 1973, the "Loi D'Orientation du Commerce et de l'Artisanat", also known as "Loi Royer," was passed to protect small retails stores from the unruly growth of new forms of the distribution. Consequently, since 1974, approval by regional zoning boards has been required for the creation and extension of any large retail store in France. It appears that between 1974 and 1998, regional zoning boards approved only about 40 percent of the applications each year.

This study compared employment effects before and after the introduction of Loi Royer between the retail trade sector and hotels and restaurants. Retail is subject to the zoning board approval but hotels and restaurants are not. The comparison indicates that both sectors experienced about the same rate of employment growth before passage of the law, namely, 0.6 percent per year for retail and 0.8 percent per year for hotels and restaurants. After passage of the law, however, employment growth in retail dropped to 0.1 percent while that in hotel and restaurant sector increased to 1.7 percent. This divergence is not driven by labor market regulations, such as minimum wage laws which affect both the sectors equally. Both sectors are also affected by similar labor regulations and employ a large fraction of low-wage workers. The estimates of the authors suggests that a 30 percent increase in zoning board approvals of applications in food retailing would imply a 3 percent increase in food retail employment. A move from the approval rate distribution from the first quartile (about 30 percent) to the third

quartile (about 50 percent) would imply a lower bound estimate of 7–15 percent increase in retail employment.

However, a cautionary point related to this set of quasi-experimental studies is that the causal effect could be biased if these entry reforms (or the entry restrictions) are endogenous to the firm's decision-making process.

3.2 IC Reforms and Growth of Firm Employment in the Formal Sector

Broader IC Reforms

From the above discussions, it appears that the majority of the firms in developing countries are micro or small in nature and the growth of their size remains stunted compared to that of the developed world. The question is whether IC has anything to do with it. While neither Hsieh and Klenow (2014) nor Ayaggari et al. (2013) explicitly analyze the determinants of firm size growth in India as opposed to the United States, both studies suggest that investment climate-related constraints may have a critical role to play. According to Hsieh and Klenow (2014), the stunted growth of plant size in countries like India or Mexico could be due to less investment in organizational capital. Returns on such investments might be lower because of factors related to higher taxes on large plants, problems in contract enforcement that make hiring of skilled manager critical for plant expansion costly, difficulty with land titling and property ownership, and higher transportation and trade costs making it difficult to access distant markets). They find that the gap in the average revenue product of inputs between high and low productivity establishments is 5–6 times larger in India and Mexico than in the United States, suggesting that more productive establishments face higher taxes, factor costs, or shipping barriers in India and Mexico.

An overall poor investment climate may also have overarching consequences as the analysis of Hsieh and Klenow (2014) indicates. For instance, when there is lack of incentive to invest in organizational capital, post-entry investment in intangible capital by new entrants is likely to be lower, consequently the productivity of older plants will be also lower. Lower life-cycle growth

reduces the competition posed by incumbents to younger and new entrants. Consequently, a larger flow of entrants may bring in marginal entrants who are less productive than infra-marginal entrants, leading to a vicious cycle of "low productivity – small firm size – less job creation". All these in turn imply that the general equilibrium effect of business entry reform, without necessary improvements in other dimensions of investment climate that are conducive to boost aggregate total factor productivity, may make things worse despite a short-term or one-time spike in job creation. Future analysis of entry reforms needs to broaden its focus from partial equilibrium analysis of the impact of the reform to general equilibrium effects to provide a comprehensive understanding of various channels through which IC reforms can affect firm productivity and job creation.

