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### **ABSTRACT**

I argue for increased reliance on non-U.S. data and policy evaluations to understand basic labor-market parameters and to predict the effects of changes in U.S. labor-market policies. Foreign experiences generate exogenous shocks to labor costs that create unusual opportunities to measure impacts on labor demand. Foreign policies often provide more variation in the underlying parameters in systems that are often structured like their American counterparts. Foreign data sets are often larger and better suited to inferring behavior. An examination of empirical studies in labor economics shows the effect of the location of the author, data set and journal on the subsequent impact of the research on other scholars.

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## **I. The Scope of International Labor Economics**

The modern theory of labor economics—the essential insights of Gary Becker, Jacob Mincer, Sherwin Rosen and others—is surely international and applicable to the behavior of firms and workers in all countries. I worry, because we Americans form such a huge fraction of practicing labor economists and generate so much research output, that we may have developed a jingoistic reticence to using research studies based on other countries' experiences and data. My purpose here is to consider the extent to which expanding our horizons to other countries might increase our understanding of basic issues in labor economics and of our own policies.

The first question is whether international evidence can help us to obtain tighter estimates of some of the basic parameters describing labor markets. For this purpose in Section II I summarize recent quasi-experimental international evidence on various aspects of labor demand. Many people believe that American labor-market policies are unique, and no doubt to some extent they are; but other countries have policies that are not completely dissimilar from ours. If the similarities are great, it may be the case that we can learn at least as much about our own policies by examining others', an argument that I make with examples in Section III and that I follow with a discussion of data sets in Section IV. One might ask, given this discussion, why not simply fill American journals with studies of foreign countries' labor markets and policies that use foreign data sets? This question leads non-North American labor economists to complain that leading American economics journals discriminate against their work, a belief that I examine in Section V.

## **II. New International Evidence on Labor Demand**

There are two central issues in the study of labor demand: 1) What are the relationships between labor costs and the components of labor demand—employment, total hours, overtime hours, etc.? 2) What do we know about the nature of these relationships, in particular, the sizes of the impacts of changes in labor costs on those components? In addition to summarizing a

number of recent international studies that try to infer the impacts of shocks to labor costs, I also indicate some potentially fruitful areas for further research.

Hamermesh (1993) included a summary of an immense literature on labor demand, both the theory and evidence on all its aspects, particularly the impact of labor costs on total employment, hours and on the employment of different types of workers. Most of the empirical studies summarized there have been criticized on the grounds that they fail to examine the impact of exogenous shocks to labor demand, relying instead on statistical methods to justify exogeneity. Here I examine a number of recent studies in which there is very little doubt that the shocks to labor costs that employers face are beyond their control. While not “natural experiments,” employers’ responses to those shocks are probably as close as we are likely to get to observing responses to exogenous events in any nontrivial case. This novel approach requires looking at specific changes in individual countries and trying to understand their structures and impacts. When constructed carefully, these studies may be an advance on the earlier literature on labor demand and will offer a better way of studying the impact of labor costs than broad-brush comparisons of the impact of labor costs across countries (such as those in OECD, 1999, Chapter 2). That they depend on unusual events that generate the exogenous shocks means that we need to be eclectic in finding examples.

#### *A. Evidence on the Overall Demand for Labor*

Angrist (1996) examined the “experiment” generated by the Palestinian *intifada* when the State of Israel occasionally closed off the West Bank and prevented Palestinian laborers who had worked in Israel from commuting for the day to fill jobs there. This policy generated large fluctuations in the supply of unskilled labor, which was greatly reduced whenever the curfew was tightened and which flowed more freely when it was relaxed. He compared the average number of days of curfew per month during the *intifada* to the daily wage rate paid to low-skilled workers

in Israel. The virtue of this study is not only the exogeneity of the change, but also that its size relative to the labor market affected was much larger than we typically see in the U.S.

The study shows very clearly that, as Israel allowed in more Palestinian workers from the West Bank—as the curfew become less stringent, the wage rate of low-skilled workers in Israel declined. Variations in the harshness of the curfew caused the market for low-skilled workers in Israel to trace out a downward-sloping demand curve for such labor. Taking his numbers, one finds that Angrist's results imply a demand elasticity of  $-3$  for these low-skilled workers. This is a much bigger elasticity than I would have thought likely for all workers. It is not totally surprising, however, when we remember that these workers have only five or six years of education and that demand elasticities generally rise as the skill level decreases.

Kugler (1999) examined evidence on the impact of a change in job-security legislation that substantially reduced severance pay and thus labor costs in Colombia. Using household data she showed that, while the reduction increased flows of workers out of employment, it increased the outflow from the pool of unemployed even more. Her research suggests that the reduction in labor costs increased employment, clearly in the short run and perhaps even in the long run. Saavedra and Torero (2000) dealt with labor-market regulation in Peru, namely with the generosity of severance pay based on years of prior service on the job. Each of four policy regimes suddenly and sharply varied the generosity of severance pay and, even more interesting, varied generosity differently at different years of prior work experience. Using establishment data and holding the usual “everything else” constant, they found that changes in severance pay generated an elasticity of employment of about  $-0.25$ .

One other thing worth looking at in both the Colombian and Peruvian studies is the relative sizes of the formal and informal sectors, which relate to the scale effects generated by changes in labor costs. In both countries the cuts in severance pay appear to have resulted in sharp increases in the size of the formal sector and reductions in the size of the informal sector. This is

just what one would expect if employers who had sought to avoid the costs of higher severance pay by going informal move back into the formal sector, and if new employers are more likely at the margin to choose to operate formally. In addition to this compositional effect, the cut in labor costs also made formal-sector firms more competitive, and these scale effects enabled them to expand employment and output (which also rose at this time).

