

The Measurement of Mythical Models

Questions, Theory and Data

in the Cases of

The Adverse Employment Effects of the Minimum Wage

and

The Wage Curve

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0. Introduction

If a Martian labour economist would land on earth to study terrestrial labour economics and, in the unlikely event that he would ask me which recent textbooks on contemporary labour economics he should study in order to acquaint himself quickly with the field of his interest, I would probably suggest him as a first introduction “L’économie du travail” by Cahuc and Zylberberg [1996]. I assume that for a Martian the French language sounds pretty much like an English dialect. On a more advanced level, I would suggest him “Labor Supply” by Killingsworth [1983] and “Labor Demand” by Hamermesh [1996].

In order to give our Martian friend some feeling of the controversy that exists in labour economics I would suggest him two books: “The Wage Curve” [Blanchflower and Oswald, 1996] and “Myth and Measurement” [Card and Krueger, 1995]. The two books have stirred a lot of attention, as can be seen from the sheer volume of articles that appeared in the literature during the last decade or so.

Although I would have given these suggestions with the best of intentions, I might have left our extraterrestrial friend and colleague with a wrong impression. The two books, and the articles that they enticed, could be interpreted as a serious criticism on the neo-classical approach to labour economics. But this is not correct in my view. The reason why these two books are so controversial is that their subjects are well studied in neo-classical economics. And neo-classical theory makes very clear predictions about these subjects. The evidence presented in both *The Wage Curve* and *Myth and Measurement*, however, do not support these predictions. This is a very serious blow to the neo-classical approach in labour economics. But, at the same time, we should remember that labour economics has a far richer tradition.

From the end of the 1930’s way up into the 1960’s labour economics was mainly descriptive. It gave rise to what is labelled *labour market studies*. John Dunlop,

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Clark Kerr, Richard Lester, Lloyd Reynolds and contemporaries had a wide and practical knowledge of the labour market, obtained through work done for the war-effort. [Freeman, 1989, p.318] They knew the institutions that prevailed and influenced the market, and they surveyed the labour market by sending questionnaires. Thus, they were informed in depth on how the market worked, which factors influenced the market. They did not focus solely on labour demand or labour supply. Rather, they investigated the interactions between demand and supply.² Only in the 1970's and the 1980's neo-classical analysis climbed to its apex, introducing mathematical rigour in labour economics. At the same time it narrowed the attention to easily quantifiable problems. This is evidenced by the studies on labour supply and labour demand. Data on labour supply was more readily available. Therefore, labour demand was relatively neglected for many years.

As already explained, both *Myth and Measurement* and *The Wage Curve* were perceived as clear attacks at the core of neo-classical labour economics. When this core was attacked, some labour economists defended it vehemently. They launched counter-attacks on every possible front.

As a stepping stone to a useful description of both books and the literature surrounding them, I will use the following considerations. According to Leamer [1998, p. 175-190] sound economics has three ingredients: questions, theory and data. When the questions are missing, but there is theory and data, then one reverts to *pointless hypothesis testing*. Questions and data, but no theory, is nothing more than *econometric journalism*. If both questions and data are missing, such that economics is boiled down to mathematical manipulation of theory, then this also will no longer constitute economics, but rather some sort of, easy, applied *mathematics*. Questions and theory without data is, still according to Leamer, manipulation. And manipulation, so it seems, is 99.99% of what economists do. Well, Leamer's benevolent character shows up and admits that it might be only 99%. Whatever the percentage, his point makes sense. Even after some clever manipulation of economics, the final test is how well it describes reality and this can only be checked by confronting the clever manipulations with carefully selected data and evaluating its results. In the following section I will try to use this triad of questions, theory and data. This may seem an easy task, but it is not. Solow [1986] urged economists to get the questions right when talking about unemployment. After all, badly posed questions make it difficult to imagine a plausible theoretical framework in which the question makes sense, or in which any answer can sensibly and unambiguously be interpreted [Solow, 1986, S24]. Although it makes sense to distinguish these three elements, very often in the literature it turns out to be very difficult to separate them. Very general questions are reformulated using some kind of theoretical framework or based on some earlier obtained empirical result. Or theory is re-examined starting from some questions and empirical facts. Questions, theory and data are the fabric of good scientific work. Separating them will leave us with a few crumpled rags that used to be closely interwoven.

In this paper I will try to present a clear view on the attacks at the core of neo-classical labour economics and the counter-attacks. To do this I will make distinction between questions, theory and data explicitly. Section 1 will do this for

² Thus, they recognised Marshall's scissors.

Myth and Measurement, putting it at the same time in a chronological framework. Accordingly, section 2 will deal with *The Wage Curve*.

1. Myth and Measurement, the new economics of the minimum wage

This book is, according to the authors (Card and Krueger, 95, from now on in this text indicated by CK), the culmination of five years of research. In this research their primary aim was to apply new techniques to the field of labour economics. More specifically, they applied it to the question of minimum wages. At that time the minimum wage effects on employment and unemployment were already extensively researched and were reviewed in Brown, e.a. [1982]. The earliest reference to a study on minimum wages that I found was published in 1915 by Obenauer and Nienburg of the Bureau of Labor Statistics. The conclusion of these early studies pointed out that minimum wages had no appreciable effect on employment. [Kennan, 1995, p.1954] In 1946 Stigler [1946] tackled the implications of the minimum wage legislation. He raised two questions: “Does [minimum wage legislation] diminish poverty?” And: “Are there efficient alternatives?”³ Attention shifted also from employment effects to unemployment effects of minimum wages. [Mincer, 1976, S87]

Because in November 1989 the Fair Labor Standards Act (FLSA) was amended, this seemed to be a good opportunity to re-examine minimum wage effects. Minimum wages had not been changed for over a decade, which meant that the real value was seriously eroded by inflation. These amendments arranged for an increase in the federal minimum wage, but also for a provision for a subminimum wage. This subminimum wage allowed employers to pay teenagers 85% of the minimum wage during the first six months of their employment. This subminimum wage amendment had a built-in “sunset-clause”. It would end on March 31, 1993, unless legislation would be passed to extend it. The amendments also required the Secretary of Labor to report to Congress on the extent to which employers have made use of the subminimum wage, the impact of the subminimum wage on employment opportunities for both experienced and inexperienced workers, and its impact on the nature and duration of training offered to workers. These factors spurred research on the effects of (sub)minimum-wage legislation. [Ehrenberg, 1992, p.3]

The questions stated in the amendment were explicitly reformulated by Katz and Krueger [1992] as the following five questions: What is the utilisation rate of the teenage subminimum wage? What determines whether a restaurant will utilise a subminimum wage? How has the minimum wage affected wage dispersion? What effect has the increase in the minimum wage had on the level of employment at firms affected by the increase? How has the minimum wage affected the price of fast-food items? [Katz, Krueger, 1992, p.6]

Spurred by this new legislation a conference was held on November 15, 1991 at the ILR-Cornell Institute for Labor Market Policies/Princeton University Industrial Relations Section Conference. [Ehrenberg, 1992, p.3] Several papers presented there, were later grouped in a symposium in *The Industrial and Labor Relations*

³ According to Stigler “minimum wage is an inept device for combating poverty, even for those who succeed in retaining employment. One principle is fundamental in the amelioration of poverty: those who are equally in need should be helped equally. There must be an objective criterion of need; equality can never be achieved when many cases are judged (by many people) “on their merits”. [Therefore] there is great attractiveness in the proposal that we extend the personal income tax to the lowest income brackets with negative rates in these brackets.” [Stigler, 1946, p.365]

Review. Of these contributions only Smith and Vavrichek [1992, p.82] raised two new questions: Do many minimum wage workers remain at that low rate for long periods of time? What characteristics differentiate minimum wage workers who remain at that rate from those who rise above it?

