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The Employment Effects of the Minimum Wage: A Review of the Literture

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I. Introduction

There is, no doubt, a voluminous body of work on the employment effects of the minimum wage. Time series techniques historically have dominated the literature up until the early 1980s and have formed the basis for upholding the predictions of the 'traditional model'. Brown (1999) observes, in the case of the U.S., that it is hardly surprising that this is so, given that the minimum wage was uniformly imposed on both high and low-wage states and state minimums were up until then relatively unimportant.

Specifically, the prediction of the traditional model - more commonly known as 'consensus' estimates and typically associated with Brown, Gilroy and Kohen[1] (1982)- was one of a small but statistically significant negative effect of the minimum wage on teenage employment. However, the validity of this finding was challenged by a number of more recent cross section or panel data studies in Canada and the U.S. (Grenier and Séguin, 1991, for Canada, Wellington 1991, Card 1992a,b, Katz and Krueger 1992, Card and Krueger 1994 and Neumark and Wascher 1995 for the U.S., and Machin and Manning 1994, 1997, for Europe). These studies claim that the effects of increases in minimum wages of the 1980s and early 1990s do not appear to be negative and, in certain cases, may actually have been marginally positive. Because some of these studies replicated earlier specifications that yielded the consensus finding, but with the sole difference that they included additional years of data, their clear message is that including the 1980s reduces the estimated effect of the minimum wage on employment.

Why would the inclusion of a few more years of data generate a different empirical estimate?[2] In an influential paper that summarises over one hundred other works, including the earlier one published in 1982, Brown (1999, p. 2119-2121) makes a few reasonable conjectures. These include the hypothesis (credited to Hamermesh, 1995) that the decline of the minimum wage relative to other wages in the U.S. in the 1980s was so substantial that its further gradual erosion had little effect. Another reason is related to the consequences of increasing dispersion[3] of the distribution of hourly wages in general and the declining position of relatively unskilled workers in particular. In addition, the possibility was raised (by Kenan, 1995 p.1955) that the "consensus estimates" were unreliable because of important omitted variables and other time series related problems.

[1] Subsequently referred to as BGK, these authors conclude, in their survey of studies covering the period up to 1981, that a 10% increase in the federal minimum wage reduces teenage employment by 1-3%.

[2] See for example the time series based study by Wellington (1991), which finds that: a) incorporating the experience of the 1980s - 1954 to1986 - yields a less than 1 percent negative impact on teen employment (and no apparent effect on young adults aged 20-24) after a 10 percent increase in the minimum wage and; b) since even this negligible impact is offset by a labor force withdrawal effect, then there is no discernible effect on measured unemployment.

[3] One of the implications of this heightened dispersion (credited to Deere et al., 1996, p. 37-38) is that the minimum wage relative to teen equilibrium wage would decrease less than the minimum relative to average wage or put differently, the number of directly affected teenagers would decline less rapidly than a relative minimum-wage variable would predict; a second one, which concerns teenagers not directly affected, is that it could either increase or reduce average wages and lead to supply responses (relative to trend).

But these are mere conjectures, and the matter remains far from settled. In fact, yet another trend appears to be emerging, if the ‘rehabilitating’ assessment of time series methodology by recent studies is anything to go by. Some of these recent studies, beyond seeking to address concerns such as why OLS time series evidence were robust up until the mid-1980s, but became relatively inadequate thereafter, attempt to introduce the modifications required to reinstate time series evidence as a highly robust (and perhaps the dominant) methodology. Others simply experiment with new specifications which they compare with ‘divergent’ cross-section based specifications that find little or no disemployment effects, and attempt to reconcile the differences between both methodologies, while maintaining the superiority of their proposed specifications. On balance, these new readings, if accepted, would reintegrate the standard textbook prediction of negative minimum wage effects for teenagers and, to a slightly less degree, for young adults.

The aim of this study is to review minimum wage analyses, with a special emphasis on the recent works that incorporate emerging trends in the minimum wage literature. However, beyond mere quantitative evaluations of possible disemployment effects, analysts of minimum wage laws often seek deep understanding of other equally important facets of the minimum wage. These include the impact of regional, cross-state or cross-national variation in average wage on both wage distribution and employment growth; the means through which firms might adjust to anticipated cost increases of the minimum wage legislation (raising prices, improving productivity, redistribution of firms’ income, layoffs/labor displacements and relocations, effort level adjustment by workers, and other offsets); the way different labour market policies and institutions influence the effects of minimum laws; and a characterization of the breath of policy issues and concerns raised in the link between minimum wage, income distribution and poor households.

The subsequent sections are organized as follows. Section II examines the theory of minimum wage floors. Section III explores traditional empirical evidence and challenges to that evidence. Section IV entertains a discussion on emerging trends as reflected in recent studies. Section V examines the possibility that both traditional evidence and the studies challenging this evidence are correct. Section VI offers (tentative) conclusions with suggestions for further research.

II. Theory

The basic model for evaluating employment effects of minimum wages is the neoclassical demand and supply model, alongside its well-known exception of monopsony. This traditional model focuses on a single competitive labour market with homogenous workers and a complete coverage of the minimum wage. Other extensions have also emerged in the form of partial sector (in a two-sector model) and heterogeneous analyses (defined below), 'shock models', and so on. I briefly consider each of these in turn.

A. Traditional Analyses: Competitive Labour Market

In a model with homogeneous labour and complete coverage, initial equilibrium levels of employment are set by the forces of demand and supply. After the imposition of a binding minimum wage, employers reduce the quantity of labour demanded. The extent to which a hike in the minimum wage reduces employment depends on the ability of firms to substitute other factors of production for the higher priced labour in response to the change in relative input prices and the negative impact the latter has on total output of the firm and the industry. This is equal to the percentage wage increase multiplied by the elasticity of demand. Thus if the demand for labour were relatively inelastic, the disemployment effects would not be severe.

Apart from the disemployment effects, an additional set of workers would be attracted into the labour force by the higher minimum wage, thus increasing the queue for the already reduced number of jobs. However, the impact of this excess supply on unemployment is not clear since what determines who is counted as unemployed or "discouraged" workers depends on what people report in the unemployment survey. Indeed, while a new minimum wage may encourage some to become active in their search for work, it could also discourage others who decide it is highly improbable to find jobs (Brown, 1999). This ambiguity does not, however, eradicate completely the harm done by the minimum wage.

B. Monopsony[4]

A minimum wage in a monopsony setting produces the opposite effect of increasing employment, rather than decreasing it, within some relevant range of wages. Thus it is regarded as an exception to the conclusion of negative effects of the minimum wage. A monopsonist is a firm whose large size relative to the size of the labour market permits it to set rather than take the wage at which it hires workers. In order to attract additional labour, the monopsonist has to raise wages; but if it lowers the wage rate, it will not lose all of its work force. As a result, a monopsonist faces an upward sloping, rather than a perfectly elastic, labour supply schedule. When the firm raises wages in order to hire additional labour, it has to pay that higher wage to its existing workers, which causes the marginal cost of an additional worker to be higher than the average cost. However, the imposition of the minimum wage serves to equate marginal cost to average cost by rendering a certain portion

[4] For a more detailed analysis, see Benjamin, D., Gunderson, M., and Craig Riddell, W. (2002)

of the monopsonist's supply and marginal cost of labour schedule coincident. In other words, this eliminates the constraints posed by the rising marginal cost associated with hiring an additional worker and induces the monopsonist to increase employment (within the relevant range: that is, only true if transactions wage is less than the competitive equilibrium wage), because although the wage it pays is higher, the marginal cost of expansion is lower since it already pays the minimum wage to its existing staff. However, how much the wage can be raised before the monopsonist's employment level begins to decline depends on the elasticity of labour supply.

C. Two-sector Model

Minimum wage provisions in most countries, and particularly in the U.S., have historically granted exemptions based on industry and size, so coverage has not been universal. Besides partial coverage, labour economists have also pointed to the importance of noncompliance with the law (Ashenfelter and Smith, 1979, Brown, 1999). Both these realities provide the basis for a two-sector model, whose covered sector's employment, like the model based on universal coverage, is affected by the minimum wage, while the uncovered sector's demand for labour depends on the market –determined wage in that sector. Workers displaced by the minimum wage from the covered sector move to the uncovered sector, whose flexible[5] wage falls as supply increases. However, since in response to the fall in wages in the uncovered sector, some of the displaced workers with higher reservation wages would still prefer not to work, it is clear, as Brown (1999) puts it, that activities in the uncovered sector may dilute but not totally eliminate the disemployment effects of the minimum wage.

D. Heterogenous Labour Model

This model was recently integrated into minimum wage analysis. Low-wage groups usually have in their fold (often doing the same tasks) other relatively skilled and better-paid workers who are only indirectly affected by the minimum wage. The members of this latter group are used as substitutes for workers directly affected by minimum wages, causing a decline in overall employment since it takes less than one skilled worker to substitute for one minimum wage worker, or to substitute one grade of labour for another. The danger in this is that the balance of overall disemployment effects of minimum wage may be small, leading erroneously to the policy conclusion that few low-skilled workers were hurt (Freeman, 1996 p. 642). Hence the justifiable focus on teenagers, blacks and females against, respectively, youth adults, whites and males, since the former group of workers has the highest concentration of the most disadvantaged (Brown, 1999, p. 2107).

[5] Several variants exist. In Welch's (1976) model, displaced workers from the covererd sector are mobile and will exert a downward pressure on wages in the uncovered sector; Gramlich-Mincer's (1976) model assumes the worker chooses one sector or the other and that total employment fall more than in Welch's; Brown et al. (1982) modify Gramlich-Mincer's model by, more realistically, allowing the uncovered sector worker to seek a covered sector job with a lower probability of success than the full-time candidates (this two-sector-with-queuing, in tandem with improvements in the seach efficiency, serves to reduce the disemployment effects of minimum wage).

E. “Shock Effects” Model

A minimum wage may “shock” firms (which were, before the increase, non-cost minimizing) into raising productivity through greater supervision and other means, thereby diluting or offsetting the disemployment effects. However, few authors doubt that “shock” can completely eliminate disemployment effects (West et al., 1980b); some also make the related point that any advantage conferred by shock effects is bound to fizzle out after the first round of productivity raising, since it is highly improbable that the firm repetitively undertakes these readjustment, especially in conditions of frequent minimum wage increase (Rees, 1973, Grenier and Seguin, 1991)

F. Offsets

Somewhat similar to the “shock” model, to the extent that they represent adjustment mechanisms or mitigating factors, are the offsets, which are measures that reduce worker compensation (such as training and other fringe benefits) and as a consequence entail less disemployment effects than would have otherwise occurred. However, the welfare costs of these offsets have not received much formal attention (Brown et al., 1982).

