Three Essays in Labor and Public Economics

by

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Submitted to the Department of Economics
in partial fulfillment of the requirements for the degree of

Doctor of Philosophy

at the

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

May 1998

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ABSTRACT

This thesis considers both empirical and theoretical topics in labor and public economics. The first chapter addresses the effects of child care costs on single mothers' labor market outcomes. In recent years, U.S. policymakers have begun to focus on the role of child care costs in determining labor supply and welfare participation choices among single mothers. In this chapter, I use access to public schooling for a single mother's five-year-old child as a source of variation in the child care costs she faces. Because only sufficiently old five-year-olds are eligible to enroll in public school, I am able to instrument for enrollment using five-year-old's quarter of birth. This approach is attractive for three reasons. First, unlike papers in the existing literature, I am able to assess directly the impact of providing free child care. Second, the interaction of quarter of birth and school entry rules provides exogenous variation in access to public schooling. The resulting natural experiment need not rely on strong functional form assumptions regarding unobservables in order to avoid selectivity bias problems commonly addressed in existing child care papers. Third, and perhaps most importantly, my natural experiment is not beset by other potential endogeneity problems typically caused by regressing quantity on price; no previous child care paper has addressed this issue. Using data from the 1980 Census, I find that quarter of birth is a powerful predictor of five-year-old's enrollment. I find that access to free public schooling has a significant positive effect on labor supply and a significant negative effect on receipt of public assistance by female heads of households whose youngest child is five. Results for married women living with their husbands suggest smaller but still significant effects of access to public schooling, regardless of youngest child's age. Among female heads who have a five-year-old, but whose youngest child is younger than five, access to public schooling has no effect on labor market outcomes.

The second chapter, joint with Lant H. Pritchett of the World Bank, considers the political feasibility and normative desirability of universal and targeted income transfers. Standard economic analysis suggests that when the budget for redistribution is fixed, income transfers should be targeted to (i.e. means-tested for) those most in need. However, both political scientists and economists long have recognized the possibility that targeting might undermine political support for redistribution. We formalize this recognition, developing a simple economy in which both non-targeted (universally received) and targeted transfers are available for use by the policymaker. When the budget can be taken as fixed, full use of the targeted transfer is optimal. However, when we allow the budget to be determined through majority voting (with the policymaker choosing the share of the budget to be spent on each type of transfer), the optimal degree of targeting is zero. More strikingly, we show that if the policymaker naively ignores political considerations, the resulting equilibrium actually *minimizes* not only social welfare, but also the welfare of poor and middle income agents. Thus political considerations cannot generally be regarded as simply another "small" extension of standard models. As a result, future models and actual policies advocating the use of targeting through means-testing should account explicitly for the role of political considerations.

The third chapter examines the relationship between educational attainment and marriage market behavior. Studies of age at first marriage typically find that people with more education get married later. In this chapter, I describe models that predict or are consistent with this relationship. I also cite reasons why empirical studies that fail to control for unobserved heterogeneity or endogeneity of educational attainment may overstate the delaying effect of education on age at first marriage. I then use data on white men and women taken from the 1980 Census and attempt to use instrumental variables based on a person's quarter of birth to control for heterogeneity and endogeneity. This approach is generally unsuccessful. Because of the need to control for a strong negative trend in age at marriage, the instrumental variables estimates must be computed conditional on controls for age. The results are very sensitive to specification of these controls. I conclude that when the dependent variable is trending strongly, QOB-based variables are unlikely to be good instruments for educational attainment.

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ACKNOWLEDGMENTS

I would like to begin by thanking my family for all of their support in encouraging me throughout my time in graduate school. While it's probably Jessica's and Bob's fault that I decided to get a Ph.D. — after all, they both had the degree, and isn't that what you do after college anyway? — they have been tireless in cheering me on when things went well and listening when they didn't. I offer similar thanks to my stepmother Kathy and my stepfather Martin, each of whom were just as supportive. My brother Scott and sisters Rebecca and Amy have always made life fun and interesting while also encouraging me. My twin brothers Alex and Frederic are too young to know what it is I do, much less why, but they nonetheless are an inspiration.

I of course offer thanks to my teachers, throughout my educational career. Those at Stamford High School helped stimulate my interest in all sorts of fields while accommodating my desire to move at my own pace. At the University of Massachusetts at Amherst, I encountered numerous faculty members whose interest and ability in teaching I hope to emulate. Perhaps most importantly, I thank my advisers at MIT, Jon Gruber and Josh Angrist. I have learned much from each, and my thesis would have looked much different without their suggestions and advice. Michael Kremer's insights have also been an exciting part of my time here, and I thank him for providing me the opportunity to work as his research assistant.

I owe a special debt to Jim Poterba. It is impossible to study public economics at MIT without developing a tremendous sense of respect for Jim's range and grasp of basically everything; his is an example anyone should want to follow. At least as importantly in my case, Jim offered me both moral support and nearly unlimited use of his office at the NBER when I confronted a severe case of repetitive strain injury that forced me to use voice recognition software in conducting my research.

I must also thank the Council of Economic Advisers for giving me the opportunity to see economic policymaking firsthand as a Staff Economist from June, 1995, to May, 1996. While in Washington, I met the best friends I can imagine having, both at the Council and outside of work. I would like to thank Deon Filmer most of all, for both his wonderful and ever entertaining friendship and his willingness to listen to all of my ideas and rantings; everyone should be so lucky to have such a friend. Numerous other friends in Washington have made my life that much more fun: I thank Kirk Deitsch, Kelly Hallman, and Signe-Mary McKernan especially.

I would also like to thank several of my friends and colleagues at the Council. Michael Ash always made things interesting, whether we were arguing, agreeing, or working together; I can't imagine a better neighbor and colleague. Likewise, Carrie Cihak's wonderful sense of humor and unique world view were always stimulating, and Scott Wallsten's sarcastic quips were always a favorite. Carolyn Fischer's seemingly endless smile, not to mention her superfly musicianship, were always fun.

It would be a crime not to thank Lant Pritchett for all of his support and encouragement. I first met Lant in the summer of 1994, when I began work as his research assistant at the World

Bank, back when PRDPH still meant "Policy Research Department, Poverty and Human Resources Division". Lant always made me feel like an equal, except when he did his best to make me feel better than him; he has been my most constant promoter in the field of Economics from day one, and for that I will always be thankful. I greatly benefited from working with him on the second chapter in this thesis, and I hope that we will continue our collaboration. I must also thank Richard Eckaus here at MIT for his assistance both in introducing me to Lant and in helping me secure the job at the Council.

Last, but certainly not least, I would like to thank my fellow students at MIT and those Harvard students I've met while working at the NBER. There are many such colleagues, and I am certain that I will unjustly omit some of them. Madeline Zavodny's unique outlook and tremendous sense of humor always made things interesting, even when they seemed a little crazy; I'll always remember the Spring of my second year fondly because of Madeline. Marianne Bitler's friendship has been a wonderful part of my time here, and I am thrilled that we will be able to continue it in Washington. Paul Ellickson has been a lively and amusing friend throughout graduate school; whether it was our very own interpretation of Durbin-Watson or Paul's hefty use of bags, he has nearly always kept me laughing. Others at MIT whom I would like to thank include Ezra Friedman, Robert Marquez, Daniel Dulitsky, Julie Cullen, Jeff Kling, Eric Wolff and Kristin Forbes. I hope I haven't left too many names out. At the NBER, I must especially thank Sendhil Mullainathan, who at times I thought should have received a check from MIT for serving as such a willing adviser. Others who have made life around the Bureau stimulating and fun include Marianne Bertrand, Kate Baicker, Jeff Brown, Courtney Coile, Alan Durrell, Erzo Luttmer, and Ellen Meara. I also owe a special thanks to Chris Foote for his SAS help when I most needed it; chapter 1 might never have been finished in time without him.

Lastly, I would like to thank Michael Rosenbaum for all of the humor and insight he has brought to our friendship throughout his time at Harvard Law School. I only hope that Michael will remember me when he rules the world.

Introduction

This thesis considers both empirical and theoretical topics in labor and public economics. The first chapter addresses the effects of child care costs on single mothers' labor market outcomes. In recent years, U.S. policymakers have begun to focus on the role of child care costs in determining labor supply and welfare participation choices among single mothers. In this chapter, I use access to public schooling for a single mother's five-year-old child as a source of variation in the child care costs she faces. Because only sufficiently old five-year-olds are eligible to enroll in public school, I am able to instrument for enrollment using five-year-old's quarter of birth. This approach is attractive for three reasons. First, unlike papers in the existing literature, I am able to assess directly the impact of providing free child care. Second, the interaction of quarter of birth and school entry rules provides exogenous variation in access to public schooling. The resulting natural experiment need not rely on strong functional form assumptions regarding unobservables in order to avoid selectivity bias problems commonly addressed in existing child care papers. Third, and perhaps most importantly, my natural experiment is not beset by other potential endogeneity problems typically caused by regressing quantity on price; no previous child care paper has addressed this issue. Using data from the 1980 Census, I find that quarter of birth is a powerful predictor of five-year-old's enrollment. I find that access to free public schooling has a significant positive effect on labor supply and a significant negative effect on receipt of public assistance by female heads of households whose youngest child is five. Results for married women living with their husbands suggest smaller but still significant effects of access to public schooling, regardless of youngest child's age. Among female heads who have a five-year-old, but whose youngest child is younger than five, access to public schooling has no effect on labor market outcomes.

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ket behavior. Studies of age at first marriage typically find that people with more education get married later. In this chapter, I describe models that predict or are consistent with this relationship. I also cite reasons why empirical studies that fail to control for unobserved heterogeneity or endogeneity of educational attainment may overstate the delaying effect of education on age at first marriage. I then use data on white men and women taken from the 1980 Census and attempt to use instrumental variables based on a person's quarter of birth to control for heterogeneity and endogeneity. This approach is generally unsuccessful. Because of the need to control for a strong negative trend in age at marriage, the instrumental variables estimates must be computed conditional on controls for age. The results are very sensitive to specification of these controls. I conclude that when the dependent variable is trending strongly, QOB-based variables are unlikely to be good instruments for educational attainment.

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Chapter 1

How Large an Effect Do Child Care Costs Have on Single Mothers' Labor Supply? Evidence Using Access to Free Public Schooling

> From now on, our nation's answer to this great social challenge will no longer be a never-ending cycle of welfare, it will be the dignity, the power and the ethic of work. . . . [The welfare reform bill] is good because without the assurance of child care it's all but impossible for a mother with young children to go to work.
>
> *President Clinton*, August 22, 1996

1 Introduction

The announcement in early January of 1998 that the Clinton Administration would seek nearly $22 billion in added funding underscores child care's current status as a major national policy issue. According to President Clinton, the initiative's increased block grants to states are meant to "double the number of children receiving child care subsidies to more than 2 million."[1]

But even before this proposal, child care had become a major focal point in ongoing welfare reform efforts. The economic case for child care's role in determining labor supply is simple. For parents of children too young to care for themselves, child care costs can act like a tax on wages: parents must find someone to watch their children while they work. As policymakers' desire to reduce the welfare rolls by increasing labor supply among single mothers has grown, they have begun to focus on this role of child care costs. The 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) included both a consolidation of the patchwork system of Federal child care expenditures into a single block grant and an increase in funding for child care subsidies to low-income families (even while direct cash and other in-kind benefits were being restricted).

Policymakers seem to be betting that increased child care subsidies, perhaps together with other services (e.g. job training and job search assistance) for welfare participants, will mitigate the effects of binding work requirements and time limits on welfare participants. The question naturally arises, then, just how much bang can the child care buck deliver in moving welfare mothers into the workforce?

In this paper, I use 1980 Census data to estimate the effect of access to free public schooling for a woman's children on her labor supply and her probability of receiving public assistance. Among single women whose youngest child is five, I find that free public schooling for the five-year-old significantly increased 1979 labor supply and income from wages and salaries, while significantly reducing the likelihood that the woman received public assistance in 1979. I find smaller, though

[1] See The White House (Jan. 7, 1998).

still significant, results for the week before sample members completed the Census questionnaire.[2] Among female heads who have a five-year-old but whose youngest child is younger than five, there is no evidence that free public schooling for the five-year-old affects labor market outcomes. These findings confirm the view that subsidizing child care costs would have a sizable impact on labor supply among single mothers, provided that subsidies are available for the youngest child. Among married mothers who have a five-year-old, access to free public schooling for the five-year-old has significant effects on labor market outcomes, regardless of the youngest child's age. However, these effects are smaller than for single mothers whose youngest child is five.

The paper makes two significant contributions to the child care literature in general, and the literature on child care costs and single mothers in particular. First, I am able to answer an important policy question to which previous analyses have been much less suited: how large an impact can be expected from direct provision of child care? While authors of previous studies have been interested in this question, none have directly addressed it.

Second, my use of access to public schooling as a source of variation in child care costs, and quarter of five-year-old's birth to construct instruments for school enrollment, relies on a plausible natural experiment. This approach allows me to avoid potential selectivity bias arising from joint determination of child care and labor supply decisions without relying on statistical assumptions regarding the distribution of unobservable factors. Moreover, I am able to avoid bias due to any simultaneous relationship between labor supply and child care prices (for example, because labor supply requires purchase of child care, and the equilibrium quantity and price of child care are jointly determined). I will argue below that results from previous studies are highly sensitive to model specification, in a way that suggests lack of identification is the most glaring problem in the existing literature on child care.

Descriptive statistics certainly point to the importance of child care costs. One approach – the one that underlies my identification strategy – is simply to compare labor force participation rates among single mothers whose youngest child is aged between six and eighteen with participation rates among those whose youngest child is younger than six. The usefulness of this indicator may seem obvious: mothers of only older children need not pay for child care during school hours, while mothers of preschoolers must.

Among divorced women with children younger than 18, 85% of those whose youngest child was six years of age or older participated in the labor force in 1995. By contrast, only 73% of

[2]This date is generally supposed to be April 1 of the Census year, though there typically is variation in the date when respondents receive and complete the form.

divorced women whose youngest child was under six years old participated.[3] The difference was similar among never-married women: 67% of never-married women with children aged between 6 and 18 (but none aged 5 or younger) participated, compared to only 53% of those with children aged younger than five.[4] Of course, child care costs may not be the only difference between single mothers with and without preschool-aged children, but differences like these are eye-opening.

Data from the Survey of Income and Program Participation (SIPP) for Fall 1993 suggest that observed averages in child care expenditures among families who purchase child care are consistent with these large differences in participation rates. Among single-parent families who paid for child care for their preschoolers, average expenditures on child care were about $60 per week.[5] Thus monthly expenditures would have been about $250.[6]

One way to put the costs of child care in perspective is to compare plausible budget set variation due to child care costs with that due to observed cross-state variation in maximum welfare benefit levels. A large and well-known literature uses cross-state variation in benefit levels to identify effects of work incentives on labor market decisions of single mothers.[7] The underlying assumption of papers in this literature is that women in states with low benefits are otherwise like those in states with high benefits, so that differences in outcomes are due to differences in the incentives facing the two sets of women.[8] Thus comparisons across the distribution of state welfare benefit levels may be used as a yardstick for assessing the magnitude of incentive effects arising from access to free child care.

As of January 1994, the standard deviation of state maximum benefits was $148 –just three-fifths the $250 monthly expenditure figure noted above.[9] Moreover, comparing this expenditure figure with differences across points in the cross-state distribution of maximum benefits reveals that this amount is sufficient to move from the 10th percentile to the 75th and is more than enough to move from the 25th percentile to the 90th. Thus having to pay for child care for preschoolers is likely to have a significant impact on a single mother's budget set, both in absolute terms and

[3]These and other descriptive statistics in this section were drawn from U.S. House Committee on Ways and Means (1994, 1996). Institutional details regarding Federal child care programs may also be found in those sources.

[4]In fact, among never-married women, the differential has often topped twenty points during the last decade. The biggest gap occurred in 1993, when 70% of never-married women with only school-aged children participated in the labor force, by comparison to just 47% of women with preschoolers.

[5]Even among families (married or not) with incomes below the poverty line, average expenditures were still $50.

[6]Note that figures like these translate into hourly rates in the neighborhood of $1-2 per hour.

[7]Much research has addressed the impact of the welfare system on labor supply of single mothers. Moffitt (1992) and Hoynes (1996) provide excellent surveys.

[8]There may of course be other differences between women in high- and low-benefit states.

[9]The average maximum monthly welfare plus Food Stamps benefit for a family of three was $681.

compared to the impact of welfare program parameters often thought to cause significant labor supply responses.

The remainder of this paper proceeds as follows. In the next section, I discuss existing literature on child care costs and women's labor supply. In section 3, I discuss the role of access to free public schooling in affecting the child care costs that women face. I address potential endogeneity of school enrollment by using information about the five-year-old's quarter of birth. I discuss my dataset in section 3.

I present the main estimates in section 5. The results imply a statistically and economically significant impact of child care costs on labor market outcomes. For single mothers whose youngest child is five, I estimate that access to free public schooling raises labor supply by 6.3% – 14.5%, raises wage and salary income by 23.1%, and reduces receipt of public assistance by 12.8%. Estimates for women whose youngest child is younger than five suggest no impact of access to schooling for the five-year-old. Since these women face high marginal child care costs even if their five-year-olds are enrolled, this finding suggests child care costs as the most likely cause of the results for women whose youngest child is five.

In section 6, I undertake a rough cost-benefit analysis of the effects of providing child care through the public kindergarten system. My results suggest that every dollar spent on kindergarten leads to an increase in labor income of 61 cents for women whose youngest child is five. Additional tax revenues and lower welfare expenditures would return another 16 cents to various levels of government.

I conclude in section 7.

2 Existing literature

The literature on women's labor supply and child care costs is reasonably large, though it has focused mainly on married women. Researchers have used several sources of variation. Most common is observed household child care expenditures; Connelly (1989, 1990, 1992), Berger and Black (1996), Averett et al. (1997), and Michalopoulos et al. (1992) are examples. Blau and Robins (1988) use site-average expenditures among participants in the 1980 Employment Opportunities Pilot Project as an index of costs facing married women. Barrow (1996) uses state averages of child care workers' wages and also applies regression techniques to household expenditure data to create two cross-state indexes of relative costs. Ribar (1992) uses a combination of data on household expenditures, area averages in wages, and characteristics of state child care regulations. Others

have used variation in tax rates: Michalopoulos et al. (1992) and Averett et al. (1997) use static variation across states or points in the income distribution, while Meara (1996) uses variation in state tax credits over time.

Nearly all of these papers have relied on nonlinear models for identification. Many of them use some method of selection correction to account for endogeneity of child care expenditures and labor force participation;[10] Connelly (1989, 1990, 1992), Ribar (1992) and Michalopoulos et al. (1992) are typical examples.

Estimated child care cost elasticities of labor supply among married women range from as low as 0 to as high as 0.8.[11] For single mothers, authors have found small elasticities – as low as 0[12] – as well as larger effects. Using data from the 1987 and 1988 SIPP panels, Kimmel (1995) estimates a labor force participation elasticity of 0.35 among all poor single mothers; among blacks, the elasticity was 1.36, while the figure for whites was 0.35.[13] General Accounting Office (1994) finds that the employment rate of poor mothers would increase from 29% to 44% if child care costs were fully subsidized, implying an (arc) elasticity of 0.52. However, this study pools single and married mothers; since married mothers have access to their husband's earnings, pooled estimates likely exceed the true elasticity for single mothers. Berger and Black (1992) find that the average subsidy from a Kentucky program increased employment by an upper bound of 25.3% (not percentage points). However, Berger and Black do not report average child care costs in the sample, so no elasticity may be computed. Connelly (1990) finds that a 50% subsidy of child care costs would reduce AFDC participation from her sample average of 20% to 11%, although she finds that subsidies would have little impact on labor force participation among women in her sample.

This wide variation in the estimated impact of child care costs on labor supply has been described as one of the most important questions facing child care researchers.[14] Kimmel (1996) presents a very interesting attempt to reconcile the disagreement among different studies. Using

[10]Blau and Robins (1988) do not correct for selection, relying only on area averages. Meara's (1996) use of differences-in-differences methodology to estimate responses to changes in laws governing state tax credits plausibly avoids the need for such procedures.

[11]Unless explicitly noted, references to child care cost elasticities of labor supply are stated in magnitudes, with the understanding that the elasticities are negative in sign.

[12]This estimate comes from Michalopoulos et al. (1992), who compute their elasticity conditional on positive hours of labor supply and positive expenditures on child care. Thus their estimated elasticity does not include effects of child care costs on the participation decision, so it is likely to be an underestimate.

[13]One might worry about conditioning on poverty status, since doing so raises possible endogeneity and selection problems.

[14]See Connelly (1992) and Kimmel (1996).

more recent data than most previous papers,[15] Kimmel (1996) first estimates her preferred specification. Her equation of interest relates labor force participation to child care costs, the mother's wage, and other controls.[16] Kimmel attempts to control for potential sample selection bias arising from the fact that only some mothers are observed purchasing child care, and only some mothers are observed working. She uses a bivariate (reduced form) probit model to predict labor force participation and purchase of child care services jointly. The estimated coefficients from these equations are used to correct for sample selection in equations predicting wages and child care costs, and these predicted values are then included in the final probit equation predicting labor force participation. She then compares her results to those generated using her data and Ribar's (1992) and Connelly's (1992) model specifications.[17]

Several of Kimmel's findings merit description here. First, her structural estimates suggest labor force participation elasticities with respect to child care cost of 0.92 for married mothers and 0.22 for single mothers.[18] Second, Kimmel is able to explain much of the difference in married mothers' elasticities estimated by Ribar (1992) and Connelly (1992) by using her own data and those authors' specifications. Ribar estimates an elasticity of 0.74, while Connelly's estimate is 0.2. Ribar (1992) has commented that part of the difference between his and other estimates may result from differing definitions of child care costs: while he defines price of care as expenditures per hour of care, other authors (including both Connelly (1992) and Kimmel's (1996) preferred specification) define price as expenditures per hour of mother's work. Using Ribar's model specification and variable

[15]Kimmel (1996) uses 1987 SIPP data, while many other papers in the child care literature (including those Kimmel focuses on) use 1984 SIPP data.

[16]The controls are mother's age, age squared, labor income, years of completed education, and total number of own children present in the family, whether the mother has any children aged 0-2, 3-5, or 13-17, whether the mother is nonwhite, whether she lives in the south, whether she lives in a metropolitan area, whether she is "unhealthy", whether any of her children are "unhealthy", her state's average (not maximum) AFDC grant for a family of 3, her state's Medicaid expenditure per recipient, and the number of other adults living in her household.

[17]Kimmel's preferred specification differs from Ribar's (1992) in two ways. First, Ribar uses a different definition (described below) of child care costs. Second, in estimating his price of care equation, Ribar includes only a single sample selection correction term, rather than the bivariate one used by both Kimmel (1996) and Connelly (1992). Kimmel also includes in her structural probit equation several variables that Ribar does not: whether the mother has any sick children, whether the mother is "unhealthy", her state's average AFDC grant for a family of 3, and her state's Medicaid expenditure per recipient. The only substantive difference between Kimmel's (1992) preferred specification and Connelly's (1992) is that Connelly (1992) does not include several variables in her structural probit equation for employment (in addition to the variables just listed, the following variables are included in Kimmel's but not Connelly's specification: the number of other adults living in her household, whether she has any children aged 13-17, and the total number of own children in her family.)

[18]The estimated coefficient on the predicted child care costs variable is statistically significant at the 0.01 level for Kimmel's married women sample, while for single mothers the coefficient is statistically indistinguishable from 0 (its t-statistic is -0.49); standard errors for the elasticity estimates are not shown.

definitions, Kimmel (1996) estimates an elasticity of 0.89, while her replication of Connelly's (1992) specification and variable definitions yields an elasticity of 0.42. Kimmel concludes her discussion of married mothers' response to child care costs by writing that

> much of the variation in child care price elasticity estimates across papers in the literature is likely due to equation specification. This suggests that researchers in this field must be very careful to explain their selection of variables to include in the various equations, and conduct sensitivity analysis to determine the importance of the specification chosen.