In 1995 *The Industrial and Labor Relations Review* organised a new symposium - this time sparked off by the publication of *Myth and Measurement*. Freeman [1995] recognised that the results of the book lead to a reorientation of policy: away from efficiency considerations, towards distributional issues, as was suggested much earlier by Stigler [1946]. Therefore, he suggested to consider whether the minimum wage distributes income to low-wage workers? Is the work force divided by the minimum wage into insiders, employed permanently at the minimum, and outsiders who suffer long-term joblessness because of the minimum? Are low-wage workers low-income workers? But the minimum wage should be seen with other economic policies, so the question that arises is: How does it fit in? (His guess is that the minimum wage looks better in the second-best world in which we live than it does in most textbooks.) And, at what level should the minimum wage be fixed if income is to be redistributed without risking sizeable job loss? [Freeman, 1995, p.833-834] But, besides the objective of the abolition of poverty, there is also the concern for reducing the employer control over wages (cfr. Osterman [1995]). But this would be better achieved by improving labour mobility. [Stigler, 1946, p.363] Osterman [1995] raised one remark and one question in that same issue of *The Industrial and Labor Relations Review*. “The minimum wage was intended to outlaw certain types of employment, typically called *sweatshops*. It was intended to force firms from one cluster of human resource practices to another.” [Osterman, 1995, p.840-841] Stigler’s conclusion, based on a simple theoretical analysis, was that “[minimum wage legislation] will be an inept device for combating poverty even for those who succeed in retaining employment.” “The deep question posed by [Myth and Measurement] is why so little progress has been made [since the research of the institutionalists of the 1940s and 1950s].”

The reason why minimum wage effects on employment are so interesting to examine is simply that neo-classical theory leads to clear, unambiguous predictions about their impact on the labour market: *it will destroy low-wage jobs*. This can be shown on the following graph [CK, 1995, p.5]:

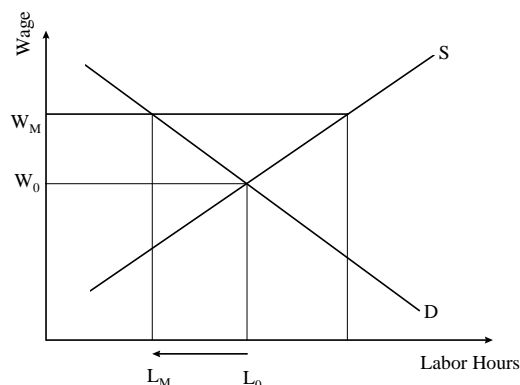


Figure 1.1 (Card & Krueger, 1995, p.5)

The increase in the minimum wage above the market clearing level will make it for the employers no longer interesting to employ those people who have a lower marginal productivity. Hence, the reduction in low-wage employment. Furthermore, it will raise the average wage for the low-skilled jobs. But even the most ardent neo-classical economist will admit that this model neglects some of the important features of the labour market. It can be argued that an increase in minimum wages will lead to a reduction in training, fringe benefits, ... There is also a clear prediction on what effect it should have on the product market: the price of the product should increase by the fraction that low-wage workers' pay is affected by the increased minimum wage. These general results were more or less corroborated by results obtained from time-series analysis on teenage employment data (teenage workers - being workers who are very often working in minimum wage, low-skill jobs - seem to be a group of people for whom the effects of an increase in minimum wages will be strongest). There is even some agreement on the magnitude of this effect. It is reported [Is There Consensus among American Labor Economists? Survey Results on Forty Propositions, Robert Whaples, 1996, p.730] that 'only 21 percent disagreed with the statement "a minimum wage increases unemployment among young and unskilled workers"(while 57 percent generally agreed).' 'The survey shows that few labor economists have accepted the new arguments [as they can be found in Myth and Measurement]. An immense majority (87 percent) accept the older conclusion that "a minimum wage increases unemployment among young and unskilled workers."' And more specifically, they believe that: *an increase of 10% in minimum wages, leads to a 2% decrease in teenage employment.*

Which were the techniques CK wanted to introduce? The first technique was borrowed from other sciences: the idea of experiments. This is not to be confused with experiments as we know them in physics. In physics one performs experiments in a laboratory where - if the experiment is well designed - it is possible to measure the effect of one factor on the subject under study. The effectiveness of a treatment can also be measured, using experiments and then comparing the treatment group with a control group. Ideally, chance assigns individuals to one of the groups. This assures that no unintended correlations will be measured. In biology, for instance, it is clear that evolutionary principles like 'survival of the fittest' cannot easily be investigated in laboratories or via some experiment with treatments. However, biologists and palaeontologists very often rely on so-called *natural* experiments. In natural experiments it is assumed that *nature herself* provides us with experiments. The only real difficulties, left for the scientist, are recognising and isolating the experiments and identifying both the treatment group and the control group. To assess the validity of the natural experiment one should bear in mind the following considerations [CK, 1995, p.23-24]:

- Are the pre-interventionist characteristics of the treatment and control groups reasonably similar?
- Have the two groups tended to move together in the past?
- Was the intervention more or less "exogenous"?
- Can the control group be compared against other plausible control groups?

How should one measure the treatment effect of a natural experiment? Make the difference between the state *after* the experiment has taken place and the state *before* the experiment happened. Do this for both the treatment group and the control group.

This assures that the specific characteristics for each group are removed. This is necessary as we cannot rely on chance to provide us with two groups that only differ in their treatment. By making the difference of these two differences the treatment effect is obtained, as the treatment should not have had any impact on the control group. If the resulting difference is significant it means that the treatment has a significant impact. The use of this, so-called, 'difference-in-difference' method can then be interpreted as a reduced-form estimate, so there is no need to start from a particular theoretical framework [CK, 1995, p.25].

However, the use of experiments in social sciences, and economics in particular, is not new. CK, themselves, indicate that this technique has been successfully used in education, immigration and unemployment. Nor was the question of the effects of a minimum wage on the labour market new. The study of these effects has generated a huge literature over the past 50 years or so. New in CKs work is that they used this 'new' technology to this 'old' question. [CK, 1995, p.ix]

I will now explain what the natural experiment is that CK use. In the USA there is a federal minimum wage, but each state has the right to institute a state-wide minimum wage, that has to be higher than the federal minimum wage. This leads potentially to two kinds of natural experiments. The first kind is when a state increases its minimum wage. The second kind of natural experiment arises when the federal minimum wage is increased. This will force some states to increase their state-wide minimum wage, while other states do not have to increase their minimum wage.

So, for instance, in 1992 New Jersey increased its minimum wage, whereas the neighbouring state Pennsylvania did not. This constitutes a natural experiment. If one wants to determine the low-wage labour market effect of an increase in the minimum wage then one compares the change in labour market outcome of the state with the minimum wage increase with the change in labour market outcome of a state without this increase. Hereby one makes the assumption that both states are otherwise similar. Although I think it is more correct to say that the states should change in similar ways, while the initial or institutional conditions may be very different. If this 'difference-in-difference' method indicates a significant effect then this effect can be attributed to the treatment, i.e. the increase in the minimum wage, while the shocks in the different states are differenced away.

