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Chapter Author: Robert Topel

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Analytical Needs and Empirical Knowledge in Labor Economics

Robert Topel

I was grateful to be able to answer promptly and I did.
I said I didn't know.
—Mark Twain

That would be a large mistake, even for an economist.
—An astrophysicist, commenting on a gross miscalculation
of the location of a galaxy

What types of new data would further our understanding of how labor markets work? My unenviable task is to summarize important analytical issues in empirical labor economics, and how these issues might be resolved through the collection of better data. This is no small assignment since it requires a parallel assessment of the state of empirical knowledge—what we know and what we should know—in the study of income and wealth. This evaluation is bound to be subjective, which may itself be a comment on our current state of knowledge. The type of data that I think should be collected and analyzed depends naturally on what I think it is important to learn about.

The paradigm for my discussion divides empirical research in labor economics into two useful functions. The first is descriptive. Perhaps more than any other scholars, labor economists are walking arsenals of facts. How has wage inequality changed over the past 25 years? How much more do Americans work than Germans? How much more do college graduates earn than high

Robert Topel is the Isidore Brown and Gladys J. Brown Professor in Urban and Labor Economics at the University of Chicago Graduate School of Business and a research associate of the National Bureau of Economic Research.

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school graduates, and how has that premium changed over time? Which worker types earn more than others? How many times does the typical worker change jobs in his or her career, and when do those changes occur? To answer these questions, labor economists describe a particular market equilibrium or compare equilibrium outcomes over time or space.

For this descriptive function the main data issue is one of detail. How can we get more, and more detailed and accurate, data that allow us to describe the labor market and employment relationships?

The second function, which is at least as important and involves substantially more economics, is to estimate the comparative statics of economic models that apply to the labor market. How does immigration affect the wages of native workers? Do declining wages cause unemployment rates to rise? How would a particular policy intervention (mandated employment benefits, payroll taxes, transfer programs, etc.) affect behavior? Does pay-for-performance improve productivity? For questions like these, the key data requirement is rarely one of detail. Instead, these issues require sources of variation in real-world data and constraints that are capable of identifying behavioral parameters.

My view is that empirical research on labor markets has been remarkably successful in the first, descriptive function. Microdata on individuals and households, which became widely available in the 1970s, have greatly advanced our stock of knowledge about basic empirical facts. There is much left to be learned, but at least the types of data that could be collected are well defined. And, if more data were collected, we can be confident that important factual questions about labor markets and the determinants of income would be answered. In turn, these facts will influence research by labor and other economists—they constrain theories to be within the bounds of what we know to be true or relevant—just as our past accumulation of empirical knowledge has done. And more constraints put on economic theorists would surely be good.

Much less can be said for our knowledge of the behavioral parameters of economic models. The same mass quantities of microdata, which have been analyzed by every labor economist, have not led to consensus on behavioral responses. What are the elasticities of labor supply and demand? The range of credible estimates of these most basic parameters is, as one survey of the labor supply literature put it, “dauntingly large.” Years of quantitative research have not done much to narrow our (or at least my) confidence bounds on these effects. This is regrettable because knowledge of behavioral parameters is the foundation of policy evaluation, about which we should have something to say. In this area, perhaps labor (and other) economists should be more willing to follow Twain’s example of modesty.

Why the dichotomy between our knowledge of facts and our ability to measure economic behavior? I think there are two reasons. One is that much descriptive research is model free. It describes what is, not why. It is one thing to point out that college enrollment rates and the relative wages of college gradu-

ates both increased in the 1980s. It is harder to show that one caused the other. Not all descriptive research is model free, however. I would categorize the ever growing literature on estimating the “true” returns to schooling as descriptive. Yet this literature seeks to identify a particular parameter, with many of the same identification issues that arise in estimating behavioral models.

The second reason is the nature of the data that labor economists analyze. Household data constructed to study income and wealth, like the census, the Current Population Survey (CPS), and the Panel Study of Income Dynamics (PSID), are *meant* to be descriptive. They are not experimental. Behavioral effects can only be teased out of such data through identifying assumptions that are typically open to dispute. “Natural experiments,” which seek arguably exogenous variations in incentives in nonexperimental data, are just another name for credible identifying assumptions. Absent true experimental data—which are only rarely available, and even then only for the narrowest of problems—this state of affairs will continue. There are no easy solutions, so progress will remain slow.

The paper is organized as follows. Section 2.1 describes a role for descriptive empirical research, outlines some successes, and discusses some areas where I believe that better data would have high marginal value. I do not propose any general strategy for collecting new data, or new types of data. I follow in section 2.2 with a largely pessimistic review of what we know about magnitudes of behavioral responses in economic models of the labor market, and the prospects for improving the situation with better data. I offer no solutions. Section 2.3 concludes the paper.

2.1 Describing Labor Markets

The growth of empirical labor economics as a field of research coincides with the availability of detailed microdata on firms and especially households. Labor economists today are vastly better informed about the details of relative wages, unemployment rates, and labor force composition than 25 years ago. The wealth of our knowledge, and our continuing efforts to document more facts, is not always counted as a blessing. One of my colleagues, an international economist, disparagingly refers to labor economists as “accountants.” Another laments that there is too little *economics* in recent empirical research. Yet the international economist studies the effects of international trade on wage inequality. His own research agenda would be entirely different had labor economists not documented the unprecedented increase in inequality that occurred over the past two decades and postulated that it might have to do with trade (Murphy and Welch 1992; Johnson and Stafford 1993; Borjas, Freeman, and Katz 1992).