While Hsieh and Klenow (2014) make the case for how IC related factors can lead to distortions, which in turn induce firm size to be smaller in developing countries, market failures or policy distortions can create fixed costs in the operation of businesses and thus lead to cost disadvantages for smaller firms (see Tybout 2000 for a review of issues). For instance, the burden of a complex and nontransparent regulatory environment may fall disproportionately on the smaller firms with fewer resources to deal with such a regime and might be subject to more frequent harassment or rent-seeking. Large firms may have more political influence and even can shape regulations in their favor. Another possibility (in line with De Soto view) is that it could be easier for smaller firms to dodge enforcement of regulations, as they could operate under the radar of the regulators, while large firms may suffer from higher tax burden and rent-seeking from corrupt officials because they are more easily identified and targeted. Thus, it is ultimately an empirical question if and how different aspects of IC differentially or non-linearly effect the employment growth and other outcomes of interest, such as profitability and productivity, of firms of different size in different countries.

Aterido et al. (2011) attempt to shed light on this issue by measuring the regulatory environment, prevalence of corruption, access to finance, and access to infrastructure on employment growth of firms of varying size. They use firm level data of 56,000 enterprises in 85

developing and 5 high-income countries.[9] The study finds that micro and small firms have less access to formal finance, face significantly greater interruptions in infrastructure services, and pay more bribes as a percentage of sales than do large firms, while larger firms spend significantly more time dealing with officials and red tape. A lack of finance and poor infrastructure reduce the employment growth of medium and large firms. Business regulations affect mostly the employment growth of small firms, which apparently prefer to remain small to evade regulators. The study concludes that a weak business environment displaces activity from large, medium, and small firms to the benefit of micro ones. To the extent that large firms are more productive, this would imply substantial resource reallocation from more productive firms to less productive ones leading to important losses in aggregate productivity. This shift in economic activity would also imply higher degree of informality and lack of innovation and growth opportunities as smaller firms tend to underinvest in innovation and training given the substantial fixed costs associated with these activities.

This cross-sectional study of Aterido et al. (2011), however, suffers from the problem of establishing direction of causality. To address the problem of endogeneity, the authors aim to reconstruct the broader business environment which was faced initially by firms of different size in a given sector of a country, and then tried to examine how that affect their employment growth.[10] The underlying critical assumption is that there was no change in business environment faced by firms of different size and sector over the years, which arguably is a very strong assumption as WBG *Doing Business* report portrays rapid changes in regulatory environments in different countries over a much shorter period of time. Morevoer, it is also difficult to gauge the direction of bias introduced through this assumption as changes in different dimensions of business environment in different countries could be driven by various time-

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[9] Access to finance is measured by the share of investment financed with external funds, the share of working capital financed externally, and/or the share of sales on credit, business regulatory condition is measured by the percentage of time managers devote to dealing with authorities or the days firms devote to inspections, extent of corruption is measured by if a firm paid bribe or the percentage of sales paid on bribes, and infrastructure bottleneck is measured by the number of days firms remained without power services and the share of average cargo's value lost while in transit.

[10] The paper constructs a measure of IC conditions faced by firm i on dimension k in country c, by averaging the responses of firms in the same country-location-sector-size cell excluding the observation for firm i in computing the average. While this measure is based on a firm's current size and responses about the current environment, to simulate the business environment faced by the firm in its initial condition, the paper assumes that the conditions faced by the firms of a certain size at the current period are the same as the conditions facing the same-sized firm in the earlier period. More specifically, suppose that a textile firm in capital city of Chile, which was small three years ago and is now observed as medium size in the survey. Then its response is used to construct the average of Santiago-textile-medium sized firms and used as the initial condition for the medium sized firms three years ago. Similarly, observations based on the current Santiago-textile-small sized firms are used as proxy for business environment faced by the Santiago-textile-small sized firms three years ago.

