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A SURVEY OF THE EFFECTS OF THE MINIMUM WAGE IN LATIN AMERICA

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The available empirical minimum wage literature, which is mostly based on US evidence, is not very useful for analyzing developing countries, where the minimum wage affects many more workers and labour institutions and law enforcement differ in important ways. The main contribution of this paper is to survey the existing minimum wage literature for Latin America.

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INTRODUCTION

Minimum wage legislation has extensively been used in developing countries as a social policy ostensibly to improve the well being of the poor. There are two important questions if that is to be achieved: Can minimum wage legislation actually help the poor? Are there alternative policies that would be more effective? This study is concerned with the first question. The minimum wage legislation will help the poor if it increases wages but does not destroy jobs and does not cause inflation. Therefore, the first step to evaluate the potential use of the minimum wage against poverty and inequality is to survey the literature for the available estimates on the wages, employment and price elasticities.

The international literature on minimum wage is scanty on non-US empirical evidence. "No single empirical study of an economic phenomenon is ever highly convincing" (Hamermesh, 2002, p. 4). Many data points are needed – many and independent data points are needed. Using non-US data is an unbiased way of extending the understanding of minimum wage effects and assessing the robustness of findings for the US. Hamermesh (2002, p. 15) argues for increased reliance on non-US data and policy evaluations: "policies like hours legislation and the minimum wage provide especially fruitful areas in which to apply the results of studying foreign experiences to the US". In particular, Hamermesh (2002) calls attention for the evidence from developing countries, which is greatly lacking in the literature.

This study represents an important contribution to the literature because it surveys the current understanding on the effects of the minimum wage in developing countries. This is crucial if the minimum wage is to be used as a policy to help poor people in poor countries. For example, minimum wage increases in Brazil, a key developing country, are large and frequent, unlike the typically small increases studied in most of the literature (Deere et al, 1996; Hamermesh, 2002; Castillo-Freeman and Freeman, 1992). Studying such increases allows a better possibility of observing the wages, employment and price effects predicted by theory and thus the link between empirical data and

theoretical models of the minimum wage. Furthermore, Hamermesh (2002) remarks that foreign experiences are especially promising if they generate exogenous shocks (an alternative to reliance on statistical methods to circumvent the problems arising from endogeneity), as in Brazil over the past 30 years.

1. LITERATURE SURVEY

This study surveys the available estimates of the effect of the minimum wage on wages, employment and prices. The signs and magnitudes of these effects depend on the minimum wage level and enforcement, as well as on the specific characteristics of the labour market where it is imposed (Freeman, 1996 and Dolado et al., 1996). Generally: (a) wage effects are expected to be positive and larger at the bottom of the wage distribution; (b) employment effects are expected to be negative, although there has been a debate in the literature where empirical evidence and theoretical arguments challenge such expectations; and (c) price effects are expected to be positive, at least in minimum wage labour intensive sectors.

Wage, employment and price effects are discussed in turn below. For each of them, theoretical arguments are laid out, the US and developed country available literature is briefly surveyed, and the available evidence for developing countries, in particular for Latin America, is presented.

1.1 WAGE EFFECTS

The minimum wage affects other wages in two ways. First, there is a direct impact on those between the old and the new minimum wage. Second, there is an indirect impact on those above (and below) the new minimum wage, the so-called spillover effects. This results from wage comparisons, inflationary expectations and labour substitution (Gramlich, 1976; Sellekaerts, 1981; Grossman, 1983). On the one hand, firms increase wages in response to a minimum wage increase because demand for more skilled workers increases and because workers' effort is a function of relative wages (Grossman, 1983). On the other hand, workers bargain for higher wages in response to a minimum wage increase because they want to maintain their relative wages. Thus, the effect of a binding minimum wage on other wages is positive. Furthermore, different worker occupations have different comparison groups

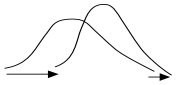
(Sellekaerts, 1981; Grossman, 1983; Akerlof, 1982 and 1984). As a result, the magnitude of the wage effects varies across the wages distribution.

In sum, a minimum wage increase has two effects: (a) it shifts the distribution to the right, increasing its mean, and (b) it changes the shape of the distribution, reducing its variance. This is because larger elasticities are expected at lower percentiles, compressing the distribution. Other things equal, (a) plus (b) implies a non-parallel shift of the distribution to the right,[1] reducing wage inequality (Card and Krueger, 1995; Brown, 1999; Fraja, 1996). Manning's (1994) model predicts a right shift of the whole distribution.

Although the potential distributional consequences of the minimum wage were long noted by Stigler (1946), not as much research has been done on the effects of the minimum wage on the wage distribution as on employment. Brown (1999) surveyed much of empirical evidence available, mainly for the US, on the effects of the minimum wage on the wage distribution and the associated spillover and compression effects. He discussed the work of Gramlich (1976), Converse et al. (1981), Grossman (1983), Meyer and Wise (1983a), Katz and Krueger (1992), Dickens et al. (1994a), Machin and Manning (1994), Card and Krueger (1994 and 1995) and Dinardo et al. (1996).[2] As predicted by theory, these studies found evidence of spillover effects and compression on the lower tail of the wage distribution. This is in line with the evidence from earlier and more recent studies as well as with studies for other countries, which are discussed below.

The Department of Labor studies published several studies on the effects of the 1961 and 1967 US minimum wage increases (FLSA, 1965 and 1969) using simple comparisons of average and other wages across industries before and after the increase, as well as computations of the number and proportion of workers whose wages were

[1] This figure illustrates a non-parallel shift to the right:



[2] A related literature is on the effect of the minimum wage on inequality. See the original article by Stigler (1946). Also see Gramlich (1976), Kelly (1976) and Johnson and Browning (1983) for the link between poverty and low pay, and more recently, Burkhauser and Finegan (1989), Horrigan and Mincy (1993), Dinardo et al. (1994), Card and Krueger (1995), Machin and Manning (1996), Neumark et al. (1998), Teulings (1998), Lee (1999) and Neumark and Wascher (2002). And for a survey, see Brown (1999).

increased. This evidence was regarded as supporting a compression effect at the bottom of the distribution.

Gramlich (1976) used US time series data from the 50s to the 70s to indirectly estimate minimum wage spillover effects. He compared the effect of the minimum wage on the wages of those directly affected with the its effect on the average wage and found the second to be roughly twice as large as the first one. He attributed the difference to spillover effects on the wage of those indirectly affected by the minimum wage, although his data did not enable him to determine where in the distribution such effects occur. As noted above, he concluded that these effects derive either from pressure from unions and workers above the minimum wage on wage bargaining or from more traditional substitution by employers towards more skilled workers. Farber (1981) used US data across industries from 1954 to 1979 to estimate the effect of the minimum wage on union negotiated wages. He estimated wage effects for different wage levels and found supporting evidence of small spillover effects, which further declined as higher wages were considered. Thus, the suggestion of Gramlich (1976) that spillover effects derive from pressure from unions was not supported by the data, although his results can still be attributed to more traditional labour substitution.

Earlier studies of the minimum wage effect on wage inflation often use general equilibrium model analysis, where the effect of the minimum wage on a number of variables is estimated. Sellekaerts (1981) reviewed eleven such studies, which found positive wage effects. She criticized these studies and attempted to overcome some problems by inserting a modified wage determination equation into the MIT/PENN/SSRC macro model of the US economy, which she estimated using 1974 to 1979 US time series macro data. One of the main contributions of this study is that this wage equation accounts for wage increases that would have taken place regardless of changes in the minimum wage legislation. That is because unless the minimum wage increase causes substantial gains in real terms, the legislation might be no more than a change in the timing of the increases. She reported evidence supporting spillover effects. Sellekaerts' (1981) study is one of eight studies published on a special volume on wage and price inflation by the US Minimum Wage Study Commission. Three of these studies are worth noting, Farber (1981), discussed above, Cox and Oaxaca (1981) and Converse et al. (1981). Cox and Oaxaca (1981) used US data from 1974 to 1978

aggregated at industry and macro levels to simulate the effect of freezing the minimum wage at its 1974 level on employment, output, wages and prices using a general equilibrium model of the US. They provided evidence of increase in the aggregate real wage bill, but not explicitly on compression or spillover effects, although the later were implicitly assumed in their model. Converse et al. (1981) used their own designed 1980 survey on US firms to ask questions on low wage workers characteristics and how their wages were affected by the minimum wage increase. Essentially they tried to identify whether the minimum wage increase triggered wage increases in addition to those that would have happened in response to overall wage increases, as did Sellekaerts (1981). However, even though they report evidence of spillovers, the timing of their analysis was such that some of the observed spillovers were indeed a response to overall wage increases.

Grossman (1983) estimated spillover effects using US non-manufacturing industries data for several occupations from the 60s to the mid-70s. She used an efficiency wage type model to introduce relative wages effects into the analysis in addition to the standard neoclassical effects (Neumark et al., 2000). As noted above, she argued that spillover effects derive from firms' response to a minimum wage increase because workers' effort is a function of relative wage and because firms demand more skilled workers. Although her results were not very precise, they suggest that the wage distribution becomes more compressed immediately after an increase (relative wage effects), but the wage structure gradually returns to its original state (substitution effects). Freeman (1996) remarks that market power of workers above the minimum wage might offset the redistributive impact of the increase.

Meyer and Wise (1983a) used 1978 US individual level data to estimate a parametric wage distribution. They first estimated the wages that individuals would receive in the absence of the minimum wage, as a function of usual explanatory variables (schooling, experience, etc.) and a normally distributed error. They then estimated the effect of the minimum wage on the distribution at or below the minimum wage, but assumed no spillover effects above it. This was criticized by Dickens et al. (1998), not only because of evidence of spillover effects in the literature, but also because this assumption thins the distribution just above, and thickens it below the minimum wage, overestimating the employment loss (Card and Krueger, 1995; Brown,

1999). Furthermore, Dickens et al. (1998) and Card and Krueger (1995) note that specific assumptions about the probabilities of those below the minimum wage (of loosing their jobs, of earning a minimum wage or of earning below it) constrain the employment effect to be negative. Dickens et al. (1998) used individual level data from the mid-70s to the early 90s in their estimations for the UK and concluded that although the method is ingenious, the estimates are very sensitive to assumptions on the wage distribution functional form and on how far spillovers are allowed to extend in the wage distribution. Other versions of Meyer and Wise (1983a and 1983b) models have also been estimated using 80s Dutch individual level data by van Opstal (1990), Van Soest (1989 and 1994) and Van Soest and Kapteyn (1989); and also using 80s Canadian individual level panel data by Green and Paarsche (1996).

