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# Labor Regulations in Developing Countries: A Review of the Evidence and Directions for Future Research

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# **Labor Regulations in Developing Countries: A Review of the Evidence and Directions for Future Research<sup>1</sup>**

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## 1. Introduction

The effect of labor market regulations on economic outcomes is the subject of an ongoing and often heated debate among economists and policymakers. To some, regulations are detrimental to economic efficiency and therefore an impediment to growth and prosperity. To others, they are essential tools to correct market imperfections and achieve goals of redistribution without hampering efficiency (for an excellent discussion on this debate, see Freeman, 2005; see also Boeri and van Ours, 2008).

Until recently, the debate on the effects of labor market regulations was confined to the context of developed economies (Lazear, 1990; OECD, 1994a, 1994b; Nickell et al., 1999; Blanchard and Wolfers, 2000). In the past decade or so, however, the debate on the role of labor market regulations within developing countries has grown considerably. In addition to the increased availability of data from developing nations that has allowed for expansion of empirical research about these countries, two major events have contributed to the shift of focus away from the United States and Western Europe: transition and globalization.

The fall of the Soviet Union and the ensuing transition of countries in Central and Eastern Europe from planned to market economies increased interest in these regions and their experiences. For these middle-income countries, the process of transition entailed a massive reallocation of resources across sectors of the economy, and this led to a dramatic increase in labor market risk for workers. To ameliorate the effects of such increased risk, policymakers were faced with the challenge of designing all new labor market institutions. Which labor market institutions these nations adopted have affected the dynamics of the transition (Boeri and Terrell, 2002) and economic growth, and have thereby drawn attention to regulation in non-OECD countries.

Recent decades also have witnessed a progressive integration of all economies into international markets. There is an ongoing debate surrounding the effects of “globalization” (i.e., shrinking barriers to international economic transactions) on poverty and inequality in both developed *and* developing economies. Existing evidence suggests that the process of globalization has been accompanied by considerable changes in the distribution of income. However, contrary to what most trade models predict, the distributional changes have not been in favor of the less skilled, who represent the relatively abundant factor in developing countries (Goldberg and Pavcnik, 2007). As the increased exposure to international market forces adds to the labor market risk inherent to economic growth, developing countries are faced more and more often with the question of providing labor market protections, especially to the most vulnerable

segments of the population. Thus, assessing the effects of labor market regulations in developing countries has become increasingly central to the policy debates surrounding globalization.

As mentioned above in the context of globalization, but relevant to many other challenges and potential policy “fixes” facing developing countries, economic theory is not always helpful in predicting the effects of labor market regulations on economic outcomes. Indeed, the predicted outcomes of any particular labor market institution depend crucially on which labor market model one believes best describes the real world. Standard, competitive models of the labor market, for example, invariably predict that minimum wages reduce employment. On the other hand, in models including some form of modern monopsony or other market imperfection that results in job matches yielding economic rents (e.g., Mortensen and Pissarides, 1994), minimum wages redistribute the rent without necessarily affecting employment levels or overall economic efficiency. In some circumstances, minimum wages might even increase employment. Because of the ambiguities of the theoretical predictions, any assessment of the effects of labor market regulations rests ultimately on empirical studies. Throughout this survey, we provide brief sketches of theory and model-based predictions, but the heart of this paper is devoted to empirical work, which provides the real-world test of theoretical predictions. In many examples throughout this text, it can be seen that either the models fail to explain the data, or the data or analysis methods are insufficient. As such, models are informative, but applied research is necessary for real-world decision making.

One of the themes we revisit throughout the survey is that the specific context of developing countries is often radically different from that of developed economies. In particular, developing countries are often characterized by weak law enforcement, a large informal sector, underdeveloped capital markets, and informal credit and insurance networks. These features of developing country economies have at least two important implications: First, results from developed countries should not be directly extended to developing countries’ settings without serious reflection upon these differences; second, within studies on developing countries, neglecting these features can lead to incorrect predictions and misguided interpretation of the empirical findings.<sup>3</sup>

Labor market regulations have the potential to affect firms’ choices over inputs, investments, technology, and output. In particular, regulations might influence the allocation of resources across firms and sectors of the economy. Because of the critical role that job

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<sup>3</sup> For instance, informal arrangements such as family networks of credit and insurance have been found to very much influence the impact of interventions such as cash transfers to the poor (Angelucci and DeGiorgi, 2008).

reallocation plays in the process of economic development (see, e.g., Caballero and Hammour, 2000; Krizan, Haltiwanger, and Foster, 2002; Bartelsman, Haltiwanger, and Scarpetta, 2004), understanding whether labor market institutions actually help or hinder reallocation stands out as an important task.

Firms' and workers' choices (or lack thereof) to function in the informal sector are likely impacted by countries' regulatory and tax structures. For this reason, the issues of enforcement and the informal sector deserve special attention in this literature. Evidence from cross-country studies indicates that poorer countries have stricter labor laws compared to richer countries (even though they offer less social protection). At the same time, strict labor regulation is associated with a larger unofficial economy (Botero et al., 2004). Since "informality" may be associated with worse working conditions and poor job "quality,"<sup>4</sup> it is important to assess, with sound empirical studies, whether informality is in fact *caused* by stricter regulations.

Which regulations and institutions to consider is also an important factor when examining the functioning of the labor market. As reflected in the organization of this survey, theoretical as well as empirical research has tended to focus on the impact of one regulation at a time, often ignoring the interactions of each labor market regulation with other policies in the labor market, as well as those in other markets. We acknowledge that institutions do not operate in isolation, and we therefore emphasize these institutional interactions throughout the survey. The effects of an increased minimum wage, for example, can be neutralized by a reduction in the non-monetary components of compensation, such as paid vacation days, healthcare plans, retirement benefits, etc. (Wessels, 1980). On the other hand, the presence of a binding minimum wage might exacerbate the adverse employment consequences of regulations requiring employers to provide certain benefits to workers (e.g., healthcare) or affect the impact of employment subsidies (Betcherman, Daysal, and Pages, 2007). Further, interventions targeted at certain individuals can affect the labor supply decisions of other family members, including children (Basu, Genicot, and Stiglitz, 1999; Basu, 2000). These observations highlight the importance of incorporating the formal and informal institutional characteristics of developing country markets into analysis, as well as investigating the general equilibrium effects of policies.

While labor market regulations may be differentially enforced across and within countries, most are introduced with the stated objective of protecting workers from uninsurable labor market risk, such as *employment* risk, or from *earnings* risk. To improve the earnings of the most disadvantaged categories of workers, governments typically set minimum wages; they might

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<sup>4</sup> This claim is, however, controversial. Employment in the informal sector might be a voluntary and efficient choice by both workers and firms.

also mandate that employers provide non-wage benefits to their workers, such as healthcare, paid vacations, maternity leave, etc. To protect workers from employment risk, governments can provide unemployment insurance to those who lose their job, and/or they can decide to protect existing jobs by restricting the ability of firms to lay off employees at will.

Thus, this paper focuses on minimum wages (MW), mandated benefits (MB), employment protection legislation (EPL), and unemployment insurance benefit systems (UIB). Even though different institutions might try to accomplish the same goal(s), the mechanisms through which each regulation interacts with market forces and affects the behavior of workers and firms are different. We have organized this survey accordingly; each of our four chapters is essentially self-contained. In each, we address both distributional and efficiency effects. While distributional goals are often the main concern of policymakers, the efficiency effects of regulations also have important consequences in determining how much there is to distribute.

Our survey is selective in that it is meant to highlight the most important knowledge gaps and promising areas for future research. In assessing the findings of this large body of empirical literature, we discuss the theoretical background and the empirical methodology behind studies of the effects of labor regulation on efficiency and equity outcomes. Although the focus of this paper is on the effects of labor regulations in developing countries, we refer to the evidence from developed countries where relevant, especially when no evidence from developing countries is available; and we believe the theory or methods could be fruitfully applied in the study of developing economies as well.

## **2. Minimum Wages (MW)**

To improve the welfare of low-wage earners, many countries mandate a minimum wage that employers must pay their workers. Whether such a policy actually achieves the intended outcome has been a subject of great controversy for decades. Theoretical predictions of the effects of minimum wages vary, and the empirical evidence has so far yielded contradictory results, depending on the country, the source of minimum wage variation, the methods of analysis, and the assumptions required for each particular econometric framework.

### *Theoretical Considerations*

Theoretically, the effects of a minimum wage vary depending on what model of the labor market one believes best describes the real world. If wage and employment are determined in a

competitive labor market, a legal minimum wage ( $W^M$ ) set above the market-clearing wage level ( $W^*$ ) will result in a reduction of employment, the magnitude of which depends on the actual wage increase ( $W^M - W^*$ ) and on the elasticity of labor demand. If this is the case, higher wages for the workers who remain employed are achieved at the cost of other workers becoming unemployed. The net effect on total welfare is then ambiguous, depending on the magnitude of the employment effect as well as the alternatives available to those who remain without a job.

Departures from the perfectly competitive framework can lead to dramatic changes in the predicted effects of a minimum wage. A number of market imperfections are capable of generating positive implications of minimum wages effects in theoretical models. In search and matching models of the labor market á la Mortensen and Pissarides (2004), for example, search frictions generate rents every time a worker-firm match is formed. In this sort of bilateral monopoly, the rent is divided between the worker and employer according to some bargaining process - typically modeled as Nash bargaining. In such a world, a mandated minimum wage can result in a redistribution of the surplus from employers to workers without reducing employment.<sup>5</sup> Other modeling environments also can generate positive effects of minimum wages. For instance, Albrecht and Axell (1984) feature a model with firms of heterogeneous quality where a minimum wage drives low-quality firms out of the market, which has positive overall welfare effects.

Further, even in the simplest competitive model of the labor market, the predicted effects of a minimum wage depend crucially on a series of institutional variables, including the degree of compliance, enforcement, penalties for non-compliance, and existence (and size) of uncovered sectors. For obvious reasons, these issues are much more likely to be relevant in developing countries.<sup>6</sup> Clearly, if enforcement is ineffective or if the penalties for paying wages below the minimum are only modest,<sup>7</sup> there is little reason to expect compliance and hence effects of minimum wages.

Related to the enforcement issue is that of informality.<sup>8</sup> In developing countries there is often a large informal sector where minimum wages and other labor market regulations do not

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<sup>5</sup> This is more likely to be true for moderate levels of the minimum wage. If the minimum wage is set high enough, employment will fall even in imperfectly competitive labor markets (including the “pure monopsony” case).

<sup>6</sup> Compliance issues are not limited to developing countries, however. Ashenfelter and Smith (1979) estimated that in the United States in 1973, just 64 percent of workers who would have otherwise earned less than the federal minimum wage were actually earning the minimum wage or above.

<sup>7</sup> In the United States, for example, employers must simply compensate their workers for the difference between the minimum required by the law and what they were actually paid (Reynolds, Masters, and Moser, 1998).

<sup>8</sup> The boundaries between formal and informal workers are often blurred. See Maloney (2004).

apply or are not enforced.<sup>9</sup> To the extent that one is interested in the overall effects of a policy, the existence of an uncovered sector complicates the analysis. Competitive models with a covered (where the MW applies) and an uncovered (where the MW does not apply) sector predict that a binding MW in the covered sector prices out of this market all those workers whose marginal product falls below the wage floor. These displaced workers will flow into the uncovered sector, where the equilibrium wage rate will fall and the equilibrium employment will rise. If this is the case, the MW could lead to a pay reduction for the lowest paid workers in the economy, and to increased wage inequality. Of course, the interdependencies across sectors can be more complicated than that.<sup>10</sup> The exact empirical predictions depend on the modeling choices regarding the intersectoral interdependencies (elasticity of labor demand and supply in the various sectors, and elasticities of substitution across different inputs or different types of workers; see Fields, 1994).

To further complicate the picture, in developing countries there are sometimes multiple minimum wages, varying across occupations, industries, and/or geographical region. In addition, high and volatile inflation induces wide fluctuations in the real value of the minimum wage in developing economies. On the positive side, however, the cross-sectional heterogeneity of the impact of minimum wage policy actually can be exploited as a source of identification in empirical work.

### *Empirical Evidence*

Given the ambiguity of the theoretical predictions, it is not surprising that a vast literature exists which tries to empirically evaluate the effects of minimum wages. Most of the existing literature has analyzed minimum wages in the United States, but many studies have been conducted in the context of developing countries. In fact, in the context of developing countries, no other type of labor market regulation has received more attention than minimum wages.<sup>11</sup>

Empirical studies on the effects of minimum wages have mainly concentrated on two outcome variables: wages (typically, only for those who remain employed in the formal sector) and employment (in both the formal and informal sectors). A more limited number of studies also have directly looked at the effects on (cross-sectional) inequality and poverty. Most of the evidence from developing nations comes from Latin America (notably Brazil, Chile, Colombia,

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<sup>9</sup> Under some circumstances, escaping tight labor market regulations actually might be the reason behind the formation (or expansion) of an informal sector.

<sup>10</sup> For instance, movements of capital from the covered to the uncovered sectors could push wages up in the uncovered sector (Saint-Paul, 2004).

<sup>11</sup> Several excellent reviews of the literature exist. The most recent and comprehensive review, on both developed and developing countries, is Neumark and Wascher (2007).



Costa Rica, and Mexico) and Indonesia. We also report on some interesting research that has been conducted in the context of the agricultural sector in Morocco (Azam, 1997), in Ghana (Jones, 1997), and in Kenya (Andalon and Pages, 2008).

### *Wage Effects*

The first concern of empirical studies on minimum wages in developing countries is whether statutory MWs actually have an impact on wages. In some cases, the level of the minimum wage could be set so low that it does not bind. This is what Bell (1997) found when looking at the Mexican experience in 1984-1990, for instance. As we previously mentioned, weak law enforcement in developing nations can lead to widespread non-compliance. Several papers find that compliance with MW laws is related to observable characteristics of workers or employers. Andalon and Pages (2008), for instance, find that in Kenya minimum wages are better enforced in the non-agricultural, urban sector. Non-compliance (measured as the fraction of workers earning below the statutory minimum wage) was especially high for less educated workers employed in agriculture and for young workers.<sup>12</sup> In contrast, Azam (1997) documents that in Morocco MWs are better enforced in agriculture. Other papers have suggested that compliance is related to firm size or age. Gindling and Terrell (2007b) document that in Honduras MWs have an impact on wages paid by medium and large firms, but not on those paid by small firms. These findings suggest that MWs can affect the size distribution of firms. Because most economic growth appears to be generated from growth of existing firms rather than from the creation of new firms (Rajan and Zingales, 1998), the potential effects of MW on firm size carry important implications for development.<sup>13</sup>

In general, researchers have tended to pay special attention to the wages of workers who are most likely to be affected by the introduction (or change) of a minimum wage, namely teenagers, females, minorities, and other groups of workers who tend to earn lower wages. Not surprisingly, most papers have found that minimum wages are most likely to affect the wages of workers at the bottom of the wage distribution, i.e., those workers who are *a priori* most likely to be affected. This is the case, for instance, of Fajnzylber (2001) for Brazil, Suryahadi et al. (2003) for Indonesia, or Gindling and Terrell (2007a) for Costa Rica.

Some papers also have presented evidence that seems to indicate that MWs have an impact throughout the wage distribution. In their study of Colombia, Maloney and Nunez Mendez

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<sup>12</sup> Of course, an issue of relevance here is whether wage observations less than the minimum wage should be interpreted as genuine noncompliance or, instead, to measurement error (Eckstein, Ge, and Petrongolo, 2006).

<sup>13</sup> Employment protection legislation has been found to affect the size of firms in Italy. See our discussion of Garibaldi, Pacelli and Borgarello (2004) and of Kugler and Pica (2008) in the EPL section of this paper.

(2004), for instance, find that although the wage effects of the minimum wage decline with a worker's position in the wage distribution, they remain substantial even for workers earning 3 or 4 times the minimum. The authors interpret their findings as indicating that the minimum wage is used in Colombia as a "numeraire" which is taken as a reference point for setting wages throughout the economy ("lighthouse effect"). Similarly, Fajnzylber (2001) and Gindling and Terrell (2007a) find effects of the minimum wage on earnings along the entire earnings distribution in Brazil and Costa Rica (but always with stronger effects at the bottom). A possible theoretical rationale for such "spillover" effects<sup>14</sup> of the minimum wage is offered by Flinn and Mabli (2008): In an environment where there are rents generated by search frictions, a higher minimum wage improves workers' outside options in the Nash bargaining problem, so there can be a wage effect even for workers for whom the MW is not directly binding.