variant unobserved factors that a country, sector, or location fixed effect cannot adequately control for. The authors also claim that previous studies could suffer from omitted variable bias by trying to capture only a particular dimension of IC, such as business entry, or labor regulation, which they overcome by covering four dimensions of business environment. However, this claim could still suffer from the potential multicolinearity problem as it is unclear to what extent different dimensions of business environment used in the paper are separable from each other. For instance, the measure of regulatory environment in terms of time spent dealing with regulations and red tape could be closely related to bribe-payment. Thus it is unclear if what is being measured are two different aspects—regulatory environment and corruption—or the same broader aspect of the regulatory environment. Hence, it is difficult to make the claim that one aspect of IC is more binding than another for a particular type of firm. Similarly, time lost in cargo transit could be another proxy for regulatory red-tape rather than a proxy for infrastructure, unless we know what specific factors are causing this delay in custom clearance in different countries: is the problem the power outage or 42 signatures in the clearance procedure? Finally, Aterido et al. (2011) make the claim that it improves upon the previous literature by using objective rather than subjective measures of corruption. However, it is important to recognize that these objective measures of corruption can suffer from false response and non-response bias (see Jensen et al. 2010) given the sensitivity associated with these types of objective questions.

All these in turn point out the need to more rigorously measure the effect of any particular IC reform and compare different aspects of IC reforms on job creation in developing countries. This should be a critical agenda for future research. To aide such future research, this paper reviews below some quasi-experimental studies on business incentives and investment promotion agency. Most of these studies are from the developed countries, with one DID analysis using cross-country data, which again reinforces the need for more rigorous evaluations of various IC measures beyond business entry reform in developing countries.

Business Incentives

In many developing countries, the government often offer incentives (primarily tax incentives) to attract investments, particularly foreign direct investments. However, in the absence of rigorous evaluation of such incentive schemes, it remains unknown if these incentives

are effective in terms of attracting investment. Do the benefits of investment in terms of employment, technology transfer, and so on outweigh the revenue foregone in terms of providing such incentives? Would the investment have come even without the incentives? And if, instead of attracting investment, do such incentives actually distract investment by sending the signal that the business environment (without the incentives) is poor?[11]

Typically, any analysis of incentives, particularly those targeted to a particular location, such as the location based tax incentive or enterprise zone program (which is prominent in the U.S.) need to tackle at least three major issues. These are as follows: (i) unobserved characteristics of the location that could be correlated with plant characteristics and the incentive scheme (e.g. local taxation), (ii) plant or firm unobserved characteristics which could be correlated with location characteristics and the incentive scheme, and (iii) the endogeneity of the incentive scheme itself. The next section discusses the studies that attempt to address one or more of these problems in the developed countries, which could guide the evaluation of similar or related incentive policies in the developing countries, where there is a scarcity of such analysis.

Compiling a large panel data-set on local business tax rate and other relevant determinants for 11,000 German municipalities for the period 2001-2005, Becker et al. (2012) analyze how business tax rates effect the number of multinational enterprises (MNEs), MNE employment, and MNE fixed assets in different German municipalities. One stream of theoretical public finance literature suggests that lumpy investment, such as firm or plant location, is sensitive to profit taxation, which in turn could lead to a race to the bottom as countries have to offer a tax rate of zero in equilibrium to attract investors (see Wilson 1987, 1999 among others). In contrast, the new economic geography literature argues that there are factors generating agglomeration economies, which in turn reduce the sensitivity of location decisions of MNEs with respect to profit or capital taxation as taxes are only one factor affecting firm location (Ludema and Wooton 2000, Baldwin and Krugman 2004, Borck and Pflüger 2006). Against this backdrop, Becker et al. (2012) attempts to improve upon the previous cross-country empirical studies (that might suffer from endogeneity issues due to omitted location specific institutional characteristics

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[11] For instance, there could be little rationale for a location that otherwise have attractive features, such as infrastructure and human capital endowment, to offer a low tax rate to attract investment. Conversely, a location that suffers from various attractive features could offer tax incentive to attract investment to compensate for deficiencies in other factors that determine an investment decision. An investor would thus weigh the importance of these different attributes in its operation and then decide on choosing (or not choosing) a particular location.