Katz and Krueger (1992) and Card and Krueger (1994 and 1995) estimated spillover effects following the 1990 and 1991 US minimum wage increase. They applied difference-in-difference and regression analysis estimation to cross-state data on teenager wages and their own data on the fast-food industry. In a similar spirit to Converse et al. (1981), they used their own designed survey and provided detailed information on firm's responses to the increase. They found evidence supporting spillover effects, which however died out fairly quickly higher up in the wage distribution, and report evidence of compression effects. Spriggs and Klein (1994) conducted a similar experiment to Katz and Krueger (1992), differing only in the timing between the change in the minimum wage and the follow-up survey. Their findings suggest evidence supporting spillover effects, which they associate to firm's attempts to maintain their wage differentials, in line with Grossman (1983).

Dinardo et al. (1996) used US individual level panel data for the 70s and 80s and a semiparametric approach to analyze the contribution of the minimum wage to changes in the wage distribution. They estimated the wage distribution that would have prevailed in 1988 had the 1979 real minimum wage and the (shape of) wage distribution (conditional on schooling, experience, etc.) had not changed. They then compare this counterfactual with the observed wage distribution and conclude that the real minimum wage decrease over the 80s explains a substantial proportion of the increase in wage inequality. Teulings (1998) criticized Dinardo's et al. (1996) strong underlying assumptions of no employment loss and no spillovers and suggested a new

methodology requiring no assumptions on how the minimum wage affects the wage distribution. He used US data from the 70s to the 90s and found evidence, qualitatively not much different from Dinardo et al.'s (1996), of compression at the bottom of the wage distribution. Lee (1999) also used a similar approach to Dinardo et al. (1996) and US individual level data for the 80s to analyze the effect of the minimum wage on the wage distribution (but used state level variation) and again found evidence of compression effect. Essentially, these studies tried to separately identify the impact of the minimum wage from "latent" wage dispersion growth during the 80s and concluded that the minimum wage accounts for much of the rise in dispersion in the bottom of the wage distribution.

Neumark et al. (2001) used US individual level data for the 80s and 90s and a methodology related to the one in Currie and Fallick (1996) and Abowd et al. (1999), who estimate employment effects, and to the one in Linneman (1982), who estimate effects on employment, hours and income.[3] The idea was to use the wage structure that existed prior to the change, where wages are modelled as a function of usual variables (schooling, experience, etc), to predict the wages of the unemployed. This counterfactual wage distribution is then used to estimate what would be the effect of a minimum wage increase.[4] In line with the above evidence, Neumark et al. (2001) also found positive wage effects on the bottom of the wage distribution. However, for those above the minimum wage, these effects were found to be later neutralized (lagged wage effects were negative), as in subsequent periods the supply for more skilled workers increases more than the demand. This is in line with Grossman's (1983) findings above.

Developed countries – Aside the non-US studies discussed above, which estimated versions of the Meyer and Wise (1983a and 1983b) approach, most of the remaining developed countries evidence is for the UK. Borooah and Forsythe (1997) used UK individual level panel data for the late 90s and also found positive effects on average

[3] Also, see Neumark et al.(1998) that estimates the effects of the minimum wage on the distribution of family income.

[4] This approach might overestimate the employment effects of the minimum wage (see Section 1.3) because it implicitly assumes that all observed disemployment is caused by the minimum wage. First, by predicting the wages of all unemployed, full employment is assumed. Second, other things are changing simultaneously with the minimum wage change that might also be causing disemployment. The comparison of the counterfactual with the observed wage distribution after the change then attributes all disemployment to the minimum wage, when in fact, this might be a combination of the already

wages. In contrast, Bell and Wright (1996) used UK individual level panel data for late 70s and 80s and found no effect on average wages. This does not suggest absence of spillover effects, but rather that these effects might not go very far up in the wage distribution. However, as discussed above, aggregate data makes it impossible to determine where in the distribution such effects occur. Dickens et al. (1995) used UK agriculture wage councils data in the early 90s to estimate the effect of the minimum wage in the first and last deciles, median and mean of the wage distribution and found that the minimum wage affects the average level and earnings distribution. Gosling (1997) used microdata for the early 90s to illustrate the positive effects of the minimum wage on other wages. Dickens et al. (1999) attempted to further determine where in the distribution spillovers occur. They used UK wage councils data on several industries from the mid-70s to early 90s to estimate the effect of the minimum wage at every decile of the wage distribution. They found larger elasticities at the bottom of the distribution and conclude that the minimum wage significantly compressed the wage distribution. Machin et al. (2003) and Dickens and Manning (2003) studied the effects of the introduction of the UK national minimum wage in April 1999 using their own data on the residential care homes industry and regression analysis. While the first study found evidence supporting a strong compression effect, the second found evidence of only of a minimal impact on wage inequality. Dickens and Manning (2002) used individual level data from the mid-70s to the 90s and two estimation procedures: first, a method similar in spirit to Dinardo et al. (1996), Teulings (1998) and Lee (1999), whereby the wages of non-employed workers are predicted from the wages of those employed and the wage distribution thus obtained is used to estimate the impact of the introduction of the minimum wage; second, a method that re-weights the wages density using those employed only. They also found evidence supporting a limited impact of the minimum wage on the wage distribution. Dickens and Manning (2002 and 2003), as well as Metacalf (1999), believe that these results stem from the low introductory level of the minimum wage. Begue (1978) used 1978 French cross section data and found evidence supporting positive effects of the minimum wage on other wages. Rosa

unemployed who simply remain unemployed (for reasons other than the minimum wage), other shocks causing increase in unemployment, and finally the minimum wage causing increase in unemployment.

(1981) used French aggregate time series for the 60s and 70s and found positive effects on average wages.

Developing countries – The available evidence on developing countries is very limited. In studies for Latin America, not only the compression effect is a lot stronger, but also observed in both formal and informal sectors. Gregory (1981) used industry aggregate data for the late 60s to early 70s and also found some evidence supporting a positive effect of the minimum wage on average wages in Costa Rica. El-Hamidi and Terrell (2001) used individual level panel data from the late 70s to the early 90s and standard techniques (coefficient of variation, Gini coefficient and Theil mean deviation) to study inequality. They found supporting evidence of a compression effect attributable to the minimum wage for Costa Rica's formal sector but not for the informal sector. Maloney and Mendez (2003) used late 90's panel data for eight South American countries (Argentina, Bolivia, Brazil, Chile, Colombia, Honduras, Mexico, and Uruguay) to estimate wage Kernel distributions and regard their evidence as supportive of a substantial compression effect in both sectors. Lora and Henao (1997) used late 80s and early 90s aggregated data and a general equilibrium model to simulate the effect of changes in the minimum wage on wages and find a positive effect in Colombia. Edwards and Cox-Edwards (2002) develop a model of the labour market in emerging economies, where formal and informal sectors co-exist, that predicts that minimum wage increases increases wages in the informal sector. In contrast, Angel-Urdinola (2002) used Colombian individual level panel data from the late 70s to the 90s and extended Dinardo et al.'s (1996) approach to include employment effects and found that wage inequality increased.

Although the evidence on developing countries is very limited, a few studies are available for Brazil. The earlier literature indirectly detects spillover effects in a similar fashion to Gramlich (1976), Bell and Wright (1996) and Borooah and Forsythe (1997). Cunha and Bonelli (1978), Bacha and Taylor (1978), Bacha (1979), Camargo (1984), Drobny and Wells (1983a), Wells and Drobny (1982), Reis (1989), Velloso (1988 and 1990), Cacciamali et al. (1994), Lemos (1997), Carneiro and Faria (1998), Soares (1998a and 1988b), Carneiro and Hanley (1998), Barros and Lemos (1998) and Foguel et al. (2000 and 2001) used aggregate (or across regions) time series data and sample periods varying across studies from the late 40s to the late 90s to estimate the effect of

the minimum wage on mean and median wages. These studies always found positive effects, sometimes larger for the informal than for the formal sector. This suggests that spillovers extend high enough in the wage distribution to significantly affect the mean and median. Furthermore, as the minimum wage cuts deeper into the informal sector wage distribution, spillover effects are larger there. However, as noted above, this approach has two drawbacks. First, it makes it impossible to determine where in the wage distribution spillovers occur. Second, its identification strategy is the one predominant in the early time series literature, whereby the minimum wage effect is not fully, but rather *ad hoc* identified (see Section 1.2). Neri (1997a) used individual level panel data and difference-in-difference estimation to analyze the 42.86% minimum wage increase in 1995 and found larger wage effects for the informal sector.