No single empirical study of an economic phenomenon is ever highly convincing, since one can worry about the representativeness of the example and particular problems with the research design. Here we have three examples where economies were substantially shocked by some policy change that is arguably exogenous, and in each case we observe some response of employment demand in the direction opposite that of the shock to labor costs. This evidence, coupled with the abundant non-experimental evidence generated in the massive earlier literature on labor demand, makes it quite clear that higher labor costs do reduce employment. Moreover, the sizes of the responses here do not seem much different from those found in the earlier literature, suggesting that Topel's (1998) concerns about the width of a confidence interval for this parameter are misplaced.

#### *B. Labor Costs and the Hours-Workers Decision*

The difficulty in evaluating the effect of changes in the relative costs of hours and workers in the U.S. is twofold. First, the federal statutory mandate that affects this relative price, the overtime provision of the 1938 Fair Labor Standards Act, has been essentially unchanged for over 60 years. This makes it hard to find evidence of what the labor market would have looked like without the policy. Second, while it is trivial to write down theoretical models linking the relative demands to the relative costs, linking those costs to particular data series is not easy: Which measurable components of labor cost affect hours per worker differently from employment?

Unfortunately neither of the two available international quasi-experiments is based directly on mandated changes in the relative price of workers and hours. Instead, both let us infer the impact of changes in standard hours, essentially changes in the point at which the price of an extra hour of labor rises, on employers' demand for hours of work.<sup>1</sup> With microeconomic household data for Germany (the SOEP) Hunt (1999) examined the impact of collectively bargained changes in standard hours that occurred in different industries at different times. Essentially using the now-ubiquitous double-difference method, she showed that cuts in the standard workweek produced declines in total employment.

Crépon and Kramarz (2000) also use micro data describing workers to study the effect of legislated cuts in the standard workweek in France in the early 1980s. Their results suggest that these caused some decline in employment and that they led to substantial job losses among those workers who had previously been observed working at the old standard weekly hours. Moreover, this job loss was greatest among minimum-wage workers (whose hourly wages presumably could not adjust to the imposed change in total labor cost). While neither study allows us to infer estimates of cross-price elasticities between hours and workers, just as most of the older literature did not, both sets of results are in line with the standard theory of labor demand.

### *C. Skill Substitution*

The issue of skill substitution—measuring cross-price and cross-quantity elasticities between different groups of workers—arises whenever we consider the potential effects of policies to alter the mix of skilled and unskilled workers in employment. Especially in the U.S., where inequality of earnings rose substantially from the late 1970s, such policies are particularly appealing. Will employers respond to changes in the relative cost of skilled and unskilled workers by substituting between groups of workers? A number of studies that have examined this issue over the last 20 years (see Hamermesh, 1993) suggest the answer on that question is yes. There are unfortunately no quasi-experimental studies like the ones I have discussed above that

inform us about this question. Here too is a fertile area for research, as other countries' differential changes in payroll taxes (e.g., the German and Dutch low-wage exemptions) or severance pay (the nearly EU-wide relaxation of job security for short-term, typically low-skilled and inexperienced workers) offer possibly exogenous shocks that allow one to identify these cross-elasticities.

### **III. Using International Evidence to Understand U.S. Labor-Market Policies**

#### *A. What Can We Learn From International Evidence on Labor-Market Policy?*

Nothing. The United States has unique institutions, a uniquely ordered labor market, and a unique history that make it impossible to use evidence from other countries' labor markets to shed light on policies here. If nothing else, the very history of the responses in those labor markets to their governments' policies means that they have developed differently from way the U.S. would have developed had those same policies been applied here in exactly the same way. Even ignoring cultural differences, there is a policy-induced hysteresis (Hamermesh, 1995) that prevents us from drawing useful inferences from foreign experiences about the likely impacts of domestic labor-market policies.

A lot. Cultures and democratic institutions are not that different, and economic theory provides clear predictions about human behavior in the face of incentives that cut across cultures and institutions. While care must be taken, common human responses suggest that we can derive useful insights from others' experiences with policy. American economists are remarkably ethnocentric about their own institutions (a comment that no doubt applies to economists everywhere). Economics journals are replete with articles on the impact of this or that policy (*viz.* the recent brouhaha about the U.S. minimum wage) in which authors bruit about the results of their empirical research as if they were universally applicable. The U.S. provides only one replication of a (federal) policy's impact; and even a true natural experiment (and most such animals are neither natural nor experimental) still only provides one replication. By using foreign

evidence we can possibly gain access to a larger number of replications. These same arguments would apply (indeed, perhaps more strongly, since the localized nature of some U.S. labor-market programs allows more domestic replications in the U.S. than in many other countries) were I viewing the situation from another nation's perspective.

We thus face a trade-off between the imperfect fit of the foreign labor market to the U.S. experience and the chance of vastly enhancing the set of available (although not necessarily completely relevant) evidence on a policy. Simple statistical theory suggests that foreign evidence will be more useful the smaller the range of policy parameters that we can observe in current or past domestic policies, and the closer that foreign policies come to having a design that we would consider here. Even if the dissimilarities between foreign and domestic institutions are great, if we have only minor variations in the parameters defining the U.S. policy, foreign evidence will be very useful. Even if there is substantial variation in the policy, if other countries' policies are similar to ours the international evidence will be also be helpful.

