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Essays on Labor and Public Economics:
Dissertation Summary

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My dissertation focuses on issues in labor and public economics. It comprises three chapters that are broadly unified by their examination of policy relevant topics.

Chapter 1

Dying to Know: Are Workers Paid Their Marginal Product?

In the first chapter, I explore how workers' compensation relates to their marginal product, a subject central to our understanding of how labor markets work.¹ Theoretical interest in the topic is longstanding. Near the turn of the previous century, John Bates Clark (1899) and Knut Wicksell (1906) helped lay the foundation for the neoclassical framework in which profit-maximizing firms set the marginal product of labor equal to its competitively set price. A long and varied stream of theoretical papers has since been developed that deviates from this simple picture. In some theories, the simple neoclassical result holds across the lifecycle, but the marginal product may not equate the wage in any given period because of firm-specific productivity or incentive issues (Becker 1962; Lazear 1979). Other theories, such as monopsony or search models, posit that firms possess wage-setting power and therefore pay workers below their marginal product (Mortensen and Pissarides 1994; Robinson 1933). Despite the fundamental relevance of the subject in distinguishing among different models of the labor market, the empirical literature is limited.

A significant challenge to identifying how the marginal product of labor relates to worker pay is finding exogenous variation in the amount of labor employed by firms. Firms' decisions over the amount of labor to hire is endogenous to productivity shocks (e.g., firms hire in response to changing market conditions). I overcome this challenge by exploiting variation in labor induced by the death of a worker from accidental causes. Accidental deaths, such as motor vehicle fatalities, are an appealing source of variation because they are plausibly (conditionally) random to firm performance over time, unexpected, and almost all near-instantaneous. Under certain assumptions that I discuss in the chapter, I can use this variation to estimate workers' marginal products and compare it to the price of their labor.

I investigate how the death of a worker from accidental causes impacts firm revenue and costs. If these workers were paid their marginal product, by definition, revenue will change by the same amount as costs, holding all else con-

stant. If the workers were paid below (above) their marginal product, revenue will drop more (less) than costs. Focusing on small firms, I test whether the reduction in firm costs following an employee's death is equal to the reduction in firm revenue. Neoclassical theory implies that they are equal, and I can compare my results to this benchmark (in addition to investigating other theories of the labor market).

I bring novel data to bear on this question. Detailed employer-employee data are necessary for the analysis, and I link together data on two-thirds of U.S. workers and firms. Worker and firm payroll data from the Longitudinal Employer Household Dynamics data set is merged to firm revenue and assets data from business tax returns filed with the IRS. These confidential data are then linked with restricted Vital Statistics Multiple Cause of Death data to infer the cause of death for workers who died in the sample period. Finally, I merge in the Economic Census for additional data on firm financials.

My results indicate that a death from accidental causes brings about an immediate drop in firm employment, and that it takes two years for firms to return to their prior employment levels. Firms experience both a drop in labor costs and revenue, but revenue goes down more, allowing me to reject the null hypothesis that workers are paid their marginal product. This result is robust to adding detailed industry by geographic controls, limiting the analysis to specific subgroups of accidental deaths (e.g., excluding accidents that occurred during work), exploiting variation from the death of workers who earn different amounts of compensation, and other robustness checks. Further, there is no differential trend in any outcome variable before the accidental death occurs (although pre-trends are found for firms that lose employees to other causes of death). More generally, I find evidence that the occurrence of an accidental death is conditionally random to firm characteristics.

Other firm inputs must be held constant in order to calculate the marginal product of labor. Consistent with the hypothesis that an accidental death does not alter other firm financial decisions, no evidence is found for an effect on firm assets, which include a measure of physical capital. I then investigate more directly the effect of an accidental death on other firm costs that do not appear in the main data. Using confidential data from the Economic Census, I find no evidence for an effect on nonlabor costs (such as capital, materials, and rent). Finally, while measures of corporate taxes and employee benefits are missing from the main data, I take these costs into account under various scenarios.² Once these adjustments are made, the null hypothesis of equality between the marginal product and compensation can still be rejected, and the results suggest that workers are on average paid no more than 85 percent of their marginal product.