### *Employment Effects*

When an effect of minimum wage provisions on wages is detected, the next step typically taken in the literature is to evaluate whether that has had consequences on employment levels. The empirical results tend to be quite mixed, both in studies of different countries and the same country. In studies in which disemployment effects of MW are found, they are typically detected for workers who earn a wage close to the minimum. As some workers become relatively more expensive, firms might substitute these workers with those who became relatively cheaper; in this sense, the employment effects of minimum wages might well extend to categories of workers for whom the MW does not bind directly. The minimum wage could, therefore, affect the composition of the workforce. Looking at the literature on effects of minimum wages throughout the distribution in Brazil, Fajnzylber (2001) finds negative employment effects of the MW, while Lemos (2004) does not detect significant disemployment effects. While Lemos (2004) looks at overall employment, though, Fajnzylber (2001) finds that the disemployment effects are mostly concentrated among workers earning around the minimum wage. Montenegro and Pages (2004) have an explicit analysis of the potentially heterogeneous effects of the minimum wage in Chile, exploiting the fact that minimum wages in that country vary by age of employee. These authors find that minimum wages reduce the employment rates of youth and the unskilled and raise those of older and skilled workers. In Mexico, Feliciano (1998) finds adverse employment effects of the MW for females of all ages, and positive employment effects for males ages 55 to 64. Suryahadi et al. (2003) find that in Indonesia the MW negatively affected the employment of blue-collar

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<sup>14</sup> In the case of Costa Rica, multiple minimum wages are set by law for workers in different industries and occupations. Therefore, as noted by Gindling and Terrell, for this country it is not entirely appropriate to talk about "spillover" effects of the MW.

workers (which the MW makes relatively more expensive) and positively affected that of white-collar workers.

#### *Informal Employment and Wages*

In developing countries, working in the informal sector (and, in general, in sectors not covered by MW laws) is the most obvious alternative to working in formal employment. Because informality may be an attempt by firms and workers to avoid MW laws, researchers have explored whether higher minimum wages induce shifts across sectors of the economy. The exercise is important also because most poor workers in developing countries are not employed in formal jobs (Lemos, 2004). Most studies that looked at the impact of MW on wages and employment in the informal sectors did find such effects, although not all papers have found effects with the same sign. Maloney and Nunez-Mendez (2004) find that higher MW has a positive impact on employment for male, full-time self-employed in Colombia. Gindling and Terrell (2007a), instead, find no effect of MW on informal sector employment in Costa Rica, where the informal sector is defined as the self-employed plus unpaid family workers. Fajnzylber (2001) finds positive effects of MW on wages and negative employment effects for salaried informal<sup>15</sup> workers as well as for the self-employed in Brazil. Interestingly, in Fajnzylber (2001) the disemployment effects in the informal sector were stronger than those in the formal sector. The author suggests that this might be due to workers exiting informality in the hopes of getting a higher-paying job in the formal sector. The varied findings highlighted above may be due to different definitions of “informal sector” across papers, as well as to country-specific circumstances that make cross-country comparisons difficult.

#### *Public Sector Effects*

A limited number of studies have examined effects of the MW on public sector wages and employment. Because of the (in principle) less stringent budget constraint of public sector employers, labor demand in this sector should be less sensitive to wage changes, and one would expect weaker employment effects of MW increases in the public sector more than in the private sector. This is consistent with the findings of Gindling and Terrell (2007b) in the context of Honduras. Lemos (2007), by contrast, finds that in Brazil the employment responses to changes in the minimum wage are very similar in the public and the private sectors.

#### *Econometric Challenges in Existing Literature*

Most of the studies mentioned above identify the effect of the MW through time and/or geographic variation. We see at least three identification problems in the literature. First, one of

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<sup>15</sup> In Brazil, informal salaried workers are those workers who report working without a signed work contract (Fajnzylber, 2001).

the most used indicators of the importance of the minimum wage (used as right-hand side variable in the regressions) is  $MW/W$  where  $W$  is typically the average wage in the economy (or for a particular category of workers). The problem with using such a measure is that the average wage,  $W$ , is potentially endogenous, i.e., itself affected by the  $MW$ . This problem is exacerbated when an economy has a single minimum wage in place so that the cross-sectional variation only comes from differences in  $W$  across categories of workers or geographic regions. On the other hand, the problem is much less severe when the variation comes directly from different  $MW$ s applying to different types of workers. This was the case of Montenegro and Pages (2004), for instance, whose analysis of Chile took advantage of different minimum wages by age. Similarly, in their analysis of Costa Rica, Gindling and Terrell (2007a) lessen the endogeneity problem by exploiting the extraordinarily rich structure of minimum wage provisions in this country. They use the fact that minimum wages vary widely by industry and occupation and are subject to relatively frequent changes over time in their identification strategy. One further advantage of multiple minimum wages is that these enable evaluation of the extent to which different models of the labor market characterize the markets for the labor services of different types of workers. As briefly discussed in the previous section, a higher minimum wage is expected to reduce employment if the labor market is perfectly competitive, but it might only result in redistribution of the surplus in labor markets characterized by search frictions and rent sharing. Gindling and Terrell (2007a) find substantial negative effects of the minimum wages on employment and hours worked in the covered sector. The fact that the effects were larger at the bottom of the skill distribution suggests that a competitive model might best describe the labor market for low-skill individuals, whereas frictions and surplus-sharing might be more important for high-skill jobs.

A second possible source of endogeneity is that the  $MW$  might be increased in response to political or electoral concerns. Results from Lemos (2005), however, suggest that this concern might not be that relevant in practice, at least in the Brazilian context. Lemos (2005) instruments the minimum wage with a series of political variables ranging from the personal characteristics of politicians passing minimum wages bills to their voting behavior and the electoral process. Her findings show the IV estimates to be very similar to the OLS estimates. Subject to the validity of Lemos' chosen instruments, this is an indication that changes in the  $MW$  are fairly exogenous to labor market conditions, at least in one country in which relatively strong effects of the minimum wage have been found.

Finally, most studies are non-experimental in nature. Recently, some authors have made efforts to identify treatment and control groups, thus creating a quasi-experimental research design. In their analysis of the Indonesian experience, Alatas and Cameron (2003) implement a

before-after technique with a careful selection of treatment and control groups. In the first half of the 1990s, the minimum wage in Indonesia was doubled. In addition to the magnitude of the increase, two other factors make the Indonesian experience particularly interesting. First, there was considerable heterogeneity across Indonesian provinces before the policy change, with minimum wages tending to converge after the change. Second, this change was prompted by pressure from international trade organizations, an indication that it was reasonably exogenous to internal economic and political conditions. Alatas and Cameron (2003) focus on two neighboring regions, Jakarta and West Java, which are similar in many respects but which differed in the pre-reform level of the minimum wage. The authors use the policy change and regional variation to implement a matched difference-in-differences specification to identify the employment effect of the minimum wage, using longitudinal data on all Indonesian firms with more than 20 employees in the clothing, textile, leather, and footwear industries. They find some evidence of disemployment effects in small, domestic firms but not in large firms (foreign or domestic). The Indonesian case also was explored by Rama (2001), who found small employment effects of the MW overall, with positive effects in large firms and negative effects for firms with fewer than 20 employees. The Rama (2001) and Alatas and Cameron (2003) results cannot be directly compared, since the data used by Alatas and Cameron are missing the entire left tail of the firm size distribution (firms with fewer than 20 employees) and are limited to the manufacturing sector. However, these results underscore the importance of corroboration of results generated by different data and methods of analysis.

#### *Poverty and Welfare Effects*

Minimum wages are often introduced with the explicitly stated goal of reducing poverty. In practice, as we have seen, empirical work has concentrated on analyzing the effects of MWs on earnings and unemployment. Implicitly, positive effects on the earnings of the poor are considered a success toward poverty reduction, and negative effects on unemployment are seen as an indication that the MW might increase poverty. In theory, though, the relationship between minimum wages, unemployment, and poverty is not so straightforward, especially when models depart from the standard framework to incorporate more realistic features of developing economies.

On the theory side, Fields and Kanbur (2007) built a model to explicitly evaluate the impact of the MW on poverty. The model incorporates income-sharing between family members, a very typical feature of developing societies (Angelucci and DeGiorgi, 2008). In the Fields and Kanbur model, the effect of the minimum wage on poverty depends on four key parameters: the level of the minimum wage with respect to the poverty line, the elasticity of labor demand, the

extent of income-sharing taking place within households, and, finally, the degree of poverty-aversion. This last parameter is a taste parameter, to reflect the fact that some societies might be more or less averse to poverty than others. Thus, even for poverty the question of the effect of MW is an empirical issue.

The available empirical evidence on the effects of the minimum wage on poverty and welfare is scant and displays somewhat mixed results. Using cross-sectional data from several countries in Latin America, Morley (1995) finds a negative correlation between minimum wages and poverty. Using longitudinal data for a larger sample of countries (from Latin America, Asia, and Africa), Lustig and McLeod (1997) also find that higher minimum wages are associated with less poverty. On the other hand, the analysis of the Brazilian case by Neumark, Cunningham, and Siga (2006) finds no evidence of positive effects of the minimum wage on poverty and income inequality, and Gindling and Terrell (2007b) find that minimum wage increases lead to only modest poverty reduction in Honduras.

One possible way minimum wages could contribute to reduce poverty is suggested by efficiency-wage-type models. In very low-income countries, where malnutrition of workers is often an issue, higher wages might allow workers more and better food, which could increase their productivity. Azam (1997) considers the impact of minimum wages in the context of the agricultural sector in Morocco, a sector where – he argues – minimum wages were better enforced in that country compared to other industries.<sup>16</sup> Because of the lack of data on agricultural employment, Azam examines the effects of the minimum wage on output, finding a positive effect of minimum wages. As the author recognizes in the paper, however, this result could be consistent with both positive and negative effects on employment levels. Azam offers an interesting interpretation of his results based on an efficiency-wage model where the productivity of laborers depends on how much they consume, and where wage income is shared among household members to fund consumption.

#### *Short-Run versus Long-Run Effects*

The paucity of research explicitly looking at the relationship between minimum wages and poverty or inequality in developing countries is exacerbated by a focus on short-run effects. In our view, this is a serious shortcoming of this literature, especially when it comes to evaluating the impact of MW on inequality. While traditional empirical papers have typically limited themselves to the estimation of average effects, in recent years economists have been much more

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<sup>16</sup> Another paper to study the experience of a poor, African country is Andalon and Pages (2008). Contrary to the Moroccan case, Andalon and Pages find that in Kenya minimum wages are better enforced in the non-agricultural sectors.

interested in distributional effects of policies and the potential heterogeneous responses to policies. Even among those papers that do explore the distributional consequences of such policies as minimum wages or unemployment benefits, however, the focus is almost invariably on short-term effects and/or cross-sectional inequality. Studies of earnings inequality often analyze dispersion of wages for people who work, at one point in time, or they compare point-in-time measures of inequality between periods. When countries are ranked, a greater degree of social welfare is associated with more equal earnings distributions. It can be argued, however, that cross-sectional inequality is not necessarily the right metric by which to evaluate the distributional effects of a policy. Consider the following quote from Milton Friedman (1962, p. 171):

*A major problem interpreting evidence on the distribution of income is the need to distinguish two basically different kinds of inequality: temporary, short-run differences in income, and differences in long-run income status. Consider two societies that have the same distribution of annual income. In one there is great mobility and change so that the position of particular families in the income hierarchy varies widely from year to year. In the other, there is great rigidity so that each family stays in the same position year after year. Clearly, in any meaningful sense, the second would be the more unequal society. The one kind of inequality is a sign of dynamic change, social mobility, equality of opportunity; the other, of a status society.*

Comparisons of inequality measures based on cross-sectional observations can be argued to be meaningful only to a limited extent. One reason is that some labor market institutions, such as minimum wages (or centralized wage bargaining), might improve the welfare of some groups in the population at the expense of others, who then bear the cost of a policy in the form of unemployment. Second, cross-sectional inequality is static in nature, measuring the degree of dispersion of earnings at a point in time. Comparisons of cross-sectional inequality across countries implicitly assume stationarity, i.e., a situation in which the units of observation (individuals or households) keep their relative position in the income parade forever. One way to analyze inequality in a dynamic perspective would be to measure and compare lifetime welfare inequality or, at least, lifetime earnings inequality.

In a recent paper, Christopher Flinn (2002) compares cross-sectional inequality to lifetime inequality in Italy and the United States. It is a commonly held preconception that the former country is more inefficient but egalitarian, while the second is more efficient but much less egalitarian. Flinn estimates a structural off-the-job and on-the-job search model separately for

the two countries, and then uses his parameter estimates to simulate labor market careers and compute measures of lifetime welfare and lifetime welfare inequality. He finds that although cross-sectional inequality in the distribution of wages is much higher in the U.S. than in Italy, this is no longer true when the distribution of lifetime welfare is concerned. Because the U.S. labor market is more dynamic than the Italian labor market (e.g., higher rates of job destruction but also higher rates of job creation and job-to-job transitions), individuals have more opportunity to change their position in the distribution of earnings. Macis (2007) applies Flinn's methodology to the North and the South of Italy, finding similar results.

Within the literature of lifetime welfare and income analysis, there is some evidence from the United States that suggests that the effects of minimum wages might not just be temporary. Smith and Vavrichek (1992) examine the earnings mobility of workers employed at the minimum wage. Their results indicate that although over 60% of U.S. workers who were earning the minimum wage in the mid-1980s were earning wages 20% higher one year later, a significant minority of unskilled workers failed to advance beyond the minimum wage. Carrington and Fallick (2001) find that about 10% of workers (mostly women, minorities, and less educated) spend at least half of their first 10 years in the labor market working minimum or near-minimum wage jobs.

Neumark and Nizalova (2007) explicitly attempt to quantify the long-run effects of minimum wages, exploring the possibility that exposure to high minimum wages at young ages, when individuals are first entering the labor market, might have longer-run effects. This could be the case, for instance, if minimum wages lead to decreased labor market experience and accumulation of tenure and diminished on-the-job training and skill acquisition. The authors also acknowledge the theoretical possibility of beneficial long-run effects of minimum wages, which could arise, for instance, if minimum wages have positive effects on the acquisition of skills, or if they lead to long-lasting rather than short-term wage increases. Neumark and Nizalova (2007) find evidence of long-term adverse consequences of minimum wages, especially among minorities. Their empirical strategy exploits geographic and time variation in minimum wages across U.S. states and ensuing variation in the exposure to higher-than-federal minimum wages at different ages. In addition to finding positive wage effects and negative employment effects of contemporaneous minimum wages for teenagers, Neumark and Nizalova find that having been exposed to high minimum wages between the ages of 16 and 19 leads to adverse wage and employment effects later in life. Effects of previous exposure are found to be stronger for African Americans and for individuals who did not complete high school.

*Interactions of the MW with Other Institutions*



The impact of the minimum wage also might depend on what other labor market institutions are in place in a specific context (Coe and Snower, 1997). There is some evidence that minimum wages interact with other institutions in significant ways. In a cross-country, longitudinal setting, Neumark and Wascher (2004) find that minimum wages have more adverse effects when labor standards (legislated working time rules, worker representation rights, and restrictions on the use of temporary workers) are more restrictive, and fewer adverse effects when employment protection is high. Interestingly, they also find that negative employment effects of minimum wages are reduced in countries where minimum wages are set by some type of national collective bargaining process, rather than unilaterally by the government. The authors interpret this evidence as being consistent with the argument that the collective bargaining process internalizes potential disemployment effects when setting minimum wages.

The effects of an increased minimum wage can in principle be mitigated by a reduction in the non-cash parts of compensation (Wessels, 1980). However, the presence of a minimum wage also might exacerbate the adverse employment consequences of regulations requiring employers to provide certain benefits to workers (e.g., health insurance). We are not aware of any study that tackles the issue of how non-cash compensation responds to increases in minimum wages. As mentioned above, one way employers could respond to an increased minimum wage is by reducing other monetary or in-kind components of compensation such as pension benefits, health benefits, training, paid vacation days, and – in developing countries – meals, clothing, and lodging. Recent developments in the construction of detailed employer-employee datasets might help researchers look into the “black box” of firms’ compensation practices so as to evaluate the degree of substitutability of wage with non-wage elements of compensation (Lazear, 2000).

