# ABSTRACT

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| Title of dissertation: | ESSAYS ON UNCERTAINTY AND LEARNING IN HUMAN CAPITAL AND LABOR MARKETS |
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This dissertation considers two aspects of the role of information and uncertainty in decision making. I begin with a broad introduction which surveys recent trends in the literature on human capital development and the role of human capital in labor markets. In Chapter 2, I explore a particular issue related to imperfect information and human capital investment. I apply a framework of investment under uncertainty to parents' decision to invest their time in their children's human capital. I show that parents' risk preferences are an important determinant of the time that they spend with their children. I develop an illustrative model which shows that parents who are more tolerant of risk should invest more heavily in early childhood, and also proportionately more in early than in late childhood. I then use the Panel Study of Income Dynamics (PSID), which contains measures of risk preferences for parents as well as multiple measures of parental time with children over childhood, to show that parents' time use follows the predicted pattern. Moreover, parents' time use follows this pattern more clearly for categories of time use which are more

related to human capital investment.

Chapter 3 considers another aspect of information, this time in the context of the labor market. I follow Gibbons and Katz (1991), who use the Current Population Survey (CPS) Displaced Workers Supplement (DWS) to measure the "lemons effect" of being laid off by comparing the wage outcomes of workers who are laid off to those who are displaced by a plant closing. I present suggestive evidence that when workers find reemployment in jobs which require a similar mix of tasks, this lemons effect of a layoff is mitigated. This finding is inconsistent with simple generalizations of the lemons effect to jobs with multiple tasks. My work begins to reconcile research which focuses on task-based microfoundations of productivity with research on employer learning. I next show that the measurement of the lemons effect is potentially hampered by a measurement issue known as recall bias. The CPS DWS asks respondents about displacement over the previous three years. While workers displaced by plant closing report displacements with equal likelihood over the previous three years, those who were laid off appear to forget displacement at a substantial rate. The measured lemons effect is driven by workers reporting displacement three years ago, when this bias is potentially most important. This is consistent with laid off workers forgetting displacement when they found new jobs with relative ease.

# ESSAYS ON UNCERTAINTY AND LEARNING IN HUMAN CAPITAL AND LABOR MARKETS

by

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# Dedication

To my wife and family.

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# Table of Contents

# List of Tables

# Chapter 1

# Introduction

## 1.1 Information, Parenting, and Wages

Since the work of Akerlof (1970), economists have studied the role of information in a wide range of contexts. When agents have imperfect information, their expectations and the uncertainty that they face are of paramount importance. In this thesis, I consider the role of information in three related contexts.

In Chapter 2, I explore a particular issue related to imperfect information and human capital investment. I apply a framework of investment under uncertainty to parents' decision to invest their time in their children's human capital. A large literature in economics, dating back to Becker and Tomes (1979), uses a framework where parents invest in their children to describe outcomes such as fertility, intergenerational transmission, and intrahousehold allocations of resources. In this framework parents' attitudes towards risk and the uncertainty that they face regarding their children's outcomes should affect their decisions to invest in their children's human capital. But almost none of the work in any of these areas takes this idea seriously.

Chapter 2 shows that parents' risk preferences are an important determinant of the time that they spend with their children. I develop an illustrative model which shows that parents who are more tolerant of risk should invest more heavily in early childhood, and also proportionately more in early than in late childhood. I then use the Panel Study of Income Dynamics (PSID), which contains measures of risk preferences for parents as well as multiple measures of parental time with children over childhood, to show that parents' time use follows the predicted pattern. Moreover, parents' time use follows this pattern more clearly for categories of time use which are more related to human capital investment.

In Chapter 3, I consider another aspect of information, this time in the context of the labor market. Gibbons and Katz (1991) make the argument that when workers are laid off, employers are implicitly sending a signal that these workers are of low quality, since these workers and not others were chosen to be laid off. In empirical work, they show that laid off workers in fact suffer larger wage losses than their colleagues whose entire companies shut down. They dub this the "lemons effect." Recent work suggests that some human capital is specific to particular tasks (e.g. Lazear (2009); Gathmann and Schönberg (2010)) rather than equally useful for any job. If this is true, the lemons effect might be larger in magnitude for workers who end up reemployed in jobs with similar task requirements. Using a definition of similarity between jobs from Gathmann and Schönberg (2010) and measures of occupational task content from Autor (2013) I provide some evidence on this point.

I also find evidence that any measure of the Gibbons and Katz lemons effect should be interpreted with some caution. I show that "recall bias" affects estimates of the lemons effect. Workers are more likely to forget being laid off if they find reemployment quickly at a good wage. By contrast, workers whose plants closed down do not appear to forget layoffs at all. The measured recall bias comes entirely from workers reporting layoffs three years ago, for whom this bias is most important. Thus, the work that measures the lemons effects using retrospective data should be interpreted with caution.

In the remainder of this section, I put the work in Chapters 2 and 3 into a broader context. Section 1.2 describes trends in the economics literature related to parenting. This includes progress on estimating aspects of children's human capital development as well as work that attempts to understand how parents allocate resources to their children. Section 1.3 describes how economists' understanding of human capital has evolved over the last two decades, with particular emphasis on exactly what makes up human capital, how employers predict their workers' productivity, and how employers learn about productivity in labor markets.

## 1.2 Understanding Parenting Behavior: Recent Work

Parents' childrearing decisions affect their children's development. Parents of differing socioeconomic backgrounds choose to raise their children in different ways. By a range of measures, children of parents with higher education and income have significant advantages in life. The literature on the intergenerational transmission of income suggests that children of wealthy parents tend to be wealthy as well, and that at least part of this association is due to parenting (Black and Devereux (2011)). Case, Lubotsky, and Paxson (2002) show that children of wealthier parents appear to respond better to negative health shocks and have better health in adulthood. They also receive more investments from their parents. In particular, Guryan, Hurst, and Kearney (2008) show that more educated parents spend more time with their children in a range of activities. Parents also choose different levels of market inputs for their children. Lino (2012) shows that, unsurprisingly, wealthier parents spend more on a variety of child inputs. Children of higher socioeconomic status parents also likely receive better genetic endowments. Holmlund, Lindahl, and Plug (2011) attempt to determine how much of the intergenerational correlation in educational attainment is due to the causal effect of parental education on children's education and how much is due to inherited genetic endowments. While analyses of twins and adopted children can apportion some of the correlation to genes, a substantial fraction of the correlation can be explained by parents' education. Thus, there are important differences by education in how parents raise their children which affect children's outcomes.

There is also a range of direct evidence on the effects of parental inputs on children's outcomes, though much of this work relies on questionable identifying assumptions. Fiorini and Keane (2012) estimate a variety of models of the production of children's skills using a large number of measures of parental inputs, and find that educational time with parents is consistently one of the most important inputs. Villena-Rodán and Ríos-Aguilar (2012) use instruments based on women's opportunity cost of time to show that an increase in maternal time in educational activities produces a substantial increase in children's test scores. And Bernal and Keane (2011) use instruments based on the generosity of welfare benefits at the state level over time to determine the effect of mothers working on the test scores of their young children. Both find that parental investments have a positive effect on test scores.

While a very large literature in economics attempts to look at the effects of various factors on children's human capital formation and outcomes, parental decisions in their own right have received relatively less attention. Parents' decisions are fundamental to a number of important areas of economic research. One example of this is the work on intergenerational transmission of economic status. In a model of this sort (see Becker and Tomes (1979, 1986) for example), parents choose to either consume out of their wealth or to invest it in their children's human capital. The more parents choose to invest, the larger the intergenerational correlation in income. Models of intergenerational transmission typically include a single parameter common to all parents which indexes preferences for investing in children relative to own consumption.[1] To the extent that economists are interested in international (Björklund and Jäntti (2009)) or intertemporal (Aaronson and Mazumder (2008)) comparisons in the intergenerational correlation coefficient, it's important to understand these preferences more deeply.

Parental decision-making is also important for the interpretation of any evaluation of public policy interventions on the behalf of children. In dynamic models of childrearing such as those in Todd and Wolpin (2003), Cunha and Heckman (2007), or Cunha, Heckman, and Schennach (2010), parents choose inputs depending on the returns to investment in a given period. To the extent that returns to investments exhibit *dynamic complementarity*,[2] a beneficial public policy intervention has two

[1]See e.g. Solon (1999) or Solon (2004).

[2]*Dynamic complementarity*, as defined by Cunha and Heckman (2007), is the concept that higher levels of investment in one period raise the *productivity* of investment in later periods.

effects on children. First, it has a direct effect on raising children's skills in the current period. But it also might make parents more willing to invest in the future, as the returns to their future investments increase as well. It is also possible that parents would see the public inputs into their children as a substitute for their own efforts. In either case, the long term impacts of any sort of intervention need to be interpreted as net of parental investment responses.

Despite this importance to a range of economic issues, the decisions of parents are relatively poorly understood. This is no doubt largely because of the difficulty of modelling parents' decisions and in obtaining detailed information on parental inputs. In considering family decision-making, issues of intrahousehold allocations often have important implications for investments. Theoretical predictions of intrahousehold allocations depend on to what extent utility is transferable within a couple and the relative importance of assortative mating.[3] They also depend on the (difficult to observe) relative preference of each household member for child inputs compared to other goods.

Another difficulty comes from the fact that we often have very coarse measures of household characteristics relevant for how preferences and budget constraints affect decisions. We easily observe characteristics like parental education and income, and we observe outcomes such as fertility, children's test scores, and whether children graduate from college. But even determining the causal relationship between, say, family income and children's test scores (as in Dahl and Lochner (2012)) leaves the family's decision-making process as largely a black box. Income might increase

[3]See Behrman (1997).

test scores either because the income elasticity of child production is positive or because credit constraints are lifted for a particular set of families. To make progress on these issues requires a clear theoretical framework and clear measures of parental inputs.

An important question in the attempt to understand how parents raise their children is whether parents view their investments and their children's human capital endowments as complements or substitutes. Becker and Tomes (1976) and Behrman, Pollak, and Taubman (1982) show that if parents care about the total absolute material well-being of their children, they will invest the most resources in the most able children, who have the highest marginal return on investment. But say parents' preferences over the outcomes of multiple children are concave, and parents care about children's earnings differently than their bequests.[4] Then, they would prefer two children to have modest but equal outcomes over a larger but unequal distribution of outcomes.

Behrman, Pollak, and Taubman (1982) use a structural model of the joint determination of twins' income and education, and find that parents provide more education to the twin with poorer economic prospects. In important recent work, Aizer and Cunha (2012) look at whether mothers exhibit more warmth towards one of their children, as a function of "unexpected" birthweight endowments.[5] They find that mothers invest more in the heavier, better endowed child, which provides

[4]If parents had concave preferences but did not care whether children received their income through earnings or bequests, then parents would invest in each child until the marginal productivity of investments were equal, and compensate the less well endowed child with transfers.

[5]They use controls including pre-natal visits to the doctor and smoking during pregnancy to compare realized birthweight to what might be expected given measured maternal investments.

evidence that investments are reinforcing. Hsin (2011) finds that this behavior depends on parental income: using data from the PSID, she shows that poorer mothers invest more in higher birthweight children, while richer mothers invest more in lower birthweight children, suggesting that compensating investments might happen at high wealth levels. Datar, Kilburn, and Loughran (2010) show that higher birthweight children are more likely to receive investments including breast-feeding, immunizations, and preschool enrollment compared to their siblings. While the evidence thus far seems to suggest that parents invest more in children with higher endowments, it is far from conclusive. And the existence of heterogeneity in these behaviors, as suggested by the work of Hsin (2011), certainly begs further inquiry.

Another important question on which little empirical evidence exists is whether and to what extent public investments in children crowd out private ones. Becker and Tomes (1976) argue that the fact that parental and public investments are substitutes explains some of the difficulty in increasing human capital levels of disadvantaged children. Parents do not need to invest as much in their children if the state will provide some of that investment. And thus rather than raising investment levels, public investments in human capital might crowd out private ones.

A small amount of direct empirical evidence actually suggests that the opposite might be the case, but more work is needed. Some very important recent evidence on this point from Gelber and Isen (2011) suggests that the Becker and Tomes story does not explain parents' reaction to public investments. Gelber and Isen show that parents of children who are randomized into Head Start as part of the Head Start Impact Study are more involved in their children's lives in general.

The work which suggests that children's stock of human capital at a given age and investment are complements (e.g. Cunha, Heckman, and Schennach (2010)), also provides some indirect evidence on this point. In this framework, consider the situation where public investment completely crowed out private investment today. If public investment was provided at a higher level than parents would have provided, children would have higher stocks of human capital tomorrow. And since investments in children with higher stocks of human capital are more productive, parents would invest more tomorrow than without public investment. In a dynamic sense then, even if public investment crowded out all private investment today, it could still to generate both higher levels of children's human capital and higher private investment in the future. Much more work of the sort in Gelber and Isen (2011), which directly links parental investments to plausibly exogenous changes in public investments is warranted.

Another open question in the study of parenting involves ascertaining parents' motivations for spending time with their children. Guryan, Hurst, and Kearney (2008) show that parents who are more educated also spend more time with their children. But Aguiar and Hurst (2007) show that more educated people tend to spend more time at work and less time in either home production or leisure. Thus parental time with children does not have the same relationship to parental education as either leisure or home production. One explanation for this relationship is that parents view time with their children primarily as an investment. More educated parents also tend to hold higher levels of most assets.[6]

[6]See e.g. the Survey of Consumer Finance: Bricker, Kennickell, Moore, and Sabelhaus (2012).

At the same time, it would be incredible to argue that parents don't *enjoy* spending time with their children. Indeed, Krueger, Kahneman, Schkade, Schwarz, and Stone (2008) develop measures of people's flow utility while performing a range of activities. They find that spending time with children is consistently rated as one of the parents' most enjoyable activities. Thus while the aggregate relationship between parental education and time with children is consistent with an investment story, parents do get some pleasure out of raising their children. Ascertaining parents' motives for making their parenting decisions is a question of first order importance, but remarkably little systematic evidence is available on the subject.

Part of the reason for the lack of evidence on parental motivations is the difficulty of disentangling the various motives in parental decisions. In addition to the fact that investments in children at different points in time are not perfect substitutes, parents trade off time along several important dimensions in a given time period. When parents use their resources and energies to raise their children, they face a contemporaneous tradeoff between work, leisure, and spending time with children. Because market inputs might be a (perhaps imperfect) substitute for parental time, the returns to parental time inputs are particularly difficult to identify. When parents spend more time at work, they spend less time with their children but also earn more, and likely purchase more market inputs. Thus an estimate of (for example) how maternal work affects the outcomes of infants will make maternal work look less deleterious, as any negative effects are in part ameliorated by increased market inputs. This will be a problem for identification unless it is possible to perfectly control for market inputs or use a valid instrument. And even a valid instrument for maternal time with children will not be able to disentangle the extent to which market inputs mediate the relationship.

The other important empirical challenge in estimating the impacts of parental inputs is that children's skills and abilities are produced through a dynamic process. The effect of improved school quality in fourth grade depends on a child's skills and abilities at that time. And these in turn depend on past inputs and initial endowments.[7] In the case of randomized interventions, it is still possible to estimate the effect of improved school quality in a particular grade on outcomes, conditional on children's previous inputs. Most easily available data sets often require the strong assumption that investment levels today are the same as all previous investment levels.[8]

One strand of literature has attempted to solve this problem by estimating ever more detailed models of dynamic parental inputs and child development. Todd and Wolpin (2003) were one of the first to estimate a model like this. Cunha and Heckman (2008) estimate a similar model using the Children of the National Longitudinal Survey of Youth and a dynamic factor model. Cunha, Heckman, and Schennach (2010) extends this work to consider more general constant elasticity of substitution production functions. This strand of work requires repeated measurements of children's underlying "abilities" and enough detail on parental investments over time that all inputs are measured.

One empirical strategy which can be useful to address the issue of unobserved

[7]See e.g. Todd and Wolpin (2003).