More recent literature used individual level panel data and a sample period varying across studies from the early 80s to the late 90s to estimate the effect of the minimum wage throughout the wage distribution. Neumark et al. (2003) used individual level panel data for the mid to late 90s, which excludes high inflation periods, and regression analysis. They estimated the minimum wage effect at lower deciles of the wage distribution in a similar spirit to Dickens et al. (1995 and 1999), and found spillovers in the bottom of the formal sector wage distribution. In contrast, Fajnzylber (2001) used individual level panel data for the 80s and 90s and followed Neumark et al. (2001) methodology, and found significant wage effects throughout the wage distribution for both formal and informal sectors. This might be because whereas Neumark et al. (2003) uses low inflation sample period, Fajnzylber (2001) used high inflation sample period as well, for which larger spillovers are expected. For example, Soares (2002) also used mid to late 90s individual level data and a similar approach to Meyer and Wise (1983a), together with regression analysis at various points across the wage distribution in a similar approach to Dickens et al.(1995 and 1999). He found substantial wage effects in the bottom but not in the top of the distribution, reporting evidence supporting compression effects. Similarly, Corseuil and Morgado (2001) and Corseuil and Carneiro (2001) used individual level panel data also for the mid to late 90s. They used an approach similar in spirit to Dinardo et al. (1996), Teulings (1998) and Lee (1999), although they did not discuss the estimation procedure for their counterfactuals. They found supporting evidence of compression effect using both the full and formal sector

sample (from which they seemed to want to infer conclusions about the informal sector wage effects, although they did not discuss results for that sector). All these studies found evidence of substantial spillover effects and compression effects, as reported in Carneiro's (2002) recent survey. The compression effect is a lot stronger and spillover effects extend higher up in the wage distribution for Brazil than for the US and they are sometimes stronger in the informal sector. Furthermore, these effects are limited to the bottom of the distribution during low inflation periods. In contrast, McIntyre (2002) used individual level data for the 80s and 90s and regression analysis where the empirical equation was delivered by a Two-Sectors model to estimate the probability of workers and firms moving from the formal to the informal sector. One of the pieces of evidence he reported, relevant to this survey, is that the minimum wage contributed to raise wage inequality at the bottom of the distribution in poor regions in Brazil.

Despite limited evidence, the spillover and compression effects of the minimum wage seem to have been established in the literature. This study follows recent strands in the international literature that try to uncover the wage distributional effects of the minimum wage to access the compression effect for Brazil and discusses a number of conceptual and identification questions crucial to estimating such effect.

1.2 EMPLOYMENT EFFECTS

Once the expected compression effect has been established, the policy potential of the minimum wage lies on its employment effects. The aim is to change the shape of the wages distribution but not to destroy jobs. Employment (E) is affected not only by the direct change in the minimum wage (MW) itself, but also by the indirect spillover effects on other wages (W) (see Section 1.1), i.e. $E = f[MW, W(MW)]$.

The effect of the minimum wage on inequality and poverty is compromised if wages and employment elasticities have opposite signs. On the one hand, if the employment elasticity is non-negative or non-significant (statistically zero), the effects of the minimum wage on inequality and poverty depend on the effect on the wage distribution. This is for example, the identifying assumption underlying Dinardo et al.'s (1996) paper. In other words, in the absence of employment losses, minimum wage

increases just transfer money from one group to another (Freeman, 1994 and 1996; Deere et al., 1996). On the other hand, if the employment elasticity is negative, this has to be accounted for when accessing the effects of the minimum wage on inequality and poverty. Furthermore, in the case of a negative elasticity, the speed and magnitude of the increase play an important role. For example, a more desirable effect might be reached with successive small increases rather than a single large increase that could lead to lay offs (Card and Krueger, 1995; Dickens et al., 1997; and Machin and Manning, 1994, 1996; Sobel, 1999).

There is currently not much consensus on the direction of the employment effects. The old debate between the neoclassical Stigler (1946) and the revisionist Lester (1946), dormant since the early 80s in an apparent consensus, has been re-awakened. The 80s consensus was of negative significant but modest effect: increasing the minimum wage by 10% decreased employment by 1%-3% (Brown, et al., 1982). On the one hand, Neumark and Wascher (1992 and 2000), Kim and Taylor (1995), Deere et al. (1995 and 1996), Currie and Fallick (1996) and Burkahuaser et al. (2000a), among others, found results consistent with the standard model prediction of a negative employment effect. On the other hand, Card and Krueger (1995 and 2000), Machin et al. (1993 and 2003), Machin and Manning (1994), Bernstein and Schmitt (1998) and Dickens et al. (1999), among others, challenge such a prediction, unable to find disemployment effects. In a recent survey, updating Brown et al.'s (1982) survey, Brown (1999, p.2154) remarks: "the minimum-wage effect is small (and zero is often hard to reject)". While there is yet no consensus, small employment effects, clustered around zero, are becoming prevalent in the literature (Freeman, 1994 and 1996; Brown, 1999).

Explanations to non-negative effects range from theory to empirical identification and data issues (Card and Krueger, 1995; Brown, 1999). On the one hand, from a theoretical point of view, attempts to reconcile the recent debate include inadequacy of the competitive model, offsets, hours worked versus employment rate, etc. The standard neoclassical model predicts a negative elasticity if the minimum wage is fixed above the equilibrium wage level. This is because, given prices, in order to afford higher wages, employers would have to lay off workers and/or reduce the hours per worker load. However, theory allows for non-negative employment effects in a monopsony framework. A minimum wage increase need not reduce employment, and

might increase it if wages are lower than productivity. In this case, the minimum wage at first increases employment, but will eventually decrease it (for large enough minimum wage increases the theory unambiguously predicts negative effects). The point at which the sign switches depends on the elasticity of the labour supply (Brown, 1999).

The monopsonistic model is general enough to allow the minimum wage to have either a negative or a non-negative effect on employment (Brown et al., 1982; Rebitzer and Taylor, 1991; Card and Krueger, 1995; Bhaskar and To, 1999; Brown, 1999). Furthermore, it can explain the existence of a spike in the empirical wages distribution and it allows different firms in the same market to be differently affected by the minimum wage. It also distinguishes between the labour supply elasticity of an individual firm (which determines its monopsony power) and the labour supply elasticity of the market (which determines the employment effects of an increase in the minimum wage) (Dickens et al., 1999). This is important because the increase in employment of a firm facing an upward-sloping supply curve is not informative of what happens to aggregate employment. The aggregate labour supply curve may be steeper than the one faced by an individual firm, because the latter reflects worker migration across regions and sectors (Dolado et al. 1996). However, some are sceptical that firms that hire minimum wage workers have monopsony power (Card and Krueger, 1995; Brown, 1999). Card and Krueger (1995) dress up the monopsony model to try and re-establish its relevance for actual minimum wage markets.

On the other hand, from an empirical point of view, attempts to reconcile the recent debate include: inappropriate econometric techniques (difference-in-difference, panel data, etc.), inappropriate empirical modelling (regional and time effects, serial correlation, endogeneity, validity of instrumental variables, etc.), data flaws, data filtering, etc. The available evidence on the employment minimum wage effect most relevant for this study is now briefly discussed for time series, cross-section, panel data, and difference-in-difference estimation. More comprehensive surveys can be found in Brown et al. (1982) and Brown (1999).

Time Series – The apparent consensus of the early 80s, documented in Brown et al.'s (1982) comprehensive survey, was mostly supported by time series studies. A national minimum wage – most of the literature is available for the US, where until

recently the minimum wage was national – coupled with the availability of national time series data naturally motivated such studies. The main criticism on these studies, however, is that the time series variation alone does not fully identify the minimum wage coefficient. Identification depends on how time is modelled – the so-called *ad hoc* identification. On the one hand, no restriction on time modelling requires one dummy for each time period. In this case, the minimum wage effect is not identified at all because of perfect multicollinearity between the time dummies and the minimum wage (Brown et al., 1982; Card and Krueger, 1995; Burkhauser et al., 2000a; Dolado et al. 1996; Lee, 1999). On the other hand, full restriction on time modelling requires a linear trend. This means that deviations from the linear trend are assumed to be entirely due to the effect of the minimum wage on employment, i.e. the minimum wage is assumed to be the only macro variable affecting employment nonlinearly. In this case, the minimum wage effect is not fully identified because its effect cannot be distinguished from the effect of other variables on employment.

Thus, the identification of the minimum wage coefficient depends on the nature of time modelling, i.e. how the effect of the other macro variables on employment is modelled, which is another way to say that the minimum wage coefficient is not robust to how time is modelled. Or put differently, the counterfactual is not clear; periods when the minimum wage was increased are compared to (counterfactual) periods when the minimum wage was not increased. However, other things changed over time and this strategy does not isolate the effect of the minimum wage from the effect of other things on employment. The implicit assumption is that when controlling for other things, employment is the same over time if the minimum wage was constant. This is a strong assumption and there is no way to test it using time series variation alone (Card and Krueger, 1995). To ensure identification, the minimum wage would have to vary over time and across regions, as discussed below, in which case even if time was modelled with a full set of dummies, the minimum wage coefficient would still be identified. Therefore, identification of the effect of the minimum wage separately from the effect of other macro variables on employment requires regional variation if no restriction on time is imposed.

On the assumption that teenagers are one of the groups most affected by the minimum wage, the typical time series model relates teenage employment (rate) to the

minimum wage (variable) plus controls by a linear relation, where the variables are often expressed in logarithms. The most common minimum wage variable is the ratio of the minimum wage to average wage adjusted for coverage – the Kaitz index (Kaitz, 1970). The controls include time modelling variables (for example a trend), a measure of aggregate demand (for example male adult employment), and often some supply side variables (reflecting the focus on teenagers, other controls are usually relative shares of teenagers in the labour force). Lags and leads are not often included, unlike other labour demand studies, which usually allow for non-contemporaneous adjustment. This is justified by two main arguments: because high turnover in low wage industries allow for adjustment in employment simply by no replacement of quitters; and because minimum wage changes are announced in advance allowing time for the adjustment (Brown, 1999).

There is some agreement that demand side variables should be held constant, but less agreement on whether supply side variables should be included as controls and, if so, which ones. The debate is about whether a reduced form or a demand equation is estimated, depending on whether the minimum wage is binding or not (Neumark and Wascher, 1992, 1995b, 1996). For those who earn a minimum wage, employment is demand determined, but for those who earn more, relative supply and demand matter. Nevertheless, even if employment is demand determined, truly exogenous supply side variables do not bias the coefficient, which will be however, more inefficient (Brown et al., 1982 and 1983). Typically, employment equations in the literature have been interpreted as demand equations, even though many include supply side variables (Card and Krueger, 1995). Despite these concerns, such estimates (from labour demand and reduced form equations) have been extensively compared in the literature without much reservation.