and aggregation bias resulting from national average of within-country variation of locational attributes), by focusing on a set of locations (municipalities) in a given country. The variability of the effective tax rate across municipalities is due to the variability in business tax rates set by the municipality authority. Taxes on income and other determinants of tax base (e.g. the method of double taxation relief, withholding tax rates, depreciation allowances, etc.) are homogenous across municipalities since they are levied at the national level. Municipal tax rates could be endogeneous as municipalities might compete over MNEs via low tax rates, and MNEs could successfully lobby for low tax rates in a municipality which they would have picked anyway for reasons other than local taxes. Thus, the authors estimate both cross-section and panel data models in which business tax rates are instrumented by characteristics of neighboring municipalities. Overall, they find that the majority of the municipalities in Germany do not attract any foreign MNEs, and the municipalities which successfully attract foreign MNEs host only a small number of them. An average municipality would have to lower its business tax rate by about 15 percent (or 2.2 percentage points) to lure only one foreign MNE into its jurisdiction. This translates into an increase in employment in foreign-owned firms by about 157 and fixed assets by about 6.28 million Euros. The study concludes that most municipalities would not find this attractive and financially viable, and hence, only the municipalities those have generally favorable environments for firm location should be able to use their tax rates more successfully to attract foreign MNEs than those with less favorable environments.

Duranton et al. (2011) attempt to assess the impact of local property taxation on firm employment growth controlling for unobserved establishment heterogeneity, unobserved time-varying site-specific effects, and for the endogeneity of local taxation. They adopt an approach that involves spatial differencing, time differencing, and instrumenting of local taxation. In order to solve for unobserved time-varying site characteristics and the endogeneity of local taxation, neither spatial differencing nor instrumenting alone suffices. Instead, a combination of the two is needed as instruments that determine local tax rates are likely to be correlated with unobserved time varying local effects, while spatial differencing may not remove all the endogeneity of local taxes. As regards instruments, political variables are often used as instruments in the literature as one would expect different political parties to set local taxes differently. However, while changes in these political variables are highly likely to cause changes in local tax rates (i.e. they satisfy

the relevance condition for a suitable instrument), the changes in political variables could still be correlated with unobserved local characteristics. This affects the voting behavior and thus the composition of political party in a given location, violating the exclusion restriction of the instruments. Hence, combining spatial difference with instrumental variable (IV) approach, one can allow for the identification of the effect of the local taxes as spatial differences now eliminates the unobserved site-specific factors that are correlated with the political variables used as IVs. Applying this strategy to the English manufacturing data, Duranton et al. (2011) identifies that local taxation has a negative and significant effect (an elasticity of -1.024) on employment growth but no effect on business entry. This finding is in contrast to the finding of the previous literature that depict a positive relationship between local taxes and employment, potentially suffering from biases resulting from one or more of the above mentioned problems.[12]

Using triple difference (DDD) with IV, Hanson and Rohlin (2011) estimate the effect of Empowerment Zone (EZ) program on employment across different industries in the U.S. The federal government has begun offering a location-based set of incentives called the EZ program to firms willing to operate in and hire residents of parts of the designated inner city and rural areas. The main component of the EZ program is a wage tax credit, which is 20 percent of the first $15,000 in wages paid for a maximum credit of $3,000 per employee living and working in the designated EZ area. EZs are chosen from a group of applications made by state and local governments. Applications are accepted from areas where at least 20 percent of the population live in poverty and 6.3 percent are unemployed. Many of the nominees that do not receive an EZ designation become part of a less generous location based incentive program called Enterprise Communities (EC) that do not include the wage tax credit.