[8]See the discussion in Todd and Wolpin (2003). Under this assumption, the estimated impacts of increased investments pick up higher investment trajectories over childhood.

child backgrounds is family fixed effects. While many aspects of a home environment are shared between siblings, eligibility for public programs at a given age could vary. By comparing differences in outcomes within a family for whom a program only applied to a subset of the children, we can learn about the effect of a program. For example, this is the strategy that Currie and Thomas (1995) as well as Aizer and Cunha (2012) use to identify the impact of Head Start on children's outcomes. The implicit assumption is that siblings are on average of similar endowment and receive similar inputs, except for the intervention of interest.

But controlling for household level effects by comparing siblings within the same household can also be problematic. Using variation in inputs within the household relies on the assumption that parents' investments in their children are independent of children's characteristics. This assumption is problematic, since Becker and Tomes (1976) show that parents will maximize their utility by making the investments in their children that are most productive. Thus if better endowed children are more receptive to parental investments, parents will invest more in their better endowed children. That the better endowed child receives higher levels of other investments might bias estimates of the returns to various other programs. Alternatively, it is possible that parents care about the equality of their children. Indeed, Behrman, Pollak, and Taubman (1982) provide some evidence using American twins that this appears to be the case.

Rosenzweig and Zhang (2009) show, using data from China, that comparing households which have a twin versus a single birth at higher parity does not consistently estimate the effect on older children of sharing resources amongst additional siblings. Twins are of lower birthweight, have lower APGAR scores, and are more likely to have trouble breathing than singleton births. So if parents reinforce differences in endowments, then the older siblings of twins will receive more investment than their low-endowment twin siblings. This will cause the tradeoff between the quantity and quality of children to be underestimated. Angrist, Lavy, and Schlosser (2010) use a similar strategy in Israel and find little evidence of a quantity-quality tradeoff. In general, the allocation of resources between children within a household must be modelled. For example Aizer and Cunha (2012) show the degree to which parental investments reinforce initial differences in ability is larger in larger families. They also extend a simple household of the type considered by Becker and Tomes (1976) to show that the quality-quantity tradeoff varies with family size. Families who have more children feel their budget constraint more strongly, and hence invest relatively more heavily in the most able children.

Birth order effects are another potentially problematic example of within-household variation. Price (2008) provides evidence using a matching approach in the American Time Use Survey that the oldest child in a household receives 20-30 extra minutes per day at a particular age, compared to their higher parity siblings. And Black, Devereux, and Salvanes (2005) show that children born earlier to a given set of parents attain higher levels of education. But it is very difficult to disentangle to what extent birth order effects are due to biological differences by birth order, parental preferences, or changes in parental skills as they gain experience.

Given the challenge of modelling parental decision-making, the work presented in Chapter 2 of this dissertation makes an important contribution. I take a simple yet unstudied implication of parental investment models, take it to its logical conclusion, and then show its empirical relevance for explaining parental behavior. This is important because it provides some direct evidence on why parents make the parenting decisions that they do.

In particular, I show that parents' risk preferences are an important determinant of their time investments in their children's human capital. The idea that parents face uncertainty when they invest in their children's human capital is fairly obvious. Parents of young children do not know precisely how their children will turn out. Nor can they be certain about the marginal impact of their actions on outcomes. The important question here is to what extent this aspect of parental preferences is important for investments.

To address this question, I develop a simple model of parental investment in their children under uncertainty. Parents have simple mean-variance preferences over their children's outcomes. While children's outcomes are uncertain, parents opportunity cost of investment is certain. This reflects the fact that when parents invest a dollar or hour in their child, they could have used that time for leisure or that dollar for consumption, each of which has a certain value. And I assume that early child represents a high-risk, high return investment while late childhood represents a lower-risk, lower return investment.[9] I show that more risk tolerant parents will invest more than their risk averse peers in all-important early childhood. I also show that they should optimally "tilt" their age-investment profile from late to

[9]While Cunha and Heckman (2007) survey a variety of evidence suggesting that early childhood is a more productive time for investment, the higher risk of early investment is a maintained assumption. I provide a more full discussion of this assumption in Section 2.2.

early childhood.

I then test these empirical implications using data from the Panel Study of Income Dynamics (PSID). The PSID collected two very detailed pieces of information that I exploit in this work. First, it collects a measurement of parents' risk aversion by asking about a series of hypothetical gambles. Second, as part of the Child Development Supplement, the PSID also collects time diaries for each child under the age of 12 as of 1997. It also collects these time diaries again for these same children again in 2002, so that there is a panel of parental time investments for each child. I show that parents who are one standard deviation more risk tolerant spend about 25% more time doing "educational" activities with their children who are four years old or under.

This work has several implications. First, and I would argue most importantly, this work contributes to our understanding of how parents make the decision to invest in their children. This should help economists think about other interventions in child development, intergenerational transmission, and the decisions that families make.

Another implication is that it implies an empirically relevant and easily implementable change to the often estimated structural models of parental investment and child development. These models should explicitly account for parental risk preferences. But fortunately, both the PSID and the C-NLSY have information on risk preferences which could be used for this purpose. Gayle, Golan, and Soytas (2011) even include parental risk aversion in a model of parental investment and labor force participation, but do not use the detailed risk preference data available in the PSID.

Finally, the result that parents' risk preferences matter so strongly for their investment decisions suggest two directions for future inquiry. First, Chapter 2 leaves implicit the source of parental uncertainty about the effects of time investments. I think future work understanding the risks that parents face could be useful. This work could estimate the risks that parents face by looking at the distribution of children's outcomes, conditional on parental inputs. Indeed work similar to Cunha, Heckman, and Navarro (2005), which estimates the ex ante risk faced by individuals investing in their own schooling, could be profitably and easily extended to parental investment decisions. This area might also be ripe for some work which tries to elicit parental beliefs directly. A few simple survey questions could elicit parents' beliefs about their children's likely outcomes and either corroborate my results or not.

Depending on the findings of this line of work, it's possible that there are a class of informational interventions which could in principle be extraordinarily cost effective at raising parental investment, utility, and children's outcomes.[10] If there are many different ways to raise children (as evidenced by the thousands and thousands of books on topics related to parenting), parents might face either significant costs to understand the true best parenting practices, or significant utility costs from the risks borne because they don't know the best way to parent. An ideal parental intervention might change parents' information sets so that they face less risk. As risk averse people, this would increase their welfare. It would also increase

[10]The potential utility gains from reducing parental uncertainty are large. By introspection, parents certainly "worry" a lot about their children. To the extent that this worry represents at least in part a flow of disutility of uncertainty about children's future outcomes, decreasing the amount that parents worry could potentially be very valuable.

their investments in their children, as they would bear lower investment costs. The potential effectiveness of these interventions does depend on both the difficulty of actually affecting parents' information sets, and the fact that the risk parents face must be due at least in part not to inherent randomness in childrearing but to a lack of knowledge. But the large size of gains, at costs much lower than those of most social investments in children's human capital, make preliminary research worth undertaking.

In the following paragraphs, I briefly turn attention to my own outlook on the future of economic research that studies parenting decisions. The discussion does not follow directly from any of my work above, but briefly discusses related directions in current and future research.

An increasing number of papers in the last several years have longitudinally linked administrative databases in order to study longterm outcomes. For example Chetty, Friedman, Hilger, Saez, Schanzenbach, and Yagan (2011) link data from the Tennessee STAR classroom experiment, where students were randomly assigned to classes of reduced size, to administrative earnings records. While this paper in particular provides important evidence on the effect of class size on children's human capital development and future wages, the prospects of this type of research for the understanding of parenting are still largely untapped. The big benefit of this type of work is that it is possible to link inputs at a particular stage of development to the true outcomes of interest. Most current work only links inputs to intermediate outcomes such as test scores.[11] I expect this trend to continue and broaden to other

[11] See e.g. Villena-Rodán and Ríos-Aguilar (2012) or Bernal and Keane (2011).

types of administrative data and other contexts. The ability to trace effects with high accuracy and large sample sizes over large swaths of an individual's life course cannot help but produce new insights.

The other strategy to deal with the problem that investments in children take a long time to come to fruition is to use survey measures of adult outcomes either as a part of a long term general purpose panel (like the PSID) or a long term specific purpose panel (like the National Educational Longitudinal Study (NELS)).[12] However this work is limited to topics covered by large, costly surveys. Even still, this strategy has not yet been carried fully to its fruition using existing data sets. Children in the PSID-CDS were born between 1985 and 1997, and so now range in age from 16-28. As these children grow, a valuable new source of rich data will be able to trace the effects of parental investments through to adulthood. For example, high quality recent work including Bernal and Keane (2011) and Villena-Rodán and Ríos-Aguilar (2012) uses well-conceived identification strategies to look at the effects of mothers working and mothers' time with children on test scores. In the future, these sorts of studies will be able to be extended to examine more distant outcomes including wages, graduation rates, incarceration rates, and other adult outcomes.

While there will always be an important role for research related to the effectiveness of public policies, the persistent concern in this work is how to generalize from a particular policy context to predict the effect of future policies. Understanding how parental investments react to changes in public investments is an important

[12]Cunha and Heckman (2008) and Cunha, Heckman, and Schennach (2010) are examples of work which does this.

aspect of this relationship. Because parents may respond to any change in external investments in their children, its crucial to understand this relationship in order to think about the validity and stability of results like those in Chetty, Friedman, Hilger, Saez, Schanzenbach, and Yagan (2011).

I expect that more work will follow the recent progress in Aizer and Cunha (2012) and Gelber and Isen (2011) in this area. This work provides important insight into parents' preferences. One approach to these sorts of problems involves looking at the effect of large scale interventions using general purpose panel surveys. The effects of large policy changes such as the introduction and roll-out of nation-wide school reforms including No Child Left Behind might be measured using the PSID. Another approach could use administrative records to look at the effect of teacher value added on measures of future parental involvement including perhaps parent-teacher conference attendance.

I think the approach in Aizer and Cunha (2012), which essentially uses a control function to separate children's endowments into expected and unexpected components, can also be applied more broadly. Aizer and Cunha (2012) look at the unexpected birthweight endowment when children are born, but this approach could be extended to measures of test scores later in childhood. While this approach is inherently based on parametric assumptions, it can be applied to the rich panel data sets where the inclusion of a rich set of controls along with careful modelling are the most defensible. It appears that the effect of parental income on children's outcomes might decrease with children's age, so it is reasonable that the degree to which parents compensate or reinforce endowment differences might vary over childhood as well.[13] It is important to understand the direction and magnitude of parental reactions since they might either reinforce or dampen policy interventions.

## 1.3 Wage Setting and Lemons

The other substantive chapter in this dissertation, Chapter 3, focuses not on the genesis of human capital in childhood, but rather on how it is understood and valued in the labor market. The vast majority of wealth in the modern world is held in the form of human capital. Christian (2010) estimates that the market component of human capital in the United States was worth over $200 trillion in 2009. The value of these assets comes from people's ability to earn wages in the labor market, producing goods or services of value. Literature in economics going back to the work of Becker (1975) breaks down human capital into a general component, which could be equally productive for any firm, and specific component, which is productive only for a worker's current job. A sprawling body of work since Becker has fleshed out and applied these ideas throughout the labor market.

One particular way that Becker (1975)'s work has been expanded is to put increasing detail on the dichotomy between specific and general human capital. Rather than human capital which is either specific to a single firm or general to all firms, this body of work argues that general human capital is general only to the extent that firms share particular characteristics. Neal (1995) studied how the reemployment outcomes of workers displaced from their firms depended on their industry of reemployment. If human capital was only either specific to a firm or general to

[13] See Duncan, Morris, and Rodrigues (2011), for example.

any firm, then reemployment wages should be similar no matter whether workers stayed in the same industry or not. To the extent that firm-specific human capital is important and is accumulated over time on the job workers should experience wage losses that are increasing in their tenure at their previous job which do not depend on whether the worker stays in the same industry or not. But in fact Neal (1995) found when workers were displaced but remained in the same 2 digit industry, their wage losses depended half as strongly on tenure as those who switched industries. This suggests that workers who were displaced but remained in the same industry were able to transfer more of their skills to their new jobs. Skills of this sort are neither specific only to a firm nor general to any firm, but are specific to the set of firms within an industry.

Another piece of evidence from Kambourov and Manovskii (2009) suggests that human capital might importantly be specific to a worker's occupation, rather than her industry. The authors use the PSID to observe workers as they change their firm, industry, and occupation, and find that there are substantial returns to occupational tenure. They also find that after controlling for occupational tenure, there are no longer returns to working at the same firm or in the same industry. Shaw (1984) attempts to construct a measure of experience which accounts for previous employment in similar occupations. She argues that occupations with many mutual job transitions require more similar skills. She then shows that a measure of experience which weights prior experience by transition-based similarity predicts wage growth over workers' careers better than pure experience measures.

The fact that workers can develop skills which are productive in some jobs but not others suggests that there is an important role for an aspect of worker's human capital which is neither fully generally applicable across jobs nor completely specific to a single job. Lazear (2009) develops a theory of a "skill weights" approach to human capital which explains this phenomenon. In his formulation, workers have differing amounts of different skills, which are the ability to perform different tasks. Skills are completely "general" in that they aid in the performance of a given task at any job. And a particular job requires the performance of a particular bundle of tasks. But because each job requires a different mix of tasks, a substantial portion of human capital will appear specific to a particular firm or job. Lazear's idea is compelling in that it provides a clear and concise explanation of the empirical results discussed above.

Several recent papers attempt to operationalize this concept and find that task specific human capital is able to explain observed patterns in the labor market which can't be described with Becker's theory. Gathmann and Schönberg (2010) extend Lazear's model to allow for returns to experience in performing a particular bundle of tasks which is useful in other jobs to the extent that they require similar tasks. Gathmann and Schönberg use the German Qualification and Career Survey, which is a survey of employees carried out by the German Federal Institute for Vocational Training (Bundesinstitut für Berufsbildung; BIBB) and the Research Institute of the Federal Employment Service (Institut für Arbeitsmarkt und Berufsforschung; IAB). This survey asks workers not just for their occupational title, but whether and to to what extent they must perform any of 19 different tasks in their job. Gathmann and Schönberg's task model predicts that workers who change jobs earlier in their career are relatively more likely to make transitions to jobs requiring different skill mixes. This prediction is born out in the German data. Their measure of task specific human capital is able to explain explain about half of wage growth over worker's careers in the data.

In the United States, there is no survey data on the tasks performed by individuals. The closest approximation comes from using information from the Dictionary of Occupational Titles (DOT) or the Occupational Information Network (O*NET), which provide information about the work involved in detailed three digit occupations.[14] Autor, Murnane, and Levy (2003) use the DOT to quantify the extent to which each occupation requires the performance of tasks which are routine and non-routine, as well as cognitive, manual, and interactive. Poletaev and Robinson (2008) use the same DOT data and address a very similar issue to Neal (1995) using factor analysis. They identify the principal skill components used in occupations based on the task categories in the DOT. Then, in addition to considering how reemployment wages vary with the change of industry or occupation upon reemployment, Poletaev and Robinson look at how wage outcomes depend on whether the new job has a different first or second principal component. The authors find that 4 principal components are able to explain more than 70% of the variation in the skill requirement of occupations. They define measures of the "distance" of changes in the skill requirements in new jobs, which capture the intuition that even if many workers change detailed occupations after displacement, many are still in

[14]For a discussion of the information included in DOT, see Autor (2013).

jobs which employ roughly the same skills.[15] Wage losses are more substantial for workers who find jobs using different skills than for those who find jobs in different occupations or industries.

In addition to explaining worker's job transitions over their careers, wage growth during voluntary job changes, and wage losses from displacement, the task approach provides a parsimonious way to model differences between jobs. Yamaguchi (2012) develops a Roy (1951) model using a low dimensional skill vector where workers have skill and preferences for each task. Yamaguchi is able to parsimoniously model a wide variety of occupation options by using their low-dimensional task content from the DOT.

In the future, work in labor economics will likely integrate the idea of tasks into a variety of different areas of inquiry. Tasks are both conceptually appealing and parsimonious. One particular area where I expect tasks will gain traction is in the understanding of employer learning and the role of information in the labor market. I provide some tentative results in this direction in Section 3.2. Below I discuss some of the trends in this literature.

