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## ABSTRACT

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# H. Gregg Lewis: Perhaps the Father of Modern Labor Economics<sup>1</sup>

H. Gregg Lewis did fundamental research outlining the economic effects of trade unions and considering how to measure them carefully. He also laid out the theory of the supply and demand for labor in careful detail that has underlain economists' thinking about these outcomes. Aside from innovating modern-style research in labor economics, his work provided an exemplar of care in thinking about and measuring economic phenomena. His study of labor markets foreshadowed numerous subsequent fundamental articles, including our theories of hedonic prices and of wage selectivity. Supervising numerous Chicago Ph.D. dissertations, all of which heavily bore his stamp and two of which were by future Nobel Prize winners, he contributed indirectly to the development of applied microeconomics through several later generations of researchers.

**JEL Classification:** B21, C29

**Keywords:** economics of unions, labor demand, labor supply, Chicago dissertations, general equilibrium, empirical methods

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## I. Introduction

Gregg Lewis was born in 1914 and spent most of his adult life, from his entry as an undergraduate until age 61, at the University of Chicago, except for leave time during World War II. He moved to Duke University in 1975 and spent the final ten years of his academic career there (for which see McElroy, 1994), passing away at age 77 in 1992. He received few honors from the economics profession during his career beyond the approbation and esteem of his colleagues, students (including as an undergraduate this author) and others who knew him. Shockingly, he was never elected a Fellow of the Econometric Society, but he was named a Distinguished Fellow of the American Economic Association in 1981. Post-mortem he was honored by the nascent Society of Labor Economists with the creation of the biennial H. G. Lewis Prize for the best article published in the *Journal of Labor Economics*.

He was part of “the Chicago School,” yet not in the way that most younger economists today and certainly most laypeople interpret that designation as describing research that has a certain political bent. Rather, he followed and expanded upon a long tradition at Chicago of using economic theory to derive and crucially then to test predictions. His lasting contribution, more than any of the specific works that I discuss here, was to create a tradition among labor economists of theory and testing that has, I would argue, made it the queen of sub-specialties in applied economics. As I will show, without knowing it many applied microeconomists today are using the same methods, although with different names, that Gregg expanded upon and used nearly 70 years ago.

Much of Lewis’s research concentrated on the impacts of trade unions, a topic in which economists, especially in the United States, have been uninterested over the past quarter century. For that reason, by the bibliometric measures that we use to evaluate scholarly impact his work has had remarkably little **direct** influence. As Table 1, based on Google Scholar citations shows, only Becker and Lewis (1973) on quality-quantity trade-offs in fertility has been acknowledged very heavily by other researchers. His biggest contributions, the University of Chicago Press books (Lewis, 1963, and Lewis, 1986a, summarized by

Lewis, 1986b), have had some, but not a very large direct impact. His other work has been essentially ignored for the past 30 years.<sup>2</sup>

This paucity of acknowledgments by other scholars in recent years is mirrored in what I believe to be the remarkable lack of awareness among labor economists today of Lewis's work and even of his name. To examine this belief, I sent a very short survey (questions listed in the Appendix) to Research Fellows and Affiliates of the Institute for the Study of Labor (IZA), 1707 labor economists located in universities and research organizations around the world. The purpose was to generate data to estimate the regression:

$$(1) Y = b_0 + b_1\text{EARLY} + b_2\text{NA} + b_3\text{EARLY}\cdot\text{NA} ,$$

where EARLY is a Ph.D. degree before 1995, NA is a North American degree, and I expect that the parameters  $b_1$ ,  $b_2$  and  $b_3 > 0$ .<sup>3</sup> The outcomes Y are whether a respondent "Knew the name 'H. Gregg Lewis'" (using a probit estimator) and, conditional on that knowledge and based on responses to an open-ended question, whether the person identified his main work as being on the economics of unionism, or answered "unions" or something like "pioneer of labor economics" or "teacher of important economists."

The survey elicited 822 responses, a rate of 48 percent, with all but 6 respondents providing complete information. Of economists who provided complete responses 28 percent had received their doctorates before 1995, and 46 percent had North American degrees. Only 40 percent stated that they knew Lewis's name; of those, 34 percent identified him with work on unionism; an additional 12 percent noted that he was a founder or pioneer of modern labor economics, or that he supervised numerous distinguished economists' dissertations.<sup>4</sup>

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<sup>2</sup>The Web of Science (WoS) gives an even starker picture, since that compilation typically excludes books, thus ignoring Lewis's two books on union wage impacts. Total citations to his work through mid-June 2020 in the WoS were 1013, of which 848 were to Becker and Lewis (1973).

<sup>3</sup>The likelihood function varied little among specifications with the EARLY indicator based on a Ph.D. year in the interval [1993, 2000], dropping off sharply outside this range. For simplicity I have chosen 1995 as the cut-off.