#### *B. Care and Prescriptions*

One possible way of bringing international evidence to bear on domestic policies is to undertake comparative studies—to analyze a particular type of policy in a large number of countries and draw general conclusions. The difficulty with this too frequent approach is that serious policy analysis requires understanding the institutions (and usually too the language in which the discussions take place). Constructing indexes of the costs imposed by different countries' institutions and estimating their impacts on outcomes in a cross-country regression is problematic. Equally problematic is estimating the same regression over a large number of international replications. In most cases we are regrettably relegated to picking out others' detailed research on their own countries and trying to draw implications for the U.S., in each case assessing the similarity of the foreign policies and institutions to our own.

Simply pointing to foreign experiences, noting some general policy differences, and drawing some broad conclusions is also a poor way to use international evidence. As a standard application of this sort of logic, the U.S. in the 1990s had much lower unemployment than in the EU (a 4.6 percentage-point difference between the U.S. and an average of the four largest EU countries during the decade). American employers (allegedly) faced much lower costs of hiring and firing than their European counterparts, a difference that has been adduced to explain the different recent unemployment histories (as European employers, loath to be stuck with employees who are expensive to fire during the next downturn, limit their hiring).

This convenient explanation ignores history: In the 1960s and 1970s European labor markets were at least as restrictive as they are now (unionization was typically higher then), although with restrictions in Northern Europe eased by the importation of Italian, Greek and Portuguese labor then as they may similarly be by Turkish, Moroccan and other labor today. Arguably too, there were fewer restrictions on hiring and firing in most segments of the U.S. than there are now. Yet during 1965-74 European unemployment rates were 2.1 percentage points below those in the U.S. A double-difference approach to the impact of imposing hiring and firing costs rules them out as a good explanation of current trans-Atlantic differences in unemployment.

With a bit more care the broad international experience can be used to help us understand our own. The premier labor-market problem in the U.S. in the past two decades has been the rise in wage inequality. While a few observers point to institutional changes as a contributing factor, most explanations center on the opening of the U.S. economy to foreign product-market competition or on skill-biased technical change. Foreign trade represents a far greater share of European GDP than it does here, and it grew during this period there as well as in the United States. Europe presumably also faced similar technologies (and similarly biased technical change); yet wage inequality rose in some parts of the EU, fell in others, and in nearly all rose less than in the U.S. (OECD, 1996, Chapter 3). How can the evidence on European wage

inequality be reconciled with the similar technology shocks and more open economies? No doubt more subtle answers can be derived (as in the excellent studies by Berman *et al*, 1998, and Ljungqvist and Sargent, 2001); but this simple comparison should vitiate in most observers' minds the usefulness of the many American studies that rely on the relation between inter-industry differences in changes in trade and technology and changing earnings inequality.

### *C. Nearby Mirrors on Specific Policies*

One of the most useful applications of foreign evidence is prescriptive, to a policy proposal that has not been tried here or on which domestic evidence cannot be obtained. In Section II I indicated how proposals to change standard hours under the FLSA (e.g., 104<sup>th</sup> Congress, H.R. 1050) might be evaluated *ex ante* using international studies. Proposals to change the penalty rate applicable to overtime work are equally amenable to foreign evidence, as the structure of overtime regulation is similar to the U.S. in many European countries, and regulations there have exhibited greater variation in the policy parameters.

In discussions of labor-market policy the greatest ratio of ink to importance has been spilled over the minimum wage. Recent discussion (e.g., Card and Krueger, 1995) has suggested that increasing the minimum wage might not reduce employment and that evidence of any negative effects is very weak. Perhaps so; but the U.S. is a particularly poor laboratory in which to evaluate the impact of mandated wage minima: 1) The mandate is national; 2) There has been relatively little variation in the effective minimum other than steady erosion after the few times it has been raised, and 3) The minimum wage rates cuts only a small way up the wage distribution. Attempts to circumvent these problems have relied on small spatial and temporal differences resulting when some state-mandated minimum wage rates exceeded the federal minimum wage rate (usually by small amounts).

Foreign evidence obviates the need for contorted econometric case histories that are designed to find needles in statistical haystacks. In Canada all wage minima are provincial

mandates; a number of developing countries, for example, Mexico and Colombia, have drastically varied effective wage minima; and others, e.g. France, have substantially varied their effective minimum wage around a much higher average than the U.S. The evidence from these comparisons is clear: If we raise the minimum wage so that it cuts deeply into the wage distribution, employment (especially that of low-skilled workers) decreases substantially (Bell, 1997; Abowd *et al*, 1999b; Baker *et al*, 1999). Contrary to what one might infer from some recent discussion, demand curves in low-wage labor markets do slope downward.

For labor economists the U.S. “system” of unemployment insurance is a gold mine, since it is not one but 53 separate systems. That fact allows us to exploit substantial, possibly exogenous, albeit not always independent variation in the major parameters describing benefits. Thus the now immense literature on the impact of higher and potentially longer benefits on unemployment duration is based on more than one set of underlying policy parameters. Even here, though, there is a problem: Intersystem variation in parameters is limited compared to most other countries, especially in terms of the potential duration of benefits. One wonders how useful the fairly strong consensus about the effects of higher benefits in the U.S. is for predicting the effect of non-marginal changes in policy, even if those might not lead to systems as generous as most of those in Europe.