The authors have applied this technique to different situations. First, as we mentioned above, CK used it on the New Jersey minimum wage hike. CK compared the labour market effects for the fast food sector. This sector is characterised by low-wage workers (very often teenagers) and they were heavily affected by the minimum wage hike. A similar exercise was done for the fast food sector in Texas around April 1991. This survey lacked a control group (which, by the way, was the reason why they repeated the survey in New Jersey and included Pennsylvania as a control group). The results for both studies were, nevertheless, very similar: relative to high-wage restaurants, employment increased at restaurants affected by the minimum wage. But also the price effect (on the product market) turned out to be anomalous: the prices increased more in high-wage restaurants.

The authors indicate that they received some criticism in the past on some aspects of the research: the surveys were possibly too close to the date of the minimum wage hike (although the authors consider this a minor point), the minimum wage hike might choke off new investment (which is very difficult to test directly, but can be done indirectly via the analysis of teenage employment), and, finally, there were criticisms on the methodology itself.

They also investigated the 1988 California minimum wage increase (a 27% increase) which seemed to have had a slightly positive effect on teenage employment, a significant impact on wages, and no large or systematic effect on employment of low-wage workers relative to a control group consisting of workers from an amalgamate of states.

The second kind of natural experiment is provided by federal minimum wage hikes. The different states have different state-wide minimum wages. An increase in the federal minimum wage will affect the different states in a different way, such that the treatment effect depends on the fraction of workers initially earning less than the new minimum wage (the increases in the federal minimum wage ranged from less than 20% to more than 60% in some states). This constitutes a simultaneous analysis of some 50 natural experiments. The average teenage wages raised more in states with higher fractions of affected workers, but the employment of teenagers did not fall, and the same holds for even a broader set of workers. “Indeed, our estimates for the restaurant industry suggest that employment actually increased *more rapidly* (italics in original) in states in which the federal minimum hike generated the *largest pay increases*.” (italics added) [CK, 1995, p.149] Teenage employment trends across different states are essentially unrelated to the wage changes induced by the federal minimum-wage hike [CK, 1995, p.115]. And, in high-impact states teenage employment increased relative to both low- and medium-impact states [CK, 1995, p.124], contradicting the choking off of investments.

Probably to make up for the promise they made in the introduction of their book CK add a chapter on how the increase in minimum wages affects the employers and the shareholders, using the technique of event studies. They admit that the results are tentative and that the evidence is mixed about the downward adjustment of investors’ valuation of firms after the news of a minimum wage hike has spread [CK, 1995, p.347-348].

After this exposition on the techniques CK introduced, I will now discuss the theoretical framework in their work.

It would be equally wrong to think that previous research relied solely on a neo-classical approach. Stigler [1946] made a clear distinction between industries where employers have control over the wage rates they pay and industries with competitive wage determination. Ehrenberg [1995, p.827] wrote in his introduction to *The Industrial and Labor Relations Review* that “[s]imple competitive demand and supply models do not provide an adequate description of low-wage markets, the very markets in which one might expect these models to ‘work the best’.” In their book CK set out to investigate an empirical regularity in the data. The theoretical models they provided were a selection of some models that could explain their observations.

Brown [1995] formulated some serious criticisms on the monopsony model, although this model is not tightly embraced by CK. Hamermesh [1995] investigated whether their natural experiments were exactly that, natural and experiments. His conclusion is negative. He makes the argument that labour is a dynamic p-complement⁴ for capital. And, as capital adjusts rather slowly, the same may be expected from employment. So, most of his criticism boils down to the time span used by CK. Starting from his assumptions their data do not allow a natural experiment analysis. Whitman [1995] claims that nonwage elements should be considered in the compensation bundle. But this is covered by CK. He also refers to Yoram Barzel's *Economic Analysis of Property Rights*. "Barzel's approach [business owners exploit all margins for adjustment available to them (not merely price and quantity) in order to reclaim losses due to regulations and other constraints] suggests that margins for adjustment cannot always be specified in advance by an economist." [Whitman, 1995, p.615] Although this argument might bear some truth in it, economists very often use the *ceteris-paribus* clause just to circumvent this problem. If not we would be left with nothing but trivial remarks about economics. Osterman [1995] topped it off with the remark that the theory is nothing more than mere speculation. But the point CK want to make is that they found an empirical relationship that does not seem to accord with a simple demand and supply scheme. So they formulate some alternative models without being able to make a case for one alternative against the others.

Burkhauser, a.o. [1996] focuses on the distributional aspects of the minimum wage. They make the point that this incorrectly treats the question of poverty. Only a small part of the poor actually work. But this is not fair to CK. In their book they refer explicitly to the *working poor*. A more sensible remark is that in order to make these analyses meaningful one should employ the concept *income-to-needs ratio of families*. [Burkhauser, ea., 1996, p.548] A point that was already made by Stigler [1946].

CK use some kind of *meta-analysis* that points in the direction of *publication bias*, *specification searching* or *structural changes*, that might have had an important effect on the results.

A final point that I would like to stress is a contribution by Kennan [1995] which explains very clearly how CK examine their claim of publication bias⁵, something that is neglected by CK. The reasoning is very simple. The t-statistic for a variable in a regression with n observations and k explanatory variables always satisfies the following relationship: $t^2 \cdot (n - k) = \frac{r^2}{1 - r^2}$ where t is the t-statistic, n the number of observations, k the number of explanatory variables, and r the partial correlation

⁴ A complete treatment of p-substitutes and p-complements can be found in : Hamermesh, Daniel, Labor Demand, 1996. When an input is a p-complement for a second input this means that an - exogenous - increase in the price of the second input will lead to a reduction in the quantity of the first input.

⁵ Publication bias in this case means that a paper in which positive employment effects of a minimum wage increase are found, will not be accepted by any editor for publication. Leaving other researchers with a false impression of the results found by their colleagues. Or, a researcher on finding 'perverse' results, will not even try to publish.

coefficient. As the number of observations, n , increases, the partial correlation coefficient, r , tends to the population value - assuming stationarity of partial correlations. So, when more observations become available and the same regression analysis is performed on the data, the t-statistic should grow at the same rate as $\sqrt{n-k}$. If this is not so, one might expect that there is publication bias, in the sense that only the favourable results have been published. A simple plot of the absolute t-values for the (un)employment impact of the minimum wage and the root of $n-k$ will indicate whether there is publication bias or not. As CK find that the t-statistics are not on the 45°-line, but that there is a negative relationship, they conclude that there is a strong (although not conclusive) indication of publication bias.