To estimate the effect of EZ tax incentive, the authors used EC areas as the best possible counterfactuals as EC areas were granted other benefits of EZ areas with the exception of wage tax credit. Their DDD strategy is to compare how the share of firms and employment in an industry changes between EZ (EC) areas and the larger city they are a part of, and test how the introduction of the EZ tax credit program changes this difference. This design reduces the

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[12] The positive relationship between tax rate and employment rather imply the reverse causality, i.e., the local authority tends to tax more the local businesses when they are doing better or some establishments are larger than others for unobserved reasons and larger establishments happen to be located in higher tax jurisdictions. The reverse could be also possible that in good times local authority can afford to keep taxes low because of the lesser need of social welfare related expenditure.

concerns regarding endogeneity and omitted variables bias by isolating the effect of the EZ (EC) from city fixed effects because it makes an across-time comparison. It also isolates the effect of the EZ (EC) from time variant citywide effects because it makes an intra-city comparison. Moreover, the comparison group used (i.e., EC) is similar to the EZ areas but is not likely subject to spillover effects from the policy because they are made up of tracts located in different cities than the EZs. As both the comparison and the treatment groups applied for EZ designation and met the requirements for unemployment and poverty, there would be no unobservable differences caused from going through the application process or being qualified. This estimation strategy requires the assumption that the difference between EZ and EC areas and their surrounding cities would have grown the same in the absence of the tax incentives and does not require the assumption that the EZ and EC areas would have grown the same.

Thus the remaining concern is zone-specific time-variant unobserved factors that might bias the result as the EZ areas could be those areas which would have been economically successful even in the absence of EZ tax incentives. To deal with this issue, the authors use congressional representation on the U.S. House of Representatives Ways and Means Committee as IV for EZ designation as previous work has shown this committee (and no others) to be a significant determinant of EZ designation. They thus find that the effect of location-based tax incentives has a heterogeneous effect across industries based on the ability of capital-labor substitutability in the production technology of different industries. In the short term, the retail and service sectors seem to benefit most from the program, increasing the share of establishments in the designated area by between 0.16 and 0.30 percentage points. These gains from the program seem to be offset by the losses in other sectors as the share of establishments in the transportation and finance, insurance, and real estate industries declined by between 0.16 and 0.19 percentage points. Interestingly, the industry level affects demonstrated in this study are not the goal of the EZ program but rather its unintended consequences. This implies that policy makers should also consider potential unintended consequences. Wherever possible, the impact evaluation should not only focus on partial equilibrium effect, but also should address the general equilibrium effects.

As mentioned, one of the key identifying assumptions of this DDD strategy is that the difference between EZ and EC areas and their surrounding cities would have grown the same in the absence of the tax incentives. This is a more relaxed condition compared with a DID strategy which would have required that the EZ and EC areas would have grown the same over time. Nonetheless, this strategy can be still weakened from the unobserved spill-over effects from the EZ and EC areas to their surrounding cities and if nature and rate of spill-over varies over time between EZ and its surrounding areas vis-à-vis EC and its surrounding areas.

To overcome this issue, exploiting a unique feature of the Texas EZ program, Freedman (2013) uses an RD approach to estimate the effect of this program on local labor market. In Texas EZ program created in 2003, participating businesses receive a combination of state and local benefits for up to five years, which can take of several forms. They can apply for state sales and use tax refunds (tied to amount of capital investment and number of jobs created) of up to $1.25 million over five years on qualified expenditures on machinery and equipment, building materials, electricity and natural gas, and construction labor. Local communities must also offer incentives to designated projects, which may include tax abatement, utility rate reductions, public service expansion (for example, road improvements), tax increment financing, expedited permitting, or other incentives. In contrast to EZ programs of other states, businesses in Texas need not necessarily locate in an EZ to receive benefits, nor does locating in an EZ guarantee that a business will receive benefits. To be eligible for benefits, a business located in an EZ must ensure that 25 percent of its new employees will meet economically disadvantaged or EZ residence requirements. A business located outside an EZ is eligible to receive benefits if it commits that 35 percent or more of its new employees will meet economically disadvantaged or EZ residence requirements. More so than the number of jobs that exist in distressed areas, the program might be expected to increase the number of jobs held by the residents of those areas.