It is crucial for employers to know how productive their employees are. In complex production processes, where workers work in groups and produce outputs whose quality is hard to observe, this is often a difficult task for managers. This might suggest that incumbent employers know more about a worker's productivity than outside employers. Jovanovic (1979) develops a model along these lines, where

[15] They show that even using the 14 1-digit occupation codes, nearly 50% of their sample reports changing occupations after displacement, but that using a skill-based definition of job, only 28% of workers switch jobs.

employers learn about workers' (firm-specific) productivity over time. Because Jovanovic models a firm specific match, employees who are found to be unproductive leave to seek out higher wages, and wages rise with job tenure. Waldman (1984) develops a model where incumbent employers know workers' ability perfectly, high ability workers have higher productivity in higher responsibility jobs, and outside employers observe only workers' assignments and not their actual productivity. Here, employers under-promote high ability workers in order to keep their private information.

There is a long literature which considers the implications of asymmetric information of this sort on employer behavior. Acemoglu and Pischke (1998) argue that employers have monopsony power because of their informational advantage, and this allows them to profitably invest in general training. And work including Golan (2005) considers how the incumbent and outside employers might engage in bidding wars when one employer has superior information.

But the empirical literature has lagged behind the theoretical literature in this area, most likely because of the considerable difficulties in measuring worker productivity and employer information sets. Even so, there is a sizable body of work attempting to measure employer learning through indirect methods. One important distinction between strands of this work is whether employer learning is modelled as symmetric or asymmetric between firms.

Farber and Gibbons (1996) and Altonji and Pierret (2001) test for the existence of employer learning under the assumption that all learning which takes place is completely public. These papers argue that as workers gain experience, their wages should be less correlated with easily observed proxies for productivity and more correlated with factors unobservable to employers but correlated with productivity, including test scores. Lange (2007) takes this idea further and quantifies the speed at which employers learn about worker ability. Lange (2007) finds that firms have learned about the vast majority of worker productivity over the first three years of employment.

There is also work which finds a sizable role for asymmetric employer learning. Gibbons and Katz (1991) estimate the "lemons effect" of being laid off by comparing the reemployment wage outcomes of workers who were laid off to those whose plants shut down. The assumption here is that by choosing to lay off a worker, employers are revealing some of their private information about worker productivity. If employers had no private information, this lemons effect should not exist. Additional evidence of asymmetric information is provided by Acemoglu and Pischke (1998) in the context of the German labor market. They show that workers drafted into the military (exogenously) have higher wages than those who quit or are laid off.[16] This is consistent with asymmetric information because the workers who choose to leave the firm are those who the incumbent firm knew were less productive, so outside firms make accordingly lower wage offers.

Schönberg (2007) attempts to test for the existence of asymmetric versus symmetric information in two ways. First, she shows that among college educated

[16]They also show, intriguingly, evidence of monopsony power on the part of firms who keep their apprentices. Workers who join the military are paid *more* than those who stay in their apprentice firm. Workers who join the military are of average productivity since they are randomly selected, while workers who remain in their firms are of above average ability since they were not laid off. Acemoglu and Pischke argue that this fact is evidence that workers who remain with their apprenticeship firms are paid below their marginal product.

workers, AFQT scores are inversely related to the probability of making job-to-job transitions. This is consistent with the theoretical prediction from the model of Greenwald (1986) that movers are adversely selected. She also looks for asymmetric information by testing whether the AFQT score increasingly determines wages over tenure at a given firm, rather than over a worker's career as in Farber and Gibbons (1996), and finds that learning appears mostly symmetric. Pinkston (2009) finds that employer learning is mostly *asymmetric*, using a related test where he argues that wages should be increasingly correlated with test scores within *spans of continuous employment* rather than tenure at a particular firm. That Schönberg and Pinkston have conflicting findings which depend crucially on assumptions about how incumbent employers match outside wage offers suggest that more work needs to be done, potentially using different empirical strategies. Zhang (2007) develops a test for asymmetric information based on workers' employment histories. If employers learned symmetrically about worker ability, then all employers will have the same current state of knowledge and previous job mobility should not be related to ability. Zhang finds evidence that the relationship between worker ability and wages is stronger for workers who have experienced more job turnover. Kahn (2008) also develops a test for asymmetric information between employers, which is based on the variance of wage changes. Kahn shows significant evidence of asymmetric information.

Most recently, work on employer learning has taken a turn towards a more nuanced view of the employment context. Indeed, it is incredible that a single measure of worker "ability" is learned about uniformly by employers in every industry and occupation. Mansour (2012) shows that the speed of employer learning, as measured by the Lange (2007) method, varies substantially between occupations. Light and McGee (2012) use multiple components of the NLSY's ASVAB tests combined with skill importance data from O*NET to show that employers learn about different skills in different occupations. Sometimes, for skills which are particularly important to a firm's production process, employers screen entrants on skills rather than learn about them after hiring. Further complicating the matter, Kahn and Lange (2010) use longitudinal administrative data from a particular firm which also includes measures of performance to understand exactly what employers learn about. They find that employers are learning about a moving target: workers' productivity, as measured by performance reviews, changes heterogeneously over their careers.

There is much more valuable work to be done along the lines of these recent papers. While it is possible to estimate aggregate measures of the speed or importance or symmetry of employer learning, these concepts are almost certainly dependent on context. Evidence from firms with easily measurable outputs, as with the fruit pickers of Bandiera, Barankay, and Rasul (2009), would be useful to further disentangle learning from heterogeneous human capital accumulation at least within a particular workplace context. Further work along the lines of Light and McGee (2012), which recognizes that people have multidimensional abilities, and different employers care about (and learn about) these abilities in different proportions, would be useful. One particular piece of low-hanging fruit would be to study learning about workers' non-cognitive skills.[17] Personnel records which combine personality and

[17]Heckman, Stixrud, and Urzua (2006) argue that non-cognitive skills are at least as important

other batteries from the hiring process with panels of productivity measures and wages might be useful in this regard.

In the longer term, I hope it will be possible to construct very comprehensive measures of productivity from workers' data trails. Textual analyses of emails, and patterns in workers calendars hold the promise of measuring productivity in a very fine way for many workers. For example, consider a worker who plans events of modest size such as weddings or small conferences. Based on dates, places, and other event parameters mentioned in emails or phone conversations, it should be possible to see how much correspondence the employee used to plan the event. It should also be possible to see when the correspondence began. Thus an analyst could produce estimates of the time spent planning an event and hence of worker productivity. Though these measures would be imperfect, they would permit very fine-grained measurement of worker productivity. While privacy issues certainly present challenges for the analysis of such personal data for research purposes, much of the data is already used for the arguably more invasive purpose of targeted advertising. The use of this textual data for economic research is far from a reality: only in the sphere of measuring political slant (e.g. Gentzkow and Shapiro (2010)) or in measuring public mood (e.g. Bollen, Mao, and Zeng (2011)) have even rudimentary patterns been gleaned from text. With measures of employee actions tied to actual measures of firm inputs and production, patterns of employer learning can at least be studied at the firm level.

for wages as cognitive skills, suggesting that they are also important for productivity and for employers to understand.

In this dissertation, I provide some suggestive new evidence on these points. First, in Section 3.2 of Chapter 3, I combine the task literature with the employer literature. I revisit the Gibbons and Katz (1991) lemons effect measurements incorporating the idea of learning about task specific human capital. In particular, I look at the task requirements of a displaced worker's current and previous job. I divide the space of all tasks into six categories, following Autor, Murnane, and Levy (2003); Autor (2013), of routine and non-routine manual, cognitive, and interpersonal tasks. I then create a measure of task overlap between the two jobs, following Gathmann and Schönberg (2010). In this framework, I argue that if employers learn about general productivity, the lemons effect should not vary with task overlap. But if employers learn about task-specific productivity, the lemons effect should be smaller when workers find reemployment performing different tasks since whether or not a worker is a lemon at a particular task is meaningless for their performance of very different tasks. I show that, surprisingly, the lemons effect is in fact *larger* when workers find reemployment in jobs requiring different task mixes.

This analysis is certainly imperfect: at it's core it rests on the assumption that the only systematic differences in wages between workers whose plants closed down and laid off workers, conditional on observable characteristics, are due to the informational effect of reason for displacement. This is an assumption which cannot be verified. But Gibbons and Katz (1991) spawned a range of work using this measure of adverse selection in the labor market. The value of Chapter 3 comes in part from the fact that it tests the assumptions that underlie this growing body of work.

In the course of this work, I also show that "recall bias" is an important driver of the measured lemons effect. Recall bias occurs when workers disproportionately forget layoffs compared to plant closings if they were subsequently reemployed at good wages. Since the lemons effect, which captures the degree of negative signal faced by workers, is measured by comparing the reemployment wages of laid off workers to those displaced by plant closings, this bias is of first order importance. The recall bias pattern can be seen in the number of workers reporting each type of displacement retroactively over different periods of time. Further, the measured lemons effect is almost exclusively driven by workers reporting layoffs in the most distant time period. I show that this recall bias story does not appear to be driving the interaction of the effect of being laid off and taking a job with a different set of skills.

While the work in Chapters 2 and 3 might seem somewhat unrelated, there are common themes. Both chapters consider the decisions that agents make as a primary object of study. Chapter 2 studies the risks and decisions that parents face in raising children, while Chapter 3 considers how firms' ability to learn about worker productivity and their demand for workers to perform particular tasks interact to generate adverse selection in the labor market. Both chapters use existing public data in new ways to shed light on these topics. Chapter 2 combines parental time diary data as a measure of investment with survey based measures of risk tolerance, while Chapter 3 uses information on the task content of occupations and surveys of displaced workers. In these two ways, the two remaining chapters fit together to form a cohesive dissertation.

# Chapter 2

# Uncertainty and Parental Investment in Children

## 2.1 Introduction

It is obvious to any parent that the time and energy spent raising children is rewarding. But there is also evidence (e.g. Guryan, Hurst, and Kearney (2008)) that parents view their children at least in part as an investment good. Investment in risky assets depends on the expected returns, preferences for risk, and the risks faced. Though there is a large literature that analyzes parental investments in children, in both a theoretical and empirical context, little of this work explicitly considers parents attitudes towards risk.

In this work, I take seriously the idea that parents' attitudes towards risk and the structure of risk matter for their investment in children, and I show that these dependencies are empirically important. Using measures of parental risk tolerance and measures of parental time investment over childhood from the Panel Study of Income Dynamics (PSID), I show that risk preferences matter for time allocation.[1] More risk tolerant parents spend more time with their children in early childhood. In late childhood, the pattern reverses, and more risk tolerant parents spend *less* time with their children.[2]

[1]In all analysis, I use a measure of *relative* risk tolerance which is adjusted for measurement error according to the method of Kimball, Sahm, and Shapiro (2009a). Details on the measure and adjustments can be found in Section .

[2]Due to data limitations, some of these empirical effects are estimated imprecisely. Yet point

I develop an illustrative model that explains this behavior. Parents invest in their children over two periods of childhood. Investment in early childhood is more productive than investment in late childhood. It is also riskier. Investments over childhood are (imperfect) substitutes (Cunha and Heckman (2008)) so that a given level of human capital can be produced with different combinations of investments in early and late periods. More risk tolerant parents choose more high risk, high return investment in early childhood and less low risk, low return investment in late childhood. They therefore "tilt" their time investments away from late childhood and towards early childhood.

Krueger, Kahneman, Schkade, Schwarz, and Stone (2008) estimate the flow value of utility that people receive from doing particular activities. They find that time that spending time with children is one of parents' most enjoyable activities. So parenting is in part a form of consumption. While there is no doubt that parents enjoy raising their children, parents also appear to view their children at least partly as an investment good. Guryan, Hurst, and Kearney (2008) show that more educated parents spend more time with their children. They discuss various interpretations of this finding, a leading one being that time spent parenting is an investment. In general time use patterns suggest that, compared to less educated people, more educated people spend relatively less time in home production and less time in leisure (Aguiar and Hurst (2007)). Thus if more educated parents spend more time with their children, this is for reasons that are not related to either leisure or home production.

estimates suggest substantial effects which can sometimes be bounded away from zero.

There is a lot of work where it is assumed, explicitly or implicitly, that parents invest in their children. Theoretical work where parents invest in children goes back to Becker and Tomes (1979), who study the implications for intergenerational mobility. Gayle, Golan, and Soytas (2011) estimate the returns to parental time investment in children and find that the returns (in the form of educational attainment) to parental time investments are higher for blacks than whites and for mothers than fathers. In the course of estimating the *technology of skill formation* – how parental investments produce children's skills over childhood – Cunha, Heckman, and Schennach (2010) account for the endogeneity of parental inputs by jointly estimating the parental investment decisions as a function of children's abilities. They show that early childhood investments have a much larger effect on cognitive skills than late childhood investments. Kalil, Ryan, and Corey (2012) show that more educated parents spend relatively more time in "developmentally appropriate activities," which is consistent with parents investing in their children, and more educated parents paying more attention to the productivity of their time with children. While these papers consider parental time with children as an investment, they pay little attention to the role of uncertainty over the rate at which time investments produce outcomes.

The uncertainty that parents face when investing in their children's human capital is likely substantial. There is evidence that investment in one's own schooling is a risky investment for individuals.[3] The uncertainty that parents face when

[3] It has been documented that the returns to investment in human capital are realized with some significant uncertainty. Palacios-Huerta (2003) analyzes the human capital risk-return profile with methods from finance. Shore, Barth, and Jensen (2010) estimate the income volatility that individuals face in their careers, and show that it can be considerable. Cunha, Heckman, and

investing in their children is necessarily even greater. To the extent that there are aspects of a child's circumstances at age 18, when they make the decision about how much higher education to obtain, that are unknown to parents when children are age four, then there is additional uncertainty. And of course the great challenge of raising children means that even the most experienced parents aren't completely sure how much their actions help their children. Despite the evidence that uncertainty must be important for parental investments in their children, it has been largely ignored in the literature on parental investments.

In the empirical section of this chapter, I use the Panel Study of Income Dynamics Child Development Supplement (PSID-CDS) time diaries and the PSID Risk Tolerance Supplement to provide evidence on how parents' time investment in children depends on parental risk tolerance. The data include measures of both the risk tolerance of the PSID sample respondent and how much time each parent spends with each of up to two children in both 1997 and 2002. By using a sample of children that was under 5 for the first time diary in 1997 and older in 2002, I can observe parents' investments over the course of childhood. Using the rich information on the activity and who was present in the time diary data, I create detailed measures of parental investment. Importantly, while risk tolerance is a fundamental parameter in parents' utility functions, I show that relative risk tolerance as measured in the PSID is essentially uncorrelated with other family level observables. This is important for

Navarro (2005) decompose the ex-post uncertainty in returns to college into a component that is predictable by individuals and the residual which represents uncertainty that faces individuals. They find that about 40% of the ex-post heterogeneity in returns to education represents uncertainty faced by students. Work following Cunha, Heckman, and Navarro (2005) refined the methods used, but to my knowledge no papers argue that students investing in their human capital is unimportant.

my analysis since I rely on the assumption that, conditional on observable control variables, risk tolerance is uncorrelated with other unobserved family characteristics that affect parental time with their children.

While the precision of my estimates is limited by a small sample size, I find evidence that parents' risk preferences are an important determinant of the time that they spend with their children. The relationship between risk preference and time use is consistent with a model where risk averse parents spend time with their children as an investment in human capital. More risk tolerant parents spend more time in educational activities with their young children. Parents who are one standard deviation more risk tolerant spend 28% (from 7 to 50% with 95% confidence) more time in educational activities with their young children. By comparison, the gap in educational time between families where the mother is a college graduate and a high school graduate is about 30%, and the gap between families where the wife does not and does participate in market work is about 50%. In overall time use and recreational activities, more risk tolerant parents spend about 4% more time with their children, and these estimates are not statistically significant. The effect of risk tolerance being larger in magnitude for educational time, which is most clearly related to human capital investment, is consistent with the model of investment. In late childhood the patterns are different. Risk tolerance is no longer related to educational time at all. And parents who are a standard deviation more risk tolerant spend about 6% (−1% to 12%) *less* time with their children overall, and about 9% (−1% to 19%) less time in recreational activities.