Most time series evidence available uses US data for the 60s and 70s. As noted above, Brown et al. (1982) thoroughly surveyed this evidence and concluded that a 10% increase in the minimum wage (variable) decreases teenage employment by 1% to 3%. Later, Card and Krueger (1995) and Brown (1999) extended this survey, including time series studies using data for the 80s, and concluded that a 10% increase in the minimum wage (variable) decreases teenage employment by around 1%. Hamermesh (1993) suggests that the smaller effect studies including more recent data results from a less

binding minimum wage in the 80s than in the 60s and 70s. However, Card and Krueger (1995) firmly rejected this explanation; they not only argue that the minimum wage variable Kaitz index was higher during the 80s, because coverage increased, but they also argue that the Kaitz index is normed by average wages and thus reflects a lower effective minimum wage. Brown (1999) suggests, in addition to a less binding minimum wage, (a) that the more recent studies specifications also include more controls – in poorly (mis)specified early studies, the minimum wage coefficient estimates might suffer from bias, as the minimum wage might capture the effect of omitted variables on employment; and (b) that the general increase in wage inequality in the 80s increased the dispersion of the wage distribution, and because of that, the teenage Kaitz index is significantly lower (than if the inequality had remained at its 60s and 70s level), therefore having a smaller effect on employment. Card and Krueger (1995) conducted a careful evaluation of the available time series evidence where they discuss a number of methodological issues and implement meta analysis techniques to assess the likelihood of publication bias and specification searching, which, they conclude, cast doubt on the available time series evidence.

Cross Sections – Another source of variation that has been used to estimate the minimum wage effect on employment is cross-section variation, in particular, cross regional variation. The notion that the minimum wage has a different impact across regions is intuitive and was long ago remarked by Stigler (1946). The impact of a national minimum wage differs across regions depending on how binding the minimum wage is (Card and Krueger, 1995) and on the overall macroeconomic performance of each region (Williams, 1993). The underlying hypothesis is that the minimum wage has a stronger effect on low wage regions, where it is more binding and affects a larger proportion of workers (Brown et al., 1982; Brown, 1999). Unlike in the time series analysis, the counterfactual is much clearer in cross-section analysis: the employment in high wage regions, where a smaller fraction of workers is affected, is compared to the employment in low wage regions, where a larger proportion of workers is affected, following a minimum wage increase.

As cross-section data became more available, several studies re-estimated versions of the time series employment equation using, for example, the 1970 US Census (Brown, 1999). The typical model in this early cross-section literature is similar to the

model in the early time series literature, but the source of data variation is crucially different. As in time series models, the strategy in cross-section models has been to use the Kaitz index as the minimum wage variable. This makes it possible to estimate the minimum wage variable coefficient – if the employment equation is specified as a function of the constant national minimum wage, the coefficient of interest is not identified at all if the equation already has an intercept. This is because, as discussed above, a national minimum wage is constant across regions, and therefore, cannot explain variations in employment across regions. However, although the cross-regional variation in the Kaitz index makes it possible to identify the minimum wage variable coefficient, it also makes it obvious that this variation is driven by the variation in the average wage (coverage did not vary much in the 60s and 70s). This is the main criticism on cross-section studies, i.e. that the variation in the ratio is driven by the variation in average wages and therefore cannot be regarded as genuine. The effect of the inverse of the average wages on employment is what is ultimately estimated (Welch and Cunningham, 1978; Freeman, 1982). In other words, once the numerator is constant, the variation in the denominator is what drives the estimated impact of the ratio on the dependent variable.[5]

Because of this grave drawback, cross-section studies have usually been dismissed as reliable evidence of the employment effect of the minimum wage (Brown, 1999; Card and Krueger, 1995). Nonetheless, some of these studies estimated employment effects at the upper end of 1%-3% range of the time series studies, although others found negligible effects (Brown et al, 1992; Brown, 1999). In general, as with the time series evidence, the richer the specification in controls, the smaller the effect of the minimum wage on employment. In the earlier cross-section literature, as in the earlier time series literature, the models were more poorly (mis)specified, and as a result, the minimum wage coefficient estimates in those studies might suffer from omitted variables bias.

Panel Data – Significant improvements in the minimum wage literature were made when microdata became more available. The availability of the US CPS coupled with the appearance of US state minimum wages (in response to the federal minimum wage

[5] For further discussion on the advantages and disadvantages of the Kaitz index, see Welch (1976), Brown et al. (1982), Castillo-Freeman and Freeman (1992), Neumark and Wascher (1994), Card and

erosion during the 80s) naturally motivated studies both at a regional and at an individual level (Brown, 1999). The main advantage of using panel data is the possibility of full identification of the minimum wage coefficient – the main drawback of using time series and cross-section data. As discussed above, identification of the minimum wage coefficient depends on modelling the effect of macro variable shocks (common to all regions) and the effect of region specific variable shocks on employment. This is because variables other than the minimum wage change over time, and regions differ in ways other than the minimum wage. If the minimum wage varies over time and across regions, time and regional fixed effects can be modelled with a full set of time and region dummies – which was not possible with time series or cross-section data. Time dummies separate macro effects and region dummies separate regional effects from the effect of the minimum wage on employment, ensuring identification of the effect of the minimum.

As in the time series and cross-section literature, the usual strategy in the panel data literature has been to use the Kaitz index as the minimum wage variable, which now varies across regions and over time. Although this variation makes it possible to use a full set of time and region dummies, it is, as before, completely driven by the variation in the average wage and cannot be regarded as genuine. Many minimum wage variables with "genuine" across region and over time variation have been suggested in the literature as alternatives to the Kaitz index. The specific variable definition depends on whether the approach was to follow a cross-section of regions over time (Card, 1992a; Neumark and Wascher, 1992 and 1994; Williams, 1993; Card et al. 1994; Card and Krueger, 1995; Deere et al. 1995; Baker et al., 1999; Burkhauser et al., 2000a; Zavodny, 2000; etc.) or a cross-section of high and low wage workers over time (Ashenfelter and Card, 1981; Linneman, 1982; Currie and Fallick, 1996; etc). Because of the estimation strategy used in this study, the focus will be on studies in the literature using the first approach. For such studies, in addition to the Katz index (Neumark and Wascher, 1992 and 1994; Williams, 1993) and minimum wage exploiting federal and state variation (Card et al., 1994; Burkhauser et al., 2000a), other minimum wage variables suggested in the literature have been a dummy for when the minimum wage goes up (Deere et al., 1996) and fraction affected, defined as the proportion of people

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Krueger (1995), and Deere et al. (1996).

earning a wage between the old and the new minimum wage (Card, 1992a; Card and Krueger, 1995). Brown (1999) compares the 'degree of impact' measures (for example, fraction affected) and the 'relative minimum wage' variable (for example, Kaitz index) and concludes that the former are conceptually cleaner.[6]

Two econometrics techniques have been used to exploit region data over time: regression analysis and difference-in-difference estimation analysis. Because this study uses the first technique, the focus will be on studies using regression analysis.[7] For such studies, the typical model in the panel data literature is similar to the model in the time series and cross-section literature. However, not only the source of data variation is crucially different, but also a wider set of controls is exploited and time and region

[6] It can be argued that employment equations using the Kaitz index controls for wages, whereas those using other minimum wage variables, e.g. fraction, do not. This could bias the fraction estimate, as fraction might either (a) capture the effects of omitted wages on employment; or (b) not account for minimum wage spillover effects on employment – if spillover effects are large, estimates using the Kaitz index might be larger because the Kaitz index is capturing not only the effect of the minimum wage, but also the effect of other wages on employment; (c) or both. As a result, estimates from equations using the Kaitz index and fraction would not be comparable because they are respectively estimates from labour demand and reduced form equations. As discussed above, the debate is about whether other wages should be controlled for or not (Brown, 1999). Despite these concerns, such estimates (from labour demand and reduced form equations) have been extensively compared in the literature without much reservation. Two main arguments could make such comparisons sensible. First, if wages do not change much, controlling for wages is not crucial, as they cancel out in the model in differences; residual differences would be controlled for by population groups controls and regional and time fixed effects. This is the implicit assumption in the US literature where spillover effects are small. Second, what theory suggests is a relative wage measure. Just as the Kaitz index relates the minimum wage to the (average of the) wage distribution, so does fraction. To the extent that they are both measures of the bite of the minimum wage, fraction is just as good as any other empirical variable (Dickens at al., 1999; Williams, 1993). In that sense, fraction, just as toughness, accounts for the effect of other wages (relative to the minimum wage) on employment.

On a separate but related issue, in addition to comparing the non-directly comparable labour demand and reduced form estimates, the literature also compares the non-directly comparable total employment, hours, and jobs estimates; and sometimes it also compares the non-directly comparable employment of teenagers and other low wage group employment estimates. First, the sign and the magnitude of estimates for the total employment effect depend on the sign and magnitude of the hours and the job estimates. Second, estimates for the entire labour force are smaller than estimates for teenagers or other low wage groups, as the minimum wage employment effect is diluted in the whole labour force.

[7] Comprehensive surveys on the difference-in-difference estimation of employment effects, including a critical discussion on its methodological issues, are Card and Krueger (1995) and Brown (1999). This has been a traditional technique to study minimum wage effects, as documented by Kennan (1995) and Brown (1999). The main finding of the early difference-in-difference literature is mixed and imprecise, but suggests employment effects within the early time series and cross-section literature range of 1%-3%. More recent difference-in-difference studies also provide mixed results, but as with more recent time series and cross-section literature, the evidence suggests, if any, smaller employment effects, towards the lower end of the range. There has been much debate and criticism in this literature regarding three methodological issues (Hamermesh, 19995; Brown, 1999): the validity of the control group, the contamination of the treatment group prior to the treatment, the amount of time elapsed between the minimum wage increase enactment date and the "after" survey. The first two can bias the estimates; the third determines whether the estimates are short or long run (see Section 1.3).

dummies model time and fixed effects, as discussed above. The panel data literature evidence is mixed and not always precise, but as in the time series and cross-section literatures, the richer the specification in controls (in particular if they include region and time fixed effects), the smaller the effect of the minimum wage on employment. Evidence produced using richer specifications offers no support for negative employment effects.