G. Effort Model

There is a variant of the traditional model, the effort-based traditional model, whose conclusions are remarkably different than those generally reached by the mainstream. According to this model:

i.) A minimum wage initially reduces employment and reallocates some jobs from high to low-rent workers.

ii.) However, this reduced employment subsequently triggers keen competition among workers and squeezes the low-rent marginal workers out of the market while retaining and attracting high-rent workers who are willing to ratchet up effort level.

iii.) This erodes the rent conferred by the minimum wage to low-rent workers and increases the value of marginal product (which reduces the size of the employment effect).

iv.) Thus if the minimum wage confers more money income on low-skilled workers, which creates some distortion in the optimum combination of wage rate and effort level, it has ultimately little effect in rent to workers, employment, total output and firm profits.

III. Traditional Empirical Evidence (First Phase) and Challenges to that Evidence (Second Phase)

A. Traditional Empirical Evidence

The majority of research work on minimum wage effects up to the early 1980s employed time series methodology. The basic model is:

$$E_t = \alpha X_t + \beta MW_t + \varepsilon_t$$

where the dependent variable E is the employment/population ratio in a given demographic group, and the independent variable X includes cyclical (or aggregate demand or business cycles) and other indicators. These other indicators usually consist of a time trend and other labour supply control variables such as school enrollment, the armed forces participation fraction, the relative share of teenagers in the labour-force age population, etc. MW is the level of the minimum wage relative to the average wage, and is usually multiplied by the fraction of employment covered by the minimum-- otherwise known as the Kaitz (1970) index.[6] Replacing E and MW by their logarithms makes $\beta = (\Delta \ln E)/(\Delta \ln wm)$ an elasticity measure. However, Brown (1999) notes that this not a "demand elasticity"[7] of the usual sort, because not all workers in E are paid the minimum wage.

Teenagers and youths were the primary focus of those studies (for being the most likely directly affected). They were often disaggregated by age (16-17, 18-19, 20-24), sex, race, enrollment status, and whether they worked full- or part-time. Unemployment rate effects dominated earlier studies; however, later studies shifted focus to employment rate effects. Also, the vast majority of the time series evidence measured employment by numbers or bodies employed and not by variation in hours per worker, which could be used to derive "full-time equivalents" (FTE). This practice was dictated by data availability issues rather than by preferences of minimum wage analysts for bodies employed. Based on the limited though largely imprecise evidence available to the few studies that explored this possibility (e.g. Gramlich 1976, BGK 1982), it would seem that employers reduced full-time equivalent employment more than bodies employed.

BGK (1982) find that the vast majority of the U.S. time series based studies surveyed up till the early 1980s – which involved diverse specifications with different sample periods but all using the same data source – associated a 1 to 3% decrease in teenage employment with a 10% increase in the minimum wage and were generally significant statistically. Estimates in the lower part of this range are however considered more plausible since they

[6] The Kaitz index has been preferred, or as BGK (1980) puts it, is superior to three other alternatives (which include the ‘‘real" minimum wage, the minimum wage relative to average wage, and dummy variables for changes in level or coverage of the minimum wage) due to its ability to summarise in a single statistic or measure all these information and even more.

[7] Defining E* and w* as the employment and average wage of those directly affected, he derives a measure of demand elasticity for low-wage labor as $\eta = (\Delta \ln E^*)/(\Delta \ln w^*)$. Alternatively, as Brown (1999, p.2155) further clarifies, minimum wage analysts usually multiply the minimum wage elasticity by about 5 in order to calculate the implied demand elasticity.

offer the best specifications. In an attempt to characterize the critical specification choices that determine the range of results, BGK identify such factors as treatment of coverage (coverage effects were weaker than level effects, both in statistical significance and magnitude)[8], assumption of instantaneous or lag adjustment, and to a relatively limited and imprecise extent, the presence of control variables such as training enrollment, fraction of teenagers in school or in the armed forces, etc. Minimum wages had little or no effect on employment levels of adults over 20 years, although this conclusion was borne out of uncertainty in the empirical work about the effects on adults. The uncertainty also extends to effects on low-wage industries (especially in retail trade) and areas, although estimates for low-wage manufacturing and agriculture were consistently negative. Relatively few studies were available on other groups such as women and on blacks versus whites.

Empirical evidence in Canada produced similar (though generally more substantial) negative estimates, on balance. Mercier's (1987) survey on Quebec and Canada for the 1966-1981 period suggested that a 10 % increase in the minimum wage caused between 0% and 0.9% reduction in employment; estimates in the 1-3 percent range were the least controversial and Mercier settled for the more realistic figure of 1% for teenagers and less than 0.05 for young adults aged 20-24. Swindisky (1980), based on a sample period covering 1956 to 1975, found that unemployment for males aged 15-19 increased by 0.5% after a 10% increase in the minimum wage, while that of females in the same age brackets climbed more drastically (6.8%). Estimates by Schaafsma and Walsh (1983) only covered the 1975-1979 period and suggested even stronger disemployment impacts of 11% and 7.9% respectively for male and female teenagers. To the extent that studies in Canada made use of cross-province variation in addition to time series data, there was some methodological difference in minimum wage researches in between the two countries.

B. Challenges to Traditional Empirical Evidence

In both Canada and the U.S., the nominal (real value) of the minimum wage remained constant (declining significantly in the U.S. and less so in Canada) for nearly a decade, but was slightly increased in the early 1990s. This provided the right setting for testing the validity of previous consensus. As it turned out, this consensus began to disintegrate as a number of studies failed to find evidence of a disemployment effect.

Before proceeding to a discussion of these empirical studies, however, it may be of interest to briefly consider the contexts in which it might be plausible, even in a fully competitive labor market model, to only have negligible disemployment effects of the minimum wage. This is what Ippolito (2003) attempts to show with his hypothesis that a fully competitive labor market will not tolerate much impact of a binding minimum wage if, as several empirical studies have revealed, workers can adjust effort level. The intuition

[8] The skepticism felt by some analysts regarding the assumption of the Kaitz index (which combines, together in one single equation, the ratio of minimum wage rates to average hourly wage calculated for each industry and weighted by the proportion of workers covered such that the weight for each industry ratio represents the number of persons employed in the industry as a proportion of total employment), which presupposes that a 10 percent increase in the level of the minimum wage creates the same impact as a 10 percent increase in coverage, leads these analysts to include separate measures of the levels and coverage of the minimum wage in the index.

behind Ippolito's theory, which is very similar to the well-known efficiency wage effect, is that the disequilibrium caused by the minimum wage in traditional models cannot persist and would be eliminated eventually by the competition arising from the need to secure the property rights to now-scarce jobs. Workers who attach more value to a job work harder and are retained by employers who tend to dismiss the less hardworking while continuously re-sampling the applicant pool for better workers. The competitive response from the supply side triggered by the minimum wage diminishes its economic impact by eroding most of the rent initially gained by some low-skilled workers and by attenuating through increased effort and higher labor productivity the impact of the wage increase, first on the employer's profitability (or output prices) and then on employment, because harder-working low-skilled workers now add more value to marginal product than in a free-market solution. Covered workers are worse off, even if they still are employed, earning higher money income at the expense of less rent (or reduced welfare) or disutility from harder work. Affected jobs are characterized by an inefficiently high wage-effort combination, as it is no longer possible in the new equilibrium to find a low-paying job that requires low effort (except in the uncovered sector).

However, despite making the point that there is no theoretical reason to believe that the impact of the minimum wage substantially affects employment of low-skilled workers, Ippolito still acknowledges that the impact of the minimum wage on employment is ultimately an empirical issue. This I pursue subsequently.

In Canada, Grenier and Séguin (1991) replicated and extended Swindisky's (1980) methodology to estimate the effects of the minimum wage on employment of teens aged 15-19. They divided their sample period into two time periods (1956-1975 and 1976 -1988) because of a break in the data series, and calculated a coverage-weighted minimum wage index, disaggregated by gender, for each of the five regions studied - the Atlantic Province, Quebec, Ontario, Prairie Provinces, and British Columbia. Their results revealed, like Swindisky's, a negative teen employment effect for the minimum wage for the 1956-1975 period (-0.13 for males; -0.45 for females), but a statistically insignificant effect for the more recent 1976-1988 period.

Fortin (1997) attempts a partial explanation of the divergent results obtained by Grenier and Séguin, with his argument that an uprating of the minimum would only generate negative consequences when the minimum wage / average wage ratio exceeds 50 %, while a ratio less than 45 % would pose absolutely no danger, with the 45-50 % range entailing increasing risk. Notice that in Grenier and Séguin's study this ratio falls between 50 and 60 % from 1965 to 1975 (period in which minimum wage effects are observed to be negative) while it ranges between 40 and 45 % from 1981 to 1988 (when the minimum wage increase has no disemployment effects). Further evidence of Fortin's ideas will be explored in the next section.

Several studies exist for the U.S. experience, the most influential being Card and Krueger's book (1995) and the related studies by the same authors. They took advantage of the "natural" experiment (or "difference-in-difference"[9]) approach to research extensively into the low-wage industries (retail and fast food) and to make comparisons between high- and low-impact states and low- and high-wage workers. Overall, they found no evidence of negative impacts of the minimum wage on employment, and in some cases they even found evidence of positive effects of the minimum wage on teen employment, reinforcing the prediction from a monopsonistic market.[10] Indeed, the clarity and simplicity with which Card and Krueger (CK) achieved their groundbreaking results, as well as the array of reactions those results have generated, warrant providing some details of their research.

In one of their cross-state, low-wage-industry studies, CK used firm-level data from individual fast-food restaurants in New Jersey and Pennsylvania collected before and after the April 1992 increase in the New Jersey hourly minimum wage to $5.05; the result shows a faster expansion in full-time equivalent employment in New Jersey (by 3.36; SE = 1.34) relative to Pennsylvania (the control state), where the minimum wage was constant.[11]

In two within-state comparisons, CK demonstrate that (a) employment increased faster (by 3.36 FTEs, SE = 1.3) in low-wage or high-impact restaurants[12] affected by the minimum wage within New Jersey relative to high-wage or low-impact restaurants that were initially paying more than a hourly wage of $5.05 and (b) roughly the same pattern emerged in an earlier study (Katz and Krueger, 1992) on Texas, where employment increased (decreased) by 0.168 in high impact (low impact) restaurants, but with no change in employment in the medium-impact restaurants. Based on a similar procedure to the New Jersey-Pennsylvania study, CK obtained elasticities of 1.85 (SE = 1.0) for employment measured in bodies and 2.64 (SE = 1.06) for full-time equivalent employment.