I then explore some of the potential mechanisms that may underlie this finding. There are multiple theoretical models that are consistent with workers being paid below their mar-

ginal product. Firms may have wage-setting power because of classical monopsony (a single or small number of employers facing many sellers of labor), heterogeneous preferences of individuals for different types of work, moving costs, or search frictions arising from imperfect information about other jobs. I test different predictions of these models regarding variation in the degree to which workers are underpaid relative to their marginal product (by parsing the impact of an accidental death across these potential sources of heterogeneity). This suggestive analysis finds some evidence consistent with models of search frictions and moving costs.

I subsequently turn to examine a class of theories that relates to the determinants of pay relative to marginal products over the life cycle.³ In the Becker (1962) model, firms bear at least some of the cost of the investment in workers' human capital and therefore pay more-tenured workers below their marginal product in order to recoup this cost. On the other hand, the implicit contracts theoretical literature following Lazear (1979) posits that in order to solve a shirking problem, more-tenured workers are paid above their marginal product and less-tenured workers are paid below. I test these sharply contrasting predictions and find that more-tenured workers are underpaid relative to less-tenured workers, suggesting that the evidence is more consistent with the Becker model than the Lazear model.

Beyond the theoretical interest, the subject of workers' marginal product and pay is prominent in various empirical debates, including the debate over interindustry wage differentials (Murphy and Topel 1987; Krueger and Summers 1989; Gibbons and Katz 1992). Central to the interindustry wage differentials debate is the question of whether pay disparities across sectors reflect underlying differences in marginal products (as opposed to other factors). Motivated by empirical evidence from this debate and the evidence on wage dispersion more generally that seemingly similar workers are paid different amounts across firms and industries (Abowd, Kramarz, and Margolis 1999), there is a presumption that workers may not (always) be paid their marginal product. But while an extensive empirical literature exists on how workers' pay relates to their marginal product, it is limited, inconclusive, and often indirect (see Ashenfelter, Farber, and Ransom [2010] for recent papers on the topic). To my knowledge, this is the first study to directly estimate the marginal product of labor while using quasi-experimental variation.⁴

Finally, there are important policy implications that arise from the subject of this chapter, namely those that relate to government's proper role in labor markets. For example, both the effectiveness of labor market interventions (such as the minimum wage, job tax credits, etc.) and the efficiency of human capital investment (and the extent to which government should subsidize it) are informed by the degree to which workers are paid their marginal product.⁵

Chapter 2

Do Local Fiscal Spillovers Exist? Evidence from Counties, Municipalities, and School Districts

In the second chapter, I explore how the fiscal decisions of one jurisdiction influence the fiscal decisions of its neighbors. A fundamental question about governments is to what extent they are influenced by one another. A large theoretical literature presumes interactions and has identified several pathways by which fiscal spillovers operate (such as via interjurisdictional tax competition, where the concern to attract and retain businesses and residents can induce jurisdictions to compete among themselves over their level of taxes and benefits).⁶ A key empirical issue in this theoretical literature is to what degree do the fiscal decisions of one jurisdiction influence its neighbors' fiscal decisions. Are they of a large magnitude, or are they small or nonexistent? A failure to find any effect would raise questions about the importance of those theoretical channels. In this chapter, I empirically explore this question of fiscal spillovers on the local level.

There are several challenges to identifying the effect of fiscal spillovers. Unobserved determinants of fiscal decisions might be correlated across neighbors, and neighbors' decisions are jointly determined in equilibrium. To provide a strong research design that addresses these challenges, I collect a new data set. In Ohio, local governments often require the explicit approval of voters to raise taxes. My data set consists of tens of thousands of these tax referenda that are economically significant and span multiple types of government and tax instruments. The elections for tax increases lend themselves to a regression discontinuity design that exploits the underlying continuity in jurisdiction characteristics around the threshold for measure approval to produce approximate random assignment.⁷

I examine whether jurisdictions respond to exogenous referendum passage by their neighbors. The analysis covers counties, municipalities, and school districts; and bonds, income tax, property tax, and sales tax measures. I first explore the issue graphically to determine whether there is evidence of any discontinuous jumps at the threshold for voter approval and then run formal econometric analyses.