<sup>4</sup>Not all those coded  $Y = 1$  on "Knew the name" really recognized it. One very distinguished labor economist responded, "he was an athlete, running I think," and subsequently emailed me noting sheepishly that he was thinking of Carl Lewis when he answered. Carl was at his time the fastest human in the world. Gregg was not the fastest producer of scholarly research, more than making up in quality what was lacking in quantity. Two respondents knew him as a leading African-American economist, and five others identified him with development economics, including a Nobel Prize. I assume that these people confused him with W. Arthur Lewis—less confusion than with Carl Lewis.

Columns (1) and (2) of Table 2 present the results of estimating (1) over “Knew the name,” with  $b_3$  first constrained to zero and then estimated freely. Older labor economists—Ph.D. degree before 1995, probably at least in their early 50s, are much more familiar with the name than are more recent Ph.D. recipients. The same is true for economists who studied in North America; and the difference by Ph.D. vintage is greater among economists educated in the U.S. than elsewhere. Among respondents who recognized the name, more senior economists were more likely to identify him with research on unions and/or knew that he was a pioneer in modern labor economics (with two respondents using the sub-titular phrase of this article).<sup>5</sup>

*Sic transit gloria mundi.* And yet Lewis’s work is still highly influential and deserves much more recognition than it apparently currently receives. In what follows I thus summarize his most important research, focusing most heavily on his studies of union behavior and impacts, moving to his contributions to the theories of labor demand and supply (broadly defined), then to one other study, thus covering all of his “Top 10” listed in Table 1. I illustrate how the methodology that he helped to develop in this work underlay the “credibility revolution,” which has dominated labor economics in the past quarter century (Biddle and Hamermesh, 2017). I show that his work was concerned both with measurement and the underlying economic behavior that generates the measurements. His scholarship provides a model for the way economists ought to be doing applied research; and as such it deserves much more attention than the results in Table 2 suggest that it receives among applied researchers today.

## **II. Trade Unions and Their Impacts**

Much of Gregg’s work dealt with studies of the effects of trade unions, with a concentration on the measuring their impacts on wages. Most of Lewis (1963) and all of Lewis (1986a) deal with empirical issues in measuring this impact, trying to reconcile them to draw consistent inferences. More important than the estimates of the effects, however, is his concentration on the economics determining those effects that must be considered in the estimation.

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<sup>5</sup>Estimates of these equations including the interaction term did not add to the models’ explanatory power.

### A. *Theoretical Underpinning*<sup>6</sup>

This discussion is contained in Lewis (1963, Ch. 2), a chapter whose opacity is extreme but whose value is even greater. At the risk of modernizing Lewis's thinking to today's ubiquitous causation-based language, consider unionization as a treatment (admittedly non-randomly applied), with the goal being to measure the impact of the treatment. Imagine an economy consisting of 3 groups, A, B and C, with the focus on wages (compensation) of Groups A (fully unionized, later only partly unionized) and B (not unionized) and with observation of wages of  $W_A$  and  $W_B$ . Group C is another group of workers whose wages are not being compared to those of Groups A and B.

Assume that all observables among all three groups have been accounted for, so that we are focusing on the impacts of unionization alone. (Of course, this assumption is extremely restrictive, and a necessary empirical trick is to adjust for a convincing set of observables to isolate the impact) The relative wage at some time  $t$  is  $R_t = W_{At}/W_{Bt}$ . Assume that we can observe workers in the same two groups at some time  $t = 0$  when none of Group A workers was unionized, and measure  $R_0 = W_{A0}/W_{B0}$ . Then:

$$(2) R' = R_t/R_0,$$

is the union relative wage effect so long as: 1) All workers in Group A are unionized, and none in Group B are; 2) There is no effect of unions in industries/occupations employing Group A workers on wages of Group B workers; and 3) There is no effect on wages of Group C workers. If and only if all three caveats hold,  $R'$  measures perfectly the desired concept. In modern language  $R'$  is the average treatment effect (ATE) of the unionization of Group A workers under these conditions.

Considering the three caveats in the previous paragraph, the first may be easy: If only some of Group A workers are unionized and we can measure both their wages and those of non-unionized Group A workers, we can measure the LATE of unionism. This is not simply a measurement issue: If, as seems likely, unionization of some Group A workers affects the wages of non-unionized Group A workers, the

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<sup>6</sup>See Freeman (1994) for another discussion of Lewis's work on union relative wage effects.

latter's wages are affected by their fellow workers' union status. The second caveat is more difficult to handle: Unless: 1) There is no long-run supply response of workers choosing to enter the occupation(s) in Group A compared to Group B—union coverage is assumed exogenous (a restriction that was relaxed empirically and convincingly by Farber, 1983); and 2) Production using Group A and B workers is described by fixed coefficients,  $W_B$  will be affected by the unionization of (at least some) Group A workers. The extent to which these considerations will bias the estimate depends on both the extent of unionization of Group A workers and the elasticities of relative supply and demand between the two Groups.

The severity of the difficulties in measurement caused the third caveat also depends on the extent to which Group C workers are substitutable in production and/or in supply for Group A and/or Group B workers. If they are completely independent—cross-elasticities of both demand (including for the perhaps different products that are produced using Group A, Group B and C labor) and relative supply, no problem; but it is very unlikely that such independence is universal.<sup>7</sup> Without that independence the effects on Group C workers will cause  $R'$  to mis-estimate the impact of unionization on the wages of Group A workers relative to those of Group B workers.