The distinct feature of Texas EZ program that Freedman (2013) utilizes is that unlike most other states, where localities often must apply for EZ designations, Texas designates areas as EZs on a noncompetitive basis. Any census block group in which 20 percent or more of the residents have income below the federal poverty line is automatically designated as an EZ. Using this fixed cut-off rule based on the poverty rate, the study employs the RD with the identification

assumption that block groups in a sufficiently narrow window around the 20 percent poverty rate threshold are similar along observable and unobservable dimensions. Using rich administrative data derived from unemployment insurance records, the study finds that EZ designation increases resident employment in block groups with poverty rates near 20 percent by 1–2 percent per year. The employment effects seem to be concentrated in jobs paying less than $40,000 annually and are largely in the construction, manufacturing, retail and wholesale trade industries.

This RD design overcomes the problem of program spill-over effects as well as the issue that treatment and control locations can differ in unobservable ways[13] and that differences can vary over time. However, this RD design enables the detection of only a LATE effect; the results are only valid for a narrow group of census block groups with poverty rates near 20 percent. Another critical assumption is that the selection criteria, namely, the poverty rate of the location, is exogenous to the EZ designation. In other words, there is no sorting of areas around EZ cut-off points and no manipulation of local poverty data in the anticipation of being designated as an EZ area. While it is difficult to rule out all these selection issues, the study does not find any apparent indication of such selection issues in the data.

Investment Promotion

Besides incentives, a number of countries around the world compete to attract FDI through investment promotion activities (IPAs). While the accurate number of total IPAs in the world is unknown, there are about 189 IPAs who are the members of World Association of Investment Promotion Agency (WIPA). A number of years ago, UNCTAD estimated the total number of IPAs to be over 1,000. China alone probably has 500 plus IPAs at national, provincial, municipal, and zones levels to attract FDI. How effective are these IPAs in attracting FDI and what is the impact of that FDI in a host country in terms of job creation? Harding and Javorcik (2011) attempt to answer this question using the data on IPAs from the Census of Investment Promotion Agencies conducted under the aegis of the World Bank and the data on U.S. outward FDI disaggregated by host country and sector for 1990-2004. As the majority of the IPAs target

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[13] As typically a location needs to apply for an EZ status in different states, the locations those apply could be different in unmeasurable ways than localities which do not apply (i.e., the surrounding areas). Similarly, the decision of a business to invest in or hire from a particular location could be also influenced by unobserved local characteristics and expectations about the future prospects of the area.

particular sectors in their efforts to attract FDI, they adopt a DID approach and compare FDI inflows into targeted sectors, before and after targeting, to FDI inflows into non-targeted sectors during the same period. In addition, the changes in the host country business environment is controlled by including country-year fixed effects, heterogeneity of sectors in different locations by country-sector fixed effects, and shocks to FDI supply in particular sectors by sector-time fixed effects.

This study finds that investment promotion appears to be more effective in countries where English is not an official language and in countries which are more culturally distant from the U.S. It works better in countries with less effective governments, higher corruption, and a longer time period required to start a business or to obtain a construction permit. IPA tends to have a positive impact on FDI inflows in the developing world but not in industrialized countries. Even within the subsample of developing countries, it works better in places with higher information asymmetries and more red tape. The back-of-the-envelope cost-benefit calculation suggests that targeted sectors receive more than twice as much FDI than non-targeted sectors in developing countries. As for the costs, the 2004 budget figures from the Census indicate that an average IPA spent $90,000 per sector targeted. Combining the benefit and the cost figures, it seems that a dollar spent on investment promotion leads to $189 of FDI inflows. Using a data set on employment in U.S. affiliates abroad, the study also shows that priority sectors experience a 68 percent increase in affiliate employment when compared to non-targeted sectors. This implies an additional 1,159 jobs for the average sector, or $78 per job created.