In spite of the abundant literature on the wage and employment effects of minimum wages, there is essentially no evidence of whether “minimum wage” jobs in developing countries are significantly worse jobs in terms of non-cash compensation, on-the-job training, or career prospects (wage growth, etc.). With regard to training, Acemoglu and Pischke (2002) nicely illustrate the two forces that could be at play here. On the one hand, consistent with Gary Becker’s human capital theory, minimum wages should reduce training investments because they prevent workers from taking the wage cuts necessary to finance the provision of training. On the other hand, though, in labor markets with frictions, minimum wages could induce employers to provide *more* training to raise the productivity of their least skilled workers. Empirically, Acemoglu and Pischke (2002) find very small effects of MWs on training for low wage workers in the United States.

*Structural Models*

As we have seen, the empirical literature on the effects of minimum wages delivers conflicting results. Further, disagreement exists among economists even in the interpretation of the same results. One reason behind these ambiguities is that different authors often have different implicit objective functions in mind. A very small body of empirical research on minimum wages takes a more structural approach, as opposed to the “reduced form” approach taken by the vast majority of papers on this topic. The advantages of a structural approach include that the mechanisms through which policy interventions affect behavior can be explicitly modeled and that general equilibrium effects can be explicitly accounted for. Structural models also typically explicitly define objective functions. Further, and perhaps most important, by estimating deep behavioral parameters, structural models allow for constructing counterfactuals, which can then be used to evaluate the effects of a policy and to extrapolate those effects to other environments (Heckman, 2000).

One of the most notable examples of a structural approach to the analysis of minimum wages is Flinn (2006). This paper builds a search and matching model, estimates its structural parameters, and uses the estimates to determine the unemployment and welfare effects of the minimum wage. This structural approach allows one to estimate the total (workers and employers) welfare effect of a policy, which is arguably the most relevant metric to evaluate a policy intervention. In Flinn’s model, search frictions generate a surplus which is then split between the worker and the firm via Nash bargaining. One feature of this class of models is that the equilibrium is efficient (welfare-maximizing) if the workers’ share of the surplus is equal to the elasticity of the matching function with respect to the size of the set of job searchers (Hosios, 1990). In this setting, if workers receive a share of the surplus which is too low, a minimum wage can be used as a tool to raise workers’ bargaining strength, increase their search activity, and bring the economy closer to the efficient outcome. Flinn also examines the effects of the minimum wage on labor force participation rates, which have, surprisingly, been largely neglected by the literature. In theory, the effect of the MW on participation depends on how the minimum wage affects the value of search, and that in turn depends on how the minimum wages impact firms’ incentives to create jobs. Using CPS data on accepted wages and unemployment durations, Flinn thus estimates workers’ bargaining power and the elasticity of the matching function, and then compares the two to determine whether the current level of the minimum wage is optimal in the sense of maximizing a social welfare function. Flinn finds that in the U.S., workers’ bargaining power is equal to about 0.4, which indicates that U.S. workers are at a

disadvantage in sharing surplus.<sup>17</sup> Flinn's empirical results show that a positive minimum wage can improve welfare compared to a zero minimum wage.<sup>18</sup>

#### *Alternatives to the Minimum Wage: Wage and Employment Subsidies*

Wage or employment subsidies are often proposed as an alternative to minimum wages as a means of sustaining the income of the least skilled individuals while at the same time encouraging their participation in the labor market and employment (IADB, 1998). Subsidies can be general, or targeted to specific groups (e.g., females, workers in disadvantaged regions). What do we know about whether employment subsidies work? While in the United States, "policies combining wage subsidies with job development, training, and job search assistance efforts appear to have been somewhat successful in improving the employment and earnings of specific targeted disadvantaged groups" (Katz, 1998), the evidence from developing countries is scarce, the outcomes mixed. Once again, the effects of the policies in question depend crucially on the context. The presence of an informal sector makes the analysis less straightforward than what it would be without, while the presence of other institutions, e.g., minimum wages, also affects the predicted impact of employment subsidies. In a recent paper, Betcherman, Daysal, and Pagés (2007) analyze the effects of employment subsidies offered by the Turkish government on formal employment, earnings, and formally registered establishments. The authors also provide an estimate of the deadweight loss of subsidies, i.e., the share of subsidized employment that would have been created even in the absence of the incentive. Exploiting the progressive expansion over time and space of the subsidy program, the authors implement a difference-in-differences methodology. Their findings indicate that registered employment increased by 4% to 15% as a result of the subsidy, depending on the specification adopted and the program analyzed (two subsidy programs were evaluated, which differed both in terms of the level of the subsidy and in eligibility requirements). The authors recognize that positive effects on formal employment in a context with a substantial informal sector might not necessarily indicate actual employment gains. Betcherman, Daysal, and Pagés find no change in energy consumption associated with the subsidy, which they interpret to mean that no *actual* change in employment occurred, but that employees may have transitioned from informal to formal status within firms. Although better

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<sup>17</sup> Other papers estimating bargaining power obtain even lower estimates (Cahuc, Postel-Vinay, and Robin, 2006) and Dey and Flinn (2005).

<sup>18</sup> The optimal level of the minimum wage, however, differed depending on whether a partial equilibrium or a general equilibrium analysis was conducted. In the partial equilibrium case, it is assumed that the MW does not affect the rate at which unemployed job searchers contact employers. In the general equilibrium case, instead, the "contact rate" could be affected by the minimum wage.

data would be needed to properly assess this conclusion,<sup>19</sup> the findings of Betcherman, Daysal, and Pagés support the notion that in countries with weak enforcement, high taxes on labor may provide a strong incentive for firms and workers to operate informally. Even if the employment gains were genuine, however, the estimates of this paper indicate that the cost of the program was also substantial, due to considerable deadweight losses. In particular, the authors estimate that between 25% and 80% of the subsidized jobs would have been created even without the program. This study thus illustrates that the estimated deadweight loss of the program crucially depends on the program design. In particular, when the subsidy was restricted to new firms employing at least 30 workers and to existing firms that increased their workforce by at least 20%, the deadweight loss dropped from a range of 47% to 78% to a range of 23% to 44%.

### *Concluding Thoughts*

The empirical literature on the effects of minimum wages in developing countries delivers a complex picture, so it is difficult to identify general patterns. We have discussed the areas in which common findings exist, and drawn attention to those areas that still appear to lack cohesive results. Based on our review of the literature, we also have identified several shortcomings of existing research and areas where we believe future research could be more fruitful, which we detail below.

First, the vast majority of minimum wage papers have focused on the effects on employment, while generally neglecting other margins such as hours and labor force participation decisions. A higher minimum wage makes working for a wage relatively more attractive with respect to, say, home production, and therefore could increase participation in the labor force by individuals who would otherwise stay out (e.g., females). Literature on effects of MW on household labor supply (e.g., child labor) is also limited.

Another limitation of the literature is that it overlooks one rather obvious possible way that employers and workers have to lessen the effects of MWs: substituting cash for non-cash compensation. There is essentially no evidence on whether “minimum wage” jobs in developing countries are significantly worse jobs in terms of safety conditions, non-cash compensation, on-the-job training, or career prospects (wage growth, etc.) or, as is relatively common in developing countries, food, clothing, and lodging.<sup>20</sup>

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<sup>19</sup> The validity of this interpretation would have been strengthened had the authors been able to control for changes in firms’ utilization of inputs other than labor. Data limitations prevented them from doing so.

<sup>20</sup> As we have seen, the evidence from developed economies is also scant. Exceptions are Smith and Vavricheck (1992) and Carrington and Fallick (2001).

We also were surprised to find that there are few studies which look directly at the impact of minimum wages on poverty, and that those that do tend to take a short-term approach rather than assessing whether MWs are lifting poor workers and their families permanently out of poverty.

### **3. Mandated Benefits**

The literature exploring the effects of non-wage mandated benefits as a distinct issue from taxes first drew a general audience in economics in 1989 with Lawrence Summers' publication in the *American Economic Review*, called "Some Simple Economics of Mandated Benefits." This paper, which followed up on Frank McArdle and Uwe Reinhardt's 1987<sup>21</sup> papers for the Employee Benefit Research Institute, claims that mandated benefit programs are "more efficient but less equitable than standard public programs." Summers explains the existence of both mandated benefits and government-funded programs as stemming from government paternalism, due to bounded rationality of citizens, the existence of externalities, and/or adverse selection effects on firm survival if only some firms provide such benefits. Researchers of mandated benefits also have cited possible welfare improvements as compared to government programs stemming from: the ability of firms to tailor benefits to the needs of employees, avoidance of inefficiencies due to the political nature of budgeting government programs, and the direct effects on labor supply from employees' valuation of benefits as part of the compensation package.<sup>22</sup>

This chapter presents the basic theory behind economists' understanding of the effects of mandated benefits, followed by an empirical section including both short summaries of the empirical findings in developed countries and more in-depth analyses of applied work in developing countries.

#### *Theoretical Framework*

There are two major types of mandated benefits considered in the literature: those that vary with hours, acting like additional wages or taxes (variable costs), and those that impose a fixed cost of employment per worker.<sup>23</sup> The former, and most commonly studied, can encompass

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<sup>21</sup> Cited in Summers, 1989.

<sup>22</sup> Summers, 1989; Mitchell, 1990; Hamermesh, 1993

<sup>23</sup> Hamermesh, 1993, p. 46

mandated bonuses, vacation pay, maternity leave, or disability programs, which are funded by an employer regardless of use, and directly linked to wages or hours worked.<sup>24</sup> Within the variable cost category of benefits, there is a distinction between benefits that cost a percentage of the worker's wage, and those that cost a fixed amount per worker hour.<sup>25</sup> We discuss the "fixed-cost per worker-hour" case below, but the percentage case is a simple extension that results in an asymmetric shift of the labor demand curve, whereas the worker-hours case shifts the labor demand curve to a position parallel from the original curve. Within the fixed-cost-per-worker category, recurrent examples include job guarantees to parents returning to their jobs from parental leave, holidays and sick leave, health insurance, and some pension programs; one-time fixed costs might include severance pay or training.<sup>26</sup>

Models of the effects of mandated benefits in the variable cost category closely mimic those discussing required government benefits funded by payroll taxes: Any benefit which tends to impose an additional cost in proportion to the amount of labor used acts as a tax on employment. However, it is important to note an oft-missed distinction between general payroll taxes and costs of benefits: Workers may better understand that contributions for benefits make up part of their compensation package. Their valuation of these benefits thus may be more likely to affect labor supply, in addition to affecting labor demand conditions.

In a standard static model of the labor market, such mandated benefits contributions will shift the labor demand curve downward by the amount of the contribution, resulting in a lower direct wage and lower employment in equilibrium. Moving from point  $(L, w)$  to point  $(L', w')$  in figure 1 shows the effect of imposition of a mandated benefit in the market, in the case where the employee values the benefit at less than its cost to the employer. The standard predicted effect of a mandated benefit in this situation is a decrease in employment and a drop in wages. Note also that, on the supply side, if workers value benefits at less than the cost of employer contributions, they will have an incentive to seek out non-complying work contracts. If the expected value of penalties to firms of offering such work contracts is sufficiently low, profit-maximizing firms may prefer to employ such workers and capture some of the resultant rents. Thus, the "formal" sector might experience very different effects of mandated benefits than the "informal" sector, and analysis of official employment data may miss the employment, wage, or efficiency effects of such policies.

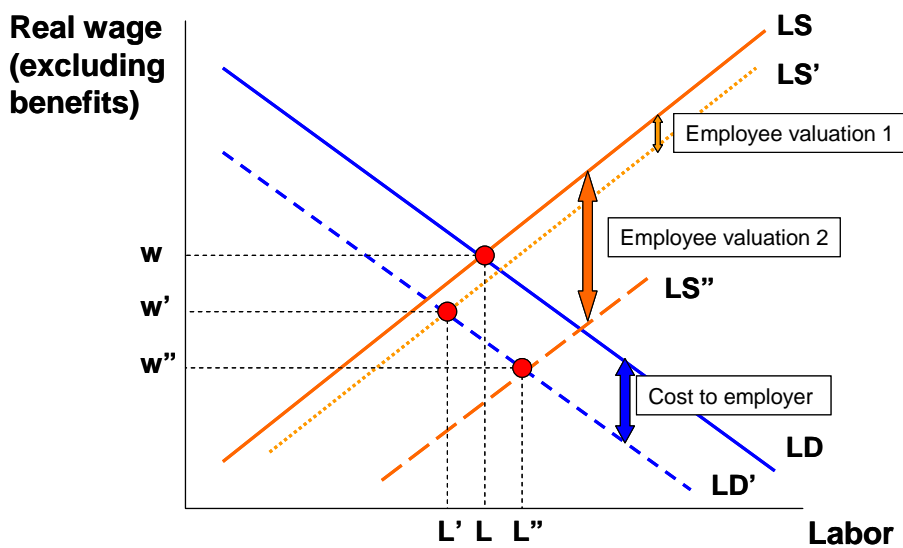
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<sup>24</sup> Hamermesh points out that, at least in the United States, many programs have low earnings ceilings, above which contributions are a fixed cost, no longer varying with hours or wage. Some benefits may therefore impose a variable cost for some workers, but a fixed cost for others. Differential effects on labor demand for workers of different wage-levels results.

<sup>25</sup> Mitchell, 1990

<sup>26</sup> Hamermesh, 1993

If a benefit is valued by the worker at or above its cost, however, the effects of a mandated benefit on the labor market are less clearly negative: The direct wage is still expected to fall, but the employment effect can be zero or positive (see point  $(L'', w'')$  in figure 1), due to increased labor supply.<sup>27</sup> The relative size of the wage effect compared to the employment effect clearly depends on the relative elasticities of the curves; in fact, it is important to note that an empirical finding of wage-shifting and zero employment effects could be due to high employee valuation *or* to a highly or perfectly inelastic labor supply curve. In the former case, there would be no deadweight loss; in the latter, employees would bear the full deadweight loss.

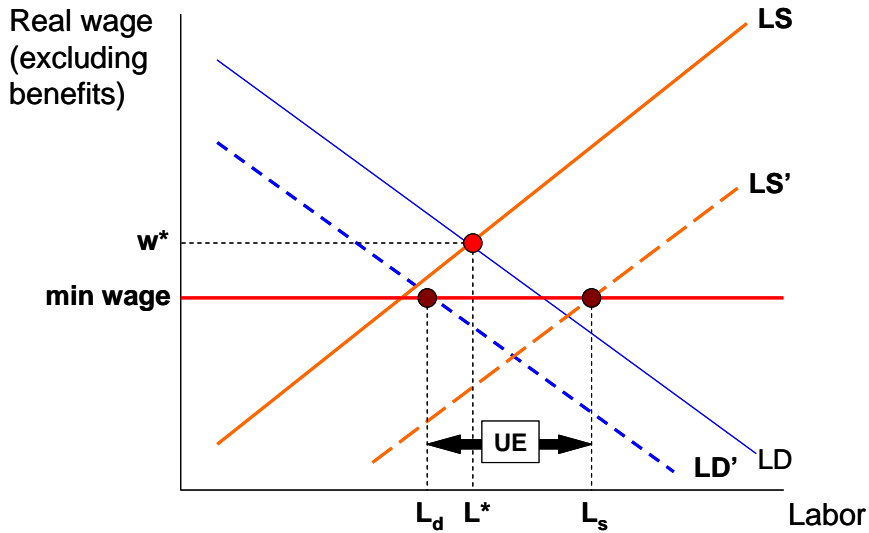


**Figure 1: Constant hourly cost of mandated benefit, no binding minimum wage.** Employee valuation 1 indicates labor supply effects with low valuation of the benefit. Employee valuation 2 represents valuation of benefits above the cost to the employer. Point  $(L, w)$  represents equilibrium employment before a benefit is mandated. Point  $(L', w')$  indicates a possible equilibrium under low employee valuation, and point  $(L'', w'')$  shows a possible outcome if a mandated benefit is highly valued by workers.