Fortunately, Kimmel (1996) repeats for single mothers the replications and sensitivity analysis just described. She writes that "The single mother estimates are much more sensitive to specification and model changes." In fact, while Kimmel's preferred specification yielded an elasticity of -0.2 computed from a coefficient not significantly different from 0, replications of Connelly (1992) and Ribar (1992) yielded coefficients significantly different (at the 5% level) from 0 and yielded the following elasticities: -0.54 (Connelly); +1.38 (Ribar's specification, but with child care costs defined as expenditures per hour worked); -4.54 (Ribar's specification and variable definitions, i.e. with child care costs defined as expenditures per hour of care purchased). The sign on the second elasticity is not a typographical error–taken at face value, Ribar's specification with Kimmel's (1996) variable definitions suggests that an increase in child care costs of 10% should *raise* labor force participation among single mothers by nearly fourteen percent. At the same time, Ribar's specification with his variable definitions suggests that a 10% increase in child care costs should *reduce* labor force participation among these women by 45%.

This extreme model sensitivity should not really be surprising. The concern in the literature about sample selection seems to have obscured another problem that is likely to be at least as serious. The typical approach assumes that in the absence of sample selection, regressing labor supply on observed expenditures would raise no econometric problems. However, this assumption is unlikely to be valid, for the same reasons that one cannot simply regress quantity on price—one doesn't know whether a supply equation, a demand equation, or some combination of the two has been estimated.

To see the analogy, suppose that one is estimating the effect of child care costs on hours of labor supply, and that sample members must purchase child care for each hour they work. Then the left hand side variable of the hours equation is identical to the quantity of child care purchased by each sample member. Suppose also that hourly prices paid for child care are observed and do not vary with the number of hours of care purchased or the number of children placed in care.

Then a regression of labor supply on child care price is equivalent to a regression of child care quantity on child care price. Now imagine grouping the data by geographic market and running the grouped-means regression; the expected value of the resulting coefficient estimates is equal to that for the microdata regression.[19] If we suppose also that no other people purchase child care, then we are simply regressing equilibrium quantity of a good on equilibrium price, which is the classic example of simultaneous equations bias.

There is clearly nothing crucial about the simplifying assumptions made in the previous paragraph. In fact, adding consideration of quality or economies of scale (in either the number of children placed in care more the number of hours purchased for a given child) would simply make identification harder.[20] Thus one must ask what is moving child care costs around without independently affecting labor supply. The endogeneity of expenditures seems to be a severe problem for researchers attempting to identify child care effects from nonexperimental cost data.

It is worth describing the ideal setting in which empirical estimates could be generated. In a perfect world, one would randomly assign child care prices to sample members and simply compare those facing high prices to those facing low prices. Absent such an experiment,[21] one needs an instrumental variables strategy. To be sure, nearly all of the papers discussed above have employed some sort of instrumental variables strategy, as they predict child care costs using specifications that included variables not included in their labor supply equations. However, it is fair to say that the instruments typically used (e.g. region dummies and household members' or children's age structure variables) are either unlikely to be strongly related to child care costs or are unlikely to pass the exclusion restriction for the labor supply equation. One promising exception would seem to be using state-level rules and regulations of the child care industry (see Kimmel [1996], for example). However, such variables typically have very weak effects in the child care cost equation.

The fact is, no one has yet found a good instrument for prices. A primary advantage of using

[19] See, for example, Greene (1993).

[20] For example, authors typically do not have price data, but instead observe only household expenditures on child care. Moreover, hourly prices are unlikely to be constant, because of both economies to scale and variations in quality purchased. A standard approach is to use expenditures per hour of work (see the discussion of Kimmel (1996) in the text above), which may be written as $q_i p(q_i)$. If child care prices are nonlinear in the quantity of child care purchased—and most studies suggest that they are—then expenditures per hour will be $p(q_i)$, which is a nondegenerate function of quantity purchased (at the individual level), causing another source of endogeneity bias.

[21] None has yet been conducted to my knowledge, though Robins and Spiegelman (1978) do use data from the Seattle-Denver Income Maintenance Experiments to estimate a child care demand equation. In their sample, people face differing Federal marginal tax rates. Since child care was a deductible expense, Robins and Spiegelman argue that they can identify their demand equation. Such data clearly cannot identify a labor supply equation, however, since there is no way to separate the effects of lower marginal tax rates *per se* from the effects of lower child care costs.

access to public schooling, then, is that there is no need to find such an instrument. Access to public schooling affects required expenditures on child care because public schooling is free. In turn, quarter of birth variables are available as instruments for enrollment because affects access to public schooling in ways unlikely to be correlated with labor supply outside of its effect on access to schooling. Thus estimates generated using quarter of birth instrumental variables work for the same basic reasons that a random assignment experiment would.

3 Model Specification and Identification

Focusing on differential access to free public schooling for five-year-olds allows me to circumvent the identification problems discussed in the previous section. Public school offers a good source of exogenous variation in child care costs for two reasons. First, public schooling is available to every U.S. child of sufficient age. Second, it is free: parents of school-aged children need not pay for child care during school hours. Thus access to free public schooling is like having free (quality-adjusted) child care for several hours a day.[22]

Thus public schooling offers a potentially promising solution to the identification problems evident in previous child care studies. But there is another way in which using variation in access to public schooling improves on the existing literature. Authors often have been interested in the effects of a 100% subsidy for child care expenditures. They address this question by estimating models of the sort described in the previous section, after which they predict labor supply with estimated child care expenditures set to zero.[23] As no previous paper has used a source of freely provided child care, this approach typically involves predicting far out of sample with respect to child care costs.

While access to free public schooling is a promising source of exogenous variation in child care costs, there are two reasons why one cannot simply compare labor market outcomes among single mothers with and without preschool-aged children. First, even with identical preferences and under otherwise identical circumstances, two women's labor supply and welfare participation decisions may vary directly as a function of their children's ages. To take an obvious example, labor

[22]Because I have been unable to find comprehensive and reliable data regarding full- versus part-day kindergarten policies, I do not attempt to control for length of kindergarten day. Thus my estimates average over districts with full-day and districts with part-day kindergarten. Provision of free full-day care therefore would likely have larger effects on labor supply than those I estimate here.

[23]Connelly (1990), Blau and Robins (1988), and Michalopolous et al. (1992) are examples.

supply decisions of a woman with a newborn and the same woman with an 18-year-old are likely to be quite different,[24] quite apart from any effects related to child care costs.

Moreover, other things equal, the population of women with children aged less than five is likely to differ in systematic ways from the population of women with only older children. For example, women with higher wages face greater opportunity costs from withdrawing from the labor force. Thus they may be less likely to have children than women with lower wages. As a result, a 30-year-old woman with a seven-year-old and a four-year-old may not be comparable to a 30-year-old woman who has only a seven-year-old, because the latter woman may have chosen not to have another child for nonrandom reasons. Labor supply and welfare participation differences between these two women therefore may be due partly to differences in child care costs, but also to differences in unobservable factors related to ability and/or offered wages. I address this problem by focusing on only those women who have a five-year-old child; for this and other reasons described below, I split the sample into women whose youngest child is five and women who also have a child aged younger than five.

The second problem with simple comparisons between mothers whose children are enrolled in public school and those whose children are not is that, depending on their five-year-old's date of birth and state of residence, parents have discretion over their children's age at enrollment.[25] Under such circumstances, a mother's choice to enroll her child may be correlated with unobservable factors that also affect her labor supply, raising potential endogeneity concerns.

For example, one might expect a mother who wishes to work in the labor market to enroll her five-year-old precisely because doing so avoids child care costs. Mothers less interested in working will enroll their children less frequently, so that the population of women enrolling their children is simply more attached to the labor market than the population of women not enrolling their children. In this case, OLS estimates would be upwardly biased.

Another potentially more serious problem arises because some mothers will enroll their children in private school, rather than public school. Suppose for the moment that private schools have the same age-at-entry rules as public schools. Then if mothers who enroll their children in private school differ systematically from those who enroll their children in public school, a regression of labor supply on a public school enrollment dummy will yield biased estimates. For example, mothers

[24] Of course, it might be difficult to imagine the appropriate counterfactual comparison, since it is unlikely that a woman's preferences regarding labor supply and welfare participation remain constant over such a long time period.

[25] Nearly every state has a maximum age by which children must enter school; this age is generally one to two years older than the minimum allowable entry age. Parents of children who meet the minimum age for entry, but who are not yet old enough so that they are required to enroll, decide whether to enroll their children.

enrolling their children in private school are likely to have higher wages than those enrolling their children in public school (simply because of the cost of private school). This effect suggests that OLS estimates will be biased downward, and will even possibly be negative.

In fact, the problem is made worse because private schools need not have the same policy on age-at-entry. The Census enrollment question includes nursery school as a type of school enrollment. Since many mothers may consider daycare centers to be nursery schools, or may use nursery schools as sources of both daycare and early childhood education, I will be unable to distinguish private nursery school enrollees from children enrolled in daycare centers. Since many working single mothers enroll in daycare centers children not yet eligible for public school, we would expect mothers whose children are enrolled in nursery school to have greater attachment to the labor force than those whose children are not.

Clearly, simple comparisons across mothers whose children are enrolled in public school and those whose children are not will fail to identify the effect of providing access to public schooling. This is the reason why the instrumental variables approach is necessary: by affecting who has access to public schooling for reasons otherwise unrelated to mothers' labor market decisions, five-year-old's quarter of birth allows identification of the treatment effect of interest.

This approach works because most states have cutoff dates by which a child must have been born in order to start school when five. Table 1 shows 1975 minimum entrance ages for kindergarten by state. As the table shows, 1 state had a first-quarter cutoff, 14 states had third-quarter cutoffs, 25 states had cutoffs in the fourth quarter, none had second-quarter cutoffs, and 11 (including the District of Columbia) had no state-wide cutoff.[26]

QOB variables were first used as instruments by Angrist and Krueger (1991) in a study examining the return to years of educational attainment. Because minimum age for dropping out of school varies by state (mostly between states that have age-16 requirements and those that have age-17 requirements), and because QOB affects when children can start school, Angrist and Krueger are able to demonstrate a relationship between educational attainment and quarter- and state-of-birth (SOB).[27] Angrist and Krueger (1991) also report specifications that use both QOB dummies and QOB×SOB dummies as instruments for educational attainment, while controlling for main SOB effects.[28]

[26]For the purposes of discussion, I consider states whose cutoffs occur on the first day of a quarter to have cutoffs in the previous quarter. These states are Alabama and Arkansas, which have October 1 cutoffs, and Arizona, Florida, and Mississippi, which have January 1 cutoffs.

[27]Angrist and Krueger (1992) focus on this relationship.

[28]Their study is noteworthy not only because of the novel choice of instruments, but also because its IV estimates

Angrist and Krueger's (1991) use of QOB and SOB variables has been criticized on two primary grounds. First, Bound, Jaeger, and Baker (1995, henceforth referred to as BJB) argue that QOB variables do not satisfy the conditions required of instrumental variables. In particular, they argue that Angrist and Krueger's 2SLS estimates suffer from omitted variable bias. For example, they argue that QOB is correlated with health conditions (e.g. schizophrenia) that plausibly are related to wages outside of any correlation with educational attainment.[29] Thus two stage least squares (2SLS) estimates will confound the structural relationship of interest and the impact of these other, omitted, factors related to both QOB and wages.

The second critique is statistical, rather than substantive, in nature. BJB point out that the relationship between educational attainment and QOB and SOB variables (i.e. the first stage) in Angrist and Krueger's data is very weak. In finite samples, 2SLS estimates are known to be biased towards OLS estimates, and BJB argue that this bias grows as the first stage relationship – measured by the F-statistic testing joint exclusion of the instruments from the first stage equation – grows weaker. Moreover, the problem is made worse as the number of instruments is increased.[30] Because Angrist and Krueger (1991) use as many as 180 instruments in specifications they report, and because the first-stage F-statistics in these specifications are very low (e.g. 2.43) relative to the rule-of-thumb minimum levels (5 to 10, depending on the number of instruments) suggested by BJB and Staiger and Stock (1997), the validity of Angrist and Krueger's estimates has been challenged.

The 2SLS estimates presented below are not subject to the problems just described, for two reasons. First, QOB variables do an extremely good job of predicting enrollment status of five-year-olds. The relevant F-statistics (i.e. those testing joint exclusion of the QOB dummies from the equation predicting five-year-old's enrollment) are in the several hundreds. Second, I focus on specifications using no more than three instruments, dummies for whether a five-year-old was born in the second, third, or fourth quarter of 1974. Thus, neither the weak first-stage nor the many instruments critique applies to the results presented here.

However, for purposes of comparison, I also present results generated using several alternative

exceed its OLS estimates. This finding suggests either that the coefficient on educational attainment in standard wage equations is not affected by endogeneity bias resulting from unobservable ability or that measurement error in reported educational attainment outweighs any such bias.

[29] Research on the role of QOB on psychological disorders includes Rezaul, Persaud, and Takei (1996) and Dassa and Azorin (1993). Demographers and population economists have considered causes of seasonality of birth patterns (e.g. Lam and Miron [1996] and Lam, Miron and Riley [1994]).

[30] A similar point has been made by Staiger and Stock (1997) in a somewhat different econometric framework.

instrumental variables estimators[31] in specifications in which I added 150 QOB×SOB variables to the instrument set. Not surprisingly, given the strength of quarter of birth in predicting public school enrollment of five-year-olds, the resulting estimates were similar to those generated using only the three QOB main effects.

As for the substantive critique lodged by BJB, QOB variables do a similarly good job of predicting five-year-old's enrollment among both mothers whose youngest child is five and those who have both a five-year-old and a younger child (see section 5). However, there is no reduced form relationship between the QOB dummies and outcome measures for mothers whose youngest child is younger than five. This finding supports the validity of QOB instruments because independent (of enrollment) effects of five-year-old's QOB on mother's outcome measures should show up whether or not the mother also has a younger child.

3.1 Specification

The models I estimate all have the following basic structure. The unit of observation is the mother, while the structural equation of interest, i.e. the second stage, is

$$y_i = \beta_0 + \beta_1 \text{enrollment}_i + \beta_2 \text{X}_i + \epsilon_i, \tag{1}$$

where y_i is the value of the outcome measure for woman i, $enrollment_i$ is a dummy variable indicating whether the woman's five-year-old is enrolled in public school, X_i is a vector of control variables, and ϵ_i represents unobservable characteristics, which may be correlated with $enrollment_i$. The coefficient of interest is β_1, which is unidentified by the OLS estimator if ϵ_i and $enrollment_i$ are correlated. It is important to understand that equation (1) has a causal interpretation. That is, while one could run the OLS regression corresponding to equation (1), one is unlikely to uncover the underlying behavioral relationship between $enrollment_i$ and y_i if $enrollment_i$ is correlated with ϵ_i.

[31] The original Angrist and Krueger (1991) paper spawned something of a cottage industry both in generating alternative estimates of the returns to schooling using QOB/SOB variables and in formulating new estimators designed to overcome the many-instruments and weak-first stage critiques. For example, Staiger and Stock (1997) show that LIML appears to be robust to large numbers of instruments, while Angrist and Krueger (1995) and Angrist, Imbens, and Krueger (1996) construct alternative estimators in the spirit of 2SLS whose purpose is to eliminate the relationship between the first- and second-stage residuals. Ironically, these more robust approaches generally yield larger estimates of the education-earnings relationship than the original Angrist and Krueger (1991) ones (though the more recently generated estimates typically have significantly wider confidence intervals than the original ones).

Thus the first stage (i.e. the reduced form for the public school enrollment status of the five-year-old) is

$$\text{enrollment}_i = \delta_0 + \delta_z Z_i + \delta_x X_i + \eta_i, \tag{2}$$

where Z_i is a vector of instrumental variables, δ_z and δ_x are coefficient vectors, and η_i represents unobservable characteristics affecting enrollment decisions. The fundamental identifying assumptions are that $\delta_z \neq 0$ and $E[Z_i'\epsilon_i|X_i] = 0$. That is, given that quarter of birth variables affect enrollment status, β_1 is identified if quarter of birth is not otherwise associated with y_i.

In the estimates reported below, Z_i includes either a dummy variable for being born in the first quarter or a set of dummy variables for being born in each of quarters 2-4, depending on the model specification. The vector of controls, X_i, is either empty or includes variables specifying race, age, and education of the mother; characteristics regarding the age structure of both the mother's own children living in the household and other household members; and fixed effects for state of residence at completion of the Census questionnaire and for the five-year-old's state of birth.

I examine seven outcome measures: weeks of work in 1979, usual weekly hours in 1979, employment in 1979, wage and salary income in 1979, whether public assistance was received in 1979, hours of work last week, and labor force participation last week. My enrollment variable refers to the five-year-old's status as of the week before completion of the questionnaire.[32] Because a child would have had to enroll at the beginning of the school year (September 1979) in order to be enrolled in public school in March 1980, the enrollment variable is appropriate for analyzing 1979 as well as 1980 outcome measures.

Nonetheless, 1979 outcome measures raise an interpretation problem, since even an eligible five-year-old in 1980 generally will have had access to public schooling only for the months of September through December. One would expect that estimates using 1979 labor supply variables will be lower than what one would expect over a full year from providing free child care.

[32]Not all respondents receive or complete the questionnaire at the same time, although April 1 of the Census year is typically considered the reference day by the Census Bureau. However, both Passover and Good Friday occurred in the first week of April, 1980, which could have affected responses regarding hours of work last week. However, since neither of these holidays lasts an entire week (at least for the purposes of school and work attendance), occurrence of these holidays is unlikely to affect labor force participation last week as much, and seems unlikely to affect five-year-old's enrollment status at all.

4 Data

I use data from the 1980 5% Public Use Microdata Sample (PUMS). Ideally, I would use more recent data. Unfortunately, the 1990 Census did not ask respondents for information regarding quarter of birth,[33] making the 1980 PUMS the most recent Census data containing QOB.

My main dataset consists of all single mothers[34] in the 1980 5% PUMS who are younger than 50 years old and have one five-year-old child;[35] women in the sample may have any number of children of other ages as well. In order to be five years old on April 1, 1980 (the questionnaire's reference date for the age question) a child must have been born between April 2, 1974 and April 1, 1975. Thus five-year-olds born in the second quarter (April-June) are the oldest ones in my sample. Third-quarter births are next oldest, followed by fourth-quarter births. Five-year-olds born in the first quarter of 1975 are the youngest ones in my sample.

The sample contains 21,488 women, whom I divide further into two mutually exclusive subpopulations. The first subpopulation contains 13,273 observations and consists of mothers whose youngest child is five. The remaining 8,215 observations are single women who have a five-year-old but whose youngest child is younger than five.

Estimating separate models for these two groups makes sense because mothers of five-year-olds whose youngest child is younger than five must still arrange child care for the younger child if they wish to work, even when their five-year-old is attending free public school. By contrast, mothers whose five-year-old is their youngest child and is enrolled need not pay for child care, at least not during school hours. Thus the sample of mothers whose youngest child is younger than five can serve as a rough control group for those whose youngest is five. That is, if public school plays a significant child care role, we would expect a larger effect of enrollment on mothers whose youngest child is five. Estimates for mothers whose youngest child is younger than five therefore provide a

[33]The documentation for the 1990 PUMS explains that this change from previous Censuses was made so that the Census Bureau could ask directly for year of birth, which could then be used to check for the accuracy of age data. Since census data pertain to April 1 of the census year, data on year of birth would allow me to separate out first-quarter births from second- through fourth-quarter births. Unfortunately, the year of birth variable is not available on the PUMS, so that one needs a special extract to be made in order to use this information. I am currently attempting to obtain funding to have such an extract made.

[34]I define a single mother as the reference person living in a household described in the PUMS as a "Family household with female householder, no husband present". The sample does contain some women (412 whose youngest child is five and 393 whose youngest child is younger than 5) who report being currently married and not separated. I choose to include these women, since they report that their husbands are not present.

[35]I drop those women having more than one five-year-old in order to maintain comparability. While twins offer another interesting source of variation, fewer than 500 single mothers reported having more than one five-year old child.

specification check on estimates computed for mothers whose youngest child is five.

However, it is important to realize that this difference between the subsamples is not the only one. By definition, women who have a five-year-old, but whose youngest child is younger than five, must have at least two children. Women whose youngest child is five may have more than one child, but they need not. This difference leads to significant demographic differences between the two subsamples, as I discuss in the next subsection.

4.1 Summary statistics

Table 2 shows variable means and standard errors for women whose youngest child is five. The first row of the table suggests a large role for QOB in determining enrollment status of a five-year-old: about four-fifths of the children born in quarters two and three are enrolled, while 55% of fourth quarter births are enrolled and just 34% of first quarter births are enrolled. The second row of the table shows the private school enrollment rate. The private school enrollment rate among fourth- and first-quarter births is relatively higher than that for second- and third-quarter births, reflecting the problems described above.

Women whose five-year-old was born in the first quarter worked significantly fewer weeks and earned considerably more wage income than those whose children were born in later quarters. Mothers of second-quarter births also worked more weeks and earned more than mothers of third and fourth quarter births. Similar, though less impressive differences, are present for the hours, employment, receipt of public assistance, and labor force participation variables.

Notably, the pattern of outcome variables across quarter of birth categories is largely monotonic, with women whose five-year-old was born later working less and receiving public assistance more on average. If child care costs are driving the results I discuss below, then this pattern is to be expected given the monotonic and positive relationship between five-year-old's QOB and five-year-old's public school enrollment status.

Maximum monthly benefits from AFDC and Food Stamps,[36] in hundreds of monthly current dollars, are quite similar across five-year-old's QOB. The table shows no other apparent systematic differences in observable demographic variables across quarter of birth categories.

Table 3 shows means and standard errors across quarter of birth of the five-year-old among

[36]Maximum benefits are computed as $A + (F - 0.3A) = 0.7A + F$, where A and F respectively are the state's maximum AFDC payment and the maximum Food Stamps benefit for a family of the appropriate size. The formula is computed as described because the Food Stamps program implicitly taxes AFDC benefits at a 30% rate.

women whose youngest child is younger than five years old. I have argued that these women may function as a rough control group. Hence a potential source of concern is that, when QOB comparison groups are chosen selectively, outcome variables generally respond in a manner similar to that described for mothers whose youngest child is five. For example, mothers of first quarter (1975) births reported usual hours of 17.07, while mothers of second quarter (1974) births reported 18.36; the difference of 1.29 was statistically significant at the .05 level. Moreover, this difference was larger in both absolute and relative terms than the corresponding difference among mothers whose youngest child was five (i.e. those described in Table 2).

However, the relationship between outcome variables and QOB is clearly nonmonotonic – outcomes look similar for mothers of third and first quarter births, and also are similar for second and fourth quarter births. Thus differences arise mainly in comparing mothers of either first or third quarter births with mothers of either second or fourth quarter births. This pattern suggests that the women in Table 3 do not respond to their five-year-old's QOB in the way that women whose youngest child is five do. Moreover, I am unable to think of any economic reason why such a pattern should arise. I will return to this issue in the next subsection.

Patterns in the demographic variables across these categories generally mirror those observed for women whose youngest child is five. However, women in this sample clearly are different from women in the first sample in several ways. First, the sample of women with children younger than five is about three years younger on average than those without younger children. Given that by construction these women have younger children than the women in the first group, this fact is not surprising.

Second, by comparison to single mothers whose youngest child is five, single mothers having a five-year-old and a child younger than five have about one more child aged younger than 18. This finding is purely mechanical: since all women in each sample have a five-year-old child, women with a child aged younger than five must have at least two children. By contrast, women whose youngest child is five need not have another child. The apparent difference in maximum welfare benefits seems also to be an artifact of this mechanical difference in family size. Among women whose youngest child is five and who have more than one child, benefit levels actually are slightly greater than among the women summarized in Table 3.[37]

The third difference is the larger percentage of nonwhites in the sample of women having a child younger than five. Tabulations (not shown) of the race dummy among women whose youngest child

[37]The average 1979 benefit level among women whose youngest child is five and who have more than one child is $401.

is five and who have at least two children reveal that only three points of the thirteen percentage-point difference (in percentage white) between the populations is due to the difference in family size. However, this population is even older, by about a year and a half, than the population shown in Table 2. Thus as a descriptive matter, the difference in racial composition of the subpopulations depicted in the two tables seems to be due to variation by race and age in fertility rates. To the extent that nonwhites have less promising labor market opportunities than whites, these differences suggest that simple comparisons of outcome levels across the two subpopulations may confound the "treatment" effect of having a child younger than five and unobservable characteristics correlated with both having a young child and outcomes.