Similar to other quasi-experimental studies, a key concern, however, is the identification assumption of this study, i.e., to what extent can one treat the sector targeting strategy of an IPA as an exogenous event? An IPA's sector targeting decision could be a response to earlier FDI experience in the sector or in anticipation/prediction of the emerging trend of U.S. or global FDI inflows in different sectors. To address this concern, the study excludes the countries that reported in the IPA census that targeting decision was based on the past success or failure in attracting FDI to the sector, and the above results seem to hold. In addition, the authors did not find any evidence suggesting that targeting took place in sectors with relatively high or low FDI inflows in the years preceding targeting. While these are important checks, these do not

completely rule out the possibility that to demonstrate the effectiveness of its promotion activities, an IPA might response that its targeting decision was not driven by historical experience or in anticipation of future FDI inflow in a given sector while in reality it was indeed the case. In other words, the findings might potentially suffer from the selection bias resulting from unobserved characteristics of IPAs and associated false response bias in the survey.

4. Knowledge Gaps and the Way Forward

The above discussion of the literature points out a number of critical knowledge gaps. **First,** in addition to having limited rigorous empirical evidence of the job impacts of different IC reforms, in most cases, we are unable to identify the gross vs. net job effects as well as short-term vs. long-term job effects. **Second,** among other sources of bias, the estimated job effects of IC reforms from the cross-country enterprise survey analysis are also likely to suffer from survivorship bias, potentially leading to an overestimation of the job effects. **Third,** overall there is a relative scarcity of rigorous evaluation on the job effects of IC reforms in developing countries. Moreover, most of the existing rigorous evaluations are in the areas of business entry reforms, leaving many other important areas of IC reforms open for future research. Despite the stylized fact that majority of the informal firms are survival-entrepreneurs, the potential job effect can be substantial even if the thin slice of the high-potential entrepreneurs can graduate from the informal to the formal sector. Thus in the firm formalization space, a critical topic for future research should be how to identify these high-potential entrepreneurs and determine if and what type of IC reform(s) would induce job creation by moving the high-potential entrepreneurs from informal to the formal sector. Another critical area for the future research agenda is to identify if, how, and what IC reforms matter most for employment and productivity growth of the firms in developing countries over their life-cycle, as firms in developing countries tend to grow at a much slower pace than those in developed countries leading to a substantial cross-country differences in aggregate total factor productivity (TFP). Even smaller differences in aggregate TFP over the long period of time can have dramatic consequences on per capita GDP of different countries. So this research agenda would be critical not only from job creation perspective, but also from the perspective of improving the overall level of development of a given country. Furthermore, unleashing the potential for firm growth in the formal sector could

also lead to gainful employment opportunities in the formal sector for the low-potential informal entrepreneurs. **Finally,** most of the existing rigorous evaluations focus on the partial equilibrium effects of particular IC reforms, while the unmeasured general equilibrium effects of such reforms could be dramatic. For instance, in the presence of other market distortions or an overall poor business environment, a single reform such as simplifying business entry—while it may lead to an increase in registered businesses—will attract lower-quality entrants, leading to an overall decline in aggregate productivity and sub-optimal firm size in the economy. The future research agenda thus should also focus on the general equilibrium effects of IC reforms and not just the partial equilibrium effects.

The future stream of IC and PSD projects can play a critical role in bridging these knowledge gaps. By doing so they will not only enrich our knowledge base but will also make meaningful contributions in our client country development agendas and thereby contribute to the effort to achieve the twin goals of reducing poverty and promoting shared prosperity. To do so, actions on three fronts are necessary:

First, concerted initiatives are needed to raise awareness and sensitization among the policy makers, development practitioners, project designers and implementers on the importance of rigorous evaluation in the area of IC reforms and job creation. Often, they are the first to design and implement an IC intervention, and thus their awareness of rigorous evaluation could open up possibilities for conducting rigorous evaluations of the planed IC interventions. This is not to say that all of them need to be converted as impact evaluation specialist or academic researchers, but basic exposures to evaluation methodologies along with practical examples of how rigorous evaluations can meaningfully impact the effectiveness of government and donor resources to achieve the intended project objectives could be useful first course of action.