Because this study uses panel data and regression analysis and because the range of estimates in existing studies of this kind is wider than that in time series and cross-section studies, this evidence will be discussed here in more detail. On the one hand, Neumark and Wascher (1992 and 1994), Williams (1993), Deere et al. (1995 and 1996) and Burkhauser et al. (2000a), find negative employment effects; on the other hand, Card (1992a), Card et al. (1994) and Card and Krueger (1995), find non-negative employment effects. These differences are due to two main reasons.

First, the estimates are sensitive to controlling for region and time fixed effects. Models that do not include year dummies, i.e. rely on time series variation to identify the minimum wage coefficient, produce larger (more negative) estimates; whereas models that do include year dummies, i.e. rely on cross-section variation to identify the minimum wage coefficient, produce smaller (towards zero) estimates. This can be clearly observed in two studies: Burkhauser et al. (2000a), who perform robustness checks both including and excluding time effects; and Deere et al.(1995), who capture the minimum wage effect by using year dummies (which might in fact, be capturing year specific macro effects). As discussed above, modelling time and region fixed effects ensures full identification of the minimum wage coefficient – it ensures that the effect of the minimum wage is disentangled from the effect of other variables on employment. Failure in doing so casts doubts on the reliability of the estimates. Furthermore, failure in doing so is failure in taking advantage of the most important aspect of panel data.

Second, the estimates are sensitive to controlling for enrolment. As discussed above, there is some agreement that demand side variables should be held constant, but less agreement on whether supply side variables should be included as controls and, if so, which ones. Of particular concern is the inclusion of enrolment, which is jointly determined with – rather than an exogenous determinant of – employment, since

schooling and working are alternative opportunities (Card and Krueger, 1995). Neumark and Wascher (1992 and 1994) report results both excluding (which might cause omitted variable bias) and including (which might cause simultaneity bias) enrolment as a strategy to bracket the true minimum wage effect. They find respectively positive and negative employment effects. However, some definition problems in Neumark and Wascher's (1992 and 1994) enrolment variable cast doubts on their results. Further to definition problems, Card and Krueger (1995) argue – not very convincingly – that there is no reason to expect higher or lower enrolment rates across states, aside from potential minimum wage effects on employment, and therefore enrolment rate can be omitted particularly if year and region effects are modelled. As claimed by Brown (1999), if minimum wage reduces both employment and enrolment, reduced form and enrolment rate constant employment equations have very different interpretations. If the minimum wage reduces school enrolment, this might be more important than adverse employment effects.

To conclude, as remarked above, there has been much debate and criticism in the literature on the employment effects of the minimum wage. Results consistent with the standard model prediction of a negative employment effect conflict with results that challenge such a prediction. However, despite the controversial recent debate, despite the different methodologies and associated methodological issues, and despite different data periods and data sources, most studies found that a 10% increase in the US minimum wage (variable) decreases (teenagers) employment by no more than 3%. More recent (and more reliable) studies suggest no more than 1%, if anything. This is a small disemployment effect of the minimum. In his recent survey, Brown (1999, p.2154), further to remarking that "the minimum-wage effect is small (and zero is often hard to reject)", also remarks that "and elasticity of -0.1 would likely seem small to anyone who had not been conditioned by the evolution of the minimum wage literature to expect such a small response".

Developed Countries – The international literature is scanty on non-US minimum wage employment effect empirical evidence. Bazen (1994), Card and Krueger (1995), Ghellab (1998) and Cunningham (2002) discuss some of the international evidence for developed countries. Here, comparisons across studies are more difficult, not only because of different techniques and data period and source as for the US literature

discussed above, but also because the effect of the minimum wage on employment depends on the minimum wage level (and enforcement) and on the particularities of the labour market in each country (Freeman, 1996 and Dolado et al. 1996). However, the overall reading for developed countries is much like the one for the US: earlier evidence suggests a wider range while more recent evidence suggests a narrower range, clustered around zero. A 10% increase in the minimum wage (variable) decreases (youth) employment by no more than 6% across all available studies. This is a large effect as compared to the one in the US, although studies using more recent data and panel data techniques found smaller estimates. This larger effect stems from two main reasons (a) because the minimum wage is generally more binding in Europe, where most of the evidence is available for; and (b) because the estimates vary widely (and for some countries they are considerably smaller) as a result of institutional differences across countries.

Most of the evidence is available for the following countries:

United Kingdom – Craig et al. (1982) used wage councils sector data in the late 60s and 70s and found no support for negative employment effects. Lund et al. (1982) used data on food industry from 60s to 80s and found negative employment effects in line with those for the US. Canning and Tarling (1985) and Morgan et al. (1985) used clothing industry data from the 50s to 70s and found negative employment effects; Canning and Tarling (1985) found smaller effects and criticized Morgan et al.'s (1985) study. Kaufman (1989) used individual level panel data from 60s and 70s, claiming improvements on the methodological approach over the last three studies above, and found large negative employment effects. Bazen (1990) and Gregg (1990) used macro data for the 80s and found negative minimum wage employment effects. This approach was criticized by many (for example, Machin and Manning, 1992 and 1994) in favour of a new approach using micro data, following improvements in the US minimum wage literature. Machin et al. (1993 and 2003), Machin and Manning (1994 and 1996), Dickens et al. (1994c, 1995, 1998 and 1999) and Dolado et al. (1996) used industry level and individual level panel data for the 80s and 90s, and found no support for a negative employment effect. Fernie and Metcalf (1996) used panel data data surrounding the abolition of the minimum wage in Britain (1993-1994) and before and after techniques as well as simulations using macroeconomic models of the British

economy and found evidence supporting negative employment effects. Machin (1997) criticized macroeconomi simulations and argued for nonnegative employment effects. Machin et al. (2003) studied the effects of the introduction of the UK national minimum wage in April 1999 using their own data on the residential care homes industry and found evidence supporting a small negative employment effect. Stewart and Swaffield (2001) and Stewart (2002 and 2003) used individual level panel data for the late 90s and did not find much support for negative employment effects. As for the US, studies using more recent data and panel data techniques found smaller adverse effects on employment, if any, in the UK.

France – Rosa (1980, 1981 and 1985), Fourcans (1980) and Martin (1983) used time series data from 50s to early 80s and found large negative employment effects, but as for the US and the UK, this early literature was criticized because of methodological issues (Skourias, 1995). Benhayoun (1990, 1993 and 1994), Bazen and Martin (1991), Ducos and Plassard (1991) and Skourias (1992, 1993 and 1995) used individual level panel data from 60s to early 90s and found smaller negative employment effects. Bazen and Benhayoun (1995) used industry sector data for the 70s and 80s and found small negative employment effect. Dolado et al. (1996) used individual level panel data for the 80s and found no support for a negative employment effect. Bruno and Cazes (1997) used individual level panel data from the 70s to early 90s and found no support for negative employment effects. Kramarz and Phillippon (2000) used individual level panel data for the 90s and found small employment effects. The smaller estimates in more recent studies confirm the trend in the US and UK literature. However, Abowd et al. (1999 and 2000) used individual level panel data for the 80s and found larger negative employment effects.

Netherlands – Van Soest and Kapteyn (1989) used 1984 cross-section data and found large negative employment effects. Van Soest (1989 and 1994) and Koning et al. (1994) used 70s and 80s aggregate data and 80s individual level data and found large negative employment effects. In addition to the usual time series criticism on the macro data evidence, the micro data evidence in these studies was criticized by Dolado et al. (1996) because of methodological issues. In contrast, Bosch and van der Hoeven (1991) and Salverda (1989) and Dolado et al. (1996) used 70s and 80s individual and aggregate level data and found no support for negative employment effect.

Portugal – Ribeiro (1993) used individual level panel data for the 80s and found small negative employment effects. Pereira (2003) used firm level panel data for the late 80s and found larger negative employment effect.

Spain – Dolado et al. (1996) used industry panel data from the late 60s to the mid-90s and found mixed evidence but concluded for no negative employment effects. Dolado et al. (1997) used individual and industry panel data from the late 60s to the mid-90s and found small negative employment effects.

Greece – Koutsogeorgopoulou (1994) used manufacturing panel data from 60s to 80s and found small negative employment effects.

Canada – Mercier (1987) surveyed eighteen minimum wage studies for Canada and found large negative employment effects, but remarked that the most reliable ones were in line with the US evidence. Two of these early studies, most often cited in the literature, are: Swidinsky (1980) who used individual level panel data from the 50s to the mid-70s and found small negative employment effects; and Schaafsma and Walsh (1983), who used individual level panel data for the late 70s and found mixed evidence but concluded for small negative employment effects. The more recent literature, as for other developed countries, found smaller estimates. Grenier and Seguin (1991) used late 70s and 80s microdata and found no significant negative employment effect. Cousineau et al. (1992) used individual level data for Ontario from the late 60s to the 90s; Cousineau (1991 and 1993) used individual level panel data for Canada from the 60s to the 80s. These studies found mixed results, the first two concluded for small, and the third for large negative effects. Yuen (1996) used individual level panel data for the late 80s and found negative employment effect. Baker et al. (1999) used individual and firm level panel data from mid 70s to early 90s and found small negative employment effect.

Developing Countries – If the international literature is scanty on non-US employment effect empirical evidence for developed countries, it is greatly lacking for developing countries. Card and Krueger (1995), Ghellab (1998), Cunningham (2002), Saget (2001) and Maloney and Mendez (2003) discuss some of the international evidence for developing countries. For the same reasons discussed above, comparisons across studies are difficult – even more so in studies across developing countries, where labour market institutions are very specific. The available evidence is limited, not only

to recent periods, but also to only a few countries and, most of the time, only one or two studies for each country. A 10% increase in the minimum wage (variable) decreases (low wage workers) employment by up to 12% across all available studies. This is substantially larger than the US employment effect because the minimum wage is far more binding in developing countries than in the US (Maloney and Mendez, 2003). The implicit hypothesis is that the small effects in the US result primarily from a low (not very binding) minimum wage. However, while it is relatively safe to conclude that employment effects are larger for developing countries, even though evidence of small employment effects have been reported to these countries (Saget, 2001), care should be taken when referring to their magnitude. This is because of three main reasons: because there are few point estimates to rely on, because the variance across the range of estimates is high (due to substantial institutional differences), and because these estimates might not be directly comparable.

