LABOR DEMAND: WHAT DO WE KNOW? WHAT DON'T WE KNOW?

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ABSTRACT

This study reviews empirical research on the demand for

labor.   The static analysis discusses the production parameters

describing homogeneous labor and labor disaggregated along

various criteria; the distinction between workers and hours; the

importance of job dynamics; and the nature of a variety of

policies that affect the long-run demand for labor.   Issues in

the dynamics of demand for workers and hours, including speeds of

adjustment for homogeneous and heterogeneous labor, asymmetries,

and the nature of adjustment costs are presented.   The paper

emphasizes how the paucity of appropriate data has limited our

ability to obtain reliable estimates of the underlying concepts.

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I. Labor Demand -- The Cinderella of Labor Economics

Labor demand is one of the two basic areas that begin most undergraduate and graduate courses in labor economics. Its central intellectual focus includes: 1) Factor substitution, of labor for other inputs, between types of workers, and between workers and hours; 2) The roles of shocks to factor prices and product demand in determining employment; 3) Factor prices and the creation and destruction of jobs; 4) Cyclical changes in factor productivity and the role of labor hoarding and lags in demand; 5) The impact on employment of alternative forms of ownership and industrial organization, including labor-managed firms, nonprofit organizations, household enterprises, and others; 6) The effects of such policies as minimum wages, payroll taxes, employment subsidies, job-security legislation, and immigration restrictions on employment and wages.

Despite its basic position in our courses, the study of the demand for labor has been the intellectual stepchild of labor economics. The static theory of labor demand has been adopted from the theory of production. The only addition that labor economists have made is to distinguish between hours and workers. The dynamic theory of labor demand is no different from firms' forward-looking demand for any other input characterized by costs of adjustment.

The relative absence of basic theoretical developments does not greatly distinguish labor demand from labor supply. What is different is the absence of profoundly original methodologies to address the basic questions about labor demand, and the remarkable underdevelopment of data that are suitable to answering those questions. Instead, we have relied almost exclusively on methods carried over from empirical studies of investment and of production generally. Much more than in the analysis of labor supply our data have been highly

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aggregated, or we have microeconomic household data that are better designed for answering questions about labor supply.

One bit of evidence for the neglect of labor demand by mainstream labor economists is a recent monograph on empirical labor economics that is divided into "halves" dealing with supply and demand (Devine and Kiefer, 1991). The second "half" takes up 14 pages of the 300-page book! A more detailed consideration of the relative importance we attach to the two areas measures the number of contributions included under the two rubrics in the Handbook of Labor Economics (Ashenfelter and Layard, 1986). Seven syntheses are included under the heading labor supply: Labor supply of men, and of women; demographics of marriage and fertility, and household production; retirement; the demand for education; and one study of data used to examine labor supply. Under labor demand two syntheses are included, one on statics, the other on dynamics. Even if we exclude the demand for education as having become part of the separate subfield of the economics of education, it is clear from this summary that there is more interest in supply, and presumably more research to be synthesized.

The proportions of research on the two topics published in leading journals illustrates the same point. Stafford (1986) summarized the direction of research in labor economics by subfield from 1965 through 1983. His results and my updating of them for 1984-90 are shown in Table 1. The recent data do provide a hint that the relative extent of professional interest in the two areas has at least changed somewhat.

The relative neglect of the subject does not mean that our occasional theorizing and the massive, though often inappropriate empirical work have gotten us nowhere. I believe that a careful and thorough reading of the literature of labor demand leads us to a number of conclusions. Many of these are as secure

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as any that economists who are concerned about measurement or about testing theories are ever likely to reach.

II. The Static Demand for Labor

One of the most important things we should have learned in studying static demand is to be careful in our terminology. For example, the phrase "the elasticity of labor demand" has been used in print in recent years to refer to the long-run demand for labor (with all other inputs variable) and to the short-run demand (with at least capital fixed) too. The same term, without modification, has been used interchangeably to discuss the substitution elasticity of labor against other factors, and the combined substitution and scale elasticities (with output varying). Clearly these concepts differ. It is possible that readers are capable of inferring from the context which definition the authors mean, but I doubt it.