This effect is one of the main roles of descriptive economic research. Economic problems are inherently empirical, seeking to explain or predict real-world outcomes and behaviors. Descriptive research then affects theory in two

useful ways. First, theories are developed in order to explain prominent facts that arise from data. The observation that wages rise with experience and job tenure, but at decreasing rates, spawned theories of life cycle human capital investment and the distinction between general and specific training (e.g., Ben-Porath 1967), as well as competing models based on search and matching (Jovanovic 1979a, 1979b) and incentives (Lazear 1995).

Second, the “facts” act as a constraint on the class of admissible theories. The observation that real wages are procyclical caused an early rejection of Keynes’s original formulation, which predicted a countercyclical wage. The fact that displaced workers suffer substantial and persistent reductions in earning power (e.g., Jacobsen, LaLonde, and Sullivan 1993) gives credence to models that emphasize the importance of specific human capital in employment relationships. Models in which human capital is general are not up to the task. Similarly, the robustness of estimated returns to schooling means that signaling theories of the demand for education are no longer given much credence.

This role for descriptive research helps to define the areas where collection of more, or more detailed, data would have the greatest returns. In what areas of labor market analysis is theory constrained the least by hard empirical facts? Anyone’s list of areas where more or better data would be beneficial is long; I will emphasize four that stand out in my own thinking: (i) the economics of personnel and internal labor markets, (ii) the determinants of wage and income inequality, (iii) the activities of low-income individuals, and (iv) the operation of labor markets in developing economies.

2.1.1 Internal Labor Markets

The typical male worker in the U.S. labor market is now employed in a job that will last 18 years (Akerlof and Main 1980). This means that the wages, incomes, and hours of work we observe in survey data are mainly the outcomes of continuing trade between a single employee and a single employer. We know that these durable employment relations evolve from rapid turnover at the beginning of careers, accompanied by substantial wage gains at job transitions (Topel and Ward 1992). We also know that the termination of long-term employment relations typically causes large and persistent reductions in earning power (Jacobson et al. 1993). But we know very little about what happens in between.

This ignorance is a boon to theory. Almost all of the literature on compensation, advancement, and incentives in organizations is based on anecdotal or impressionistic evidence, or just introspection. Examples from universities loom large. The facts impose few constraints, so theories are built with little notion of which factors are important in real-world employment relations and which are not. Among the things worth knowing are the following:

1. What is the relationship between total compensation and wages for workers at different skill levels? Aggregate evidence suggests that much of the

slowdown in aggregate real wage growth during the 1970s and 1980s can be attributed to the growing value of nonwage benefits (Council of Economic Advisers 1987). Collection of detailed data from *firms* on what they pay, and the value of benefits they offer, would give us a more detailed picture of the distribution of well-being. It is likely that the distribution of compensation has become even more unequal than the distribution of income, but we will not know until the data are collected.

2. How do careers develop? Do movements of employees among tasks and levels of firms mirror patterns of mobility between firms? How does within-firm mobility contribute to wage growth? Do raises mainly occur at times of promotion (McCue 1996), or is there substantial growth among workers who remain at a single task? Does within-cohort wage inequality increase, as most theories of learning about talent would imply?

3. Who leaves an organization? Is it the stars, who have risen rapidly, or the poorer performers who may be poorly matched?

4. How is performance evaluated and rewarded? How does evaluated performance vary over a career?

5. Do compensation policies emphasize equity relative to measured performance? To what extent are wages tied to jobs, or tasks, rather than to individuals? Under what circumstances is pay more likely to be individual based, as opposed to job based?

What kinds of data would allow us to answer these questions? Most large organizations that I have encountered maintain detailed personnel histories in computer databases. Analyzing a single company is a research project in itself (for initial attempts, see Lazear 1995; Baker, Gibbs, and Holmstrom 1994). This means that truly generalizable results will be slow in coming. But we know so little now that any information on what goes on inside the “black box” of employment relationships would be useful. It is noteworthy that in other countries—such as Korea, Japan, and France—detailed data are collected from firms on the compensation and characteristics of individual employees. Similar data for the United States would be a major step forward.¹

2.1.2 Determinants of Wage Inequality

Why do some workers earn more than others? Information on personal characteristics in the typical survey file—like the CPS or PSID—does not go much beyond a respondent’s age and years of schooling. These observable dimensions of human capital explain about 30 percent of the variance in wages. The rest is open to theorizing. For example, the fact that some industries or firms pay more than others (Brown and Medoff 1989; Krueger and Summers 1987) is interpreted by some as evidence of economic rents (for a summary, see Katz 1986) and by others as evidence of selection on talents that are unobserved by

1. Abowd and Kramarz (1994) analyze the French data, which are longitudinal and cover most of the workforce.

econometricians (Murphy and Topel 1987). Similarly, race and gender differences in wages may reflect market discrimination or premarket differences in unobserved human capital.

Collection of more detailed information on personal characteristics could go some distance toward resolving these puzzles. What types of schools did people attend? What did they study, and how well did they do? An unresolved issue is the role of standardized tests in measuring the talents that are valued in the market. Johnson and Neal (1996), using the National Longitudinal Survey (NLS) youth cohort, find that performance on the Armed Forces Qualifying Test (AFQT), taken in high school, helps to predict subsequent earnings. A 1-standard-deviation increase in AFQT performance raises earnings by about 20 percent. They find that wage differences between young blacks and whites are greatly reduced when AFQT scores are controlled for. These results suggest that market discrimination may be less important than differences in premarket opportunities in determining earnings. More important, they point to an important role for basic premarket skills in affecting earnings. These skills are unmeasured in most sources of survey data.