Ghellab (1998) and Cunningham (2002) surveyed studies for worldwide developing countries and reported a negative correlation between the minimum wage and employment, although Ghellab (1998) warned that methodological issues cast doubts on this result. They, together with Card and Krueger (1995), Saget (2001) and Maloney and Mendez (2003), surveyed studies for developing countries in America. Most of this evidence is available for the following countries:

Mexico – Villarreal and Samaniego (1998) used individual level panel data for the mid-50s to the 70s and found small negative employment effects. Bell (1997) used manufacturing industry panel data for the 80s and found no employment effects. Feliciano (1998) used individual level panel data from the 70s to the 90s and found small negative employment effects. Small employment effects for Mexico are in line with a not very binding minimum wage there (Bell, 1997).

Puerto Rico – Reynolds and Gregory (1965) used manufacturing panel data for the mid-50s and found mixed evidence. Rottenberg (1981) discusses the imposition of the US minimum wage in Puerto Rico and concludes for adverse employment effects. Castillo-Freeman and Freeman (1992) estimated the effects of the US minimum wage on the Puerto Rican labour market using time series and industry level panel data from the 50s to the 80s and found very large negative employment effects in Puerto Rico, in contrast to small employment effects in the US. On the one hand, Card and Krueger (1995)

criticized these findings, which they argue are upwards biased because of methodological issues; on the other hand, migration might have offset the employment effects.

Costa Rica – Gregory (1981) used industry aggregate data for the late 60s to early 70s and found some evidence supporting negative employment effects in Costa Rica. Gindling and Terrell (2002) used individual level panel data for the 80s and 90s and found no support for a negative employment effect. This is in line with El-Hamidi and Terrell (2001) who used individual level panel data from the late 70s to the early 90s and found positive employment effects for lower minimum wage sectors and small negative employment effects for higher minimum wage sectors. The minimum wage system in Costa Rica is complex, with many different levels by category and industry, which makes it difficult to pinpoint the employment effect. Furthermore, non-compliance is extraordinarily high, which might undermine this effect (Saget, 2001; Gindling and Terrell, 1995). However, the available evidence suggests that for high (more binding) minimum wages, the employment effects are negative.

Chile – Corbo (1981) estimates the effect of the minimum wage on employment using manufacturing census data for 1967 and finds negative effects.

Colombia – Bell (1997) used manufacturing industry panel data for the 80s and found very large negative employment effects. Maloney and Mendez (2003) used individual level panel data for Colombia in the late 90s and confirm that employment effects in Colombia are roughly twice as large as those for the US. Large employment effects for Colombia are in line with a very binding minimum wage there (Bell, 1997).

Brazil – A 10% increase in the minimum wage decreases employment by no more than 5%, typically by no more than 1% (not always statistically significant), across all available studies for Brazil.[8] Like for the US, this is a small employment effect, although unlike for the US, the minimum wage is binding and wage effects are sizeable in Brazil (see Section 1.2). Thus, it appears that the evidence for Brazil is at odds with the theory prediction of larger negative employment effects in presence of more binding minimum wages, as observed for Europe and for other developing countries. In a way,

[8] Dropping the Corseuil and Carneiro (2001) outlier estimate, the employment effect is no more than 3%; dropping the Corseuil and Morgado (2001) estimate, this effect is no more than 1% across the remaining

this is reflected in the volume of available minimum wage studies for employment and wages effects in Brazil. While there are 22 wage effects studies and they use data from as far back as the 40s (see Section 1.2), there is only 10 recent employment effects studies and they use (mostly individual level panel data) for the 80s and 90s. Neri (1997a) used individual level panel data for the mid-90s and found small negative employment effects. Foguel (1997) used individual level panel data for the mid-80s and found small negative employment effects. Carneiro (2000), Foguel et al. (2000 and 2001), Corseuil and Carneiro (2001), Corseuil and Morgado (2001), Fajnzylber (2001) and Soares (2003) used individual level panel data and time series data for the 80s and 90s and found mostly small negative employment effects. Neumark et al. (2003) used individual level panel data for the late 90s, and found small negative employment effects. An outlier study is McIntyre (2002), who used individual level data for the 80s and 90s and regression analysis where the empirical equation was delivered by a Two-Sectors model to estimate the probability of workers and firms moving from the formal to the informal sector. One of the pieces of evidence he reported, relevant to this survey, is that because of this expected move, the minimum wage increases the size of the informal sector.

1.3 PRICE EFFECTS

As discussed in Sections 1.1 and 1.2, despite disagreements over employment effects, there is agreement that minimum wage increases raise wages – which, ultimately, raise prices. Although the potential effect of the minimum wage on prices and inflation was first noted by Stigler (1946), not as much research has been done on price as on employment effects of the minimum wage. There is very little empirical evidence on the effects of the minimum wage on prices. While there were over 300 studies on the *employment* effect of the minimum wage by 1995 (Card and Krueger, 1995), only 3 studies had been written on its *price* effects by then (Wessels, 1980; Katz and Krueger, 1992; Spriggs and Klein, 1994), plus the US Labour Department reports (FLSA 1965 and 1969; and MWSC 1981), and not many more have been written since.

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10 studies. These two studies are closely related, and both have been informally criticized by other Brazilian authors, which cast doubts on their reliability.

Perhaps because the international literature mainly utilizes data from the US, and price effects are small there, little further research has been carried out.

A comprehensive survey on the minimum wage price effects is not available in the literature. Brown (1999) surveyed only three studies: Wessels (1980), Katz and Krueger (1992), and Card and Krueger (1995). Because of that, the existing studies are here briefly surveyed. These studies use five different methodologies: general equilibrium model analysis, Phillips curve estimation analysis, input-output model analysis, difference-in-difference estimation analysis and regression analysis. They can be broadly divided into two categories: estimation of the effect on prices in various industries and estimation of the effect on inflation nationwide. This categorization is associated to the extent to which they account for the several steps through which the minimum wage affects prices and inflation (transmission mechanism). First, there is a direct impact on those between the old and the new minimum wage. Second, there is an indirect impact on those above (and below) the new minimum wage, the spillover effects (see Section 1.1). Third, firms raise prices in response to these higher labour cost. Fourth, firms adjust the associated level and mix of input and output (consistent with cost minimization subject to expected demand) (see Section 1.2). Fifth, the resulting new employment and wages levels combine to produce a new equilibrium income level, aggregate demand and, after some lag, production. Sixth, the inflation and unemployment rates consistent with the new equilibrium might in time again affect wages (Sellekaerts, 1981).

The main difficulty in comparing general equilibrium and input-output model estimates with other estimates available in the literature is that most of them are obtained from single equation models. It is important to distinguish whether the single equation represents (a reduced form of) a partial or a general equilibrium model. Furthermore, it is important to distinguish whether the parameter of interest in single equation models is a structural or a reduced form parameter, which is not always clear. The single equation estimates can describe two very different processes. If the single equation describes the partial equilibrium adjustment process in a particular market or industry, it does not account, unlike general equilibrium models, for the various steps of the transmission mechanism. If the single equation describes the inflation process in the economy, it accounts for all steps of the transmission mechanism. In the first case, the

single equation estimates are not comparable to the general equilibrium and input-output model estimates; in the second case, they are.

General Equilibrium Model Analysis – Earlier studies of the minimum wage effect on prices and inflation often use general equilibrium model analysis, where the effect of the minimum wage on a number of variables is estimated. A Phillips curve relation, as a function of the minimum wage, is often inserted in the general equilibrium macro model. Sellekaerts (1981) reviewed four such studies; the effect on wage and price inflation of a 10% increase in the minimum wage across them ranged from 0.15% to 0.76%. She then criticized these studies on the grounds of several methodological problems, in particular because they did not account for all steps of the transmission mechanism. As discussed in Section 1.1, she attempted to overcome such problems by inserting a modified wage determination equation into the MIT/PENN/SSRC macro model of the US economy, which she estimated using 1974 to 1979 US time series macro data. One of the main contributions of this study is that this wage equation accounts for wage increases that would have taken place regardless of changes in the minimum wage legislation. That is because unless the minimum wage increase causes substantial gains in real terms, the legislation might be no more than a change in the timing of the increases. She reported evidence supporting spillover effects. She found that the average annual total impact of a 10% minimum wage increase is 0.6% for wage inflation and 0.2% for price inflation. Sellekaerts' (1981) study is one of eight studies published on a special volume on inflation by the US Minimum Wage Study Commission (MWSC, 1981). The implicit message across these studies is that the effect of the minimum wage on inflation was too small to be a concern. Two of these studies are worth noting, Cox and Oaxaca (1981) and Wolf and Nadiri (1981).

Cox and Oaxaca (1981) used US data from 1974 to 1978 aggregated at industry and macro levels to simulate the effect of freezing the minimum wage at its 1974 level on employment, output, wages and prices using a general equilibrium model of the US. They were primarily concerned with the allocative effects of the minimum wage, which they argue, can only be accurately assessed by a general (not by a partial) equilibrium model. Their results indicate that the minimum wage is not neutral with respect to production, employment, prices and wages, and that structural adjustment occurs following an increase. They reported that a 10% increase in the real minimum wage

increases the aggregate real wage bill by 0.1%-0.5% (they do not report the effect on prices, but hint that it is larger than that reported in the then existing literature). This study was an ambitious attempt to model the effects of the minimum wage on key variables in the economy. One of its main contributions was to account for the crucial role of monetary policy accommodating (or not) the minimum wage increase. Two of its drawbacks are noteworthy: the strong assumptions underlying the model and the inappropriate data used to construct empirical counterparts of theoretical variables, which contaminated the results with measurement error (Corcoran, 1981).

More recently, Wilson (1998) reported estimates developed by The Heritage Foundation using the 11 US macro model of the US economy. The proposed 19.4% 1999-2000 increase in the minimum wage was estimated to increase overall prices by 0.2% in the first year and by an additional 0.1% in the second year.