CK also dedicate a chapter on *additional employment outcomes*. After a minimum wage hike they find a spike at the minimum wage in the wage distribution. This is not easy to explain in a neo-classical model. After all, this model propagates the law of one price [CK, 1995, p.153]. This means that after an increase in the minimum wage one would expect the distribution to be truncated at the minimum-wage level, whereas everyone with a higher productivity still receives the same pay. Even firms that are exempt from the minimum-wage law seem very often not to use sub-minimum-wages, which again leads to a spike in the wage distribution at the minimum-wage level. So we see a bizarre combination of facts: There is wage variability for identical workers in different firms. But some workers with different skills are paid the same wage (the spike in the wage distribution). Furthermore, we see that some workers whose wage was already above the new minimum-wage, enjoy a wage increase (the so-called *ripple-effect*) [CK, 1995, p.160]. And there is proof that low-wage employers discriminate among equally productive employees on the basis of personal characteristics, not related to productivity. This seems to point out that there might be unobservable characteristics that cloud this sort of analysis. Or that employers adjust non-wage benefits, training, ... in response to a higher minimum wage. Or that employers use the new minimum-wage as a focal point (e.g. a 'fair' wage). Or, simply, that low-wage labour markets are not that competitive as is suggested in the textbooks and that employers do have some power to determine the wage [CK, 1995, p153-160] and, in some cases (41% surveyed), even prefer to maintain the wage structure instead of simply increasing only the wages for those affected by the new minimum-wage [CK, 1995, p.161].

So, instead of finding similar (or more precise) results as in previous research, CK hit upon a "puzzle" [CK, 1995, p. ix]. They found, in general, that the predicted effects on employment were not observed. On the contrary, very often they found no effect on employment levels and in some cases they even uncovered a significantly positive effect. This puts the neo-classical model somewhat under strain. Therefore, CK decided to have a closer look at previous, widely reported, results. In a very meticulous way they reinvestigate the existing time-series results and find that the results obtained with the same data sets - updated as far as possible and using the same specifications - no longer hold up, in the sense that the effect is no longer significantly negative and in some cases that the sign has changed. Not to mention that most of these studies have focused their attention on employment, rather than on unemployment, because with the two-sector models (covered vs uncovered) that were used, unambiguous predictions about the effect of the minimum wage on unemployment could not be made [CK, 1995, p.179]. The authors also remind us

that, in the last two decades, time-series analysis has lost a lot of its appeal in many applications of labour economics, except with respect to the minimum wage effects.

Besides time-series evidence, they also concentrate on the evidence from cross-sections and panel-data. The advantages of micro-data over time series is that the unit of observation corresponds to the decision maker in economic theory. Unlike in time-series multiple factors are not changing. And, the use of a suitable control group is possible. [CK, 1995, p.209] CK find that “the bulk of the empirical evidence on the employment effects of the minimum wage is shown to be consistent with our findings in chapters 2-4 [...]”[CK, 1995, p.236] That is to say, not in accordance with the neo-classical model.

To make sure that they are not uncovering some peculiar characteristic of the American labour markets CK cast their eye on the international evidence. They look closely at the evidence for Puerto Rico, Canada, and the United Kingdom. In several countries the minimum wage is not the same for everybody, but it varies across industries or regions. There is also a difference in the administrative level on which the minimum wage is agreed or negotiated. In Puerto Rico the minimum wage is imposed by the mainland US government, thus excluding the question of endogeneity of the minimum wage. In Canada it is imposed on the provincial level, whereas in the United Kingdom the Wage Councils have the authority to increase the minimum wage. “[F]or these three economies [the evidence on the employment effects of the minimum wage] does not provide unambiguous support for the textbook model of the minimum wage.” [CK, 1995, p.271]

CK try to determine how the minimum wage affects the distribution of wages, the distribution of family earnings, and poverty. This is, according to the authors, the Achilles heel of the political discussion on minimum wages. And they find that increases in minimum wages have a narrowing effect on the distribution of the wages and family earnings and that it may have led to a modest reduction in the rate of poverty *among workers*.

In the final chapter of their book the authors look for an explanation for their findings and offer some alternative models. CK do not rule out that the standard model can be used for some labour markets or that at high enough increases employment will drop. [CK, 1995, p.355] Although they make a reference to Paul A. Samuelson [CK, 1995, p.8], who says that even the most basic and elementary parts of the [wage determination and labour economics] are uncertain, and a reference to the social economics revisionists, who claim that the minimum wage could increase employment in some instances and reduce it in others, their claim is that “the standard model is incomplete” and that “many alternative models depart only slightly from the standard model, and yet yield different predictions about the effect of the minimum wage.” [CK, 1995, p.355-356] The rest of the final chapter presents a (messy) overview of some alternatives.

With the techniques of natural experiments and event studies CK also introduced new data. These data were collected through extensive surveys and consist of data on firm-level or data on individuals (both are called microdata).

But some serious reservations were revealed with respect to the use of time-series. Both Kennan [1995] and Williams and Mills [1998] devote some space to explain why time-series are not the best data series to examine the impact of the minimum wage on (un)employment. “Even the seasonally adjusted employment rate for teenagers displays large cyclical swings and a high degree of serial correlation. The median increase was 12%, which would produce at most a three percent reduction in the teenage employment rate. ... We are looking for employment rate changes of about one percentage point, and such changes happen all the time, even from one month to the next. In short, we are looking for a needle in a haystack. ... But this kind of [time-series] estimate is surely not reliable - it rests on heroic aggregation assumptions, some potentially important explanatory variables are left out, and the serial correlation in the employment series is a mystery.” [Kennan, 1995, p.1955-6] Besides the difficulty of choosing the control variables and the problem of how to measure the minimum wage, there is also the problem that, because of the aggregation, all the demand elasticities have to be the same, and there is the problem of potential endogeneity of the minimum wage and some of the controls. [Williams, Mills, 1998, p.397] Basically, the discussions on the enrolment of teenagers is about the endogeneity of this control variable. It can be argued that for teenagers choosing for education is an alternative to working that becomes more expensive when minimum wages are increased. But there is a dispute on whether the variable provided in the data base used is appropriate to capture this. Even more so, because most students seem to have a low-wage job, which contradicts the alternative-use-of-time argument. [Neumark, Wascher, 1992, 1994, 1996] [Card, Katz, Krueger, 1994] [CK, 1998]

Welch [1995, p.844] had serious reservations on how the survey was done and pointed out some serious mistakes in the original data. This raised the question whether research should be done on primary or secondary data. Secondary data has the advantage that it has been extensively screened and described in the literature and, as such, it is well known to the researchers. The main disadvantage is that it mostly consists of publicly available, macroeconomic, time-series data. On the other hand Kennan [1995, p.1964] argues that in labour economics there is no need for more sophisticated inference methods, but for more sophisticated data. This could be understood as primary data to be screened by the profession and made available to other researchers so they can replicate studies. This screening and replication turn out to be very important, e.g. the results of a study performed by Castillo-Freeman and Freeman, 1992, were very sensitive to the specifications that were used. [Whitman, 1996, p.617] even suggest that *anecdotal studies* - in the sense of ‘thick descriptions’ - would be an acceptable substitute for statistical precision. (This seems to smack of the approach by the generation of John Dunlop, Clark Kerr, Richard Lester, Lloyd Reynolds.)

I would like to finish this part on *Myth and Measurement* with an important result that is worth mentioning - and which is almost never referred to in the subsequent publications - that is wage mobility. The typical low-wage worker was part-time (<35 hours/week) employed in the retail trade. After one year of employment the typical worker had a wage-increase of 30%, while 81% remained in the labour market, and 73% were employed. But after two years, still some 25% of the low-wage workers remained at the minimum wage.

I will now discuss *The Wage Curve*.