Fourth, private school enrollment rates of the woman's five-year-old are lower among women who also have younger children, particularly among first quarter births. This difference likely reflects the lower marginal value to women with children younger than five of enrolling their five-year-old. Lastly, these women clearly exhibit much lower levels of labor supply and much greater levels of public assistance participation than women whose five-year-old is their youngest child. This finding may reflect the greater child care costs faced by women having children younger than five. However, it is also likely the result of the differences in observable characteristics just described.

4.2 Effects of QOB on public school enrollment and outcome measures

The simple means reported in Tables 2-3 suggest both that QOB is strongly related to public school enrollment and that other variables potentially correlated with labor market outcomes (e.g. race and education) are not systematically related to QOB. These tables therefore suggest the validity of QOB variables as instruments for public school enrollment.

Table 4 proceeds with the analysis by establishing the first stage relationship in a regression framework and by demonstrating a clear reduced form relationship between QOB and labor market outcomes among single women whose youngest child is five. Results reported in Table 5 suggests that among women who also have a younger child, the first stage relationship is present, but labor market outcomes are unrelated to QOB. Each table reports OLS regressions of enrollment status and the outcome variables on three QOB dummy variables, demographic control variables, and fixed effects for state of residence and the five-year-old's state of birth.

Turning first to the sample to those women whose youngest child is five, public school enrollment status of five-year-olds living with these women is clearly greatly affected by QOB. Children born in quarters 2-4 are much more likely to be enrolled than those born in the first quarter. This effect is more than twice as strong for second- and third-quarter births (44.5–47.7 percentage points) as it

is for fourth-quarter births (21 percentage points). Moreover, these coefficients are approximately the same as simple differences in enrollment rates for children born in the given quarter and those born in the first quarter (see Table 2). Mother's age, age squared, years of education, and central city residence all have small but statistically significant effects on public school enrollment of the five-year-old. The large coefficient on race probably reflects racial differences in income as well as any cultural differences.

QOB variables also have a clear reduced form impact on the outcome measures among single mothers whose youngest child is five. For example, mothers of second-quarter births work about 2.4 weeks more than mothers of first-quarter births, while about half as large an effect is present among mothers of third- and fourth-quarter births. Employment in 1979 and last week's labor force participation are significantly greater (by about 3 points) among second-quarter mothers than among first-quarter mothers. Weekly hours also respond, with second- and third-quarter mothers working between 0.8 and 1.4 hours more per week, depending on the choice of hours variable and QOB.

Among mothers whose five-year-old was born in the second quarter, wage and salary income for 1979 was significantly greater–by $564–than it was among those whose five-year-old was born in the first. Mothers of third quarter births also had significantly greater wage and salary income. Lastly, having a second quarter birth reduced frequency of public assistance receipt by 2.6 percentage points. In nearly all cases, the QOB effect is monotonic, as should be expected if the effects are due to availability of public schooling.

Table 5 presents analogous estimates for mothers whose youngest child is younger than five. The effect of QOB on enrollment of these women's five-year-olds matches that in Table 4. However, Table 5 shows no consistent relationship of any kind between five-year-old's QOB and mothers' labor supply. No consistent pattern is evident in the sign of the QOB variables' coefficients, while these coefficients are almost uniformly statistically insignificant. I interpret these results as suggesting that the reduced form relationship between labor supply and five-year-old's QOB among women whose youngest child is five reflects child care costs, rather than simply effects due to differences in five-year-olds' age.[38] If women simply worked more when their children got older, such a relationship should also be present among women whose five-year-old is not their youngest child.

However, given the discussion in the previous subsection regarding the differences in outcomes across quarters among mothers whose youngest child is younger than five, the results in Table 5

[38]See Angrist and Evans (1996) and the discussion in the next section for more on this issue.

bear further investigation. If QOB is uncorrelated with other demographic variables, then including such variables should have no impact on coefficients of QOB dummies.

To try to isolate the reasons why results in Table 3 and Table 5 differ, I ran several alternative specifications of the reduced form regressions reported in Tables 4 and 5. First, I regressed the seven outcome variables on the QOB dummies, with no other variables included; I ran these specifications for both populations of women (i.e. those whose youngest child was five and those whose youngest was younger than five). Second, I added fixed effects for state of residence in 1980 and five-year-old's state of birth. Third, I dropped those fixed effects, instead including (only) the demographic controls included in Tables 4 and 5.

Among women whose youngest child was five, the results varied little, either in statistical significance or the pattern of estimated coefficient values, across reduced form specifications. By contrast, the significance of the estimated coefficients for women whose youngest child was younger than five did appear to vary across specifications. For the regression with neither fixed effects nor demographic controls, coefficients on the second quarter dummy were statistically significant at the .05 level for weeks of work in 1979, employment in 1979, and usual hours in 1979; the coefficient on the fourth quarter dummy was significant at the .10 level for the usual hours equation. In all four of these cases, the percentage effect (relative to the mean of the dependent variable) exceeded that for the analogous specification among women whose youngest child was five. However, in general, the nonmonotonic relationship between QOB dummies and outcome variables observed in Table 3 persisted.

When I added the fixed effects, the significance of the three second quarter coefficients (for 1979 weeks, employment, and usual hours) fell to the .10 level. Since the standard errors were virtually unchanged compared to the no-controls specification, this change resulted entirely from a (small) reduction in the estimated coefficient values. A similar change occurred for the fourth quarter dummy for usual hours, as it lost all significance. However, the estimated coefficients for these four dummies were clearly within sampling variance of those estimated in the no-controls specification.

Adding the demographic controls in lieu of the fixed effects eliminated all statistical significance of these four dummies (though the third quarter dummy for the weeks equation became significant and negative at the .05 level). Again, most of the change appears to have come from the coefficient levels rather than the standard errors. However, the estimated coefficient levels remain within sampling variance of those from the earlier specifications.

Thus it appears that most of the difference between Tables 3 and 5 occurs because of the demographic controls, although the difference could be due to sampling variance. While I would obviously prefer that the controls played no such role, I do not believe that this issue presents a

serious problem. First, if child care costs and public school enrollment were driving the differences in outcomes, then these differences should be monotonic across quarters. While that hypothesis is confirmed for mothers whose youngest child is five – regardless of the reduced form specification I use – it clearly is rejected for those whose youngest is younger than five.

Second, regardless of specification. only four of the 21 QOB dummy coefficients are ever significant among mothers whose youngest child is younger than five. Third, there is no *a priori* reason to believe that public school enrollment and QOB dummies should have zero effect on outcomes among women whose youngest child is younger than five; rather, one simply expects these variables to have a smaller effect in this population than among women whose youngest is five. Lastly, I ran several 2SLS regressions of the seven outcomes on the public school enrollment dummy, instrumenting with three QOB dummies. I tried including as exogenous right hand side variables no controls at all, only fixed effects, only the demographic controls, and the combination of fixed effects and demographic controls. The 2SLS coefficient on the enrollment dummy was smaller in magnitude than its standard error in all 28 cases, and the coefficients' sign tended to bounce around. I conclude that there is little evidence that female heads whose youngest child is five respond to access to public schooling for their five-year-old.

5 Effects of Access to Free Public Schooling on Outcomes

The previous section establishes that five-year-old's QOB may be used to construct credible instruments for access to free public schooling. In this section, I focus on female heads of households whose youngest child is five. I use QOB dummies to generate instrumental variables estimates of the structural effect of access to free public schooling on the labor market outcomes discussed above: weeks of work in 1979, usual weekly hours in 1979, employment in 1979, wage and salary income in 1979, whether public assistance was received in 1979, hours of work last week, and labor force participation last week. I also discuss in subsection 5.4 results for women who are either the reference person or the reference person's wife in married couple households; I present results for such women both when the youngest child is five and when the youngest is younger than five.

5.1 OLS and 2SLS results

Table 6 presents OLS and Two-Stage Least Squares (2SLS) estimates of equations using weeks of work in 1979 as the dependent variable; only women whose youngest child is five are included in

the equations for this table. Means of right hand side variables are shown in Column [1]. Column [2] offers a univariate OLS specification (i.e. no controls are included on the right hand side). The endogeneity and selection problems discussed earlier regarding the public school enrollment dummy are clearly displayed in this column: access to public schooling appears to have a *negative* effect on labor supply, reducing 1979 labor supply by 4 weeks of work.

Most of the negative effect can be accounted for by control variables, however, as demonstrated in Column [3]. The regression estimated in this column includes variables controliing for welfare benefit levels, age structure variables for own children and other people living in the household, and mother's years of education, age, race, and central city residence. Fixed effects for current state of residence and the five-year-old's state of birth also are included. The magnitude of the enrollment coefficient falls from about 4 to about 1, while its sign and standard error are unchanged. These results reflect the fact that enrollment of the five-year-old in public school is correlated with other observable variables that independently affect labor supply.

Column [4] presents Wald estimates of the same model. The Wald estimator is a special case of 2SLS with one endogenous right hand side variable, one binary instrument, and no other right hand side variables. The estimator is defined by separating observations according to their value for the instrument. Then one constructs differences in the means of the dependent variable of interest and the endogenous variable and takes the ratio of these differences, which yields the estimate. Thus the Wald estimator is simply the ratio of the coefficient on the instrument in the reduced form equation of interest and the coefficient on the instrument in the first stage. The estimator is useful in generating simple instrumental variables estimates that may be compared to their simple OLS counterparts (e.g. the estimate presented in Column [2] of Table 6).

The Wald estimate in Column [4] dramatically illustrates the effect of endogeneity and selection in determining public school enrollment: the estimate implies that access to public schooling actually leads to an increase in labor supply of 4.2 weeks of work. Moreover, this estimate is highly statistically significant.

Column [5] departs from the Wald specification by adding all controls used in Column [3] and using all three QOB dummies as instruments for enrollment of the five-year-old. The Column [5] estimate of 3.756 weeks is notable for several reasons. First, the estimated effect is statistically very significant–nearly four times its standard error.

Second, the estimate is economically significant. Predicting weeks of work using the Column [5] coefficients but setting the enrollment dummy equal to 0 for all sample members yields an average prediction of 25.986 weeks worked. Setting the enrollment dummy equal to 1 raises this average prediction by the size of the Column [5] coefficient, for a new predicted average of 29.742. Thus

providing access to public school enrollment for the five-year-old raises average weeks of work by 3.756/25.986 = 14.5%.

Third, unlike in the OLS case, adding controls did not affect the coefficient on enrollment appreciably—the reduction from 4.2 to 3.8 is well within the range of reasonable sampling error.[39]

Coefficient estimates for the other variables in these equations are consistent with previous labor supply studies and generally confirm predictions from economic theory.[40] Race, age, age squared, the central city residence dummy, and education of the mother all have signs typically observed in labor supply studies.

The interpretation of the children's age variables is somewhat subtle. Having another child aged 6-12 significantly reduces weeks of work (the coefficient in Column [5] is nearly -3), which should be expected, since these children will require after school child care.[41] Adding an own child aged 13-17 to the woman's household appears to have no effect on weeks of work, as its Column [5] coefficient is just -0.437, barely half its standard error. The number of own children aged 18 or older in the household has only a marginally positive impact on weeks of work (its coefficient of about 1.05 is significant at the ten percent level).

However, it would be incorrect to say that the 13-17 or 18+ categories are not important, since these coefficients are estimated conditional on the number of children in other age categories. Consider the impact of having a twelve-year-old instead be a thirteen-year-old: weeks of work would increase by 2.491 (having one less twelve-year-old increases labor supply by 2.928, while having one more thirteen-year-old reduces labor supply by 0.437 weeks). If instead, the twelve-year-old were replaced by an eighteen-year-old, weeks of work would increase by 3.974. Thus it is important to know not only a woman's *number* of children, but also their age structure.

Similarly, one needs to know not only the number of other household members, but also their ages. The number of other household members aged 18 or greater increased weeks of work by about 2. This effect is to be expected, because these family and household members may be able to care for young children. The number of other household members aged younger than 18 almost exactly offsets the effect of adult other household members, possibly because they add to household child

[39]It should be noted that the instrument set is different in the two cases, but eliminating this difference has little effect.

[40]It is interesting to observe that instrumenting for the enrollment variable has little impact on the coefficients of other variables included in the Column [3] OLS specification. This result underscores the fact that quarter of birth variables are uncorrelated with the controls (as Tables 2 and 3 demonstrated).

[41]Of course, family structure may be endogenous. If so, then all household age structure variables will pick up both the child care-related effects of household age structure and the correlation between these variables and unobservable characteristics of the mother.

care costs for women who wish to work. Thus adding two other household members could increase labor supply by four weeks, reduce it by four weeks, or have zero effect, depending on the new household members' ages.

The coefficient on the level of AFDC plus Food Stamps benefits, measured in hundreds of dollars, is -2.151 and is significant at the ten percent level. One unit of standard deviation for the estimation sample in Table 6 is $113, so raising maximum benefits by one unit of standard deviation reduces labor supply by 2.43 weeks, or about 8.6%.[42]

Table 7 reports the coefficients on the public school enrollment dummy obtained by estimating the specifications just described for all seven outcome measures among women whose youngest child is five.[43] The results reported in the table establish that access to free public schooling has an unambiguously significant effect on labor market outcomes for women whose youngest child is five. Moreover, the pattern across specifications mirrors that seen in Table 6: the simple OLS estimate is wrong-signed and large in magnitude; including controls greatly reduces the estimated magnitude, but without changing its sign; the Wald estimates are large and of the expected sign; and the Column [5] estimates using the controls and all three QOB dummies as instruments are similar to the Wald estimates.

In order to obtain an economically meaningful idea of the magnitude of the estimated effects, it will be useful to normalize the estimated effects by the mean of the predicted dependent variables, given that enrollment is set to 0. The result is the percentage change in the dependent variable that the model predicts by comparing the hypothetical world in which there is universal kindergarten access for all mothers in the sample with the world in which no one has kindergarten access.

Estimated percentage effects are presented in Column [6] of Table 6. These estimates imply that public schooling increased 1979 weeks of work by 14.5%; increased usual hours for 1979 by 11.5%; reduced the probability of welfare receipt by 12.8%; increased wage and salary income by 23.1%; increased hours worked in the week before filling out the Census questionnaire by 9.7%; and increased labor force participation in that week by 6.3%.

There are several things to note in these results. First, both employment and usual hours in 1979 appear to respond by more than labor force participation and hours of work for last week. As mentioned above, this finding could partly be the result of the timing of Good Friday and Passover,

[42]Because I use state fixed effects in all specifications including benefit variables, the coefficient on benefits is identified only by differences in family size. More precisely, the coefficient is identified by cross-state variation in differences in benefit levels by family size.

[43]The pattern of estimated effects for the controls in the equations for the six other outcome measures generally is very similar to that for weeks of work. Full tables of these estimates are available on request.

which occurred in the first week of April in 1980. Also, it seems likely that at least some children may have been on spring vacation. If there mothers reported them as being enrolled but took time off from work to care for them, then the estimated effect for last week variables should be expected to be smaller than the 1979 counterparts.

Second, wage and salary income responds a good deal more than the labor supply variables. Two possible explanations for this finding come to mind. First, it is possible that women receiving child care through public schooling are able to take jobs with longer hours, higher wages, or both. For example, a woman working in only part-time jobs is likely to earn less per hour than a woman working full-time; unless she works more than one part-time job, she will also work fewer hours than she would if she worked full-time. If there are fixed costs to working, or working full-time, then the number of hours of child care provided by public schooling need not be large to induce this sort of effect.

A different, but perhaps complementary, explanation is that women who receive free child care through public schools are more likely to report labor income. Since various components of the welfare system impose large implicit taxes (sometimes exceeding 100 percent) on labor income, participants have an incentive not to report income to the government. It is well-known that reported consumption in household surveys among welfare participants is far greater than reported income (see Edin and Lein [1997]), suggesting that women who earn income under the table may be hesitant to report that income to survey-takers.[44] If access to free child care causes women not to participate in welfare programs, they may be more willing to report income they would have earned even while participating in such programs.

Third, the results for 1979 variables are derived from provision of free public schooling only between the beginning of the school year in early September and the end of the calendar year. One way to correct for the underestimate would be to scale the 1979 variables by the fraction of the school year during which free public schooling was likely to be available for five-year-olds.

Assuming 17 weeks of free schooling suggests a scale factor of approximately 3. This scaling would lead to enormous effects–for example, an increase in yearly weeks of work of 43.5%. However, it is possible that women might take jobs in, say, July, knowing that they will have free child care beginning in September. Nonetheless, the estimates presented in this subsection certainly understate the actual effects that should be expected under provision of free, year-round child care.

[44]In fact, Edin and Lein's (1997) informal survey of low-income women finds that nearly all welfare recipients have at least some labor income that they do not report to their caseworkers.

5.2 Instrument validity

In assessing the results reported in this section, it is important to remember that their validity hinges on the validity of excluding the QOB dummies from the second stage (i.e. the outcome equations of interest). A simple alternative hypothesis seems plausible: QOB may affect mother's outcomes outside of its effects on five-year-olds' public school enrollment because QOB is directly (and linearly) related to five-year-olds' age. Angrist and Evans (1996) find that, among women aged 21–35 who have at least two children, presence of a third child reduces mother's labor supply. More importantly for present purposes, the effect declines with the third child's age until the third child turns 13.

While it is impossible to test whether or not each of the exclusion restrictions are individually valid, overidentification tests allow one to examine the hypothesis that they are jointly satisfied in a straightforward way. Thus I report in Column [5] of Table 6 a χ^2 overidentification test statistic. This statistic is computed by regressing the predicted residuals from the second stage on all the exogenous variables (i.e. the controls as well as the instruments). The product of the number of observations and the R^2 from this regression yields a statistic that is asymptotically distributed as χ^2 with degrees of freedom equal to the number of instrumental variables minus the number of endogenous variables.[45] If my results are driven by such an age-of-five-year-old effect, then this test statistic should differ greatly from zero, suggesting that the variables treated as exogenous jointly have too much power in explaining second stage predicted residuals.

The value of the test statistic in Column [5] of Table 6 is 5.7; using a χ^2 distribution with two degrees of freedom, the associated p-value is 0.059. Thus this test rejects exogeneity of the QOB dummies at significance levels of six percent or greater. χ^2 test statistics for the same specification using the other measures of labor supply were as follows: for usual weekly hours in 1979, 0.7; for wage and salary income in 1979, 1.3; for receipt of public assistance, 2.2; for employment in 1979, 1.0; for hours worked last week, 0.2; for labor force participation last week, 6.0. Of these six variables, the only one for which the overidentification test suggests any problems at conventional levels is labor force participation last week.

The results for weeks of work in 1979 and labor force participation last week suggest that there may be some effect of QOB dummies on these variables outside of their effects on five-year-old's enrollment. However, there is no obvious reason why the QOB dummies should be good

[45]The test is justified under the usual generalized method of moment conditions, provided that the estimator in use is asymptotically efficient (see Greene [1993]). In the present case, there are three QOB dummy instrumental variables and one endogenous variable.

instruments for the other dependent variables of interest but not for weeks or participation last week. Moreover, if age had an appreciable independent effect for women in my sample, then there is no reason why such an effect should appear only for women whose youngest child is five. The reduced form regressions in Table 5 suggest that among women who have a five-year-old but whose youngest child is younger than five, five-year-old's QOB has no impact on labor market outcomes.

Moreover, even though the summary statistics presented in Table 3 allow one to choose five-year-old's QOB selectively in order to show such a relationship, the pattern across quarters is clearly nonmonotonic. If aging were driving my results, then certainly one would expect greater levels of labor supply and earned income, and lower levels of public assistance receipt, as the five-year-olds in question become older.

As a further test, I examined the reduced form relationship between QOB dummies and mother's labor supply for a sample of single mothers whose youngest child was three years old (the criteria for inclusion in this sample were otherwise identical to criteria for inclusion in the main sample discussed in that text). I chose three-year-olds rather than four-year-olds because relatively few three-year-olds (12.8%) in this sample were reported enrolled in public school (compared to 27.9% of four-year-olds).[46] While the QOB dummies have a small and statistically significant impact on the likelihood of three-year-olds' public school enrollment (e.g. second quarter births are 10% more likely to be enrolled than first quarter births), they have no impact whatsoever on the labor market outcomes considered here. In no case is any QOB dummy statistically or economically significant in predicting labor supply, earned income, or receipt of public assistance among single mothers whose youngest child is three.

The foregoing arguments support the conclusion that simple age effects are unlikely to play a large role in generating the results reported in Tables 6 and 7.

5.3 Estimates using QOB × SOB instruments

In their study of the returns to education, Angrist and Krueger (1991) consider specifications of their model that use QOB-times-state of birth (SOB) variables as instruments for educational attainment levels. Their primary reason for doing so is to improve the precision of their estimates by improving the first stage fit (see Angrist and Krueger [1991] and BJB [1995]). As discussed

[46] It is not clear what kind of programs these children are attending. One possibility is Head Start; also, states and local areas may provide pre-kindergarten education, although I am unable to observe access to such programs when children are not enrolled in them.

above, the weakness of this first stage relationship has been the subject of much debate.

One result has been the finding that the limited information maximum likelihood (LIML) estimator is more robust than 2SLS when there are many weak instruments (see for example Staiger and Stock [1997] and Angrist, Imbens, and Krueger [AIK, 1995]).

AIK also develop two alternative estimators, which they term jackknife instrumental variables estimators (or JIVE). These estimators seek to eliminate finite sample correlation between the instruments' value for a particular observation and that observation's second stage residual. The operating principle is to run a separate first stage for each observation in the sample, with the ith observation excluded from the ith first stage regression.[47] The ith observation's vector of endogenous variable is then predicted using the resulting first stage coefficient estimates, and these predicted values are used as instruments for the observed endogenous variables according to the standard instrumental variables formula (see AIK or Greene [1993]). The idea is that eliminating observation i's vector of instruments from the procedure for predicting its endogenous variables should eliminate any finite sample correlation between the instruments and second stage unobservables for observation i. AIK present both theoretical and simulation-based evidence suggesting that their two variations on this theme, JIVE1 and JIVE2, perform favorably relative to both 2SLS and LIML when the number of instruments is large relative to the number of observations.

In the present case, QOB main effects are by no means weak predictors of public school enrollment, suggesting that 2SLS estimates are unlikely to be problematic. Moreover, the effects in Column [5] of Table 6 are quite precisely estimated: in no case are the coefficient estimates less than twice their standard errors; in four of the seven cases, the coefficient estimate is more than three times its standard error. However, it is perhaps interesting to examine results using 2SLS and the alternative estimators just discussed when the relationship between the instruments and the endogenous variable is strong.

In order to create the conditions most likely to cause divergent results, I estimated equations for each of the left hand side variables of interest using 2SLS, LIML, JIVE1, and JIVE2. Table 7 reports these results. All specifications are identical to those in Column [5] of Table 6 except for the addition of 150 QOB $\times$ SOB instruments. As expected, the 2SLS estimates have lower standard errors than those using only QOB main effects. However, the estimated coefficients are similar across the two instrument sets.

The 2SLS estimates are slightly smaller than the LIML estimates, but statistically there is no appreciable difference. There is also little contrast between the 2SLS and LIML standard errors.

[47] One need not actually run a first stage regression for each observation; see AIK for an exposition of the approach.

Similarly, the JIVE estimates are statistically indistinguishable from the LIML and either of the 2SLS estimates. The one noticeable difference occurs in the standard errors estimated for JIVE1. For several of the dependent variables, these estimates are substantially greater than those generated by the other estimators. However, given the overall precision of the estimates, this difference is not enough to have any meaningful effect on inference.