Under this standard model, the existence of a binding minimum wage will result in exacerbated unemployment in the covered sector, since law-abiding employers will reduce employment if reducing wages would drop them below the legal minimum. See figure 2 below for an illustration of a mandated benefit that, while highly valued, creates or contributes to

<sup>27</sup> Empirical evidence of such worker valuations of mandated benefits has, for example, been presented by Baker and Milligan, 2005.

significant unemployment. This case of a binding minimum wage is particularly salient in developing countries where *de jure* minimum wages apply to a large portion of the workforce.



**Figure 2: Cost of mandated benefit proportional to hours worked, with minimum wage.** In this illustration, imposition of a mandated benefit may cause a minimum wage to bind (or exacerbate the effects thereof in cases where it already binds), causing unemployment even if a benefit is highly valued.

The above discussion focused on a single labor market with standard labor supply and demand curves and assumed homogeneous workers. Relaxing the assumption of perfectly competitive markets in this model, also a consideration in developing country literature, can result in workers capturing more rent through benefits.<sup>28</sup> De la Rica and Lemieux (1994) provide a more rigorous model of such decisions.

Next, consider an industry in which there are higher-skill, high-productivity workers who can be substituted (especially in the long run) for lower-skill workers. The cost of benefits (if linked to hours or employment and not to wage) will make lower-skill workers less attractive to employers because their payroll costs increase by a higher percentage than the payroll costs of higher-skill workers. This will result in substitution away from lower skill workers by compliant firms, with the disemployment effect for lower skill workers likely increasing over time. Thus,

<sup>28</sup> De la Rica and Lemieux, 1994



the group toward which a mandated benefit may have been intended may actually be harmed in the longer run.<sup>29</sup>

When mandates involve a fixed cost (lump sum) per worker, the above substitution from low-wage to high-wage workers may still take place, but employers also will have a general incentive to reduce the labor force, and to increase hours and overtime of remaining employees.<sup>30</sup> Rabin-Margoloth (2002) emphasizes that increased demand for labor in the uncovered sector (younger workers, workers in smaller firms,<sup>31</sup> part-time and temporary employees, etc.) is another predicted effect of such mandates. Examples of such fixed-cost benefits include severance payments, unpaid medical leave, health insurance, or mandatory advanced notification of plant closures in the United States.<sup>32</sup>

Furthermore, differential applicability of mandates to firms, for example, by size, is predicted in a theoretical model by Anderson and Meyer (1996) to lead to distortions in allocation of employment across firms and deadweight losses *even* when a large portion of benefit costs are passed through to wages.

While the above theoretical sketch has assumed mandated benefits that are privately administered, many programs are administered by governments. The sketches above still apply to the case of government-administered benefits, with two important caveats. First, the effects of government-administered benefits on employees' labor supply depend on the extent of linkage of benefits to employment and/or wages. If some level of benefits can be obtained by anyone, or through a worker's spouse or parents, that benefit will not be valued as highly, if at all, by workers in the labor supply decision. Second, worker valuation of benefits depends on their assessment of the likelihood of receiving the benefit. In the case of government-administered benefits, it comes as no surprise that some workers do not believe the benefits will be there when they need them, or that some feel they would be better served investing the money for themselves.

### ***Empirical Evidence***

The effects of many types of mandated benefits on labor markets have not been studied systematically in the case of developing countries. Therefore, the following section includes studies on developed countries when no similar work has been written in the context of developing countries. The empirical work on developing countries that does exist tends to focus

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<sup>29</sup> Mitchell, 1990

<sup>30</sup> Hamermesh, 1993

<sup>31</sup> Anderson and Meyer, 1997

<sup>32</sup> Rabin-Margoloth, 2002

on non-wage pay, Social Security payments or maternity benefits; work on developed countries (primarily the U.S.), additionally, includes a vast literature on the effects of mandated health coverage, as well as some work on unemployment insurance.

### *General Studies of Mandated Benefits*

As in many other veins of empirical work, many of the first studies in this area initially used aggregated time-series data or data in cross-section only. The estimates of wage and employment effects of policies in these studies were ambiguous, which recent researchers have attributed to omitted variable biases, measurement error, and simultaneity biases (Gruber, 1997; Kugler and Kugler, 2002), and attempted to solve by using individual country panel datasets. Of course, given the difficulty of obtaining high-quality data that capture within-country variation in the effects of benefits, cross-country studies have not entirely fallen out of favor in the last decade: An ambitious study by Forteza and Rama (1999) uses cross-country data for 100 countries to estimate the effects of labor market rigidity on growth before versus after reforms. The authors find significant negative effects of an aggregate rigidity index, but among the components of the index, mandated benefits and minimum wages are shown not to slow growth in fixed effects and random effects regressions immediately before reforms.<sup>33</sup>

### *Payroll Taxation for Unemployment Insurance and Social Security Benefits*

Payroll taxation for employee benefits has been more extensively studied than mandated benefits that are not administered by the government. Such studies have been included in this chapter due to the paucity of studies on mandated benefits because, as discussed above, theory suggests that payroll taxes and mandated benefits will have similar effects in the labor market. If employees attach less value to government-provided benefits than to privately administered mandated benefits, the static model predicts more disemployment effects from the government programs. There also may be efficiency benefits to non-government programs, which may further skew the effects of government benefits toward disemployment, compared to non-government-administered benefits (Summers, 1989).<sup>34</sup>

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<sup>33</sup> Unionization (-2% to -5%) and government expenditures (0 to -7.3%), however, are associated with slower growth before reforms, and slower growth after. The authors attribute these results to possible delays and changes in substance of reforms due to lobbying from these groups.

<sup>34</sup> For example, privately administered benefits may be better tailored to workers' needs or desires if firms and workers can negotiate the desired coverage, subject to a government-mandated minimum (Summers, 1989).

In 1991, Gruber and Krueger completed one of the early studies attempting to measure the effects of payroll taxes with differential applicability within a country and over time, finding almost full wage shifting and little disemployment effect of workers' compensation insurance payments in the United States. Anderson and Meyer (1997) later investigated the effects of taxes that vary at the firm level through the example of unemployment insurance in the United States, which is experience-rated. They report that, while market-level tax is largely borne by workers, firm-level variation in taxes appears to be borne by firms, both in terms of wages and employment. The authors emphasize that employment reallocation across firms and micro-level deadweight losses may therefore exist even if no aggregate employment effect is apparent. This is an important idea, and one that underscores the desirability of careful firm-level studies.

On developing countries, two careful studies have found diametrically opposite effects of Social Security taxes. Gruber (1997) studies the effects of a large *decrease* in payroll taxes after a Social Security reform in Chile in 1981. Using several different estimation methods, including differences-in-differences and IV estimation strategies, Gruber attempts to mitigate bias from measurement error in wage,<sup>35</sup> which is used as the dependant variable and also as a denominator in the right-hand-side variable of interest, the firm's tax burden. This paper is one of the earlier careful studies on payroll taxation or mandated benefits in a developing country. The author points out that even his preferred estimates are still probably biased, but most estimates in this study imply a high percentage of wage shifting, and no discernible employment effects.

Potential weaknesses in this study are that it only tests for long differences, which may mask transitional employment effects; also, it only examines a recessionary period, during which real wages dropped in general. Probably the most worrisome feature of this study is that average wage is the dependent variable, and a denominator for a right-hand-side variable; despite attempts to address this issue, the magnitudes of some coefficients remain implausible. (Because the average wage is imputed, measurement error in wages is likely to bias the findings *toward* a finding of full wage-shifting.) The author uses the zero coefficients on the effect of the tax on employment as a dependant variable as support for his full wage-shifting findings, but this would only imply full wage-shifting if the standard, stationary model adequately represents the true labor market along the dimensions studied, and if the variation in wages upon which identification depends is only from the effects of the change in payroll taxation or benefits.

Kugler and Kugler (2002) set out to study the effects of an *increase* in payroll taxes for Social Security funding. They are motivated by Gruber's surprising findings and the likely asymmetries in the effects of changes in taxes on the labor market due to the downward rigidity

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<sup>35</sup> He uses average plant-level wages, but these data were not reliably pre- or post- tax.

of wages and employment associated with minimum wages and the relative difficulty of firing versus hiring. Using similar estimation methods to Gruber, and a panel dataset of manufacturing plants in Colombia that includes some ability to estimate variation in tax rates and compliance across plants and industries, their preferred estimates imply a low extent of wage shifting: a 2.4% drop in wages as a result of a 10% increase in payroll taxes, and strong disemployment effects: a 4.5% to 4.9% drop in employment for a 10% increase in taxes. A particular strength of this study is that it examines effects both in expansionary and recessionary periods, finding similar results in each. It is important to note that this study suffers from the same bias due to wage imputation as the Gruber study, so estimated effects of payroll taxation on wages may be biased *toward* a finding of wage-shifting (which is estimated to be quite low anyway). Thus, the estimated effects between the Gruber (1997) and Kugler and Kugler (2002) studies are at odds with one another. The differences in estimated effects on wages and employment could be due to the asymmetry of rigidity in labor markets, cross-country differences, or any manner of data or institutional issues, underscoring the need for more analysis of the effects of payroll taxation and government-administered benefits using developing country data.

#### *Pregnancy and Maternity Coverage*

Another focus of the existing mandated benefits literature has been maternity benefits, especially in developed countries. Gruber (1994) uses the natural experiment of differing implementation dates of state-level anti-discrimination in health insurance laws in the United States in the 1970s which forced health insurance to cover pregnancy and childbirth-related costs. He estimates the effect on wages, hours worked, and employment of married women of childbearing age via several empirical approaches, the most rigorous of which is a difference-in-difference-in-difference estimation scheme (control groups were other states and other demographic groups). Point estimates of the extent of wage shifting ranged from approximately 100% to as high as 214%; however, large standard errors made results only marginally statistically significant. Weekly hours worked for the most costly group (women 20-40) increased by 4.9%. Weaknesses of this work are the time period of the data (late 1970s), which seems problematic due to the macroeconomic conditions of the time (real wages were dropping), and that the estimation of the costs of these mandates were based on insurance data from 1990 (inflation-adjusted), over a decade after the dates of the relevant policy change.

More recent work by Baker and Milligan (2005) studies the effects of a one-time fixed cost to employers in the form of guaranteed time off and the right to return to the same or a similar job for new mothers. The predicted outcome of such a policy, according to the static

theory outlined previously, is substitution away from the costly group of workers on the side of employers. This substitution can be avoided if the expected cost of leave is mitigated through increased productivity or more firm-specific human capital accumulation (due, for example, to increased labor market attachment in anticipation of receipt of maternity benefits). Much previous empirical work on labor market effects of maternity leave in Europe, the U.S., and Canada has found increased labor force participation by women before childbirth, and increased rates of return to the same employer; some negative wage effects have been found.<sup>36</sup> Baker and Milligan used temporal and spatial (by province) variation in maternity leave law with the monthly Labour Force Study to confirm positive labor supply effects of maternity leave in the case of Canada, showing that even modest leave durations decrease quits and increase return rates to employers, despite increasing leave-taking.

Developing country evidence on labor market effects of maternity leave is scarce, but does exhibit some of the same findings. Zveglic and van der Meulen Rodgers (2003) use repeated cross-sectional household data on Taiwan from 1982 to 1989 to examine the effects of women's working hours restrictions and maternity benefits on labor market outcomes. They find no significant effect on women's wages as result of maternity benefits, either before or after enforcement. The laws themselves were found to have insignificant effects, having a statistically significant impact on labor market outcomes only *after* the laws began to be enforced. Using a difference-in-difference framework, working hours are estimated to have increased by 4.26% to 4.5% after enforcement, and employment is found to have increased by 2.48% to 2.76% for this group after enforcement. "Basic differences" results were similar. Many of the same weaknesses mentioned with respect to Gruber (1997) and Kugler and Kugler (2002) also extend to this study. Additionally, only a pseudo-panel was used, and the subsample of ages the authors used may have led to biases due to women in the "treatment" group aging past prime childbearing years by the end of the study window. Despite the weaknesses of this paper, it is one of only a handful of careful studies of mandated benefits in developing countries and represents a strong start.

*The "Thirteenth Salary," vacation pay, and other required bonuses*

In many countries – both more and less developed – Christmas bonuses, vacation pay at greater than 100% of the wage, and other required bonuses make up an important part of the wage structure. MacIsaac and Rama (1997) study the impact of Ecuador's 13<sup>th</sup>, 14<sup>th</sup>, 15<sup>th</sup>, and 16<sup>th</sup> salaries, plus the mandated cost-of-living, complementary, and transportation bonuses, which account for approximately 75% of the cost of employing a worker at the minimum wage. While

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<sup>36</sup> Baker and Milligan (2005) cite work by Waldfogel Cate, Winegarten, and Bracy and Ruhm.

there is no natural experiment in the form of a policy change through which to examine the impact of these payments, the authors use sectoral differences, unionization status, and formal employment status to identify the effects of different bonus pay amounts on wages and employment in Ecuador. Estimates show that the total compensation package averages just 18% more in private sector jobs complying with labor regulations than otherwise identical jobs, implying that the cost of mandated benefits is largely offset by a reduction in base wage, probably enabled by weak minimum wage enforcement. A comparison with Bolivia (which has a more flexible wage regime) showed comparable take-home pay across industries between the two countries, supporting the authors' first finding that, on average, base pay tends to adjust to offset the cost of mandated pay benefits. The fact that there is more take-home pay dispersion between sectors in Bolivia implies that, while much of cost of the mandated benefits is offset by wage adjustments, Ecuador's requirements may help compress wage dispersion, thus contributing to greater equity.

#### *Concluding thoughts*

The literature on mandated benefits, separate from other labor legislation, is one area in the labor law and enforcement literature that has not yet been thoroughly or systematically explored, and conflicting conclusions in this literature do not yet allow for broad conclusions to be drawn. However, because mandated benefits are very heterogeneous across countries in terms of policy structure, implementation, and enforcement, it may in fact be impossible to draw broad conclusions. It seems unlikely that mandated benefits as a class of labor market institution, or even particular types of these, such as maternity leave or worker's compensation insurance, will be called distortionary or efficient, or good or bad for an economy, in the near future. Perhaps a more attainable aim of this literature would be to analyze individual policies in individual countries, or across countries in cases of very similar policies for welfare, distributional, and informalizing effects in an attempt to find insight into what makes a particular policy affect one economy differently from any other and, eventually, to guide in sensible policy formation.

To aid in analyses of these policies, more modeling should be done that explicitly takes into account the different ways policies are funded and benefits are allocated. These models should be extended to allow for market imperfections: Especially in the case of fixed-cost benefits, if there is monopsony, mandated benefits might actually be shown to be beneficial to workers, even in a simple, static framework. Models also should be extended into a dynamic, general equilibrium framework, and with heterogeneity and other relaxations of standard

assumptions. As with many research areas in labor legislation, there is room for more analysis of the impact of mandated benefits on the growth of informality.

#### **4. Employment Protection**

##### *Theoretical Background*

EPL is a multidimensional institution, including severance payments, advance notice, and other procedural requirements established by law or collective agreements that limit the ability of firms to lay off their employees. In spite of the multidimensionality of EPL, its various dimensions can be conceptually divided into two components. The first component can be seen as a monetary transfer from the employer to the worker (e.g., severance payments), while the second can be modeled as a tax in that it corresponds to a payment to a third party, outside the employment relationship (e.g., litigation and other procedural costs). This distinction is important because the two components have different predicted effects on the employment choices of firms and hence on labor market outcomes.

Most of the theoretical literature on the effects of employment protection on labor market outcomes models EPL as a simple severance payment a firm must make to reduce its labor force. Lazear's (1990) oft-cited two-period model shows that in *perfect* markets with no credit constraints, and if wages are free to adjust, EPL that takes the form of a pure transfer has no effect on firms' hiring decisions. The effects of the EPL are neutralized by a transfer from worker to firm at the time a work contract is signed. However, this neutrality result is easily interrupted by market imperfections. Important in the case of developing countries is the reliance of this model on perfect credit markets and contract enforceability: Because these assumptions are less reflective of reality in many developing economies, this model's predictions are unlikely to hold. Furthermore, Lazear writes that the effects of EPL can only be completely offset if the severance payment is received in lump sum directly by the worker; any intermediary in the process creates distortions. In many countries, an intermediary is used to ensure that the transfer takes place.

Given imperfect markets and credit constraints, Lazear's model predicts that underemployment will result from mandated severance payments. Furthermore, if only some groups of workers (for example, full-time, or workers in large firms) are covered by the legislation, substitution away from employment of this group is expected.