Concern about the robustness of U.S. results to large changes in policy parameters seems misplaced: Within the range observed in the U.S., the impact of marginal changes in the potential duration of benefits, and especially in replacement rates, seems roughly the same as for equal-sized marginal changes in European countries with their more generous systems (Atkinson and Micklewright, 1991). (Some very careful evidence, Narendranathan and Stewart, 1995, suggests, however, that increases in potential duration beyond what we observe in the U.S. cause only minute effects on the actual duration of long spells of unemployment in times of high unemployment.) Consensus estimates from the first ten studies of this issue (Hamermesh, 1977),

that each 20-percent increase in net replacement raises the actual duration of spells of unemployment by one-half to one week, seem to have stood up well against both the huge amount of subsequent U.S. research and against international evidence.

The unique aspect of American UI systems is their reliance on partly experience-rated payroll taxes. These are designed to encourage employers to minimize separations by making them pay (at least part of) the costs they impose on their state's UI system. Only recently have other countries begun to experiment with experience-rated financing of UI benefits; and some do not even rely on a payroll tax to finance benefits. A growing amount of evidence (e.g., Anderson, 1993) indicates that greater experience rating reduces employment variation and temporary layoffs. The difficulty with these believable results is that they do not jibe with the European experience: Temporary layoffs are very rare in Europe, and employment fluctuations are smaller than in the U.S., yet there is no experience rating of payroll taxes.

Do European employers not optimize the way our models say they will? Here the institutions of employment relations are sufficiently different that we cannot expect to transfer the evidence directly. Employers' and workers' responses to the underlying incentives do, however, transfer. In the Dutch system in the 1990s employers, especially in the manufacturing sector, appeared to use their generous and long-duration UI benefits to ease workers into a comfortable early retirement and postpone (and thus raise) their eventual pension benefits (Hartog, 1996). Just as in the United States the failure to experience rate fully leads to cross-subsidization from some industries to others that can easily lay off workers, so too in the Netherlands employers and workers in industries where early retirement could be offered easily were subsidized by other firms. The response was the same, but it was along a dimension that is more consistent with other labor-market institutions.

American immigration policy is essentially federal, allowing only one domestic replication. Infrequent policy changes mean that immigration is one area where international

replications may be especially useful. Perhaps not surprisingly, the ubiquity and importance of such policies has generated large amounts of international evidence. An area of special concern in the U.S. has been the impact of immigration on natives' earnings and unemployment, with the conventional wisdom from most studies suggesting little effect (see the summary in Hamermesh, 1993, Table 3.10), but with at least one recent challenge (Borjas *et al.*, 1996).

Two foreign-based studies are especially relevant here. Hunt (1992) examines the impact of the *pieds noirs* on France after Algerian independence and concludes that this massive flow had only a small effect on natives' earnings, and an even tinier one on unemployment. Withers and Pope (1985) test for Granger causality between unemployment and immigration using very long time series on Australia. They find no evidence that (very large) variations in Australian immigration altered domestic unemployment (although flows of immigrants responded sharply to recent changes in unemployment). This very convincing foreign evidence does not support challenges to the conventional wisdom.

#### *D. Basic Policy Research*

In many cases the best policy research is not specific, but instead seeks to generate good estimates of basic parameters that will provide the foundations for evaluating proposed policies that lack current close analogs. A huge amount of policy evaluation in government is based on this approach (tax incidence, child-care policies, mandated employee benefits, tax cuts, and others). Such estimates should also provide grist for the busy macro-calibration and computable general equilibrium mills. To the extent that government policy evaluators and the operators of those mills are interested in grinding real instead of imaginary wheat, do they gain anything from evidence generated by other economies? The main issues here are the elasticities of labor demand and labor supply—the basic behavioral parameters in labor economics.

Section II already discussed some new international evidence on substitution and scale parameters in labor demand. International evidence, albeit not of the quasi-experimental variety,

may be even more useful in the area of the dynamics of labor demand. Ideally such dynamics can be informative about the impacts of policies that mandate limits on hiring and firing—particularly those that alter firing costs. The size of the costs imposed by mandates on hiring and firing, along with the much greater range of these mandates in Europe, necessitates relying on foreign evidence. A comparison among studies based on the appropriate firm-level data suggests that the adjustment of employment in response to demand shocks is slower in Europe than in North America (Hamermesh and Pfann, 1996). While the results are not so closely linked to theory that we can quantify the impact on employment dynamics of policies that raise hiring or firing costs, the results strongly suggest that raising those costs does induce additional labor-market rigidities. Moreover, that they add to employers' costs means that these mandates reduce the level of market employment and thus measured output, as some of the evidence in Section II showed.

A booming industry in estimating labor-supply parameters has slowed down in the past 15 years and gone off in the directions of concern about the nature of household decision-making, worries about abstruse econometric detail, and recently generalizations linking observed behavior to more complicated dynamic optimization models (Lich-Tyler, 2001). The best American estimates, mostly from research completed by the mid-1980s (summarized, for example, by Heckman, 1993), suggest that for adult males and increasingly (Shaw, 1994) adult women too labor-supply elasticities are quite small. Probably the most important policy application of these estimates is to inferring *ex ante* the likely impact of cuts in taxes on labor. The relatively narrow band of marginal tax rates in the U.S. should leave us a bit skeptical about inferring these impacts. This lack of variation suggests obtaining evidence using the greater variation in tax rates that exists in most European countries. Perhaps the best evidence on this is Stuart's (1981) from Sweden's then extremely high marginal tax rates on labor, which implied large labor-supply responses at very high tax rates.