The same difficulty arises in the use of the terms "substitutes" and "complements" to denote pairs of inputs. While most of us are accustomed to using these terms to discuss the impact of exogenous shocks to the price of Factor A on the amount of Factor B demanded, the terms are increasingly used to refer to the dual problem. This makes it important to distinguish between p- and q-substitutes (and complements), that is, whether substitution is initiated by the factor price or quantity varying exogenously. That A and B are p-substitutes tells us nothing about whether they are q-substitutes or complements.[1] Without being precise about this issue we cannot address, for examples, such important questions in factor substitution as the impact of immigration on a labor market, or the effect of capital deepening or capital subsidies on the wages and/or employment of different types of workers.

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Table 1

Articles in Major Economics Journals, by Subject, 1965-90[a]

YEARS

SUBJECT:	1965-69	1970-74	1975-79	1980-83	1984-90
Labor Demand					
Basic labor demand	10	11	11	10	19
Adjustment and dynamic demand	6	4	6	10	14
Minimum wage	1	2	4	4	4
Labor Supply					
Population size and structure	7	14	19	10	13
Household production	0	11	8	8	11
Labor supply of men	2	5	7	6	9
Labor supply of women	0	3	9	6	7
Labor supply of others and income support disincentives	2	7	16	15	14
Retirement	0	1	4	1	6
Educational demand	3	11	9	2	6
Migration	13	9	14	4	9

[a]Columns (1)-(4) are from Stafford (1986). Column (5) is tabulated using the same six journals counted by Stafford, the American Economic Review, Econometrica, International Economic Review, Journal of Political Economy, Quarterly Journal of Economics, and Review of Economics and Statistics. Only articles are included; notes and comments are not.

These are not merely semantic fine points. If we take measurement and use of labor demand seriously in our study of labor markets, we cannot bandy about the same word in several contexts and hope to impart or gain understanding. The least we owe the readers of our empirical work and the consumers of conclusions about policy based on our research is a clear understanding of the particular bit of economic theory in which our analysis is grounded.

With these and other distinctions in mind I have examined the now immense empirical literature that measures various concepts of labor-demand elasticities, substitution between types of workers, and between workers and hours. There are over 100 studies that focus just on the demand for homogeneous labor. They cover most developed and many less developed economies. These allow one to infer either σ, the elasticity of substitution between capital and labor, or η_{LL}, the long-run constant-output demand elasticity for labor. A few others focus on the total elasticity of demand. Whether that is a short- or long-run elasticity is a question in macroeconomics (or perhaps theology). I do not want to examine it here, other than to note that the empirical work makes it clear that shocks to output account for more short-run variation in employment than do shocks to input prices.

I classify the literature on static labor demand according to five distinctions. Is the study based on: 1) Time-series, cross-section, or panel data; 2) Household, or establishment data; 3) Large industries or even entire economies, small industries, or individual firms or establishments as units of observation; 4) Data for the United States, or another economy, and 5) Simple production functions, generalized approximations, systems of demand equations, or others?

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Two trends are apparent in research styles in this literature. One is the increasing sophistication of functional form that has allowed researchers to estimate production relations involving labor and several other inputs without restrictions. The less well-known is the growth of studies based on sets of microeconomic establishment data. Such data allow the researcher to circumvent the aggregation problems that are so blithely ignored in most empirical work that purports to deduce microeconomic parameters from macroeconomic data.

Like any other body of empirical research this literature has produced a wide range of estimates. Nonetheless, weeding out those studies that do not generate estimates of <u>any</u> concept of a demand elasticity that can be related to theory, the overwhelming majority of estimates of η_{LL} fall within the range [-0.15, -0.75]. <u>A "best guess" for the long-run constant-output labor-demand elasticity based on this literature is -0.30</u>.

Interestingly, with a value of labor's share in developed economies of around 0.70, this "best-guess" value of η_{LL} is roughly consistent with Cobb-Douglas production (since the implied value of $\sigma = \eta_{LL}/[1-s_L]$ is -1). The literature that estimates the constant-output demand elasticity for labor that is summarized here has truly led us "to arrive where we started and know the place for the first time."[2] If one is willing to ignore problems of aggregation, the estimates based on both micro and aggregate data suggest the Cobb-Douglas function is a satisfactory way of describing aggregate production based on capital and labor.