This evidence suggests that risk tolerance matters for parental time investments, especially those in educational activities. The magnitudes of these effects, while sometimes imprecise, are large and economically significant. The signs of some of these coefficients vary throughout childhood in ways that might not be immediately obvious. For example, it doesn't seem to be the case that more risk tolerant parents just invest more in their children throughout childhood. Why might risk tolerant families invest more time altogether in early childhood but less in late childhood? And why might risk tolerant parents spend more educational time with their children in early childhood but a similar amount in late childhood?

In order to explain these stylized facts, I build a simple illustrative model that represents parental decision making under uncertainty. Two periods of development are necessary to generate different relationships between risk tolerance and investment over childhood and reflect the growing literature which underscores differences in the productivity of investments over childhood and a lack of perfect substitutability of investments over time (e.g. Almond and Currie (2011) and Cunha, Heckman, and Schennach (2010)). Parents are uncertain about the effectiveness of their investments in their children. The more they invest in a given period, the more that period's investment contributes to the risk that they face. The risk of investment in early childhood is greater than the risk in late childhood. In this framework, parents can produce the same expected level of human capital in their children using different combinations of time in early and late childhood, with different total time costs and different risks.[4]

Using this framework, I consider how parental investment in children varies

[4]The model does not include non-time investments.

over childhood by parental risk tolerance. Risk tolerance across parents affects their behavior along two margins. First, consumption is certain, while the future payoffs to investment in children's human capital are uncertain. Therefore, more risk tolerant parents will choose higher levels of investment throughout childhood. More risk tolerant parents also change the composition of their investments over the course of childhood. In particular, since investments in younger children are riskier, but presumed to be more effective per unit of time, while investments in school-age children are less risky but also less productive, risk tolerant parents will choose to invest relatively more intensely in their young children and relatively less intensely in their school-age children.

Before proceeding further, it is worth clarifying the nature of risk and uncertainty that I am discussing here and how parental investments contribute to the risk that they face. When parents decide to devote a unit of time to their child, they give up the opportunity of using this time for a certain level of benefit. Parents might have worked for an hour in the labor force or they might have engaged in an hour of leisure. Each of these provides a certain benefit. But when they spend an hour investing in their child, they aren't sure exactly how much this investment will improve their child's prospects. As parents invest more in their children, they are increasingly sacrificing certain utility in the form of wages and leisure for uncertain children's human capital. Thus the total uncertainty that they face increases as they invest more in their children's human capital.

There are several potential sources of this uncertainty which parents face. It might be that children are of different types, so that some types of children will respond well to an investment while others don't. For example, some children might have a relatively higher capacity to learn reading skills. Children of this type would by definition benefit more significantly from time spent reading together than children of another type.[5] To the extent that a child's type is unknown to a parent, this will make parents uncertain about the effects of reading to a child. Another potential source of uncertainty is the price of skills in the labor market. Even if parents knew exactly how much skill they would endow in their children from each unit of investment, to ultimately realize the benefits of human capital, the children would need to sell their skills in the labor market. But as can be seen from the growth in the returns to college over the last three decades (see e.g. Acemoglu and Autor (2011)), the price of skill can vary considerably between early childhood and adulthood. A third potential source of uncertainty is that parents have a "skill" at parenting, which is their specific rate at which a unit of their time instills human capital, and parents are uncertain about this skill.

In Section 2.2, I introduce a simplified illustrative model of a parent investing in a single child during two periods of childhood where uncertainty is present. This model generates predictions that are consistent with the empirical work in Section 2.3. Section 2.3.1 discusses the PSID Child Development Supplement which includes the time diary data used in this chapter as well as the PSID Risk Tolerance Supplement which provides estimates of parental risk preferences. Section 2.3.2.1 shows that risk tolerance is virtually uncorrelated with observable parental characteristics

[5] Children might also have unobserved "ability" types which make any sort of investment more productive.

and are thus is unlikely to be correlated with unobserved parental characteristics either. I present the results of regressions of parental time investments on risk tolerance in Section 2.3.2.2. I conclude in Section 2.4.

## 2.2 Illustrative Model of Uncertain Parental Investment

In this section I develop a simple illustrative model that can explain the patterns in the data. In particular, this model explains why more risk averse parents spend more time with their children in early childhood and less in late childhood. Economic models of parenting go back to at least to Becker and Tomes (1979), who model parents as choosing between their own consumption and investments in their children's human capital. A model of this sort, with only a single parental investment, cannot fully capture parental responses to the varying productivity of investments over childhood. Cunha and Heckman (2008) model parental investments in children over multiple periods of childhood and show that early childhood investments are substantially more productive. They also note that investments in early childhood raise the productivity of investment in late childhood, which they term *dynamic complementarity*. I start with an illustrative model like that of Cunha and Heckman with two periods of investment, and build in uncertainty over the efficacy of inputs.

Consider a situation where a two parent household exogenously has a single child. The child is young for one period, school-age for one period, and then child services are realized. In each period, parents have $H$ hours to divide between time investment in children and work. Parents receive wage $w$ for hours spent working and purchase consumption when the child is young ($c_1$) and/or school-age ($c_2$). Mothers and fathers are perfect substitutes for each other both in parenting and in the labor force.[6] Parents maximize a unitary joint utility function over consumption and child services.[7] Parents can borrow and lend freely at no interest, and discount the future at rate $\delta$. Parents choose time investment when children are young ($y$) and school-age ($s$). These physical investments generate random levels of *effective investment* when children are young ($Y$) and school-age ($S$). These effective investments produce children's labor market outcomes through a human capital production function $H$. Parents have utility $u_c$ over children's realized human capital $H$. Thus parents' problem can be written as:

$$\begin{aligned} \max_{c_1,c_2,y,s} \quad & u(c_1) + \delta u(c_2) + \delta^2 E\left[u_c[H(Y,S)]\right|y,s] \qquad (2.1) \\ s.t. \quad : \quad & \underline{\mathrm{y}} \leq y \leq H \\ & \underline{\mathrm{s}} \leq s \leq H \\ & c_1 + c_2 \leq w(2H - y - s) \end{aligned}$$

Note that $y$ and $s$ are also bounded from below by $\underline{\mathrm{y}}$ and $\underline{\mathrm{s}}$ in Equation (2.1). This simplification captures the intuition that there are potentially two very differ-

[6]The assumption that mothers and fathers are perfect substitutes serves only to simplify the analysis. Imperfect substitution would result in similar results holding for maternal and paternal time separately.

[7]There is a long literature on the validity of the assumption of unitary households and intra-household bargaining. As will become clear in Section 2.3.1.2, I only observe one measure of risk tolerance per household, so unitary preferences greatly simplify the relationship between my empirical measure of risk tolerance and the representative preferences of the household.

ent domains of investment in children. At the low end, if parents are completely neglectful and provide inadequate resources for their children, there are important chances for child mortality. This is the domain that readers may initially consider when they think about risk and parenting. To the extent that in reality parents might have a completely different decision framework over these sets of outcomes, I rule them out by assuming investments only affect the probability of them happening with $y < \underline{y}$ and $s < \underline{s}$.[8]

In order to explain the empirical regularity that more risk tolerant parents tilt their investment profile, I need the risks of investment to be different for investments in early and late childhood. This, combined with the assumption from Cunha and Heckman that early investments are more productive gives parents a problem that is similar in some ways to a portfolio allocation problem. Early childhood investment is high risk and high return (like a stock) while investment in late childhood is low risk and low return (like a Treasury bond). Just as more risk tolerant people invest a greater share of their portfolios in risky assets as in (e.g. Breeden (1979), Kimball, Sahm, and Shapiro (2008)), more risk tolerant parents will invest relatively more of their total time investment in their children during early childhood.

Early childhood investments could reasonably be more risky for two different reasons. First, different unobserved 'types' of children might create additional uncertainty. Consider the situation where a child has a specific but unobserved receptivity to investment, so that for some children a unit of investment is more

[8] It would be possible to use a single utility maximizing framework to encompass the entire range of children's outcomes, from untimely death to successful college graduate. But the assumption that $y$ and $s$ are bounded from below, combined with the assumption of an interior solution, serves to focus ideas for the present discussion.

productive while for others it is less productive. If parents do not know the specific productivity of their child, then this uncertainty represents a source of risk to them. In a simple learning model, it is possible that as children age, parents learn about this child-specific productivity of investment. As parents learn, this extra source of risk disappears, and the risk of investment decreases.[9] Another mechanism which could create decreasing risk with age is that parents face uncertainty over the price of the skills that they transmit to their children in the labor market. If parents knew exactly how productive their investments were in terms of generating skills, but did not know the price of these skills in the labor market, then early investments would be very uncertain. When a child is two years old, there are still 16 years until he or she would graduate high school and potentially enter the labor force. When the child is age 12, there are only another 6 years. To the extent that the price of skills is more precisely forecastable less far in the future, the risk that parents face will be decreasing as their children age.

I assume that realized human capital investments are distributed with mean given by the investment level chosen by parents $(y, s)$ and a variance that is proportional to the investment level. That is, $(Y, S)$ are distributed with mean and variance given by Equations 2.2 and 2.3, respectively.[10] The implications of the

[9]It is also conceivable that this child-specific characteristic is not a pure vertical shifter of investment productivity, but instead represents horizontal differences between children. For example, some children might respond better to investments related to reading, while others respond better to investments related to numerical learning. When parents do not know their child's type, this represents an extra source of uncertainty. But when parents learn the type, they are able to correctly tailor their investments and they decrease the uncertainty that they face.

[10]Note that Equation (2.3) suggests that variance increases linearly in investment levels. The "toy" example of betting on a football game in the introduction would suggest that each unit of investment is perfectly correlated, so variance would increase with $y^2$. The assumption here is equivalent to each unit of investment having an independent effect. Note that this does not mean that there is no dynamic complementarity: higher early investments imply higher later

illustrative model should not change as long as variance increases *at least* linearly in investment levels. This generates utility costs of risk borne which are weakly convex in investment.

$$E\left[\begin{pmatrix} Y \\ S \end{pmatrix}\right] = \begin{pmatrix} y \\ s \end{pmatrix} \tag{2.2}$$

$$\mathrm{var}\left[\begin{pmatrix} Y \\ S \end{pmatrix}\right] = \begin{pmatrix} y\sigma_y^2 & 0 \\ 0 & s\sigma_s^2 \end{pmatrix} \tag{2.3}$$

Parents choose to invest an actual number of hours, $(y, s)$, throughout childhood. These are *actual hours* in the sense that they enter into the time budget constraint of Equation (2.1). The larger the choice of time investment, the higher the mean (and variance) of the distribution of realized human capital investments, $Y$ and $S$. That realized human capital investments don't equal inputs in general captures the fact that parents are uncertain about the exact effects of their investments. Parents know that if they choose to spend more time with their children, this will, on average, improve the children's outcomes. That $dE[Y]/dy = 1$ amounts to a normalization.

I assume that parents have complete knowledge of the way that realized human capital investments $Y$ and $S$ translate into labor market outcomes $H(Y, S)$. Though this assumption seems strong, it also represents a normalization within my empirical context. To take this illustrative model to data, I will look at how time investments depend on uncertainty. It is possible that parents are uncertain about how their time

productivity of investments. The *stochastic realization* of early and late productivity are assumed to be independent.

investments translate into human capital, the form of the human capital production function, or in the future value of human capital in the labor market. I choose to put all uncertainty into how parents' time builds children's human capital, but this is just an arbitrary division of uncertainty between uncertainty in the efficacy of inputs and the production function for outputs. Other divisions would increase the complexity of the situation without empirical content in this context.

The next necessary assumptions are on the form of $H(Y, S)$. I assume that for any level of time investment $X$:

$$\frac{\partial H(X,X)}{\partial Y} > \frac{\partial H(X,X)}{\partial S} \tag{2.4}$$

Equation (2.4) says that at the same level of human capital investment at each point in childhood, the marginal unit of investment when children are young is greater. This is the assumption that brings in the findings from a growing literature on early childhood investments that investments in young children are productive (see Almond and Currie (2011)).[11] While it is not crucial to the points considered here, this illustrative model can allow for the findings from Cunha and Heckman (2008) and Cunha, Heckman, and Schennach (2010) that early and late investment in children are complementary in the production of adult skills.

[11]Since $H$ is concave in each argument, it shouldn't in general be true that $\partial H(Y,S)/\partial Y > \partial H(Y,S)/\partial S$ at any $(Y,S)$. I assume that investment in early childhood is more productive by saying that if investments over childhood were at equal levels, investment in early childhood would be more productive.

### 2.2.1 Equilibrium in the Illustrative Model

When parents optimize, the following first order conditions hold:

$$u'(c_1) = \delta u'(c_2) \tag{2.5a}$$

$$u'(c_1) = \delta^2 EV_y \tag{2.5b}$$

$$\text{Where } EV_x \equiv \frac{\partial}{\partial x}\left(E\left[u_c(H(Y,S))\right| y,s]\right),\ x \in \{y,s\}$$

$$EV_y = EV_s \tag{2.5c}$$

$$c_1 + c_2 = 2Hw - w(y+s) \tag{2.5d}$$

The set of Equations 2.5 characterizes parents' optimal choices. Equation (2.5a) shows that parents smooth their consumption over time.

Equation (2.5b) describes how parents substitute between consumption and investment in their child. Parents equate the marginal utility of consumption with the marginal impact on expected utility of investing in children. The marginal impact on expected utility of investment includes both the fact that investment raises the average level of children's human capital and that it increases the uncertainty that parents face.

The tradeoff between investment in early and late childhood is represented by Equation (2.5c). Because investing a unit in $y$ is more risky than investing a unit in $s$ (since $\sigma_y^2 > \sigma_s^2$), risk averse agents will in general choose an allocation of $(y, s)$ where the expected marginal productivity of $H$ is higher with respect to $y$ than with respect to $s$. As agents become more tolerant of risk, they will choose allocations of $(y, s)$ that are more productive, in the sense that they produce a given level of expected human capital with lower time investment. That is, they will choose to produce a given expected level of child outcomes $\bar{H}$ with more investment when young ($y$) and less when school-age ($s$).

The tradeoff between risk and return can be clarified by assuming that utility is quadratic.[12] While restrictive, this allows a clear focus on the tradeoff that parents face between increasing their children's human capital on average and the risk that they bear. Under this assumption, parents only care about the mean and variance of their children's outcomes and expected utility simplifies to the sum of the same quadratic utility function evaluated at the expected value of human capital and an additively separable term that is decreasing in the variance of the random variable. That is, expected utility can be written as in Equation 2.6:

$$\begin{aligned} E\left[u_c[H(Y,S)] \,|\, y,s\right] &= u_c(E[H(Y,S)]) - B\mathrm{var}(H(Y,S)) \qquad (2.6) \\ &\quad \text{Where } u_c(H) = A \cdot H - B \cdot H^2 \end{aligned}$$

Here, more risk tolerant families face a relatively lower utility cost of bearing risk, $B$, but have otherwise similar preferences for child services $A$.

Thus as parents become more risk tolerant, they will change their time investments along two dimensions. They will choose higher investment throughout childhood (more of both $y$ and $s$) through Equation 2.5b and they will substitute

[12]See e.g. Christiansen, Joensen, and Nielsen (2007) and Palacios-Huerta (2003), who use this assumption to justify considering the mean and variance of human capital portfolio allocations. In general, individuals' decision in a problem like this depends on the third derivative of the objective function (see Kimball (1990)).

investment when young ($y$) for investment when school-age ($s$) through Equation (2.5c).[13]

These two parental responses to differences in risk tolerance cannot be detected directly in the data. We observe only investment decisions $y$ and $s$ for parents with different levels of risk tolerance. But the two effects work in opposite directions in late childhood. As more risk tolerant parents shift investment from late childhood to early childhood, they decrease $s$, but as they increase investment throughout childhood, they increase $s$. Thus the net effect on investment in late childhood is ambiguous. The two effects that can be detected in the data are that more risk tolerant parents unambiguously increase investment in early childhood ($y$ is larger) and that they tilt their investment profiles towards early childhood ($y - s$ is larger). In the empirical section of this chapter, I will check for these two empirical effects on parental investment behavior.