How important are these skills in affecting inequality? Many have hypothesized that increased income inequality in the United States is driven by an increased price of unobserved skills. Murnane, Levy, and Willet (1995) provide some direct evidence, using cognitive test scores from the National Longitudinal Study of the Class of 1972 data and *High School and Beyond*. Their results show strong effects of mathematics scores on earnings at age 24, even controlling for years of completed schooling. As important, this effect was stronger in 1986, when the price of skill is expected to be higher, than it was in 1978.

These results indicate a strong role for skills in determining income differences. But test scores are surely poor proxies for the array of talents that are valued in labor markets. (The mathematics test measures only concepts taught before eighth grade.) Other tests, or collection of more detailed data on the skills of individual workers, may get us closer to understanding the important determinants of income differentials.

The early literature on income mobility—the movement of individuals between portions of the wage distribution over time—drew a distinction between inequality of income and inequality of lifetime wealth or utility (Lillard and Willis 1978). If poverty is a transitory state, then it is arguably less worrisome as a social problem. This issue has taken on added import with the steady increase in income inequality over the past 25 years, which most economists believe is driven by a change in the “price” of skill. In this context, the question of income mobility can be phrased in different ways. First, is rising inequality simply a spread in the distribution of pay across a relatively fixed distribution of skill? That is, are those who are at the bottom of the wage distribution in 1994 the same people who would have been at the bottom in 1974? Or has the increase in inequality been partially caused by the movement of people from the middle of the income distribution toward the bottom? This type of “mobil-

ity” suggests that previously valuable human capital has become obsolete, as seems to occur when workers are displaced from long-term employment relationships.

The second question is: Once people reach the bottom of the wage distribution, what are their prospects for recovery? If human capital is firm or industry specific, then movements down in the distribution are likely to reflect the obsolescence just mentioned. Then mobility is a one-way street and recovery is unlikely, especially for experienced workers who have lost their previously valuable skills. Poverty is more of an absorbing state in this case, with large effects on lifetime wealth.

These issues have hardly been addressed in the burgeoning literature on wage inequality. (See Gottschalk and Moffitt 1994 for an exception. Topel 1993 contains preliminary calculations.) In part this is because the two main sources of panel data, the PSID and the NLS, are fairly small, so that movements between portions of the wage distribution are difficult to gauge accurately. The only solution is to obtain larger longitudinal data sets. For example, in Sweden it is possible to obtain longitudinal data on tax returns *for the entire population*. That is a data set up to the task of measuring income mobility. Concerns about confidentiality are greater in the United States, so the likelihood of obtaining such data is small. An alternative that could go some of the way there would be to match *individuals* (not households) from one census year to another, or at least to ask about earnings histories in standard cross-sectional surveys. Data like the Displaced Workers Supplements of the January CPS have proved useful in this regard, but they are limited to workers who have lost a job in a five-year window.

2.1.3 Activities of Low-Income Individuals

The low-income “underclass” is one of the most serious social problems of our day. Reported incomes of persons at the bottom of the U.S. income distribution are grindingly low, having fallen by nearly a third since the early 1970s (Juhn, Murphy, and Pierce 1993). Aggregate statistics tell us that declining earning power is associated with rising rates of labor force withdrawal among prime-aged men (Juhn, Murphy, and Topel 1991; Juhn 1992). How do these people survive? Do they rely on families and friends, or are there other sources of income that are not reported in survey data? What role is played by the underground economy?

These questions are important if we are to understand the causes and consequences of poverty, and the workings of low-wage labor markets generally. Yet economists rarely touch these issues. Sociologists have played a much more active role (e.g., Wilson 1987) by collecting their own data instead of relying on government sources alone. Recent efforts to interview individuals in inner city labor markets, part of the NBER’s project on unemployment, are a step in the right direction.

2.1.4 Labor Markets in Developing Countries

The recent resurgence of interest by macroeconomists in economic growth has emphasized the role of human capital as an “engine” of growth (e.g., Lucas 1988). At a descriptive empirical level, the growth accounting exercises of Young (1992, 1994) have stressed the contribution of human capital accumulation to the growth “miracles” of Korea, Singapore, Taiwan, and Hong Kong. In these countries, the labor force has been transformed—over a relatively brief period—from a predominantly rural, unskilled, and agricultural base to being relatively skilled, urban, and industrial. In these countries, productivity and real wages more than tripled in the space of two decades.

What is the role of the labor market in the development process? What forces drive the industrial migration of labor? Does industrial expansion require rising educational attainment? Do changing factor proportions, caused by rising average schooling levels, change relative wages? These are basic and answerable questions about what happens during the growth process. Aside from a few country-specific studies, however, little is known about their answers.

What kinds of data would help? Collecting new data would help us to understand the role of the labor market during future episodes of growth, and so one might wish that governments or international agencies collected data on the model of the American CPS. Many governments do, but there is no centralized agency—say, the World Bank—that serves as a repository for the data. Perhaps greater progress could be made if *existing* data were made available to economic researchers. In Japan and Korea, for example, ministries of labor collect detailed individual data on random samples of employees for all firms with more than 10 workers. While tabulations of these data are published, the raw data—which exist in tape form—are generally not available to outside users. Much can be learned from these data, but they have to be pried from the fingers of bureaucracies.

2.2 Parametric Models: Gauging Behavioral Responses

Most of economics is about incentives and behavioral responses to varying constraints. One of the main functions of an empirical economist is to be knowledgeable about these responses, which may range from simple supply and demand elasticities to the parameters of value functions in models of dynamic optimization. Yet in spite of the increased sophistication of econometric methods, we remain largely ignorant about the magnitudes of even the simplest behavioral responses, such as the elasticities of labor demand and supply for particular types of workers. Our knowledge of other behavioral responses—such as how changes in a firm’s compensation policy affect the effort and performance of employees, or how rising educational wage premiums affect investment in human capital—is weaker still.