CK also looked at evidence in California (which experienced a minimum wage increase in 1988) and surrounding states that did not change their minimum wage laws; they noted that although teenage average hourly wages climbed by 10% in California relative to other states, there was no evidence of any relative decline in the employment of teenagers and other low-wage workers in California. Nor did this pattern change in their cross-states study of the effect of the1990-1991 federal minimum wage legislation on the predominantly low-wage retail industry. According to CK, these contradictory results (coupled with the fact that their reanalysis of previous consensus research on the basis of similar specification but

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[9] The "difference in differences" technique proceeds by first defining the appropriate "treatment" group (the high impact individuals, regions or states) and the "control" group (the low impact individuals or regions) and then calculating the differences in employment change between the two. The impact of treatment is thus estimated as (YT2 – YC2) – (YT1 – YC1) or as (YT2 – YT1) – (YC2 – YC1), where Y represents employment, T and C are treatment and controls, and 1 and 2 stand for the period before and after treatment.

[10] Interestingly, recent monopsony-based studies, whether in the U.S. or Europe, have consistently produced positive estimates of minimum wage effects. See for example Dolado et al. (1996) and Bernstein et al. (2000).

[11] CK obtained elasticities of approximately 0.34 (SE = 0.26) after regressing the proportional change in FTE on the required proportional increase in starting wage, or gap, in each of the states. They ruled out the possibility that the employment expansion might have been due to business cycle.

[12] Low-wage firms employ workers earning very close to the minimum wage and, relative to high-wage firms, are more impacted by a minimum wage increase.

with extended sample period yielded no significant disemployment effects) were too conspicuous and too numerous not to cast doubt on the validity of previous consensus estimates.

CK's results did not go unchallenged, however. One criticism by Hamermesh (1995), related to the New Jersey and Texas experiments, is that the survey used to measure employment levels was conducted too close to the date of the minimum wage increase (so that employers' awareness of the impending increase would have triggered some staffing "pre-adjustment", and therefore missing out on some of the employment responses) and too soon after the law took effect, leaving not enough time for any serious adjustment and creating bias in the results. CK dismiss this criticism with the reasoning that high quit rates in the fast-food industry, coupled with the uncertainty surrounding the legislation prior to implementation, cause adjustments to occur with neither leads nor lags. Strengthening this view is Brown's (1999) observation that assuming inappropriate timing did actually dampen the disemployment effects, it could not have changed the sign.

Kim and Taylor's (1994) within-state reanalysis of the impact of the California's 1988 minimum wage using County Business Patterns (CBP) data, suggests reductions of 5 percent and 8 percent respectively for retail trade and restaurant employments in 1988-1989, the year following the minimum wage increase; Kim and Taylor's position is that robust demand in the form of unobserved demand shocks in 1988-1989 must have offset the employment loss in CK's analysis. In response, CK fault Kim and Taylor's specification (which used both lagged values and average establishment size as instruments for wage growth)[13], which they establish through tests to: a) be biased toward large, negative values and; b) show no evidence of correlation – between establishment size and industry wages.

Welch (1995) argues that the employment gain experienced by fast-food outlets was probably at the expense of other low-price, "mom and pop" restaurants (not included in the sample) that might have been forced to close down by the minimum wage increase; CK counter by observing that both higher-wage restaurants that were not directly affected by the increase and low-wage restaurants had the same employment gains.

Neumark and Wascher (1995, 1998) focus on the quality of CK's data, which are based on telephone survey. Using actual payroll records from fast-food restaurants from the same geographic areas and chains as CK's sample, they find the employment variation in CK's data to be "implausibly large" (up to four times larger than is observed in payroll data, which has a higher correlation of 0.81 relative to a correlation of 0.52 found in the telephone survey) with the patterns of differences being consistent with rather severe measurement

[13] To explain their use of instrumental variables Kim and Taylor observe: a) with respect to the lagged value of the average wage, that a low average wage reflects a larger wage increase in response to the minimum wage and; b) that average firm size is positively correlated with relative wage growth and since this positive correlation indicates noncompliance behaviour, then larger firms are more likely to comply with the minimum wage law, causing a larger impact of the law on industries with larger average firm sizes.

error. And their regression, contrary to CK's claim, reveals a 4.8 % decline[14] in FTE employment in New Jersey relative to the Pennsylvania control group.

However, when CK (2000) attempt to reconcile these contrasting findings (using the Bureau of Labor Statistic's employer-reported data file to analyse administrative employment data), they still find that employment grew faster in New Jersey than in Pennsylvania, but this time the differential is small and statistically insignificant. Neumark and Wascher (2000) are skeptical about this approach as well. They think "the longitudinal data in this period suffer from the potential problem of changes in the level at which data are reported, because the Bureau of Labour Statistics was in the process of encouraging multi-establishment reporters to provide their employment estimates at the establishment or county level rather than grouping all of their establishments together."

To summarise, CK's contributions to the minimum wage research has been important both for policy and theory; but their premise and conclusions have been hotly contested from the viewpoints of methodology, quality of data, timing of sampling, the likelihood of unobserved demand shocks, etc. We shall have occasion to revisit some of these issues in greater detail in the next section.

[14] By merely replicating CK's difference-in-difference estimation with the payroll data, they find a 3.9 - 4.0 % decline in fast food employment in New Jersey relative to Pennsylvania.

IV. Recent Studies (Third Phase)

Thus far two important phases have been identified in the history of the minimum wage empirical research in both Canada and the U.S. Earlier traditional studies (extending up till the early 1980s and based on time-series methodology) that predicted a "consensus" teenage disemployment effect of 1-3 percent following a 10% increase in the minimum wage, dominate almost entirely the first phase. The second, more recent phase (whose precursors are Card and Kruger for the U.S. and Grenier and Seguin for Canada) is dominated by panel-data based research works that found insignificant and in some cases even positive effects of the minimum wage. There was a chain of reactions and counter-reactions generated by such "challenging findings".

Recent readings of the literature would seem to have identified a third, incipient phase (discussed in the next several paragraphs) revalidating to some extent the consensus estimate of a negative employment effect of the minimum wage.

Baker, Benjamin and Stanger (1999) took advantage of the unique experiment afforded by the Canadian data relative to the U.S., in order to estimate minimum wage effects based on the 1975-1993 sample period.[15] They estimate only for teenagers (ages 15-19), and do not disaggregate on the basis of gender. They find a significantly negative (-0.25) teenage minimum wage elasticity, which, they argue, is driven or dominated by low-frequency variation in the data.[16] In most provinces, there is a prominent spike at the adult minimum wage, but the spike tends to be somewhat less prominent in provinces with subminimums.

One implication of the dependence of the elasticity on low-frequency variation, they argue, is that employment adjusts to long-run, evolutionary changes in the minimum wage, which in turn suggests that employment dynamics in the minimum wage sector are not well described by short-run labour-adjustment costs, but by explanations that focus on the turnover of firms, or on the substitution of alternative factors that have longer

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[15] Federal minimum wage laws in the U.S. (in the 1980s when minimum wage remained virtually stagnant and Federal minimum wage still superceded those of states) occur at the same time and nationwide, and do not allow researchers to observe what would happen to employment in the absence of a minimum wage increase; unlike in Canada where control and implementation are at the provincial level, allowing researchers to conduct a quasi-experiment and compare the impact of minimum wage increases occurring at different times in different provinces. Thus even though two recessions occurred in Canada (in 1982 and 1991), Baker, Benjamin and Stanger are able take advantage of these different legislative timings across provinces to separately determine the portion of the decrease in employment associated with the recessions and the portion related to minimum wage increases.

[16] They find that the effect of minimum wages on teenage employment varies according to whether low (which is based on annual data) or high frequencies (based on monthly data) are applicable. At low frequencies, which correspond to cycles of roughly 6 years or more, they obtain elasticities that are at the upper end of the range of consensus estimates; at high frequencies, however, the elasticities are small, insignificant, and sometimes positive. Frequencies, or time-domain-based analyses are lengths that make up the time series variations in the data, and they serve in this context to reflect the behaviour of the employment dynamics of the minimum wage over time.

replacement/planning horizons. In other words, the before-and-after measurement used in most "natural experiments" may capture short-run minimum wage variations that do not elicit a firm or market response, while long-difference estimators of the elasticity will be needed to capture the minimum wage effect. Generally speaking, then, quasi-differencing puts greater weight on the high-frequency variation in the data and should lead to a smaller estimate of the elasticity.

Another, probably more subtle implication, is that Baker et al. believe they have unraveled the mystery behind why recent studies (that are based on case-study or “naturalist” approach) have been estimating insignificant or positive minimum wage effects, as well as the shortcoming or unsuitability of any such methodology. Elasticity inferences of these recent studies are based on short (typically first) differencing of the data, which they claim relies on the high-frequency variation in the data and prematurely censors employment adjustments. An appropriate response to these new findings, they conclude, is research that emphasizes the importance of lags and the manner in which the data should be filtered before analysis (since the minimum wage effect is not constant across frequencies).[17] Whereas shorter lags, up to the second lag in their study, make little difference on long-run elasticities, longer lags do have a substantive effect, as evidenced from the fact that the fifth and sixth lags of annual data produce elasticity estimates greater than -0.6 (table 7). Against the odds that the short range of data sample used in performing this aspect of their experiment (1981-93: only 13 years) might raise suspicion of the existence of spurious relationship in longer lags, however, the authors go to considerable lengths to demonstrate ex ante, with the low/high-frequency decompositions in table 5 and the spectra analysis in figure 6, that it is highly likely these longer lags are highly significant.

A major question they attempt to address is how these lags would be accommodated in a dynamic low-wage labor-demand model. One mechanism, Baker et al. observe is the employment variation that could result from the substitution of alternative factors of production over longer planning horizons, such as capital adjusting in response to the cost of capital. The higher costs of this longer-term substitution -higher than those of short- run employment variation that does not involve a change in the capital stock- may rationalize the importance of low, rather than high, frequency changes in the minimum wage. In addition, over a period of six years and above the adjustments may be interfirm or market effect (which can occur via a change in the market composition through the turnover of firms), rather than intrafirm or firm effect (which merely constitutes the actions of specific firms).

A second mechanism they identify is expectations, which, not surprisingly counters Brown’s (1999) view on the role of a related concept (which Brown refers to as “leads”). Firms' "estimates" of teen labour costs adjust slowly to changes in the minimum wage, and

[17] Data can be filtered (or allotted different weights) on the basis of the length of lags and according to whether short or long differences apply. Any filters-based estimation method should have predictable effects on the estimates of the minimum wage elasticity. For example, comparing both the Lagrange multiplier (LM) test for first-order autocorrelation and generalized least square (GLS) estimates of the elasticity (which involves correcting for serial correlation quasi-differencing the data) shows that although the effects of the GLS correction are negligible on the standard-error estimates, they are quite dramatic on the parameter estimates.

apparently do not lead firms to substantially revise their expectations of wage costs. However, in choosing the capital/labor mix, firms tend to focus on longer-run (low-frequency or "permanent") moving averages rather than period-to-period differences in the minimum wage. This, they believe, lends credence to Sargent's (1978) impression that lagged wage coefficients may reflect both the role of expectations and adjustment costs.[18]

Within this framework, therefore, Baker et al. conclude that the different estimates of the minimum wage elasticity are not so much the basis of debate but rather pieces of evidence that, when considered together, document the dynamic adjustment of employment to minimum wages over different lengths of lags. Estimates from lag specifications emphasize the long-run adjustment of employment to low-frequency changes in the minimum wage, whereas the case-study method relies on the high-frequency covariation of minimum wages (which, they were able to confirm, is small) and employment.