Even if one accepts, contrary to nearly all the evidence, that the labor supply of most workers responds strongly to changes in wage rates at current tax rates, one would be able to infer that cutting tax rates would sharply increase labor supply only if workers treat tax changes as equivalent to increases in wage rates. While there is a strong American consensus that this is true (based on Rosen, 1976, and several subsequent studies), given the small range of tax rates in effect today it would be difficult to replicate those studies in the United States. This is especially unfortunate, since none of them used the now widely accepted techniques designed to account for complexities in measuring tax effects (e.g., the work of Hausman, 1985). The American consensus seems contradicted, however, by a careful recent German study (König *et al*, 1995), the first on this issue to use the econometric techniques of the past 20 years, and one that is based on a somewhat wider range of tax rates than exists in the U.S. That study suggests tax rates are perceived only imperfectly, with the surprising finding that the greatest failure of perception is among higher-income families. If this result holds true in subsequent research, it implies even less change in work effort in response to tax cuts.

#### *E. Worth the Candle?*

As the discussion above should make clear, the usefulness of foreign evidence ranges from moderate to crucial. Figure 1 illustrates the idea of the intellectual trade-off implicit in the discussion here by summarizing for each of a number of general areas of applied research in labor economics the degree to which other countries' institutions approximate those in the United States and the extent of variation in the relevant policy parameters in the U.S. program(s). Movements up the vertical axis imply a greater similarity of U.S. and foreign institutions, and rightward movements along the horizontal axis describe lesser variation in the parameters describing the U.S. institutions. The best field for international evidence is that where other countries' institutions are similar to American ones, but for whatever reason they have experimented with a broader range of values of the parameters underlying the institutions than we

have. I summarize the discussion in this section by including a number of specific policy types here. No doubt other policies could be placed on this graph, and the value of international evidence on them could be assessed too. The preference map in the Figure is my own, and readers are free to impose their own preferences. Clearly, however, because of the small domestic variation, both cross section and over time, and because the underlying economic questions are fairly general, policies like hours legislation and the minimum wage provide especially fruitful areas in which to apply the results of studying foreign experiences to the U.S. policy debate.

#### **IV. International versus U.S. Data**

Stafford (1986) perceptively pointed out the role of new data sets in reinvigorating empirical research and in providing the basis that allows us to generate new ideas about the nature of labor markets. The quality of applied research is limited by the extent and quality of the data sets that are available to analyze the issues. In the area of household behavior—labor supply and consumer behavior—the U.S. has long been a leader. Our longitudinal data sets, particularly the Panel Study of Income Dynamics and the National Longitudinal Surveys, have provided models for surveys in many other countries, both in the English-speaking world and in numerous other nations. The Current Population Survey has a longer tradition and is as comprehensive as labor-force surveys anywhere. The raw data necessary to provide estimates of the basic labor-supply parameters are as good here as they are anywhere.

There is much that cannot be learned about labor supply and household behavior using the snapshot and retrospective information in which U.S. household data sets have specialized. They fail to tell us what the respondents are doing, and when and with whom they are doing it. Unfortunately in this area of household behavior the U.S. has been in the *derrière garde* in generating useful data sets. The last representative time-budget study for the U.S. was conducted in the mid-1970s. Far more comprehensive and larger surveys have been produced in many other

countries (with Australia and Germany having two of the best of these). They have been repeated at various intervals over the past two decades and can be used to generate a new and otherwise unobtainable understanding of household behavior (cf. Gronau and Hamermesh, 2001).<sup>2</sup>

The situation is still bleaker if one wishes to study labor demand or the interactions of firms and workers. Cross-section matches of large databases of workers and establishments have been produced in the U.S. (e.g., Troske, 1998); but the U.S. has just one establishment-level longitudinal data set (the Longitudinal Research Database). That source is limited to manufacturing industry and is only now being linked to other data sets that might provide information on workers' characteristics. France, for example, has a detailed economy-wide sample providing longitudinal information on matched workers and firms (Abowd *et al*, 1999b); Denmark has similarly detailed information covering the entire population of firms (Albæk and Sørensen, 1998); the Netherlands, Britain and Australia have surveys that, while not as broad as the French or Danish, are far more extensive than anything on offer in the U.S. Powerful fears about confidentiality, and no doubt some bureaucratic inertia, have left the U.S. behind many other developed countries as a place to study how jobs are generated and how workers and employers interact to create job matches.<sup>3</sup>

## **V. International and North American Data and Scholars in the Marketplace for Economic Ideas**

A reasonable question in response to these arguments is: If greater internationalization of empirical labor economics is potentially so profitable, why do editors of the leading North American economics journals not publish more studies using international data? Are they unaware of the importance of such studies? Do they simply publish work by people (North American economists) with whom they are familiar? These possibilities go hand-in-hand with the oft-voiced complaints of European labor economists that their research is rejected by the leading U.S. journals because it is based on non-North American data. In this section I examine the empirical validity of this claim and the more general issue of how the locations of authors and

of data sources in studies in empirical labor economics affect where they are published and their subsequent impacts on other scholars.

#### *A. Editorial Motivation*

The background question is what goals editors of general economics journals have in mind. No doubt some have venal goals, but the main goals must surely be: 1) To maintain readership by presenting papers that engage intellectual and occasionally popular interest; 2) To encourage additional scholarly research that will, more concretely, generate recognition (in the form of citations and public acknowledgment) of the journal. Editors who fail along both criteria are likely to see their journal's importance diminish over time (as undoubtedly has happened to some general economics journals over the past three decades).