These results are unsurprising in light of the fact that the many instruments problem occurs only when the first stage relationship is weak. I conclude that when the first stage relationship is strong, estimates generated using many instruments are unlikely to differ much from those generated by simpler specifications, regardless of the estimator used.

5.4 Married women

In this subsection, I briefly discuss results generated using married women living in married couple families. A woman is included in the married sample if (i) she is either the household reference person (and her husband lives in the household) or if she is the wife of the reference person, and (ii) she meets all sample selection criteria described for single mothers above. The sample of married women includes 61,785 women living in married couple families whose youngest child was five, and 58,896 who had a five-year-old and a younger child (as before, women in the sample could have any number of children not aged 5).

In the interest of conserving space, I have chosen not to include tables of means for these two groups. However, as with female heads, means of demographic control variables show no systematic variation across five-year-old's QOB for either population. Patterns of enrollment rates in public school were generally similar to those for female heads, although fourth and second quarter birth five-year-old children of married women were substantially less likely to be enrolled in public school in public school. For example, among female heads whose youngest child is five, 55 percent of fourth quarter births and 34 percent of first quarter births were enrolled in public school. Among married women, the corresponding figures only 44 and 20 percent. There was virtually no difference in five-year-old's public school enrollment status (either across quarters or overall) between married women whose youngest child was five and those whose youngest was younger than five. However, both populations of married women enrolled their five-year-olds in private school more frequently than did female heads, and there is a clear increase across the quarters in private school enrollment rates.[48] Full tables of these summary statistics are available on request.

[48]For example, among married women whose youngest child is five, 14 percent of second quarter births were

I also computed reduced form regressions analogous to those presented in Tables 4 and 5 (these results are also available on request). Estimated coefficients from these regressions suggest a definite impact of five-year-old's QOB on outcomes among both populations of married women, though married women whose youngest child is five respond somewhat more than married women whose youngest is younger than five.

Tables 9 and 10 present estimates from OLS and 2SLS regressions analogous to those considered in Table 7; results in Table 9 are for married women whose youngest child is five, while those in Table 10 are for women whose youngest child is younger than five. Column [1] reports means of the dependent variables. Among married women whose youngest child is five, average labor supply and earned income are about 25 percent lower than among female heads whose youngest is five (see Table 2). These averages are also lower among married women whose youngest child is younger than five compared to levels for the corresponding sample of female heads. Among both populations of married mothers, receipt of public assistance is nearly zero, suggesting that most public assistance reported in the female heads sample likely involves categorical eligibility tests.

The OLS results in Tables 9 and 10 generally would suggest that labor supply of married women does not respond to access to public school for the five-year-old (although wage and salary income appears to be lower when public school is available). However, both the Wald and more general Column [5] estimates suggest that while there was no impact on receipt of public assistance, married women's labor supply and earned income are increased by access to public school for the five-year-old. By contrast to the results for single mothers, there is little difference in the size of the responses between married mothers whose youngest child is five and those whose youngest is younger than five. For example, access to enrollment increased weeks of work in 1979 by 7.2 percent among mothers whose youngest was five, and 7.0 percent among mothers whose youngest was younger than five.

In all cases except labor force participation last week, percentage effects in each of the married populations were smaller than those among single mothers whose youngest child is five. Since mothers living in married couple families generally have access to other income (i.e. husbands' earnings), these findings suggest that the responsiveness of female labor supply to child care costs is declining in income. Of course, there are likely to be many other differences, both observable and unobservables, between female heads and married mothers living with their husbands, so this conclusion is not necessarily written in stone.

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enrolled, compared to 32 percent of first quarter births; among married women whose youngest child was younger than five, these figures are 14 and 28 percent, respectively.

6 A rough cost-benefit analysis

Fortunately, a rough cost-benefit analysis does not require accurate estimates of the full-year treatment effect. The Department of Education provides statistics on average expenditures per day per pupil in attendance in U.S. public schools. For the 1979-1980 academic year, U.S. schools spent an average (in current dollars) of $2,491 per pupil in attendance (see U.S. Department of Education [1980]).[49] Estimated effects for 1979 variables result from provision of free public schooling from the beginning of the school year through the end of the calendar year. Assuming that the school year begins on September 1 and lasts ten months yields a total schooling cost for Fall 1979 of $\$2,491 \times 0.4 = \$996.40$.

This figure may misstate the relevant cost for two reasons. First, children in kindergarten often attend only part of the day, suggesting that the figure overstates the cost of providing kindergarten. Second, and potentially more importantly, the costs of expanding access to public schooling to children younger than those of kindergarten age may be quite different from the average cost of educating children currently in school. Capacity would have to be added to accommodate larger numbers of enrollees; the resulting costs would include both one-time construction and ongoing maintenance. Also, it seems unlikely that kindergarten class sizes would be deemed appropriate for younger children, so that more staff would be required. One would also like to determine whether provision of kindergarten costs more or less than provision of the average education, holding constant the number of hours and class size.

As for benefits, there is no obvious way to assess the value of the schooling the children actually received.[50] However, one can determine the increase in mother's income expected as a result of access to free child care. It is important to remember that some of this income is returned to various levels of government through the tax system. Determining how much is not easy, however. Because average earnings in the sample are so low—about $7,547 among those women with positive earnings—it is hard to know what the appropriate marginal and average tax rates should be. Moreover, the Earned Income Tax Credit would have to be taken into account.

Another problem arises because added earned income will reduce public transfer receipts for women on a variety of programs. Without knowing more about which programs sample members

[49]This figure includes expenditures on people costs, capital outlay, and interest on school debt, all of which are relevant costs for considerations of the long-run cost of providing schooling.

[50]Mayer (1997) argues that facets of childhood other than household income, e.g. the quality of educational experiences a child has, have a much larger impact on children's outcomes than does income. Thus the figures relating to mother's income described in the text above may fail to capture an important source of benefits to children themselves.

participate in, it is impossible to compute the savings to any of these programs. In any event, added tax liabilities and reduced public assistance payments simply represent transfers from sample members to various governmental entities. Thus estimated increases in labor income may still be used for cost-benefit purposes, so long as it is understood that the results of such analysis refer to total benefits; one cannot draw conclusions regarding the benefits specifically received by single mothers and those specifically received by taxpayers via the government. Lastly, if some of the increase in labor income is due to nonreporting of income by women without access to free child care, then presumably offering care will increase tax revenues by more than the increase in actual income. Public assistance payments will also decline by more than what they would under the assumption that all labor income increases are real, rather than reporting-based.

With all of these caveats, any cost-benefit analysis is necessarily tenuous. However, one can gain a rough idea using the $996.40 cost figure and the estimates reported here. The results in Table 7 suggest that among single mothers whose youngest child is five, access to free schooling raises labor income by an average of $1042.14 per mother in the sample. Using this estimate, each dollar spent providing public schooling for a five-year-old who is her mother's youngest child returns $1.05 in additional labor income earned by the mother.[51] Given the current desire of policymakers and voters to move single mothers off of welfare and into the workplace, this price does not seem particularly steep. These findings confirm the view that provision of free child care (of kindergarten-quality) likely is a relatively cost-effective way of redistributing resources to single mothers while increasing their labor supply, provided that child care is available for all preschool-aged children.

7 Conclusion

In this paper, I have argued that access to free public school for a woman's five-year-old has a large impact on the child care costs she must bear in order to work. OLS estimates of the effect of five-year-old's enrollment status on single mothers' labor supply suggest a large degree of endogeneity of enrollment. A variety of instrumental variables estimates provide evidence that access to public schooling for the five-year-old has a strong effect on labor market outcomes among mothers whose youngest child is five years old. No effect is apparent among mothers of five-year-olds whose youngest child is younger than five, supporting the view that the marginal child care effect is being

[51] One should keep in mind that if some of the increase in labor income is due to increased reporting of existing income, as discussed above, then this measure will overstate the actual increase in labor income.

identified among women in the first group.

The results imply that, among single mothers whose youngest child is five, access to free public schooling for the five-year-old during Fall 1979 increased labor supply for all of 1979 by between 8.9% and 14.5%. Wage and salary income for 1979 was increased by 23.1%, while 1979 receipt of public assistance was reduced by 12.8%. Results for labor force participation and hours of work in the week preceding completion of the Census questionnaire suggest somewhat smaller effects of 6.3% and 9.7%, respectively (though the timing of religious holidays and school vacations may have affected these estimates). Among married women, effects generally were relatively smaller, though still statistically and economically significant. Access to public schooling for the five-year-old had no impact on outcomes for single mothers who also had at least one child aged less then five.

Results from a back-of-the-envelope cost-benefit analysis suggest that child care subsidies may be an efficient way of redistributing resources and increasing labor supply of single mothers: each dollar spent on provision of public schooling appears to generate $1.05 in additional labor income for a single mother whose youngest child is five. Some part of this increase will be returned to governments via tax revenues and reduced expenditures on public assistance. Also, the figure may overstate the gain in labor income due to changes in reporting of earned income as earned income rises and welfare participation falls. However, this figure does not account for the value of the education received by five-year-olds enrolled in public schooling.

In any case, my results clearly imply that the provision of free public schooling has a large and statistically significant effect on the labor market outcomes of single women whose youngest child is of kindergarten age. Existing studies of child care costs and female labor supply commonly make the assumption that provision of a given level of child care quality has similar effects on labor supply, regardless of children's ages. Under this assumption, my results suggest that subsidies for child care expenditures or universal provision would have a large and positive impact on labor supply and wage income among single mothers, while reducing welfare participation.

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Table 1: 1975 State cutoff dates for school eligibility.

Alabama	10/1	Montana	9/10
Alaska	11/2	Nebraska	10/15
Arizona	1/1	Nevada	.
Arkansas	10/1	New Hampshire	.
California	12/1	New Jersey	.
Colorado	.	New Mexico	11/1
Connecticut	.	New York	12/1
Delaware	1/31	North Carolina	10/16
District of Columbia	.	North Dakota	9/30
Florida	1/1	Ohio	9/30
Georgia	9/1	Oklahoma	11/1
Hawaii	12/31	Oregon	11/15
Idaho	.	Pennsylvania	12/31
Illinois	12/1	Rhode Island	12/31
Indiana	.	South Carolina	11/1
Iowa	9/15	South Dakota	10/31
Kansas	9/1	Tennessee	9/30
Kentucky	12/31	Texas	9/1
Louisiana	12/31	Utah	.
Maine	10/15	Vermont	11/30
Maryland	.	Virginia	12/31
Massachusetts	.	Washington	12/31
Michigan	12/1	West Virginia	9/1
Minnesota	9/1	Wisconsin	12/1
Mississippi	1/1	Wyoming	9/15
Missouri	9/30		

Note: Children who are five years old on April 1, 1980, are eligible to begin school in 1979 if they were born before the cutoff date in 1974 if the cutoff date occurs in quarters 2-4 or the cutoff date in 1975 if the cutoff occurs in quarter 1.

Table 2: **Means and Standard Errors of Variables Used, By Quarter of Birth (Youngest child is 5)**

Variable	Full Sample	*Quarter of five-year-old's Birth* 74:II	74:III	74:IV	75:I
Public school enrollment of five-year-old	0.63	0.82	0.79	0.55	0.34
	(4.2E-03)	(0.01)	(0.01)	(0.01)	(0.01)
Private school enrollment of five-year-old	0.18	0.12	0.14	0.2	0.24
	(3.3E-03)	(0.01)	(0.01)	(0.01)	(0.01)
Weeks of work, 1979	28.21	29.29	28.27	28.3	26.98
	(0.2)	(0.41)	(0.38)	(0.4)	(0.4)
Employment, 1979	0.7	0.71	0.7	0.69	0.69
	(4.0E-03)	(0.01)	(0.01)	(0.01)	(0.01)
Usual weekly hours, 1979	25.59	26.1	25.89	25.32	25.04
	(0.17)	(0.34)	(0.32)	(0.33)	(0.34)
Hours last week	21.52	21.99	21.78	21.48	20.83
	(0.18)	(0.36)	(0.34)	(0.35)	(0.36)
Labor force participation last week	0.67	0.69	0.66	0.67	0.65
	(4.1E-03)	(0.01)	(0.01)	(0.01)	(0.01)
Received public assistance	0.34	0.33	0.34	0.35	0.35
	(4.1E-03)	(0.01)	(0.01)	(0.01)	(0.01)
Wage and salary income, 1979	5144.31	5429.53	5245.87	5066.61	4830.89
	(50.47)	(104.93)	(99.13)	(99.84)	(99.6)
Age of mother	30.32	30.74	30.39	30.36	29.79
	(0.05)	(0.1)	(0.09)	(0.1)	(0.1)
Mother's years of education	11.7	11.68	11.72	11.7	11.7
	(0.02)	(0.05)	(0.04)	(0.05)	(0.05)
AFDC plus Food Stamps, 1979	3.49	3.55	3.48	3.48	3.43
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
AFDC plus Food Stamps, 1980	3.8	3.87	3.79	3.78	3.74
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Live in central city	0.33	0.34	0.32	0.33	0.32
	(4.1E-03)	(0.01)	(0.01)	(0.01)	(0.01)
White	0.61	0.61	0.62	0.61	0.61
	(4.2E-03)	(0.01)	(0.01)	(0.01)	(0.01)
No. of own children in HH aged:					
0-5	1	1	1	1	1
6 – 12	0.78	0.8	0.78	0.78	0.77
	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)
13 – 17	0.27	0.3	0.28	0.27	0.22
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$\geq$ 18	0.07	0.07	0.08	0.07	0.06
	(2.9E-03)	(0.01)	(0.01)	(0.01)	(0.01)
No. of other HH members aged:					
< 18	0.07	0.08	0.07	0.07	0.07
	(3.2E-03)	(0.01)	(0.01)	(0.01)	(0.01)
$\geq$ 18	0.19	0.19	0.2	0.19	0.19
	(4.2E-03)	(0.01)	(0.01)	(0.01)	(0.01)
N	13273	3161	3588	3304	3220

Note: AFDC plus Food Stamps expressed in hundreds of current dollars per month.

Table 3: **Means and Standard Errors of Variables Used, By Quarter of Birth (Youngest child younger than 5)**

Variable	Full Sample	*Quarter of five-year-old's Birth* 74:II	74:III	74:IV	75:I
Public school enrollment of five-year-old	0.65	0.85	0.8	0.57	0.36
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Private school enrollment of five-year-old	0.1	0.07	0.08	0.13	0.13
	(3.3E-03)	(0.01)	(0.01)	(0.01)	(0.01)
Weeks of work, 1979	16.25	17.37	15.22	16.71	15.84
	(0.23)	(0.47)	(0.43)	(0.47)	(0.46)
Employment, 1979	0.49	0.52	0.48	0.5	0.48
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Usual weekly hours, 1979	17.57	18.36	16.81	18.11	17.07
	(0.22)	(0.44)	(0.41)	(0.45)	(0.44)
Hours last week	12.05	12.67	11.34	12.52	11.77
	(0.2)	(0.41)	(0.38)	(0.41)	(0.41)
Labor force participation last week	0.44	0.45	0.42	0.45	0.43
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Received public assistance	0.54	0.53	0.54	0.54	0.55
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Wage and salary income, 1979	2574.14	2695.76	2494.6	2628.12	2486.62
	(49.82)	(103.27)	(97.31)	(103.68)	(93.8)
Age of mother	27.22	27.41	27.35	27.34	26.75
	(0.05)	(0.1)	(0.1)	(0.11)	(0.11)
Mother's years of education	10.93	11.09	10.94	10.83	10.88
	(0.03)	(0.06)	(0.05)	(0.06)	(0.06)
AFDC plus Food Stamps, 1979	4.23	4.24	4.24	4.23	4.22
	(0.01)	(0.03)	(0.02)	(0.03)	(0.03)
AFDC plus Food Stamps, 1980	4.61	4.62	4.61	4.6	4.59
	(0.01)	(0.03)	(0.03)	(0.03)	(0.03)
Live in central city	0.38	0.36	0.37	0.37	0.41
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
White	0.48	0.51	0.47	0.48	0.45
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
No. of own children in HH aged:					
0-5	2.28	2.3	2.28	2.26	2.26
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
6 – 12	0.63	0.58	0.63	0.65	0.64
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
13 – 17	0.13	0.12	0.15	0.14	0.12
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$\geq$ 18	0.03	0.03	0.03	0.03	0.03
	(2.6E-03)	(0.01)	(0.01)	(4.2E-03)	(0.01)
No. of other HH members aged:					
< 18	0.07	0.06	0.06	0.07	0.08
	(3.9E-03)	(0.01)	(0.01)	(0.01)	(0.01)
$\geq$ 18	0.19	0.19	0.19	0.19	0.19
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	8215	1985	2236	2021	1973

Note: AFDC plus Food Stamps expressed in hundreds of current dollars per month.

Table 4: **First Stage And Reduced Form Regressions (Youngest child aged 5)**

				1979 Variables			*Last week variables*	
Variable	PS Enrollment	Weeks	Employed	Usl Hrs.	Wage/Sal	Pub Asst.	Hours	LFP
74:II	0.477	2.431	0.032	1.393	563.634	-0.026	1.061	0.033
	(0.011)	(0.529)	(0.011)	(0.446)	(137.275)	(0.011)	(0.474)	(0.011)
74:III	0.445	1.228	0.021	1.035	382.278	-0.013	0.783	0.005
	(0.011)	(0.511)	(0.01)	(0.431)	(132.578)	(0.011)	(0.458)	(0.011)
74:IV	0.21	1.297	0.008	0.341	190.576	3.1E-04	0.473	0.009
	(0.011)	(0.521)	(0.011)	(0.44)	(135.329)	(0.011)	(0.468)	(0.011)
AFDC plus Food Stamps:								
1979	0.018	-2.108	-0.041	-0.752	-671.756	-0.034		
	(0.025)	(1.184)	(0.024)	(1)	(307.397)	(0.025)		
1980							-0.632	-0.032
							(0.942)	(0.022)
No. of own children in HH aged:								
6 – 12	0.023	-2.823	-0.038	-2.485	-482.42	0.101	-3.096	-0.053
	(0.019)	(0.937)	(0.019)	(0.79)	(243.069)	(0.02)	(0.816)	(0.019)
13 – 17	0.016	-0.359	0.003	-0.671	-88.787	0.074	-1.302	-0.007
	(0.019)	(0.941)	(0.019)	(0.795)	(244.348)	(0.02)	(0.82)	(0.019)
≥ 18	0.014	1.114	0.012	0.4	86.72	-0.006	0.624	0.022
	(0.014)	(0.654)	(0.013)	(0.552)	(169.833)	(0.014)	(0.587)	(0.014)
No. of other HH members aged:								
< 18	0.007	-2.364	-0.027	-1.514	-514.253	0.029	-2.271	-0.056
	(0.011)	(0.527)	(0.011)	(0.445)	(136.884)	(0.011)	(0.473)	(0.011)
≥ 18	-0.006	2	0.038	2.107	336.749	-0.035	1.703	0.041
	(0.008)	(0.399)	(0.008)	(0.337)	(103.551)	(0.008)	(0.358)	(0.008)
Age of mother	-0.021	3.258	0.044	1.739	988.98	-0.039	3.221	0.057
	(0.006)	(0.311)	(0.006)	(0.262)	(80.67)	(0.007)	(0.279)	(0.007)
Age of mother squared	2.6E-04	-0.049	-0.001	-0.029	-14.068	0.001	-0.047	-0.001
	(9.9E-05)	(0.005)	(9.7E-05)	(0.004)	(1.241)	(1.0E-04)	(0.004)	(1.0E-04)
White	-0.114	3.078	0.063	2.534	401.779	-0.09	3.391	0.039
	(0.009)	(0.436)	(0.009)	(0.368)	(113.242)	(0.009)	(0.391)	(0.009)
Mother's years of education	-0.009	1.949	0.041	1.456	578.975	-0.043	1.586	0.039
	(0.002)	(0.078)	(0.002)	(0.066)	(20.179)	(0.002)	(0.07)	(0.002)
Live in central city	-0.029	-3.417	-0.084	-3.085	-368.069	0.059	-1.965	-0.071
	(0.009)	(0.445)	(0.009)	(0.376)	(115.552)	(0.009)	(0.399)	(0.009)
N	12925	12925	12925	12925	12925	12925	12925	12925

Note: Standard errors in parentheses. All specifications include fixed effects for state of residence and five-year-old's state of birth.

Table 5: **First Stage And Reduced Form Regressions (Youngest child aged less than 5)**

				1979 Variables			*Last week variables*	
Variable	PS Enrollment	Weeks	Employed	Usl Hrs.	Wage/Sal	Pub Asst.	Hours	LFP
74:II	0.498	0.326	0.015	0.425	-31.757	0.01	0.164	0.002
	(0.014)	(0.624)	(0.015)	(0.595)	(141.091)	(0.015)	(0.553)	(0.015)
74:III	0.445	-1.205	-0.011	-0.598	-112.9	0.005	-0.836	-0.021
	(0.014)	(0.604)	(0.014)	(0.576)	(136.601)	(0.015)	(0.536)	(0.015)
74:IV	0.218	0.246	0.012	0.631	35.128	-0.002	0.171	0.009
	(0.014)	(0.618)	(0.015)	(0.589)	(139.639)	(0.015)	(0.547)	(0.015)
AFDC plus Food Stamps:								
1979	0.016	-0.329	-0.01	1.178	-145.57	-0.008		
	(0.029)	(1.311)	(0.031)	(1.251)	(296.573)	(0.032)		
1980							0.099	-0.028
							(1.046)	(0.029)
No. of own children in HH aged:								
0-5	-0.031	-4.826	-0.113	-5.271	-650.628	0.089	-4.01	-0.103
	(0.023)	(1.04)	(0.025)	(0.993)	(235.261)	(0.025)	(0.909)	(0.025)
6 – 12	0.004	-2.715	-0.056	-3.335	-503.42	0.073	-2.363	-0.04
	(0.022)	(0.989)	(0.024)	(0.944)	(223.728)	(0.024)	(0.863)	(0.024)
13 – 17	-0.01	-0.854	-0.01	-1.643	-327.409	0.04	-1.351	-0.008
	(0.024)	(1.061)	(0.025)	(1.012)	(239.946)	(0.026)	(0.924)	(0.025)
$\geq$ 18	0.045	4.004	0.059	3.467	252.159	-0.016	3.205	0.066
	(0.024)	(1.05)	(0.025)	(1.002)	(237.442)	(0.025)	(0.931)	(0.025)
No. of other HH members aged:								
< 18	0.026	-0.722	-0.009	-0.277	168.244	-0.004	0.214	0.004
	(0.015)	(0.652)	(0.016)	(0.622)	(147.468)	(0.016)	(0.578)	(0.016)
$\geq$ 18	0.009	2.619	0.065	2.955	377.377	-0.025	2.082	0.046
	(0.011)	(0.47)	(0.011)	(0.448)	(106.219)	(0.011)	(0.416)	(0.011)
Age of mother	0.021	2.606	0.035	1.547	478.071	-0.034	1.901	0.039
	(0.009)	(0.401)	(0.01)	(0.383)	(90.672)	(0.01)	(0.355)	(0.01)
Age of mother squared	-4.0E-04	-0.038	-0.001	-0.025	-6.089	3.8E-04	-0.027	-0.001
	(1.5E-04)	(0.007)	(1.6E-04)	(0.006)	(1.504)	(1.6E-04)	(0.006)	(1.6E-04)
White	-0.115	2.012	0.044	1.916	315.644	-0.093	2.321	0.004
	(0.011)	(0.503)	(0.012)	(0.48)	(113.673)	(0.012)	(0.446)	(0.012)
Mother's years of education	-0.001	1.567	0.037	1.29	335.452	-0.036	1.289	0.039
	(0.002)	(0.094)	(0.002)	(0.089)	(21.152)	(0.002)	(0.083)	(0.002)
Live in central city	-0.021	-3.15	-0.109	-3.82	-322.723	0.085	-2.442	-0.075
	(0.012)	(0.513)	(0.01	(0.489)	(116.002)	(0.012)	(0.455)	(0.012)
N	7971	7971	7969	7971	7971	7971	7971	7969

Note: Standard errors in parentheses. All specifications include fixed effects for state of residence and five-year-old's state of birth.