## 2.3 Empirical Analysis of Uncertain Parental Investment

In this section, I take the predictions of Section 2.2 to the data using the Panel Study of Income Dynamics. The PSID has measures of parental investment over childhood and measures of parents' risk tolerance. I use these measures to test whether more risk tolerant parents reallocate time towards early childhood and whether more risk tolerant parents spend more time at each point in childhood.

[13] The comparative statics in the quadratic case are described in Appendix A.1.

### 2.3.1 Description of Measures of Risk Tolerance and Time Investment

#### 2.3.1.1 Measures of Time Investment from the PSID Child Development Supplement

I use the PSID Child Development Supplement (CDS) time diary data as a measure of parental investment. Time diaries give detailed information on individual's daily activities. The PSID CDS time diaries, in particular, focus on the time use of children, which allows observation of how much time children spend with their parents. These time diaries have been used elsewhere as direct measures of human capital investment (Villena-Rodán and Ríos-Aguilar (2012)). They have also been used in work which studies family processes and decisions (Folbre, Yoon, Finnoff, and Fuligni (2005); Hofferth and Sandberg (2004).

The PSID is a nationally representative panel survey. It began in 1968 with a sample of about 18,000 individuals in 5,000 families in 1968.[14] Members of these families and their descendants have been followed until the present day. Beginning in 1997, the PSID selected a sample of children ranging from birth to age 12 from existing PSID families to follow in the context of their development. This is known as the Child Development Supplement and consists of interviews in 1997, 2002, and 2007 as well as "Transition to Adulthood" modules in 2007 and 2009. All eligible children (those under age 12) were included in the sample, up to a maximum of 2 per family. In families with more than 2 children under 12, the two children were

[14] See http://psidonline.isr.umich.edu/Guide/Brochures/PSID.pdf

sampled randomly.

As part of the CDS, each family recorded two 24 hour time diaries for each child in the supplement. These time diaries included information on what activity each child was doing at every point during the day as well as both "with whom" the child did the activity and "who else was present" with the child at the time. Because one weekday and one weekend day are recorded for each child, I can aggregate data to a synthetic "week".

Because of their richness, there are several classification decisions that go into creating a measure of parental "investment". To the extent possible, I try to follow existing literature and show the results of different classification decisions. I aggregate the activities that children were doing "with" their parents to a simulated week. This is useful for the purposes of this chapter, since time when parents are actively engaged with their children is likely the most relevant for human capital investment. It is also close to what Guryan, Hurst, and Kearney (2008) do, in that they focus on time spent in "primary" activities, which is the parent's activity that requires the majority of their attention. Villena-Rodán and Ríos-Aguilar (2012), who use the same data, also focus on this measure of parental time investment. Since different types of time might be differentially related to children's human capital development, risk tolerance might be related to different types of time in different ways. I therefore also aggregate activities into categories as in Guryan, Hurst, and Kearney (2008): recreation, education, and basic care.[15] Recreation includes time spent watching

[15]I ignore Guryan, Hurst, and Kearney (2008)'s other major category, travel time, since it's relationship to children's human capital development is more ambiguous. Time spent in transit is unlikely to develop human capital directly. But especially in late childhood when children participate in activities outside the home, travel is likely complementary to many investments.

television, playing sports, attending meetings and events, socializing with friends, doing arts and crafts, or playing games, among many other activities. Education includes time spent together on homework, reading, or teaching children. Basic care includes time spent attending to basic physical needs of children, including bathing, feeding, and providing medical care. Of these types of time use, educational time is conceptually the most related to children's human capital. Recreational time is also related to human capital development, since especially young children often learn by playing games for example, rather than by reading or intensive study. I argue that time in basic care is less related to the development of children's future labor market skills.[16] Altogether, the time diaries provide a detailed look into the types of time that parents spend with their children and are hence useful for measuring investment.

Since I observe know whether one or both parents is participating "with" the child, I have to make decisions about what level of involvement from each parent constitutes investment. I follow Folbre, Yoon, Finnoff, and Fuligni (2005), who argue that time spent with both parents consists of higher quality, lower stress interactions than time with one parent and thus should be counted separately. Though my results are robust to either considering only maternal time or counting the total number of hours that *either* the mother or father was involved, for the majority of

The patterns for travel time are qualitatively similar to those for time in basic care, discussed below.

[16]Though neglecting basic care of children could increase risk of serious illness, or death, grooming and feeding children is less directly related to future labor market productivity than are the decisions about spending time with them in educational activities. Therefore, time that parents spend in basic care might also capture the propensity for children to be "at-risk" in the sense of a large probability of something very bad happening.

the work I use the definition of time investment as the sum of time that a child spends in activities with his or her mother plus the time in activities with his or her father.[17] Thus I count as investment time in activities reported as "with" the parent, since these represent more intensive, and likely beneficial, parental interactions. I construct the measure of total parental investment as the total time that the mother spent in primary activities with the child plus the total time that the father spent in primary activities with the child.

In order to provide a clean test of the predictions from Section 2.2, I need measures for individual children when they are "young" and when they are "school-age". I operationalize the definition of "young" as children who are under 5 years old in 1997. These same children, when they are surveyed in 2002, are between 5 and 10 years old, the period I will call "school-age". These time periods correspond especially closely to the concept of "early" childhood in the literature.[18] I restrict my estimation sample to families where the CDS child lives with both biological parents in both 1997 and 2002. I also restrict the sample to parents who were also together in 1996, so that the risk tolerance measure discussed in Section 2.3.1.2 applies to the same family. These restrictions excludes more than half of the sample. Thus my sample is of families where the CDS child lived with both parents at least

[17]Folbre, Yoon, Finnoff, and Fuligni (2005) also argue that a true measure of the parental cost of raising children involves the supervisory time when parents are present but not actively engaged with children. While this is no doubt true, I am interested primarily with parental investments that increase the future human capital and skills of children. Time spent directly with children, and particularly time spent in education and active recreational activities are the most likely to lead to these types of skills. I do briefly use *total time with parents present* as another falsification test, since having a parent present but not actively engaged likely represents less of an investment.

[18]Almond and Currie (2011) surveys *Human Capital Development before Age 5*, while Cunha, Heckman, and Schennach (2010) estimate separate production parameters for children aged 0 to 5-6 and for children aged 5-6 and up.

Table 2.1: Summary statistics for PSID CDS sample children

| | Mean | St. Dev. | S.E. | Min | Max |
|---|---|---|---|---|---|
| Both Parents, All Time in '97 | 50.7 | 22.0 | 1.27 | 4.683 | 149.3 |
| Both Parents, All Time in '02 | 35.9 | 17.7 | 1.02 | 0 | 98.75 |
| Mother, All Time in '97 | 31.8 | 15.5 | 0.90 | 2.550 | 93.02 |
| Mother, All Time in '02 | 21.3 | 10.6 | 0.61 | 0 | 65 |
| Both Parents, Recreational Time in '97 | 18.8 | 14.4 | 0.83 | 0 | 77.42 |
| Both Parents, Recreational Time in '02 | 15.0 | 12.1 | 0.70 | 0 | 59.83 |
| Both Parents, Basic Time in '97 | 15.5 | 7.99 | 0.46 | 0 | 47.75 |
| Both Parents, Basic Time in '02 | 8.04 | 5.31 | 0.31 | 0 | 26.25 |
| Both Parents, Educational Time in '97 | 1.97 | 2.62 | 0.15 | 0 | 15.17 |
| Both Parents, Educational Time in '02 | 1.78 | 3.06 | 0.18 | 0 | 20 |
| $\Delta$ Time, 1997 – 2002 | 14.8 | 23.2 | 1.34 | -77.67 | 106.2 |
| $\Delta$ Educational Time, 1997 – 2002 | 0.20 | 4.19 | 0.24 | -20 | 15.17 |
| $\Delta$ Recreational Time, 1997 – 2002 | 3.81 | 15.7 | 0.91 | -48.78 | 63.50 |
| Age of Child in '97 (Months) | 34.5 | 18.1 | 1.05 | 2 | 65 |
| Child is Non-White | 0.088 | 0.28 | 0.016 | 0 | 1 |
| Male | 0.52 | 0.50 | 0.029 | 0 | 1 |
| In School | 0.45 | 0.50 | 0.029 | 0 | 1 |
| Observations | 298 | | | | |

*Note:* This table presents weighted summary statistics for children in the PSID-CDS sample who were aged 5 or under as of the 1997 interview date. All time variables are presented in units of hours per week. The sample is limited to families where the same mother filled out time diaries in 1997 and 2002, the child lived with both biological parents in both 1997 and 1992, and parental education and fertility histories were available. Among these families, the sample is further restricted to those where the couple also lived together in 1996 and one of the members responded to the risk tolerance supplement that year.

until the second sample period in 2002, and the family was also together during the 1996 PSID survey. I balance the "panel" of time diary data, so that I only consider respondents in 1997 who also report time diaries in 2002. Though I don't restrict the sample to married parents, 100% of my sample, is in, fact married. My sample selections on family stability will limit inference to families who more closely resemble the prototypical "middle class" family.

Table 2.1 presents summary statistics from the child's perspective. Parents spent about 51 hours per week actively engaged with a child in 1997, and mothers accounted for about 32 of the hours. Compared to the rest of the CDS time diaries, this sample has relatively high paternal involvement due to the sample selection on biological parents and marital stability. Of the total time that parents spent "with" their children in the synthetic week in 1997, they spent on average about 20 hours in recreational activities, 16 hours basic care, and 2 in educational activities including homework help and parents reading to the child.[19] Notice that parents spent more of all types of time with their children in 1997, when they were an average of just under 3 years old than in 2002. This pattern is noted widely in the literature, including in Price (2008), Bryant and Zick (1996), and Guryan, Hurst, and Kearney (2008). The decline in parental time with children is mostly related to time spent in basic care. Time spent in other activities declines less rapidly.[20]

Table 2.2 shows summary statistics of families who have children in the PSID CDS. There are about 300 children in my sample who come from about 240 families. On average, the mothers are 32 and the fathers are 34. Because the families are selected for stability, they are richer, more educated, and older than the population as a whole.

[19]As a point of reference, if both parents spent every waking hour with a child, this would result in 224 hours of parental time per week.

[20]These times are generally larger than those reported in Table 1 of Guryan, Hurst, and Kearney (2008) and from estimates using the American Time Use Survey in general. This is mostly due to the restrictive definition of time with children in the ATUS, which requires the child to be essential for the activity. By comparison, in the PSID time is counted if the child was doing an activity with the parent. If a parent and child went to a movie together, this would count as parental time in the PSID but not in the ATUS. The parent could easily have gone to the movie without the child so it would not count as parental in the ATUS. But the parent was doing the activity with the child, so this would count as parental time in the PSID.

Table 2.2: Summary statistics for PSID CDS sample families

| | Mean | St. Dev. | Min | Max |
|---|---|---|---|---|
| Age of Youngest Child ('97) | 2.026 | 1.187 | 1 | 5 |
| Biological Children in HH ('97) | 1.968 | 1.157 | 1 | 11 |
| Mother in LF ('97) | 0.741 | 0.439 | 0 | 1 |
| Total income / $100k ('97) | 0.672 | 0.402 | 0.0431 | 2.605 |
| Mom Col. Grad | 0.396 | 0.490 | 0 | 1 |
| Mom HS Grad | 0.951 | 0.216 | 0 | 1 |
| Non-White Mother | 0.0924 | 0.290 | 0 | 1 |
| Age of Mother ('97) | 32.16 | 5.411 | 19 | 46 |
| Age of Father ('97) | 34.09 | 5.888 | 20 | 49 |
| Risk Tolerance (KSS, Standardized) | 0.0284 | 1.013 | -1.075 | 2.925 |
| Risk Tolerance (KSS) | 0.532 | 0.240 | 0.270 | 1.220 |
| Observations | 244 | | | |

*Note:* This table presents weighted summary statistics for PSID-CDS sample families that are used throughout this chapter. The sample is restricted to "stable" families where at least one child in the CDS aged 5 or under was living with both biological parents in 1997 and 2002. For more detail on the sample see the notes to Table 2.1.

#### 2.3.1.2 Measure of Risk Tolerance in the PSID

In 1996, the PSID asked a series of questions to estimate the risk preferences of the survey respondent in households with employed heads.[21] Participants were asked questions of the following form:

> Suppose you had a job that guaranteed you income for life equal to your current, total income. And that job was (your/your family's) only source of income. Then you are given the opportunity to take a new, and equally good, job with a 50-50 chance that it will double your income and spending power. But there is a 50-50 chance that it will cut your income and spending power by a third. Would you take the new job?

[21] The survey respondent is whoever answered the survey in a particular year. This might or might not be the "head of household" who is almost always male partner in a couple. In my sample, the respondent was the male partner in 1996 in about 65% of the households.

In addition to asking about risking a third of income, the same question was asked about fractions of 3/4, 1/2, 1/5, and 1/10, and each respondent was classified as having an indifference point within a range given by their responses. Because this question asks about gambles relative to an individual's permanent household income, the elicited measure is one of *relative* risk tolerance.

Knowing the range of gambles that an individual would accept and reject, and making assumptions about the functional form of people's utility functions and the population distribution of the utility function parameters, it is possible to estimate expected values of risk aversion and risk tolerance for each individual, conditional on their responses. Values of personal "risk tolerance" were computed initially by assuming a log-normal population distribution of the parameter $\theta$ in a CRRA utility function over income $x$ of the form in Equation 2.7 (Luoh and Stafford 1997).

$$u(x) = \frac{x^{1-\frac{1}{\theta}}}{1-\frac{1}{\theta}} \tag{2.7}$$

A very similar question had previously been fielded in the Health and Retirement Study (HRS) in 1992, and refielded to a subset of earlier respondents. This feature of the HRS allows insight into the degree of measurement error and persistence in the answers to this question. Barsky, Juster, Kimball, and Shapiro (1997) and Kimball, Sahm, and Shapiro (2008) analyze the measurement error and find it to be quite substantial. Sahm (2007) analyzes the persistence and determinants of the risk tolerance measure in depth. She finds that the measurement error component of risk tolerance is roughly twice as large as the persistent component. She also finds that the portion of the persistent error that is explainable by unobserved individual characteristics is quite large. Unobserved differences that are consistent for individuals over time explain about twice the variation as persistent differences that are related to observables. Thus the measurement error component of risk tolerance is roughly four times as large as the component that is related to observable characteristics. While Kimball, Sahm, and Shapiro (2008) show that even this quite noisy measure of risk tolerance is related to investment behavior, by correcting for measurement error they demonstrate a relationship that is substantially stronger.

Kimball, Sahm, and Shapiro (2009a) use the measured variance of measurement error from the Health and Retirement Study to generate a measurement error corrected measure in the PSID. I use this measure in the bulk of the empirical work in Section . The tabulations of the distribution of the measurement error corrected relative risk tolerance measure $\theta$ from Equation (2.7) in my sample and the Health and Retirement Study (HRS) that Barsky, Juster, Kimball, and Shapiro studied are presented in Table 2.3. The rows of Table 2.3 present information for groups of respondents who answered the risk tolerance questions in the same way. The first two columns represent the smallest share of income at risk that would be rejected and the largest share of income accepted. That is, respondents counted in the third row would reject a gamble risking half of their income or more, but accept losing a third of their income or less. The third column presents the value of risk tolerance ($\theta$) assigned to respondents in this group.[22] The next three columns present

[22]To calculate the expected cardinal value of risk tolerance given responses, I follow Kimball, Sahm, and Shapiro (2009b) in order to correct for measurement error and status quo bias.

the fraction of the Health and Retirement Study, the PSID, and my sub-sample of the PSID who answered the risk tolerance question in each way. Compared to the HRS (column 4), the PSID sample is significantly more risk tolerant. Nearly twice as many respondents would accept any gamble, and one third fewer reject all gambles. This likely reflects the fact that the PSID sample of working-aged people is significantly younger than the HRS sample of those over 50, combined with the fact that risk tolerance decreases with age.[23] Column 6 of Table 2.3 presents the distribution of risk tolerance in my sample. Compared to the sample of all PSID Risk Tolerance Supplement respondents in Kimball, Sahm, and Shapiro (2009a), my sample of young, married parents has fairly similar risk tolerance. The mean and standard deviation in my sample are 0.53 and 0.24, respectively.