A rational editor will therefore publish pieces that he/she believes will meet these goals. Specifically, such an editor, while uncertain about the future impacts of the articles that are submitted, should on average be publishing articles whose expected impacts are identical. Obviously the expected impacts will differ across individual articles (some are obvious blockbusters right from the start, while others are, rightly or wrongly, accepted in full realization that they will be far from path-breaking). If, however, we define a set of categories, the rational editor will make sure that the expected impacts of the marginal article in each definable category will be equal. While we cannot know what is the marginal article in a particular category, as a first approximation we can examine this issue by studying whether articles in different categories by location have different impacts on average. Thus an initial test of whether American editors are publishing "enough" research using foreign data would compare the impacts of studies using North American and other data that are otherwise identical.

#### *B. Publications in Empirical Labor Economics, 1990-98*

To examine how the locations of authors and data affect publication in labor economics I collected data on all empirical papers in labor economics published from 1990-98 in what are

arguably the top North American general journals: the *American Economic Review*, *Journal of Political Economy* and *Quarterly Journal of Economics*. As a comparison group I also included empirical papers in labor economics published in three major general European economics journals: the *Economic Journal*, *European Economic Review* and *Review of Economic Studies*. Given my classification of research as empirical, this search resulted in a data set of 376 articles. The studies were classified along three criteria: 1) Did at least one of the authors have a North-American affiliation (treating Canada and the U.S. as one)?<sup>4</sup> 2) Were the data used North American, non-North American, or mixed (i.e., several data sets, including a North American one, were used, or the study involved a cross-country analysis that included North American data)? 3) Was the journal one of the three North American outlets, or not?

The frequency distributions of the articles in the samples along these three criteria are shown in the top two panels in Table 1. Quite unsurprisingly, the U.S. journals tend to publish articles with at least one North American author (93 percent of the papers), while the European journals also have a “domestic” focus (although they exhibit a bit more diversity—with “only” 77 percent of the published papers having no North American authors).<sup>5</sup> Just as the U.S. journals publish few non-North American authors, the North American authors they do publish concentrate almost exclusively on North American data sets: The admonitions in the previous sections have certainly not been reflected in publishing practices in major U.S. journals. 82 percent of the articles in those journals are by North Americans and use North American data. The record is slightly more diverse in the European journals, but even in them 68 percent of the articles are by non-North Americans (and no North American coauthor) who are using data exclusively from outside North America. One might well infer that there is essential segregation between these two markets for the dissemination of ideas.<sup>6</sup>

It is interesting to see how these results compare to those for this *Journal*, arguably the most widely cited labor economics journal that does not focus on one country specifically

(Kalaitzidakis *et al*, 2001). The bottom panel in Table 1 classifies in the same way as for the general journals the empirical articles published in regular issues of this *Journal* from 1992-2001, a sample of 204 articles. The frequency distribution of articles shown in Table 1 looks quite similar to that describing the top North American general journals, in particular in the fraction of articles that are authored by North Americans and that use North American data. There are, however, two interesting differences: 1) This *Journal* seems more open to research by non-North Americans using non-North American data than are the top general North American journals; and 2) This *Journal* publishes fewer studies that use mixed sets of data, perhaps reflecting a lesser interest in comparative studies or those that use experiences in many economies to test some general hypothesis.

The issue here is whether the apparent segregation between the U.S. and European general journals is rational, in particular, whether U.S. editors have not moved too far down the quality level of studies using North American data compared to foreign data sets.<sup>7</sup> To examine this possibility I collected data on subsequent citations by other scholars to each of the studies in these six general journals. Whether this is a reasonable measure of the studies' quality is arguable, but it is one that has been used many times in the literature on academic productivity (Hamermesh *et al*, 1982, is a very early example). Because it takes time for studies to be recognized, I include each of the first four years of post-publication citations in the estimates.

Table 2 presents the means and standard errors of the means of subsequent citations to the 344 articles in the sample that did not contain mixed data sets. In this sample the articles published in the North American general journals are more frequently cited than their European counterparts. Among the articles published in North American journals, those that use North American data receive more citations (significantly more on a cumulative basis after the first year of publication). On the other hand, articles published in European journals that use non-North American data receive (almost significantly— $t=1.81$  on the cumulative four-year citation count)

more citations than those using North American data that are also published in those journals. There may be citation networks that generate citations to papers published on one's "home journals," or perhaps these contrasting differences merely demonstrate the parochialism of researchers on both sides of the Atlantic.

To examine these issues in more detail, I estimate negative binomial regressions describing subsequent citations to these 344 articles. (I use this technique because the data are integer counts of citations and because initial estimates showed that the simpler Poisson distribution did not characterize the variation well (Wooldridge, 2002, Chapter 19).) Because an article's subsequent citations may depend on the author's reputation independent of the quality of the publication *per se*, I include in the regressions a vector of indicator variables describing the first author's citation count in the year the article was published (10-49 citations, 50+ citations, with fewer than 10 citations per year the excluded category).<sup>8</sup>