I doubt that there is much useful left to be learned about this one particular (though very important) parameter. It's not that we know everything, but that there has been so much work that the gain to additional research is small. The same cannot be said for most other issues in static labor demand.

The only other point that we know with a high degree of certainty is that labor and energy are p-substitutes, with a very small cross-price elasticity. This inference is based on more than the wide array of highly sophisticated studies using the data on capital, labor, energy and materials assembled for U.S. manufacturing by Berndt and Wood (1975). Other sets of data on manufacturing outside the U.S., and for individual industries such as trucking, electric power generation, and iron and steel, confirm it. This conclusion allows us to predict how any future shocks to energy prices will affect labor demand and eventually labor productivity.

A wide variety of studies have used combinations of the same five approaches to data and estimation to generate parameters describing substitution among heterogeneous workers. They allow us to infer that: 1) Capital and skill are probably p-complements, while capital and skill are p-substitutes with unskilled labor; 2) Skill and technical progress are p-complements; 3) Own-wage demand elasticities decrease with the amount of skill embodied in a group of workers, and 4) Elasticities of complementarity between migrants and natives are small, implying that immigration of sizes observed in developed economies in the last 40 years has had fairly small impacts on relative wages.

The problem with all these conclusions is that there has not been enough careful research to make them as firm as the conclusion about η_{LL}. Questions about substitutability involving heterogeneous labor are inherently more difficult. (If nothing else, there are more questions!) I doubt, though, that we will get very far in increasing the certainty of our conclusions on them using the approaches of most existing studies. At least one of three problems plagues nearly all of them: 1) The data stem from household surveys and do not reflect firms' behavior; 2) Units of observation are at best small industries, and often

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are time series of large aggregates; 3) There is no justification for treating quantity or price as exogenous.

The first two problems are solvable with better <u>microeconomic establishment</u> data. Some efforts have been made along these lines in small individual projects on substitution among heterogeneous labor in establishments within a particular industry (e.g., Jensen and Morrisey, 1986; Field, 1988); but there has been little use of a broad-based set of microeconomic establishment data for this purpose.

There are two ways to solve the third problem. The difficult, and usually unconvincing way is to model supply and demand simultaneously or to instrument price or quantity variables with supply-side determinants. This approach imposes the additional task of constructing a satisfactory model of multifactor supply, including relative supply decisions except in those few cases where the categorization of workers is almost fixed by nature (as with race or sex). A better choice is to find cases where wage or quantity shocks are truly exogenous. A good example of this (that unfortunately used household data) is Card's (1990) study of the impact of the 1980 Cuban immigration on the labor market in Miami. A natural experiment circumvents problems of endogeneity.

Research on substitution between employment and hours has proceeded in such a variety of ways as to preclude strong conclusions. At this point we can only tentatively conclude that: 1) <u>Employers do substitute hours for workers when their relative prices change</u>, including the price of an hour of overtime work and the relative wages of part- and full-time workers; 2) <u>Both hours and workers are p-substitutes for capital</u>, and 3) <u>Reducing the standard workweek reduces hours</u>, but on a less than one-for-one basis. It increases employment, but reduces total worker-hours.

How changes in the price of hours of one group of workers affect the demand for other employees has not even been examined. Nor has substitution among weekly hours, days per week, and employment been studied. Thus the basic empirical research necessary to estimate the potential impacts on wages and employment of such policies as mandating employer-paid benefits for part-time workers, or providing paid parental leave, has not even begun.

All of these literatures rest on the well-developed static theory of production <u>in a continuously-operating, infinitely-lived firm</u>. Businesses do not live forever, though. They are born; some grow; almost all eventually die. This process has attracted increasing attention in the theory of industrial organization (e.g., Jovanovic, 1982). In the past ten years it has also led labor economists and others to compare flows of jobs in continuously-operating firms or plants to those occurring as plants are born or die, i.e., to study <u>job dynamics</u>.

The research thus far has ranged from manufacturing to all industries, from individual states in the U.S. to the entire economies of the U.S., Belgium, Canada, France, Germany, Indonesia, Italy and Sweden. The research has not yet examined behavioral relationships. Instead, it has essentially involved counting flows, measuring the identity:

(1) $$\Delta E = B + G - D - C ,$$

over some interval of time. Each term in (1) denotes a flow of employment in a firm or establishment resulting respectively from Births, Growth, Deaths and Contractions, and ΔE is the net change in employment.