The importance of Baker et al.'s contribution, and that of most of the studies I will discuss presently, can only be fully appreciated within the context of a commonly identified research gap[19] in the literature. There is no gain in stating the fact that a lagged dependent variable incorporates sluggish response of employment to changes in the fundamental determinants of labour demand or helps to account for omitted variables that evolve slowly and are not already captured by the other control variables in the model. Brown's (1999, p.2119; 1982 p.496) conclusive remark on the issue of leads and lags acknowledges that "the data are not rich enough to identify long-term responses" (assuming these are different from short-time effects), a situation that causes analysts to embrace the simpler alternative of contemporaneous-response assumption of the minimum wage. Nonetheless, this issue continues to "highlight the largest and most important gap in the minimum wage literature" (Brown, 1999, p.2119). A similar concern figures prominently among CK's (1995, p.398-399) identified list of high-priority areas for future empirical work.[20] Also, Goldberg and Green (1999) admit to a relatively significant longer-term employment effect in Canada (0 to 2%), notwithstanding their finding of little or no immediate impact.

Interestingly enough, the importance of the longer term had been recognized for over two decades (see for example an extensive discussion by West and Mckee's, 1980, p.59); but as was mentioned in the previous paragraph, the difficulties it present seem rather daunting, and this is, at the very least, the sense in which Baker et al. may be considered to have added some value.

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[18] This would typically be a reduced-form employment equation that makes the assumption that firms adjust for inflation by reacting to moving averages of the minimum wage rather than period to period differences.

[19] Another such gap, on which we shall make a few comments in due course, is the measurement of employment by hours rather than bodies employed.

[20] Their words are: "Although we have presented some longer-term comparisons, the bulk of the evidence in this book is based on changes that have occurred over a period of one to three years. It is possible that the full impact of a higher minimum wage will become apparent only after a relatively long time" (p.398). Neumark and Wascher (2003), to be discussed later, argue that additional factors, such as strict legal restrictions on dismissals in Europe, should make for an even slower adjustment.

The study by Bazen and Marimoutou (2002) is an interesting one. Using the same data set (as BGK, 1982, and extended up to 1993 by Card and Krueger) to re-examine the time series effects of minimum wages on employment of teenagers aged 16-17, they analyse the basis and stability of time series models (based on the specifications used in the 1970s) with a view to exposing their inability to account for observed changes in (trend, cyclical and seasonal components of) teenage employment in the 1980s and early 1990s, and proposing an alternative empirical model that overcomes such shortcomings. The study finds that a more flexible structural time series model that treats the unobservable trend, cyclical and seasonal components of teenage employment as stochastic, and that includes the traditional, deterministic approach as a special case, successfully accounts for changes in teenage employment in the period since 1980 and yields a long run, negative, statistically significant and reasonably stable estimate of 2–3% in response to a 10% increase in the minimum wage. In other words, they stress, that the negative sign and size of the effects have hardly changed during the 1980s and early 1990s from those obtained up to the 1980s.

They observe that earlier models estimated with time series data through to 1979 become unreliable when the data run is extended through the 1980s and 1990s because the specification of the trend, seasonal and cyclical components was a deterministic, local (i.e. short-lived) approximation, and the deterministic nature of the trend and seasonal components end up dominating the predicted time path of the out-of-sample teenage EP ratio. However, the more flexible structural model proposed by Bazen and Marimoutou yield minimum wage elasticity estimates that are stable[21] through the 1980s and 1990s and, contrary to the time series estimates realised by Card and Krueger, are increasing in statistical significance as the sample period is extended (appearing to be higher in the 1990s). In the short run, the elasticity is around -0.1, rising to -0.3 in the long run. As the real value of the minimum wage falls over the 1980s, teenage employment increases relative to what it otherwise would have been. The increases implemented at the beginning of the 1990s significantly reduced teenage employment – a conclusion contrary to that obtained by Card and Krueger in a number of cross-section contexts.

Specifically, the authors make the point that in various reviews of Card and Krueger's cross-section-based work – which forms the main basis for claims that there was no negative employment effect associated with increases in the minimum wage – there is concern about the data not enabling the long run effect (that would be negative and significant) to be identified. Their present results, they claim, as well as those of recent studies examining employment-minimum-wages relationship in the U.S. using a fairly long run of state-based data (they cite in this regard Burkhauser, Couch and Wittenberg, 2000, and Keil, Robertson and Symons, 2001), vindicates this concern, as the long run effect is more than twice the size of the short run effect.

21 The forecast error and CUSUM plot are always within two standard errors of zero. Further test of stability consisted in re-estimating the model over a period longer than the initial one of 1954–79, i.e. the period 1989:4 (the last year for which the nominal minimum wage was frozen; overall the estimated long run elasticities are not dissimilar for both periods. This, the authors claim, is a mark of stability.

The authors conclude that their model successfully tracks the time path of the teenage employment population ratio within and out of sample[22] by treating the seasonal, trend and cyclical components as stochastic; and that it also appears to be consistent with the processes generating the data since the various diagnostic tests suggest no evidence of model misspecification (the hypotheses of the absence of autocorrelation and heteroscedasticity, and the normality of the errors are not rejected).

They specify their structural model as $EPt = \alpha 1MWKt + \alpha 2Mt + \alpha 3Awt + \alpha 4EPt\text{-}1 + \mu t + \psi t + \gamma t + \varepsilon t$, where the stochastic specification of the trend, cyclical and seasonal are respectively μt , ψt, and γt and white noise is εt. There is greater flexibility in the representation of unobservable influences (which is to be expected, since all non-parametric forms give greater flexibility).[23] The stochastic trend picks up any slowly changing demographic factors. The dependent variable is defined in terms of the overall teenage population, which already controls for the key demographic factor affecting teenage employment. In case there is autocorrelation and rejection of an AR (1) 'correction' as a solution, the lagged dependent variables will reject any sluggishness in the adjustment of teenage employment to its desired level. Firms' desired adjustment to changes in labor costs are not expected to be instantaneous, since quarterly data are used. To resolve Kaitz-index related problems, the minimum wage and average earnings are entered as separate variables.[24]

Having persuasively marshaled their arguments, Bazen and Marimoutou draw two obvious implications related to methodology and magnitude of estimates in the U.K. research, both of which they consider important in the context of the debate that followed the work of Card and Krueger in the US. They establish that: a) although existing impact studies in the UK suggest little or no disemployment effect of the minimum wage, the longer run may be more important, since over time alternative forms of adjustment are open to firms (such as the choice of technology and the progressive substitution of low paid workers affected by the minimum wage by machines or by other types of worker) and; b) the stability of the (relatively small) size effect found in the U.S. is valid only within the relevant range, and the imposition of a minimum wage level similar to those in France and the Netherlands could yield substantially larger elasticities.

At this point, a comment or two might be useful to situate their work in a proper perspective. Brown's (1999, p.2119-2120) various conjectures on the puzzle posed by the

22 Two increases that enable their model to be tested out-of-sample were (a) October 1996 increase from \$4.25 to \$4.75 and (b) September 1997 raising to \$5.15, and these data were only made available to them after a previous version of this paper.

23 They explain the economic interpretation of the unobserved components as follows. The trend incorporates technical progress, underlying trend productivity growth, social and (e.g. increasing tendency to remain in education), demographic changes, etc. The cyclical component is (not necessarily the business cycle) depends on recruitment rates for young persons entering the labor force for the first time and this concentrated in a small subset of economic sectors. Seasonality reflects demand-related employment variation and teenage employment, due, for example, to breaks in the school year. Its stochastic nature allows the impact of seasonal factors to change over the relatively long sample period, running from 1954 to the 1990s. For example teenage employment could shift towards sectors that exhibit less seasonal variation or teenage intensive sectors themselves could experience less seasonal variation over time as consumption patterns change.

24 They justify this separation with the data asymmetry. For instance, the adjustment parameter of 0..514 (in their Table 2, Col. 2) suggests that half of the adjustment occurs in the same quarter as the minimum wage increase, a further quarter in the second quarter and the adjustment completed by the fourth quarter. Also see footnote 8 for more details.

increasingly insignificant estimates of recent times series studies cause him to reflect on the role that data handling and specification could play. His (correct) impression is that experimentations with a more flexible functional form to predict how much the decline in the relative minimum wage should have reduced elasticity estimates have not been very successful, though he also admits in another context that specifications that successfully include more control variables, such as time trends, school enrollment, participation in the armed forces and the relative share of teenagers in the labour-force age population, are generally preferred. It is probably too early to estimate the extent of success of Bazen and Marimoutou's experimentation; but at any rate, the same comments made earlier regarding the merits of Baker et al.'s contribution are equally applicable to them

Another study by Yuen (2003) exploits the generally heterogeneous aspect of low-wage labour to estimate minimum wage effects in Canada, and draw implications for research design and policymaking. The issue of heterogeneity appears important enough to have deserved an entire subsection in Brown's (1999, p. 2106-2107) compact survey. The long and short of Brown's reflection on this is that: a) low-wage groups usually include (although there is no observable skill indicator that can be used to make this "neat" distinction) both the unskilled workers that are directly affected by minimum wage laws and other relatively better-paid workers who in the event of an increase may be substituted for the former and; b) disemployment effects of the minimum wage may "reflect an unattractive balancing of gains by relatively advantaged workers and losses by those directly affected."

Yuen's sample covers the 1988 to 1990 period and contains 71002 observations on 9379 individuals from the Labour Market Activity Survey (LMAS); of which 4379 are teenagers aged 16-19 (in 1988), while the remaining 5000 are young adults aged 20-24. In making his submission, Yuen observes the following: a) that previous U.S. panel estimates of minimum wage effects have been criticized on the grounds that they are based on comparisons between low wage and high-wage workers; and as a result, the estimated disemployment effect may be driven by the difference in employment stability between the two groups; b) that a Canadian panel data (1988 to 1990) seems to support this criticism; c) that when high-wage workers are included in the control group, changes in the minimum wage have a strong negative impact on low wage employment; c) that this result is consistent with other U.S. panel studies of minimum wage and finally; d) that the estimated minimum wage effect nonetheless becomes insignificant once the control group is limited to low-wage workers in provinces with no minimum wage change.