Supply and demand responses remain at the center of current debate in labor economics, and they are essential for even the most simple problems of policy evaluation. Consider the following issues that have attracted substantial research and policy attention but remain unresolved.

Immigration and wages. How does an increase in immigration, particularly the immigration of less skilled workers, affect the wages and welfare of natives? A number of papers have found small or negligible effects (Borjas 1987; LaLonde and Topel 1991; Altonji and Card 1991; Card 1990; Hunt 1992). The magnitude of these effects depends inversely on demand elasticities. In spite of negative results from previous research, many economists are convinced that substantial effects are being missed. More recent evidence suggests that the effects of immigration of less skilled Hispanics and Asians to California may have substantially reduced the wages of less skilled natives. Wage inequality increased more in California than in any other region of the country (Topel 1994).

Welfare states and income transfers. In many contexts, the distortionary effects of income transfer programs depend on elasticities of labor supply. The NBER's recent project on Sweden sought to evaluate many of these effects in the context of the world's most aggressive welfare state (Freeman, Topel, and Swedenborg 1997). Has the growth of the public sector affected women's employment? How distortionary is state-provided child care? Have policies that compress the Swedish wage distribution affected incentives to invest in education and human capital? All of these questions depend on supply elasticities, about which there is substantial uncertainty (Aronsson and Walker 1997). Proponents of redistribution policies will argue that the effects are small, while opponents predictably argue that they are large (Lindbeck 1993; Lindbeck et al. 1994).

Education and relative wages. Changes in the educational composition of the workforce shift factor ratios, which change relative wages in inverse proportion to demand elasticities and elasticities of substitution (e.g., Freeman 1976; Welch 1979). The general pattern of these effects has been confirmed in the United States (Murphy and Katz 1992), Sweden (Edin and Holmlund 1995), Korea (Kim and Topel 1995), and Taiwan (Lu 1993). Magnitudes are not consistent across countries, however. For example, growth in the supply of more educated labor was accompanied by a greater reduction in the college wage premium in Sweden than in the United States. Is the elasticity of substitution smaller in Sweden, or were factors other than relative supply acting to narrow the Swedish wage distribution?

Cyclical fluctuations in employment. Much of what is done in economics is a result of our ignorance about behavioral parameters. Nowhere is this more ap-

parent than in macroeconomics, where the labor market plays a central role. Since Keynes, the welfare implications of economic fluctuations have been a central issue in traditional macroeconomics. Keynesians and neo-Keynesians typically treat economic fluctuations as market failures in which labor markets fail to clear. Activist government policies can then be welfare improving. In contrast, “real business cycle” (RBC) models treat economic fluctuations as efficient responses to real shocks. Labor and other markets operate smoothly, and all gains from trade are realized. As one RBC proponent characterized cyclical contractions: “You don’t have to be out of equilibrium to suffer.”

At its core, the Keynesian-RBC debate is founded on divergent views about the validity of the intertemporal substitution hypothesis (ISH) in labor supply. If intertemporal substitution of work effort is “small,” then it is difficult to reconcile the magnitudes of economic fluctuations in employment with the observed behavior of wages and productivity. Labor supply is insufficiently elastic to reconcile large fluctuations in employment with relatively small changes in wages. In contrast, if intertemporal substitution is “large,” then contractions of employment and hours might be market-clearing responses to real productivity shocks. The remarkable state of our empirical knowledge is that these opposing views can coexist, and not just for a short while. In spite of empirical research carried out over a period of *decades*, most economists remain unconvinced by empirical research on the ISH. It is fair to say that many give greater weight to their own priors about the intertemporal elasticity of labor supply, and perhaps properly so.² A final issue serves as a useful example to frame the remaining discussion.

2.2.1 An Example: Policy Evaluation and Employer Mandates

Recent proposals to extend health insurance coverage to the uninsured relied heavily on “employer mandates.” Under these proposals, employers that do not now provide health insurance coverage for their workers would be required to do so. Most economists would recognize that mandated benefits act as an implicit tax on employment that will distort hiring and labor supply decisions. If we concentrate on the market for a particular labor type that does not now receive employer-provided benefits—so benefits have less private value than wages, at the margin—the distortionary effects of the tax are proportional to the induced reduction in employment. This reduction is given by

$$(1) \quad d \ln N = \frac{E_s E_D}{E_s E_D} \cdot \frac{\tau}{w}.$$

2. For reviews of the empirical literature, see Pencavel (1986) and MaCurdy (1985). Pencavel suggests that further research on the topic “should not proceed without some assessment of whether this extraordinary effort and expense will yield sufficiently high returns.” Mulligan (1995) offers evidence from a variety of sources that supports the ISH. His evidence suggests that an intertemporal labor supply elasticity of about 2.0 is consistent with the data events that he studies.

In equation (1), E_D and E_S are the elasticities of demand and supply for the type of labor in question, and τ/w is the implicit tax as a proportion of the wage.³

Either explicitly or implicitly, equation (1) was at the center of policy debates over the effects of employer mandates. Policymakers were interested in how many “jobs” would be lost, and the expertise of labor economists was sought. Supporters of mandates looked for economists who could attest that one or the other of the elasticities in equation (1) is small (zero would do nicely). Opponents sought the opposite view. It should come as no surprise that both opinions were in ample supply. Reputable economists could cite research that would support either small or large effects of this particular policy because the economics profession has achieved no consensus on the parameters of equation (1). What is the state of knowledge about these parameters?