Lewis identifies one more difficulty that, at least implicitly, goes beyond these impacts: A **threat effect**, where non-union employers, even ignoring issues of substitution or endogenous union coverage, alter wages to avoid having their workers unionize. They will presumably do this only if they believe that a unionized work force would reduce the total (pecuniary **and** non-pecuniary) benefits that they derive from the firm that they own (or lead). I interpret Lewis's discussion of this effect as being independent of the effects of substitution in supply and demand that might alter wages in both Groups A and B, although extricating these effects empirically is not easy.

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<sup>7</sup>Lewis implicitly assumed a closed economy, which made sense for the U.S. in 1960, when exports plus imports accounted for 11 percent of GDP. In 2020, when this sum exceeds 25 percent of GDP, this assumption is no longer tenable. This poses difficulties, since the content of imports will be affected by domestic union-induced cost changes, and the number of migrants and their skills (or lack thereof) will change depending on where unionization occurs domestically and its effects on wages—of both union and non-union workers.

While the discussion is based on the relative substitutability (complementarity) of different types of labor, it is imprecise—there is no explicit analysis of the magnitudes of substitution parameters that would bias estimates up or down. This would require modeling unrestrictive multi-factor production functions—and before the early 1960s we did not have the tools to do that. Remember, even the two-factor CES function, allowing the elasticity of substitution to differ from its value of unity in the Cobb-Douglas function, was only produced by Arrow *et al.* (1961).

At a time when private-sector unionization in the U.S. is almost non-existent, and unions are in decline throughout the industrialized world, why should anyone care about this extremely complex discussion (even as greatly simplified here)? The reason is that Lewis’s work provides a simulacrum for the impact evaluation of any government program or any shock to which employers and workers can react. Even where no supply response is possible or is very difficult, for example, looking at racial/ethnic differences in some outcomes, unless we make the extremely restrictive assumption that, given other observables, workers are perfectly substitutable in production, the same general equilibrium confounds that concerned Lewis are present in the contemporary evaluation literature. They are generally ignored; but they should not be, and Lewis provides the appropriate framework for thinking about and trying to account for them.

Without going into all the details/caveats from the 1963 book, Lewis (1986a, Ch. 2) added some very useful, and clarifying discussion to his earlier thoughts, linking his ideas to the by-then burgeoning micro-based empirical studies on the effects of unions. The central equation (Lewis, 1986a, p. 11) is:

$$(3) \ln W = a_n + a_{nX}X + a_{nY}Y + U[(a_u - a_n) + (a_{uX} - a_{nX})X + (a_{uY} - a_{nY})Y] ,$$

where  $W$  is a wage (or measure of compensation),  $X$  is a vector of controls,  $Y$  is a measure of the extent of unionization in the occupation (industry, geographic area, or whatever) in which the worker is classified,  $U$  is an indicator equaling 1 if the worker belongs to a union (or is covered by a union contract), the  $a_j$  are parameters to be estimated, and for convenience I ignore the disturbance term. Equation (2) essentially estimates the wage as a function of the control variables, unionization in a group (typically occupation or industry) and individuals’ union status, including full interactions of union status with  $X$  and  $Y$ .

Lewis defines the union/nonunion *relative wage gap* as:

$$(4) M = (a_u - a_n) + (a_{uX} - a_{nX})X + (a_{uY} - a_{nY})Y,$$

i.e., the difference between the union and non-union wage for otherwise identical workers facing the same extent of unionization in the group.  $M \neq 0$  because unionized workers may earn more (given X and Y) and because the differential returns to workers' characteristics and the extent of unionization may differ between unionized workers and others. He then defines the *relative wage gain* as the union/nonunion relative wage compared to the absence of unionism anywhere (essentially equivalent to treating the worker's union status and that of his/her occupation, etc. as two separate exogenous experiments imposed on the economy). Clearly, the wage gap is measurable, ignoring issues of defining the relevant groups on which to measure Y. The wage gain does not seem measurable absent an experimental framework.

#### B. *He DiD It*

Most of both Lewis (1963) and Lewis (1986a) summarize, synthesize and, most important, analyze huge numbers of results in empirical studies of the effects of unions on wages/compensation. Many of the studies in the earlier volume were masters or doctoral dissertations which were produced "under a faculty committee of which I [Lewis] was a member" (e.g., Lewis, (1963, page 57, fn. 17) and on which his influence appears pervasive. While the studies used different methodologies, four industry studies stand out: Irvin Sobel (1951) on rubber; Rush Greenslade (1952) on bituminous coal; Elton Rayack (1957) on men's clothing; and Leonard Rapping (1961) on merchant seamen.<sup>8</sup>

All four studies were produced before even mainframe much less personal computers were available, and when even canned regression packages were unavailable. Each was based on data on some occupation in cities/areas divided into union (U assumed equaling 1) or non-union (U = 0) at time  $t_1$ , when the former group was already unionized, and  $t_0$  when neither group was unionized, and either compared