In addition to the criticism on the strong underlying assumption discussed above, a further criticism on general equilibrium models is the implicit assumption of a uniformly proportional inflation effect throughout the economy. Minimum wage overall inflation effects are hard to find; the minimum wage might cause more inflation in sectors or industries overpopulated by minimum wage workers. Input-output models and partial equilibrium models (difference-in-difference and regression analysis) discussed below estimate sectoral price effects of the minimum wage.

Phillips Curve Estimation Analysis – A Phillips curve relation, as a function of the minimum wage, is also often estimated on its own, i.e. not inserted in a general equilibrium macro model. Sellekaerts (1981) reviewed seven such studies on wage and price inflation, among which Gramlich (1976) and Falconer (1978) (see Section 1.1). The effect on wage and price inflation of a 10% increase in the minimum wage across these studies ranged from 0.2% to 1.8%; if the outlier 1.8% is dropped, the range is from 0.2% to 0.37%.

Not included in Sellekaerts' (1981) review is a series of four articles, Gordon (1980) Frye and Gordon (1981), Gordon (1981) and Gordon (1982), which are related to an earlier (Gordon, 1975) and a later (Gordon, 1988) papers, where the authors explain inflation by estimating various versions of the Phillips curve relation using US annual time series data from 1890 to 1980. The most relevant of these articles for this survey is Frye and Gordon (1981), which focus on the impact of episodes of

Government intervention (e.g. minimum wage increases) in the US inflation. A 10% increase in the minimum wage was found to increase inflation by 0.02 percentage points.

The main contribution when estimating Phillips curve relations as a function of the minimum wage is the argument that econometric explanation of inflation requires supply shocks (e.g. oil price, exchange rate, productivity growth, etc.) and Government intervention or push-factors (e.g. minimum wage, social security taxes, employment protection, unions, etc.) in addition to the usual inertia and aggregate demand variables. This is because push-factors play an important role in the price and wage setting process, affecting real wages and the natural level of unemployment that makes inflation constant. This was argued by, among others, Layard and Nickell (1985 and 1986), Jackman et al. (1996) and Staiger et al. (1996).[9]

To the extent that the way endogeneity problems were dealt with is credible, the above models describe the inflation process in the economy through a reduced form equation and the minimum wage estimates should be comparable to the general equilibrium model estimates reported above.

Input-Output Model Analysis – Wolf and Nadiri (1981) used an input-output model that simulates the changes in policy parameters (e.g. the minimum wage), on employment, output, and prices in the aggregate economy and in each of the industry sectors by tracing the inter-industry flow of goods and services. They used data from the US CPS to trace the direct and indirect price effects of the 1963, 1972, and 1979 minimum wage increases. Assuming full pass-through effect, no substitution effect, no employment effects and no spillover effects, they estimated that a 10%-25% minimum wage increase raises prices by 0.3%-0.4%. An important contribution of their model is to account for the failure of input-output models to predict longer run responses. This is because of the implicit assumption of no substitution among goods and services, as their relative prices change, and the associated assumption of employment and output fixed in the short run. Wolf and Nadiri (1981) introduced price and (labour-capital) substitution elasticities in their model that can then be regarded as a medium run model

[9] See Ball et al. (1988) and Goodfriend and King (1990) for surveys on prices and inflation modelling. Also see Gali et al. (2001) on the so-called New Phillips curve, which however, does not include the minimum wage.

(Adams, 1981). Another important contribution of this study is the broad approach to the benefits and costs of a minimum wage increase. On the costs side, there are the higher consumer prices; on the benefits side, there are the higher productivity and the higher output growth resulting from income distribution towards low wage groups who have an above average propensity to spend. One drawback of their model is the overly simplistic model with strong assumptions that cast doubts on the results (Sheldon, 1981).

More recently, Lee and O'Roark (1999) used US earnings and industry data from 1992 and 1997, and a similar input-output analysis to compute the minimum wage price effect. Once more assuming full pass-through effect, no substitution effect, no employment effects and no spillover effects, they estimated that a 10% minimum wage increase raises prices among eating and drinking places – industries overpopulated by minimum wage workers are – by 0.74%. An important contribution of their work is to produce sectoral estimates of the minimum wage price effect. Another important contribution is that they partially relaxed the no spillover effects assumption by allowing for limited spillover effects higher up in the wage distribution. Relaxing this assumption is important because further to allowing for the indirect effect of the minimum wage on other wages, it also allows for the wage price interaction in the real wages bargaining process that follows a minimum wage increase. If spillover effects and wage price interaction effects are not accounted for, the inflationary effects of the minimum wage might be understated. They re-estimated their model allowing for different degrees of spillover effects and found that the larger the extent of spillover effects allowed, the larger the price effects, up to 1.5%.

As Wolf and Nadiri (1981), MaCurdy and O'Brien-Strain (1997), O'Brien-Strain (1999) and O'Brien-Strain and MaCurdy (2000) also have an approach to the benefits and costs of a minimum wage increase. They use a similar input-output model and SIPP and CES data to show that the 1999-2000 US minimum wage increase would mean California's families have to pay more for goods and services than they would receive through higher earnings. To calculate the benefits, they identify which families have workers earning below the new minimum wage, assume they will have their wages increased to the new minimum wage level, and then calculate the new family's earnings. To calculate the costs, they first determine the costs of the minimum wage

increase by estimating the expected increase in labour costs and tracing these costs through to consumer prices; they then use these implied price increases to determine what is the extra (consumption) cost for all families. Once again assuming full pass-through effect, no substitution effect, no employment effects and no spillover effects, they estimated that a 10% minimum wage increase raises prices by 0.3% to 2.16% in California, depending on the commodity, and compared their results to Lee and O'Roark's (1999). Using an extended sample of US states, MaCurdy and McIntyre (2000) applied the same methodology and SIPP and US Census data to analyze the 1996-1997 US minimum wage increase. They estimated that a 10% minimum wage increase raises overall prices by 0.25%, and prices of food consumed outside (inside) home by 1.2% (0.8%). They compared their results with Lee and O'Roark's (1999) and Aaronson's (2001) (they compare it with an earlier version of Aaronson's paper) and argue that differences with the later stem from the difference in methodology. They also estimated the effect of the national 1996-1997 minimum wage increase on four states: California, Florida, New York and Texas but did not find qualitatively different results.

Despite of the insightful way the authors exploit the short run nature of the input-output model, an important drawback of these studies is the assumption of no employment effects. They argue that the short run effect of a minimum wage increase may be a price increase, with employment effects becoming evident only in the longer run, and argue that this is in line with employment effects evidence in the literature (Brown, 1999). However, the assumption that employment is fixed, and therefore that output is fixed, can only be maintained because of the assumption of no change in the spending patterns. Although this offers estimates for the short run, it abstracts from the real issue, as acknowledged by the authors, that most people will adjust their spending in response to higher prices, affecting employment and output. On the one hand, this might overestimate the cost (and price) effects of a minimum wage increase, which would be mitigated by a reduction on employment or a reduction in profits (although adverse employment effects might also mitigate the benefits of a minimum wage increase). On the other hand, the benefit effects of the minimum wage might be underestimated because of the no spillover assumption, whereby only families with workers earning below the minimum wage benefit from the minimum wage increase.

These, together with other restrictive assumptions underlying the input-output model, produce a highly stylized and unrealistic model and cast doubts on the results.

In addition to the criticism on the no substitution, no spillover effects, and no employment effects assumptions discussed above, three other usual assumptions in input-output models are full pass-through, full coverage and full compliance. This might overstate the price effects of the minimum wage; because of that, input-output model estimates are usually regarded as upper bound effects of the increase. An advantage of input-output models is that they account for the minimum wage effect propagating throughout the economy through its effect on intermediate goods. Even if an industry employs no minimum wage workers, its prices might rise because of its use of goods or contracts for services produced with minimum wage labour.

To the extent that the way the assumptions underlying input-output models were dealt with is credible, the minimum wage estimates should be comparable to the general equilibrium model and Phillips curve relation model estimates reported above. It appears, however, that despite of important improvements, the final estimates still did not account for all the steps in the transmission mechanism (and therefore would not be fully comparable to the above estimates). They represent, nonetheless, valuable evidence, especially given that empirical evidence is so limited. It is noteworthy that their directions and magnitudes are in line with the above evidence.

Difference-in-Difference Estimation Analysis – A technique to estimate the minimum wage effect on other variables (e.g. prices) that has been extensively used in the minimum wage literature is difference-in-difference estimation (Brown, 1999). The idea is to compare high and low wage regions, on the assumption that the minimum wage has a larger effect on prices in lower wage regions. This makes it possible to remove the effect of common factors that affect prices of all regions, such as common macro shocks. If the remaining factors are randomly distributed across regions, the change in relative prices is a measure of the minimum wage effect on prices.

The Department of Labor studies published several studies on the effects of the 1961 and 1967 US minimum wage increases (FLSA, 1965 and 1969) using difference-in-difference estimators to compare US Southern and non-Southern industry prices, assuming greater minimum wage effect in Southern industries. Wholesale prices of industrial commodities and price trends for low wage industries were relatively stable.

Even though the minimum wage increases became effective during a period of rising prices, they were said to have had little influence on this upward trend.

Using the same method and data, Wessels (1980) re-examined the evidence from the Department of Labor Studies. He remarks that a limitation of the competitive approach is that prices should be identical if Southern and non-Southern industries sell their goods in the same markets and consumers regard these goods as nearly the same. In this case a minimum wage increase has no effect on the relative prices of Southern goods but results in relative decrease in Southern employment. Wessels (1980) pointed out that evidence supporting the latter is weak and concluded that Southern firms should be able to pass higher relative costs on to consumers prices. He found little consistent pattern in price increases in manufacturing, but faster price increases in Southern services. A 10% increase in the minimum wage was found to increase prices in the services sector by 2.71% following the 1966-1967 minimum wage increase.