Table 3 presents the estimates of these equations for the first post-publication year and for cumulative citations through years  $t + 2$ ,  $t + 3$  and  $t + 4$ . Unsurprisingly, articles whose authors are more heavily cited are themselves significantly more heavily cited, whether because the articles are of higher quality or because of a "Matthew Effect" (Merton, 1968) is unclear. The estimated coefficients of the main effect terms on the location of author, data and journal and their interactions are not particularly interesting by themselves, so instead I focus the interpretation of the results on the partial derivatives presented toward the bottom of this table. Articles published in North American journals are more widely cited ( $\delta\text{CITS}/\delta\text{NAJournal}$  is significantly positive), even conditional on the author's prior citations. Despite this, it is gratifying to note that  $\delta\text{CITS}/\delta\text{NAAuthor}$  is significantly different from zero only in the equation estimating the impact on the four-year cumulative citations, and even there  $t=1.81$  only. The author's location does not have a highly significant effect on the impact of the article, once we account for the author's prior productivity. Otherwise identical articles (in terms of journal

outlet, author's affiliation and author's prior scholarly impact) that use North American data, however, are generally more widely cited than those that do not. While the estimate of  $\delta\text{CITS}/\delta\text{NADData}$  is not statistically significant for all cumulative citation counts, after year  $t + 1$  the t-statistic always exceeds 1.64.

In no way do these results suggest that North American editors are averse to publishing high-quality articles by foreign authors. Nor do they imply that those editors favor studies using North American data. Rather, the estimates suggest that, if anything, the editors have moved further down the quality curve on articles using non-North American data than on those using North American data. They appear to have favored studies by non-North American authors and studies using non-North American data, at least as measured by the subsequent impact of these studies on other scholars' research. Thus both grounds on which European researchers might complain about their treatment at the hands of U.S. editors appear to be unfounded.

These conclusions generate a conundrum: If research using foreign data sets is no more productive than that using North American data, what happens to the arguments made in the Sections II and III for using foreign data and foreign labor-market policies to study the American labor market and American labor-market policies? The results presented in this section can easily arise even if foreign data and policies are highly valuable in studying U.S. labor markets. They only demonstrate that the U.S. labor economists, who generate the bulk of academic research in our subdiscipline and thus the bulk of citations to other work, currently pay little attention to and fail to cite research that uses foreign data. A major purpose of the arguments in Sections II and III is to demonstrate to U.S. labor economists that our failure to acknowledge the value of foreign material is misplaced and that we may have gotten ourselves trapped in a suboptimal intellectual equilibrium. That equilibrium is reflected in the empirical results in this Section.

## **VI. Conclusions**

I have provided a variety of justifications designed to lure North American labor economists away from a narrow view of potential sources of evidence on the general labor-market and specific North American issues that concern us. These admonitions in no way are meant to suggest that we should search mindlessly for whatever cute natural experiment or tweaking of policy is thrown up by other countries' experiences: As I hope the discussion in Section III made clear, the cleanliness of the evidence that is generated must always be weighed against the relevance of the foreign experience. I am not offering a panacea, merely a way to accomplish our goals of understanding basic labor-market behavior better and provide better estimates of the likely impact of proposed policy changes.

How is this proposed greater catholicity of practice to be attained? In many cases the data most appropriate for our purposes are described in a language incomprehensible to us. In other cases we are not even aware that the best possible data even exist outside the U.S. In the most egregious cases we refuse even to contemplate the possibility that looking outside the U.S. might be valuable. I hope the examples I have provided here reduce the number of such cases. If they do, making an effort to learn what other economies' experiences have to offer and what data sets are available suggests itself as a valuable step for any newly reformed jingoistic American economist. And, if the data are not easily understood, using foreign data with a coauthor familiar with them is a good way to enhance the quality of one's own research.

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**Table 1. Frequency Distributions of Empirical Labor Economics Articles by Journal Location, Author Location and Location of Data**

**Author:**

**North American Non-North American Total**

**North American General Journals, 1990-98 (N=228)**

**Data Type:**

North American	.816	.022	.838
Non-North American	.048	.022	.070
Mixed	.070	.022	.092
Total	.934	.066	1.000

**Non-North American General Journals, 1990-98 (N=148)**

North American	.128	.020	.148
Non-North American	.095	.682	.777
Mixed	.007	.068	.075
Total	.230	.770	1.000

***Journal of Labor Economics*, 1992-2001 (N=204)**

North American	.833	.010	.843
Non-North American	.049	.088	.137
Mixed	.015	.005	.020
Total	.897	.103	1.000

**Table 2. Means and their Standard Errors of Subsequent Citations by Geographic Location of Data and Journal**

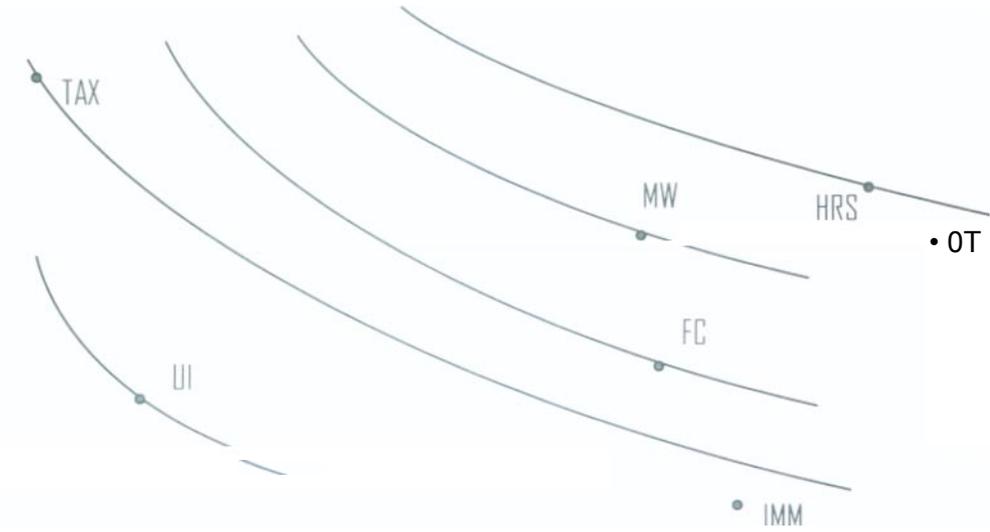
<b>Cumulative Citations Through Year</b>				
	t+1	t+2	t+3	t+4
<b>North American Journals</b>				
<b>Data Type:</b>				
North American	1.85 (0.16)	5.27 (0.46)	9.74 (0.87)	14.85 (1.46)
Non-North American	1.38 (0.39)	2.73 (0.84)	3.00 (0.71)	6.75 (1.28)
<b>Non-North American Journals</b>				
North American	0.45 (0.17)	1.45 (0.41)	2.22 (0.74)	3.31 (0.97)
Non-North American	0.57 (0.09)	1.81 (0.22)	3.24 (0.36)	4.92 (0.57)