2.2.2 Supply Elasticities

Consider the elasticity of labor supply. No other parameter in all of economics has attracted more research time and money than this one. Potential distortions in equation (1) would be most relevant in markets for less skilled workers, who currently have the lowest health insurance coverage. What value might we assign to E_S for these workers? The most widely cited survey articles on labor supply, by Pencavel (1986) and Heckman and Killingsworth (1986), make no mention of how responses might vary across skill groups. Even so, they provide the most exhaustive reviews of the profession’s state of knowledge about labor supply responses.

For women, Heckman and Killingsworth note that most studies find large and positive labor supply responses, yet “the range of estimates of the uncompensated elasticity of annual hours is dauntingly large.”⁴ The studies they review report uncompensated supply elasticities ranging from -0.30 up to $+14.0$. What they call a “reasonable guesstimate” of the elasticity of women’s labor supply is probably positive, but it has a huge standard error (much larger than reported in any of the cited papers). A value of $E_S = 0.5$ might be reasonable (e.g., Hausman 1981), but elasticities well above 2.0 (e.g., Heckman 1976, 1980) are just as likely on the basis of current evidence. If we wish to apply equation (1) to women, then, our “guesstimate” might be off by a factor of 5 or more.⁵

Econometric estimates of labor supply elasticities for men have a much smaller range. At the end of an extraordinarily careful review of the literature on male labor supply, Pencavel (1986) concludes that “the vast proportion of

3. If $m < 1$ is the value of \$1 in insurance benefits to workers, then $\tau = 1 - m$. Then equation (1) says that there is no distortion if workers are indifferent between mandated benefits and wages.

4. The uncompensated elasticity is appropriate here because real incomes are not held constant for the group in question.

5. Rosen (1997) applies a value of $E_S = 2.0$ in his welfare calculations for Sweden, yielding large distortions from child care subsidies.

[empirical work on men's labor supply]—both that based on the static model and that based on the life-cycle model—indicates that the elasticities of hours of work with respect to wages are very small. In other words, the focus of most economists' research has been on behavioral responses that for men appear to be of a relatively small order of magnitude." Indeed, in Pencavel's review, 19 of 22 reported estimates of the uncompensated elasticity of labor supply from nonexperimental data are *negative*, while the largest estimate from eight studies based on negative income tax experiments is 0.2. The average estimated elasticity over all cited studies is -0.08 . If we take this as a "consensus" estimate of the elasticity of male labor supply, the evidence is that time worked is completely unresponsive to changes in wages. For men, the existing literature suggests that the employment change in equation (1) is likely to be negligible.

I do not think there is consensus, however. Economists' objections to the canonical model of labor supply, applied to cross-sectional data, are numerous and well known. (Most workers are engaged in long-term employment relationships, where the current wage is not a summary statistic for the terms of trade. Wages are measured with substantial error in the microdata sets used to estimate labor supply, and valid instrumental variables are hard to come by. And so on.) In contrast to the evidence produced by the labor supply literature, my guess is that most economists *believe* that the true elasticity of male labor supply is positive, at least among men who earn low wages. That is, most economists think that the policies underlying equation (1) will have some distortionary effect.

In support of this view, figure 2.1 shows the relationship between weeks worked per year and hourly wages of prime-aged men for the 20-year period 1970–89. The data are from the March CPS, as described in Juhn et al. (1991). The figure shows that those who earn more typically work more too. In fact, the curve looks suspiciously like the labor supply curves we are accustomed to drawing in class. Of course, the displayed relationship between wages and weeks worked does not mean that a reduction in the wages of low-wage workers would cause them to work less. In fact, the "consensus" estimates from the male labor supply literature predict that their hours would remain roughly unchanged.

An experiment of this type has occurred in the United States during the past 25 years. Widening inequality has reduced the real wages of workers at the bottom of the wage distribution by as much as 30 percent since 1970. This widely documented change in real wages is surely demand driven (Katz and Murphy 1992; Bound and Johnson 1992), which allows us to test the prediction that working time will not fall with wages. Figure 2.2 compares the distributions of changes in real wages and changes in annual time worked across deciles of the wage distribution, based on calculations in Juhn et al. (1991). The figure demonstrates that both dimensions of nonwork—nonparticipation and unemployment—increased over this period. More important, declining em-

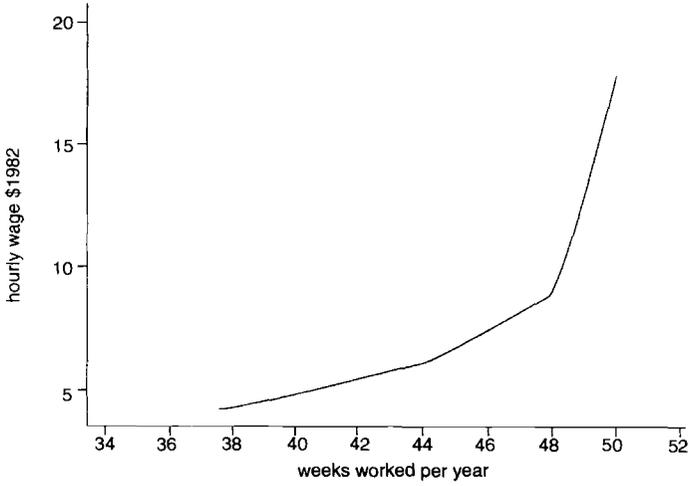


Figure 2.1 Relationship between hourly wages and weeks worked for prime-aged men, 1970–89

Source: Calculated from March Current Population Survey files, 1968–90. See Juhn, Murphy, and Topel (1991) for a description of the data.