When Yuen proceeds to a closer examination within the low-wage workers, however, he discovers considerable heterogeneity in impacts based on the differences in skills/lengths or stability of employment. For 'transitory' low-wage workers, who have fewer than 3 quarters of low wage employment throughout the sample period, the effects of the minimum wage are virtually zero. There is a statistically significant disemployment effect, however, for the complementary group. For workers with more than 3 quarters of low wage employment, a minimum wage increase of 8.4 percent leads to a 7 percent decrease in teens' employment.[25] The effect on young adults is even greater at 10 percent. Most of the

[25] Comparing this set of results with other minimum wage studies requires some care, as a very small portion of the entire youth population (9.5 percent of the teens and 2.4 percent of the young adults) forms his basis of analysis.

'transitory' low-wage workers are either full time students working in low paid summer jobs or 'high wage' workers temporary trapped in low wage positions. Thus, their current wage rates are likely lower than their marginal productivity, and they are less likely to be affected by the minimum wage.

A few details about how Yuen reached his conclusions might prove instructive. First, he follows the 'traditional' approach (in which he includes all employed individuals) by modeling the probability of being employed in the period after the minimum wage (i.e. re-employment equation) as a function of a set of control variables and the variable measuring exposure to the minimum wage increase. The OLS estimates of this linear probability model (as per his Table 4) reveal a negative and statistically significant coefficient estimates for affected teens and young adults. On average, after an 8.4 percent increase in minimum wage, 'at risk' teenagers and young adults are, respectively, 6.9 and 14.8 percent less likely to be re-employed.[26]

In spite of these results, Yuen is under no illusion that his OLS estimates would not be subject to the same criticism of bias leveled at other panel studies of minimum wage. Indeed, given the fact that his "control group" consists primarily of high-wage workers, if high-wage workers are 'better' than low-wage workers in some unobserved quality that has a positive effect on employment and earnings, the results from the OLS estimation would be biased since they also capture these 'high-low' differences.[27] (So, an important variable is the difference between the marginal revenue product and the wage).

To address this problem, Yuen refines the control group to include only 'low wage'[28] individuals in provinces with no minimum wage increase, and then re-estimates the baseline equation using panel-data OLS. The result is a much more precise estimate that is negative but insignificant (even at 10 percent confidence level) with large standard error for teens and for young adults. This evidence (i.e. the elimination of minimum wage effect on youth employment as a result of the restriction of comparison group to low wage individuals) confirms the criticism of previous U.S. panel studies concerning the identification of an appropriate control group, and motivates Yuen to subsequently apply Fixed Effects (to control for the unobserved heterogeneity), which causes the estimates to be negative and statistically significant for both age groups.[29]

Yuen then observes that this seemingly homogeneous group of low-wage workers can be divided into 'transitory' (excluded sample) and 'permanent' (FE sample) low-wage workers, and that an increase in the minimum wage displays different effects on both groups – it has significant impact on the 'permanent' low-wage workers employment, but is trivial

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[26] This is the average percent increase of all 19 provincial minimum wage changes weighted by the number of 'at risk' teens in each change.

[27] One quick experiment seems to vindicate his suspicion: the inclusion of fixed effects (FE) elements to control for the unobserved heterogeneity between the two wage groups yields estimates that, though still statistically significant, are slightly less negative than the OLS estimates, a fact which, in addition to the Hausmann specification test results, constitutes evidence of correlation of the unobserved heterogeneity with the regressors.

28 This is to eliminate the unobserved heterogeneity among workers, since similar low wage individuals (not affected by minimum wage) will very likely experience similar conditions and therefore display similar employment stability.

[29] For a detailed exposition of the fixed effects methodology, see Appendix 1.

on the 'transitory' individuals.[30] Most previous studies use current wage rates to identify low-wage workers as those who are the most likely affected by the change in minimum wage policy. For Yuen, the difference between the OLS and FE estimates cannot be solely explained by unobserved heterogeneity among workers, but also by the insufficiency of using the worker's current wage rates to identify the "directly affected" (relying on wage may, for example, underestimate the marginal products of those with extremely limited employment histories).

To conclude, Yuen argues that from a public policy perspective, these results suggest that an increase in the minimum wage is very unlikely to lead to a reduction in overall youth employment level, even for the 'transitory' low-wage workers; although it would, nonetheless, have a significantly negative impact on workers with poor economic prospects, thus establishing the existence of a tradeoff between wage level and employment for those who are the primary focus of the minimum wage policy. That is, when both the treatment and the control groups are defined appropriately, the standard 'textbook prediction' of a negative employment effect, he maintains, still applies.

It is not yet clear how minimum wage analysts will react to Yuen's work, especially given the fact that: a) his estimates appear to be rather on the high side and; b) there may well be justifiable concerns about the possibility of spurious associations caused by the relatively short range of his data sample – a concern he does not appear to have addressed. Nonetheless, the ingenuity he applies in addressing the heterogeneity issue and in identifying the appropriate treatment group, seems credible and commendable.

A study by Quebec's Department of Finance (2002) estimate six equations to isolate the impact of the minimum wage disaggregated by age and sex on employment in Quebec from 1981 to 2000. Half-yearly data are used since minimum wage was increase occurred at most twice yearly during the course of the study period. OLS method is used. Autocorrelation problems are corrected for by the use of auto regressive moving average (ARMA) method. The model estimated is:

$$\mathbf{E1 = B0 + B2Y + B3w\ min + \sum f{=}1\ 6\ Bf{+}3\ Nj + B7t + \varepsilon\ t}$$

where E1 represents employment of male and female aged 15-19, 20-24 and 25 and above; Y is the real Quebec's real GDP; w min is real minimum wage; Nj is the population of the sample under study in Quebec; t is the trend variable.

The results (replicated in the following table) appear to be consistent with the traditional prediction of 1-3% disemployment effect of a 10% increase in the minimum wage on teenagers.

[30] The transitory workers have less than three quarters of low-wage employment over the sample period, are mainly students working in low-paid jobs during the summer whose current wage rates are lower than their marginal productivity, or 'high-wage' workers temporarily trapped in low-wage positions, and are less likely to be affected by the minimum wage; the permanent workers have longer, low-wage employment history.

IMPACT of 10 % INCREASE IN MINIMUM WAGE ON EMPLOYMENT (BY AGE AND SEX)

Age and Sex	Impact on employment
Male	
- 15 to 19 yrs	-1.93 %
- 20 to 25 yrs	-0.62 %
- 25 yrs and above	Insignificant
Female	
- 15 to 19 yrs	-2.75 %
- 20 to 25 yrs	-0.5 %
- 25 yrs and above	Insignificant

Specifically, male teens are worse hit than female (by 40 %), but females aged 20 to 24 are worse hit than their male counterparts (by 20 %). Furthermore, the negative impact borne by male teenagers are three times more than males aged 20 to 24; for female teenagers, it is five times.

As per the fit of the model, the R^2 statistics for four of the regressions are impressive, exceeding 0.98. The lower fit for males and females aged 15 to 19 (0.85 and 0.92 respectively) suggests that such variables as parents' financial situation and level of schooling could be important in explaining employment trends for this age bracket.

The study, however, cautions against concluding that the insignificant coefficient recorded for adults implies zero impact of minimum wage as a more appropriate specification with larger fraction of adult population (in the present report, adult proportion is well under 40%; Fortin's 1983 study gives a better specification) might well reveal a statistically significant effect for this age bracket.

The Department of Finance also attempts to verify the applicability of the 'threshold of risk' of the minimum wage / average hourly wage ratio as established by Fortin; the results partially confirm the danger of a ratio in excess of 50 % but nonetheless still signals some danger during the years in which the ratio[31] only averages 41.5 %. While this inconsistency may be a signal for further research in establishing an appropriate range for the threshold of risk, the robustness of the upper bound estimate of 50 % appears to have been earlier reinforced by Shannon and Beach's (1995) paper, which uses the LMAS database to explore

[31] In Quebec, the ratio varies between a low of 37.4 % (in 1986) to a high of 46 % (in 1995). Although the level of nominal minimum wage in Quebec has since 1983 been comparable to those of Ontario and the U.S. federal minimum (for Ontario the ratio varies between 35.6 % in 1983 and 45 % in 1995; for the U.S. it was from 35% in 1989 to 42% in 1983), the fact that it has a relatively lower average hourly wage makes for a higher minimum wage /average wage ratio than those of Ontario and the U.S. Other international comparison (by Metcalf, 1999) shows that in 1998, the ratios were generally higher for continental Europe, where it ranged from 50 – 60 %, to the exception of Spain where it stood at 32 %; Canada and the U.S.

the potential distributional effect of an increase in the Ontario minimum wage to 60 percent of the average Ontario wage; they find a disemployment effect of -0.12 to -0.15 percent, with most of the affected workers being women, young (60 percent) and part-time workers (with high-school education or less), and half full-time students; the Retail, Accommodation and Food industries are the most hit.

This brings us to the innovative study by Neumark and Wascher (2003) who, exploiting cross-national variation in the minimum wage, and based on a pooled cross-section time-series data set from OECD published sources on 17 OECD countries for the period 1975-2000, examine the way different labour market policies and institutions influence the effects of minimum wages on "teenagers" (15-19 years), "youths" (15-24 years) and "adults" (25-54 years).[32] Their empirical procedure is based on tests of Coe and Snower's (1997) model on labor market policy complementarities, which claims that stricter job security measures, more generous unemployment benefits, and greater bargaining strength for incumbent employees tend to exacerbate the negative employment effects from an increase in the minimum wage, while policies designed to increase rates of job creation tend to mitigate those effects.

The results obtained by Neumark and Wascher show general evidence of negative, statistically significant employment effects of minimum wages (around -0.18 for teenagers, -0.13 for youths and no effects for older workers aged 25-54). A clear negative correlation between the level of the minimum wage and youth employment-to-population ratios appears both in the raw data, and in time-series cross-section regressions relating employment rates to minimum wages, with controls for overall economic conditions and cross-country variation in labor market policies and institutions. The disemployment effects also appear in models that control for country-specific factors (including country-specific time trends) using fixed effects,[33] indicating that the results are not solely driven by cross-country differences in minimum wage levels and youth employment rates. (Fixed effect approach allows each country to have different wage equations since there may be unobservable differences in each country-specific factors, such as youth employment programs as well as cultural or other institutional variables across countries that may lead to cross-sectional variation in the propensity of youths to work. Thus year effects control for global shocks or policies that might influence youth employment rates in all countries, while country-specific trends are intended to capture factors that might influence employment trends within a country)

The evidence also suggests that the impact of minimum wages differs substantially across countries. One reason is that other cross-country differences[34] play a key role in

[32] In fact, the authors acknowledge an earlier exploration of this cross-country approach by a recent OECD study (1998) which, based on pooled time-series cross-section regressions for seven OECD countries, estimate negative and statistically significant minimum wage effects on the employment rates of teenagers across a variety of specifications and a consistently negative, though statistically insignificant, point estimates of the minimum wage employment elasticity for 20-24 year-olds.