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<sup>8</sup>To save space I do not list these in the References, as they are all referenced fully by Lewis (1963). Their dates are included to indicate their vintages.

averages of wages across the two types of city or compared individual city/area averages between times  $t_0$  and  $t_1$ . Implicitly these studies estimated the parameters  $a$  and  $b$  in the regressions:

$$(3') \quad \ln W_{it} = a + bU_{it}, \quad i = \text{city}, \quad t = t_0, t_1,$$

a tightly constrained version of (3). This is equivalent to estimating:

$$(3'') \quad \ln W_{it} = \beta_0 + \beta_1 U_{it} + \beta_2 \text{POST}_t + \beta_3 U_{it} \text{POST}_{it}, \quad i = \text{city},$$

where  $\text{POST} = 0$  if  $t = t_0$ , 1 if  $t = t_1$ .

Every contemporary student of applied economics should recognize (3''): It is the pervasive difference-in-difference (DiD) equation that is included in so much contemporary research. Lewis used this methodology and ensured that his students in the late 1940s and 1950s did so too, a novelty at a time when econometrics was dominated by the structural methods of the Cowles Commission. His students were unable, given extant data and computing technology, to estimate multiple regressions that might include large vectors of controls  $X$ .<sup>9</sup> Recognizing this difficulty, in his analyses of these studies Lewis (1963) discusses various possible correlated differences between workers in different locations in these narrowly defined occupations to infer how they might be biasing the estimated  $\beta_3$  in (3'').

Between Lewis (1963) and the 1980s a huge literature estimating relative wage gaps using either averages from industry, occupation or area wage surveys, or publicly available micro datasets, such as the CPS, PSID or the National Longitudinal Surveys, grew up. He refers to estimates based on the former group as macro estimates, those based on the latter as micro estimates. Lewis (1986a) summarizes this literature but not in the standard, basically non-analytical way of meta-analyses.<sup>10</sup> Nor did he estimate a few of what he might have viewed as his own best-practice regressions.

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<sup>9</sup>I assume that they were constrained to use math tables to convert raw wage data to logarithms, and to calculate the relevant own- and cross-sums of squares on mechanical desk calculators. This was state-of-the-art in empirical work in those Dark Ages of empirical research in economics.

<sup>10</sup>Additional, although less complete summaries are provided in Lewis (1983), the lead article in the first issue of the *Journal of Labor Economics*, and in Lewis (1986b).

In producing the 1986 volume he instead obtained many of the original and “massaged” data sets from their authors, tried to put their estimates into a common framework and, where he could not infer exactly what had been done, queried the authors about their research (all in pre-email days).<sup>11</sup> With this abundance of studies based on “modern” regression techniques, he synthesized what we had learned about wage gaps by race, industry, firm/establishment size and numerous other disaggregations of the labor force. One very distinguished labor economist, when asked why Lewis did this, responded that he just was not very smart. Aside from the sheer nastiness of this response, it is substantively incorrect: One learns far more from synthesizing and analyzing the results of large numbers of studies of some parameter or effect based on a large variety of data sets and methods than from a home-produced small set of regressions, no matter how convinced one might be of their importance and originality.

The macro estimates are less interesting, and Lewis (1986) spends less time analyzing them, focusing instead on summarizing the micro estimates and, more important for implications for today’s research on other topics, discussing issues of inference from such data. First, he notes in panel estimation on micro data that there may be a positive correlation between person fixed effects and the variable of interest—union status. This problem of changing non-observables (the assumption of parallel trends) is ubiquitous (and too often ignored) in many studies and is only demonstrably vitiated when a relevant experiment can be designed (not possible in the case of unionization and so many other phenomena of interest). He also notes that reported changes in union status (or in some other forcing variable X in other contexts) will include errors, leading to classical measurement error in the crucial independent variable and the resulting under-estimation of its impact. Both these points stand as warnings to researchers considering the impact of any self-reported forcing variable on any outcome of interest.

With the rise of estimates based on micro datasets Lewis (1986) was able to infer union wage gaps for different demographic groups, a topic that he alluded to in Lewis (1963) but, given available data, was not able to deal with satisfactorily at that time. Among the important conclusions are: 1) Union relative

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<sup>11</sup>Having been queried several times about my three studies that he summarized, his tenacity in understanding different methodologies that was so apparent in Lewis (1963) was yet again impressed upon me.

wage gaps are larger among African-American than white workers; and 2) The relative wage gap by workers' age is U-shaped with a minimum at or slightly beyond prime working age. Both results are consistent with the equalizing effects of unions on wages. Of special interest here is the discussion of estimates of  $[a_{uY} - a_{nY}]$  in (4). Lewis's inferences suggest that, once adjustments are made for differences in method, the myriad estimates in the literature yield no consistent conclusion. The nice thing to note from these synthesizing chapters is that the reduced but still substantial amount of research post-1986 on these issues has not altered our conclusions about them—a tribute to the care that Lewis took in discussing the then-extant literature.