2. The Wage Curve

Apparently, an article by Bils [1985] kindled the interest of Blanchflower and Oswald - from now on referred to as BO - in this subject. Bils found that an increase in the unemployment rate of one percent point was associated with a decrease in real wages of between 1.5 and 2 percent [1985, p.668].

One well-known fact is that sometimes enormous differences in unemployment rates between regions within national economies exist, suggesting that aggregate unemployment within a country cannot be well understood without a realistic analysis of the reasons why unemployed people do not move to areas with better employment prospects.

Bils also spent some time to the limitations of the use of aggregate data. The use of aggregate data assumes that the composition of the work force remains the same. But, this is obviously not the case. As the employment of the less educated and less experienced is more variable, in times of high employment real wages are averaged over a larger group of people, but this group has lower earning potential. The reverse holds in times of low employment. This introduces a countercyclical bias between real wages and unemployment. There is also scope for procyclical bias, as employment in manufacturing (esp. durables) is more variable, but pays higher wages than other sectors. Furthermore, aggregation assumes that the relationship between real wages and the probability of becoming unemployed is the same for everyone. If this is not true it introduces a specification bias in the estimations. Finally, aggregation involves a loss of information and, therefore, of estimating efficiency. [Bils, 1985, p667] We could also mention that some doubt was shed on the reasonableness of the use of real wages versus nominal wages. [Solow, 1985, S24-S30]

BO use enormous databases consisting of individual data to uncover aspects of economic behaviour that lie hidden in the aggregated time series. Analyses based on individual data exhibit more degrees of freedom than time-series analyses. [BO, 1996, p.3]

After starting a single paper on the role of local unemployment in British wage determination, BO believed that there was “a more general finding to be uncovered”. [BO, 1996, p.ix] In the following years they assembled data sets of, in total, 3.5 million people (random samples) in 12 different countries.

The two questions they raised were formulated in Blanchflower, Oswald and Garrett [1990]. First: Are wage rates set as if by a neo-classical competitive market [p.143 and p.144]? And, secondly: Does unemployment depress pay? These questions are implicit in all the articles that will follow, except for [Hoddinott, 1996] who, explicitly, asks whether an empirical rule found in western economies will hold in the context of developing countries (Côte d’Ivoire).

Using comparable data sets from different countries allows BO to use the concept of *natural experiments*. [BO, 1996, p.3] Reality is full of natural experiments [BO, 1996, p.99] - such that one can discern “deep explanations of economic and social

behaviour [...] that are common to peoples with different histories and institutions” [BO, 1996, p.4]. The attempt is made “to establish the existence of a relationship between pay and joblessness in a manner more compelling than can be achieved - no matter how carefully executed - by time-series methods.” [BO, 1996, p.366]

The second chapter of *The Wage Curve* records its historical background. Here, the first generation of literature on the relationship between wages and unemployment across space start with the Harris-Todaro model. This model states - based on a host of assumptions to be found in BO [1996, p.16] - that regions with high unemployment should also be regions with high wages. The rationale behind this is the argument of compensating differentials: in order to have an equilibrium (that is, no migration) the working people in a region with less attractive features, should be compensated for this by receiving higher wages. Thus, leading to a positive relationship between regional unemployment and wages. The next generation of research was based on micro-econometric data. Statistically significant negative relationships between local unemployment and wages were found while the other explanatory variables maintained the correct signs and remained significant. [Blackaby & Manning, 1987] So, this second generation research uncovered a breach of neo-classical theory.

It is against this background that BO started their research. The name, the wage curve, for the inverse relationship between wage and local unemployment⁶ was first coined in a discussion paper [DP 344, Centre for Labour Economics, LSE, 1989]. In [BO, 1990] BO use a bargaining process, and refer to detailed suggestions by David Soskice and Meghnad Desai, which they promised to use in their next book. A rent-sharing model is still the theoretical starting-point in [Blanchflower, Oswald, Garrett, 1990].

Pissarides & McMaster [1990, p.823] consider the finding of a negative relationship between local unemployment and wages ‘perverse’. They interpreted it as a form of a Phillips curve effect, where a rise in relative unemployment induces in the short-run a fall in relative pay. Also Card (1990) found a statistically significant negative effect, which also holds true for the reservation wage [Jones, 1989].

[Blanchflower, Oswald, Garrett, 1990] is also the place where, for the first time, some kind of reference is made to the Phillips curve and to the possibility to measure this Phillips curve with micro-data. This is readily recognised by Paldam [1990], who is a self-proclaimed proponent of the Phillips curve: “The curve found is surely the good old Phillips curve”. He is particularly rejoiced about the fact that, finally, the Phillips curve has found a solid micro-basis. Paldam had probably no idea how right he was, when he said that this was the “top of a major iceberg, from which several hundred more pages will later emerge” [Paldam, 1990, p.238]. Rødseth [1990] identifies a theoretical problem in that unemployment has to vary due to some outside factor, and that it does not depend on the wage. In Scandinavian countries, Rødseth thinks, a theoretical argument can be made for the negative relationship caused by national agreements on minimum wages.

Graafland [1992] explains the difference between a wage curve and the Phillips curve, but then uses time-series data (on annual basis and without regional data) to make

⁶ Local means on a smaller level of aggregation than for an entire country.

some estimates. Looking at the definition of the wage curve this seems to be very awkward. But, with respect to policy, Graafland makes it very clear that, since the wage curve is not the Phillips curve, one can no longer rely on the disequilibrium adjustment process behind the Phillips curve. This means that there is no reason to assume that unemployment will automatically return to frictional unemployment.

In the September 1994 issue of *The Economic Journal* [BO, 1994], a formal definition of the wage curve was given for the first time. The wage curve is ‘actually’ a *spatial* relationship (which links it automatically with some kind of micro-data), which should not be confused with the well-known Phillips curve and which runs against the normal Harris-Todaro argument (as a reminder, this model assumes a positive relationship between local unemployment and wages using the compensating differential argument).

BO find that the wage curve can be represented as: $\ln w = -0.1 \ln U + \text{other terms}$. The *other terms* in this expression refer to control variables for further characteristics of the worker and his/her sector. Their findings can be represented as the wage curve in the following graph:

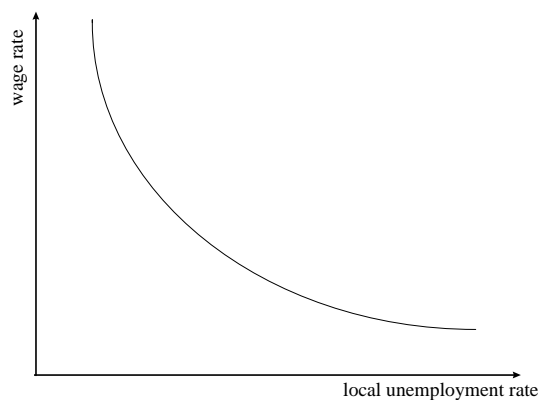


Figure 1.1 The Wage Curve

The idea of a negative relationship between local unemployment and wages should not be interpreted as saying “that high-wage countries have low unemployment levels, or that secularly growing wages will be associated with slowing reducing levels of unemployment. The efficiency wage model, for example, makes it clear that richer countries have (vertically) higher non-shirking conditions because they will have higher benefit levels for the unemployed to live on.” [BO, 1996, p.92]

One chapter in *The Wage Curve* deals with theoretical issues. It gives four different reasons why high unemployment could be associated with low pay: contract models, efficiency wage models, bargaining theory (which was the starting point in their first articles) and persistent disequilibrium. We will briefly present the four different models.