My empirical strategy rests on the assumption that family's risk preferences are not meaningfully related to other unobservable family characteristics. I examine this issue in my own sample in Section 2.3.2.1, but findings of other authors are important as well. Two of the most important demographic differences in risk tolerance are that the old are relatively more risk averse than the young and that women are relatively more risk averse than men (see Sahm (2007) for a discussion). Neither of these relationships is important in my context. In my illustrative modelling framework in Equation 2.1, discussed in Section 2.2, there is only one value of risk tolerance for the household. Whether the survey respondent who answered the question was male or female, I use the response to proxy for the household's overall level of risk

[23]See the discussion in Sahm (2007), Kimball, Sahm, and Shapiro (2009b), or Barsky, Juster, Kimball, and Shapiro (1997).

Table 2.3: Measured risk tolerance in the PSID CDS and the HRS.

| (Fraction of Current Family Income At Risk) Safest Rejection | Riskiest Assent | $E[\theta]$ | HRS Fraction | PSID Fraction (Whole PSID) | CDS Fraction (My Sample) |
|---|---|---|---|---|---|
| None | 3/4 | 1.22 | 0.038 | 0.066 | 0.060 |
| 3/4 | 1/2 | 0.79 | 0.071 | 0.137 | 0.154 |
| 1/2 | 1/3 | 0.60 | 0.142 | 0.150 | 0.170 |
| 1/3 | 1/5 | 0.49 | 0.129 | 0.156 | 0.162 |
| 1/5 | 1/10 | 0.40 | 0.174 | 0.182 | 0.240 |
| 1/10 | None | 0.27 | 0.445 | 0.309 | 0.203 |

*Note:* This table presents a tabulation of the risk tolerance parameter $\theta$ from Equation (2.7) in the Health and Retirement Study (from Barsky, Juster, Kimball, and Shapiro (1997)) and in this chapter's sample of PSID families that responded to the risk tolerance supplement, had children 5 or under, and remained together and filled out time diaries for the children in 1997 and 2002. For more information on the sample, see the notes to Table 2.1.

*Note:* The $E[\theta]$ column represents the expected value of relative risk tolerance given a particular response. The expectation is calculated for the PSID, adjusting for measurement error and status quo bias, by Kimball, Sahm, and Shapiro (2009b). The $E[\theta]$ column does not apply to the HRS sample, since the underlying distribution of $\theta$ is different.

tolerance. If the distribution of men's and women's risk tolerance differ only in means, I can control for this by controlling for the respondent's gender.

In order to use the risk tolerance measure of one spouse to proxy for the entire household, the spouse's risk tolerance should be positively correlated with the household's overall effective level of risk tolerance. This will be the case as long as assortative mating on risk tolerance is positive, so that more risk tolerant individuals are in more risk tolerant households. There is empirical evidence for positive assortative mating on risk tolerance. Kimball, Sahm, and Shapiro (2009b) note that the correlation between husbands and wives in the HRS is 0.41.[24] Dohmen, Falk, Huffman, and Sunde (2008) look at a different survey based measure of risk tolerance in the German Socioeconomic Panel, where individuals rate themselves on their willingness to take risk on an 11 point scale. Using this measure, controlling for demographics, cohabiting and married couples have a correlation of 0.27.[25] Risk tolerance thus appears to be positively correlated within the household. To the extent that a single spouse's risk tolerance is an imperfect proxy for the household's, any relationship between risk tolerance and investment will be biased towards zero.

There are also systematic differences in risk tolerance over the life cycle. These differences are relatively less important for my context. My sample contains only children from birth through age 5 in 1997, so the parents in my sample have a very compressed age distribution relative to the population as a whole. Therefore, differ-

[24]This correlation excludes husbands and wives who answered the question in the same sitting, and who hence might be more likely to respond in a similar way.

[25]Dohmen, Falk, Huffman, Sunde, Schupp, and Wagner (2005) validate this survey instrument by asking it to a representative sample of 450 Germans and measuring risk taking behavior through lottery choices. They find that the two measures are strongly related.

ences between families in risk tolerance reflect differences that are not attributable to their location in the life course.

One important caveat in the argument that risk tolerance is unrelated to other individual characteristics is that the measure of risk tolerance is a relative one. Recall that the survey instrument does not ask about a gamble over absolute amounts of money, but rather talks about a job that would affect household permanent income proportionally. Thus the gamble is more risky in an absolute sense for households with larger current income. Since $\theta$ in Equation (2.7) is fit to a CRRA utility function, $\theta$ is implicitly related to observables to the extent that these observables correlate with current income. Households with higher permanent incomes are asked about proportionally larger income risks. That risk tolerance is uncorrelated with income suggests that higher income households are more risk tolerant (in an absolute sense) in proportion to their income. Other work has found similar relationships. Sahm (2007) finds that income and wealth are unrelated to measured risk tolerance in the HRS. Guiso and Paiella (2008) use a slightly different measure of risk tolerance in Italy and find that even though relative risk aversion does not explain household behavior, absolute risk aversion is decreasing in wealth.

### 2.3.2 Empirical Relationships

#### 2.3.2.1 Risk Tolerance and Observable Characteristics

In Section 2.3.2.2, I show that risk tolerance is related to time use in my sample of intact families with children under 5 in 1997, conditional on a set of family level control variables. In order to interpret the coefficient on risk tolerance as related to parents' underlying preferences and not other family level unobservables, I need to assume that risk preferences are conditionally uncorrelated with any other unobserved determinants of time use. This is an inherently untestable assumption. However, it is possible to test the extent to which risk preferences are correlated with *observed* family characteristics. If risk preferences are not correlated with observables, they can only be correlated with unobservables which are themselves uncorrelated with observable characteristics. Since I observe a wide range of socioeconomic indicators, I can show that risk preferences cannot be correlated with any unobservables which are correlated with socioeconomic characteristics. Towards this end, Table 2.4 presents the raw correlations between the normalized risk tolerance measure on each control variable. Aside from the time use variables in the first few rows, notice that most variables have relatively low correlations with risk tolerance (generally below 0.1 in most cases).

Next, I show that the conditional correlations of $\theta$ with sets of covariates are also small. Table 2.5 shows a series of regressions of the risk tolerance parameter $\theta$ on various sets of controls. The row indicated by $F$ and $p$ indicates the $F$-statistics and the $p$-values for tests of joint significance of the coefficients. The control variables are a set of dummy variables for the number of children up to 4, a set of dummy variables for the age of the youngest child, and demographic variables including the mother's race, the children's sex, and the mother's age and household income. I use family size and the age of the youngest child because these are important determinants of parental time with children (Price (2008)) and I also use them

Table 2.4: Raw correlation coefficients between risk tolerance and family observable characteristics

| | Corr. Coeff. | $p$ |
|---|---|---|
| Male 1996 Respondent | 0.105 | 0.103 |
| All Time in '97 | 0.0769 | 0.232 |
| Recreational Time in '97 | 0.0555 | 0.388 |
| Basic Time in '97 | -0.0428 | 0.506 |
| Educational Time in '97 | 0.169 | 0.00818 |
| Biological Children in HH ('97) | 0.0199 | 0.757 |
| Age of Youngest Child ('97) | 0.0726 | 0.259 |
| Non-White Mother | -0.106 | 0.0987 |
| Age of Mother ('97) | 0.0340 | 0.597 |
| Age of Father ('97) | 0.0363 | 0.573 |
| Mom HS Grad | 0.0446 | 0.488 |
| Mom Col. Grad | -0.0366 | 0.569 |
| Dad HS Grad | 0.0595 | 0.354 |
| Dad Col. Grad | -0.0275 | 0.669 |
| Mom Years of Schooling | 0.0270 | 0.674 |
| Dad Years of Schooling | 0.0285 | 0.658 |
| Total income / $100k ('97) | 0.0377 | 0.558 |
| Observations | 244 | |

*Note:* This table presents the raw correlation using household level weights between the 1996 PSID risk tolerance supplement measure of risk tolerance (corrected for status quo bias and measurement error as in Kimball, Sahm, and Shapiro (2009a)) and other PSID household variables. The reported $p$ values indicate the statistical significance of these correlations. These are the same $p$ values that would be calculated from running a regression of the risk tolerance on each variable and a constant. For a description of the sample selection, see the notes to Table 2.1.

Table 2.5: The explanatory power of demographics and family structure for household risk tolerance in the PSID

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Age of Youngest | | | X | | X | X | X |
| Family Size | | X | | X | | X | X |
| Demographics | X | | | X | X | | X |
| Father is Resp. | X | X | X | X | X | X | X |
| $R^2$ | 0.0270 | 0.0272 | 0.0260 | 0.0413 | 0.0426 | 0.0396 | 0.0550 |
| Adj. $R^2$ | -0.00174 | 0.0109 | 0.00561 | 0.000328 | -0.00259 | 0.00709 | -0.00248 |
| $N$ | 245 | 245 | 245 | 245 | 245 | 245 | 245 |
| $F$ | 1.365 | 2.084 | 1.880 | 1.400 | 1.717 | 1.903 | 1.591 |
| $p$ | 0.221 | 0.0835 | 0.0985 | 0.181 | 0.0705 | 0.0604 | 0.0826 |
| $k$ | 7 | 4 | 5 | 10 | 11 | 8 | 14 |

*Note:* This table presents a series of regressions of the PSID risk tolerance measure on the sets of controls indicated. All regressions contain a control for whether the husband or wife was the respondent that answered the PSID risk tolerance question in 1996. Age of Youngest is a set of dummy variables for the age, in years, of the youngest child in the household. Family Size is a set of dummies indicating whether there are one, two, three, or four or more children in the family. Demographics include controls for parent's age, race, family income, and mother's schooling. Below the table, the $F$ row presents the $F$ statistic of the joint test of significance of the set of variables contained in that regression. The $p$ value corresponds to the test of the joint equality of zero of all coefficients except the constant. For more information on the sample selection, see the notes to Table 2.1.

later in Section 2.3.2.2. The coefficient on non-white families in Table 2.5 is the closest to individual statistical significance, consistent with the results in Table 2.4. Though models with the number of children in the household and the age of the youngest child are jointly statistically significant at the 10% level, none of the sets of coefficients in any combination are statistically significantly related to $\theta$ at the 5% level. Given that each of the variables is individually uncorrelated with risk tolerance as seen in Table 2.4, this is not entirely surprising.

Finally, the illustrative model in Section 2.2 assumes that parents' decision to have children is exogenous and not a function of risk tolerance. The fertility decision certainly affects the risks that parents bear, but the direction of this effect ambiguous. Having additional children requires parents to put more resources into their children, which would tend to increase the risk that parents face. However, distributing a fixed amount of resources among more children would tend to *decrease* the risk that parents face to the extent that children's outcomes do not perfectly covary. The net implication of risk preference for parental fertility is an empirical question. If parental risk preferences were a strong predictor of fertility decisions, it would suggest the fertility decision should be in the model as well. I address this point by using a sample of older PSID risk tolerance respondents who have completed their fertility to examine the relationship between risk tolerance and fertility. I consider the current fertility histories (as of 2009) for individuals who responded to the risk tolerance question in 1996. The age of respondents is restricted to women over 45 and men over 50, who can be reasonably expected to have completed their fertility. Table 2.6 provides statistical evidence on this point. Since this sample did not have to also have CDS children and complete multiple time diaries, the sample size is much larger (nearly 1600 individuals). The first column presents the estimated coefficients for a probit of a dummy variable for whether the individual had any children on the risk tolerance measure. The marginal effect of this estimate is about a 1.1 percentage point decrease in the probability of having any children in response to a 1 standard deviation increase in risk tolerance, but the coefficient is not statistically significant. The second column presents a linear probability model on the same set of controls, which has a similar point estimate that is again statistically insignificant.

The second set of columns in Table 2.6 present OLS regressions of the level of completed fertility on risk tolerance. Column 3 suggests that risk tolerance does not matter for completed fertility. Column 4 estimates the same model as column 3, but the sample is now conditioned on parents having at least one child, in order to see whether risk tolerance is related to having additional children. This is the most relevant issue for my work on investments over childhood, since family structure is related to time investments, changes in family structure will be related to differences in time investments over time. The coefficient on risk tolerance in column 4 suggests that this is not an issue. Note that the standard error is about 0.03, which suggests that I can rule out effects above 0.07. By way of comparison, the propensity to have an extra child after having two children of the same sex, as estimated from US Census data by Angrist and Evans (1998), is also about 0.07. Thus it appears that completed fertility is not related to individual risk tolerance.

Schmidt (2008) finds that while risk tolerance is unrelated to women's completed fertility, it is related to the timing of births. She models the hazard of women's first birth as depending on risk preferences. Specifically, she finds that more risk tolerant women have higher fertility hazards in their teenage years. Among unmarried, college educated women, more risk tolerant women delay childbearing as they reach the end of their fertile periods. Despite the effects of risk tolerance on the timing of births, there is no net effect on completed fertility. Schmidt shows (in her Table 9, where she estimates a model very much like column 2 of my Table 2.6) that risk tolerance is unrelated to whether mothers *eventually* have a child.

Taken as a whole, the results in Table 2.4 to 2.6 present a potentially unexpected result. Family risk tolerance does not seem to be related to fertility or other

Table 2.6: Regressions on the relationship between risk tolerance and fertility in the PSID

| | Any Children | | Number of Children, OLS | |
|---|---|---|---|---|
| | Probit | OLS | Whole Sample | Parents |
| Risk Tolerance (KSS) | -0.0538 | -0.0113 | -0.0282 | -0.00195 |
| | (0.0409) | (0.00804) | (0.0379) | (0.0368) |
| Married | 0.797*** | 0.178*** | 0.368** | -0.0995 |
| | (0.123) | (0.0260) | (0.122) | (0.126) |
| College Deg. | -0.333*** | -0.0724*** | -0.264** | -0.0996 |
| | (0.0909) | (0.0180) | (0.0851) | (0.0837) |
| HS Deg. | -0.522** | -0.0654* | -0.749*** | -0.610*** |
| | (0.179) | (0.0259) | (0.122) | (0.113) |
| Male | -0.282* | -0.0741** | -0.106 | 0.113 |
| | (0.131) | (0.0280) | (0.132) | (0.135) |
| Non-White | 0.156 | 0.0337+ | 0.325*** | 0.276** |
| | (0.103) | (0.0190) | (0.0894) | (0.0859) |
| Constant | 1.475*** | 0.891*** | 2.754*** | 3.033*** |
| | (0.187) | (0.0284) | (0.134) | (0.126) |
| Observations | 1588 | 1588 | 1588 | 1396 |

*Standard errors in parentheses:* + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
*Note:* These models present regressions of completed fertility from the PSID Childbirth and Adoption History File on individual level controls. The first column presents a probit on a binary indicator of the individual having any live births. The reported coefficients are probit coefficients, and the marginal effect of risk tolerance is $-0.011$. The second column estimates the same model using a linear probability framework. Columns 3 and 4 present regressions on the total number of live births to an individual over their lifetime on the same set of controls. Column 3 includes individuals with no children, while column 4 restricts the sample to families that have had at least one child.
*Sample Selection:* The sample for these regressions is women over 45 or men over 50 as of the interview date in 2009, who answered the risk tolerance supplement in the 1996 PSID and are likely to have completed their fertility.

family level observables. At the same time, Table 2.4 shows that risk tolerance is *not* uncorrelated with parental time use. It is worth noting that some other authors have found measured relative risk tolerance to be related to some observable characteristics, though the evidence is far from conclusive (for a survey, see Borghans, Duckworth, Heckman, and ter Weel (2008)). Many authors find that risk tolerance is positively related to education (Sahm (2007)), but Barsky, Juster, Kimball, and Shapiro (1997) find education to be uncorrelated with risk tolerance, conditional on race, religion, and gender and Dave and Saffer (2008) find risk tolerance to be conditionally uncorrelated with education among those aged 21-54 in the PSID. Sahm (2007), Dave and Saffer (2008), and Barsky, Juster, Kimball, and Shapiro (1997) shows that risk tolerance is related to gender, but Andersen, Harrison, Lau, and Rutström (2008) Holt and Laury (2002) find that risk tolerance is not correlated with gender.