Second, rigorous evaluation should be viewed as an integral part of an operational project design and implementation, not an accessory. Often in the world of project operation, rigorous evaluation is viewed as an interesting academic exercise. However, it can enable the policy makers and development practitioners to make effective use of their limited resources through a strategy of evidence-based policy making. Increasingly, there is evidence of rigorous evaluation

even in some of the most challenging environments in the world in different fields of development. The key here is to have the appropriate mindset and to have a much closer interaction between policy makers, project implementers, and researchers from the very beginning of the design of a particular IC program or policy intervention, so that all the options and possibilities for rigorously measuring the impact of such intervention can be thought through before the start of the project implementation, as opposed to the evaluation becoming an afterthought.

Third, the ability of tracking jobs in different locations and sectors over time is a necessary pre-condition to be able to detect the job effect of any intervention or policy change. This implies the importance of good quality data both from surveys and administrative sources in developing countries. A key reason why most of the quasi-experimental evaluations of business incentives are from the United States or other developed countries is the absence of good quality data on relevant variables of interest in many developing countries. Increasingly, the use of information and communication technologies (ICT) is becoming an integral part of IC and PSD projects which include online registration and tax filing, online applications for various government services, and automation of different government agencies, such as business registry, tax authority, and investment promotion agency. Similarly in different countries efforts are underway to maintain a database of businesses that tracks different incentive schemes. All these initiatives would significantly reduce the cost of collection and maintenance of good quality administrative data, which could be leveraged for rigorous analysis. The World Bank Group in coordination with development partners should discuss up front with the relevant government counterparts the possibility of granting the use of this type of administrative data for research purposes, observing, of course appropriate confidentiality.[14] At the same time, efforts are also needed to improve the coverage and quality of relevant surveys related to jobs such as, household, labor force, and business surveys.

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[14] Currently, a team from the investment climate department has proactively engaged with business registration and tax authorities of different countries to use administrative data to analyze firm dynamics and job creation in these countries.

5. Summary and Conclusion

The current landscape of non-farm jobs and the private sector in developing countries suggest a number of potential channels through which different IC reforms related to business entry, operation, and exit can positively affect job creation. Despite these conceptual links, however, rigorous empirical evidence on IC reforms and job creation in developing countries is relatively scarce, and most of these studies focus on business entry reforms with a handful of them also focusing on business taxation and investment promotion. Overall, the findings are quite mixed. While none of the RCTs find any evidence of entry reforms on firm formalization and job creation, a few quasi-experimental studies demonstrate job effects from firm formalization through tax reforms and job effect from business entry reforms where the job effects are most likely coming from the creation of new businesses rather than informal businesses graduating to the formal sector.

Further research is needed to bridge a number of critical knowledge gaps related to the different areas of IC reforms (beyond business entry) and job creation. The research needs to disentangle a number of factors. Among them: the gross vs. net and short-run vs. long-run job effects; and the general equilibrium effects of different IC reforms related to jobs, productivity, competition, and other developmental outcomes. Most of the existing rigorous evaluations focus only on the partial equilibrium effects. Another critical finding from the literature is that employment growth of firms in developing countries is much slower than in the developed world, leading to substantial aggregate total factor productivity gap, and presumably contributing to the divergence in per capita GDP. While cross-sectional analysis points out the significant role of poor investment climate as a potential cause of these differentials, such analysis is prone to establish rigorous causal link between IC reforms and employment growth in the formal sector. More rigorous evaluations are thus needed in this area going forward.

To bridge these current knowledge gaps on IC reforms and job creation, the World Bank Group, in partnership with other development partners, can play a significant role by sensitizing the policy makers, development practitioners, project designers, and implementers to the importance of rigorous evaluation in the IC space. Much closer interaction among project

implementers, policy makers, practitioners, and researchers from the very beginning of an IC intervention design will help, as will improvement in the quality of administrative and survey data related to jobs and private sector development in developing countries. ICT led IC and private sector development reform initiatives can play a critical role in this regard by reducing the cost of collecting and maintaining good quality administrative data and making it accessible for rigorous analysis.

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