In both books one of the “bottom lines” was the attempt to provide an estimate of the economywide average union relative wage gap—a statistic summarizing the impact of unionism on the structure of pay. The best estimate—a 15 percent gap on average—is part of the intellectual heritage of American labor economists. But this is just an average; and in both volumes Lewis was also concerned with its cyclical variation (which he attributes in part to longer-term union wage contracts) and to temporal variations in it as the extent of unionization changed. The former is clear—more rapid inflation lowers the average gap. It is more difficult to generalize about the latter, as threat effects probably increase with the extent of unionization, biasing the estimated gap downwards, while increases in unions' strength as more workers are organized and bargain collectively raise the estimated gap.

Lewis's final published research (1990) continued his work on the union relative wage impact by considering the subject in the context of public employment, especially relevant since by that date the unionization rate in the public sector had already overtaken that in the private sector. The care in the analysis of what was already a large array of empirical studies is like that of Lewis (1986). The novelty was the detailed consideration of a variety of threat effects—particularly relevant in the public sector where many non-union cities explicitly base pay on comparable unionized wages (achieve “parity”), which makes calculating even a relative wage gap difficult. Laying out the necessary conditions for the appropriate counterfactual, an occupation/area/group of workers whose wages are not based on parity with those of unionized workers, he calculates how this consideration alters standard estimates of the gap. Throughout

he makes it absolutely clear that even this re-calculation does not yield the desired estimate of a relative wage gain, thus closing out his final publication by harking back to what was by then his nearly 30 years of published research on union wage effects (and his 40 years of supervising students' work on the subject).

### *C. Unions and Wage Inequality*

In both volumes Lewis concludes with a discussion of the impact of unions on wage inequality. We want to measure the change in the standard deviation of log wages across individuals arising from unionization (compared to what it would be in the absence of unions):

$$(5) \Delta\sigma_{ij} = \Delta\sigma_i + \sum s_i \Delta\sigma_j,$$

where  $i$  is an industry (or other unit),  $j$  is an individual in that unit, and  $s$  is the weight of that unit in employment (or total compensation) economywide. The answer to this relative wage gain question is not knowable with existing data; but even the change in the dispersion of the relative wage gap is not obtainable from macro estimates, since they *ipso facto* ignore any within-unit change in inequality induced by unionization. Lewis (1963, Ch. 9), however, argues that the second term in (5) is probably smaller than the first, so he infers that unionization probably increased overall inequality slightly.

Even with micro data the decomposition in (5) is difficult to calculate, as it requires knowing the extent of unionization, the relative wage gaps, and how these are correlated. Inferring how the dispersion in the relative wage gains to unionization relates to inequality is not possible in the real world—the counterfactual cannot be estimated. Once again, as in his discussion of individual relative wage gaps, Lewis in both books underscores the importance of considering the appropriate base case—what we would observe absent unionization anywhere.

### *D. The Theory of Unions, and a Normative Approach*

In two other articles Lewis thought deeply about the economic determinants of trade union behavior and success (impacts on wages and working conditions). Lewis (1959) distinguished between competitive unions, those operating in competitive product markets and with workers free to move among firms and join or not join unions; and monopoly unions, those in monopolized industries (in firms with only imperfect product substitutes) and which can control employment opportunities. The theory of equilibria in the former

is a remarkable *tour de force* of applied economic theory. The outcome is an equilibrium resulting from the distribution of (potential) workers' tastes for being unionized and (potential) employers' distastes for having their plants unionized. The idea of the equilibrium being determined by a **distribution** of tastes is exactly that underlying Becker's (1957) roughly contemporaneous theory of racial discrimination based on employers' preferences (a **one-sided** determination of the equilibrium). (This and other of Becker's work is discussed by Teixeira (this volume). That the equilibrium depends on the distributions of tastes on **both sides** of the market is a rudimentary version of Lewis (1968 and 1969) which underlay Rosen's (1974) more thoroughly spelled out fundamental discussion of hedonic outcomes.<sup>12</sup>

Lewis recognizes that competitive unionization is not very interesting, as it is not likely to yield the positive relative wage gaps whose existence he and his students had already demonstrated. He thus discusses monopoly unions, concluding that their economic effects (as examined at much greater length in Lewis, 1963) depend on the relevant product, labor and labor-labor demand and substitution elasticities, as well as on supply elasticities to different firms. He also spells out the importance of these underlying parameters in determining the distribution of unionization across industries and occupations.

Lewis (1951), although a serious discussion of the economic impacts of a policy idea, is despite its title fairly viewed as his only published work that is classifiable as normative economics. Clearly influenced by the 1946 passage of the Taft-Hartley Act and union activity thereafter, he argues for a simple proposal: Limit collective bargaining coverage to a single firm—one union, one firm—essentially allowing only what my grandfather derisively called “company unions.” Lewis points out that competition among firms, each of which bargains collectively with its own union, would obviate the need for any restrictions on strike activity, on featherbedding or other “bad” union practices, since product-market competition would eliminate these outcomes in equilibrium. The economic analysis is correct and clear. Also clear is the implicit political viewpoint: “... *complaints* by employers against the “one-sidedness” of the Wagner Act

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<sup>12</sup>About Rosen's work, see McLaughlin, this volume.

only to replace it with *shouting* by union spokesmen that the Taft-Hartley Act is a "slave-labor law" [italics mine]" (Lewis, 1951, p. 287).