Table 6: **Estimates Of The Effect Of Access To Public School On Weeks Of Work In 1979 (Youngest child is 5)**

Right Hand Side Variable	[1] Means	[2] OLS	[3] OLS	[4] Wald	[5] QOB
PS Enrollment of five-year-old	0.63	-3.971	-1.063	4.236	3.756
	(0.004)	(0.408)	(0.39)	(1.224)	(0.96)
AFDC plus Food Stamps	3.487		-2.022		-2.151
	(0.01)		(1.185)		(1.192)
No. of own children in HH aged:					
6 – 12	0.784		-2.866		-2.928
	(0.008)		(0.937)		(0.943)
13 – 17	0.267		-0.355		-0.437
	(0.006)		(0.942)		(0.948)
$\geq$ 18	0.07		1.141		1.046
	(0.003)		(0.655)		(0.659)
No. of other HH members aged:					
< 18	0.071		-2.342		-2.386
	(0.003)		(0.528)		(0.531)
$\geq$ 18	0.194		2.003		2.02
	(0.004)		(0.399)		(0.402)
Mother's years of education	11.7		1.939		1.981
	(0.023)		(0.078)		(0.079)
White	0.614		2.978		3.5
	(0.004)		(0.439)		(0.451)
Live in central city	0.327		-3.429		-3.294
	(0.004)		(0.446)		(0.449)
Age of mother	30.318		3.314		3.343
	(0.049)		(0.311)		(0.313)
Age of mother squared	951.602		-0.05		-0.05
	(3.221)		(0.005)		(0.005)
χ^2[df]					5.7[2]
p-value					0.059
F[df]				1719.3[1]	855.3[3]
State fixed effects		N	Y	N	Y
Demographic controls		N	Y	N	Y
Instruments		–	–	Q1 dummy	Quarter main
N		13273	12925	13273	12925

Note: Standard errors in parentheses.

Table 7: **Effect of Access to Public School On Labor Supply Measures (Youngest child is 5)**

Left-Hand Side Variable	[1] Means	[2] OLS	[3] OLS	[4] Wald	[5] QOB	[6] Percentage Effect
Weeks of work in 1979	28.208	-3.971	-1.047	4.236	3.756	14.5%
	(0.198)	(0.408)	(0.385)	(1.224)	(0.96)	
Usual hours in 1979	25.592	-3.103	-0.705	1.901	2.748	11.5%
	(0.166)	(0.343)	(0.325)	(1.02)	(0.809)	
Employment in 1979	0.697	-0.068	-0.009	0.039	0.059	8.9%
	(0.004)	(0.008)	(0.008)	(0.025)	(0.019)	
Received public assistance in 1979	0.343	0.089	0.032	-0.04	-0.048	-12.8%
	(0.004)	(0.009)	(0.008)	(0.025)	(0.02)	
Wage and salary income in 1979	5144.313	-1035.148	-453.558	1085.118	1042.139	23.1%
	(50.469)	(104.176)	(99.323)	(312.37)	(249.682)	
Hours last week	21.524	-3.543	-0.958	2.408	1.977	9.7%
	(0.176)	(0.363)	(0.344)	(1.084)	(0.858)	
Labor force participation last week	0.667	-0.062	-0.008	0.04	0.041	6.3%
	(0.004)	(0.008)	(0.008)	(0.025)	(0.02)	
State fixed effects		N	Y	N	Y	
Demographic controls		N	Y	N	Y	
Instruments		–	–	Q1 dummy	Quarter main	

Note: Standard errors in parentheses. The top number in each cell is the coefficient on the enrollment dummy in the equation estimated using the indicated row variable on the left hand side.

Table 8: **Alternative Estimates of the Effect of Access to Public School On Labor Supply Measures (Youngest child is 5)**

Left-Hand Side Variable	[1] Means	[2] 2SLS	[3] LIML	[4] JIVE1	[5] JIVE2
Weeks of work in 1979	28.208	3.142	3.494	3.362	3.336
	(0.198)	(0.892)	(0.889)	(1.284)	(0.91)
Usual hours in 1979	25.592	2.54	2.788	2.679	2.646
	(0.166)	(0.752)	(0.75)	(1.152)	(0.767)
Employment in 1979	0.697	0.056	0.06	0.059	0.059
	(0.004)	(0.018)	(0.018)	(0.031)	(0.018)
Received public assistance in 1979	0.343	-0.048	-0.055	-0.053	-0.054
	(0.004)	(0.019)	(0.019)	(0.017)	(0.019)
Wage and salary income in 1979	5144.313	970.206	1072.322	1028.501	1021.538
	(50.469)	(232.203)	(231.455)	(243.585)	(236.832)
Hours last week	21.524	1.875	2.098	2.035	2.062
	(0.176)	(0.798)	(0.795)	(0.993)	(0.814)
Labor force participation last week	0.667	0.042	0.045	0.044	0.044
	(0.004)	(0.019)	(0.019)	(0.03)	(0.019)

Note: Standard errors in parentheses. The top number in each cell is the coefficient on the enrollment dummy in the equation estimated using the indicated row variable on the left hand side. All specifications include three QOB main effects and 150 QOB times SOB interaction effects as instruments. All other variables included in Table 6, Column [5] specifications are included as RHS exogenous variables.

Table 9: **Married Families: Effect of Access to Public School On Labor Supply Measures (Youngest child is 5)**

Left-Hand Side Variable	[1] Means	[2] OLS	[3] OLS	[4] Wald	[5] QOB	[6] Percentage Effect
Weeks of work in 1979	21.952	-0.442	-0.063	1.182	1.515	7.2%
	(0.091)	(0.182)	(0.178)	(0.458)	(0.363)	
Usual hours in 1979	19.306	-0.107	0.255	1.119	1.586	8.6%
	(0.076)	(0.153)	(0.148)	(0.385)	(0.301)	
Employment in 1979	0.581	0.002	0.012	0.035	0.045	8.1%
	(0.002)	(0.004)	(0.004)	(0.01)	(0.008)	
Received public assistance in 1979	0.022	0.006	0.002	0.001	0.001	4.7%
	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)	
Wage and salary income in 1979	3583.828	-242.349	-181.196	125.366	192.756	5.5%
	(21.425)	(43.048)	(41.807)	(108.275)	(85.211)	
Hours last week	15.909	-0.188	0.133	1.285	1.388	9.2%
	(0.076)	(0.152)	(0.149)	(0.383)	(0.303)	
Labor force participation last week	0.523	0.008	0.014	0.04	0.048	9.7%
	(0.002)	(0.004)	(0.004)	(0.01)	(0.008)	
State fixed effects		N	Y	N	Y	
Demographic controls		N	Y	N	Y	
Instruments		–	–	Q1 dummy	Quarter main	

Note: Standard errors in parentheses. The top number in each cell is the coefficient on the public school enrollment dummy in the equation estimated using the indicated row variable on the left hand side. Sample size is 61,785 for columns [1], [2], and [4] and 59,674 for [3] and [5]. Percentage effects are the ratio of the column [5] coefficient to predicted mean of the dependent variable with enrollment set to zero.

Table 10: **Married Families: Effect of Access to Public School On Labor Supply Measures (Youngest child younger than 5)**

Left-Hand Side Variable	[1] Means	[2] OLS	[3] OLS	[4] Wald	[5] QOB	[6] Percentage Effect
Weeks of work in 1979	14.523	-0.067	0.032	0.729	1.221	7.0%
	(0.083)	(0.166)	(0.163)	(0.418)	(0.322)	
Usual hours in 1979	14.022	0.102	0.194	0.261	0.985	5.6%
	(0.074)	(0.149)	(0.146)	(0.376)	(0.287)	
Employment in 1979	0.443	0.005	0.009	0.014	0.03	5.6%
	(0.002)	(0.004)	(0.004)	(0.01)	(0.008)	
Received public assistance in 1979	0.03	0.01	0.005	-0.004	-2.2E-04	-1.0%
	(0.001)	(0.001)	(0.001)	(0.004)	(0.003)	
Wage and salary income in 1979	2214.465	-137.741	-142.229	61.884	117.42	4.0%
	(18.076)	(36.294)	(35.628)	(91.44)	(70.57)	
Hours last week	10.109	0.121	0.173	0.629	0.959	7.7%
	(0.069)	(0.138)	(0.136)	(0.347)	(0.267)	
Labor force participation last week	0.363	0.008	0.008	0.023	0.032	7.5%
	(0.002)	(0.004)	(0.004)	(0.01)	(0.008)	
State fixed effects		N	Y	N	Y	
Demographic controls		N	Y	N	Y	
Instruments		–	–	Q1 dummy	Quarter main	

Note: Standard errors in parentheses. The top number in each cell is the coefficient on the public school enrollment dummy in the equation estimated using the indicated row variable on the left hand side. Sample size is 58,896 for columns [1], [2], and [4] and 56,689 for [3] and [5]. Percentage effects are the ratio of the column [5] coefficient to predicted mean of the dependent variable with enrollment set to zero.

Chapter 2

Is More for the Poor Less for the Poor? The Politics of Means-Tested Targeting (*Joint with Lant H. Pritchett*)

1 Introduction

During the 1995-96 debate over the federal budget, the question of whether to means test Medicare benefits was raised. Representative Charles Rangel, a liberal Democrat who represents Harlem, argued against doing so, apparently defending the view that the rich should continue to receive exactly the same benefits as the poor. Speaker of the House Newt Gingrich, a conservative Republican, argued for targeting benefits, so that the rich would receive less generous benefits than the poor. At first glance, such a situation seems a curious political inversion: one politician who regards himself as the defender of his poor constituents arguing in favor of spending on rich ones, and another politician not usually identified that way arguing against such spending.

Moreover, such political behavior seems to contradict both common sense and a fair bit of economics. Common sense suggests that fewer people sharing the pie means larger slices: means-testing, or targeting, means more for the poor. Theoretical assessments of targeting generally have followed this view, building normative models in which one assumes the budget for redistribution is fixed, while the structure and degree of targeting is chosen to maximize social welfare (or minimize poverty). Alternatively, both the budget (i.e. degree of taxation) and targeting variables are chosen simultaneously. While the literature has considered informational constraints, incentive compatibility, and efficiency losses, some degree of targeting is always found to be optimal in the models examined.[1]

But what is it that an experienced politician like Rangel knows but that the models do not capture? Why is it often said among policy makers that "programs for the poor are [budget] poor programs"? As political scientists, politicians and policymakers suspect, the size of the pie is unlikely to be fixed. If the budget for redistribution is politically determined, the impact of targeting cannot be determined without accounting for the effect of changes in the degree of targeting on the size of the budget available for redistribution.

Of course, this idea has been bandied about economics seminars for years, but the literature to date contains no formal treatment of political feedback effects on the budget. Perhaps the typical view among economists is that while such effects should be accounted for in practice, they constitute at most a "small" addition to economic models. That is, it may be thought that so long as targeting is sufficiently beneficial for the poor, political economy considerations are likely to change only the magnitude and not the direction of targeting's benefits.

As this paper's title suggests, we construct a plausible counterexample in which any positive

[1] For examples in a variety of settings, see Akerlof (1978), Atkinson (1995), Besley and Kanbur (1990), Diamond and Sheshinski (1995), Kremer (1997), Nichols and Zeckhauser (1982), Sen (1995), Stern (1982), and Viard (1996).

amount of targeting actually makes the poor worse off relative to the performance of a universally received transfer (which is similar to the basic grant in a negative income tax system). This finding alone suggests that more attention is warranted when implementation of targeting is being considered.

But our model also yields a much more striking and troubling result. When the policymaker mirrors standard economic models in ignoring politics, there exists a unique equilibrium which *minimizes* both social welfare and the utility of poor agents over the set of politically feasible policies.

From such a result, one can only conclude that standard models of targeting are not robust to political considerations. Put another way, ignoring politics does not result in a "nearby" equilibrium to the best one available to the policymaker. While our model is admittedly a special case, it can be viewed as representing primarily a counterexample. Thus while other models might overturn our findings, it is not enough to suggest that possibility. Rather, if targeting through means testing is to be considered optimal in the presence of endogenous budgets, such models would have to be constructed and analyzed; they cannot be viewed simply as extensions of existing models that ignore politics.

It is worth noting that, as we show below, our model *does* yield optimality of targeting in the special case when political considerations do not matter. Thus it is not the economy, but rather the polity, that drives our results.

The rest of the paper proceeds as follows. In Section 2, we introduce the basic structure of the model. In Section 3, we present our main results. We conclude in Section 4.

2 The Model

2.1 The basic model

We consider a population having unit measure and consisting of three types of agents: low income, middle income and rich (subscripted by l, m, and r, respectively); group i's population share is σ_i. If employed, these agents have maximum marginal products equal to μ, 1, and r, respectively, where $\mu < 1 < r$. There are three types of jobs, each of which pays either μ, 1, or r. An agent may work in any job paying no more than her maximum marginal product, and we assume that there are always just enough jobs of each type to employ all workers in their chosen type. We assume that poor and middle income agents have some probability p of being "unemployed" (having zero

pre-transfer income) and probability $q \equiv 1-p$ of being employed. Rich agents are always employed.

Workers in jobs paying μ pay no taxes; by contrast, jobs paying 1 and r are taxable at the proportional rate τ. We motivate this assumption by imagining that there are tax-free "informal" and taxable "formal" sectors in the economy. This assumption follows Kramer and Snyder (1988), who use it in their analysis of the politics of constant versus increasing marginal tax rates. By replacing variable labor disutility with this assumption, one greatly simplifies the analysis, allowing for closed-form results. Introducing constant labor disutility, as in Akerlof (1978), for example, would cause only minor differences while changing none of the results as stated in the main text.

All agents have the identical von Neumann-Morgenstern utility function u, with $u' > 0 > u''$. Given that informal sector income is untaxed, middle income and rich agents will work in formal sector jobs only if doing so yields greater utility than choosing to work in the informal sector. after accounting for differences in transfers available to workers as a function of job choice. We will assume throughout the paper's main text that this requirement is met strictly for both middle income and rich workers.

A 1 (Formal Sector Work) *The utility function u and all parameters of the model are such that in any equilibrium, employed middle income and rich workers always strictly prefer formal sector work to informal sector work, after accounting for all cross-sector differences in taxation and transfers.*

Dropping this assumption complicates the analysis greatly, but would not systematically change our substantive findings.[2]

We define the tax base as $\bar{y}$. Under assumption A 1 (and because there is no variable labor disutility) the tax base does not depend on the tax rate. Since there are $q\sigma_m$ middle income workers earning 1 unit of income each and σ_r rich workers earning r, the tax base is $\bar{y} = q\sigma_m + \sigma_r r$. By definition, the total budget available to the government for redistribution is $\bar{y}\tau$.

Two types of transfers are feasible. The first, N, is non-targeted and thus is received universally by all agents. We make the informational assumption that the policymaker is unable to distinguish agents working in jobs with marginal product of μ from agents who are unemployed,[3] so that the targeted transfer θ, is received only by those agents with zero formal-sector income. Since rich agents are never unemployed, and since assumption A 1 guarantees that rich agents always work

[2]Because there are few changes in our results, and because the generalization requires significantly more notation and rigor to carry out, we have chosen in this paper to focus only on cases when A 1 is satisfied.

[3]Again, this assumption follows the spirit of Kramer and Snyder (1988).

in the formal sector, they never receive the targeted transfer. All poor agents receive the targeted transfer, while middle income agents receive it only if they are unemployed. That the targeted transfer θ provides insurance is obvious; since the universal transfer N is received by unemployed agents whose nontransfer income is zero by definition, it also provides insurance.

We may define the takeup rate for the targeted transfer θ as

$$\bar{\delta} \equiv \sigma_l + p\sigma_m, \tag{1}$$

and we may now write the government's budget constraint as

$$N + \bar{\delta}\theta = \bar{y}\tau \tag{2}$$

That is, total expenditures (the LHS of the budget constraint) are equal to the sum of total untargeted expenditures, N, and total targeted transfers, which in turn are equal to the product of the takeup rate $\bar{\delta}$ and the targeted transfer θ. Total revenues (the RHS of the budget constraint) have been seen above to be the product of the (constant) tax base $\bar{y}$ and the tax rate, τ.

Our principal task in the model is to investigate properties of equilibria in a game played between a policymaker and the electorate, where the strategy spaces are the level of the budget (for the electorate) and the budget's distribution between universal and targeted transfers (for the policymaker). We define the fraction of the budget spent on targeted transfers as k, so that we may rewrite the budget constraint (2) as the two identities:

$$\theta = \frac{k\bar{y}\tau}{\bar{\delta}} \tag{3}$$

$$N = (1 - k)\bar{y}\tau \tag{4}$$

As an aside, it will be useful below to have notation for the tax level at which employed middle income workers are just indifferent between formal and informal sector work, given that all employed middle income and rich agents choose the formal sector. That is, fixing k we want to find the tax level such that $N + \theta + \mu = N + 1 - \tau$. This level may be written

$$\tau_m^a(k) \equiv \frac{\bar{\delta}(1 - \mu)}{k\bar{y} + \bar{\delta}} \tag{5}$$

Thus, assumption A 1 requires that in any equilibrium, (k, τ) must satisfy $\tau < \tau_m^a(k)$. Table 1 displays the model's basic components.

Since some agents will not receive the targeted transfer, i.e. $\overline{\delta} < 1$, the argument that targeting can increase welfare seems well-grounded: a given amount of revenue spent on targeted transfers allows a greater transfer *per recipient* than the same amount spent on untargeted transfers. Put another way, $\theta|_{k=1} > N|_{k=0}$. Thus the favorable budgetary performance of the targeted transfer stems from the fact that it need not be given to all agents, as the universal transfer must. Favorable *social welfare* performance of targeting hinges on whether those agents excluded from receiving targeted transfers have less "need" for them than those who are included.

Using the integral of agents' utilities as the social welfare function, we show below that *full* targeting – i.e. spending as much as possible on the targeted transfer and as little as possible on the universal one (without violating the incentive compatibility of formal-sector work for middle class and rich agents) – passes this test when the budget does not vary (i.e. when one ignores politics).[4] Targeting makes use of information about agents' before-tax and -transfer incomes, so failing to use targeting generally entails ignoring valuable information.

We can now write own-utility functions (i.e. utility functions excluding any altruism) generated by equilibrium job choice behavior. Recognizing that both N and θ vary with the degree of targeting k and the tax rate τ, the own-utility functions of middle income and poor agents are

$$U_l(k, \tau) \equiv pu(N + \theta) + qu(N + \theta + \mu) \tag{6}$$

$$U_m(k, \tau) \equiv pu(N + \theta) + qu(N + 1 - \tau) \tag{7}$$

Note that poor agents' utility is a strictly increasing transformation of total transfers $N + \theta$. To allow altruism, we introduce the overall utility function for middle income voters,

[4]For a given tax rate, and hence a given budget, targeting transfers will redistribute resources from non-targeted agents to targeted ones. Hence there will always exist some social welfare function for which the policymaker would choose not to target when the budget is fixed. As an example, if the social planner cared only for the rich and, then no targeting would be used when the budget is taken as fixed..

$$
\begin{aligned}
V_m(k,\tau;\alpha_m) &= (1-\alpha_m)U_m(k,\tau) + \alpha_m U_l(k,\tau) \\
&= pu(N+\theta) + \alpha_m qu(N+\theta+\mu) + (1-\alpha_m)qu(N+1-\tau) \qquad (8)
\end{aligned}
$$

where α_m is the altruism coefficient for middle income voters: the greater is α_m, the more relative concern middle income agents show for the welfare of poor agents. It will be convenient to use the notation $V_l(k,\tau) = U_l(k,\tau)$, as we will assume that poor agents do not care about the welfare of either rich or middle income agents.

Since all rich agents work in the formal sector, their own-utility is simply

$$U_r(k,\tau) \equiv u(N + r(1-\tau)) \qquad (9)$$

Hence rich agents' own-utility is a strictly increasing transformation of their net consumption $N + r(1-\tau)$. To allow altruism for rich agents, we define

$$V_r(k,\tau;\alpha_r) = (1-\alpha_r)U_r(k,\tau) + \alpha_r U_l(k,\tau), \qquad (10)$$

where α_r is the coefficient of altruism for rich agents.

To make things interesting, we assume that the function u is concave enough that middle income voters always want some positive level of taxation.

A 2 (Positive Taxation) *For any degree of targeting, the utility function u and the parameters of the model are such that middle income voters' overall utility is increasing in the tax rate when there is zero taxation.*

A sufficient condition for this assumption is $\lim_{c\to 0} u'(c) = \infty$, so that middle income workers are always better off buying some positive amount of consumption insurance (for any finite price). Any constant relative risk aversion utility function – e.g. log utilities – will satisfy this requirement.

2.2 Definition and existence of majority voting equilibrium

In our analysis of optimal policymaking in Section 3, we assume that the policymaker chooses a level of targeting, k, after which an election is held to determine the level of taxation. Our task

in this subsection is therefore to describe the winning tax rate for each value of k.[5] Typically, one requires that an equilibrium tax rate receives support from a majority of the population. In the present case, we will assume that no majority is possible without support from at least two types of agents. This assumption does not restrict the population shares σ_i, since it is possible that a given type of agent represents more than half the population but for some reason has less than half the political power in the society.[6] Hence we may treat the determination of the tax rate as a three-person voting game.

Under assumption A 1, all three utility functions V_l, V_m, and V_r are twice continuously differentiable and strictly concave in the degree of taxation. This fact implies that they are also single-peaked, so that a majority voting equilibrium tax rate (i.e. a Condorcet winner) always exists and is given by the median-preferred tax rate; we now demonstrate this fact.

Given the degree of targeting k, it will be convenient to define $\tau^*(k)$ as the value of the tax rate that solves the middle income FOC, i.e. $\partial V_m(k, \tau^*(k))/\partial\tau = 0$. We will need the following assumption, which ensures that middle income voters' preferred tax rate is always the Condorcet winner:

A 3 *Fix the degree of targeting k. Rich agents are never so altruistic that they prefer a greater tax rate than do middle income agents. That is, $\partial V_r(k, \tau^*(k))/\partial\tau \leq 0$.*

Since $\partial V_m(k, \tau^*(k))/\partial\tau = 0$ is the first order condition for middle income voters' optimal tax rate, given the degree of targeting, $\partial V_r(k, \tau^*(k))/\partial\tau \leq 0$ implies that at the given degree of targeting, rich voters oppose taxes greater than $\tau^*(k)$, preferring $\tau^*(k)$ instead. By concavity, we have established that both middle income and rich agents favor $\tau^*(k)$ over all greater tax rates (given that the degree of targeting k is fixed).

On the other hand, since poor voters never pay taxes but always receive transfers, they must favor all tax increases and oppose all reductions, no matter what the degree of targeting. Therefore, both middle income and poor agents prefer $\tau^*(k)$ over all lower tax rates Therefore $\tau^*(k)$ defeats all other tax rates in any election requiring support from two or more agent types. Concavity of all

[5] While this choice of political institution is *ad hoc*, we think that it reflects the critical issues quite accurately: policymakers typically have more scope over the design and administration of government programs than they do over the level of funding (in the U.S., for example, the President has much more discretion over program structure through rules making than he does over program funding, which of course must be approved by Congress). In any event, as we note in the introduction, we are outlining a counterexample rather than a general theory.

[6] The assumption is restrictive in that we are choosing to focus only on cases when no one type of agent can implement a tax rate unilaterally. While such a situation could occur, it is uninteresting from a political economy perspective, so there is no harm in making it.

utility functions then implies that (fixing the degree of targeting) no other tax rate can have this property. Hence $\tau^*(k)$ is the majority voting equilibrium given k.

We will say that a targeting-taxation policy (k, τ) is *politically feasible* if and only if $\tau = \tau^*(k)$. That is, a policy is politically feasible if and only if, given that the degree of targeting is k, the accompanying tax rate is the one that would be chosen through an election of the kind just described.