Many of the individual characteristics which the literature finds to be correlated with risk preferences are not applicable to the current situation. I assume a unitary household and use the respondent's risk tolerance to proxy for that of the household as a whole. Every household in the sample contains one male and one female, and I assume that after controlling for level differences for the gender of the respondent, The single household measure, combined with a control for the gender of the respondent, accounts for differences in average risk tolerance.[26] Risk tolerance depends on the respondent's age, but as shown in Table 2.2, no parents of young

[26]Schmidt (2008) shows that whether the husband or wife is the respondent is correlated with some household level observables, and I do control for the gender of the respondent in the regressions in Section 2.3.2.2.

children in my sample were over age 50 in 1997 (and hence in 1996). Thus while risk tolerance increases with age, parents in my sample are roughly similar in age and age differences between them likely pick up other characteristics. My sample is also entirely married, so to the extent risk tolerance is also correlated with marriage, my results should be interpreted as the estimate for married families.

It is a relatively common finding in the literature that relative risk tolerance is only weakly (if at all) correlated with income or wealth. The income relationship that Dave and Saffer (2008) find is partly related to state effects, and might be dependent on their context using the range of PSID respondents. Indeed, Guiso and Paiella (2008) find that absolute risk aversion depends roughly proportionally on income, suggesting that constant relative risk aversion preferences are uncorrelated with income. Sahm (2007) also finds relative risk tolerance to be only very weakly related to income and wealth. It is also important to note that the measure of risk tolerance is in part reported net of socioeconomic status by its relative nature. Higher socio-economic status families are likely to have higher family incomes, and are thus asked about financial risks that are greater in magnitude.

#### 2.3.2.2 Risk Tolerance and Parental Investment

In this section I show that parental investment behavior is related to risk tolerance in ways that are consistent with the illustrative model of Section 2.2. The illustrative model predicts that risk tolerant parents should spend more time with their children in early childhood. This implication makes sense to test on parental time use data because in early childhood a large fraction of investments in children come from parental time. The illustrative model of Section 2.2 also implies that more risk tolerant parents should tilt their time investments towards early childhood and away from late childhood.

In order to estimate how risk tolerance affects parental investments, I estimate equations of the form in Equation 2.8.

$$I_i = \beta\theta_i + X_i\gamma + \varepsilon_i \tag{2.8}$$

In the equation, $I_i$ represents one of 4 measurements of time investment by household $i$, $\theta_i$ represents household $i$'s risk tolerance, $X_i$ represents a set of family level controls, and $\varepsilon_i$ is the error term.

I begin by estimating an equation like 2.8 using the time measure for 1997, when all the children are between the ages of 0 and 5, and are therefore in what I call "early childhood." I expect that the estimated coefficient $\beta$ will be positive, as more risk tolerant parents invest more in early childhood. Table 2.7 presents the result of estimating Equation (2.8) using several different parental time measures from early childhood and several different sets of controls. The first set of controls contains a set of clearly predetermined family and child demographic controls, including the child's age, sex, and race, as well as the age of each parent and the mother's education.[27] In order to give a sense of the conditional correlations between the

[27] In tables that are not presented here, I also used the father's education as a control. Due to assortative mating, father's and mother's education are highly correlated, so I only present results with mother's education here.

controls in column one and a set controls that are important for parental time use, but may well be endogenous, I add additional controls in column two. This set of controls also includes family income, whether the child is enrolled in any school, and sets of dummies for the number of siblings (up to 3), and the age of the youngest child in the family. Finally, the third column adds risk tolerance, my main variable of interest, and an indicator for whether the father was the PSID respondent. In addition to adding the coefficient of interest, comparing the second and third column within each group allows the reader to see that including risk tolerance changes the estimated coefficients on other control variables only slightly. To the extent that including risk tolerance does not affect the coefficients on other determinants of time use, the interpretation of the estimated risk tolerance coefficient as causal is strengthened.

There are four groups of regressions in Table 2.7, one on each of total parental time, educational time, recreational time, and basic care time. Overall time spent "with" a child clearly contains some components which are investments and others which are not. Time spent in educational activities most clearly represents parental investments in their children's future outcomes. Recreational time, especially to the extent that activities like arts and crafts or sports help to build skills, also likely represents parental investment to some extent. Time spent in basic care is the least strongly related to human capital, so the relationship between risk tolerance and time use should be least strong for this category. Each group of columns for a particular type of time use contains the same sets of controls, in the same order. Throughout the table, standard errors are reported in parentheses below the estimates.

I first investigate how the total parental time "with" children is related to risk tolerance in the first three columns of Table 2.7. The first column presents a regression of total parental time spent with children on the predetermined controls. None of coefficients on these controls are statistically significant at conventional levels. Non-white children receive about 7.5 fewer hours per week of overall parental time, which though a substantial point estimate is very noisy. Though it is estimated imprecisely, there is a large education gradient: children of mothers with a college degree receive about 3.2 additional hours per week than children of mothers with a high school diploma, and children in families with high school educated mothers receive 3.5 additional hours per week compared to mothers with no high school degree. This is consistent with the findings of Guryan, Hurst, and Kearney (2008) that more educated parents spend more time with their children. The second column adds family income, a control for whether the child is enrolled in any school (or pre-school), and sets of dummies for the number of siblings (up to 3), and the age of the youngest child in the family. These variables do have significant explanatory power, which can be seen as $R^2$ increases from 0.05 to 0.12.[28] This is not surprising, since the age of the youngest child and the total number of siblings in the household are large drivers of parental time use, as in Price (2008). Furthermore, children who attend pre-school receive about 12 fewer hours per week of parental time. This is a large and mechanical effect: if children are in pre-school, they are away from their parents.

[28]While the increase in $R^2$ only shows that the variables have explanatory power jointly, in fact the dummies for family size and the age of the youngest sibling have explanatory power individually as well.

Though imprecisely estimated, the point estimate on family income suggests that parents with an additional $100,000 in income spend about 40 additional minutes per week with their children.

The third column adds the standardized risk tolerance measure and the dummy variable for whether the father was the respondent to the risk tolerance question.[29] The coefficient on risk tolerance in column 3 is 1.86, indicating that a standard deviation increase in family risk tolerance is associated with an extra 1.75 hours per week of total parental time with children, but this coefficient is not statistically significant. Importantly, adding risk tolerance in column 3 does not change the estimated coefficients on other variables compared to column 2. A Hausman test of the relevance of the risk tolerance measure for the estimation of the coefficients in column 2 is unable to reject the hypothesis that the coeficients are unchanged from column 2 to column 3. That the inclusion of risk tolerance does not affect the other coefficients is consistent with the measure of risk tolerance picking up true risk tolerance and not unobserved factors that are correlated with both risk tolerance and time use. Given that risk tolerance is essentially uncorrelated with the observables, as shown in Tables 2.4 and 2.5, this is not surprising. An alternative to column 3, presented in Appendix Table B1, contains only the predetermined controls from column 1; the results are very similar.

Column 4 through column 6 repeat the analysis in columns 1 through 3 using educational time, which is a clear representation of parental time investments in

[29] Risk tolerance is slightly correlated with the gender of the respondent (since men are more risk tolerant than women) and the gender of the respondent is correlated with family socioeconomic status. See the discussion in Section 2.3.1.2 and in Schmidt (2008).

Table 2.7: 1997 parental time with children in the PSID CDS and risk tolerance

| | All | | | Education | | | Recreation | | | Basic Care | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Risk Tolerance (KSS) | | | 1.861 | | | 0.558** | | | 0.715 | | | -0.262 |
| | | | (1.561) | | | (0.208) | | | (1.009) | | | (0.572) |
| Child Age (Months) | -0.0396 | 0.0413 | 0.119 | 0.129** | 0.168** | 0.191*** | -0.0492 | 0.0459 | 0.0808 | -0.387** | -0.409* | -0.423* |
| | (0.363) | (0.398) | (0.412) | (0.0437) | (0.0554) | (0.0573) | (0.243) | (0.277) | (0.285) | (0.146) | (0.175) | (0.178) |
| Child Age$^2$/1000 | -1.604 | -1.211 | -2.154 | -1.648* | -1.963* | -2.244** | 0.0273 | -0.541 | -0.993 | 3.550+ | 4.011 | 4.204 |
| | (5.111) | (5.724) | (5.903) | (0.645) | (0.816) | (0.836) | (3.380) | (3.763) | (3.868) | (2.121) | (2.527) | (2.563) |
| Non-White | -7.455+ | -3.538 | -3.033 | 0.845 | 0.695 | 0.852 | -1.614 | 0.349 | 0.317 | -3.979*** | -3.503** | -3.422** |
| | (4.252) | (4.393) | (4.416) | (1.019) | (0.750) | (0.703) | (2.713) | (2.678) | (2.804) | (1.015) | (1.152) | (1.165) |
| Male | 0.792 | 0.750 | 0.906 | -0.358 | -0.248 | -0.203 | 0.769 | 0.853 | 1.004 | 0.0136 | -0.0826 | -0.165 |
| | (2.923) | (3.001) | (3.087) | (0.328) | (0.325) | (0.316) | (1.877) | (1.871) | (1.917) | (0.963) | (0.993) | (1.000) |
| Age of Mother | -0.375 | -0.561 | -0.535 | 0.0351 | -0.00493 | 0.00209 | -0.640 | -0.703* | -0.665+ | 0.177 | 0.194 | 0.171 |
| | (0.627) | (0.582) | (0.625) | (0.0612) | (0.0664) | (0.0643) | (0.392) | (0.347) | (0.380) | (0.166) | (0.171) | (0.167) |
| Age of Father | -0.200 | 0.0395 | 0.0329 | -0.0402 | -0.0232 | -0.0248 | 0.0726 | 0.177 | 0.158 | -0.198 | -0.167 | -0.155 |
| | (0.648) | (0.665) | (0.698) | (0.0547) | (0.0520) | (0.0522) | (0.376) | (0.378) | (0.401) | (0.148) | (0.156) | (0.152) |
| Mom Col. Grad | 3.204 | 3.434 | 3.655 | 0.817* | 0.527 | 0.594+ | 2.126 | 3.234 | 3.280 | -0.579 | -0.547 | -0.552 |
| | (3.048) | (3.315) | (3.415) | (0.404) | (0.365) | (0.337) | (2.191) | (2.411) | (2.486) | (1.051) | (1.174) | (1.170) |
| Mom HS Grad | 3.573 | 4.023 | 3.615 | 0.862 | 0.339 | 0.219 | 3.631 | 3.362 | 3.116 | -2.460 | -2.248 | -2.131 |
| | (5.863) | (6.283) | (6.262) | (0.569) | (0.521) | (0.474) | (3.504) | (3.602) | (3.655) | (1.701) | (1.818) | (1.796) |
| Total Income / $100k | | 0.626 | 0.304 | | 1.415+ | 1.321+ | | -0.816 | -1.046 | | -1.169 | -1.052 |
| | | (3.481) | (3.410) | | (0.779) | (0.745) | | (2.377) | (2.368) | | (1.093) | (1.106) |
| In School | | -12.39*** | -12.46*** | | -0.200 | -0.224 | | -6.811** | -6.771** | | -0.339 | -0.374 |
| | | (3.293) | (3.283) | | (0.393) | (0.378) | | (2.169) | (2.192) | | (1.063) | (1.071) |
| Male 1996 Respondent | | | -0.679 | | | -0.185 | | | -1.108 | | | 0.663 |
| | | | (3.080) | | | (0.302) | | | (2.118) | | | (0.969) |
| Number of Children | | X | X | | X | X | | X | X | | X | X |
| Age of Youngest | | X | X | | X | X | | X | X | | X | X |
| $R^2$ | 0.0473 | 0.124 | 0.130 | 0.0785 | 0.172 | 0.214 | 0.0484 | 0.115 | 0.118 | 0.146 | 0.164 | 0.166 |
| N | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 |
| Mean of Dep. Var. | 50.67 | 50.67 | 50.67 | 1.973 | 1.973 | 1.973 | 18.78 | 18.78 | 18.78 | 15.50 | 15.50 | 15.50 |

*Standard errors in parentheses:* + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

*Note:* These tables present regressions of the time that PSID CDS children spend in activities where either the mother or father are listed as individuals "with whom" the child does the activity. The time that children spend in activities of this sort with their mothers is added to the time they spend in activities with their fathers, as in Folbre, Yoon, Finnoff, and Fuligni (2005). For a discussion of the sample selection, see the footnote to Table 2.1.

*Variables Definitions:* Risk tolerance comes from the 1996 PSID risk tolerance supplement and is adjusted based on the response bin to the levels suggested by Kimball, Sahm, and Shapiro (2009a). It is then normalized to have mean 0 and standard deviation 1. Child age represents the child's age as of the 1997 interview. Non-white is a dummy variable for the child's race or Hispanic ethnicity. The mother's education dummy variables are not mutually exclusive but take the value of 1 if the mother has that degree of education. That is, a family with a college graduate mother will have both Mom Col. Grad and Mom HS Grad equal to 1. Total income was total family income reported in the 1997 survey about tax year 1996. The *In School* variable captures whether a child was "in a child care center, nursery school, preschool, pre-kindergarten, Head Start Program, or in Kindergarten" or a higher grade of school. *Male 1996 Respondent* indicates whether the husband was the respondent to whom the risk tolerance supplement was asked in 1996. *Number of Children* indicates a set of dummy variables for whether the family had one, two, three, or four or more children in 1997. *Age of Youngest* is a set of dummy variables for the age, in years, of the youngest child in the household in 1997.

children's skills. If risk tolerant parents are in fact investing more in their children in early childhood, more educational time is very likely a part of this. Compared to the first three columns, there is now enough statistical power to begin to see the relationship between parental educational time and the covariates. The relationship between educational time and the age of the child is increasing and concave, which largely reflects the fact that little time is spent in educational activities with children under one to two years of age. In column 4, the coefficient on whether the mother has at least a high school education is 0.8, which is large relative to the mean of 2 hours per week.

Column 6 of Table 2.7 adds family risk tolerance. The estimated risk tolerance coefficient on educational time is 0.56, and statistically significant at the 1% level. This coefficient indicates that a standard deviation increase in family risk tolerance increases parents' educational time with their children by 0.56 hours per week during early childhood. This is a large effect: the 95% confidence interval is between 7 and 50% of the sample average of 2 hours per week in educational activities. The illustrative model from Section 2.2 implies that more risk tolerant families should increase investment in early childhood, and in the case of educational time, the clearest measure of time investment in the data, this does appear to be happening. Once again, adding family risk tolerance again does not change the coefficients on demographics.

In addition to educational time, recreational time that parents spend with their children is likely to be effective in developing children's skills. It is likely that even if these activities are less formally related to skill development, young children still gain at least some important skills from them. Columns 7 through 9 examine the determinants of time in recreational activities. The point estimate on the coefficient of risk tolerance for recreational time use indicates that a one standard deviation more risk tolerant family will spend 0.7 extra hours per week in recreational time, but this is imprecisely estimated.

Columns 10 through 12 of Table 2.7 form a sort of falsification test by using parental time in basic care as an outcome. Time spent in basic care, as discussed previously, is less likely to be related to children's future labor market skills, and thus should present less of a risk to parents. If more risk tolerant parents also spend longer in basic care, this might suggest that the risk tolerance measure is picking up other family level unobservables that are related to time with children in general. When risk tolerance is introduced in column 12, it is clear that more risk tolerant parents do not spend more time in basic care for their young children. A one standard deviation increase in risk tolerance is associated with about 0.3 fewer hours per week in basic care. This estimate is half the size of that in column 6, and is only 2% of the mean level of basic care time. This is consistent with risk tolerance being uncorrelated with other family characteristics but related to parents' propensity to invest in their children's risky human capital.