In the labour contract model workers are paid a competitively determined level of utility, very often assumed to be equal to the wage, describing a neo-classical theory of the wage curve that does not rely on the existence of labour-market rents [BO, 1996, p.63]. Regions can be different in 4 economic ways: nonpecuniary features, the

technologies, the probabilities of demand shocks, and the unemployment benefit levels. BO derive a few propositions saying that high-nonpecuniary regions pay low wages, that there is a negatively sloped function (the *wage curve*) linking wages and unemployment across regions and that, if one holds the nonpecuniary features constant, there is a positively sloped function linking wages and expected unemployment. [BO, 1996, p.38-47] The point here is that unemployment does not 'cause' wages, nor wages the level of unemployment. Rather, pay and joblessness are determined in a simultaneous system, driven by demand shocks and the characteristics of the region [BO, 1996, p. 48]. The following propositions state that there is a negatively sloped curve linking wages and unemployment across regions - regions with more favourable distributions of demand shocks pay lower wages, employment is higher, other things constant, in regions with greater productivity, such that highly productive areas have low wages, and holding constant the level of regional productivity, there is a negatively sloped curve linking wages and unemployment. When the unemployment benefit varies by geographical area, it is not possible to prove that there must always be a reduced-form wage curve. If the regional unemployment benefit is held constant, however, there is a negatively sloped wage curve linking wages and unemployment. So, in order to measure this negative relationship empirically, one needs to control for regional benefit levels. [BO, 1996, p.51-57]

Using a no-shirking condition, the efficiency framework leads to firms which find that, in recessions, it is feasible to pay their employees less well, because unemployment frightens the workers. Again, some seven propositions can be formulated, leading to the investigation of the different effects of differences in regions attributed to nonpecuniary utility levels, unemployment benefit levels, required levels of effort at work, shirking detection rates, and production functions. [BO, 1996, p.64-82]

In the bargaining model the key assumption is that 'outside' unemployment weakens the bargaining power of the workers and, thus, reduces the share of profits that the workers can appropriate. [BO, 1996, p.83] The proposition, which is derived, says that, under union bargaining, the equilibrium wage is a decreasing function of unemployment in the outside labour market, and an increasing function of profit-per-employee. But these are long-run correlations. That is: they exist in steady-state equilibrium. [BO, 1996, p.84] So, in order to investigate the empirical role of local unemployment in wage determination, one might have to include a profitability variable as an extra regressor. [BO, 1996, p.88-90]

And, finally, the disequilibrium approach is based on the conventional competitive theory, but modified to allow for sluggish adjustment of wage rates. But this view mixes up the idea of low wages with that of falling wages. [BO, 1996, p.92]

So, the role of demand shocks is very different in the models presented. The wage curve in the contract model is not a consequence of the demand shocks, but the wage curve does not conflict with the demand shocks. Whereas, in the efficiency model, the demand shocks are vital, as they trace out the no-shirking condition. In the bargaining model they complement, because profits should be an extra independent variable. [BO, 1996, p.92]

The authors also dedicate some time to some of the problems of each model and the empirically distinctive predictions. The multi-region contract curve rests on the assumption that there is no real labour demand. Although, there is little empirical evidence for this [Hamermesh, 1996] and collective bargaining is normally over pay and not over employment. [BO, 1996, p.94] The wage rigidity arises only under very restrictive assumptions. When no inefficiencies are assumed the model collapses to a competitive framework. Finally, the theory allows the possibility of overemployment, which make the model unpalatable for most economists. [BO, 1996, p.95]

Four empirical implications follow from the contract models:

“1) There is a positive correlation between employment and the wage. Wage regressions using unemployment as an independent variable might be expected to perform more poorly than those that rely on employment per se.

2) In the principal model where employers are risk-neutral, the wages are independent of demand shocks and differences in regional wage levels are derived from unchanging characteristics of the region (e.g. climate). This implies that regressions that control for regional effects should show little or no sign of a negative unemployment effect on pay.

3) A negative association between pay and unemployment is not causal but rather reflects a partial correlation. This might be expected to show up as a lack of robustness in estimated coefficients and high sensitivity to different lists of included variables.

4) Contract theory presumes that employer and employees perceive themselves to be in a lasting relationship where there is little likelihood of turnover. A traditional labour demand framework, by contrast, might be considered more plausible for low-skilled with highly transitory jobs. This suggests that a contract model would predict a negative wage-unemployment correlation more strongly and more naturally in the well-paid primary sector than in poorly-paid secondary activities.” [BO, 1996, p.95]

The testable implications for the efficiency wage approach are:

“1) In sectors where the monitoring and supervision of effort levels is especially easy, the negative effect of unemployment upon pay should be small or even negligible.

2) In jobs where workers earn large rents the unemployment elasticity of pay might be expected to be small. The reason is that such jobs are inherently attractive to workers, so that variation in the outside unemployment rate ought to make little difference to their willingness to put in effort at work.

3) Because it is unemployment per se that creates the incentives, estimated wage equations should be better with unemployment, rather than employment, as a regressor.” [BO, 1996, p.96]

In the bargaining approach the relative power of workers declines as local joblessness grows. This leads to the following empirical predictions:

“1) Where workers’ power is negligible, firms should be pure wage-takers and the unemployment rate should then have no influence.

2) Wages will be higher where profitability-per-employee, *ceteris paribus*, is greater.

3) The unemployment elasticity of wages is likely to be lower the higher is profitability.

4) The level of unemployment should be a more effective regressor than the level of employment.” [BO, 1996, p.96]

The negative relationship between local unemployment and wages does not conflict with the Harris-Todaro model. The latter states that ‘permanent’ values of pay and joblessness will be positively related across regions in a long-run equilibrium. “[P]ermanent values can be positively related while movements around the mean are negatively related.” [BO, 1996, p.93]

BO claim that “[t]his study is principally an examination of the role that local unemployment plays in pay determination - where causality is to be thought of as running from the amount of joblessness to the level of wages” [BO, 1996, p.3]. This means that they adhere little credence to the efficient-contract model. In a review of the book Card gives a hard judgement of the book. At the end he even wonders why the authors are so enthusiast about the efficient contract model [Card, 1995, p.796]. To be true, BO are not that enthusiast, they rather provide this particular model because it exists and it might explain the empirical results they find. (In the conclusions of his review Card becomes more mild.)

BO also look for the correct interpretation of the statistical pattern found in the data and perform some other checks trying to make some clear judgements on some of the empirical predictions of the various models. Is the wage curve a statistical chimera or is it a mismeasured labour supply curve? They check this by including the labour-force-participation rate and the employment-to-population rate in the wage equations, by replacing the year dummies with national unemployment rates and the aggregate consumption price levels. [BO, 1996, p.210] If the neo-classical supply interpretation would be correct, one would expect that young people, being the most mobile, exhibit a flat wage curve. But, contrary to what this theory predicts, BO find that the young turn out to have a very high unemployment elasticity of wage (-0.2). [BO, 1996, p.214] In [BO, 1995] the authors suggest that the wage curve might replace the conventional labour supply function in macro-economics with a wage-fixing function, that allows for involuntary unemployment, hardly fluctuating real wages and a long-run supply of workers that is vertical. They find that it is not a normal supply-and-demand curve that is repeated in disequilibrium.