3 Social Welfare, Optimal Policy, and Equilibrium

We argue in subsection 3.1 that the optimal policy with a fixed budget (i.e. with no political feasibility constraint) is full targeting.[7] In subsection 3.2 we distinguish "sophisticated" policymaking – recognizing budgetary endogeneity – from "naive" policymaking – failing to recognize it. We define "naive equilibria" (NE) and "sophisticated equilibria" (SE) as situations in which (1) the policymaker's targeting choice is optimal given the kind of policymaking involved and (2) the tax rate is politically feasible, given that targeting choice. We argue that a unique naive equilibrium must exist, in which all revenues are spent on targeted transfers and none are spent on universal transfers.

In subsection 3.3, we focus on the set of politically feasible policies, discussing their welfare properties. In particular, we argue that on the set of politically feasible policies, the overall utility of both poor and middle income agents is strictly decreasing in the degree of targeting. By contrast, the opposite is true for the *own*-utility of rich agents. Moreover, we argue that social welfare will be strictly decreasing in the degree of targeting, from which it follows that there is a unique sophisticated equilibrium, in which all revenues are spent on universal transfers and none are spent on targeted transfers.

3.1 Defining social welfare

We may write the social welfare function as[8]

[7]Because of the formal sector work constraint, "full targeting" may not mean setting $k = 1$, i.e. spending all revenues on targeted transfers. Except at very low tax rates, doing so would lead at least some agents to forgo formal sector work. For any tax level, we derive the full-targeting level of k below.

[8]Note that we have defined the social welfare function in terms of the *own*-utility functions U_i. There is no loss of generality here; we could as well define it over the overall utility functions V_i, with only notational differences arising.

$$S(k,\tau) \equiv \sigma_l U_l(k,\tau) + \sigma_m U_m(k,\tau) + \sigma_r U_r(k,\tau), \tag{11}$$

Given a fixed budget, we could demonstrate optimality of full targeting by grinding out the first order condition, holding the tax rate constant. However, a more intuitive approach is available. The basic result to which we appeal is that a policymaker maximizing a weighted average of concave utilities will always want to undertake a policy that reduces the "spread" of the after-tax and -transfer income distribution.

By raising the sum of targeted and universal transfers $N+\theta$ but lowering the universal transfer N,[9] fixed-budget increases in targeting redistribute income from employed agents (who have income of either $N+1-\tau$, if middle income, or $N+r(1-\tau)$, if rich) to unemployed ones (who have income of either $N+\theta$ or $N+\theta+\mu$). Such a policy moves population density equal to $p(\sigma_l+\sigma_m)$ from the initial income level $(N+\theta)_0$ to the higher income level $(N+\theta)_1$, while moving the density $q\sigma_l$ from the initial income level $(N+\theta)_0+\mu$ to the higher income level $(N+\theta)_1+\mu$. At the same time, the increase in targeting reduces the universal transfer N (since a smaller share of the fixed budget is now spent on universal transfers), so that income for the density $q\sigma_m+\sigma_r$ of employed middle income and rich agents falls by N_1-N_0.

Technically speaking, the income distribution with k_0 is second order stochastically dominated by the distribution with k_1. Hence for any increasing and concave utility function, it follows that fixed-budget targeting raises social welfare.

Thus it appears that fixed-budget increases in targeting should be pursued so long as these are feasible. However, except at low levels of taxation, high levels of targeting will make the combination of informal sector work and large targeted transfers more attractive to middle income or rich agents than formal sector work without targeted transfers. As such, for any tax rate τ there generally will exist a threshold level of targeting above which not all employed middle income and rich agents will choose formal sector work, violating assumption A 1.

To find this threshold level for middle income agents, we simply find the degree of targeting that makes an employed middle class agent just indifferent between sectors, given the levels of transfers that arise when all agents who can, choose to work in the formal sector. That is, we set $N+1-\tau = N+\theta+\mu$, where N and θ are as defined above (i.e. the tax base and takeup rate reflect the choice of all employed middle income and rich agents to work in the formal sector). Rewriting,

[9] From the budget identities, we may write $N+\theta = [\bar{\delta}+(1-\bar{\delta})k]\bar{y}/\bar{\delta}$, which is clearly increasing in k By contrast, $N=(1-k)\bar{y}\tau$ is decreasing in k.

we have the threshold level

$$\hat{k}(\tau) = \frac{\bar{\delta}(1-\tau-\mu)}{\bar{y}\tau} \tag{12}$$

Hence for any τ and $k > \hat{k}(\tau)$, some positive fraction of employed middle income agents will choose work in the informal sector, while all of them (and all rich agents) choose formal sector work for any $k \leq \max[\hat{k}(\tau), 1]$. Thus $\hat{k}(\tau)$ is the highest degree of targeting, given the tax rate, for which all employed middle income and rich agents work in formal sector jobs. We refer to this degree of targeting as "full" targeting.[10,11]

To sum up this subsection, the value of k that maximizes social welfare for any fixed degree of taxation is cither 1 or the greatest value of k such that all rich and employed middle income workers choose to work in the formal sector. This result accords with the standard economic intuition that valuable information should be used, while also demonstrating that there is nothing in the structure of our economy that stacks the deck against targeting.

3.2 Equilibrium with naive and sophisticated policymaking

The naive policymaker does not recognize budgetary endogeneity. Instead, she takes the tax rate as fixed and then seeks to maximize social welfare over the degree of targeting. Since there is no guarantee that, given an arbitrary tax rate τ, the maximizing choice of k will satisfy political feasibility, we must incorporate this requirement explicitly into the definition of naive equilibrium (NE). Hence an NE is any policy (k^*, τ^*) jointly satisfying the requirements

$$k^* = \arg\max_{k\in[0,1]} S(k,\tau)$$

[10]Note that full targeting entails setting $k = 1$ for any tax no greater than $\bar{\delta}(1-\mu)/(\bar{y}+\bar{\delta})$. This quantity is clearly positive, so that there will exist taxes low enough such that full targeting always entails zero universal transfers, i.e. spending the whole budget on the targeted transfer θ.

[11]It is possible to show that when k is increased a small amount above $\hat{k}(\tau)$, the tax base falls and the takeup rate rises quickly enough to more than offset the beneficial distributional impact of raising the degree of targeting. That is, there is a region on which increases in targeting cause employed middle income agents to switch continuously from the formal to the informal sector. Once all have switched, we again have a constant tax base and takeup rate, so that increases in targeting are locally improving. When the degree of targeting becomes great enough that rich voters are just indifferent between sectors, all further increases in k end up lowering social welfare. Hence either the degree of targeting $\hat{k}$ or the value of k leaving rich voters indifferent between sectors must be optimal when the budget is fixed.

$$\tau^* = \tau^*(k^*) \tag{13}$$

We know from the previous subsection that full targeting is always optimal given a fixed tax rate. Now, under assumption A 1, we have $\tau^*(k) < \tau_m^a(k)$ in any equilibrium. Thus no politically feasible policy $(k, \tau^*(k))$ can entail full targeting unless $k = 1$, i.e. all revenues are spent on the targeted transfer. That is, for any politically feasible tax and level of targeting at which any revenues are spent on the universal transfer N, it is always possible to increase the degree of targeting a small amount while keeping the tax rate fixed and maintaining the tax base. It therefore follows that the only possible NE is $(1, \tau^*(1))$. In fact, since this policy is politically feasible while satisfying (13), it actually must be a naive equilibrium. Therefore, there is a unique NE at $(1, \tau^*(1))$, where all revenues are spent on the targeted transfer and none on the universal one.

Turning now to sophisticated policymaking, a sophisticated equilibrium is any policy $(k^*, \tau^*(k^*))$ that satisfies the following:

$$k^* = \arg\max_{k \in [0,1]} S(k, \tau^*(k)), \tag{14}$$

The sophisticated policymaker recognizes that the politically feasible tax rate will depend on the degree of targeting. Because the politically feasible tax rate τ^* is the solution to middle income voters' first order condition, it must vary continuously with the degree of targeting k.[12] Existence of a sophisticated equilibrium is thus reduced to noting that a continuous function takes a maximum on a compact set. Any value of k at which this maximum is obtained, k^*, is then an optimal choice for the sophisticated policymaker, so any policy $(k^*, \tau^*(k^*))$ is thus a sophisticated equilibrium. Existence of each kind of equilibrium is thus proved.

3.3 Welfare properties of politically feasible policies

In this subsection, we demonstrate that total transfers $N + \theta$ are strictly decreasing in the degree of targeting on the set of politically feasible policies. This fact effectively reverses the second order stochastic dominance argument used above, so that income distributions with lower levels of targeting stochastically dominate those with higher levels. It follows that social welfare is also

[12] Actually, without assumption A 1, this result is not guaranteed. In fact, it is possible that there is a single $\bar{k}$ at which the majority voting equilibrium τ^* both jumps up and can have either of two values.

strictly decreasing in the degree of targeting, so that the unique naive equilbrium *minimizes* social welfare on the set of politically feasible policies.

We begin by reformulating middle income voters' optimization problem (choosing the equilibrium tax rate) into one that looks like a standard consumer theory problem. This approach has the advantage of making it clear what "goods" are being traded off against one another. Define $z = N + \theta$, so that z is the amount of consumption insurance purchased by a targeting-taxation policy; hence z is received by all poor and unemployed middle income agents. Next, from the definitions of N and θ, we have $N + \theta = [\bar{\delta} + k(1 - \bar{\delta})]\bar{y}\tau/\bar{\delta}$ and $N + 1 - \tau = 1 - [1 - \bar{y} + k\bar{y}]\tau$. Therefore we may write

$$N + 1 - \tau = 1 - \pi(k)z, \tag{15}$$

where $\pi(k)$ is defined as follows:

$$\pi(k) \equiv \left(\frac{1 - \bar{y} + k\bar{y}}{\bar{\delta} + [1 - \bar{\delta}]k}\right)\left(\frac{\bar{\delta}}{\bar{y}}\right) \tag{16}$$

Intuitively, $\pi(k)$ is the price of insurance when the degree of targeting is k. Fixing the degree of targeting (and thus the price π) and denoting middle income agents' net income when employed, $N + 1 - \tau$, as w, we have thus transformed the problem of maximizing middle income agents' utility into the following one:

$$\max_{w,z} f(z) + g(w) \text{ s.t. } w + \pi z = 1, \tag{17}$$

where each of $f(z) = pu(z) + \alpha_m qu(z + \mu)$ and $g(w) = (1 - \alpha_m)qu(w)$ is strictly increasing and strictly concave. Intuitively, $f(z)$ is the expected utility received by a middle income voter from resources consumed by a representative poor agent as well as the resources that the middle income agent herself receives if unemployed. The middle income agent receives expected utility of $g(w)$ from resources she will consume if she is employed (given that she will want to work in the formal sector).

The solution to the problem in (17) is given by that value of z that solves $f'(z)/g'(1 - \pi z) = \pi$. We are interested in the effects of changes in the degree of targeting k on the optimal level of z satisfying this first order condition. The analogy to consumer theory ends here, because changes in k have a direct impact on z, since $z = [\bar{\delta} + (1 - \bar{\delta})k]\bar{y}\tau/\bar{\delta}$. In fact, changes in z have three effects. First, by raising the level of insurance z, they increase income received by recipients of the targeted

transfer. As a result, marginal utility of those agents, given by $f'(z)$, must fall when k is increased.

Second, increases in the degree of targeting raise the price of insurance, i.e. $\pi'(k) > 0$. In demonstrating this fact, it will be useful to consider the percentage change in the price of insurance π for a small change in the degree of targeting k. That is, taking the natural log of the price π and differentiating it with respect to the degree of targeting, we have

$$\frac{d \ln \pi}{dk} = \frac{\bar{y}}{1 - \bar{y} + k\bar{y}} - \frac{1 - \bar{\delta}}{\bar{\delta} + (1 - \bar{\delta})k} \tag{18}$$

The first term on the RHS of (18) arises due to the impact of greater targeting on employed-state income for middle income agents: an increase in targeting reduces the fraction of the budget spent on universal transfers, thereby reducing employed-state income – and raising the price of unemployed-state insurance – accordingly. On the other hand, an increase in the degree of targeting also means that the share of tax revenues going to nonrecipients of the targeted transfer – or the fraction $(1 - \bar{\delta})$ of the population – will be lower, meaning that the level of insurance z will be greater (for fixed τ). This effect, represented by the second term on the RHS of (18), tends to lower middle income agents' price of insurance.

Whether the price of insurance rises or falls depends on which of these effects is larger. The price will tend to rise when we have either or both of a large tax base – representing foregone universal transfer revenues – or a large takeup rate – representing relatively small increases in the size of the targeted transfer for given increases in the degree of targeting. Cross-multiplying terms on the RHS of (18) implies that it will be positive if and only if

$$1 < \bar{y} + \bar{\delta} \tag{19}$$

Now, $\bar{y} + \bar{\delta} = (\sigma_r r + q\sigma_m) + (p\sigma_m + \sigma_l)$, which can be rewritten as $\sigma_r r + \sigma_m + \sigma_l$. Since $r > 1$, (19) must be satisfied. Therefore the price of insurance is strictly increasing in the degree of targeting.

The third effect of an increase in k is to reduce consumption of employed middle income agents. To see this fact, note that their consumption is $N + 1 - \tau = 1 - \tau[1 - \bar{y} + k\bar{y}]$, or $1 - \pi z$. The expression involving k explicitly is obviously decreasing in k, while we have seen that both π and z are increasing in k; either way, it is clear that middle income agents' employed-state consumption falls with an increase in targeting.

Now, when the degree of targeting is increased, all three of these effects make total transfers z too high to satisfy the first order condition for the consumer theory problem above. Hence if middle income voters are to maintain satisfaction of their FOC, they must vote to reduce the tax

rate, thereby reducing total transfers $z = N + \theta$.[13]

Hence we have established an important result: *politically feasible increases in the degree of targeting must reduce total insurance* $z = N + \theta$. Moreover, since poor agents' utilities may be written entirely as a strictly increasing function of z (i.e. $V_l = pu(z) + qu(z + \mu)$), it follows that poor agents' utility is strictly decreasing in the degree of targeting – "more for the poor" *is* less for the poor when political feasibility is respected.

Moreover, by the envelope theorem, the only effect of an increase in the degree of targeting k on middle income voters' utility is $\partial V_m/\partial k$; in terms of the consumer theory analogy, this effect is equivalent to $[\partial V_m/\partial \pi][\partial \pi/\partial k]$.[14] Thus we are left with $dV_m/dk = -zg'\partial\pi/\partial k$, which is negative (since each of z, g', and $\partial\pi/\partial k$ is positive). Therefore, middle income voters' utility also is strictly decreasing in the degree of targeting on the set of politically feasible policies.

As for rich agents, it is straightforward to show that $r > 1$ implies that if total transfers $N + \theta$ are decreasing in the degree of targeting (as we have just shown), then rich agents' consumption $N + r(1 - \tau)]/dk > 0$ must be increasing in the degree of targeting.[15] That is, if increases in the

[13]The total effect on their employed-state consumption, $N + 1 - \tau$, cannot generally be signed. On the one hand, the increase in targeting reduces the budgetary share and hence the size of the targeted transfer N. On the other hand, the induced fall in the tax rate raises the term $N - \tau$. To see this fact, note that assumption A 1 can hold only if $\bar{y} < 1$; otherwise middle income voters would always benefit from higher taxes when there is no targeting (i.e. $k = 0$) since we would have $N - \tau = \tau[\bar{y} - 1]$. In this case, both middle income and poor voters would always prefer $\tau_m^a(k)$, the maximum tax rate for which all employed middle income and rich agents choose formal sector work, to any lower tax rate, thereby violating the assumption.

[14]To see this fact, note that

$$\begin{aligned}\partial V_m/\partial k &= f'(z)\partial z/\partial k - [\partial(\pi z)/\partial k]g'(1 - \pi z)\\ &= [f' - \pi g']\partial z/\partial k - zg'\partial\pi/\partial k\end{aligned}$$

But $[f' - \pi g'] = 0$ by the FOC for an optimum in z, leaving only the term $-zg'\partial\pi/\partial k$, which is what we get by applying the envelope theorem to the consumer theory problem.

[15]Totally differentiating and rearranging $N + \theta = [\bar{\delta} + (1 - \bar{\delta})k]\bar{y}\tau_m^*(k)/\bar{\delta}$ with respect to k and noting that this derivative must be negative, we have

$$-\frac{1}{\tau_m^*(k)}\frac{d\tau_m^*}{dk} > \frac{1 - \bar{\delta}}{\bar{\delta} + (1 - \bar{\delta})k} \tag{20}$$

Now, writing $N + r(1 - \tau_m^*(k)) = r - \tau_m^*(k)[r - \bar{y} + k\bar{y}]$, we may differentiate this term and rearrange so that $N + r(1 - t)$ is increasing in k iff

$$-\frac{1}{\tau_m^*(k)}\frac{d\tau_m^*}{dk} > \frac{\bar{y}}{r - \bar{y} + k\bar{y}} \tag{21}$$

Since (20) must hold, if we can show that its RHS exceeds the RHS of (21), then (21) will hold as well. The derivation works as follows:

degree of targeting reduce the amount of consumption insurance z, then the tax rate must fall by enough to offset rich agents' lower universal transfers with greater after-tax labor income, thus *increasing* their post-policy income. Hence rich agents' own-utility must also be strictly increasing in the degree of targeting. It follows that for any values of the other parameters of the model, there will exist some positive degree of altruism for rich voters, α_r, such that their overall utility also will be strictly increasing in the degree of targeting on the set of politically feasible policies.

This finding suggests a stark conclusiong regarding politics and targeting: since middle income and poor voters' utility is strictly decreasing in the degree of targeting on the set of politically feasible policies, if rich voters' utility is strictly increasing then it follows that any politically feasible policy is Pareto efficient in the set of politically feasible policies. The "efficiency" argument for targeting can hardly hold up under such circumstances. Of course, when labor disutility is variable, so that targeting allows lower – and hence less distortionary – labor income taxes, this result will be less likely to hold. However, because our results hold strictly in the economy we consider, there will always exist some generalization of this economy, incorporating the desired improvements, such that all the results carry through.[16]

To sum up the results of this section, we have constructed a plausible model that stands on its head the conventional wisdom regarding the optimality of targeting. Where the conventional approach is to take the budget as fixed and maximize social welfare with respect to the degree of targeting, we show that this procedure *minimizes* social welfare in political equilibrium. Where conventional wisdom suggests that at least some targeting should be used, we show that social welfare is maximized in political equilibrium only when all revenues are spent on universal transfers

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$$\begin{aligned} \frac{1-\bar{\delta}}{\bar{\delta}+(1-\bar{\delta})k} > \frac{\bar{y}}{r-\bar{y}+k\bar{y}} \quad &\Leftrightarrow \\ (1-\bar{\delta})[r-\bar{y}+k\bar{y}] > \bar{y}[\bar{\delta}+(1-\bar{\delta})k] \quad &\Leftrightarrow \\ (1-\bar{\delta})[r-\bar{y}] > \bar{y}\bar{\delta} \quad &\Leftrightarrow \\ r(1-\bar{\delta}) > \bar{y}[\bar{\delta}+(1-\bar{\delta})] = \bar{y} \quad & \end{aligned}$$

Thus we require $(q\sigma_m + \sigma_r)r > q\sigma_m + \sigma_r r$, which must hold since $r > 1$. Therefore rich agents' income must be increasing in k.

[16]For example, suppose that there is some additive disutility of labor supply, $v(l;\rho)$, where l is the fraction of an agent's time spent working (so that middle income voters who work l receive earnings of l, while rich voters receive rl), and ρ is some parameter such that $v(l;0) = 0$ for all l and $\partial v/\partial\rho > 0$. Then our results are what one would get by including labor disutility of that form but evaluating at $\rho = 0$. Under sufficient continuity conditions on v with respect to ρ, there will always exist a $\rho^* > 0$ such that all of our results hold when $\rho < \rho^*$.

and none spent on targeted ones. Where conventional wisdom says that targeting should benefit the poor, have ambiguous effects on the middle income, and redistribute from the rich, we show that targeting redistributes from the poor, makes the middle income worse off, and benefits the rich in political equilibrium. It seems difficult to imagine a more complete reversal of what admittedly reasonable, other-things-equal analysis would suggest at first glance.

4 Summary

Our main objective in this study has been to assess the welfare properties of targeted income support transfers when a basic political feasibility condition is imposed on the levels of targeting and taxation. In the economy we consider, full targeting would be optimal if the budget could be taken as fixed. The intuition here is simple: when the budget is fixed, increasing the degree of targeting amounts to reallocating consumption from rich and employed middle class agents to poor and unemployed agents. Since this process contracts the income distribution while maintaining the mean level of income, it must increase the integral of utilities.

However, when the budget is determined by majority voting, the equilibrium tax rate falls sharply enough that transfers to poor and unemployed agents are actually decreasing in the degree of targeting, while they increase consumption for rich agents. Thus any increase in the degree of targeting induces a mean-preserving spread of the income distribution, reducing social welfare.

While the idea that narrowing the group of voters receiving a program's benefit might also reduce overall political support for that program of course is not new, we know of no prior attempt to formalize the issue in any coherent way. At the same time, there is a large economic literature – both theoretical and empirical – that considers targeting while ignoring politically-driven budgetary endogeneity. Reliance on this literature for policy advice has to be based on the notion that accounting for politics introduces only a "small" change to the policy environment.

Yet in our example, optimality of targeting is not only overturned, but also stood on its head: it is entirely possible that both social welfare and the utility of poor agents are *minimized* in the equilibrium with no account taken of political constraints. Other models that consider politics might well reverse our findings, but that is not the point we wish to argue. Rather, in the presence of a plausible counterexample, our view is that such models not only have to be constructed, but also shown to be relevant to actual economies, if targeting is to be judged desirable.

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Table 1

Agents' Characteristics

Type	Pop Share	Max Marg Prod	Prob Unemp	Get θ?
Low	σ_l	μ	p	*Always*
Middle Class	σ_m	1	p	*If unemployed*
Rich	σ_r	r	0	*Never*

Policy Parameters and Variables

θ	Targeted transfer
N	Universal (untargeted) transfer
τ	Tax rate
k	Budget share spent on θ
$\bar{y}$	Tax base $= q\sigma_m + \sigma_r r$
$\bar{\delta}$	Takeup rate $= \sigma_l + p\sigma_m$
$\tau_m^a(k)$	Maximum tax for which all middle class and rich workers choose formal sector work, given k.

Chapter 3

An Investigation of the Relationship Between Education and Age at First Marriage Using Quarter of Birth Instrumental Variables

At least since Becker (1973), economists have been interested in the nature and functioning of marriage markets. A large literature has considered both theoretical and empirical issues relating to the optimal timing of marriage as well as the determinants of any given agent's "opportunity set" of potential spouses. Several theoretical papers (Keeley [1977], Boulier and Rosenzweig [1984], and Bergstrom and Bagnoli [1993], all discussed below) predict or are consistent with a positive relationship between years of completed schooling and age at marriage. This relationship has been confirmed in empirical work (also discussed below) by both economists and demographers; a common finding is that an additional year of schooling is associated with an increase in age at first marriage of one to several months, with the effect being greater for women than for men. However, such research typically does not address possible econometric problems, including spurious correlation and endogeneity of educational attainment.[1]

The relationship between education and age at marriage is interesting partly because of the regularity of the empirical finding of a positive association. However, it is also interesting because, as numerous theories imply and Boulier and Rosenzweig (1984) emphasize, "How individuals sort themselves through marriage into household units has important implications for the distribution of incomes, consumption, and fertility." A potentially related issue is the well-known apparent "marriage premium" for men in the United States; if marriage actually raises productivity, then age at first marriage will affect economic well-being directly.[2] Also, some authors have argued that the functioning of marriage markets and the division of labor within marriages plays a large role in determining gender differences in wages (e.g. see Mincer and Polachek [1974] and Siow [1998]).