The results indicate no evidence that spillovers exist for any jurisdictional type or revenue source. Plots yield no jump in the dependent variables at the threshold for voter approval. Formal analyses never find a statistically significant effect, and the estimates are reasonably precise. The main measure of neighborliness is spatial proximity (i.e., where I test for the existence of spillovers), and the results are robust to alternative ways of defining neighbors. The results are also robust to focusing only on the largest of measures as well as limiting the analysis to geographic areas where spillovers

are most likely to be present. Lastly, no effect of referendum passage on mobility or sorting is found. The results therefore call into question theoretical models that presume spillovers and suggest any distortions (or efficiency-enhancing effects) possibly generated by fiscal spillovers are small.

Chapter 3

Child Schooling and Parental Behavior: Evidence from the Head Start Impact Study

The final chapter of my dissertation, written jointly with Alexander Gelber, addresses how government investment in children influences parents' own investment in their children. In analyzing the return to schooling, it is possible to distinguish the direct impact that schooling programs have on children from the indirect impact that is mediated through the effect that schooling has on parent investment in children. If parents have large impacts on their children, these indirect effects may be important.⁸ *A priori*, we do not know the sign of these indirect effects: schooling inputs could encourage or crowd out parent inputs. Furthermore, the degree to which government inputs cause parent inputs to increase or decrease helps to determine the efficiency of government expenditure on schooling: many believe that government-provided schooling may supplant parents' role to some extent. As Becker and Tomes (1976) write in their theoretical analysis of crowd-out of parent investment by public expenditure: "Compensatory responses of parents apparently greatly weaken the effects of . . . some Head Start programs."⁹ If investment in children is costly to parents, then estimating the impact of schooling programs on parents' investment is also relevant to a full welfare analysis of the programs. Despite the importance of these questions, there is little empirical work in economics on how schooling programs impact parents' effort investment in children.

The Head Start Impact Study (HSIS) represents a promising setting for investigating this issue in the context of Head Start (HS). HS is a government program that provides preschool to low-income children. Like many schooling programs, one specific goal of HS is to increase parent involvement with their children. First-time applicants to HS for the fall of 2002 were randomly selected by HSIS for access to HS.¹⁰ HSIS followed the children and their parents for several subsequent years, collecting information on a variety of child and parent outcomes both during and after the preschool years (U.S. Department of Health and Human Services 2010).

We find that in response to children's HS access, parents are substantially and statistically significantly more involved with their children along a wide variety of dimensions, particularly along those dimensions that appear to be investments in child human capital.¹¹ For example, parents

read to their children more often, and for a longer amount of time at each sitting, when their children have access to HS than when they do not. Interestingly, even after children are no longer attending HS, their parents appear to invest more in them. This stands in striking contrast to work on the impact of HS on test scores, which finds that HS has positive effects on test scores while children are enrolled in HS but that these test score gains may quickly "fade" (Currie and Thomas 1995; U.S. Department of Health and Human Services 2010). When we use access to HS as an instrument for HS enrollment, we find that the point estimates of the mean increase in parent investment in children is 15 percent of a standard deviation while children are in their preschool years and 6 percent of a standard deviation after their preschool years. These findings on parent involvement during and after the experiment constitute the core of the chapter. Our results show that HS is successful in its goal of increasing parent involvement with children.

Intriguingly, we find that across HS programs, those programs that raised children's cognitive test scores more also tended to increase parents' involvement with their children. We discuss a variety of mechanisms that may be consistent with this and our main findings, including the possibility that HS programs that are particularly effective in raising children's cognitive scores also tend to be particularly effective in raising parent involvement, as well as the possibility that HS impacts parent involvement in part because parents perceive their involvement to be complementary with child schooling in the production of child qualities. We present a simple model that captures these potential explanations for the observed effects.

Despite the fact that test score gains fade after HS ends, there is evidence that HS does have impacts on long-run child outcomes (Deming 2009; Garces, Thomas, and Currie 2002; Ludwig and Miller 2007). The reemergence of schooling impacts on children in later life, despite the finding of fading gains, has also been noted in other contexts (e.g., Chetty et al. 2011; Dynarski, Hyman, and Schanzenbach 2011; Heckman et al. 2010). It is possible that persistent increases in parent investment constitute a channel through which the long-run impact on child outcomes is mediated.