### III. Labor Demand and Supply

Lewis also published articles in these two basic areas of labor economics. Particularly important (and regrettably nearly unheralded) is Lewis (1969), published in Spanish but basically a translation of an earlier undated discussion paper (Lewis, 1968—I assume). The article asks what happens to outcomes (wages and employment) if we relax the assumptions in standard labor demand and supply models that: 1) Hours per worker (H) are equally productive to the firm independent of their number. In other words, in the context of a two-factor production-function  $F(L(NH), K)$ , where N is the number of workers and K is the capital stock, what if:

$$(6) \partial L / \partial H \neq N,$$

perhaps because of worker fatigue or because absenteeism reduces labor productivity;<sup>13</sup> and 2) There are fixed costs of employment per worker.

The latter question has been studied many times, stems from Oi (1962) and underlies the substantial amount of work on the impacts of overtime penalties (Ehrenberg, 1971). The former has received much less attention, although empirical research has examined how hourly wages vary with H (e.g., Biddle and Zarkin, 1989). Relaxing assumptions on both sides of the labor market, Lewis derives a general theory of the determination of equilibrium wages and hours per worker. He then demonstrates how the equilibrium changes with increased fixed costs of employment, increasing rates of fatigue, and, most interestingly, in response to the imposition of an effective minimum wage rate and/or restrictions on overtime hours (the central provisions of the American Fair Labor Standards Act of 1938). The crucial point here is that, beyond the well-known impacts on employment of the former and on hours per worker and employment of the latter, thinking about the two-sided nature of the labor market means that wage rates in equilibrium are also

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<sup>13</sup>While an important extension, even the assumption about production is highly restrictive, as it constrains hours and workers to be perfect substitutes in L.

altered by such laws; and the changes are jointly determined. With few exceptions this point has regrettably not inspired empirical research (but see Trejo, 1991).

Louis Court, a University of Chicago mathematician, and Gregg Lewis co-authored a paper (1942-43) which essentially laid out the duality between production and cost functions. Noting that the relationship is the analog to the duality of consumers' utility and living costs, the article ground through the mathematics that demonstrates this relationship. As most students of economics know, this fundamental idea was laid out by Shephard (1953) and is basic in production theory. This study essentially did the same thing, but without the beauty of Shephard's Lemma, which summarized the duality succinctly; and with its extremely dense mathematics, it is not surprising that the article has received so little attention.

Lewis's first contribution to the study of labor supply was his 1957 article, in which he puzzled over long-run rises in real wages and property income simultaneous with the decline in average work hours. This study is most well-known for its description of a model in which the individual's utility function yields income effects that exceed substitution effects that produce the observed outcome. This discussion underlay Mincer's (1962) path-breaking study of female labor supply. But Lewis's article did much more than laying out this now-standard model. 1) It considered how the price of leisure varies over the life cycle, noting that the productivity effects of aging and the declining gains to work experience it might reduce this price among older people. It uses this observation to rationalize the bunching of leisure in old age; and 2) It asked why people do not mix work and leisure during the day, attributing this phenomenon to the desire to minimize travel costs. This is a clever observation—noting the fixed costs of labor supply; but here I think the implicit assumption that employers are indifferent about the timing of work is incorrect. This is at least partly a demand-side phenomenon

The notes in Lewis (1972) ask: Can one relate estimates of elasticities from equations describing the determinants of (nonzero) hours of work to those describing the determinants of labor-force participation using a single utility function? Lewis's concern was stimulated by the then-skyrocketing focus on using microeconomic data to estimate these equations and an interest in seeing how the empirical work hangs together. (In that sense these Notes follow a similar style, albeit much more strongly grounded in

standard consumer theory, than the discussion in Lewis, 1963, Ch. 2.) He shows that partial elasticities of responses of hours and participation to some forcing variable, perhaps unearned income, do not imply the same things about any underlying parameter of the representative individual's utility function. Even ignoring the specifics of these notes, they are one of the most useful expositions of the theory of labor supply, highly recommend reading for use in a Ph.D. labor class (assuming the instructor is interested in presenting any theory).

More important, pathbreaking and extremely prescient are: 1) The recognition that the appropriate specifications of both equations must depend on selection based on the distribution of individuals' reservation wages. While he does not use the term Mill's Ratio, he recognizes how the estimates depend on the means of truncated distributions, and he even gives examples under the assumptions of normal or triangular distributions. Much of the economic idea in Gronau (1974) and Heckman (1974) is foreshadowed here.

These notes (page 19) also point out the pitfalls of estimating equations describing hours using a "wage rate" calculated by dividing earnings (weekly, monthly or whatever) by hours (measured over the same time period), noting how measurement error in the latter will induce negative biases in the estimated hours-"wage" relationship (thus foreshadowing Borjas, 1980). They also emphasize the non-observability of the market wage rates of non-participants in the labor force, a difficulty that all of us trying to estimate hours equations around that time knew existed and that too was solved later by Heckman (1979).