[33] Hausman/Sargan tests strongly reject the hypothesis that the fixed country effects capturing country-specific factors such as youth employment programs as well as cultural or other institutional differences can be omitted from their specification.

[34] From their descriptive statistics on Table 1, continental Europe has the highest minimum wage levels while Japan and other Anglo countries (or those whose wage floor is set by statutes, not by active unions), except Australia, have the lowest levels with ratios of about 45 percent or below in 2000; over 25 years, the worst declines in the youth employment-to-population ratio comes from Netherlands, Greece and Spain, and the only meaningful increase is from Luxembourg.

minimum wage systems. Neumark and Wascher find, in this regard, that the negative relationship across countries between youth employment and minimum wages is smaller when the wage floor is set by collective bargaining process rather than by regulation, and that youth sub-minimum wage tends to reduce the negative impact of the overall minimum wage on teen employment. In contrast, countries with substantial regional or industry variation in minimum wage rates tend to exhibit larger negative minimum wage effects on youth employment rates.

(The issue of how subminimum wages can dampen the disemployment effects of the minimum wage received an earlier attention of the same authors (Neumark and Washer, 1991) who constructed a U.S. panel data on teen and young adults for the 1973-1989 period. They attempted, in a different context, to address the empirical question of whether states with subminimum wages exhibit higher employment rates, after controlling for the state minimum wage level and other labour market conditions. An earlier study (Katz and Krueger, 1991) that focused on the Federal subminimum, as opposed to state-level subminimum, detected no evidence of impact. But N.W. found evidence (in their Table 10) that the youth-subminimum wage, and NOT the student subminimum, moderate the negative effects of the minimum wage on teenagers, with their (statistically significant) coefficients ranging from 0.04 to 0.05 for contemporaneous specifications and 0.02 to 0.03 for lagged specifications. This makes for an offset amount of approximately one-third of the estimated disemployment effects of the minimum wage (which is -0.15 for both teen and young adults). The plausibility of these results was, however, challenged by Card, Katz and Krueger (1993) with the argument that employers do not frequently use subminimum wage provisions. In response, Neumark and Washer (1993) saw no inconsistency between low rates of usage of subminimum wage provisions and the fact that such provisions cushion minimum wage effects, because the estimated disemployment effects of minimum wages are small relative to teen employment)

A second explanation advanced for the difference in impacts - and on which the cross-national study focuses more attention -is the presence of other labor market policies or institutions could either exacerbate (e.g., rigid labour standards, such as restrictions on hours' adjustment[35]) or mitigate (e.g., stronger employment protection policies or greater use of active labor market policies to reduce unemployment) the effects of minimum wage laws on employment. On balance, however, minimum wage laws have a more pronounced impact on the least regulated labour markets in the sample -namely the U.S., U.K., Canada, and Japan- thus highlighting the importance of accounting for institutional and other policy-related differences in cross-national minimum wage analysis. The following table or matrix tells the story in a more visually appealing fashion. The labels (a-j) at the bottom right are for identification purposes only.

[35] Neumark and Wascher stress that the relatively higher point estimates in this regard (-0.30 for all youths and -0.39 for teenagers) indicate that other specifications are weakened by the omission of information on labor market policies and institutions, causing the coefficients to be attenuated.

measures	***more restrictive / high level enforcement***	***less restrictive / low level***
Measure of generosity of unemployment insurance programmes	Exacerbate unemployment generally; but is irrelevant to disemployment effect of Min. Wage **(a)**	Mitigates unemployment **(j)**
level of employment security from unions	exacerbates instead of mitigating Min. Wage disemployt effects and rel. wage changes **(b)**	Mitigates disemployment effects of Min. Wage **(i)**
public expenditure on defined programmes or active labor market policies	mitigates disemployment effect **(c)**	exacerbates disemployment effect **(h)**
level of rigidity of labor standards	exacerbates disemployt effect **(d)**	mitigates effects **(g)**
level of employment protection regulations	mitigates disempyt effect **(e)**	exacerbates effects **(f)**

Signs
a + d = relatively lower youth employment rates
c + e = relatively higher youth employment rates
Magnitudes
c + d + e = small positive (youth) and negative (teenagers) effects
c + f = predominantly negative minimum wage effects
g = consistently negative and statistically significant (especially for the least regulated labor markets)

It is pertinent to observe that Neumark and Wascher's study was most probably inspired by minimum wage studies that use cross-state comparisons. Some of the advantages of using cross-state comparisons are that it allows researchers to introduce state-level fixed effects[36] in the analysis; it permits the evaluation of minimum wage impacts on wages,

[36] This is because each state may have different unobservable policies that may affect the impacts of minimum wages on employment.

enrollment, employment, and on the interaction between enrollment and employment (Brown, 1999, p.2122). Neumark and Wascher applied similar strategies.

All the studies thus far analysed (including those to be discussed subsequently) are built on microeconomic foundations – except the study by Tulip (2004) which exploits the indirect relationship between the relative minimum wage and the Non-Accelerating Inflation Rate of Unemployment (NAIRU) to estimate minimum wage effects from a macroeconomic standpoint.

Tulip argues that conventional measures of minimum wage effects are limited as they focus only on "affected workers" (partial equilibrium) ignoring overall impacts (general equilibrium), the Phillips curve effects, and considerations of the more important policy issue of long-term changes in total unemployment. He estimates that on average, over the last two decades, a 10 percent increase in the minimum wage, relative to average wages, raises nominal wage growth and hence inflation, and this effect can cause an increase of 0.5 percent in the NAIRU (the unemployment rate at which inflation is stable37). This implies that an actual unemployment rate higher than 0.5 percent is required to stabilise inflation. Some of the channels through which the minimum wage might increase the NAIRU include: a) by reducing the demand for unskilled workers, thus putting little downward pressure on wages; b) through the "ripple" effects, which causes other nominal wages to gradually increase at a given rate of unemployment; c) when the minimum wage act as a safety net or "outside option" that affects workers' bargaining position, etc. These various factors, Tulip asserts, explain the increase, and subsequent decline, of the NAIRU in the U.S. during the 1960s and 1970s; the surprisingly favorable behavior of inflation and unemployment in the U.S. over the 1990s, which is attributed to the low level of the minimum wage; as well as why the NAIRU escalated in continental Europe relative to the U.S.[38]

Moving to specific estimates (based on four distinct data sets which include wage growth from 1948 to the 1970s; wage growth from the 1970s to 1998; the behavior of prices; and international comparisons of changes in the NAIRU), Tulip finds that a 10 percent increase in the relative minimum wage in mid-1998 would have raised aggregate nominal wages immediately by 0.3 percent and subsequently by 0.084 percent a quarter, which can be offset by an extra 0.59 percentage points (0.0084/0.0014 x 10%) of unemployment; a 30 percent reduction in the coverage-adjusted relative level of the minimum wage over the 1980s requires an offset in the NAIRU of about 2 percentage points of unemployment; the planned post-2000 increase in the U.S. federal minimum wage from \$5.15 to \$6.15 an hour would raise the NAIRU by one percentage point of unemployment.

To fix ideas, Tulip shows in his Table 2 (partially replicated below) the effect on the NAIRU (allowing for an offsetting effect from unemployment) of minimum wages in the

37 Tulip clarifies that, because accelerating prices cannot be sustained, the long-run level of unemployment arising from the minimum wage will approximately equal its effect on the Non-Accelerating Inflation Rate of Unemployment.

38 A plot (Chart 2) of changes in the NAIRU against changes in the 10/50 ratio between 1980 and 1995 shows that countries where wages at the bottom of the distribution have reduced relative to other wages – such as the USA and the UK – experience reductions in the NAIRU while countries in Europe with contrary experience show an increased level of the NAIRU.

periods before and after 1980. There is a direct relationship. A glance confirms the NAIRU's decline over the second sample period,

Effect of Minimum Wages Before and After 1980

Period	Contribution to NAIRU	
	Coefficient (in %age points)	Standard error
1948:3 to 1998:2	5.9	1.3
1948:3 to 1980:1	7.7	1.8
1980:2 to 1998:2	3.5	2.0

which coincides with that of the minimum wage over the same period (though it is the past relationships in the data, not this 'coincidence', that drives the results). There are low frequency variations in the data, supporting the emphasis on the long term.

The central message from Tulip's paper seems to be that, in the long run offsets to small supply shocks caused by minimum wage increases usually require, within the context of the Philips curve effects, large increases in unemployment. However, his model seems to struggle with problems connected with structural breaks (occurring in the early 1960s) and the related possibility of omitted variable bias on wages.

V. What if Phases I and II were Actually Two Sides of the Same Coin?

This is the intriguing question being posed (and tentatively answered) by Michl (2000) with his rescheduling hypothesis. According to Michl, both CK -who claim with irrefutable supporting facts that the New Jersey minimum wage had no negative impact on employment of low-wage workers- and Neumark and Wascher (who argue with convincing evidence to the contrary) are, whatever their disagreements, correct, to the extent that the New Jersey minimum wage probably increased the economic well-being of the targeted low-wage, fast-food-industry workers.

The intuition behind Michl's model, based on hours versus number of workers, is that the New Jersey experiment resulted in some rescheduling of workers, in terms of reduction in hours per worker (calculated to be around 5 per cent, or one hour and fifteen minutes per week), without reducing the overall number of workers on the payrolls of the fast-food restaurants. Indeed, based on his theoretical postulate (which follows all the assumptions of the standard competitive model) and a Cobb-Douglas scheduling isoquant, Michl's analysis compares the reduction in average hours (total number of hours declined but mostly through shorter hours) to the 17 percent increase in the legislated wage and concludes that the average worker will have experienced an increased level of welfare, or an increase in leisure time with no change in weekly income.

Not surprisingly, Michl concludes with two caveats related to: a) the need to be careful about the usage of the term "employment", whose two possible distinct definitions (hours or bodies of workers) can sensibly move in opposite directions and; b) the need to channel greater efforts into securing data on both measures in order to make reasonably precise statements about the effects of the minimum wage.

Obviously, Michl's analysis raises important issues. First, through the recognition that his findings still raise questions concerning distribution of the rescheduling among the existing workforce (since it is theoretically possible that some workers' hours may be so sharply reduced that they experience welfare loss), Michl unconsciously re-establishes the formal development of welfare economics as additional aid in the quantification of minimum wage effects, on the list of necessary topics for further research. Brown (1982, p. 496) discusses the same issue.