Because many studies of EPL effects have revealed ambiguous or zero effects on employment, dynamic partial and general equilibrium models also have been widely used in this

literature. These have motivated studies of the effects of EPL on individual firm hiring and firing decisions, firm entry, survival, size, and efficiency. Also, based on such models, some recent research has studied employment and firm entry/exit flows, since these may better illustrate the effects of EPLs that are obscured by aggregate numbers.

The effects of firing costs on firm hiring and firing decisions in response to shocks were first formally studied in the early 1990s (Bentolila and Bertola, 1990; Bertola, 1990; and Bentolila and Saint-Paul, 1992, among others). Bentolila and Bertola (1990) present what has become perhaps the standard reference model: a dynamic model featuring a monopolistic firm which faces idiosyncratic shocks and firing costs in a partial equilibrium setting (taking the wage as given). The authors predict that firing costs will reduce job turnover and the volatility of employment and unemployment over the business cycle, but that EPL has an ambiguous impact on the level of employment.<sup>37</sup> The general equilibrium model of Hopenhayn and Rogerson (1993) extends the above model and predicts that increased firing costs will result in higher employment at existing firms, but lower firm entry and job creation rates in an economy as a whole.

Blanchard and Portugal (1998) introduced general equilibrium matching models in which wages are formed as a result of bargaining between workers and employers. These models reflect many aspects of insider-outsider theory, predicting higher wages for insiders and lower employment rates (and thus lower chances of good jobs for outsiders) than the efficient level. In these models, firing costs reduce turnover and increase the duration of unemployment.

Freeman (2000), contrary to arguments for negative efficiency effects of EPL, claims that effects of EPL on unemployment tend to be small or negligible. He emphasizes instead that such institutions result in changes in the distribution of earnings through transfers. MacLeod and Nakavachara (2007) show that when relationship-specific investments are important, employment protection leads to lower turnover, more productive relationships, and possibly higher employment. This is one area in this literature that awaits expansion.

### *Empirical Evidence*

#### *Cross-Country Studies*

Beginning largely with Lazear in 1990, much of the EPL literature has focused on the employment effects of severance pay and advance notice requirements in *developed* countries

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<sup>37</sup> Compared to a scenario with no firing costs, employment is higher in bad times and lower in good times. The effect on average employment is ambiguous, depending on such factors as the functional form of labor demand, the stochastic environment (persistence of shocks, etc.), the discount rate, etc.



(Grubb and Wells, 1993; Scarpetta, 1996; Addison and Teixeira, 2003, 2005; and Nickell and Layard, 1999). The majority of such papers use cross-country employment data, including only a few with inter-temporal policy changes (natural experiments). The early evidence, mostly based on cross-sections or panels of OECD countries, typically finds a weak or zero partial correlation of EPL and employment (negative or zero) or unemployment (positive or zero), generally confirming, or simply not rejecting, what is suggested by Lazear's theoretical discussion. There is a wide literature which identifies effects of EPL from cross-country variation (see, e.g., Nickell, 1997; Botero et al., 2004).

Cross-country evidence of effects on job flows, however, is also weak. Barriers to cross-country studies' consistent identification of the effects of EPL include insufficient sample sizes, highly aggregated data, EPL indices that allow for ordinal comparisons only, lack of true panel data on individuals, and few changes in EPL levels within any particular country. In what is perhaps the most rigorous existing cross-country study, Heckman and Pagés (2000 and 2004) noted that previous studies used only ordinal measures of the costs of EPL, and so computed expected severance cost (in multiples of the average monthly wage) at the time of hiring a full-time, permanent worker for a sample of Latin American, Caribbean, and industrialized nations. They use cross-country data over time to assess the impact of legislative reforms on employment and unemployment rates. Results of their 2000 analysis show a large negative impact of EPL on employment rates. However, their preferred estimation method, a fixed-effects regression, features estimates which, while negative, were not statistically significant at standard levels. Estimates also point to increases in informal employment, partially offsetting formal employment loss. Further, the 2003 update of this work states that "contrary to previously reported estimates, we have found little evidence of a systematic relationship between advance notice and indemnities for dismissal on employment or unemployment...."<sup>38</sup> Two other particularly credible cross-country studies are Micco and Pages (2006) and Lafontaine and Sivadasan (2008). Micco and Pages (2006) use industry-level cross-country panel data, and implement a difference-in-differences methodology based on the theoretical result that EPL should be more binding in industries that are "intrinsically" characterized by higher volatility in demand or supply shocks. They not only find that stricter EPL reduces job flows, but also detect a reduction of value added and employment, the latter being driven by reduced firm entry.

Among the problems with cross-country studies is that because differences exist in country-specific factors (including labor market regulations), it is likely that different types of

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<sup>38</sup> The authors mention, however, that individual studies within their 2003 book do show negative effects of EPL on employment in individual countries.

firms (firms of different size, adopting different technologies, managerial practices, etc.) operate in different countries. If not appropriately accounted for, these make comparisons quite problematic. Lafontaine and Sivadasan (2008) exploit a cross-country, longitudinal dataset obtained from an international fast-food chain, with information on labor choices at a weekly frequency across more than 2,500 outlets in 43 countries. The fact that they use data on the same firm, producing the same product with the same technology ensures that the results are driven by differences in EPL across countries (measured by the index developed by Botero et al., 2004) rather than by unobservable differences in products or technology. Lafontaine and Sivadasan find strong effects of EPL stringency on labor choices at the establishment level, proof that EPL reduces a firm's ability to adjust labor to the desired level in response to demand or productivity shocks.

In the last decade, work on employment protection legislation has added to the literature in three main ways: exploring the effects of EPL in developing countries in both static and dynamic frameworks; focusing on single countries, using panel data on individuals or individual firms; and the study of worker flows into and out of employment (both formal and informal), unemployment, and (but less frequently) on output and productivity. In many cases, papers include all three of these innovations. In general, increased interest since the early 2000s in the role of EPL and better data availability have generated a greater focus on the effects of such legislation in developing countries, especially in Latin America and India.

#### *Firms' Decisions with Heterogeneous Applicability of EPL*

Theory predicts that EPL reduces firms' propensity to adjust employment levels in response to positive or negative shocks. Caballero, Engel, and Haltiwanger (1997) study the gaps between "desired" and actual employment levels of firms using panel data on United States manufacturing plants. They conclude that the firm's employment adjustment problem is similar to a standard (S,s) model with asymmetry, in that individual firms' adjustment costs are non-linear and non-convex, resulting in larger, less frequent than desired, and asymmetric adjustments of labor. Also, aggregate shocks account for most average employment fluctuations, with individual firms' adjustment functions exacerbating these fluctuations.

In the developing or transition country context, an obvious extension of such a model is one that includes choice between formal, full-time employment and informal or otherwise less-protected workers, and to what extent labor laws and enforcement affect adjustment patterns and employment. Fully covered workers may be more productive, but also more costly in wage and non-wage payments, including firing costs. Informal workers or other more flexible work contracts that are not always covered by EPL are thus one way in which firms can adjust more

easily to shocks in a heavily regulated (at least *de jure*) labor market. The extent of use of more flexible contracts, and frequency of adjustment of this portion of the workforce, may therefore shed light on the actual cost of labor regulations in the covered sector, the effects on the mix of employment in the economy, and the inefficiencies from barriers to labor force adjustment.

In most countries, the same employment protection rules apply to all firms. In some countries, however, the regulations vary geographically, or according to firm size, or based on workers' characteristics (e.g., tenure). These differences have all been exploited in the literature as sources of identification of the effects of EPL.

Autor, Kerr, and Kugler (2007) exploit variation in the timing of adoption of wrongful discharge protection across U.S. states from the late 1970s to the early 1990s. They distinguish between two common-law "exceptions" to the employment-at-will doctrine: the "good faith" exception and the "implicit contract" exception. The "good faith" exception prohibits employers from firing workers for "bad cause" (e.g., terminating a worker "to deprive her of a promised benefit"), while the "implicit contract" exception applies when an employer implicitly promises not to fire its employees without good "cause."<sup>39</sup> Autor, Kerr, and Kugler find that the "good faith" exception reduced turnover, induced firms to increase their capital intensity and to reduce their use of production workers in favor of more skilled workers. This implied higher productivity of labor but lower total factor productivity.

One of the earlier papers focusing on a developing country and seeking better identification through temporal, within-country variation as opposed to cross-country variation is Kugler (1999). This paper uses Colombian household data to analyze the effects of a 1990 reduction in firing costs on the differences in hazard rates for entry and exit of unemployment status between formal and informal workers. The source of identification rests in the different coverage between formal and informal sector workers. Kugler's model uses differential monitoring and firing costs to predict that 1) higher firing costs reduce the likelihood of a firm choosing to operate in the formal market, and 2) higher firing costs reduce hiring by formal firms, compared to informal firms. Results of this study are taken to imply that a "reduction in firing costs [due to 1990 labor law reform in Colombia] increased the hazard rate out of unemployment by up to 1.06% and the hazard rate out of unemployment of covered workers by up to 1.7% relative to uncovered workers."

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<sup>39</sup> See Autor, Kerr, and Kugler (2007) for details. The authors also consider the so-called "public policy exception," which protects workers against "discharges that would inhibit them from acting in accordance with public policy," e.g., firing a worker for performing jury duty, claiming worker compensation, reporting wrongdoings of other employees (or the employer's). The authors, however, recognize that "the public policy exception is not generally thought to impose substantial constraints on employer behavior."

Besley and Burgess (2004) exploit time and regional variation in India to implement a careful difference-in-difference methodology, results of which are supported by many robustness checks. Their results suggest strong and statistically significant output and employment effects: Pro-worker legislation states experienced lower manufacturing growth by around 23% to 24%; pro-employer legislation states experienced increases by a similar amount. The authors also found negative effects of pro-worker legislation on investment and value added per employee, and a positive relationship between pro-worker legislation and increased poverty and informal sector activity. However, this study did not focus on EPL directly, so the effects of EPL as distinct from other labor legislation are not immediately identifiable.

If EPL applies differently to different types of workers, it has the potential of distorting the optimal composition of employment. For instance, severance payments often depend on a worker's job tenure. Therefore, workers with shorter tenure (young, females) are cheaper to dismiss and are expected to suffer higher dismissal probabilities in downturns. This is consistent with what Montenegro and Pagés (2003) find in their analysis of Chile. Using household survey data from 1960 to 1998, they show a negative effect of job security regulation on the employment probabilities of women compared to men of prime age and youth. However, higher rates of job finding among women also have been found, possibly because of lower expected firing costs.

Similarly, where EPL is more stringent for workers employed by large firms, the firm size distribution in the economy might be distorted as a result. In Italy, the rules governing dismissals are much stricter in firms with more than 15 employees. Boeri and Jimeno (2004) consider the implications of EPL thresholds for dismissal probabilities in Italy and Spain, finding that workers employed in firms subject to stricter regulations face a lower dismissal probability. Garibaldi et al. (2004) and Schivardi and Torrini (2005) find that EPL in Italy reduces the firms' propensity to grow, especially around the 15-employee threshold (although the estimated effects are quantitatively modest). Schivardi and Torrini (2005), however, find that turnover in firms with more than 15 employees is higher than in smaller firms. This result might appear surprising, but the authors argue that it is probably due to the fact that firms with more than 15 employees make larger use of flexible employment contracts, precisely to avoid the stricter firing rules that apply only to "regular" contracts. One of the few studies to look at the potential effects of EPL on firm size in a developing country is Almeida and Carneiro's (2008) analysis of Brazil, which we describe below.

Most EPL indicators are based on the legal constraints that apply in each country, or region within a country. The extent to which EPL "bites," however, depends on whether the regulations are actually enforced. As a matter of fact, differences in enforcement (e.g., related to

the efficiency of a country's legal system) across countries are as important or perhaps more important than differences in the letter of the law (Bertola, Boeri, and Cazes, 2000). Only a few papers have used any measures of enforcement when analyzing the effects of EPL. Cabellero, Cowan, Engel, and Micco (2006) use a panel of 60 countries around the world and find that stricter labor regulations have adverse effects on job turnover and firms' speed of adjustment to shocks, but only in countries with strong rule of law and government efficiency (taken as measures of enforcement of regulations). However, as with many cross-country studies, the limited extent of intertemporal variation is problematic for identification of effects, as opposed to correlations. Moreover, there are several reasons to believe that enforcement is endogenous to economic outcomes (Bertola, Boeri, and Cazes, 2000; Ichino, Polo, and Rettore, 2002).

In their study of Brazil, Almeida and Carneiro (2008) use geographic variation in the degree of enforcement of labor rules while adopting an instrumental variable approach to solve the endogeneity problem. They measure enforcement as the number of inspections per 100 firms, and use the distance between the town where a firm is located and the surrounding enforcement offices as an instrument for the degree of enforcement faced by a firm. To lessen the possible endogeneity coming from the fact that firms' location choices might be affected by the extent of EPL enforcement, the authors control for a rich set of firm and town characteristics. Almeida and Carneiro propose that stricter enforcement could reduce firm size for two reasons: First, because it effectively increases the cost of labor, and second, because small firms are less likely to be detected by enforcement officials. Almeida and Carneiro's findings strongly support the idea that stricter enforcement reduces firm size (measured by the number of registered employees), output, and capital stock. Although data limitations prevented the authors from assessing the effects of enforcement on the number of unregistered (i.e., informal) workers, they speculate that because stricter enforcement should reduce the share of informal workers, it is most likely that the overall (formal+informal) size of firms is reduced by stricter enforcement. Almeida and Carneiro also provide some evidence that stricter enforcement is associated with lower labor productivity and lower capital/labor ratios, although these effects were not statistically significant.

Most studies of the impact of EPL on outcomes which rely on microdata tend to focus on the intensive margin of adjustment, often ignoring adjustments on the extensive margins through entry and exit of firms. Aside from the previously mentioned Autor, Kerr, and Kugler (2007) and Micco and Pages (2006) studies, among the few papers to have looked at the extensive margin is Kugler and Pica (2005). Kugler and Pica (2005) exploit a legislative change that occurred in Italy in 1990 that increased employment protection for small firms, and implement a difference-in-differences methodology. Consistent with the Italian studies we mentioned above, they find that

increased dismissal costs reduced job creation and job destruction in small firms compared to large firms. In addition while they find no effects of the reform on firm exit rates, they find that small firms became less likely to enter the market after the reform. This is an area of research that awaits expansion, especially in the context of developing countries.

### *Interactions with Other Institutions*

A general theme of this survey is that institutions do not act in isolation, and that ignoring institutional interactions can lead to wrong predictions and interpretations of the empirical findings. As mentioned earlier, if wages are flexible, they will adjust in response to hiring and firing costs. However, this mechanism fails when wages are downwardly rigid, e.g., in the presence of minimum wages or with centralized wage bargaining. Other institutional features of a country also might interact with EPL to shape its effects. In particular, the effects of employment protection are likely to vary depending on the degree of development of financial markets and the degree of regulation in product markets.

Although the extent, nature, and strictness of regulations vary widely across countries, all countries intervene in product markets by regulating entry and exit of firms, the quantity and types of output, and sometimes even prices. Moreover, governments often directly take ownership or control of businesses in particular sectors (e.g., public utilities). Clearly, these interventions might have an impact on the level and dynamics of GDP in the economy, as well as on the level and composition of employment (Bertrand and Kramarz, 2002). Further, they could impact efficiency and allocative outcomes indirectly by interacting with labor market institutions. For example, if some sectors are protected so that entry is restricted and employment is reduced to artificially low levels, the ability of unprotected sectors to absorb the workforce displaced by these restrictions depends crucially on wage flexibility as well as on the overall stance of EPL. Moreover, strict EPL can further strengthen the position of “insiders” in protected sectors, thus contributing to the “dual” nature of labor markets that characterizes highly regulated economies (Saint-Paul, 1996).

Possible interactions of EPL and financial markets development have been recognized only very recently. The basic idea is that protection against labor market uncertainty is more important in environments where workers have limited access to consumption-smoothing instruments. As formally shown by Bertola (2004), if financial markets are perfect and complete, employment protection plays little or no role because workers can already obtain perfect insurance against idiosyncratic labor market shocks. In economies with market imperfections, on the other hand, individuals cannot perfectly ensure against labor income uncertainty, and EPL can

have beneficial effects on workers' and overall economic welfare. Empirical work on the interactions of EPL and financial markets is essentially non-existent.