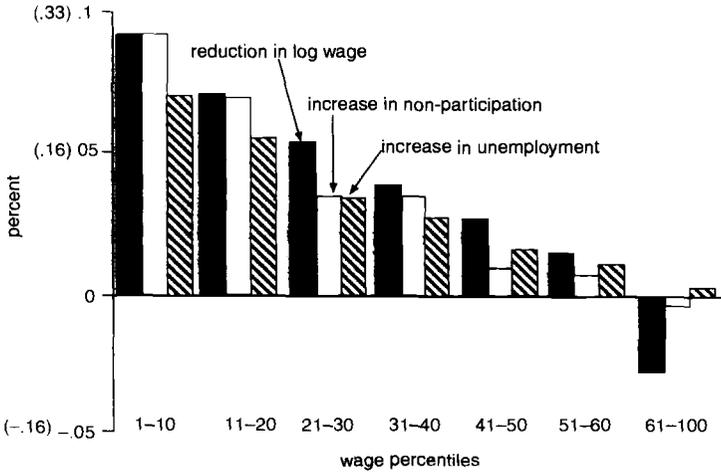


Fig. 2.2 Changes in log wages, unemployment, and nonparticipation by wage percentile, 1967–69 to 1987–89

Table 2.1 Elasticities of Labor Supply and Changes in Weeks Worked across Percentiles of Wage Distribution

	Percentiles of Wage Distribution				
	1-10	11-20	21-40	41-60	61-100
Elasticity of supply					
From figure 2.1	.42	.29	.21	.15	.07
From regional variation ^a	.49	.31	.17	.07	.08
	(4.31)	(4.53)	(3.56)	(1.60)	(2.02)
Change in weeks worked, 1972-89					
Actual	4.83	2.54	1.66	.41	.10
Predicted from supply elasticity	4.58	2.96	1.71	.78	.05

Note: See text and figure 2.1.

^aNumbers in parentheses are *t*-ratios.

ployment was concentrated among workers whose earning power was also falling. Among workers above the median of the wage distribution there was no change in labor supply behavior.

These calculations suggest that labor supply did respond to changes in wages over this period. To test this idea, we used regional data on wages and employment rates to estimate

$$(2) \quad e_{irt} = A_r + B_r + C_i w_{irt} + \varepsilon_{irt}.$$

In equation (2), e_{irt} is the employment rate of workers from interval i of the wage distribution in region r and year t , and w_{irt} is the average log wage for that group. Estimated uncompensated “labor supply elasticities” from equation (2) are shown in table 2.1. I also show the elasticities implied for each interval from the curve in figure 2.1. The correspondence is fairly remarkable: actual changes in working time, generated by time-series changes within regional labor markets, are not much different from what the cross-sectional relationship of wages to work would imply. These estimates also do well in predicting the distribution of changes in labor supply across intervals of the wage distribution, as shown at the bottom of table 2.1.

The “labor supply elasticities” shown in table 2.1 are far above the consensus estimates one might draw from the existing literature. For the workers at the bottom of the distribution our estimates imply an elasticity of weeks worked with respect to wages in excess of 0.40. It is worth noting that much of this effect is driven by labor force withdrawals among unskilled men, whose wages fell. This margin is often ignored in male labor supply studies, but it is surely relevant for our example, or any other welfare calculation.

Does this evidence mean that male labor supply really *is* responsive to changes in wages? Perhaps not. Perhaps other factors also changed over this period, and they coincidentally caused men with declining wages to work less. But the conceptual experiment—do people work less when wages fall?—

is surely appealing as identifying leverage for estimating labor supply responses. At the least, this evidence makes me unsure of any prediction based on small estimated elasticities of supply, especially as it may apply to low-wage markets.

Labor supply responses play a central role in policy evaluation, yet labor supply studies seem to have gone out of vogue in empirical labor economics. Most studies that underlie our working knowledge of labor supply are based on a small number of data sets that were collected in the 1960s and early 1970s. Labor markets have changed dramatically since then, and most would argue that changing relative demands for different worker types have been the major driving force. These forces provide a potentially useful environment in which to study labor supply. Given the importance of these issues, and the weak state of our current knowledge, I believe that a reassessment is warranted.

2.2.3 Demand Elasticities

The best source of evidence on labor demand is Hamermesh (1993). His survey of studies that estimate the effect of wages on employment concludes that the output-constant, long-run elasticity of labor demand (i) is bounded by zero and 1.0 for most firms, with a likely confidence interval from 0.15 to 0.75, and (ii) is larger among less skilled workers than more skilled ones. We might inflate these bounds a bit to reflect scale effects and additional opportunities for substitution at the market level of aggregation, and larger elasticities for the less skilled. If applied to equation (1), this range of estimates might yield a range of several hundred thousand jobs.

But even Hamermesh's rather wide confidence interval is probably optimistic. Studies of inverse demand systems—where factor supplies are allowed to affect wages—typically find only small effects. For example, Welch (1979) and Berger (1989) find elasticities of wages with respect to cohort size that are smaller than 0.2. LaLonde and Topel (1991), Card (1990), and Hunt (1992) find negligible effects of increases in the supply of immigrants on the wages of other immigrants and of similarly skilled natives. The implication of these studies is that marketwide, own-price demand elasticities for less skilled labor are fairly large, vastly above Hamermesh's upper bound of 0.75 for direct estimates. At the other extreme, recent studies of the impact of minimum wages in less skilled markets attempt to make the case that the own-price elasticity of demand for low-wage labor is virtually zero (Card and Krueger 1992).

Unlike the literature on labor supply, most of the demand studies just cited did not have the goal of estimating a demand elasticity *per se*. Instead, they are case studies that apply a labor demand framework to a particular problem. They provide useful answers to the problem at hand—did immigration have a discernible effect on natives' wages?—but they are wildly inconsistent with each other, at least within the context of a labor demand model. The results cannot be generalized in any obvious way. Indeed, the fact that we undertake so many case studies is evidence of our uncertainty about (a) the model itself

when applied in any particular context and (b) the parameters of the model, even when appropriately applied. The combination of these leaves me with an extraordinarily wide confidence interval for demand elasticities in low-wage markets. A value of 0.3 does not seem unreasonable, but neither does 1.3.