Lewis's comments (1974) on the pathbreaking Gronau (1974) discussion of selectivity in wage equations present both critiques and extensions. Of the former, the most important is the point that the underlying distributions of wage offers that potential participants face while searching cannot be viewed as exogenous. Rather, they depend on the searcher's prior experience and, more important, on the equilibration of wage rates generated by the actions of all searchers in a market. Not much can be done empirically with this fundamental point, but it makes us realize that a quick-fix selection correction does not vitiate problems of endogeneity. Of the latter, Lewis points out that the solution would be complicated still further by assuming that members of any broad group (e.g., women, minorities, an age group) are heterogeneous, so

that search costs, rates of time preference and the variances of underlying wage-offer distributions are not identical within a group whose wage-offer distribution we are trying to compare to that of another group. Finally, he notes that the same model can be used to obtain corrected estimates of the return to schooling and of the gains from migration (perhaps foreshadowing Willis and Rosen, 1979, and Borjas, 1987).

Lewis's (1975) note was his contribution to an American Economic Association session on an "Assessment of Recent Research" in labor economics. Recognizing even by that early date that research results on labor supply and time use were too many to summarize extensively, much less succinctly within the confines of a session at the AEA meetings, he focuses on the findings of some of the then recently completed Negative Income Tax (NIT) experiments. Writing down and parameterizing a lifetime utility function for a two-person household, he simulates labor-supply responses to changes in NIT parameters (the guarantee and implicit tax rate). The essential point is that intertemporal substitution may lead evaluations of the experiments to underestimate the programs' effects and that these may be substantial. Aside from its substantive contribution, the short paper beautifully illustrates both Gregg's seriousness about research and his essential professional modesty.

Becker and Lewis (1973), a comment on a set of papers in a special issue of the *Journal of Political Economy*, is rightfully considered as a discussion of labor supply, since fertility helps to determine the amount of labor supplied to an economy. This most heavily-cited of Lewis's works grew, I believe, out of Becker (1960), the introduction of the idea of children as consumer goods, analyzable by thinking of their price, including the prices of parents' time, and including a discussion of income and price effects in fertility. While Becker (1960) discussed the quality-quantity trade-off, this article provides a formalized effort to think about the relationship between the quantity and quality of children and its implications for observed income and price elasticities of demand for kids.

The central and quite reasonable assumption is that the price of quantity of children rises with the quality of each, and vice-versa. This assumption guarantees (without any restrictions on preferences for quantity/quality) that an exogenous increase in household income will alter the price of quality by altering the demand for numbers of children (and vice-versa). This result in turn means that measured income effects

understate true income effects, with the understatement probably being greater for the income elasticity of demand for numbers of children, under the reasonable assumption that child quality is more closely substitutable for the composite of all other goods than is the quantity of children. This approach—and the essential and inarguable point is that the prices of quality and quantity depend positively on the amount of the other—can be and has been used since this publication to rationalize the changing relationship between fertility and, for examples, education, household incomes, real wage rates and relative female-male earnings.

#### **IV. Other Research**

While it is not worth summarizing all of Gregg's published and unpublished *oeuvre*, one other article merits mention in this survey. His first journal publication was, I believe, Lewis and Douglas (1939). (The Douglas is of Cobb-Douglas fame to economists, but of much greater fame to Illinoisans like me, as he was our U.S. senator for 18 years, and perhaps the “grandfather of modern labor economics.” His research is discussed by Bergmann, this volume.) This study, apparently stimulated partly by Keynes' emphasis on the marginal propensity to consume, derives income and expenditure elasticities of consumption and notes that Allen and Bowley's (1935) use of linear expenditure curves to describe spending patterns is overly restrictive. Proposing the use of a loglinear function, the article demonstrates its lesser restrictiveness, but notes that even it imposes probably unrealistic restrictions on the second derivatives of expenditure functions.<sup>14</sup> Wonderfully, in commenting on the use of these loglinear functions, the authors note that the data used to estimate such functions are typically cross sections and thus produce errors due to each individual having had a time-varying history of income fluctuations. The permanent-income theory in a nutshell!

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<sup>14</sup>One might view this study as doing for consumer theory what Cobb and Douglas did for production theory. The motivation appears to be similar: Trying to make sense of patterns in data on which Douglas had been laboring for many years (Douglas, 1976).

## V. Legacy

Gregg Lewis's research gave to the economics profession a body of work on several topics, especially the impacts of trade unions, that underlay and defined subsequent research in those areas. I doubt that most economists today care much about this specific issue; and at a time (2019) when private-sector unions in the U. S. cover only 7 percent of employees, compared to nearly 35 percent in the 1950s, this indifference may be warranted.<sup>15</sup> So Gregg's lasting influence cannot be the specifics of his work on unionization, as careful and thoughtful as it was. Instead, it is his research as an exemplar of serious thinking about economic behavior, care with data and concern about "getting it right."