A rough guess at these magnitudes can be obtained by averaging the available estimates, even though the coverage and methods differ. The estimates come from the eleven studies, of the United States and six other economies, that

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measure all four gross flows. (Some research only measures B+G and D+C.) **Based** on the summary in Hamermesh (1993, Table 4.2), we can replace the symbols in (1) with these averages as:

(1')   1.0 = 3.5 + 6.0 - 3.0 - 5.5 ,

where these are annual percentage flows of jobs, and all but two of the underlying studies measured flows each year. A fair conclusion from this research is that existing plants account for roughly two-thirds of gross flows of jobs. This suggests that it is not completely wild to base studies of labor demand that use employment levels on the theory of an infinitely-lived firm.

That one-third of the annual flow of jobs results from plant or firm entry or exit should be at least a bit disturbing, though. It means that for much of any net change in employment there is no link between the elasticities we measure and any commonly-accepted underlying theory of production. A solution is to construct theories based on what I call quasi-demand elasticities for labor through plant openings and closings that yield estimable parameters. So far very little effort has been devoted to measuring these necessary alternatives.[3] Without research on how factor prices affect gross flows of jobs we cannot be sure that a particular shock will not produce sharply different responses from what production theory implies, because the shock operates on the margin of plants opening or closing.

The absence of research in this area also means that we know very little about how exogenous changes in factor prices affect the location of plants. There are many studies of how interregional differences in factor prices alter factor intensity; there are almost none that use the appropriate longitudinal data to examine how changes in factor prices affect employment through job dynamics.[4] Research in the general area of job dynamics should be very fruitful

in the next decade, especially if it moves beyond counting employment outcomes to studying the behavior that determines them.

Policies that affect long-run labor demand are too diverse to be summarized anywhere. A better approach is to classify them so that the basic empirical research I have discussed can be used along with a few archetypal policies to indicate the probable general impacts of any current or future policy. A useful schema for classifying policies that affect long-run labor demand distinguishes them by whether they are: 1) P- or Q-policies --- is the government intervention on wages or other factor prices, or on the quantity of labor or other inputs? 2) General or specific --- aimed toward all workers, or disproportionately toward some group of workers? and 3) On labor, employment-hours or nonlabor --- intervention in the labor market that is neutral on employment- hours choices, that is not neutral, or that is on other factor markets.

P-general policies include flat-rate payroll taxes and subsidies as labor policies, investment tax credits as nonlabor policies, and premium pay for overtime as an employment-hours policy. P-specific policies include payroll taxes/subsidies with low ceilings and requirements for payment by "comparable worth." Q-general policies include changes in standard hours legislation (an employment-hours policy) and immigration policy (a labor policy). Affirmative-action policy is Q-specific, and immigration policy may be too if it selects workers nonrandomly by ethnicity or skill level. The main point is that this taxonomy is broad enough to include any policy that affects the long-run demand for labor. It allows the economist who is interested in labor-market policy to use prior knowledge about a particular example in one of the 12 categories to structure the analysis of any other policy in that category.

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It is impossible to summarize even a few illustrations within these categories; but it is worthwhile going over one well-studied policy, legislated minimum wages, to demonstrate what we know and how the approach might be useful. An imperfectly enforced minimum wage can be viewed as a P-specific tax on employment below the minimum. In most cases the tax is an increasing percentage of the discrepancy between the worker's marginal product and the nominal minimum (because enforcement is more stringent the further below the minimum is the worker's wage).

Brown et al (1982) summarized studies of the effect of legislated minima on employment in the U.S. There have been only a few studies for the mainland U.S. since then, but a number of empirical analyses of minimum wages elsewhere, including Canada, France, the United Kingdom and Puerto Rico, have been conducted.