Taking into account institutional interactions might help explain some otherwise puzzling findings. For instance, firm turnover and job turnover rates<sup>40</sup> are very similar in Europe and the United States, even though EPL is much stricter in European countries than in the U.S. Because theory predicts that stricter EPL should unambiguously reduce firm entry and exit rates, as well as both job creation and job destruction, one might be tempted to conclude from many empirical studies that employment protection provisions are of no consequence in practice. Perhaps this is because they are poorly enforced or because firms and workers use Coasian contracts or transactions to undo the undesired effects of those provisions. Institutional interactions, however, provide alternative (and more plausible) explanations for the observed findings.

One possible explanation for the similarity of job creation and job destruction rates in Europe and the U.S. is offered by Bertola and Rogerson (1997). These authors note that in addition to having stricter EPL, European countries differ from the U.S. in the higher degree of wage compression due to centralized wage bargaining. They propose that such greater compression of wages across firms may actually *increase* the rates of job creation and job destruction, thus counterbalancing the effect of strict EPL. A similar mechanism is proposed by Koeniger and Prat (2007). In Koeniger and Prat (2007), however, the key interaction is between EPL and product market regulation (PMR). PMR is modeled in their study as a sunk entry cost, capturing administrative procedures and licenses that firms need to acquire before they can start operating, and a flow cost capturing the bureaucratic requirements firms have to bear every period. Koeniger and Prat show that PMR increases the size of firms and has a positive effect on both job creation and job destruction, thus counteracting the detrimental effect of EPL on job turnover. Given that these two sets of regulations have opposite effects on firm and job flows, the fact that EPL strictness and PMR strictness are positively correlated across OECD countries (see Boeri, Nicoletti, and Scarpetta, 1999 and Nicoletti and Scarpetta, 2003) implies that the failure to find effects of EPL on firm and job flows might be due to cross-country analyses neglecting the role of the EPL-PMR interaction.

Taking into account interactions of EPL and other regulations is also important when evaluating (or forecasting) the effects of labor markets or product markets reform. Because labor and product markets are interrelated, the impact of reforms in one market are likely to be affected by how heavily other markets are regulated, as well as by reforms in these other markets.

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<sup>40</sup> The similarity of firm turnover rates across OECD countries is documented in OECD (2003), and that of job turnover rates in Bartelsman, Haltiwanger, and Scarpetta (2004).

Theoretically, labor and product market reforms could complement (i.e., reinforce) or be substitutes for one another.

Recent work by Fiori et al. (2007) looked at the interaction of labor and product market reforms in OECD countries over the period 1980-2002. They find that employment gains from product market reforms (for example, privatizations and liberalizations) have been greater in countries with highly regulated labor markets. They also find evidence that in countries which have deregulated their product markets, workers' bargaining power declined over time.

Bertola and Koeninger (2007) combine individual-level survey data with indicators of credit supply conditions for a cross-section of OECD countries, and document that poorer individuals' demand for redistribution is higher in countries where consumer credit is less freely available.<sup>41</sup> Their findings indicate that EPL reform should be easier to implement if accompanied by increased access to financial markets. The literature on the political economy of labor market reforms strongly suggests that reforms that weaken employment protection are more likely to be accepted if they are accompanied, at the same time, by increased generosity of unemployment insurance (Boeri, Börsch-Supan, and Tabellini, 2002).

Together with imperfections in capital markets, weak law enforcement in developing countries also could explain why severance payments, which according to the theory can be neutralized by the parties via wage adjustments, do end up having effects on hiring and firing decisions and other margins. Credit-constrained individuals might not be willing to take wage cuts upfront. Employees also might fear that the firm will renege on its implicit contract and "take the money and run" (Kugler, 2005).

In 1990, Colombia went from a traditional severance payments system to a new system of individual savings accounts (SPSA). Before the change, employers in Colombia were required to pay severance at the time of separation, while employers in the new system make monthly deposits to individual accounts. Kugler (2005) argues that this policy change greatly reduced uncertainty about payment of severance, making it easier for workers and firms to work around the legislation with wage adjustments. Kugler's analysis indicates that after the introduction of the SPSAs, wages were lowered by 60% to 80% of total severance payment contributions. The analysis in Kugler (2005) thus provides support for the hypothesis that the design of severance payments can be altered to overcome institutional barriers and to reduce efficiency distortions.

#### *How to Ease the Transition for Displaced Workers*

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<sup>41</sup> Further, this effect is stronger for younger individuals, who, ceteris paribus, are more likely to have a stronger demand for borrowing.



Labor or product market deregulation often leads to workers being displaced from protected firms or industries, and unemployment. One possible way of easing the transition is to offer training to displaced workers. The literature on the evaluation of training programs in developed countries is extensive (see Friedlander, Greenberg, and Robins, 1997; Heckman, LaLonde, and Smith, 1999). In the case of the United States and Europe, the balance of the evidence – especially from randomized evaluations – seems to indicate that 1) job training programs have small effects and 2) the effects vary with the type of training and the characteristics of the recipients. In particular, on-the-job training seems to work better than classroom training (Heckman, Hohmann, and Smith, 2000), and females tend to benefit more than male workers.

In recent years, several countries in Latin America have offered training programs for particular groups of workers, mainly low-income, low-skill youth. Several of these programs have been evaluated econometrically (see, e.g., Victor Elias et al., 2004; for an overview, see Betcherman, Olivas, and Dar, 2004) and have generally shown positive effects, particularly for women. Unfortunately, most of the existing studies have used non-experimental techniques (in most cases, propensity score matching methods). Therefore, their results might be biased due to the non-random selection of individuals into participation in the programs. In our survey, we found only two randomized evaluations of training programs in developing countries, both very recent. The first is a study by Card et al. (2007) on the Dominican Republic, and the second is an evaluation of a Colombian program by Attanasio, Kugler, and Meghir (2008).

Card et al. (2007) evaluate a randomized job training program implemented in early 2004 in the Dominican Republic (“Juventud y Empleo,” financed by the Inter-American Development Bank) for low-income youth in urban areas. The program consisted of several weeks of classroom instruction followed by an internship at a private sector firm. Importantly for the research design, the program featured randomized assignment of eligible individuals to participate in the program. Follow-up interviews were collected 10 to 14 months after most trainees had finished their course work. The authors examined the effects of the program on a number of outcome variables, including employment, hours of work, and hourly wages. Results of the evaluation revealed a negligible impact of the program on employment and positive effects on average monthly wages (about a 10% gain), although these effects were not statistically significant at conventional levels.

In a similar fashion, Attanasio, Kugler, and Meghir (2008) evaluate the impact of a randomized training program for disadvantaged youth introduced in Colombia between 2001 and 2005 (“Jovenes en Accion,” financed by the World Bank and the Inter-American Development Bank). In this program – targeted to young people between the ages of 18 and 25 in the lowest

socio-economic strata of the population – participants were given 3 months of in-classroom training and 3 months of on-the-job training (in the form of internships in private companies). In this case the program also was designed so that participation was randomly assigned. Follow-up interviews were collected 13 to 15 months after most trainees had completed the program.

In contrast to the Dominican Republic program, the Colombian program seems to have had significantly positive effects on employment and earnings for both males and females, with larger effects for females. Attanasio, Kugler, and Meghir estimate that the program increased the earnings of males by 8 percent and those of females by 18 percent. The authors document that most of these earnings gains are attributable to the increased employment of trainees in the formal sector. Similar to findings on developed countries, the positive impact of training was greater when individuals spent more time on on-the-job training; classroom training is found to have no impact on the returns to training. A cost-benefit analysis reveals that the program generated net gains, in particular for female participants.

The contrasting results obtained by the Card et al. (2007) and the Attanasio, Kugler, and Meghir (2008) study suggest that the results from any particular study evaluating the effects of a training program – even randomized trials – cannot be immediately extrapolated to other contexts. In fact, the “causal effect of training” is very much likely to fall into the category of *implementation-specific effects* (DiNardo, 2007) in that it depends heavily on the way the “treatment” is administered in practice.<sup>42</sup> While learning from others’ experience seems to be of limited help, one possible strategy to evaluate the effects of a program would be to start with a small-scale, randomized “pilot” and carefully evaluate the effects before deciding whether to expand, make adjustments, or discontinue programs. The most important caveat to this approach would be that large-scale programs could have general equilibrium effects that might go undetected in small-scale pilot studies. As a way of gauging such general equilibrium effects, randomization in “pilot” experiments should be done not only across units (e.g., villages) but also within units (Angelucci and DeGiorgi, 2008).

### *Concluding Thoughts*

Although empirical research focusing on EPL in developing countries is more limited compared to that on minimum wages, the models on this type of labor legislation have been well developed, and the most up-to-date methodologies have been used in the empirical literature. In contrast to the minimum wage research, though, the results of the empirical literature on the

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<sup>42</sup> Another example of “implementation-specific” effect would be the “causal effect of stricter monitoring on job search effort” (Micklewright and Nagy, 2008). See the discussion in the Unemployment Benefits section of this paper.

effects of EPL on economic outcomes seem to send a more consistent message: Regardless of the methodology adopted (cross-country studies, within-country difference-in-differences, instrumental variables approaches), dismissal costs are found to reduce the ability of firms to adjust their workforce in response to shocks, to negatively affect output, investment, and productivity, and to induce firms and workers to operate in the informal sector of the economy.

As in many areas of applied economics, and enabled by ever-increasing computing power, extensions looking at differential effects by types of workers, their income levels, firm size, or formality status seems to be a current expansion direction in this literature, but other research directions still exist. In our view, the greatest limitation of existing research on the effects of EPL is that this institution has so far mostly been studied in isolation from other labor market institutions, from regulations in product markets, and from the development of financial markets. Bertola (2004) and Bertola and Koeninger (2007) have shown that the interactions of EPL with credit markets are manifold and significant. Research from developed countries shows that consumption fluctuations (and inequality) are strongly related to wage fluctuations (Attanasio and Davis, 1996; Blundell and Preston, 1998) because labor earnings constitute the main (and often the only) source of income for most households, and access to financial instruments is very limited. These considerations are even more important to examine in the context of developing countries. Unfortunately, data limitations have so far largely limited researchers in their ability to study the interplay between labor and financial markets. Research efforts would greatly benefit from the construction of large-scale micro datasets integrating labor force survey data with information on households' access to formal or informal credit as well as households' savings and investment decisions.

Because much research has shown that the type of firms that operate in a market is endogenous to the whole set of institutional features of a particular country (including, but not limited to, product market regulation), cross-country studies of the effects of EPL that neglect differences in PMR across countries risk biased estimates and may draw the wrong conclusions. As Koeninger and Prat (2007) point out, empirical studies based on firm-level, longitudinal data are particularly promising, since their panel structure allows one to limit firm selection issues by controlling for establishment or firm-level fixed effects (see, e.g., Autor, Kerr, and Kugler, 2007).

In terms of major areas of research still to be done, almost absent in EPL research is focus on countries in East and Southeast Asia and Africa. Also, many of the theoretical models were originally developed to study wealthier countries, in which markets are likely to be closer to the “perfect and complete” assumption. Extensions of theory into market imperfections like

monopsony, search frictions, and heterogeneous workers and firms have yet to be exhausted, and empirical work motivated by such models may be particularly useful in developing countries.

At a basic level, we still do not know much about what amount of a country's EPL consists of pure transfers and how much is instead more similar to a tax. Using panel data and a structural model, Barros, Corsueil, and Gonzaga (1999) discuss both variable (paid monthly to an account administered by the government) and fixed cost (lump-sum severance costs) EPLs in Brazil, but do not uncover evidence of labor market effects of a large change in severance policy in their analysis. To the best of our knowledge, the only paper to have attempted a decomposition of firing costs between transfer and tax components is Garibaldi and Violante (2005) in the context of Italy. Because the two components are predicted to have very different implications for labor market outcomes, any effort to build a database in which the two components are separated for a large set of countries would be of great importance. Also, in analysis of reforms this work would be useful in separating the effects on transfer versus tax components of policies. Giving a precise monetary value to the transfer and the tax components, moreover, would be valuable in estimating labor demand shifts resulting from EPL.<sup>43</sup>

## 5. Unemployment Insurance

Unemployment insurance benefits (UIB) are payments made to workers who lose their jobs. As such, they provide insurance against an otherwise uninsurable unemployment risk. Moreover, being *contingent* payments (i.e., they are paid only in case of job loss) they deliver greater welfare levels compared to individual savings. At the same time, by decreasing the price of leisure and raising the worker's reservation wage, unemployment insurance increases unemployment duration. Hence, optimality for unemployment insurance schemes requires balancing the insurance (consumption-smoothing) benefits with the distorted (reduced) job-search incentives. This basic theoretical scheme has been taken as the benchmark for the vast majority of the rich empirical literature on the effects of unemployment benefits.<sup>44</sup>

In this chapter of our survey, we go beyond the issues covered by traditional surveys to include aspects of unemployment insurance schemes that have been highlighted by the literature only recently. Three issues, in particular, have only recently gained considerable attention in the

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<sup>43</sup> Heckman and Pagés (2000, 2003) have computed such a measure; they do not consider the tax versus transfer components separately.

<sup>44</sup> Atkinson and Micklewright (1991) and Krueger and Meyer (2002) provide excellent surveys of this rich and insightful literature.

economic analysis of unemployment benefits. The first issue concerns the channels through which UI benefits affect search behavior. In a recent paper, Chetty (2008) provides evidence that the increase in unemployment durations caused by UI benefits in the United States is due to relaxed liquidity constraints rather than to distortions in marginal incentives to search (moral hazard). Because relaxing liquidity constraints is welfare-enhancing, and given that it is likely that a far larger proportion of workers in developing countries are liquidity-constrained compared to the United States, the findings of Chetty (2008) cast a new light on UIBs and invite economists to conduct investigations in the same vein in the context of developing countries. Second, a recent paper by Card, Chetty, and Weber (2007) calls into question the finding that unemployment exit rates exhibit a “spike” around benefits exhaustion, which was, until recently, the most compelling evidence of the moral hazard effects of unemployment insurance (e.g., Feldstein, 2005; Vodopivec, 2004). Card, Chetty, and Weber (2007) present evidence that the “spike” finding is largely an artifact of measuring unemployment spells as time spent on the unemployment system rather than time to next job. This highlights the importance of a careful mapping of theoretical concepts into an empirical framework, and of using high-quality data. Finally, recent and largely untested theories have pointed to the possible effects of UIBs on the extent and quality of job reallocation in the economy. In traditional studies, jobs were largely treated as homogeneous. Recent research (e.g., Acemoglu and Shimer, 1999, 2000) has challenged this view, arguing that UI schemes also can alter the allocation of resources by influencing *what kinds* of jobs workers seek and firms create. The issue of what jobs are created is particularly important when an economy is in the midst of structural change, as is the case, for instance, in transition economies (Boeri and Terrell, 2002). To our knowledge, the only paper to explicitly look at the effect of UIBs on job reallocation is Boeri and Macis (2008), in the context of a cross-country panel study exploiting the introduction of UIB systems in a number of transition and developing economies.

While all OECD countries have some unemployment insurance scheme in place, UIB schemes are uncommon in developing countries, essentially because of these countries’ limited capacity to administer such programs. As documented by Vodopivec (2005), in low-income countries income “support schemes for the unemployed consist almost exclusively of public works and severance pay programs.” In Africa, for instance, only four countries (Algeria, Egypt, South Africa, and Tunisia) offer any kind of unemployment insurance program.<sup>45</sup> Therefore, it is not surprising that most of the empirical evidence on the impact of such programs comes from the experience of developed countries. In recent years, however, a substantial number of middle-income countries have introduced unemployment benefit schemes for the first time. With the

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<sup>45</sup> Nigeria provides some coverage to the unemployed through some other program (SSW, 2007).

exception of a few formerly planned economies in Central and Eastern Europe and from one cross-country-panel study (Boeri and Macis, 2008), this wealth of “natural experiments” has not yet been fully exploited.

#### *Measurement issues and cross-country comparisons*

As is the case with EPL, unemployment benefits are often treated as a one-dimensional institution, even though there are at least three key dimensions which identify an unemployment benefit system: eligibility conditions, level of payments (typically measured as a fraction of previous earnings), and duration over which they are paid.