Several of the articles that I have discussed contain the germs of later researchers' fundamentally important research. If I were not familiar with this later literature, Lewis's implicit contribution to it might not be so apparent; and in claiming his work as fundamental antecedents to several of the articles that every labor economist today views as the most important in the sub-discipline I am in no way saying that later researchers took Lewis's ideas. Rather the ideas are stated clearly, albeit in rudimentary form, in Lewis's articles. As such, this is one more reason why his research deserves more recognition than it appears to receive.

Just as important as Lewis's research is the obvious impact that his example and direct influence had on the work of his students. Two Chicago Ph.D. students whose dissertations he supervised later won Nobel Prizes. Gary Becker (1957) was based on Becker's Ph.D. thesis; and while Robert Lucas is known for his fundamental work in macroeconomics, his dissertation, in part published as Lucas (1969), was very much a product of Gregg's influence and interest in labor demand. (Lucas's work is discussed by De Vroey, this volume). Sherwin Rosen would, I believe, have been honored with a Nobel Prize had he not died relatively young. His dissertation, revised and in part published as Rosen (1968), reflects Gregg's interest in the determination of work hours. Walter Oi's (1962) work on labor demand stemmed from a chapter of a dissertation that Gregg had supervised. Glen Cain's thesis book (Cain, 1966) is another piece that bears

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<sup>15</sup><https://www.bls.gov/news.release/union2.t03.htm> .

the clear stamp of Lewis's supervision. All these studies illustrate the careful thinking about economic behavior and the attention to empirical detail that underlay Gregg's own research. The work of still other students, not only his supervisees, was clearly influenced by their participation in the Labor Workshop at Chicago.

In Winter quarter 1963 I took Gregg's undergraduate course in labor economics. The textbook (Reder, 1957) was a mix of traditional institutional labor with substantial discussion of neoclassical economics as applied to labor. The lectures introduced us to the economics of labor supply and demand and to the theory of investment in human capital. Although they lacked the empirical superstructure provided by the immense body of subsequent empirical work on these topics, they expounded basically the same set of ideas that pervade today's courses but which at that time were so novel—and so inspiring for an undergraduate.<sup>16</sup>

Gregg asked me to be one of his research assistants, a job that I performed from Fall Quarter 1963 through Winter Quarter 1965.<sup>17</sup> Copying data and calculating logarithms from the *Minerals Yearbook* may not seem like a profound or intellectually exciting activity; but seeing the care with which he organized his research impressed on me the same concern for data that I learned from further study of his work. His influence has pervaded my own work, in the same way that it has influenced the research of so many other economists whose contributions have been much more important than mine.

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<sup>16</sup>The course cemented my decision to continue in economics (instead of switching to statistics). I was hardly alone in my appreciation of his teaching: The University honored Gregg in 1972, naming him one of the four annual winners of its Quantrell Award, probably the oldest undergraduate teaching award in the country.

<sup>17</sup>In September 1964 I asked to place my assistantship work on hold, and he kindly approved. He asked why, and I said that I wanted to work in President Johnson's election campaign. He looked askance but said nothing. That was the only indication I ever had from him directly or from his work (since I had not read Lewis, 1951) of any political views. Very far from the stereotype of Chicago economists of that time.

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**APPENDIX: Questionnaire on Knowledge of Lewis and His Work**

Please answer off the top of your head: Is the name H. Gregg Lewis familiar to you?  
If yes, what do you know about him (one sentence or so)?

In what year did you receive your Ph.D. or other highest degree?

Is that degree from a North American institution?

**Table 1. Google Scholar Citations to Lewis Works\***

<b>Article/book</b>	<b>Citations</b>
Becker and Lewis, 1973	3820
Lewis, 1986b	973
Lewis, 1963	897
Lewis, 1974	244
Lewis, 1957	125
Lewis, 1969	104
Lewis, 1990	83
Lewis, 1983	70
Lewis, 1951	38

\*Recorded June 9, 2020.

**Table 2. Determinants of Familiarity with Lewis's Name and Contributions<sup>a</sup>**

<b>Ind. Var.:</b>	<b>Dep. Var.:</b>	<b>Knew the Name?</b>		<b>Known For?</b>	
				<b>Unions</b>	<b>Unions or Pioneer/Teacher</b>
<b>Ph.D. Before 1995</b>	0.465 (0.035)	0.377 (0.052)	0.219 (0.051)	0.266 (0.054)	
<b>North American Ph.D.</b>	0.186 (0.036)	0.131 (0.043)	0.037 (0.054)	0.139 (0.057)	
<b>Interaction</b>	-----	0.217 (0.085)	-----	-----	
<b>Pseudo-R<sup>2</sup></b>	0.162	0.168	0.045	0.070	
<b>N</b>		816		325	
<b>Mean of Dep. Var.</b>		0.398	0.338	0.458	

<sup>a</sup>The coefficient estimates shown are probit derivatives. Standard errors are in parentheses. The sample consists of respondents to a survey of 1,707 Research Fellows and Affiliates of the Institute for the Study of Labor (IZA).