**Table 3. Negative Binomial Estimates of Subsequent Citations to Empirical Labor Articles in General Economics Journals<sup>a</sup>**

	Cumulative through Year:			
	t+1	t+2	t+3	t+4
Author 10-49 Citations	.394 (.150)	.472 (.130)	.528 (.122)	.551 (.127)
Author 50+ Citations	.874 (.189)	.949 (.180)	1.026 (.173)	.970 (.185)
No. Amer. Journal	.450 (.355)	.468 (.562)	.070 (.522)	.289 (.552)
No. Amer. Author	.894 (.355)	.751 (.304)	.404 (.301)	.416 (.320)
No. Amer. Data	-.273 (.761)	-1.682 (1.121)	-2.345 (1.110)	-1.204 (.662)
NA Journal/Author	-.399 (.709)	-.847 (.741)	-.702 (.717)	-.403 (.745)
NA Journal/Data	1.227 (.554)	2.288 (1.370)	3.827 (1.316)	.580 (1.120)
NA Author/Data	-.719 (.726)	1.023 (1.189)	1.759 (1.175)	.517 (.775)
NA Journal/Author/Data		-.969 (1.478)	-2.038 (1.429)	.780 (1.257)
$\delta\text{CITS}/\delta\text{NAJournal}$	1.278 (.374)	.939 (.291)	1.157 (.273)	1.266 (.280)
$\delta\text{CITS}/\delta\text{NAAuthor}$	.225 (.162)	-.042 (.424)	-.576 (.664)	1.329 (.733)
$\delta\text{CITS}/\delta\text{NADData}$	.234 (.328)	.660 (.392)	1.203 (.406)	.692 (.405)
Pseudo R <sup>2</sup>	.080	.069	.074	.067
N	344	306	273	238

<sup>a</sup>*American Economic Review, Journal of Political Economy, Quarterly Journal of Economics, Economic Journal, European Economic Review and Review of Economic Studies.* Standard errors are in parentheses below the coefficients and partial derivatives.

Institutional  
Similarity



(US Parameter Variation)-1

Figure 1. Policy Variation in an International Setting

- OT = Overtime Premium
- HRS = Standard Hours
- MW = Statutory Minimum Wage
- UI = Unemployment Insurance
- IMM = Immigration Policy
- FC = Firing Costs
- TAX = Taxes and Effort

## Footnotes

\*Centennial Professor of Economics, University of Texas at Austin, and research associate, National Bureau of Economic Research and Institute for the Future of Labor (IZA). This article is a revised version of a presidential address delivered at the annual meeting of the Society of Labor Economics, April 20, 2001. Elaine Zimmerman provided helpful research assistance for the analysis in Section V. I thank Joop Hartog, David Neumark, Gerald Oettinger and Gerard Pfann for helpful comments.

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<sup>1</sup>OECD (1998) provides information on a substantial variety of changes in overtime penalties that occurred in member countries during the 1990s. A series of studies of their effects on employment and hours, based on microeconomic data, would be a tremendous addition to our knowledge of labor demand.

<sup>2</sup>In 2003 the U.S. Bureau of Labor Statistics is beginning an effort to bring America into the forefront of this line of research by instituting a small monthly time-budget survey. While it will lack some of the features of the infrequent, but larger foreign surveys, it is a very welcome step forward.

<sup>3</sup>There are some rays of hope along this dimension. John Abowd, Julia Lane and others are engaged in a major project designed to make a large array of existing administrative records usable by researchers.

<sup>4</sup>None of the results changes qualitatively if I delete Canadian-authored studies from the sample, or if I lump them together with non-North Americans.

<sup>5</sup>This somewhat greater openness to non-residents is shown more generally by Elliott *et al* (1998).

<sup>6</sup>One might wonder whether the results for the three non-North American journals might look different if we exclude the *Economic Journal*, which may arguably be British rather than European generally. Excluding this journal reduces the sample of articles to only 59, but the frequency distribution looks remarkably similar to that in the second panel in this Table: The first column has the values .136, .050, 0 and .186, while the second column has the values .034, .644, .136 and .814.

<sup>7</sup>It might be the case that this apparent segregation has resulted from an unwillingness of non-North American authors to supply high-quality papers to the top U.S. general journals, and a similar unwillingness of North American authors to supply such papers to the European journals. The former possibility, at least, is inconsistent with the frequent complaints of European economists.

<sup>8</sup>If we exclude the author's citations the results are somewhat stronger.

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