In this paper, I attempt to address potential spurious correlation and endogeneity of educational attainment via the method of instrumental variables. Following Angrist and Krueger (1991), I construct instruments from a person's quarter of birth (QOB). QOB affects educational attainment because of the interaction of fixed school-entry rules and compulsory attendance laws. Children born earlier in the calendar year typically begin school at an older age than do children born later in the calendar year, so that they reach any given age having completed less education than children born later in the year. Since compulsory attendance laws typically specify a minimum age for dropping out of high school, children born earlier in the calendar year will be able to drop out with fewer years of completed education.[3] As long as quarter of birth is uncorrelated with age at

[1] Boulier and Rosenzweig's (1984) empirical analysis is an exception; I discuss their results in subsection 1.2 below.

[2] For discussions of the marriage premium as well as consideration of alternative hypotheses including endogenous selection into marriage, see Korenman and Neumark (1991) and Ginther and Zavodny (1997).

[3] I refer the reader to Angrist and Krueger (1991) for a discussion of the binding nature of compulsory schooling laws.

marriage outside of its effects on educational attainment, QOB variables can serve as instruments for educational attainment in regressions of age at marriage on years of completed schooling.

Unfortunately, it turns out that age at marriage has a strong negative trend in my sample of ever-married whites born 1911–1940. Since QOB is correlated with age, this trend induces spurious correlation between QOB variables and age at marriage unless one controls for the trend. In turn, controlling for the trend renders QOB-based instrumental variables too weak to identify the effects of education on age at marriage, unless one also omits year of birth controls. The end result is that estimates are too sensitive to specification of these controls to allow firm judgments regarding potential bias in the OLS results.

The remainder of this paper proceeds as follows. In section 1 I discuss several theoretical papers that consider determinants of age at first marriage. I also discuss previous empirical work on the relationship between educational attainment and age at first marriage. In section 2, I briefly review the growing literature using QOB and related variables as instruments. In section 3 I describe the data, which are drawn from the 5% Public Use Microdata Sample (PUMS) of the 1980 U.S. Census.

In section 4 I present the principal results. Ordinary least squares (OLS) estimates of the effect of educational attainment on age at first marriage are similar to estimates in the literature: they suggest that an additional year of educational attainment induces white men to delay marriage by 1–2 months while causing white women to delay marriage by 3–4 months. Because of the need to control for the trend in age at first marriage, instrumental variables results generally are inconclusive. Controlling for both age and year of birth undermines the power of the instruments, making unlikely the rejection of the null hypothesis that ordinary least squares (OLS) estimates are uncontaminated by spurious correlation or endogeneity. Failing to control for this trend induces omitted variable bias in the 2SLS estimates, which may then be badly negatively biased. Controlling for only the trend, and not the year of birth, leads in several subsamples to results that seem too large to be credible. I conclude in section 5.

1 Existing Literature

In the next subsection I describe three papers that model marriage market behavior. In subsection 1.2, I discuss a number of empirical studies that consider the relationship between educational attainment and age at first marriage.

1.1 Theoretical Work on Age at Marriage

Keeley (1977) is the earliest paper to consider the effects of educational attainment in a search-theoretic model of the marriage market. He points out that exogenous increases in education should have two opposing effects on a person's marriage market behavior. First, by increasing one's attractiveness to potential partners, exogenous increases in education should increase the maximum quality spouse willing to accept an offer of marriage. Second, because it causes one to expect better options at any given future time, an exogenous increase in educational attainment should raise one's own reservation level for accepting an offer of marriage.

However, Keeley argues that the net effect of educational attainment should be a delay in marriage, since[4]

> the possible increased gains to marriage do not occur until the process of acquiring an education is completed. Also, acquiring an education is a time-intensive process and thus precludes at least part of the gain to marriage. Since women specialize in the home and in child rearing, which is also time-intensive, education should tend to delay marriage more for them than men. If education enhances market productivity more than non-market productivity, then more highly educated women would delay marriage because of their greater market opportunities and smaller gain from marriage. (p. 245)

Two alternative theories of the marriage market are presented by Boulier and Rosenzweig (1984) and Bergstrom and Bagnoli (1993). Boulier and Rosenzweig focus on the structural interrelationships between the choices of when to marry and how much education to obtain. They also consider the effects of educational attainment and search time on the "quality" of spouse a person can expect to realize, although they treat this issue in an *ad hoc* way.[5] Boulier and Rosenzweig do not derive implications of the effect of educational attainment on men's age at first marriage, choosing instead to focus on women. They assert that

[4]Which of these effects dominates will depend on numerous factors not modeled in the articles described here. Clearly, the dynamics governing the arrival rate of potential spouses of varying qualities will play a crucial role, since educational attainment affects both a person's minimum acceptable and maximum feasible quality spouse. A person's discount rate will determine how willing he or she is to wait for a better option, given a current offer of marriage. Another determinant of the relationship between education and age at marriage is the effectiveness of education in signaling long-term labor market performance. Preferences regarding spouses' education independent of education's impact on earnings should also play a part.

[5]For example, Rosenzweig and Boulier do not explicitly describe the dynamics governing the arrival rate of marital offers. Thus any variation in realized spousal quality due to a person's uncertainty over his or her future opportunity set of spouses is left unaccounted for by their model.

> If we assume that (1) schooling raises market more than home productivity, so that an increase in schooling raises single more than marital income for women ... while (2) [a person's exogenously given attractiveness] ... increases the gains to being married and (3) time in school is incompatible with both work and marriage, while spouse search can be conducted jointly with schooling and work ... then it can be shown that an exogenous rise in schooling will lead to a higher female marriage age, holding constant the level of the exogenous [attractiveness]. (p. 718)

Bergstrom and Bagnoli (1993), who are primarily concerned with explaining why men tend to marry later than do women,[6] take a different approach. They focus on what they call "traditional societies", in which men specialize as economic providers while women's "anticipated tasks will be childbearing, child care, and traditional household labor" (p. 186). They argue that information about men's abilities to succeed in the labor market is likely to be revealed more slowly than information about women's abilities to tend house and raise children. The central prediction of this highly stylized model is that

> males who expect to prosper will delay marriage until the evidence of their success allows them to attract more desirable females. The most desirable females, on the other hand, have little to gain by postponing marriage since the relevant information about their quality is available at an earlier age. In the long-run stationary equilibrium of this model, males with poor prospects marry at an early age, whereas those who expect success will marry later in life.

Bergstrom and Bagnoli do not explicitly model the relationship between education and age at marriage. However, they do write that "One of the most convincing ways that a young man can demonstrate to potential mates that he is able and diligent is to finish a college degree." This argument actually leads to ambiguous predictions regarding the effects on age at first marriage of exogenous increases in educational attainment. Males who would have dropped out might marry later, either because more education means more earnings, or because being in school hinders search or reduces gains to marriage (as suggested by Keeley). However, to the extent that education signals imperfectly observable spousal quality, exogenous increases in education would allow at least some males in the Bergstrom and Bagnoli model to marry earlier, since then education informs potential

[6]Focusing on differential fecundity, Siow (1998) presents an alternative model also aimed at explaining this difference.

spouses that they need not observe as much of the potential partner's earnings history to be assured that the partner's quality exceeds a given level. Thus, conditional on having arrived at age A without having married, having more education should reduce time to marriage.

All three of the models described above focus on "traditional societies". Their predictions of sexual differences in marriage market behavior all flow from the assumption that married women specialize in nonmarket activity while married men work for pay. While this assumption is becoming less valid as women increasingly combine family and work, it seems reasonable (if only descriptively) for the cohorts on which I focus (those born 1911–1940). Even if married women wanted to work, those studied here may well have faced serious institutional barriers. For example, Goldin (1988, 1989) points out that during the Great Depression, "many firms instituted or extended policies, known as 'marriage bars,' that required women be fired when they married and married women not be hired" (Goldin [1989, p. 29]). While such policies today would be flagrant violations of labor law, they and similar implicit rules seem likely to have played a role in influencing labor market opportunities for married women.

An interesting prediction from the traditional societies focus of these models is that the sexual difference in the delaying effect of educational attainment should narrow during times when married women participate more regularly in the labor force. The OLS estimates I present in section 4 suggest that the difference did in fact fall among the cohort born 1921–1930, among whom many women joined the labor force (because of World War II) around the time they would typically marry, relative to the 1911–1920 cohort. The difference then rose again for the 1931–1940 birth cohort, the members of whom reached adulthood after World War II.

1.2 Empirical work on age at marriage and schooling

Econometric problems arise in simply regressing age at marriage on educational attainment. First, Boulier and Rosenzweig point out that people expecting to do worse (either in terms of eventual spouse's quality or in terms of the time it takes to receive marital offers) on the marriage market are likely to get more education. This occurs both because education raises income when single and because education may be a substitute for other quality factors (e.g. looks) from the point of view of potential spouses. Second, if Bergstrom and Bagnoli are correct that education signals earnings ability, then if men who expect to prosper have a higher return to education than those who don't, they will both get more education and marry later. Third, people may terminate education

precisely because they receive an acceptable offer of marriage (this is the "MRS degree" effect).[7] Fourth, people who place relatively greater value on spousal quality than on time actually married (i.e. relatively patient people) may get more education because doing so increases eventual spousal quality, even though they must wait longer to marry.

These spurious correlation and endogeneity problems suggest that OLS estimates may be upwardly biased. Therefore, an instrumental variables approach is necessary to sort out structural effects from unobservable heterogeneity and endogeneity. I now turn to the empirical literature on the relationship between education and age at first marriage, most of which has simply ignored the spurious correlation and endogeneity issues.

Keeley (1977) reports OLS regressions of age at first marriage on various regressors, notably including years of schooling. Because his sample contains many young people (it is composed of people aged 18–65 in the Survey of Economic Opportunity), Keeley faces a serious censoring problem. While he notes this fact, he chooses simply to ignore it. He finds that an additional year of schooling delays marriage for 3.1 months for women and 2.4 for men.

Boulier and Rosenzweig (1984) use data on once-married women aged 35–45, whose spouse is present, sampled in the 1973 National Demographic Survey of the Philippines. Their study is the only one of which I am aware that uses instrumental variables to correct for the possible sources of upward bias described above. Whereas their OLS estimates suggest that an additional year of education raises a woman's age at first marriage by 3.6 months, their three stage least squares[8] estimate of this effect is approximately a third lower. However, the instrumented estimate is not significantly different from the OLS estimate.

Goldscheider and Waite (1986) discuss the potential reverse causality problems inherent in simply regressing age at marriage on education attainment, but do not attempt to control for such problems. In logit specifications that include both years of education and enrollment status, they find that educational attainment increases probability of marriage of both young men and young women. They write that "strong, positive effects [of educational attainment on marriage], coupled with the negative effects of school enrollment at the earlier ages, suggest that education per se operates primarily on the timing of marriage, postponing it into ages after schooling is completed." (p. 104) They go on to cite Bloom and Bennett's (1985) result that educational attainment positively affects age at marriage but not whether a person ever marries.

[7]Goldin (1995) finds a greater sex differential in college graduation rates than in college attendance rates among people born around 1890.

[8]Boulier and Rosenzweig's instrumental variables estimates for their age at marriage equation are computed jointly with equations for woman's education and husband's (log) earnings at the time of marriage.

Hogan (1978) refers to the general finding that men with higher educations report marrying later. He does point out that "the difficulty with such evidence is the questionable order of causation" (p. 162). Hogan's empirical results suggest that another year of schooling delays marriage for approximately one month; for younger sample members (whose inclusion again raises questions regarding censoring), Hogan finds about twice as large an effect.

Bloom and Bennett (1990) use maximum likelihood techniques in what is essentially a hazard framework to control for censoring. They are mostly interested in explaining trends in marriage behavior, but they do include educational variables as covariates in their analysis. They comment that "Consistent with earlier work, the results also indicate that education is a powerful covariate of the timing of first marriage" (p. 1009). For example, they find that white women with a high school diploma and aged 30-34 in 1985 marry 3.6 years later than do dropouts. They also find that blacks marry later than whites. However, as Bloom and Bennett point out, the censored nature of their data means that the validity of their results relies greatly on functional form assumptions. In particular, endogeneity of education would be problematic. Heckman et al. (1985) also use hazard techniques. They find large effects of education in delaying age at first union in their sample of Swedish women. While they assume that education is exogenous, they also find strong evidence that a considerable degree of unobserved heterogeneity is present.

In a series of papers, Claudia Goldin and coauthors have considered historical causes and effects of both marriage patterns and educational outcomes. For example, Goldin (1995) focuses on college graduation and marriage patterns among women born 1890–1944. She finds that more than 30% of women born around 1890 who graduated from college never married. She concludes that "For most women in this group [the choice between college education and family] was one or the other." (p. 1) Women born around 1910 married more often and also entered the workforce after graduating from college, but while they typically worked for several years, they rarely had full careers. Goldin (1995) characterizes this cohort as attaining "job then family". Among women born around 1933, Goldin (1995) finds that women who attended college stood "a considerably higher chance of marrying a college-educated man, [and she] also married the higher income-generating man from among the college-educated group as well as from the highschool educated group." (p. 3)

Goldin (1989) has noted the existence of substantial heterogeneity in labor force behavior among married women born from the 1880s to the 1910s. She finds that, once in, married women tend to stay in the labor force. However, women who begin work in manufacturing jobs tend to exit the workforce when they marry. Her results suggest that the compositional effects of married women's entering the labor force with little work experience have been a primary cause of the stability of average experience among working married women. She concludes that "years of work experience

among married women was considerable and that 'wage discrimination' was and is substantial" (p. 46), since the earnings gap be explained simply by sex differences in experience.[9] These findings are important for current purposes in that they suggest sex-based differences in returns to paid labor, supporting the "traditional societies" assumption made by Keeley (1977), Boulier and Rosenzweig (1984), and Bergstrom and Bagnoli (1997).

2 QOB and Educational Attainment

Public school entry rules for kindergarten and first grade are largely based on a child's calendar date of birth. Therefore, QOB will be correlated with children's age at school entry while plausibly being otherwise uncorrelated with economic outcomes of interest. It follows that QOB may sometimes be used as an instrument variable for school-related variables predicted by economic theory to affect such outcomes.

The first paper to use this approach was Angrist and Krueger's (1991) study of the returns to education. They use a variety of dummy variables constructed from a person's QOB, state of birth (SOB), year of birth (YOB), and the interactions of these variables as instruments for educational attainment of prime-aged males. Their approach relies not only on the variation in age at school entry, but also the fact that teenagers may drop out of high school when they reach their 16th or 17th birthday (depending on their state of residence).

Since school entry laws cause first quarter births to start school at an older age than children born in other quarters, first quarter births will be older than other children in their grade. Thus, on average first quarter births reach minimum dropout ages having completed less schooling than children born in other quarters. This relationship forms the basis for QOB's role in affecting educational attainment.

Angrist and Krueger's (1991) results demonstrate that QOB variables unambiguously affect educational attainment in the ways hypothesized. For example, using data from the 1980 Census, Angrist and Krueger find that among men born 1930–1939, fourth quarter births received 0.124 years more education than first quarter births, third quarter births received 0.099 more years, and second quarter births received 0.038 more years. An F-test for the joint exclusion of the QOB dummies from the years of attainment equation strongly rejects exclusion. These variables also have a definite, if small, reduced form relationship with log weekly earnings. For example, weekly

[9]Goldin and Polachek (1987) have considered this issue in more detail.

earnings among men born 1930–1939 in the 1980 5% PUMS were 0.011 log points lower for first quarter births than for fourth quarter births.

Using QOB-based instrumental variables, Angrist and Krueger generate 2SLS estimates of the returns to education that exceed OLS estimates. This finding was surprising to some observers because various theories predict that higher-ability people are likely to get more education. If so, then OLS estimates should be contaminated by the mutual positive correlation of unobserved ability and education on the one hand, and ability and earnings on the other. Angrist and Krueger conclude that either such spurious correlation is not a problem in the data or that mismeasurement of educational attainment outweighs the unobserved ability problem.

Angrist and Krueger's (1991) results have been criticized on two grounds. First, several authors have noted that the relationship between QOB instruments and educational attainment, particularly when the QOB dummies were interacted with state and year of birth dummies, is very weak. Several additional papers were written using QOB, SOB, and their interactions (see Bound, Jaeger, and Baker [1995], Angrist and Krueger [1995], Angrist and Krueger [1997] and Staiger and Stock [1997] for a discussion of the issues) in order to test both the empirical relevance of the theoretical critiques and to evaluate the performance of estimators other than 2SLS. The principal conclusion from that literature is that when many weak instruments (e.g. the 30 QOB×YOB plus 150 QOB×SOB instruments used in Table 7 of Angrist and Krueger [1991]) are used, the 2SLS estimator can give misleading results.

The second critique is substantive, rather than statistical. Bound, Jaeger and Baker (1995) argue that Angrist and Krueger's (1991) results showed evidence of omitted variables bias. For instance, 2SLS estimates of the return to education in Table 6 rise from 0.055 to 0.095 when age and age squared are added to equations estimated for men born between 1940–1949. This difference is apparently due to the fact that for these men, the trend in (log) weekly earnings is quite strong and negative with respect to year of birth (that is, men born later earn less due to the well-known age-income profile; see Figure 5 of Angrist and Krueger [1991]). Since QOB is naturally correlated with age, such a trend in the dependent variable can induce spurious correlation in instrumental variables estimates. As I discuss in detail below, age at first marriage showed a strong negative trend throughout the period I consider in this paper, raising serious problems for a QOB-based approach.

Use of QOB variables has not been confined to earnings equations. In Gelbach (1998), I used QOB variables for a different purpose. The opportunity to enroll her child in public school may reduce a woman's child care costs, and therefore access to public school enrollment for a woman's five-year-old should have significant effects on women's labor market outcomes. However, parents

have some discretion over a child's age at entry to kindergarten in that they may choose to hold their children back a year before enrolling them, even when the children become eligible to enroll. Also, parents may choose to enroll children in private schools. As a result, one might expect women for whom the child care component of kindergarten is more valuable to be more likely to enroll their five-year-old, introducing endogeneity bias into OLS regressions of labor supply on enrollment status of the five-year-old. Other differences in women's characteristics may also induce spurious correlation between public (as opposed to private) school enrollment of the five-year-old and mother's labor supply.

Therefore I take an instrument variables approach in Gelbach (1998), using QOB dummies as instruments for five-year-olds' age at school entry. I find that QOB dummies are a powerful predictor of public school enrollment status of five-year-olds. For example, second- and third-quarter births are about 45% more likely to be enrolled in public school than are first-quarter births,[10] even after controlling for a variety of covariates, while fourth-quarter births are about 20% more likely to be enrolled. Among single mothers whose youngest child is five, 2SLS estimates using three QOB dummies as instruments for public school enrollment imply that access to public school for the five-year-old during Fall 1979 raised labor supply (for all of 1979) by between 9–15% while reducing the incidence of public assistance receipt by 13%. Results for single mothers with a five-year-old and a younger child suggest no effect of free public schooling; results for married women who have a five-year-old suggest a significant effect, though smaller than for the first group of single mothers. The strong performance of the three QOB instruments renders the many weak instruments critique irrelevant for the 2SLS estimates in Gelbach (1998). However, to test the relative performance of 2SLS under these more favorable conditions, I also computed estimates using several alternative estimators and 153 instruments (3 QOB plus 150 QOB×SOB dummies). I found little difference in the results across specifications.

3 Data

The data I use in the present study come from the 1980 5% PUMS. Because younger people will not have completed their spells of being "at risk" of getting married, including them in standard linear regression models induces censoring on the dependent variable. As Greene (1993) writes, "The

[10]Because the Census is conducted with reference to April 1, and because school starts in the fall, five-year-old first quarter births are unlikely to be enrolled, since they will have just turned five at the time of the Census.

consequences of ignoring censoring in duration data are not unlike those which arise in regression analysis." However, as mentioned in subsection 1.2, this concern has not stopped some authors from running OLS regressions with young sample members; examples include Hogan (1978) and Keeley (1977). Hazard modeling provides an alternative approach and is used by Bloom and Bennett (1990), Heckman et al. (1985), and Heckman and Singer (1984), among many others.

I am unable to use hazard models in working with data from the Census, because it is impossible to determine when education terminated for people in the Census. Since education is likely to vary early in adulthood (while people attend college, for example), hazard modeling would necessarily involve serious measurement error at times when people are very likely to be getting married. However, the large samples afforded by the PUMS allow me to construct a sample of mature men and women, i.e. those whose spells of being at risk of marriage are likely to have ended. My sample includes ever married whites[11] born 1911–1940.[12] The youngest sample members are 157 quarters old (or 39 years and one quarter old), an age by which nearly all people who marry have done so. Thus restricting consideration to this sample is unlikely to affect my results.

Tables 1 and 2 report summary statistics for men and women, respectively. Age at marriage is much higher among men, as commonly observed in the literature and predicted by the Bergstrom and Bagnoli (1993) model. Across all cohorts, years of completed education increase as quarter of birth increases, as one would expect given Angrist and Krueger's (1991) results as discussed above.[13] At the same time, age at first marriage clearly declines across the quarters. Thus it is clear that simple Wald estimates, in which a first quarter dummy serves as the single instrument,[14]

[11]Because there are large racial differences in age at marriage (e.g. see Brien [1997]), I chose not to pool whites and blacks. Since QOB variables are generally not very strong instruments for educational attainment, large samples are necessary. Therefore I focus on whites.

[12]Goldin and Katz (1998) that before about the 1930s, compulsory schooling laws "do not appear to have constrained youths to remain in high school, let alone to have graduated from high school." (p. 30, note 15) They rely for this conclusion on the argument that many states provided labor permits to children younger than 16, and that typically only eight years of education were required to obtain such permits. However, they note that as of 1917, 30 states had compulsory schooling laws requiring children to attend school until their 16th birthday. By 1928, the minimum dropout age was 17 in five states and 18 in five others (See Goldin and Katz [1998, note 15, p. 30] and Keesecker [1929], cited in Goldin and Katz).

[13]Regressions of educational attainment on three QOB dummies and full sets of YOB and SOB dummies showed strong effects of the QOB variables not only in my pooled sample, but also among the 10-year birth cohorts starting in 1911, 1921, and 1931.

[14]The Wald estimator is a special case of instrumental variables in which there is a single, binary, instrument and there are no exogenous right hand side variables. The estimator is calculated as the ratio of the difference in means of the dependent variable of interest to the difference in means of the endogenous variable of interest, where the differences are taken with respect to the value of the instrument. Thus the estimator is simply the ratio of the reduced form coefficient on the instrument to the first stage coefficient on the instrument in simple linear regressions.

will yield a negative relationship between years of education and age at first marriage. For example, a Wald estimate of the effect of years of completed education on age at marriage among the pooled sample of women suggests that an additional year of education actually reduces age at marriage by more than 7 months; by contrast, the simple univariate OLS estimate suggests an increase in age of marriage of about one and a half months.

As I argue in more detail in the next section, this difference may be due to spurious correlation caused by a strong negative trend in age at marriage. Figures 1 and 2 depict this trend among white men and women, respectively. Over the 1911–1940 time period, average age at first marriage fell from more than 107 quarters to less than 95 quarters among men and from about 95 quarters to less than 85 among women.[15] This trend raises problems for simple estimates like the Wald one just described because QOB is naturally correlated with age, since people born early in the calendar year are older than those born later when year of birth is held constant. This correlation is easy to see in the means reported in Tables 1 and 2. For example, women born in the first quarter are aged 217.1 quarters, while fourth quarter births are aged only 214.2 quarters. Moreover, average age in quarters declines monotonically across the quarters.

As long as the trend in age at marriage is not entirely due to trends in educational attainment, then this trend will cause quarter of birth to be correlated with age at marriage outside of its correlation with educational attainment. Thus QOB variables can be legitimate instruments only once we condition on the trend in age at marriage. Put another way, since age at marriage is trending down and people born in earlier quarters are older, then people born in earlier quarters will tend to be observed marrying later partly because of this trend, inducing a negative relationship between QOB and age at marriage. Since years of completed education are positively related to QOB, the trend–QOB relationship therefore induces negative bias in instrumental variables estimates when we do not control for the trend.