2.2.4 Data Needs for Parametric Models

My assessment of the state of our knowledge about supply and demand elasticities in the labor market is pessimistic, but I do not think excessively so. Even less is known about other important behavioral effects. How do skill differentials in wages affect human capital investment decisions? Does performance pay improve work effort? How does geographic mobility respond to interarea wage differentials? Theory and common sense tell us the sign of these effects, and perhaps some values that would be utterly unreasonable, but that is about all.

What can be done to obtain better estimates of behavioral effects? In the absence of experimental data, the obvious answer is to study real-world conceptual experiments in which the identifying assumptions of econometric models are convincingly satisfied. There is nothing new in that statement. But the cause of our uncertainty about key parameters is that such conceptual experiments are few and far between, not that economists have ignored them when they occur. The identification problem is difficult enough in the case of descriptive models that estimate, say, the returns to schooling. It is much harder in behavioral models that try to isolate the adjustments of agents to differences or changes in market equilibria. Uncertainty about behavioral effects may be the nature of things.

More data can only help if we believe that there are real-world experiments for which too little information is now collected. Most empirical economists can think of plausible examples where this is the case. Is it important to understand whether incentive pay affects employee performance, which is the foundation of agency literature on employment relations? Then we need to observe cases where performance is measurable and firms changed their compensation policies. Examples like this one are obvious. A long list could be drawn up. Beyond them, I cannot offer concrete proposals or an agenda for data collection that will reduce our uncertainty about behavioral effects in the labor market.

2.3 Conclusions

Empirical research in labor economics has been remarkably successful in describing labor market outcomes. For the most part, this knowledge has been fueled by the availability of large-scale microdata sets that are publicly collected or funded. These data have vastly improved our understanding of how labor markets work and the magnitudes of social problems, and they have

greatly influenced the direction of economic research (Stafford 1986). Use of these data also points to feasible areas where more information is needed, and where the payoffs in terms of hard empirical knowledge are likely to be large. I described above some areas that I think are important.

Empirical research has been less successful in calibrating economic behavior. Despite substantial efforts to estimate behavioral responses, our confidence intervals for these effects remain embarrassingly large. For policy evaluations that depend on these effects, it is often the case that the state of our empirical knowledge hardly crosses the threshold of being useful. In part, this is the nature of the beast. Real-world conceptual experiments that allow us to isolate relevant parameters are rare, and even when they occur it is not clear that the results are easily generalizable to other contexts and problems. It is hard to be optimistic that progress will come quickly on these issues.

There is irony in this. The power of economic analysis comes from its ability to model human behavior in a systematic way. Economics has something to say about behavior, but empirical economics is a long way from accurately measuring it.

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Comment Frank P. Stafford

Several years ago at a conference of this nature Al Rees made a remark along the lines of the Mark Twain quote. He said that labor economics was different from other fields in economics and from the rest of the social sciences. Labor economists know what it is they don't know. If anything, this remark has become more applicable in recent years. Labor economists have led the way in published work addressing the issues of measurement and data quality. Some of this has been disheartening. In one sense we can say that we know what we don't know, and every day we add to our knowledge!

Based on numerous studies there is the sense that the gap between perceived ex ante data quality and actual data quality has often been large. For example, the research interest in fixed effects models has shown up the extent to which wage measures based on respondent reports of hours and earnings are subject to large doses of measurement error (Björklund 1989; Hamermesh 1989). On the other hand, some studies have shown that external records from an employer can be extremely useful (Stafford and Sundström 1996). Even if such external records are not available, we can proceed as long as the character of the data errors is known.

The main thesis in Topel's paper is that data and estimation and the development of economic theory *should* be interactive. Theory shapes the questions to be addressed empirically, but theory is stimulated by observing puzzles, either in the data or in casual observation. Labor economics has become distinct from

other fields in economics by the extent to which the empirical side of the ledger is given attention, undue attention in the minds of some of our colleagues in fields such as industrial organization or economic theory per se. Only in labor economics are studies that are “purely descriptive” deemed to be professionally respectable, or at least unlikely to bring on a revocation of tenure! In fact, however, much of the descriptive work is motivated by the discovery that facts implied by prevailing theory and facts turned up empirically are often difficult to reconcile.

A classic example of data leading theory has to be the apparent wage slowdown and decline in the per capita earning power or real wage in the United States during the past 25 years. Either our theories are bad or our data are bad, or both! Wages should not stall out when investment (broadly defined to include human and physical components) increases, trade expands rapidly, and significant new information technology pervades the society! As pointed out in the paper on divergent trends in alternative real wage series (Abraham, Spletzer, and Stewart, chap. 8 in this volume), a good part of the problem seems to be that hours of market work are not well measured either by employer reports of hours paid for or by respondent reports of hours of market work in household surveys.

The latter problem of household survey estimates of market hours is underscored by comparisons with data from time diaries. Quite an extensive set of methodological studies show that diaries provide unbiased estimates of market work and other activities and are designed by construction to “add up” to the constraint of 24 hours per day. It has long been known that time diaries show a stronger trend toward reduced hours of market work in postwar industrial economies, in comparison to such trends estimated from conventional hours reports by respondents. This can be shown for the United States and Japan over the period 1965–80 (Stafford and Duncan 1985). A recent methodological comparison suggests that for adult men in the United States, weekly hours of work beyond 40 from respondent reports are virtually all exaggeration (Robinson and Bostrom 1994). If proper measures of real wage growth (based on hours estimates from diaries) were to show somewhat greater growth, there would be more credibility to our prevailing theories as well as an opportunity to support or refute the innovative new variants of growth theory.