Using difference-in-difference estimation and their own data, Katz and Krueger (1992) and Card and Krueger (1995) compared prices at fast-food restaurants in New Jersey and Pennsylvania following the 1992 New Jersey minimum wage increase. They also used the same data and regression analysis to estimate the minimum wage price effect using reduced form equations. They found that average prices rose in New Jersey by about enough to cover the costs of the higher minimum wage (they found a positive but statistically not significant estimate). Within New Jersey, however, they found that prices rose just as quickly at restaurants paying the minimum wage and restaurants already paying as much as or more than the new minimum wage. The explanation they offer to this is that restaurants within New Jersey compete in the same product market, and therefore those most affected by the minimum wage increase are unable to increase their prices by more, whereas restaurants in Pennsylvania compete in a different product market, enabling prices to rise in New Jersey relative to Pennsylvania. Similar findings in their Texas study suggest that prices rose at about the same rate in fast-food restaurants that made larger or smaller wage adjustments following the 1990-1991 US federal minimum wage increases (they found a negative but not statistically significant estimate).

As pointed out by others (Aaronson, 2001) this evidence is limited to restaurants in three states and two minimum wage episodes. Card and Krueger (1995) provided

further evidence by comparing restaurant average price increases across a broader cross-section of cities and states following the 1990-1991 US federal minimum wage increases. They used regression analysis and two different sources of price data, CPI and ACCRA. They found evidence that restaurant prices rose faster at (a) states that made larger adjustments following the federal minimum wage increase, and (b) cities with higher proportions of low wage workers in 1989 (they found positive and sometimes significant estimates). Overall, Card and Krueger's (1995) findings are imprecise and mixed, but suggest that a 10% minimum wage increase raises prices by up to 4%. This is consistent with predictions from a competitive model that a minimum wage increase raises prices in proportion to the minimum wage labour's share in total cost; they find that the ratio between the price and wage effects approximates this share. Furthermore, this is in line with the non-negative employment effect they reported using the same data and techniques.

Spriggs and Klein (1994) conducted a similar experiment to Katz and Krueger (1992), differing only in the timing between the change in the minimum wage and the follow-up survey. They utilize data for one month before and after the 1991 US minimum wage increase, which, they argue, already accounts for long run adjustments because the increase was announced two years in advance. Their findings suggest that the minimum wage did not significantly affect prices, which continued changing following a prior trend.

There has been much debate and criticism in the literature regarding three methodological issues in difference-in-difference estimation (Hamermesh, 19995; Brown, 1999). The first is the validity of the control group, which needs to capture the change that would happen to the variable of interest (e.g. prices) in the absence of a minimum wage increase, i.e. due to other common macro shocks. The second is the contamination of the treatment group prior to the treatment (for example, because minimum wage changes are announced in advance, firms might start adjusting prices prior to the enactment date). The third is the amount of time elapsed between the minimum wage increase enactment date and the "after" survey (for example, if data is collected too soon after the increase, there might not have been enough time to allow for the impact of the increase on prices). The first two can bias the estimates; the third determines whether the estimates are short or long run. In sum, the reliability of the

estimates lies on the non-contamination of the control and treatment groups by the treatment, and by the appropriate timing of the surveys. Card and Krueger (1995) have been extensively criticized on these three issues (Brown, 1999). Hamermesh (1995) is particularly critical of the timing of their surveys, arguing that the "before" survey was after firms had already started to adjust to the minimum wage increase and the "after" survey was before full adjustment had occurred. Card and Krueger (1995) rely on the traditional argument that adjustment occurs with neither leads nor lags because turnover is high in the fast food industry (Brown, 1999). Despite the criticisms, Card and Krueger's (1995) study represents a valuable attempt to estimate the minimum wage price effects.

Difference-in-difference estimates do not compare to the above general equilibrium model, Phillips curve relation and input-output model estimates because they do not account for all the steps in the transmission mechanism. They describe the partial equilibrium adjustment process to minimum wage increases in a particular industry (for example, fast-food industry). The reason for choosing this industry is that it is overpopulated by minimum wage workers and therefore a larger effect on prices is expected. The estimates here reported are only comparable to the sectoral (food industry) estimates in Lee and O'Roark (1999) and in MaCurdy and McIntyre (2000), which however, are not restricted to the fast-food industry.

Regression Analysis – In addition to the Katz and Krueger (1992) and Card and Krueger (1995) regression models estimates discussed above, Aaronson (2001), MacDonald and Aaronson (2002), and Aaronson et al. (2003) used regression analysis to examine the effect of 80s and 90s minimum wage increases on prices in the US and Canada. This allowed them to exploit variation in time and location to identify their estimates. As Card and Krueger (1995), Aaronson (2001) also used BLS data for metro areas between 1978 and 1997, and in addition, ACCRA and StatCan data; Macdonald and Aaronson (2002) used the Food Away from Home component of the CPI in a wider sample of metro areas from 1995 to 1997 as well as CPS and MSA data. They estimated that a 10% minimum wage increase raises prices by 0.72%-0.73%. These estimates are remarkably close to Lee and O'Roark's (1999) estimates, which use an entirely different methodology and data. The authors contributed to the literature by performing a number of robustness checks, for example: (a) They argued that the

minimum wage might be endogenously determined with prices if politicians favour minimum wage increases in high inflation periods (when the real minimum wage erodes faster). However, in their re-estimations, they found robust estimates and concluded that endogeneity was not much of a concern. (b) They estimated the minimum wage price effect in low and high inflation periods and found that high inflation partially drives the significant minimum wage pass-through coefficient, up to 1.6%. (c) They also found evidence that prices respond quickly to minimum wage increases, within a 4 to 6 months window around the increase. The implications of this are that: although the minimum wage increase is announced many months in advance, there is no price response leading up to the minimum wage increase; and the price effect of the minimum wage is a short run phenomenon that dissipates over time. This is in line with the traditional argument that adjustment occurs with neither leads nor lags discussed above. They warn that minimum wage increases might not generate the sort of coordination failure and stickiness in prices that other costs or demand shocks produce. (d) As Card and Krueger (1995), the authors remarked that the evidence they found is consistent with predictions from a competitive model of full pass-through of costs onto prices. Moreover, they found evidence that suggests that firms seem to increase by more the price of a subset of items and keep constant the price of other items. (e) Their evidence also suggests that prices increase more in low wage areas, in line with prior expectations.

Using their own data and regression analysis, Machin et al. (2003) study the effects of the introduction of the UK national minimum wage in April 1999 on the residential care homes industry, a heavily affected sector. They found no evidence that prices rose by more in low wage firms. However, an important drawback, acknowledged by the authors, is that price regulations limit the extent to price adjustments on this particular market.

As always, the main issue in regression analysis is identification. To ensure identification: (a) the empirical model needs to be correctly specified according to theory; (b) observable and unobservable variables that have a direct effect on prices need to be controlled for in the empirical model; (c) the empirical model needs to be flexible enough to capture the short and long run effect of the minimum wage on prices;

(d) the empirical counterpart of the theoretical variables needs to be constructed as accurately as possible, which hinges on the quality of the data.

The main drawback of the above regression models is the missing link between the empirical specifications and theory. These studies are grounded on the standard theory prediction that if employers do not respond to changes in the minimum wage by reducing employment or profits, they respond by raising prices. However, none of them explicitly discusses the theoretical model that delivered their empirical equation specification. This is a generalized problem in the minimum wage literature, where empirical models are only loosely related to theory (Brown, 1999); it is a particularly worrying problem in price models because of the various channels through which the minimum wage affects prices. Unless the empirical equation is clearly grounded on theory, it is difficult to pinpoint which step of the transmission mechanism is being estimated. The failure in accessing to which extent the pass-through coefficient accounts for the transmission mechanism makes it difficult to compare estimates across studies. Related to that is the estimation of short and long run price effects; only MacDonald and Aaronson (2002) and Aaronson (2001) estimate the long run effects, which for Canada and the US seem to be small. A further criticism, of which few econometrics models are exempt, is whether unobservable variables, possibly correlated to the minimum wage, have been controlled for. Only Aaronson (2001) attempted to discuss the potential endogeneity of the minimum wage in price models, which for the US does not seem to be strong.

As for the difference-in-difference estimates, the above regression analysis estimates do not compare to the above general equilibrium model, Phillips curve relation and input-output model estimates because they do not account for all the steps in the transmission mechanism. Once more, they describe the partial equilibrium adjustment process to minimum wage increases in a particular industry (for example, fast-food industry, care homes industry, etc.). As before, these estimates are only comparable to the sectoral (food industry) estimates in Lee and O'Roark (1999) and MaCurdy and McIntyre (2000) and in addition, to the difference-in-difference estimates above.

Despite the different methodologies, data periods and data sources, most studies found that a 10% US minimum wage increase raises food prices by no more than 4%

and overall prices by no more than 0.5%. This is a small overall inflationary effect of the minimum. Brown (1999, p. 2150) in a recent survey – where, however, he only reviews three of the above studies – remarks, "the limited price data suggest that, if anything, prices rise after a minimum wage increase".

As remarked above, there is very little evidence on the effect of the minimum wage on prices in the international literature, and none whatsoever for developing countries.

CONCLUSION

The international literature on minimum wage is scanty on non-US wages and employment effect empirical evidence; in particular, it greatly lacks empirical evidence from developing countries. Furthermore, there is very little evidence on the effects of the minimum wage on prices in the international literature and none whatsoever for developing countries.

The overall reading of this evidence, taking together wages, employment and price effects, is as follows. (1) The wage distribution compression effect following a minimum wage increase is well established in the international literature. However, although spillover effects in the wage distribution are well established, the evidence on the extent of such effects is mixed even when only more recent evidence is considered and even within the same country. While some authors found evidence supporting large spillover effects extending relatively high up in the wage distribution, others found evidence supporting small and limited spillover effects. (2) There is no consensus on the direction of the employment effects and the debate is certainly not over. Results consistent with the standard model prediction of a negative employment effect conflict with results that challenge such a prediction. However, small effects, clustered around zero, are becoming prevalent in both the international. (3) There is very little evidence on the effects of the minimum wage on prices in the international literature, and none whatsoever for any developing country. This limited literature suggests that it is hard to find overall price effects, and even sectoral (food sector) price effects are small.

In sum, the up to date evidence suggests that the minimum wage increases the wages of the poor, does not destroy too many jobs, and does not raise prices by much.

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