The empirical part of their research uses a wide collection of data sets on which the same statistical techniques are used. The argument here is that if you find similar results over a wide set of random data this will strengthen your conclusions.

BO give two possible criticisms of their work. First, it would be better to use real wage-per-hour. They correct for this, but it does not seem to matter whether annual earnings or an hourly wage is used. And second, it would be better to use regional price deflators. They return to this point later in their book and find that studies where it was possible to use regional price deflators yielded the same results. [BO, 1996, p.179-180]

In their first article Blanchflower, Oswald and Garrett [1990] used British micro-economic data (the Workplace Industrial Relations Survey of 1984 = WIRS2). But no reference was made about policy implications. In [BO, 1990] the data sets are

expanded in the sense that new data sets are used. As there is also a reference to the Phillips curve, policy implications can no longer be avoided. The wage curve is flattened out at the bottom, because leisure will always have some positive value [BO, 1990, p.223]. This means that (exogenous) shocks to the economy have no impact on wages, but can lead to a substantial increase in unemployment. This is more or less accepted by Paldam [1990, p.240-241] who makes the following qualification: with high unemployment active labour market policy will have effect. But Paldam also remarks that micro-data might be a problem because they are not publicly available.

Groot and Mekkelholt [1992] explain that the shape of the wage curve cannot be derived from the bargaining model on which it rests. The exact shape is rather an empirical question. In 1993 Sessions [1993] supplements the neo-classical theory with behavioural assumptions based on the concept of *status* in *minority-majority groups*, borrowed from the social-psychology literature (this resembles the approach of Akerlof). This takes into account the stigma of being unemployed. And it explains some of BO's results that could be interpreted as an upward sloping wage curve for a high enough rate of unemployment. BO's reply [BO, 1993] - using a larger data set - boils down to the statement that the minimum of the wage curve is situated at $\pm 20\%$ unemployment, which seems to be out of the range of reasonable values, such that there is no longer any evidence of a U-shape.

For the US BO find that regional and industry wage curves are orthogonal to one another, meaning that including one variable in the wage equation does not affect the estimated coefficient for the other variable.

Micro-data allow disaggregated unemployment elasticities to be calculated taking differences in characteristics into account. Katz & Krueger [1991] found that the starting wage for the least-skilled (usually young) workers is significantly lower the higher the city unemployment rate. Workers with the least education, the young, those working in non-union workplaces, and in sectors that are highly cyclical in nature, have weak or no bargaining power. So, we expect the effects of unemployment upon pay to be greater for them. [BO, 1996, p.149] BO find that the unemployment elasticity of pay is greater for the less educated, for the young, and for those employed in the service sector. There are no differences in the disaggregated unemployment elasticities based on race, gender or union-status [BO, 1996, p.151] - although the results are sensitive to the sample period - e.g. the pay of those in US trade unions is half as flexible to unemployment as the pay of non-union members [BO, 1996, p.159]. Private sector pay is more responsive (between -0.10 and -0.14) than the public sector (between -0.02 and -0.05), which makes sense as the public sector is more centralised in the way she decides pay. [BO, 1996, p.159] The unemployment elasticities of pay seem to change over time, in the sense that they are generally larger (in absolute terms) at the end of the sample period than at the beginning. [BO, 1996, p.161]

In [BO, 1994, p.1028-1029] the authors correct for the fact that they use data of different aggregation levels. After all, they use data on individuals with unemployment data for regions. For individuals living in the same region this will lead to the same value for this variable. The standard errors of the estimated parameters will be biased downward, introducing an artificially high t-value for these

parameters, as is documented by Moulton [1986 and 1990]. To correct for this error, which is now known as *the common group error*, one has to ensure that the data used belong to the same aggregation level. Therefore, BO calculate averages and re-estimate their models obtaining very similar results, but this time with the correct t-values.

The general conclusion for the US data is that the unemployment elasticity of pay is -0.10, such that “a hypothetical doubling of unemployment would be associated with a fall in workers’ remuneration of approximately 10% (...). This is to be understood as a *ceteris paribus* change, holding other things, including macro variables, constant.” [BO, 1996, p.178]

BO also find that wages are positively correlated with profitability per employee [BO, 1996, p.221], which refers to the bargaining model.

As the local unemployment rate is not really exogenous, but a function of pay, the estimates are prone to simultaneous estimation bias. In order to correct for this BO have instrumented the unemployment rate in their estimates in various ways and found that this does not influence their results. [BO, 1996, p.224]

Finally, the authors demonstrate that the wage curve in the US is not an illusion, a trick of the eye caused by some omitted variables. Nor is it a consequence of some *composition effect*. “It is not the case that people’s incomes drop when they lose their jobs, nor because the economy is in a recession, nor because low-paid workers are forced to stay in depressed areas because mobility costs are prohibitive. The wage curve fitting is robust to the removal from the Current Population Survey (CPS) sample of all people with an unemployment spell in the period (results not reported).” [BO, 1996, p.235]

BO repeat the exercise for British data, noticing that all the UK regions closely track the path of the national unemployment series and that regional cycles seem less important than in the US. [BO, 1996, p.245-247] Five different data sets are used, but the main conclusion is that wages are low, other things equal, in areas where unemployment is high. [BO, 1996, p.289]

One of the findings is that the Phillips curve might be an illusion, due to the use of aggregated data - this might be labelled *the aggregation effect* to contrast it with *the composition effect*. Just as in the case of the US there seems to be no autoregression in the wage equations. BO conclude on this subject: “The apparent autoregression in macro pay levels may be the result of aggregation error or measurement error or specification error or all three.” [BO, 1996, p.284] They try to be more explicit on what the Phillips curve might be: “A well known problem with time-series modelling is that aggregate variables routinely look close to random walks. Macroeconomic wage equations could be spuriously generating a lagged dependent variable with a coefficient close to unity, and thereby producing a correlation that would have the appearance of a wage change specification. Inflation would then intrude by mistake into an analysis of the determinants of the level of pay. Perhaps this is what lies at the heart of Phillips (1958).” [BO, 1994, p.1035] This clearly indicates that BO changed their minds about the empirical regularity they were studying. Although Graafland

[1992] misuses the idea of the wage curve on Dutch data (because he uses macrodata), his presentation of the difference between the wage curve and the Phillips curve is very good. It is rather remarkable that BO never refer to this article in any of their subsequent writings.

BO also look at the International Social Survey Program (ISSP) that collect comparable data for 21 countries. The countries BO study are the former West-Germany, Austria, Italy, The Netherlands, Ireland, Switzerland and Norway. The data for all these countries exhibit wage curves, and the elasticity is in each case reasonably close to -0.1. [BO, 1996, p.336] Although not all t-statistics are well defined.