To what extent is it possible to use even ideally error-free data to test hypotheses and estimate theoretically relevant relationships? How often does nature give us the equivalent of experiments? These concerns have motivated the era of social experiments (Hausman and Wise 1985). The impact of these experiments on the research community has been quite small, at least judging from the extent to which they led to published papers, even during the era when the data were collected (Manser, chap. 1 in this volume). Instead, the use of nonexperimental data has come to the fore with microdata sets, notably the NLS, PSID, and CPS, having a remarkably high share of the research publications in labor economics. Further, the research uses of the NLS and the PSID

are branching out into related social sciences (in the area of intergenerational mobility, child development, and early human capital formation) and into macroeconomic areas (based on income dynamics and asset accumulation).

Microdata sets have been applied in “natural experiments” across different market economies (Kim and Topel 1995; Blank 1994) and have turned up not so much clean tests of the “experimental” variety but much deeper insight into the variety of ways in which market economies and their related social institutions accomplish economic functions. On the experimental side, there have been some interesting “tests” too. The recent tax reform in Sweden has apparently had only small short-run effects on labor supply. Panel respondents were placed into three groups: substantially lower taxes, substantially higher taxes, and no significant change in taxes. Preliminary results indicate no significant differences in behavior between the lower tax and higher tax groups (Klevmarken 1994). Perhaps a difference will emerge through time, or perhaps the prior tax reforms had already muted the incentive effects of tax changes in Sweden. In any event this is an interesting case to study since many of the tax changes can be regarded as income compensated. After-tax wage rates rose, but “fixed” taxes on fuel and other items were raised to preserve overall macroeconomic budget balance.

Before becoming too pessimistic about what can be learned, we should reflect occasionally on our successes. Application of microdata has had some dramatic successes both within labor economics and in other fields. The first big success story with microdata was in the area of consumer behavior. The theories of the permanent income hypothesis and the life cycle hypothesis were tested, as well as the simple Keynesian consumption function. My impression is that the work in that area has been remarkably successful. In the area of the permanent income hypothesis, use of microeconomic panel data established the robustness of both the short-run consumption function and the longer run function, including disaggregation of family income into various components (Holbrook and Stafford 1971).

The life cycle consumption hypothesis has not been such an apparent success, but it is harder to test since it implies smoothing over a much longer time period. This longer time period gives rise to much more complex issues of income and asset uncertainty, information, and changing family and household composition through time. Work with the PSID has shown that liquidity constraints lead younger workers to desire *more hours of work* in light of a wage decline rather than fewer (Dau-Schmidt 1984). If so, the life cycle consumption hypothesis should be merged with labor supply under uncertainty and could be useful in explaining the inability of wage changes to clear the labor market entirely when demand declines: if constrained workers seek more hours (and recent data show how little most workers, even in preretirement years, have in the way of liquid assets), then when labor demand shifts inward, labor supply shifts outward, placing an extreme burden on wages as the sole clearing mechanism, particularly in light of our theories of labor contracting.

The discussion by Topel on the lack of clear results from the studies of labor supply elasticities is illuminating if disheartening. To a range of theoretical concerns about the excessive simplicity of the modeling as it applies to individuals (who may be working under a long-term arrangement) one must add a host of data problems. Progress seems possible if the right "experiment" comes along. One pointed to is the sharp fall in the wage of less skilled workers in the past two decades. There is some evidence from the CPS that these workers have responded by *increasing* their hours of work (Bosworth and Burtless 1992). In contrast, other evidence (Juhn, Topel, and Murphy 1991) suggests that these workers have responded by *reducing* their hours of work.

It is anxiety creating to have well-known empirical researchers reporting such different results. A closer look suggests that the differences likely stem, at least partly, from alternative conceptual approaches. In the Bosworth-Burtless paper, the analysis adjusts for cyclical unemployment and looks at (increasing) hours supplied as the wages of less skilled workers have declined through time. The implication is that income effects have induced more (desired) hours. The Juhn-Topel-Murphy paper treats long-term shifts into unemployment and out of the labor force as part of labor supply and implies that hours of market work have declined via substitution effects. The matter here is heavily one of theoretical approach to analysis of the data rather than data gaps or problems. Other areas of labor supply research are clouded by both data problems and differences in conceptualization.

If we define success by a better consensus concerning the empirical regularities highlighted by theory, a leading candidate for the designation of "success" has to be the life cycle human capital theory. It seems to me that while this is not the whole wage story, the basic elements are supported and appear to be better supported the better the measures are. For example, virtually any disaggregation of work history into work experience of different types of spells out of the labor force "works" in the sense that earnings variation is better explained. The success of these partial equilibrium models may not carry over to studies that consider the overall labor market as a set of interconnected markets. This demand side of the market and a topic discussed by Topel, internal labor markets, will require that far better data be available on the employer side of the market, for one thing.

Recent progress in merging establishment data with individual data (Abowd and Kramarz, chap. 10 in this volume) appears promising. On the other hand, "employment units" are inherently more difficult to survey than "household units." Individuals can be followed, and they attach to new families and firms. Large firms change quite a lot, and small firms are always changing. It is also hard to know who is actually making decisions in a firm, so that verbal corroboration of the rationale for some critical observed behavior is not available to the extent it is in households. On the other hand, I draw a fairly strong conclusion from the results reported by Marilyn Manser (chap. 1 in this volume). The establishment of an interactive process to receive input from the larger research

community is important for success in the design and use of such a database. Large projects directed from within government agencies without research community involvement and guidance from conceptual models are unlikely to contribute much to our knowledge.

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