Second, and not less important, the study not only echoes Brown's (1999, p. 2117) observation that lack or imprecision of data, rather than preferences of minimum wage analysts, has dictated the practice (of the time series literature) to measure employment only by bodies to the neglect of variation in hours per worker, but it also, on the basis of a more formal theoretical reasoning, confirms the limited evidence suggesting a larger reduction in the length of the "scheduled" workweek relative to teenage bodies employed (Brown's imprecise estimate being 40% higher in "full-time equivalents"). But, as Brown (1999 p. 2156) puts it, "surprisingly, this line of attack – i.e bodies versus hours - has not been prominent in the recent research, on either side of the debate."

Perhaps, ultimately, Michl's reconciliation of Neumark and Washer versus Card and Krueger would improve our general knowledge of how to obtain more precise measurements of minimum wage effects, an exercise which, to borrow a word from Benjamin, Gunderson and Riddell (2002, p.217) has proved "distressing" due to the myriad of difficulties it usually presents. Indeed, it would appear that this distress is pervasive, to the extent that one can easily point to examples (within and outside of the U.S.) of divergent estimates that very closely resemble the Neumark-Washer-Card-Krueger debate on one hand, and those that counter it, on the other. I illustrate with three examples.

The first (by Zavodny, 2002) examines the impact of minimum wage on both teen employment and hours of work using both state- and individual-level panel data in the US. Data sources are from the NBER extracts of the Current Population Survey (CPS) outgoing rotation groups for the years 1979–1993. State annual averages are used to examine the effects of the minimum wage on aggregate teen employment and the average weekly hours of work. Panel data on individuals are used to examine whether employed teens aged 16-19 initially earning near the minimum wage incur employment or hours losses relative to higher-wage teens when the minimum wage rises.[39]

The results indicate that minimum wages do not significantly reduce teen hours (which contradicts Michl's rescheduling hypothesis), but may have a small effect on employment. In the state-level results, the minimum wage is not significantly negatively associated with teen average hours of work, either among employed teens or all teens; also higher minimum wages may result in lower teen employment rates, but the estimates are sensitive to the choice of the minimum wage variable. Individual-level data show that teens likely to be affected by minimum wage increases do not experience a significant decline in hours relative to unaffected teens, even when teens not remaining employed are included in the analysis. The results suggest that affected teens are about 2.2% less likely to remain employed a year later than unaffected teens; however, low-wage workers are equally less likely to remain employed than high-wage workers during periods when the minimum wage did not change. That is, they appear to experience similar relative changes in employment status and hours during periods when the nominal minimum wage is constant, suggesting that minimum wage increases do not adversely affect employment and hours among initially employed teens or among young adults aged 20–24 and less-educated adult women aged 25–64. Thus the effect of the minimum wage increases may be smaller than other more traditional or consensus estimates would suggest and may more closely resembles the results obtained by Card and Krueger (1995).

A second illustration will briefly examine the studies by Stewart (2002) and Machin et al. (2003) that were separately conducted on the 1999 minimum wage increase in the U.K.,

[39] According to Zavodny, both state- and individual-level data can be combined in a complementary fashion by using state-level data to capture aggregate or total effect of minimum wages on transitions from employment to nonemployment and from nonemployment to employment, while individual-level data on workers initially employed (in its role as a tool for estimating the effect of minimum wage increases on the probability of remaining employed) can be used to overcome the inability of state-level data to distinguish between the two transitions.

with different results. In the legislation adult rate was set at £3.60 per hour, with a subminimum youth rate of £3.00 per hour for those aged 18–21.

Stewart explores, over the 1998–2000 period, the considerable regional wage variation in the U.K and the impact that this geographic diversity has on both wage distribution and employment growth upon the introduction of the minimum wage in 1999. He initially constructs cross-sectional estimates of the impact of the legislation on employment rates of the "most-likely-affected" and then uses panel data to incorporate a comparison with the earlier period without a minimum. His results indicate that, although the minimum wage had differential wage-distribution effects across the 140 areas of the country, employment growth after its introduction was not significantly lower in areas of the country with a high proportion of low-wage workers, whose wages had to be raised to comply, from that in areas with a low proportion of such workers (the Annual Business Inquiry (ABI)- based estimates were slightly lower than the Labour Force Survey (LFS)-based rates).

Machin et al., on their part, examine the impact of the same 1999 minimum wage on the low-wage home care workers, relating changes in employment before and after the minimum wage introduction to the fraction of low paid workers in the pre-minimum wage period; they find a sizable positive wage impact and negative employment and hours effects which, depending on the specification, range from -0.35 to -0.55 (the estimates evolve from negative but not significantly different from zero in their basic model, to more negative in absolute terms and significantly different from zero in the models that allow for restriction and control variables such as the initial proportion of females, proportion with nursing qualification, proportion of care assistants and average age, occupancy rate, etc.).

The papers reach seemingly differential conclusions about disemployment impacts of the minimum wage (using the difference-in-difference approach), despite a consensus about its bite and effect on the wage distribution of the low-wage workers. A second observation concerns the way employment is measured; while Stewart uses 'bodies' employed, Machin et al. also considers hours in addition to bodies. Finally, there is the issue of the quality of data; the shortcomings of New Earnings Survey (e.g., undersampling of part-time workers) caused Machin et al. to construct, using the Yellow Pages Business Database, a database they considered more suited to the "before and after" methodology.

More recently in April 2000, the Irish government introduced a national minimum wage of £4.40 an hour, prompting Nolan, O'Neill and Williams (2002) to investigate the employment effects of this change, based on data from a specially designed panel survey of firms for the 1998-2000 period. Initially, they find no significant difference in employment growth between the affected low-wage firms and the unaffected high-wage firms. However, when Nolan, O'Neill and Williams control for the fact that some workers in classified under the low-wage firms would have experienced increased wages over time irrespective of the legislation, the estimate turns negative and statistically significant (though this final estimate, -0.02, is rather modest).

VI. Summary and Conclusions

A. Summary

The evidence provided by most of the studies in this survey is consistent with the conventional (supply-demand model) view of a negative effect of minimum wage upratings on employment. This category of studies is based on time series or panel data indicating negative effects on teenage and youth employments but little or no effect on employment levels of adults over 20 years; they either emerge prior to the 1980s or resurface in recent times (from the late 1990s to date). However, a few other studies based on the same methodologies unexpectedly yield contrary results of no disemployment effects (e.g. Grenier and Seguin's, 1987 second sample period, for Canada; Wellington, 1991 for the U.S.); these studies extend from the early 1980s to the mid-1990s.

A second category of considerably fewer studies than those realised with time-series or panel data methodologies, is based on "natural" or difference-in-difference techniques; these studies, which also extend from the early 1980s to the mid-1990s, are consistently unanimous in their findings of zero or negligible and sometimes even positive employment effects of increases in the minimum wage on teenage employment. If these challenging findings have gained widespread attention, from policy makers and from theoreticians alike, they also attracted a barrage of criticisms (related to methodology, timing, measurement errors, biases, unobserved demand shocks, etc), prominent among which is the quality of the data – since it lacks information on wage rate and hours worked- upon which the analyses and inferences are built.

Interestingly, very similar issues can be identified with the data quality of the generality of minimum wage studies realised in the U.S., since many of the studies use the Current Population Survey (CPS), which suffers from the same problems confronting all surveys (i.e. self-selection, validity, reliability, verifiability, etc).

The foregoing reflections can only qualify but not change the picture that easily emerges from the wide range of evidence contemplated in this survey (see table below) - that is, that the minimum wage is generally harmful to teenage, and to a large extent, youth employment. There is little or no negative employment effect for adults aged 25 and above. When compared to the conventional figures, the estimates of this survey appear to be just slightly wider in the range (-0.142 to -0.3735 %) but are consistent in the average (-0.258 %).

Authors (date)	Place and Time	Method	Impact	Qualifications
Swidinsky (1980)	Canada and Five Provinces (1956-1975)	Panel data (OLS)	15-19 yrs M Fm	Can : QC -01; -079 (Skeptical) -0.27; -0.93 (Skeptical)
BGK (1982)	U.S.	Time Series	16-19 yrs. -0.1 to -0.3	Exclusion of coverage amplifies disempyt. Effect; controlling for wewlfare benefit, and using teen pop. Share as a control var. both reduce impact unempyt; estimates vary slightly depending on assumptions (instantaneous or lag).
Schaafsma and Walsh (1983)	Canada 1975-1979	Time Series (OLS)	15-19 20-24	M: -0.17; -0.2 Fm: -0.28; -0.21
Mercier (1987)	Quebec and Canada (1966-1986)	Survey		-0.1
Wellington (1991)	U.S. (1954-1986)	Time Series	Teen Adults	(< -0.1); (no effect)
Grenier and Séguin (1991)	Five Canadian Regions	Time Series (OLS)	15-19	1956-1975 1976-1988 M: -0.13 insig Fm: -0.45 insig
Neumark and Wascher (1991)	U.S. (1973-1989)	Panel data	Teen/Young Adults	-0.15 (No submin) -0.045 (With Submin) Note: Submin dampens one-third of min. wage adverse effects
Card and Krueger (1995)	N. Jersey and Penn (1988)	Diif-in-diiff (panel) Cross-section		+0.185 (elasticity) +0.264 (FTEs) Tel. Survey method
Shannon and Beach (1995)	Ont.-Canada (1989)	Microdata Approach Retail sales & food services		-0.12 to -0.15 Most-affected: women, young and part-time workers
Burkhauser, Couch and Wittenburg (1996)	U.S. (1990-91)	Cross-section Panel Data	16-19 16-24	(-0.87)** (-0.36)**
Newmark and Wascher (1995, 1992, 1998, 2000)	N. Jersey and Penn (1988)	Diff-in-diff (replication) pooled Cross-section Time Series panel		-0.39 to -0.40 -0.48
Baker, Benjamin and Stanger (1999)	Nine Canadian Provinces 1975-1993	Time Series Cross-section		-0.25 data driven by low freq. high freq. models produce little or no disempyt effect
Goldberg and Green (1999)	B.C. Alberta, Ontario and		15-19 20-24	M: -0.14 F: -0.16 M: -0.096 F: 0.02