BO search their data for the wage curve in countries as diverse as Canada, South-Korea, Australia, the Côte d'Ivoire, urban India, Japan and Sweden. Although, these countries differ in many aspects, e.g. the institutional settings in which wage agreements are settled and also the culture, in general, we can conclude that the results obtained are very similar for these countries [BO, 1996, p.356]. The elasticity seems to lie in a band of -0.05 to -0.20. A remarkable result, which is strikingly different from the results obtained for all the other countries in this book, is the case of Australia: "[T]he unemployment elasticity of pay is higher in absolute terms for managers and professionals, and those individuals with the most education, whether education is measured by years of schooling or by highest qualification. The elasticity is relatively low for the young, part-time workers and foreign-born individuals." [BO, 1996, p.345] This can be contrasted with the case of South-Korea. There the unemployment elasticity of pay of those with primary school education is -0.14 compared with a mere -0.01 for those with college education. [BO, 1996, p.348]

Other authors jumped the bandwagon and estimated the wage curve for other countries. Johansen [1995] uses aggregated data upon which he applies error-correction models and steady-state analysis. Besides finding evidence for Norwegian wage curves, he also points out that for the Norwegian data the composition effect plays an important role. Because he uses other kinds of data for the concept of the wage curve, which inevitably leads to a low number of degrees of freedom, it is difficult to appreciate the true value of this remark.

Winter-Ebmer [1996] finds evidence for wage curves in Austrian data. The unemployment elasticity of the wage varies a lot, depending on whether regional unemployment data is used (-0.02 to -0.07) or occupational unemployment data (-0.01 to -0.02). These contrast starkly with the estimates obtained by BO [1996] (-0.12 to -0.16). This is probably due to differences in the data sets used. Winter-Ebmer finds, however, that the duration composition of unemployment is important and enters the wage equations as a stable coefficient. This has implications for policy, in the sense that "[i]n the pure wage-curve story, at high unemployment rates the equilibrating forces of the labour market simply break down, whereas in the other case direct interventions in favour of the long-term unemployed are called for" [Winter-Ebmer, 1996, p.433]. As can be seen this author has left the purely empirical interpretation of the wage curve behind, and assumes there is some kind of causation mechanism.

The question whether this empirical relationship holds outside western economies is a very interesting one and it might provide the wage curve with a broader basis. Hoddinott [1996] finds that the unemployment elasticity of wage is equal to -0.12, remarkably similar to the estimates of BO. There is an additional policy question here: What is the effect of stabilisation and structural adjustment programmes? As interest rates are raised and government spending is reduced, aggregate demand will drop. If wages are downward sticky, these reductions in aggregate demand will lead to higher unemployment. If, on the other hand, these markets are characterised by the wage curve, potential increases in unemployment may be mitigated by falling wages. [Hoddinott, 1996, p.1610] (It is not very clear to me whether some sort of causal relationship is assumed here, or that it is assumed that simply both effects will happen at the same time, in which case the negative relationship should be interpreted as a correlation.)

The results for Ireland, however, are left out because Ireland seems to be an outlier. “The regularity in the international data seems too uniform to be plausibly interpreted as a chance correlation, some statistical serendipity, caused by an identification problem.” “Bargaining and efficiency wage models are consistent with the observed patterns. Before getting swept away by this fact, however, one should bear in mind that the test is not a sharp one. These models predict a downward sloping locus in wage-unemployment, and there appears to be one. There may, however, be other as yet undiscovered explanations for the pattern. Little has been said about the non-competitive restrictions and monitoring assumptions underlying these models.” [BO, 1996, p.365] “The statistical significance of profit-per-employee alongside unemployment in a wage equation is in the spirit of bargaining theory. Moreover, ‘weak’ bargaining groups - the young and relatively unskilled and nonunion - might be predicted by this model to have the greatest responsiveness to unemployment. As the book’s disaggregated unemployment elasticities of pay confirm, that is an almost characteristic of the data. For all nations, except Australia, the unskilled, the non-union, and the young seem to have relatively high unemployment elasticities of pay.” [BO, 1996, p.366]

BO also find a place for themselves in “a small tide of new macroeconomic papers [that] has swelled to become a movement to be taken seriously.” Reference is made to “a new paradigm in the economics of booms and slumps [in which] the hallmark of this theoretical approach is a labour market that exhibits involuntary unemployment” BO [1996, p.366], thus, supply somekind of *quasi-labour supply curve* or *equilibrium wage locus*. As their ambition was to demonstrate the existence of the relationship, not to explain it, the message BO have for policy is rather dim. First of all, involuntary unemployment is existing in equilibrium. And second, government should consider whether it is possible to alter beneficially the slope of the wage curve. But, again, the policy issue is not really the subject of the book.

3. General Conclusions

What could we conclude from books that treat such different subjects as the labour market effects of minimum wage hikes and the spatial, negative relationship between unemployment rates and wage levels?

After reading the two books, one might be left with the impression that there is no future for neo-classical models in labour markets, that they are completely inappropriate to study labour markets. But this is not entirely fair. A far better conclusion is to say that, if one is to study labour markets, the core neo-classical model - a short-sighted application of the supply-and-demand model - is incomplete. Solow suggests to use an equilibrium concept where involuntary unemployment is possible. A careful reading of the two books leaves one with a whole range of alternative models. Models that are neo-classical in approach but, that take into account some peculiar or specific aspect of the labour market. This abundance of alternative models is perceived by Freeman [1995, p.831] as the “richness” of neo-classical models.

The limited progress in understanding labour markets has led some people to herald and to encourage the arrival of, both, new techniques and new data. Although this position is not held by everyone in the field.

There is also a change in the approach of economics. Instead of taking a deductive stance, the authors of both books look upon the questions from an empirical point of view. This automatically leads to a somewhat weaker theoretical frame. The authors do not neglect the existing literature in their field. On the contrary, they spend a lot of time, space and energy on very sophisticated analyses of both their new data and the results that were obtained in previous papers. Thanks to the profound knowledge of the authors with econometrics, both these books can serve very well as an how-to-do-empirical-labour-economics textbook and explain clearly some of the caveats of some techniques. Furthermore, the authors of both books saw it fit to use natural experiments for their investigations.

What about the wider relevance of these books? Should everybody know about these results, or is this the exclusive playing ground of (labour) economists?

With respect to the minimum wages the relevance of this kind of work can hardly be overstated. A cursory glance at the home pages of the Joint Economic Committee dedicated to this discussion gives an impression on how sensitive this issue is in the public debate.⁷ In this debate, however, it is not always very clear what the relevant questions are or whether some arguments are not inspired by some political body of thought⁸. The key result of *Myth and Measurement*, that there is no or almost no employment effect of minimum wage hikes, should put the discussion back in its

⁷ One of the authors of *Myth and Measurement* was chief economist in the Clinton Labor Department.

⁸ There is some indication that one of the most serious critiques on *Myth and Measurement* was formulated with data provided by *The Economic Policy Institute*, a liberal-oriented Washington think-tank that seems to have provided crummy data, thus providing counteraction to the conclusions of CK. After some, public indignation about not releasing the data, the data was made available to CK and they identified the problems of this data set. [Card, Krueger, 1998]

proper dimension and certainly remove some of the platitudes used in these discussions.

The Wage Curve urges us to look at some issues with different eyes. It might not be appropriate to look at unemployment on a very aggregate scale. The policy implications are even more serious. Although the correct causational link has not yet been uncovered, we should be very prudent in using some of the conclusions held in the heyday of the Phillips curve.

Lucas once said that the economy is a miserable experimental design. Therefore, we should not be surprised that these new techniques are criticised. After all, even physicists do not agree whether cold fusion in a jam jar is possible or not.

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