A common thread in the results of this research is that it is wrong to apply an extraneous estimate of any long-run labor-demand elasticity to infer the impact of minimum wages on employment. The elasticity combines with the degree of truncation of the distribution of wages that the minimum wage produces to generate the specific outcome, an employment-minimum wage elasticity. The elasticity is large when the trunction is far to the right in the wage distribution, as when the United States mainland minimum wage was applied to Puerto Rico (Santiago, 1989, and Castillo-Freeman and Freeman, 1991). It is small when the minimum wage "tax" applies to very few workers, as in the United States in the late 1980s (Wellington, 1991). The elasticity is an outcome that varies with an unchanging technology. It is not a parameter of a production function, and we cannot expect it to be independent of the size of the "bite" of the minimum wage tax into the distribution of wages.

III.   The Dynamic Demand for Labor

Compared to static factor demand there has been a looser link between the theory of dynamic factor demand, based on dynamic optimization in the presence of adjustment costs, and the estimation. The theory is very elegant; but only rarely (and recently) has the empirical work provided estimates of any of the parameters that the theory generates.

Labor economists have perhaps rightly paid little attention to this literature, other than to view it as an application of the theory of fixed costs of employment. That is unfortunate, for the literature is now sufficiently well developed to allow us to make some inferences about how fixed costs affect dynamics and how the effect varies over time and by demographic group. We are also beginning to learn something about the structure of the costs that generate dynamic behavior and are approaching the point where we can link what we have learned to policies that can be modeled as altering those costs.

In one form or another the end result of most research in this large empirical literature has been an estimate of the adjustment path of the demand for employment or worker-hours between equilibria.   Best-practice technique (Nickell, 1984; Shapiro, 1986) now pays attention to expectations about forcing variables, so that the effect of adjustment costs can be isolated. For various reasons, including problems of both spatial and temporal aggregation, the only strong conclusions are: 1) The demand for worker-hours adjusts fairly rapidly to shocks to expectations about product demand or factor prices, with the half-life of the lag being around six months, and 2) The costs of adjusting employment exceed those of adjusting hours.

All sorts of changes will no doubt continue to be rung on these themes using the same highly aggregated data, including the specification of more

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aesthetically pleasing models of expections. I would argue, though, that the social returns to such research are already diminishing rapidly (even though the private returns are regrettably large). Further research on these general issues does not seem profitable.

In other areas enough empirical work has been produced to allow some tentative conclusions. These are that: 1) The rate at which the demand for homogenous labor is adjusted is not greatly affected by disequilibria in employers' demands for other factors. The dynamic p-substitution or complementarity of homogeneous labor with other inputs (nonzero off-diagonal terms in a matrix of adjustment coefficients) is not very important, and 2) The dynamic response of employment is asymmetric; adjustment is less rapid in response to positive shocks and when unemployment is lower. What we do not know here is why these empirical regularities arise. Does the average cost of adjustment differ depending on whether employment is increasing or decreasing? Do the structures of the costs of upward and downward adjustments differ? Is it something else?

Unlike in the study of static demand, the problem is not one of trying to distinguish demand from supply effects or of finding appropriate natural experiments. All the evidence suggests that dynamics are much more rapid on the demand side. We can treat supply as fixed and assume that the path of employment demand does not bump up against the constraint that the fixed labor supply imposes.

The problem here is the almost complete lack of effort to link the estimation of employment dynamics to any underlying structures of adjustment costs. We assume quadratic costs for mathematical convenience; we then estimate models on aggregate data that are so spatially aggregated that structural

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inference is impossible. All five of the attempts to examine the assumption of quadratic costs suggest it is wrong (for example, Hamermesh, 1989). Whether this error affects inferences about real-world adjustment in aggregates is unclear; but it is a central question that needs more work. Without it we cannot be sure that the results of clever attempts to test for asymmetry or to measure the size of adjustment costs using aggregate time series (e.g., Burgess and Dolado, 1989) are anything more than artifacts of aggregation.

Assuming that the lengths of adjustment lags are not biased because the studies ignore problems of excessive aggregation and the weak ties between theory and estimation, roughly 15 studies have examined how these lags differ across countries, over time and by skill. Only two conclusions seem fairly secure from this literature: 1) Adjustment speeds decrease with the skill of the workers, and 2) Employment adjusts more quickly in the United States than in most other developed economies.