Typically, job losers have access to a system that replicates an insurance scheme for short unemployment durations: They draw benefits that are proportional (at least above a benefit floor and below a given threshold) to their past payroll tax contributions to an unemployment insurance fund. This “previous employment contingent” insurance component of unemployment benefits provides transfers proportional to the previous wage and for a duration proportional to length of previous employment. Note that this structure reduces incentives to elude or evade payments of payroll contributions.

Transfers to job seekers during longer periods of unemployment are generally independent of past contributions, and are offered in combination with other cash transfers to individuals who are not working (e.g., social assistance). This component of unemployment benefits generally involves flat subsidies and a maximum duration that is not conditional on the length of the contribution period. It also can be integrated with general social assistance, in which case it is offered for unlimited duration and can be means-tested, that is, provided only to the unemployed individuals who have incomes and assets lower than a given (poverty) threshold. Linking the transfer to household assets ties individual and family labor supply decisions together; if these are not properly taken into account, the availability of unemployment assistance after the exhaustion of unemployment benefits might induce a bias in the estimated effects of the UIB (Micklewright and Nagy, 1999; Pellizzari, 2006).

Early research on the effects of UIBs was motivated by the idea that differences in institutions could explain differences in economic performance across countries. Mapping all the various features of unemployment benefits into a one-dimensional measure, however, is not an easy task. In OECD countries, replacement rates after tax tend to be higher than gross replacement rates, since income taxes are progressive and unemployment benefits are in some countries exempt from income taxation. Replacement rates are also generally declining over time (there are optimal insurance arguments for this design feature), and there is considerable variation

across countries in replacement rates offered as the unemployment spell proceeds. In order to account for these differences in the generosity of the system at longer unemployment durations, the OECD tabulates a "summary measure of benefit generosity," which is defined as the average of the replacement rates in the first two years of unemployment for an average production worker having sufficiently long seniority to be offered the benefits up to their maximum duration. There is also considerable cross-country variation in the replacement rates, since some countries offer much more generous transfers to unemployed people than other countries. However, generosity is also related to the coverage of unemployment benefits: The fraction of job losers reporting to receive unemployment benefits after the loss of a job varies significantly across countries (see, e.g., Vodopivec et al., 2005). In sum, the challenges posed by the multidimensionality of UIBs exacerbate the problems faced by cross-country studies in estimating genuine causal effects of UIB schemes.

#### *Unemployment Benefits and Consumption Smoothing*

Unemployment benefit schemes are introduced by governments to provide insurance against an otherwise uninsurable labor market risk. The increased consumption smoothing from the availability of unemployment insurance is the first and most obvious benefit from UI to be weighed against the distortions to worker search behavior (discussed in the next paragraph). The empirical literature on the consumption-smoothing benefits of UIBs is (perhaps surprisingly) quite thin. Gruber (1997) was the first paper to empirically measure the effects of unemployment insurance on consumption smoothing during unemployment spells. Using data from the Panel Study of Income Dynamics for the period 1968-1987, Gruber finds strong evidence that UIBs help individuals substantially in smoothing consumption<sup>46</sup> after losing a job. Gruber's results indicate that a 10 percentage-point increase in the after-tax replacement rate (a measure of generosity of UIBs) will reduce the drop in consumption induced by unemployment by 2.65 percent. His estimates also imply that the fall in consumption for those who become unemployed given a zero replacement rate would be between 23% and 26%, depending on the specification adopted. In an earlier paper, Gruber (1994) estimated that unemployment is associated with a drop in consumption of 6.8% on average. The estimates of Gruber (1997) therefore indicate that without UI, the drop in consumption upon unemployment would be more than three times as large (about 20%).

Clearly, the importance of UIBs for consumption-smoothing depends on the extent to which households are liquidity-constrained. Using Canadian data, Browning and Crossley (2001)

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<sup>46</sup> Gruber (1997) uses food consumption as a proxy for total consumption.

isolate liquidity constraints or ‘transitory income effects’ from the ‘permanent income’ shock of job loss. Browning and Crossley improve upon the previous literature by using total expenditure rather than just expenditure on food, and by exploiting specific legislative changes in benefit levels rather than just state-time variation. The findings of Browning and Crossley (2001) indicate that for most Canadian households, consumption seems not to be sensitive to marginal changes in the level of UIBs. These authors argue that this is because as many as 90% of households in their sample are not liquidity constrained. They also find significant effects of UIB generosity among households who do not have assets at the time of job loss and/or in which there is a spouse who is not employed (proxies for liquidity constraints). For these households, there is nearly a one-to-one relationship between UI benefit levels and total expenditure. This is, in fact, the most important finding of Browning and Crossley (2001): The effect of benefits is very heterogeneous in the population in a way that is consistent with the theoretical prediction that the consumption-smoothing effects of UIBs are more important for households that are liquidity constrained.

We are not aware of any study directly comparable to Gruber (1997) or Browning and Crossley (2001) in the context of a developing country. Indirect, aggregate evidence of effects of UIBs on consumption patterns in countries other than the U.S. or Western Europe does exist for some countries in Eastern and Central Europe. Vodopivec et al. (2005) document that in Hungary and Poland unemployment benefits were received by the vast majority of unemployed households, and were largely directed at households in poverty or which would be in poverty without UIBs. This is an indication that UIBs have probably helped households in these countries smooth consumption in the face of earnings losses due to unemployment. In contrast, in other countries in Vodopivec et al.’s sample (e.g., Bulgaria, Estonia, and Latvia) coverage was very limited (UIBs reached only a small fraction of unemployed households) and the effects on poverty were also quite small. This indicates that the effects of UIBs on consumption-smoothing (and hence, indirectly, on poverty and inequality) depend on the way UIB schemes are implemented, and highlights the need for within-country studies based on individual- or household-level data.

One important issue that must be taken into account when studying the consumption-smoothing role of UIBs in developing countries is that unemployment benefits (as well as any other income support program) might have substantial "crowding out" effects on informal, traditional channels of insurance or self-insurance. A large literature highlights the relevance of such channels in developing countries. A recent paper by Angelucci and DeGiorgi (AER, forthcoming) examines how cash transfers to eligible households in Mexico indirectly affect the consumption of non-eligible households living in the same villages, and find significant spillover



effects that operate through informal insurance and credit markets. In particular, ineligible households benefit from their neighbors' higher incomes by receiving more transfers, borrowing more, and reducing their precautionary savings. The findings of Angelucci and DeGiorgi highlight the importance of taking a “general equilibrium” approach when studying the effects of transfer programs in developing countries.

#### *Effects on Search Effort and Unemployment Duration*

A great deal of empirical research has been devoted to studying the effects of unemployment benefits on the duration of unemployment. Once again, most of the existing empirical studies were conducted in the context of developed countries, notably the U.S. Until very recently, the received wisdom on UIBs was that they have adverse efficiency effects. Indeed, evidence from cross-country as well as microstudies seems to indicate that more generous unemployment benefits – both in levels and duration – have a positive effect on the duration of unemployment spells and hence on the unemployment rate (for the U.S. and Western Europe see, e.g., Moffitt, 1985; Meyer, 1990; Layard et al., 1991; Katz. and Meyer, 1990; for Central and Eastern European countries, see Vodopivec et al., 2005). One piece of evidence, in particular, has been taken by many as the single most convincing indicator of the inefficient, moral hazard implications of UIB systems: the existence of a “spike” in the exit rate from unemployment at or around the date of expiration of unemployment benefits.

A “spike” out of unemployment at the time of benefit exhaustion is consistent with the theoretical prediction that as the end of the entitlement period approaches, the unemployed decrease their reservation wage and/or increase their search effort and are thus more likely to accept job offers and exit unemployment. In other words, the presence of a “spike” at exhaustion would indicate that the unemployed are essentially “waiting” to exhaust the benefits before returning to work.

Recent work by Card, Chetty, and Weber (2007), however, urges researchers to exert greater caution when interpreting the above-mentioned results. These authors argue that the “spike at exhaustion” finding is often an artifact of measuring unemployment spells as time spent on the unemployment system rather than time to next job. Card, Chetty, and Weber review the existing empirical literature on this topic, and conduct an empirical analysis using administrative data from Austria. They find that both in existing studies and in the Austrian data, the way in which unemployment spells are measured has large effects on the magnitude of the spike at exhaustion. Large spikes are found when the unemployment spell is defined as time spent on the unemployment system (e.g., Moffitt, 1985; Meyer, 1988, 1990; Katz and Meyer, 1990; Lalive et

al., 2007), while much smaller effects are generally detected when unemployment is measured as time to the next job (Fallick, 1991; Carling et al., 1996; McCall, 1997; Bratberg and Vaage, 2000; Kyrya and Ollikainen, 2006).<sup>47</sup> In the Austrian case, directly examined by Card, Chetty, and Weber, the exit rate from registered unemployment rises by over 200% at the expiration of benefits, while the hazard rate of re-employment goes up by just 20%. Their results imply that ending dates of actual unemployment spells are “manipulated” to end precisely at the exhaustion of benefits in less than 1 percent of cases.

Further doubts on the proposition that UIBs have large efficiency costs come from Chetty (2008). Chetty (2008) explores the channels through which unemployment insurance affects search behavior, distinguishing between a welfare-enhancing “liquidity effect” and a welfare-reducing “moral hazard” effect. If households are unable to smooth transitory income shocks relative to permanent income in the event of job loss, unemployment insurance can ease liquidity constraints thereby influencing search intensity. For liquidity-constrained households, search intensity is affected by both moral hazard and liquidity effects; but if households are unconstrained, only the moral hazard effect operates. Because the liquidity effect is welfare-enhancing (it partially corrects the distortions created by imperfect credit and insurance markets) while the moral hazard effect is inefficient, it is crucial to empirically evaluate the relative importance of these two channels. Using data from the United States, Chetty (2008) estimates that most of the increase in unemployment durations caused by UI benefits is due to liquidity effects rather than moral hazard effects.

#### *Effects on Labor Force Participation and Informality*

Unemployment insurance might affect labor supply decisions as well as the choice of working in the formal vs. the informal sector. As noted by Hamermesh (1980), because unemployment benefits increase the value of labor market participation, one might expect the level of benefits to positively impact the decision to enter the labor force. Yaniv (1982) presents a model where the level of benefits depends on previous earnings (as is the case in many real-world unemployment insurance schemes). Because the model allows workers to influence the level of benefits by choosing how many working hours to supply while employed, Yaniv’s model predicts

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<sup>47</sup> One exception is van Ours and Vodopivec (2006), which finds a large spike in the re-employment hazard at the point of UI exhaustion for job seekers in Slovenia. Card, Chetty, and Weber (2007) attribute the Slovenian finding to the fact that “UI recipients are working in the informal sector and waiting until their benefits expire to return to the formal sector (where their new job start is measured.”

that more generous unemployment benefits can actually increase labor supply on the intensive margin (e.g., from part-time to full-time).<sup>48</sup>

Boeri and Garibaldi (2007) develop an extension of Mortensen and Pissarides' (2004) model that allows for endogenous sorting of workers into the formal or informal sector – an extension which is well-suited for labor market conditions in many developing countries. In this model, the introduction of UIBs induces workers to move from the uncovered (informal) sector to the covered (formal) sector, generating equilibria with higher participation and job creation in the formal sector, but also higher unemployment. The key factor here is related to the presence of “entitlement effects,” that is, the presence of a segment of job applicants who are not currently receiving UIBs but who qualify for benefits only by working in the formal sector. The introduction of a UIB system increases labor supply in the formal sector, which then mitigates the effects on wages of a higher outside option for those who already work in the formal sector. Analogous is the case where first-time job seekers or new entrants in the labor market are not eligible for benefits. The introduction of UIBs increases job creation for this group.

#### *Effects on Structural Change and Match Quality*

Little attention has yet been devoted by applied economists to investigating the macroeconomic, reallocation effects of unemployment benefits. This is a serious oversight, since a number of recent theoretical contributions point to potentially important effects of UIBs on job reallocation and labor productivity. General equilibrium models of the labor market à la Mortensen and Pissarides (1994), stochastic job matching models (Acemoglu and Shimer, 1999, 2000; Marimon and Zilibotti, 1999) suggest that UIBs act on both the job creation and job destruction margins, as well as on the quality of job matches and hence average productivity. Because of the critical role that job reallocation plays in the process of economic development,<sup>49</sup> understanding whether unemployment benefits in fact help or hinder structural change stands out as an important task with far-reaching policy implications.

Boeri and Terrell (2002) analyze the impact of unemployment insurance schemes and other institutional arrangements on labor reallocation in Central and Eastern European countries. The transition of former communist, centrally planned economies to market economies has

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<sup>48</sup> UIBs also might have an effect on the labor supply decisions of UIB recipients' family members. Cullen and Gruber (2000) find that in the U.S. the labor supply of wives is indeed strongly responsive to unemployment benefits received by their husbands. Their estimates imply that for each dollar of UI receipt wives earn up to 73 cents less.

<sup>49</sup> See, e.g., Caballero and Hammour (2000), Krizan, Haltiwanger, and Foster (2002), Bartelsman, Haltiwanger, and Scarpetta (2004).

involved a great deal of labor reallocation as jobs are destroyed in the public sector and a new private sector emerges. However, different countries have experienced different patterns of labor reallocation. Boeri and Terrell argue that such differences can be explained by different institutional arrangements in the various countries. In particular, a key role has been played by the presence (or absence) of unemployment benefit systems. Countries in Central and Eastern Europe assigned a more important role to safety nets such as unemployment benefits than did the countries of the former Soviet Union. Higher unemployment benefits acted as floors to wage distributions and prevented declines in wages. This implies that the adjustment in Central and Eastern Europe had to take place mainly on the employment margin, while wages adjusted more in the former Soviet Union. While the former group of countries witnessed a more sizable increase in unemployment, they also experienced a more substantial and effective reallocation of labor. In the former Soviet Union, wages were free to drop and labor reallocation was hampered since, in many cases, old, large, and inefficient firms could survive.

An important macroeconomic role of reallocation is also assigned to UIBs by political economy models. Those addressing the challenges of economic privatization, for instance, pointed to an additional role of UIBs in winning support among workers for outsider privatization and enterprise restructuring (Dewatripont and Roland, 1994; Blanchard, 1997). Models of political-economic institutional interactions in the labor market (Saint-Paul, 2000) suggest that unemployment benefits reduce the demand for employment protection legislation (EPL) (Boeri, Conde-Ruiz, and Galasso, 2006; Algan and Cahuc, 2006) since both institutions protect workers against labor market risk. "Flexicurity" configurations with more UIBs have less EPL, thus helping the job reallocation process because UIBs are more "mobility friendly" (Bertola and Boeri, 2002), and such configurations can therefore better accommodate large-scale restructuring (Blanchard and Tirole, 2003).

Boeri and Macis (2008) use annual panel data from a large number of countries to empirically evaluate the relationship between unemployment benefits and structural change. Unlike previous work assessing the effects of UIBs on labor market stocks, this study focuses on flows and relies on policy "experiments," notably the introduction of unemployment benefits in many countries, to implement a difference-in-differences methodology. To lessen the omitted variable and endogeneity problems, they estimate models including year fixed effects, country fixed effects, and country-specific time trends. They find a positive, sizable, and significant effect of the introduction of UIBs on job reallocation, arising mainly from the job destruction margin, although this effect fades away over time. The introduction of unemployment benefits was associated with about a 1.5 percentage point increase in the yearly rate of job destruction and a

2.7 percentage point increase in job turnover. This implies a positive effect on job creation as well, but this effect was not found to be statistically significant when estimated separately. UIBs also were found to induce sectoral shifts from agriculture (a proxy for low productivity jobs) to services (high productivity jobs).

New insights come from recent papers that have gone beyond estimating mean effects and have explored the potentially heterogeneous effects of UIBs. Using data from Portugal, Centeno and Novo (2006) examine the impact of unemployment insurance generosity on the distribution of the match starting wage and tenure by applying quantile regression techniques. They find evidence of a positive shift in the mean and variance of both variables. Their results indicate that more generous UI reduces the mass of the lower tail of match quality (shorter and lower-paid jobs), and increases the quality of matches available to all workers. There is no evidence from developing countries on this topic, and there is no reason to expect that results from the U.S. or other developed economies would extend to other contexts. Van Ours and Vodopivec (2008) exploit a natural experiment created by the 1998 change in the Slovenian UI system and find that the reduction of the potential duration of benefits did not have any significant impact on the type of the contract (temporary vs. permanent), the duration of the post-unemployment jobs, or the wage earned in this job. Unlike Centeno and Novo (2006), however, Van Ours and Vodopivec (2008) do not allow for heterogeneity, but assume homogeneous responses in the population.