4 Results

Table 3 reports OLS estimates of the effect of years of completed schooling on age at first marriage, measured in quarters. All specifications include full sets of YOB and SOB dummies. Estimates in this table of the coefficient on educational attainment are extremely precise. Columns [1] – [3] focus on men, with the difference between them being the choice of controls for trend and age.

[15] For a discussion of long-term trends in marriage patterns, see Haines (1996).

Column [1] includes only year of birth dummies, while column [2] adds controls for age in quarters and squared age in quarters; column [3] drops the YOB dummies, including only the quadratic trend. The coefficient estimates for educational attainment are insensitive with respect to inclusion of these age controls, and the age controls themselves are not statistically significant, which may reflect their collinearity with the year of birth dummies.

While the estimated coefficient on educational attainment in the pooled sample of men is 0.59 quarters, the OLS estimates in Table 3 clearly suggest that the delaying effect of education on age at marriage has risen over time. Among men born 1911–1920, another year of schooling raised age at marriage by 1.5 months, while the delay rose to 2.2 months for the 1931–1940 birth cohort.

Estimates for women are reported in columns [4] - [6]. Again, these estimates are insensitive to the inclusion of the quadratic trend in age. The delaying effect of education seems first to have fallen and then risen among women. Women born in the 1911–1920 cohort got married about 4.2 months later when they got an additional year of education, while this delaying effect fell to 3.6 months among those born 1921–1930 and then rose to 4.8 months for the 1931–1940 birth cohort.

These results confirm the difference in levels between men and women predicted by the models discussed in subsection 1.1 above. Moreover, women born in the middle cohort should have been less subject to the arguments regarding "traditional societies" made in those papers, since World War II allowed (or required) many of them to enter or stay in the labor force at ages typically associated with first marriage. A straightforward reading of any of the models discussed above suggests that such greater attachment to the labor force would cause the difference between men and women in the effects of education on age at first marriage to be smaller for this cohort than for the one preceding it. Since Rosie the Riveter was told to go home after the war, one would also expect the differential to rise in the 1931–1940 cohort, as observed. These results suggest that as labor force participation has become more typical for married women, the differential in the educational delay should also have fallen.[16]

Table 4 presents 2SLS estimates. The first three columns report estimates for men, while the second set of columns reports estimates for women. Columns [1] - [2] and [4] - [5] use QOB and QOB×YOB instruments for educational attainment, with YOB dummies included in all four of these specifications. Columns [3] and [6] drop the YOB dummies and use only QOB main effects as instruments (controlling for the quadratic trend in age). Moving from column [1] to column [2] and from [4] to [5] suggests that controlling for the trend in age at first marriage while also

[16]Unfortunately, the 1990 Census contains no information about age at marriage, so comparable estimates for women born later cannot be computed using that more recent data.

including the YOB dummies makes an enormous difference. Omitting the trend, the 2SLS results for education mirror the Wald estimate described in the previous section: these results are negative and significant, apparently contradicting predictions from the theories described above.

However, when the age variables are included, the 2SLS estimates for education are imprecise and generally positive in sign. While most of the cohort-specific point estimates differ substantially from their OLS counterparts, these differences are never statistically significant. The pooled estimates in columns [2] and [5] of the top panel do basically resemble their OLS counterparts. However, dropping the YOB dummies while continuing to control for the quadratic trend again makes a large difference. While results for men suggest basically a zero effect of educational attainment on age at first marriage, results for women in column [6] are similar to those in column [4] for all but the 1911–1920 samples. These results suggest that QOB and QOB×YOB instruments have little power left once I control for the quadratic trend.[17]

To further investigate the relationship between schooling and marriage behavior, I present OLS and 2SLS results in Tables 5 and 6 of the effects of dropping out of highschool on the probability of marrying as a teenager (i.e. at an age of 79 quarters or less). Specifications in these tables are identical to those in Tables 3 and 4, except that the RHS variable of interest is now defined as a dummy equal to 1 for those sample members who have completed fewer than 12 years of education and equal to 0 otherwise; the LHS variable of interest is a dummy for having married as a teen.

The OLS estimates in Table 5 suggest that dropping out increases the probability of a teen marriage by about 7% for men and about 21% for women in the sample. The 2SLS estimates in Table 6 suggest quite the opposite: they are generally negative and often highly significant. However, it is hard to know what to make of these results, both because they again appear sensitive to the approach used in controlling for trend and because in several cases the estimated coefficient on the dropout dummy simply is too large to be credible. For example, among women born 1931–1940, the column [6] estimate suggests that exogenously dropping out reduces the probability of marrying as a teen by 115 percentage points. The results for the pooled sample of women are only slightly more plausible.

One last question I can address with PUMS data is the effect of schooling on the probability of giving birth as a teenager. Tables 7 and 8 report OLS and 2SLS estimates of the impact of having completed less than 12 years of education on the probability of having a child among females aged

[17]The linear term in age (in quarters) is a linear combination of YOB and QOB main effects, while the quadratic term is a linear combination of QOB, YOB, and QOB×YOB effects, so two instruments must be dropped to run the 2SLS regressions with the full set of year dummies.

60–80 quarters in the 5% 1980 PUMS.[18] The specifications in columns [1]–[3] of these tables are analogous to those in the previous tables, i.e. the first column includes YOB dummies, the second column adds the quadratic trend, and the third column retains the trend while dropping the YOB dummies; all specifications include a dummy for being white and a full set of state of birth dummies.

The OLS estimates in Table 7 imply that dropping out raises the probability of having a birth as a teen by about 7%, regardless of the specification used. Results reported in Table 8 again suggest that 2SLS estimates are highly sensitive to the method of controlling for age. Column [1] suggests that having completed fewer than 12 years of education actually reduces the probability of having a child by 22%, and this effect is highly significant. However, omitting the quadratic age controls is likely to be particularly inappropriate here, since QOB will again be correlated with age, which is very likely to be correlated with having given birth among a sample of teenagers. Column [2] suggests that adding the age controls has an enormous impact on the coefficient on having completed fewer than 12 years of education; the estimate falls in magnitude to only 3%. Dropping the year dummies, in column [3], further reduces this coefficient, to a statistically insignificant 1%.

Unfortunately, I conclude that the one consistent finding in Tables 4, 6 and 8 is that 2SLS results are highly sensitive to the approach used in controlling for trends in and age effects on marriage and fertility behavior. The lesson to be learned from these results is that when the dependent variable is trending significantly (as in the case of age at first marriage) or is strongly related to age (as in the case of teenage fertility), 2SLS results using QOB-based instruments for educational attainment are likely to be unwieldy. Failing to control for age-related trends may lead to seriously biased instrumental variables estimates. Controlling for such trends either requires one to drop YOB controls or renders the instruments too weak for useful identification.

5 Summary

In this paper, I have attempted to use quarter of birth-based variables as instruments for educational attainment in regressions of age at first marriage on educational attainment. I described several models of the marriage market that predict or are consistent with a positive effect, as well as a larger delay among women than among men. I also discussed a number of empirical studies whose findings confirm these predictions and discussed reasons why results presented in these studies might be

[18]The sample contains 516.370 observations; average age in quarters was 70.2; average completed education was 10.2 years; 79.2 percent of the sample had not completed 12 years of education; 8.5 percent of the sample had a child.

contaminated by either spurious correlation or endogeneity. My OLS estimates are similar to those in the literature and confirm the predicted difference between men and women. They also suggest that this difference narrowed for cohorts for which World War II increased labor force attachment among married women, also as predicted.

Because of the strong downward trend in age at first marriage, I am forced to control for age in the instrumental variables specifications. Doing so while retaining controls for year of birth robs the QOB-based instruments of their power, so that 2SLS results are extremely imprecise. Dropping the year of birth controls while including controls for age leads to results similar to those generated with the dummies and without the age controls. However, results in Table 6 in particular point to the likely inappropriateness of the QOB approach, as the results are simply too large to be believed. I conclude that QOB-based instruments simply do not allow one to test for possible sources of bias in OLS regressions of age at marriage on educational attainment.

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Table 1: **Summary Statistics by QOB and Cohort, White Men**

	Full Sample	Quarter of Birth I	II	III	IV
Pooled: 1911-1940					
Age in quarters	213.686	215.284	214.182	212.985	212.311
	(0.031)	(0.061)	(0.062)	(0.06)	(0.062)
Age at First Marriage, in Quarters	99.106	99.32	99.216	99.004	98.887
	(0.021)	(0.042)	(0.042)	(0.04)	(0.041)
Years of completed education	11.988	11.9	11.945	12.038	12.069
	(0.003)	(0.006)	(0.007)	(0.006)	(0.006)
N	1204407	300733	294147	314299	295228
1931-1940 Birth Cohort					
Age in quarters	175.991	177.647	176.553	175.32	174.481
	(0.018)	(0.036)	(0.036)	(0.035)	(0.036)
Age at First Marriage, in Quarters	94.591	94.803	94.763	94.437	94.37
	(0.028)	(0.058)	(0.058)	(0.055)	(0.057)
Years of completed education	12.781	12.706	12.74	12.825	12.851
	(0.005)	(0.01)	(0.011)	(0.01)	(0.01)
N	418250	103927	101506	110082	102735
1921-1930 Birth Cohort					
Age in quarters	216.462	218	216.913	215.94	214.974
	(0.017)	(0.035)	(0.035)	(0.034)	(0.035)
Age at First Marriage, in Quarters	98.755	98.839	98.939	98.747	98.49
	(0.034)	(0.068)	(0.068)	(0.067)	(0.068)
Years of completed education	11.973	11.884	11.923	12.032	12.054
	(0.005)	(0.011)	(0.011)	(0.01)	(0.011)
N	435284	109101	107516	112994	105673
1911-1920 Birth Cohort					
Age in quarters	255.175	256.503	255.602	254.776	253.833
	(0.019)	(0.038)	(0.039)	(0.038)	(0.039)
Age at First Marriage, in Quarters	104.924	105.272	104.874	104.833	104.715
	(0.044)	(0.088)	(0.089)	(0.086)	(0.087)
Years of completed education	11.062	10.965	11.025	11.096	11.161
	(0.006)	(0.012)	(0.012)	(0.012)	(0.012)
N	350873	87705	85125	91223	86820

Note: Standard errors in parentheses.

Table 2: **Summary Statistics by QOB and Cohort, White Women**

	Full Sample	*Quarter of Birth* I	II	III	IV
Pooled: 1911-1940					
Age in quarters	215.525	217.071	216.03	214.779	214.236
	(0.029)	(0.059)	(0.059)	(0.058)	(0.06)
Age at First Marriage, in Quarters	88.043	88.288	88.236	87.912	87.741
	(0.019)	(0.038)	(0.038)	(0.036)	(0.037)
Years of completed education	11.59	11.529	11.549	11.626	11.656
	(0.002)	(0.005)	(0.005)	(0.005)	(0.005)
N	1319734	330335	322538	343233	323628
1931-1940 Birth Cohort					
Age in quarters	176.108	177.704	176.708	175.45	174.603
	(0.018)	(0.035)	(0.036)	(0.034)	(0.036)
Age at First Marriage, in Quarters	83.615	83.828	83.849	83.455	83.34
	(0.026)	(0.052)	(0.053)	(0.05)	(0.051)
Years of completed education	12.196	12.126	12.161	12.231	12.262
	(0.004)	(0.008)	(0.008)	(0.007)	(0.008)
N	433866	107960	105291	114080	106535
1921-1930 Birth Cohort					
Age in quarters	216.695	218.185	217.1	216.213	215.244
	(0.017)	(0.033)	(0.034)	(0.033)	(0.034)
Age at First Marriage, in Quarters	87.606	87.929	87.864	87.407	87.217
	(0.03)	(0.06)	(0.06)	(0.058)	(0.06)
Years of completed education	11.632	11.576	11.593	11.667	11.695
	(0.004)	(0.008)	(0.008)	(0.008)	(0.008)
N	472820	119128	116828	122574	114290
1911-1920 Birth Cohort					
Age in quarters	255.591	256.951	256.016	255.227	254.188
	(0.018)	(0.036)	(0.036)	(0.035)	(0.036)
Age at First Marriage, in Quarters	93.196	93.365	93.268	93.265	92.884
	(0.04)	(0.08)	(0.08)	(0.078)	(0.078)
Years of completed education	10.906	10.85	10.857	10.933	10.984
	(0.005)	(0.009)	(0.01)	(0.009)	(0.009)
N	413048	103247	100419	106579	102803

Note: Standard errors in parentheses.

Table 3: **OLS Estimates of the Effect of Educational Attainment on Age at First Marriage**

	Men			*Women*		
	[1]	[2]	[3]	[4]	[5]	[6]
Pooled: 1911-1940						
Years of completed education	0.588	0.589	0.589	1.408	1.41	1.41
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
Age in quarters		0.014	-0.138		0.217	-0.151
		(0.116)	(0.008)		(0.103)	(0.007)
Squared age in quarters		3.8E-04	0.001		8.7E-05	0.001
		(2.7E-04)	(1.9E-05)		(2.4E-04)	(1.7E-05)
N	1204407	1204407	1204407	1319734	1319734	1319734
1911-1920 Birth Cohort						
Years of completed education	0.491	0.493	0.493	1.438	1.44	1.44
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Age in quarters		-0.613	0.49		0.614	0.694
		(1.349)	(0.187)		(0.813)	(0.166)
Squared age in quarters		0.002	-0.001		-0.001	-0.001
		(0.003)	(3.7E-04)		(0.002)	(3.2E-04)
N	350873	350873	350873	413048	413048	413048
1921-1930 Birth Cohort						
Years of completed education	0.544	0.545	0.545	1.215	1.218	1.217
	(0.01)	(0.01)	(0.01)	(0.011)	(0.011)	(0.011)
Age in quarters		0.129	-0.96		-0.287	-0.387
		(0.551)	(0.122)		(0.499)	(0.105)
Squared age in quarters		5.6E-05	0.002		0.001	0.001
		(0.001)	(2.8E-04)		(0.001)	(2.4E-04)
N	435284	435284	435284	472820	472820	472820
1931-1940 Birth Cohort						
Years of completed education	0.734	0.735	0.735	1.619	1.621	1.62
	(0.009)	(0.009)	(0.009)	(0.01)	(0.01)	(0.01)
Age in quarters		-0.633	-0.679		-0.935	-0.678
		(0.386)	(0.082)		(0.338)	(0.072)
Squared age in quarters		0.002	0.002		0.003	0.002
		(0.001)	(2.3E-04)		(0.001)	(2.1E-04)
N	418250	418250	418250	433866	433866	433866

Note: Standard errors in parentheses. Specifications in [1], [2], [4], and [5] include QOB and QOB×YOB instruments and full sets of YOB and SOB dummies. Columns [3] and [6] include only 3 QOB dummies as instruments and full sets of SOB dummies as controls.

Table 4: **2SLS Estimates of the Effect of Educational Attainment on Age at First Marriage**

	Men			*Women*		
	[1]	[2]	[3]	[4]	[5]	[6]
Pooled: 1911–1940						
Years of completed education	-1.612	0.857	-0.381	-2.946	1.153	-2.252
	(0.267)	(0.552)	(0.429)	(0.347)	(0.69)	(0.53)
Age in quarters		0.02	-0.133		0.206	-0.135
		(0.116)	(0.009)		(0.107)	(0.008)
Squared age in quarters		4.1E-04	0.001		8.4E-05	5.0E-04
		(2.7E-04)	(3.4E-05)		(2.4E-04)	(3.2E-05)
N	1204407	1204407	1204407	1319734	1319734	1319734
1911–1920 Birth Cohort						
Years of completed education	-2.14	-0.077	-0.25	-1.739	2.02	-0.176
	(0.528)	(1.522)	(0.915)	(0.63)	(1.325)	(1.069)
Age in quarters		-0.077	0.498		0.525	0.654
		(0.883)	(0.188)		(0.785)	(0.171)
Squared age in quarters		4.8E-04	-0.001		-0.001	-0.001
		(0.002)	(3.8E-04)		(0.002)	(3.3E-04)
N	350873	350873	350873	413048	413048	413048
1921–1930 Birth Cohort						
Years of completed education	-1.193	0.95	-0.285	-4.377	-0.575	-3.306
	(0.428)	(0.88)	(0.622)	(0.642)	(1.177)	(0.907)
Age in quarters		0.213	-1.067		-0.487	-0.354
		(0.607)	(0.147)		(0.525)	(0.123)
Squared age in quarters		-7.9E-05	0.003		0.002	0.001
		(0.001)	(3.2E-04)		(0.001)	(2.9E-04)
N	435284	435284	435284	472820	472820	472820
1931–1940 Birth Cohort						
Years of completed education	-1.512	1.421	-1.602	-2.946	0.655	-4.082
	(0.441)	(0.768)	(0.721)	(0.525)	(1.264)	(0.882)
Age in quarters		-0.588	-0.744		-1.196	-0.94
		(0.387)	(0.091)		(0.387)	(0.104)
Squared age in quarters		0.002	0.002		0.004	0.003
		(0.001)	(2.5E-04)		(0.001)	(2.8E-04)
N	418250	418250	418250	433866	433866	433866

Note: Standard errors in parentheses. Specifications in [1], [2], [4], and [5] include QOB and QOB×YOB instruments and full sets of YOB and SOB dummies. Columns [3] and [6] include only 3 QOB dummies as instruments and full sets of SOB dummies as controls.

Table 5: **OLS Estimates of the Effect of Dropping Out on Probability of Teen Marriage**

	Men			*Women*		
	[1]	[2]	[3]	[4]	[5]	[6]
Pooled: 1911-1940						
Completed education less than 12 years	0.071	0.072	0.072	0.216	0.217	0.217
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age in quarters		-0.009	-0.002		-0.023	-0.003
		(0.002)	(1.2E-04)		(0.002)	(1.6E-04)
Squared age in quarters		1.6E-05	1.5E-06		3.9E-05	1.6E-07
		(3.9E-06)	(2.8E-07)		(5.3E-06)	(3.8E-07)
N	1204407	1204407	1204407	1319734	1319734	1319734
1911-1920 Birth Cohort						
Completed education less than 12 years	0.054	0.054	0.054	0.218	0.218	0.218
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age in quarters		0.003	-0.008		-0.045	-0.025
		(0.008)	(0.002)		(0.015)	(0.003)
Squared age in quarters		-6.5E-06	1.5E-05		8.4E-05	4.7E-05
		(1.6E-05)	(3.8E-06)		(2.9E-05)	(5.7E-06)
N	350873	350873	350873	413048	413048	413048
1921-1930 Birth Cohort						
Completed education less than 12 years	0.072	0.072	0.072	0.21	0.211	0.21
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age in quarters		-0.003	0.002		-0.016	-0.03
		(0.008)	(0.002)		(0.012)	(0.002)
Squared age in quarters		1.5E-06	-8.3E-06		1.7E-05	6.0E-05
		(1.9E-05)	(4.1E-06)		(2.8E-05)	(5.6E-06)
N	435284	435284	435284	472820	472820	472820
1931-1940 Birth Cohort						
Completed education less than 12 years	0.09	0.09	0.09	0.221	0.222	0.221
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Age in quarters		0.008	0.011		0.002	0.01
		(0.008)	(0.002)		(0.01)	(0.002)
Squared age in quarters		-3.3E-05	-3.5E-05		-2.8E-05	-3.3E-05
		(2.2E-05)	(4.8E-06)		(2.8E-05)	(6.1E-06)
N	418250	418250	418250	433866	433866	433866

Note: Standard errors in parentheses. Specifications in [1], [2], [4], and [5] include full sets of YOB and SOB dummies. Columns [3] and [6] include only full sets of SOB dummies as controls.

Table 6: **2SLS Estimates of the Effect of Dropping Out of Highschool on Probability of Teen Marriage**

	Men			*Women*		
	[1]	[2]	[3]	[4]	[5]	[6]
Pooled: 1911–1940						
Completed education less than 12 years	-0.175	0.026	-0.237	-0.583	-0.459	-0.663
	(0.033)	(0.059)	(0.062)	(0.054)	(0.105)	(0.085)
Age in quarters		-0.009	-0.002		-0.018	-0.006
		(0.002)	(1.4E-04)		(0.003)	(3.6E-04)
Squared age in quarters		1.6E-05	3.8E-06		3.9E-05	1.2E-05
		(4.0E-06)	(5.5E-07)		(6.4E-06)	(1.3E-06)
N	1204407	1204407	1204407	1319734	1319734	1319734
1911–1920 Birth Cohort						
Completed education less than 12 years	-0.041	-0.162	-0.025	0.015	0.067	-0.097
	(0.045)	(0.134)	(0.096)	(0.071)	(0.152)	(0.148)
Age in quarters		0.005	-0.009		-0.036	-0.025
		(0.01)	(0.002)		(0.014)	(0.003)
Squared age in quarters		-7.7E-06	1.7E-05		6.9E-05	5.0E-05
		(1.9E-05)	(4.4E-06)		(2.7E-05)	(6.2E-06)
N	350873	350873	350873	413048	413048	413048
1921–1930 Birth Cohort						
Completed education less than 12 years	-0.212	-0.161	-0.152	-0.848	-0.708	-0.744
	(0.056)	(0.101)	(0.085)	(0.1)	(0.194)	(0.131)
Age in quarters		0.008	0.015		0.008	-0.019
		(0.01)	(0.005)		(0.016)	(0.004)
Squared age in quarters		-2.0E-05	-3.5E-05		-2.3E-05	3.9E-05
		(2.2E-05)	(1.1E-05)		(3.7E-05)	(8.4E-06)
N	435284	435284	435284	472820	472820	472820
1931–1940 Birth Cohort						
Completed education less than 12 years	-0.276	0.214	-0.612	-0.884	-0.745	-1.164
	(0.074)	(0.117)	(0.138)	(0.113)	(0.215)	(0.167)
Age in quarters		0.005	0.01		0.014	0.007
		(0.008)	(0.002)		(0.014)	(0.004)
Squared age in quarters		-2.8E-05	-2.8E-05		-4.4E-05	-1.6E-05
		(2.3E-05)	(6.4E-06)		(3.8E-05)	(1.0E-05)
N	418250	418250	418250	433866	433866	433866

Note: Standard errors in parentheses. Specifications in [1], [2], [4], and [5] include QOB and QOB×YOB instruments and full sets of YOB and SOB dummies. Columns [3] and [6] include only 3 QOB dummies as instruments and full sets of SOB dummies as controls.

Table 7: **OLS Estimates of Effect of Having Less Than 12 Years of Education on Probability of Having a Child.**

	[1]	[2]	[3]
Completed education less than 12 years	0.0726	0.0773	0.0761
	(0.0012)	(0.0012)	(0.0012)
Age in quarters		-0.0529	-0.077
		(0.0042)	(0.0017)
Squared age in quarters		0.0005	0.0006
		(3.0E-05)	(1.2E-05)
White	-0.1039	-0.1037	-0.1037
	(0.001)	(0.001)	(0.001)
N	516370	516370	516370

Note: Standard errors in parentheses. Sample includes all females in 5% 1980 PUMS aged 60–80 quarters. Specifications in [1], [2], [4], and [5] include full sets of YOB and SOB dummies. Columns [3] and [6] include full sets of SOB dummies as controls.

Table 8: **2SLS Estimates of Effect of Having Less Than 12 Years of Education on Probability of Having a Child.**

	[1]	[2]	[3]
Completed education less than 12 years	-0.2168	-0.0313	-0.0104
	(0.0093)	(0.015)	(0.0131)
Age in quarters		-0.0287	-0.0378
		(0.0054)	(0.0062)
Squared age in quarters		0.0003	0.0003
		(4.0E-05)	(4.7E-05)
White	-0.1133	-0.1072	-0.1066
	(0.0011)	(0.0011)	(0.0011)
N	516370	516370	516370

Note: Standard errors in parentheses. Sample includes all females in 5% 1980 PUMS aged 60–80 quarters. Specifications in [1], [2], [4], and [5] include QOB and QOB×YOB instruments and full sets of YOB and SOB dummies. Columns [3] and [6] include QOB dummies as instruments and full sets of SOB dummies as controls.