Authors (date)	Place and Time	Method	Impact	Qualifications
	Quebec (1968 to 1997)		25-54	M: -0.01 F: -0.09 Summary: 0 to 0.2 (long-term 0 (short term)
Quebec's Dept of Finance (2002)	Quebec (1981-2000)	OLS Time Series	15-19 20-24 25-54	M: -0.193 F: -0.275 M: -0.062 F: -0.05 M: Insig. F: Insig.
Zavodny, 2002	U.S. (1979-1993)	Panel Data	16-19	High impact group: 2.2% less likely to be emplyd; but no adverse impact on hours relative to low impact group Concl: no impact among teens, young adults aged 20-24 and less-educated adult women aged 25-64
Stewart (2002)	U.K. (1998-2000)	Panel data/Cross-sectional "difference-in-difference"	Low-wage workers	No impact/(insig.)
Nolan, O'Neill and Williams (2002)	Ireland (1998-2000)	Panel Survey		With "wage drift" -0.02 Without "wage drift": no impact
Bazen and Marimoutou (2002)	U.S.	Flex. Structural Time Series	Only teenagers (16-17)	-0.2 to -0.3 (long run); -0.1 (short turn)
Yuen (2003)	Canada (1988-1990)	Panel Data OLS with Fixed Effects	16-19 20-24	Heterogeneity drives the data teens: -0.83 less likely to be employd Youth overall: no impact
Neumark and Wascher (2003)	17 OECD (1975-2000)	Pooled C-section Time Series	15-19 20-24 25-54	Normal regr. -0.18; -0.13; nil Other mkt pol. -0.3; -0.39; nil
Machin, Manning and Rahman (2003)	U.K. (1998-1999)	Diff-in-diff method	Low paid workers	-0.35 to -0.55 Note: basic models are negative but insignificant; models with controls such as initial proportion of fremals, care assistants etc. are more negative and significantly diff. from zero.
Tulip (2004)	U.S.	Macro study	1948-1998 (-0.59) 1948-1980 (-0.77) 1980-1998 (-0.35)	NAIRU based; long term driven

AVERAGE OF THE ESTIMATES -0.258
RANGE OF THE ESTIMATES -0.142 to -0.3735

**Statistically significant at the 10 per cent level; sign and magnitude of the employment effect indicated where no point estimate is available.

Other notes: estimates from macro-based studies and other outliers are excluded in the calculation of the range and the average

Beyond indulging in the simple exercise of cataloguing the pains or gains of the minimum wage, however, there is an increasingly important controversy surrounding the effectiveness of minimum wages as a tool for eradicating poverty.

Several authors are of the view that the correlation between membership in poor, low-income families and low wages is relatively weak, since many of the poor are unemployed (so raising wages cannot possibly help them), and many low-wage workers live in high-income families. This weak correlation weakens the impact of the minimum wage on the distribution of household income, whatever its effects on the distribution of earnings (BGK, 1982). More recently, Shannon and Beach's (1995) research on Canada find that: a) about 20 percent of Ontario workers would have received a raise from an increase in the minimum wage of 35 percent -and this only corresponds to 11 percent of hours worked since the majority are part-timers; b) poor families received only 28 percent of the additional earnings, which indicates a significant "leakage" of the benefits of higher minimum wages towards individuals from rich families who received a higher share of 31 percent; the legislation would only reduce Ontario poverty rates from 16.9 to 16.6 percent and; other redistributive policies (e.g. job creation and income support programmes) could complement the minimum wage for effective targeting of the poor.

Since minimum wages are apparently here to stay, it may be useful to recast the policy recommendations of Quebec's Department of Finance (2002) regarding the modalities for the review[40] of the minimum wage in Quebec. The Committee anchored their decisions on the conditions prevailing in the economy generally, and on the level of the ratio of the minimum wage to the average hourly wage, in particular.[41] When this ratio is less than or equal to 0.47 and the proposed increase in the minimum wage would have the effect of increasing the ratio more than 0.47, they recommended that a standing interdepartmental Committee (S.I.C.) should be consulted beforehand; whereas a ratio less than or equal to 0.47 would require no consultation. They also suggested that when the ratio exceeds 0.47, a consultation should be made with the S.I.C., which might suggest the most appropriate method to reduce the ratio to 0.47 or less. Goldberg and Green (1999) suggest a ratio of 0.5, but are wary that pegging the minimum wage to the average hourly wage may exacerbate inequality trends when hourly-wage workers fall behind salaried workers; this makes them suggest the alternative of pegging the minimum wage to the Statistics Canada Low Income Cut Off (LICO).

40 Four common methods of review are identified. These include automatic indexation, as with France, Belgium and The Netherlands; decree, as with several Canadian provinces and the U.K; senate voting, as with the U.S and; by independent body as with Australia and Mexico. European countries with no policy (eg. Germany and Sweden) rely on collective bargaining between employers and employees.

41 In comparing this ratio in Quebec, Ontario and the U.S. the study shows that in Quebec, the ratio has varied between a low of 37.4 % in 1986 to around a high of 46 % in 1995, while this variation was, for Ontario, between 35.6 % in 1983 and 45 % in 1995 and for the U.S., from 35% in 1989 to 42% in 1983. In 2001, this ratio was highest in Quebec (45 %,) relative to Ontario (around 40 %) and the U.S. (36 %). Also, this disparity between Quebec and others has widened since 1994 (relative to Ontario, -3.89 % in 1994 to +5.53 % in 2001; relative to the U.S. it grew from +2.25 % in 1994 to +9.37 % in 2001). Although the level of nominal minimum wage in Quebec has since 1983 been comparable to the rest, the fact that it has a relatively lower average hourly wage makes for a higher minimum wage /average wage ratio than those of Ontario and the U.S.

B. Implications For Further Research

This survey has examined works that, put together, cover a substantial part of primary areas in the minimum wage debate. One of such areas is the unanimous recognition of the need for analyses that embrace a long-term horizon for labour force adjustment in response to a change in the minimum wage, and with a view to incorporating vital evolutionary changes and dynamics in the labour market. Minimum wage analysts might justifiably invest their time in the search for a suitable methodology, part of which might involve addressing questions such as: a) what drives the minimum wage data (low- or high-frequency variation?) ; b) what is the more appropriate level of differencing (short or long?) in making elasticity inferences; c) to what extent are lags important (in reflecting expectations, adjustment costs, etc.) and are they feasible? ; d) what extra information can be gained from more flexible structural time series models, given the high level of sophistication they require? ; e) what role does a Phillips curve effect or for that matter macro analysis play?; what impact and how widespread is the use of offsets (which are factors that mitigate the harsh effects of the minimum wage) in the adjustment process?; what is the appropriate threshold of risk concerning the ratio of the minimum wage to average wage levels?.

Another priority area is the heterogeneous nature of low-wage workers. A widespread assumption has been that the estimated disemployment effects of the minimum wage may be driven by the difference in employment stability between low wage and high-wage workers; but surprisingly little research work has contemplated the implications of the existence of that stability difference within the low-wage bracket itself.

A third recurrent theme is the correlation of unobserved variables with employment and earnings, leading to bias in OLS results. Efforts in confronting this issue (e.g., examining how different labour market policies and institutions could either exacerbate or mitigate the effects of minimum wages on employment (Neumark and Wascher, 2003)) should yield a lot of dividends.

Finally, it is worth observing that if Michl's rescheduling hypothesis (which hints at the possibility that the seemingly divergent views on minimum wage are one and the same thing) appears to be novel, the implications associated with such a possibility (preferences for hours over bodies in employment measurement, greater attention to welfare economics as an additional aid in the quantification of minimum wage effects, amongst others) are not necessarily novel, as past works have already identified them as gaps in the literature. However, Michl's work undeniably lends a more urgent note to their analysis.

Appendix 1

The Fixed Effects Methodology

1. Introduction

Analyses based on cross-country (or, in Canada, cross-region) regressions have been criticized on two grounds. One of such criticisms concerns data comparability across countries. For a few countries where multiple measures of the variable of interest are available, the different measures can give different, sometimes contradictory, patterns even for the same time periods. Since the data that cross-country regressions have to rely on come from potentially different methodologies, they can produce misleading results when pooled together. One solution that has been generally applied is the use of dummy variables adjustment for data differences. But the inadequacy of dummy variables approach in cross-country analysis has also been highlighted (Atkinson and Brandolini, specific criticism is on the poor quality of inference associated with pooling data across OECD countries).

The second difficulty with cross-country studies has to do with the fact that differences in cultures, legal systems, or other country-specific institutions other than the independent variable of interest (in this case, the minimum wage) may also be relevant for the outcome variable under study (e.g., employment). These factors are usually unobserved and difficult to quantify, and therefore to be controlled for, in cross-country regressions. But the inclusion of fixed effects in panel regressions helps.

Within a given country and over a relatively short time period, culture, legal system or other institutions can, with the aid of fixed effects, more plausibly be held constant, so as to enhance the researchers' ability to isolate the effect of, say, minimum wage on employment.

2. The Model

a) Assume a regression model of the form:

$$y_{it} = x'_{it}\beta + z'_i \alpha + \varepsilon_{it}$$

with k regressors in x'it (which includes no constant term). The heterogeneity, or individual effects parametre, is z'iα where z'i contains a constant term and a set of individual or group specific variables which may be observed (e.g. race, sex, location) or unobserved (e.g. family specific characteristics, individual heterogeneity in skill or preferences, etc), all of which are taken to be constant over time t. Note that if:

1) zi is observed for all individuals, then the entire model can easily be treated as an ordinary linear model and fit by least squares, with the aid of pooled regression in the case of a panel data;

2) zi is unobserved, but correlated with the included variables --xit (random effects would be applicable where the unobserved variable is uncorrelated with xit) then the OLS estimator, β will be biased and inconsistent as a consequence of an omitted variable. The way around this is to apply the fixed effects, in which case the regression is redefined as:

$$y_{it} = x'_{it}\beta \quad + \quad \alpha_i \quad + \quad \varepsilon_{it}$$

where $\alpha i = z'i\ \alpha$ embodies all the observable effects and specifies an estimable conditional mean. This fixed effects methodology takes αi. to be a group specific constant term in the regression model, and assumes that differences across units can be captured in differences in the constant term, or as parametric shifts of the regression function.

b) Put differently, the fixed effects estimation: is a method of estimating parameters from a panel data set. The fixed effects estimator is obtained by OLS on the deviations from the means of each unit or time period. This approach is relevant when one expects that the averages of the dependent variable will be different for each cross-section unit, or each time period, but the variance of the errors will not. In such a case random effects estimation would give inconsistent estimates of β in the model:

$$yit = x'it\beta + \varepsilon t$$

The fixed effects estimator is: (X'QX)-1X'Qy, where Q is the matrix that "partials out" (or "nets out") the averages from the groups that have different variances.

Example: Define L as IN x 1T, where x is the Kronecker cross product operator, T is the number of time periods, and N is the number of cross-section units (individuals, say). Now individual effects (or heterogeneity) can be screened out by premultiplying the model's equation by Q and running OLS, or equivalently using the estimator equation above.

Shortcoming

The main shortcoming of the fixed-effects approach is that it considers the errors of measurement estimated for the subjects as the only source of variation when estimating the population mean. That is to say, only within-subject variation is accounted for. No consideration of between-subject variation is considered; therefore it is valid only for the subjects chosen in this experiment (no sampling variation).

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