#### *Effects of Monitoring Search Effort*

Theoretical and empirical research also points to the importance of specific design features of unemployment benefits related to eligibility and entitlement criteria, in addition to the level and duration of the benefits (Nickell, Nunziata, and Ochel, 2005). One crucial element of the UIB administration is the extent to which benefit recipients' job search activity is monitored. For given replacement rate and duration of benefits payments, closer monitoring of search effort and stricter sanctions for inadequate effort could reduce unemployment duration. Unfortunately, the administrative details of UIB schemes tend to be correlated with other characteristics of UIB systems, so that isolating the effects of one particular feature becomes empirically challenging. One way to overcome the difficulty of creating ideal "ceteris paribus" conditions is to run a randomized control trial (RCT) where the experimenter can change just one variable at a time while keeping everything else constant.

One interesting example in this vein is provided by Mickelwright and Nagy (2008), who study the effects of stricter monitoring of UIB recipients on unemployment duration by

conducting a field randomized experiment. The experiment took place in Hungary, where the authors, in collaboration with the Hungarian Ministry of Labor, randomly allocated claimants to a “treatment” and a “control” group. In the experiment, the “treatment” group was subjected to more frequent attendance at the local employment office (every three weeks), and was required to report job search activities (employers contacted, jobs applied for, etc.). The “control” group had to attend once every three months and did not have to report on search activities. The idea behind the experiment is that more frequent interactions with the employment office could induce greater search effort as well as increase exposure to a source of information about vacancies. The experiment lasted four and ½ months, and it involved 2,134 persons. The results of the experiment indicate that, overall, the tightened monitoring did not have any effect on unemployment duration. The only exception is represented by women older than 30. For this particular group, which represents about 28% of the sample,<sup>50</sup> the treatment was found to increase the hazard rate out of unemployment by 60% compared to the control group.

When interpreting the results (or absence thereof), it is important to point out that the control group regime was the norm at the time of the experiment. Therefore, the experiment is not a test of “monitoring” vs. “no monitoring” but rather a test of “tighter monitoring” vs. “light monitoring.” The finding that tighter monitoring did not seem to have an effect on most of the unemployed population might be due to several factors (most of which are discussed by the authors). First of all, it is possible that the unemployed already exerted high search effort so that the marginal return to extra search is small. Second, no formal sanctions were applied to those unemployed who reported no search activity. Third, the lack of results could be due to the ineffectiveness of the employment office in providing information on vacancies (except perhaps for females age 30 or older). If the unemployment office is an ineffective source of re-employment possibilities, more frequent interactions might actually be disruptive because they could take time away from the actual job search activity. Finally, the study mentions the possibility that stricter monitoring might encourage search in the formal sector and away from the informal one. In other words, it is possible that the zero net effect masks a change in the composition of the new jobs, away from informality and more in the formal sector of the economy.

More generally, these observations suggest that the effects of unemployment benefits are likely to be highly *implementation-specific* (DiNardo, 2007), i.e., they depend heavily on how the “treatment” is administered. Therefore, it is not at all clear to what extent the results from any

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<sup>50</sup> The authors do not report what fraction of the whole unemployed population in Hungary is represented by females of ages 30+.

single study can be generalized to other contexts. Another dimension that is missing from the picture is a discussion of the costs of stricter monitoring. In any program-evaluation exercise, analysis of the cost side is crucial for assessing the overall worthiness of a public policy intervention. This appears to be particularly important in the case at hand, given the apparent ineffectiveness of the treatment.

### *Concluding Thoughts*

The empirical literature finds a positive effect of unemployment benefits on unemployment duration, which has traditionally been taken as compelling evidence of the distortionary moral hazard effects of unemployment insurance. Recent evidence, however, suggests that the distortionary effects of UIBs on search effort might have been overstated. First, much of the evidence of “spikes” in unemployment exit rates at the point of UI benefits exhaustion may in fact be an artifact of using “time in the unemployment records” rather than “time to next job” (Card, Chetty, and Weber, 2007). Second, new evidence from the U.S. suggests that most effects of unemployment benefits on unemployment duration come from relaxed liquidity constraints rather than from distortionary substitution effects, and these effects are very much heterogeneous in the population. Because these developments are very recent, it is not surprising no work along those lines has been done using data from developing countries.

Further, a number of recent theoretical contributions point to potentially important effects of UIBs on job reallocation and labor productivity (Mortensen and Pissarides, 1994; Acemoglu and Shimer, 1999, 2000; Marimon and Zilibotti, 1999). Boeri and Macis (2008) provide evidence from a cross-country, difference-in-differences study that indicates effects of UIBs on job reallocation in countries that have introduced UIB schemes from scratch. However, more evidence is needed. Especially useful would be within-country studies examining turnover and productivity data based on firm or establishment-level information.

## **7. Discussion and Conclusions**

We have offered a review of empirical research on the impact of labor market regulations, with a focus on developing countries, and on four major areas of government intervention in the labor market: minimum wages, mandated benefits, dismissal costs, and unemployment insurance. For several reasons, our job was not an easy one: The literature on each of these institutions is vast,

the mechanisms through which each regulation interferes with market forces and affects the behavior of workers and firms are different, the theoretical predictions are often ambiguous, and the empirical findings vary widely depending on the data being used, the time periods considered, the methodology adopted, and the assumptions behind the methodology. Our focus was on developing countries, where the task of studying the effect of labor regulations is made difficult by weak compliance with regulations, often due to imperfect enforcement, the existence of a large informal sector and the presence of informal arrangements such as informal networks of insurance and credit. Our survey was not meant to be exhaustive, but rather to provide an assessment of the current state of the knowledge and highlight what we believe to be the most important knowledge gaps and areas where future research would be most fruitful.

As far as minimum wages and mandated benefits are concerned, our conclusion is that, to a large extent, “the jury is still out.” The empirical literature on the effects of minimum wages in developing countries delivers a diverse and complex picture, making it difficult to identify general patterns. The only exception is perhaps represented by the fact that most studies find adverse (albeit generally small) effects of minimum wages on the employment prospects of low-wage workers (young, females, and low-skill workers). Based on our review of the literature, we have identified several shortcomings of existing research and areas where we believe future research could be more fruitful. First, the vast majority of minimum wage papers have focused on the effects on employment, while generally neglecting other margins such as hours, labor force participation decisions, and labor reallocation. A higher minimum wage makes working for a wage relatively more attractive with respect to, say, home production, and could therefore increase participation in the labor force by individuals who would otherwise stay out (e.g., females). These margins should be examined. Another limitation of the literature is that it overlooks one rather obvious possible way that employers and workers have to mitigate the effects of MWs: substituting cash for non-cash compensation. There has been little, if any, research on the topic of whether “minimum wage jobs” in developing countries are significantly worse jobs in terms of safety conditions, non-cash compensation, on-the-job training, or career prospects (wage growth, etc.) or, relatively common in developing countries, payment in kind in the form of food, clothing, or lodging. Literature on effects of MW on household labor supply (e.g., child labor) is also limited. These areas also are ripe for further research. Finally, there are few studies which look directly at the impact of minimum wages on poverty, and those that do tend to take a short-term approach rather than assessing whether MWs are capable of helping poor workers and their families rise out of poverty permanently. Taking a longer-term perspective would be beneficial in several ways. First, it typically takes time for firms to adjust their input



mix in the face of the increase in the cost of production factors (Hamermesh, 1995). Studies based on long differences or studies that allow for lagged effects of MW have typically found larger effects compared to studies that just looked at contemporaneous effects (Neumark and Wascher, 1992; Baker et al., 1999). Second, and more important, we argue that the relevant notion of “inequality” is not cross-sectional inequality but rather lifetime inequality. Minimum wages or employment protection might improve the earnings of workers who remain employed, increasing their welfare and perhaps reducing cross-sectional inequality. However, some workers might be displaced because of the regulations (either by becoming unemployed, underemployed, or employed in the informal sector) and the consequences of such displacement (e.g., lost labor market experience, less on-the-job training) might translate into permanently reduced productivity and actually *increase* inequality in lifetime resources.

The literature on mandated benefits, separate from other labor legislation, is one area in the labor law and enforcement literature that has not yet been thoroughly and systematically explored. Conflicting findings in this literature do not yet allow for broad conclusions. However, mandated benefits are very heterogeneous across countries in terms of policy structure, implementation, and enforcement; this may prevent any sensible, broad conclusions from being drawn. It seems unlikely that mandated benefits as a whole, or even particular types of these, such as maternity leave or worker compensation insurance, will be called distortionary or efficient, or good or bad for an economy, in the near future. Perhaps a more attainable aim of this literature would be to analyze the impact of individual policies in individual countries, or across countries in cases of very similar policies for welfare, distributional, and informalizing effects. Such studies would attempt to find insight into what makes a particular policy affect one economy differently from any other and, eventually, to guide in sensible policy formation. To aid in analyses of these policies, more modeling should be done based on the different ways policies are funded and benefits are allocated. These models should be extended to allow for market imperfections: Especially in the case of fixed-cost benefits, if there is monopsony, mandated benefits might be beneficial to workers, even in the simple, static framework. Also, these models should be extended into a dynamic, general equilibrium framework, eventually to include heterogeneity and other relaxations of standard assumptions. As with many research areas in labor legislation, more analysis could be done to assess the impact of mandated benefits on the growth of informality.

Unemployment benefits and employment protection legislation are two instruments designed to achieve a similar purpose: protecting workers from the risk of job loss. In fact, these institutions can be seen as substitutes for each other: Having a generous unemployment insurance system reduces the need for firing restrictions, and vice versa. This trade-off is visible over a

cross-section of European countries (Boeri, Conde-Ruiz, and Galasso, 2006). Because most developing countries lack the resources (administrative capabilities) to provide comprehensive unemployment insurance for their workers, protection to workers often takes the form of stringent employment protection (Botero et al., 2004). The evidence we have reviewed in this paper, however, seems to indicate that because EPL is very detrimental to job turnover and job reallocation, it blocks an important channel for aggregate productivity growth (Foster, Haltiwanger, and Krizan, 1998).

On the other hand, unemployment insurance appears to be more “mobility-friendly” (Bertola and Boeri, 2002), and can better accommodate large-scale restructuring (Boeri and Terrell, 2002; Blanchard and Tirole, 2003; Boeri and Macis, 2008), although more evidence is needed. Especially useful would be within-country investigations of the impact of policy changes on firm- or establishment-level turnover and productivity.

One major limitation of existing research is the tendency to focus on one institution at a time while largely ignoring institutional interactions. In our review, we have emphasized that the effects of a given labor market regulation are likely to vary depending on what other labor regulations are in place in a particular country, the extent to which product markets are also regulated, and on the development of formal financial markets. Taking into account interactions of labor market and other regulations is also important when evaluating or forecasting the effects of labor market or product market reform. Because labor and product markets are, of course, interrelated, the impact of reforms in one market is likely to be affected by how heavily other markets are regulated, as well as by whether other markets are being reformed at the same time. Theoretically, labor market and product market reforms could complement and reinforce one another, or substitute for each other. Recent work by Fiori et al. (2007) looked at the interaction of labor market and product markets reforms in OECD countries, finding that employment gains from product market reforms (privatizations, liberalizations) have been greater in countries with highly regulated labor markets. At the same time, they also find evidence that in countries that have deregulated their product markets, workers’ bargaining power declined over time. The study of institutional interactions is an area in which there is much room for expansion.

We also have highlighted that analysis of interventions in developing countries should take into account general equilibrium effects. For instance, the funding of UBs and other transfers creates tax displacement effects, which in developing countries may be larger than elsewhere. One of the channels through which the effects of a policy could affect individuals not directly reached by the policy is by informal arrangements, such as family networks of credit and insurance. A recent study by Angelucci and DeGiorgi (forthcoming) examines how cash transfers

to eligible households in Mexico indirectly affected the consumption of non-eligible households living in the same villages, and find significant spillover effects which operate through informal insurance and credit markets. The findings of Angelucci and DeGiorgi highlight both the importance of taking a general equilibrium approach when studying the effects of transfer programs in developing countries, and the potential payoffs to building household-level datasets that integrate labor force survey information with data on consumption, savings, and access to formal and informal credit markets.

The balance of the empirical evidence suggests that employment protection should be reduced in developing countries, while social protections such as unemployment benefits should be increased. As Boeri and van Ours (2008) put it, “*UBs should not be considered as a sort of luxury good to be offered only by rich countries (...) The most relevant issues do not concern whether or not a country should have a UB system, but how the system should be designed along its several dimensions.*” The challenge for policymakers in developing countries is to design instruments of social protection so that they achieve their intended goals while minimizing the inefficiencies (see Hamermesh, 1992; Boeri and van Ours, 2008).

Research on the effects of labor market reforms in developed countries has shown that reforms (e.g., those reducing employment protection) have winners and losers. Because the implementation of reforms is often difficult to achieve due to the resistance of the “insiders” (i.e., those who would lose from deregulation), the political economy of reforms suggests that some sort of “compensation” needs to be offered to those who are hurt by the reform. For instance, the literature on the political economy of labor market reforms indicates that reforms that weaken employment protection are much more likely to be accepted if they are implemented contemporaneously with increased generosity of unemployment insurance (Boeri, Börsch-Supan, and Tabellini, 2002).

Another way of easing labor market transitions is to offer re-training programs to those who lose their job in the aftermath of a reform. However, since training programs are costly, it becomes important to properly evaluate whether their returns are sufficient to cover their costs. While “*policies combining wage subsidies with job development, training, and job search assistance efforts appear to have been somewhat successful in improving the employment and earnings of specific targeted disadvantaged groups*” in the United States (Katz, 1998), the evidence from developing countries is scant and does not support any one conclusion. In recent years, several countries in Latin America have offered training programs for particular groups of workers, mainly low-income, low-skill youth. Several of these programs have been evaluated econometrically. Unfortunately, the research on the effect of training programs on labor market

outcomes does not provide clear-cut results. In fact, even randomized trials such as Card et al. (2007) and Attanasio, Kugler, and Meghir (2008) obtain conflicting results on the benefits of training programs. This suggests that the results from any particular study often cannot be immediately extrapolated to other contexts. This is especially the case for policies that fall into the category of *implementation-specific effects* (DiNardo, 2007) in that they depend heavily on the way the “treatment” is administered in practice and on the specificities of the context. Thus, rather than aiming at answers or prescriptions with general validity, the focus should be more on a case by case basis, with programs started small, carefully evaluated, and then expanded, adjusted, or discontinued depending on the results of the evaluations. The most important caveat to this approach would be that large-scale programs can be expected to have general equilibrium effects which might go undetected in small-scale interventions. As a way of gauging such general equilibrium effects, randomization in “pilot” experiments should be done not only across units (e.g., villages) but also within units (Angelucci and DeGiorgi, 2008).

We hope this resource has, in addition to summarizing existing work on the topic of labor regulation in developing countries, also shed a light on potential directions for future work. The preceding conclusions have described several areas in which the literature on labor regulation and enforcement can be expanded. Given the depth and breadth of this literature, it is both surprising and encouraging that such vast areas for new study still exist. Advances over the last decade in quality and availability of data, theoretical and econometric advances, and wider interest in developing nations due to major economic transitions and globalization have paved the way for new work in this area. As economies continue to become more integrated, such studies also become ever more important.

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## Summary Findings

We provide a critical review of the empirical evidence on the effects of labor market regulations in developing countries, and we highlight the main knowledge gaps and the directions for future research. Our analysis focuses mainly on minimum wages, unemployment insurance, employer-provided benefits, and employment protection legislation. We pay equal attention to the efficiency and distributional effects of regulations. Even though the focus of our analysis is on the effects of labor regulations in developing countries, we refer to the evidence from developed countries whenever it proves relevant, and when no evidence from developing countries is available. One of the main themes of this critical survey is that the specific context of developing countries is often radically different than that of developed economies, and that neglecting these specificities in the theoretical models can lead to incorrect predictions and misguided interpretations of the empirical findings.

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