Notes

1. *Marginal product* is used in lieu of the term *marginal revenue product*, although formally speaking, marginal product refers to the change in physical output whereas marginal revenue product (my outcome of interest) refers to a change in actual revenue. Marginal product multiplied by marginal revenue equals the marginal revenue product.
2. Any change in profits is moderated by corporate taxes. Results on benefits using the Economic Census indicate a negative effect, but one that is lower than is found for payroll.
3. This also has implications for some of the potential mechanisms.

4. The word *direct* is used in the sense that I explicitly estimate how a change in employee labor impacts revenue (the approximate partial derivative of revenue with respect to labor). This language is used to differentiate the approach from one that estimates a parameter other than the marginal product of labor that tests implications of relevant models or with theory can be used to recover the marginal product (e.g., estimating the correlation between firm concentration and wages or the labor supply curve facing an individual firm).
5. This is but a small list of examples. In the latter example, individual decisions over the amount of human capital in which to invest (such as schooling and job training) are efficient if the returns to the investment are fully captured by workers. This is not the case if workers are paid below their marginal product, which would therefore likely justify government involvement.
6. This fiscal competition, depending on the model, can lead to suboptimally low levels of public goods, or so-called races to the bottom, but it can also lead to efficient levels of public good provision (Brennan and Buchanan 1980; Oates and Schwab 1988; Wilson 1986; and Zodrow and Mieszkowski 1986). In another type of fiscal spillover, known as *yardstick competition*, residents use the fiscal decisions of neighboring jurisdictions as a benchmark for their own jurisdiction to correct an information asymmetry between themselves and politicians about the cost of public good provision (Besley and Case 1995). To distinguish between good and bad elected officials, voters examine whether their tax rates are higher than in surrounding jurisdictions, which constrains the tax setting behavior of politicians who wish to be reelected. Conventional spillovers might arise where residents of one jurisdiction consume, whether in a tangible way or not, the public goods of another jurisdiction. The last source of spillovers is Tiebout resorting, where individuals move in response to a particular fiscal change to better match their public good preferences.
7. The prior empirical literature on fiscal spillovers has grown in recent years. Relative to the previous studies, the main innovation in this chapter is to use a regression discontinuity design that plausibly isolates exogenous variation in taxes and spending. Most studies in the literature test for spillovers by instrumenting for neighbor fiscal behavior using neighbor characteristics, such as demographics, as well as neighbor lags, in taxes and spending. Papers have examined strategic fiscal behavior among countries, states, municipalities, and school districts. The empirical literature has tended to find large positive spillovers across jurisdictions, and I replicate those results with my data using neighbor characteristics as instruments.
8. Indeed, parenting is often thought to play an important role in explaining differences in child outcomes across racial and socioeconomic status groups. Randomized control trials of efforts to involve parents in children's lives have often found positive impacts on children, including on children's cognitive scores (Nye, Turner, and Schwartz 2006).
9. This was the source of debate between Becker and Tomes (1976) and Goldberger (1985), who the latter of whom points out that parents could increase investment in children in response to schooling programs. Becker and Tomes also write that "Government programs may have effects [on parent behavior] by changing rates of return on parent investments in children . . . However, we have emphasized the redistribution effects of many programs—including Head Start programs . . . because the redistribution effects are clear, while effects [due to changing the marginal rate of return] are not clear, even in direction . . ." By *redistribution effects*, Becker and Tomes are referring to the compensatory responses of parents that could cause a decrease in parent investment in response to an increase in child schooling. The related model of Peltzman (1973) predicts that private education expenditures are decreased by public expenditure on higher education.
10. The HSIS randomly assigned a sample of 3–4-year-old HS applicants either to the HS group or to the control group. The HS group was allowed to enroll in HS at the HS center to which they applied, while the control group was not granted access to HS at that center (but may have received similar services through other available programs chosen by their parents—in occasional cases through other HS centers).
11. By parent involvement we mean activities that parents undertake that require time or effort and directly involve their children.

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