Why these common results arise is, of course, the interesting question. We love to attribute changes in outcomes in time series to changes in the institutions that we are studying. Along with others, such as Nickell (1979), I (1988) am guilty of that sin. We also enjoy making international comparisons of parameter estimates, attributing differences to institutional differences that concern us (Abraham and Houseman, 1989). Even though the comparisons may give the correct inferences, they cannot be very convincing. These are hardly controlled experiments that allow us to focus on carefully isolated treatment effects.

Only a careful specification of a particular institutional change and of the way it affects the costs of adjusting labor demand can be the basis for believable estimates of the effect of the institution. Good examples in the

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literature on job security policy are Houseman's (1991) study of job protection in the European steel industry and Dertouzos and Karoly's (1990) examination of the effects of limiting employment-at-will using a state-industry panel in the United States. Neither of these parameterizes the policies' impact on the labor market, but both offer much more than just before-after or international comparisons.

The evaluation of static policies is convincing because the basic empirical research is linked to theory, and much of the specific policy evaluation is linked to both. That this has not occurred in the evaluation of dynamic labor-demand policies is due to the absence of these links. No neat schema like the one I have outlined for static policies is possible here, because the linkages between theory, estimation and policy do not exist.

We are unlikely to advance both basic knowledge and policy research unless we obtain and use the kind of microeconomic establishment data that have helped to secure our knowledge about the parameters describing static labor demand. The task is harder here, for frequent observations on a panel of establishments must be obtained to make the estimation of dynamics possible. Without such data, though, we cannot make the structural inferences that our theories allow. Without them and a serious microeconometric foundation for macroeconometrics, much of the basic empirical research will degenerate still further into displays of pyrotechnic time-series estimation on the same tired macroeconomic time series. Without them and an effort to analyze policies such as employment protection, financing unemployment insurance benefits, and others in terms of their effects on adjustment costs, policy evaluation in this area will continue to offer little more than series of case studies and macroeconomic before-after comparisons.

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IV.   Where Do We Go From Here?

The study of labor demand may be the intellectual Cinderella of our subdiscipline, the specialty that is least likely to marry the Prince. But in terms of what it has taught us it is certainly no uglier than its more favored stepsisters. (Convince yourself of this by attempting to construct an outline like this one for your own favorite major line of research in labor economics.)

The vast empirical literature has given a convincing answer on one central issue, the long-run elasticity of demand for homogeneous labor. In other areas too we know more than we did even 15 years ago. This includes a feel for the sizes of various substitution parameters, the nature of employment-hours substitution, job dynamics, paths of employment adjustment and how they vary by skill and across countries, and the structure of adjustment costs.

What we do not know much about is the microeconomic structure of labor demand (other than the demand for homogeneous labor). Our theories are based solely on the behavior of the firm, but most of our inferences rest on highly aggregated data. One reason for this lack of knowledge is the paucity of microeconomic establishment data. That has resulted in much of the best research in the area being done on a "retail" basis, with the researchers scrounging for micro data. Some countries have begun to remedy the problem by assembling detailed panels of establishments. These include the Longitudinal Research Database in the U.S. (which unfortunately is limited to manufacturing) and the Danish IDA (Integreret Database for Arbejdsmarkedsforskning), a complete annual file beginning in 1980 of all establishments. Germany is undertaking a similar project. In the U.K. and Australia the Workplace Industrial Relations Surveys could form the bases for longitudinal files if they are extended.

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Despite this progress and the growing interest in labor demand and recognition of its importance, the data resources still lag far behind those available for research on household behavior, including the study of labor supply and search by workers. Even in the new sets of data, we do not observe agents frequently enough to hope to infer very much about the dynamics of labor demand. Also, because they offer no link to detailed demographic and household data on employees, they offer no possibility of learning anything about the demand for heterogeneous labor, which we saw is the area where progress is most needed.

Half of the papers that I referee finish with a tag line like, "More research on this topic is necessary." That is true in the study of the demand for labor. But more research based on inappropriate or overused data will be sterile. More and better data are the necessary underpinning for more research. That is especially true in the United States, where we seem to be behind many other nations in our development of establishment-based data sets that are useful for research.

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FOOTNOTES

1. Sato and Koizumi (1973) made this distinction and originated the terminology.

2. T.S. Eliot, _Little Gidding_, Section V.

3. Hamermesh (1988) and Berger and Garen (1990) are the only exceptions.

4. But see Carlton (1979) and Papke (1991).