The time that parents spend on basic care is related very strongly to age. By age 5, columns 10 and 11 suggest that children receive about 10 fewer hours per week of basic care than at birth. This is unsurprising, as the amount of time required to feed, clothe, and care for children moderates dramatically over the first few years of life. Parents of non-white children also spend about 4 fewer hours per week in basic child care than parents of white children. While adding income, school enrollment, and family structure in column 10 increases the $R^2$ substantially for other types of time use, these variables have little additional explanatory power for basic care. This is consistent with the idea that while attending to children's most basic physical needs is important, the marginal benefit diminishes relatively quickly once the basic needs are met.

Altogether, Table 2.7 provides evidence that is consistent with parents treating their time with children as an investment in a risky asset, as described in Section 2.2. The point estimates suggest that more risk tolerant parents spend an additional 0.56 hours per week in educational activities with their children. This coefficient is 25% of the sample mean, and is very statistically significant. More risk tolerant parents also appear to spend more time overall and in recreational activities with their children in early childhood, but these effects are not statistically significant. More risk tolerant parents do not spend more time during early childhood with their children in basic care. Thus in addition to estimating a large and statistically significant impact of parental risk tolerance on educational time use, I also show that risk tolerance is more clearly related to parental time use within categories that more closely represent investment in children's human capital.

I next turn my attention to parental time as measured in 2002. At this point, the children in the sample are now between ages 5 and 10. The theory of Section 2.2 does not have a strong prediction for how time in late childhood should depend on risk tolerance. More risk tolerant parents prefer higher overall investment levels in their children's human capital throughout childhood, which suggests that more risk tolerant parents would invest more time in their children in both early and late childhood. At the same time, if early childhood represents a riskier but more productive investment, more risk tolerant parents will tilt their time investments away from late childhood and towards early childhood, potentially resulting in less time spent in late childhood. The relative strength of each of these effects is an empirical question, which I explore in Table 2.8.

The first three columns of Table 2.8 regress total parental time in 2002 on a similar set of controls to the analogous columns in Table 2.7.[30] As can be seen from comparing columns 2 and 3 of Table 2.8, including risk tolerance again does not appear to change the coefficients on other demographic variables significantly, and this is again borne out by a Hausman test. The risk tolerance coefficient suggests that more risk tolerant families spend 2.0 fewer hours per week with their children overall. This coefficient is statistically significant at only the 10% level. This is consistent with parents' substitution of time investments towards early childhood outweighing the desire for more investment throughout childhood, resulting in decreases in time spent in late childhood.

Columns 4 through 6 of Table 2.8 again use time spent in education as a dependent variable. Income and family structure are also important determinants of late childhood educational time – the $R^2$ increases from 0.03 in column 4 to 0.13 in column 5. Adding in risk tolerance from column 5 to column 6 does not change the other coefficients, and the estimated coefficient is essentially zero (0.038). That this coefficient is essentially zero suggests that while more risk tolerant parents might

[30] I use the number of siblings and the age of the youngest child from 2002.

Table 2.8: 2002 parental time with children in the PSID CDS and risk tolerance

| | All | | | Education | | | Recreation | | | Basic Care | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Risk Tolerance (KSS) | | | -1.992+ | | | 0.0378 | | | -1.348+ | | | 0.00501 |
| | | | (1.155) | | | (0.186) | | | (0.726) | | | (0.307) |
| Child Age (Months) | -0.181 | -0.312 | -0.381 | -0.0597 | -0.0538 | -0.0506 | -0.136 | -0.117 | -0.161 | -0.0218 | -0.0530 | -0.0526 |
| | (0.273) | (0.308) | (0.304) | (0.0503) | (0.0522) | (0.0539) | (0.193) | (0.219) | (0.216) | (0.0883) | (0.101) | (0.103) |
| Child Age$^2$/1000 | 1.871 | 4.940 | 5.881 | 0.921 | 0.976 | 0.927 | 1.776 | 2.228 | 2.816 | 0.135 | 0.861 | 0.855 |
| | (4.089) | (4.682) | (4.628) | (0.735) | (0.753) | (0.768) | (2.978) | (3.378) | (3.363) | (1.255) | (1.502) | (1.524) |
| Non-White | -3.612 | -3.611 | -4.594 | -0.584 | -0.145 | -0.238 | -4.004 | -4.111 | -4.952+ | -1.813+ | -1.647 | -1.657 |
| | (3.649) | (3.991) | (4.166) | (0.409) | (0.470) | (0.452) | (2.484) | (2.566) | (2.629) | (1.001) | (1.115) | (1.164) |
| Male | 1.675 | 2.696 | 2.807 | -0.200 | -0.0232 | 0.0301 | 2.939+ | 3.962* | 4.124** | -0.0927 | -0.000466 | 0.00535 |
| | (2.326) | (2.408) | (2.371) | (0.462) | (0.445) | (0.421) | (1.529) | (1.596) | (1.590) | (0.688) | (0.704) | (0.718) |
| Age of Mother | 0.288 | 0.340 | 0.414 | -0.0569 | -0.0701 | -0.0549 | 0.212 | 0.386 | 0.463 | 0.0000882 | 0.00118 | 0.00282 |
| | (0.398) | (0.395) | (0.395) | (0.0734) | (0.0737) | (0.0743) | (0.347) | (0.358) | (0.354) | (0.120) | (0.136) | (0.137) |
| Age of Father | -0.698+ | -0.509 | -0.543 | 0.0783 | 0.137+ | 0.128+ | -0.400 | -0.342 | -0.380 | -0.0975 | -0.0582 | -0.0591 |
| | (0.365) | (0.353) | (0.353) | (0.0711) | (0.0717) | (0.0710) | (0.275) | (0.267) | (0.264) | (0.102) | (0.113) | (0.113) |
| Mom Col. Grad | -3.317 | -2.687 | -3.090 | 0.175 | 0.104 | 0.0723 | -3.059+ | -2.600 | -2.935 | 0.361 | 0.668 | 0.665 |
| | (2.540) | (2.513) | (2.505) | (0.477) | (0.423) | (0.418) | (1.753) | (1.867) | (1.859) | (0.726) | (0.787) | (0.788) |
| Mom HS Grad | 10.59** | 6.834+ | 6.771+ | 0.543 | 0.196 | 0.154 | 5.423* | 2.933 | 2.822 | 0.206 | -0.430 | -0.435 |
| | (3.348) | (3.925) | (3.970) | (0.521) | (0.552) | (0.591) | (2.638) | (2.710) | (2.758) | (1.278) | (1.411) | (1.414) |
| Total Income / $100k | | -0.734 | -0.772 | | -0.213 | -0.267 | | -1.756 | -1.869 | | -0.290 | -0.296 |
| | | (3.287) | (3.274) | | (0.391) | (0.392) | | (2.208) | (2.217) | | (0.871) | (0.879) |
| Male 1996 Respondent | | | -1.815 | | | -0.519 | | | -2.099 | | | -0.0563 |
| | | | (2.290) | | | (0.467) | | | (1.585) | | | (0.758) |
| Number of Children | | X | X | | X | X | | X | X | | X | X |
| Age of Youngest | | X | X | | X | X | | X | X | | X | X |
| $R^2$ | 0.0531 | 0.135 | 0.151 | 0.0247 | 0.126 | 0.132 | 0.0572 | 0.133 | 0.154 | 0.0213 | 0.104 | 0.104 |
| N | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 |
| Mean of Dep. Var. | 35.90 | 35.90 | 35.90 | 1.777 | 1.777 | 1.777 | 14.97 | 14.97 | 14.97 | 8.037 | 8.037 | 8.037 |

*Standard errors in parentheses:* + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
For a discussion of the sample selection, see the footnote to Table 2.1. For a discussion of the time category definitions and specifications, see the notes to Table 2.7.
*Variables Definitions:* See the notes to Table 2.7. The controls for the number of children and the age of the youngest child are those for the family in 2002, rather than in 1997.

both invest more in their children's education time and tilt their time investment profile towards early childhood, the net effect on educational investment time in late childhood is zero.

Columns 7 through 9 of Table 2.8 present the analysis for recreational time in 2002. Male children receive about 3 additional hours per week of parental time, which is statistically significant and consistent with. This is consistent with the findings of Lundberg, Pabilonia, and Ward-Batts (2006), who find that parents spend more recreational time with sons. Families where the mother has graduated high school spent over 5 additional hours per week in recreational activities with their children relative to other parents. Adding risk tolerance into column 9, one standard deviation more risk tolerant parents spend about 1.1 fewer hours per week in recreational time with their older children. Though this effect is not precisely estimated, this point estimate is consistent with more risk tolerant parents substituting their investments towards early childhood.

Columns 10 through 12 of Table 2.8 again use time spent in basic care as a form of falsification test. The change in the age coefficients from column 10 to 11 again suggests that having a younger sibling is an important factor in the degree of time in basic care that children receive. The risk tolerance coefficient in Column 12 indicates that more risk tolerant parents spend essentially the same amount of time in basic care as more risk averse parents. Again the point estimate is close to zero and effects larger than 10% off of the mean in either direction can be ruled out.

Next, I turn attention to perhaps the most effective test of the theory in Section 2.2, which comes from looking at how the slope of the time investment profile varies with risk tolerance. To do this, I take the difference between time investments in 1997 and 2002. More risk tolerant parents have two motivations for changing their investments that will affect the difference between their time investments in early and late childhood. First, they would prefer to invest more in their children throughout childhood. Without uncertainty, increasing investment will require proportional increases across childhood.[31] Thus as parents invest more throughout childhood, the absolute difference between time when young and when school-age will increase. Second, more risk tolerant parents prefer to tilt their time investment profiles. That is, they prefer to shift their investment from relatively certain but relatively unproductive time in late childhood towards relatively risky but relatively more productive time in early childhood. Both of these motives suggest that more risk tolerant parents should have positive coefficients in regressions of the difference between time in early and late childhood on risk tolerance. These coefficients are estimated in Table 2.9.

Because Table 2.9 exploits the panel nature of the PSID CDS time diary data, by using as the dependent variable the time in 1997 minus time in 2002 by a family with a particular child, I difference out any family or child fixed characteristics which affect time use equally throughout childhood. Therefore, this specification has an added advantage over the cross-sectional results which can only control for observable differences across families and children.

Columns 1 through 3 of Table 2.9 present regressions on the difference in overall

[31] This is because the child production function is homogeneous of degree one or constant returns to scale.

Table 2.9: Differences in parental time (1997 – 2002) over childhood in the PSID CDS and parental risk tolerance

| | All | | | Education | | | Recreation | | | Basic Care | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Risk Tolerance (KSS) | | | 3.761* | | | 0.531+ | | | 2.069* | | | -0.198 |
| | | | (1.586) | | | (0.286) | | | (0.897) | | | (0.625) |
| Child Age (Months) | 0.141 | 0.603 | 0.780+ | 0.188** | 0.259** | 0.283** | 0.0868 | 0.266 | 0.363 | -0.365* | -0.221 | -0.231 |
| | (0.373) | (0.446) | (0.459) | (0.0690) | (0.0826) | (0.0865) | (0.258) | (0.331) | (0.336) | (0.148) | (0.191) | (0.189) |
| Child Age$^2$/1000 | -3.475 | -9.684 | -11.82+ | -2.569* | -3.525** | -3.810** | -1.749 | -4.211 | -5.369 | 3.415 | 1.187 | 1.321 |
| | (5.261) | (6.455) | (6.629) | (1.013) | (1.198) | (1.262) | (3.580) | (4.611) | (4.690) | (2.161) | (2.772) | (2.736) |
| Non-White | -3.843 | 0.122 | 1.731 | 1.429 | 0.349 | 0.613 | 2.391 | 5.773* | 6.699* | -2.166+ | -2.518 | -2.556 |
| | (4.010) | (4.899) | (5.025) | (1.208) | (1.017) | (0.976) | (2.840) | (2.880) | (2.992) | (1.308) | (1.655) | (1.723) |
| Male | -0.883 | -1.932 | -1.656 | -0.158 | -0.134 | -0.112 | -2.170 | -3.424 | -3.290 | 0.106 | 0.263 | 0.229 |
| | (3.083) | (3.297) | (3.298) | (0.578) | (0.583) | (0.549) | (2.052) | (2.170) | (2.176) | (1.048) | (1.097) | (1.115) |
| Age of Mother | -0.663 | -0.548 | -0.584 | 0.0921 | 0.118 | 0.108 | -0.852* | -0.994** | -1.020** | 0.177 | 0.302 | 0.297 |
| | (0.603) | (0.561) | (0.604) | (0.0991) | (0.107) | (0.107) | (0.415) | (0.378) | (0.391) | (0.205) | (0.213) | (0.209) |
| Age of Father | 0.498 | 0.553 | 0.584 | -0.118 | -0.204* | -0.197* | 0.473 | 0.632+ | 0.653+ | -0.101 | -0.166 | -0.164 |
| | (0.562) | (0.589) | (0.627) | (0.0921) | (0.0918) | (0.0895) | (0.350) | (0.354) | (0.365) | (0.157) | (0.171) | (0.168) |
| Mom Col. Grad | 6.521* | 4.989 | 5.478 | 0.642 | 0.322 | 0.405 | 5.186* | 5.170* | 5.455* | -0.940 | -1.770 | -1.778 |
| | (3.106) | (3.331) | (3.448) | (0.653) | (0.576) | (0.564) | (2.280) | (2.595) | (2.691) | (1.128) | (1.231) | (1.250) |
| Mom HS Grad | -7.013 | -5.731 | -6.147 | 0.318 | -0.107 | -0.157 | -1.792 | -1.513 | -1.732 | -2.666+ | -1.699 | -1.666 |
| | (5.252) | (5.872) | (5.901) | (0.811) | (0.904) | (0.880) | (3.468) | (3.859) | (3.889) | (1.581) | (2.033) | (2.014) |
| Total Income / \$100k | | 0.837 | 0.419 | | 1.550 | 1.511 | | 1.537 | 1.330 | | -0.922 | -0.874 |
| | | (3.995) | (3.932) | | (0.941) | (0.918) | | (3.074) | (3.133) | | (1.337) | (1.332) |
| In School | | -5.892+ | -6.290+ | | -0.166 | -0.231 | | -2.938 | -3.167 | | 1.252 | 1.261 |
| | | (3.448) | (3.392) | | (0.609) | (0.605) | | (2.167) | (2.178) | | (1.163) | (1.174) |
| Male 1996 Respondent | | | 0.0542 | | | 0.156 | | | 0.194 | | | 0.185 |
| | | | (3.295) | | | (0.675) | | | (2.353) | | | (1.093) |
| Number of Children | | X | X | | X | X | | X | X | | X | X |
| Age of Youngest | | X | X | | X | X | | X | X | | X | X |
| $R^2$ | 0.0358 | 0.147 | 0.170 | 0.0518 | 0.212 | 0.228 | 0.0557 | 0.158 | 0.174 | 0.101 | 0.188 | 0.189 |
| N | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 |
| Mean of Dep. Var. | 14.77 | 14.77 | 14.77 | 0.195 | 0.195 | 0.195 | 3.813 | 3.813 | 3.813 | 7.468 | 7.468 | 7.468 |

*Standard errors in parentheses:* + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
For a discussion of the sample selection, see the footnote to Table 2.1. For a discussion of the time category definitions and specifications, see the notes to Table 2.7.
*Variables Definitions:* See the notes to Table 2.7. The controls for the number of children and the age of the youngest child include both the 1997 and 2002 variables for each family.

parental time between 1997 and 2002. Children with college graduate mothers receive investment profiles that are 6.5 hours per week steeper than their peers with high school graduate mothers. In column 2, this difference drops slightly to 5.0 hours and becomes statistically insignificant. The average slope of the investment profile in the sample is 14.8 hours per week, which suggests that the average child in this sample received nearly 15 more hours of parental time in 1997 than in 2002. The coefficient on school enrollment in column 2 is large and negative ($-5.9$), which is as expected. Parents whose children were enrolled in school in early childhood spent less time with them in 1997, while by ages 5-10 all children are enrolled in school. This coefficient is smaller than the 12.0 fewer hours per week that children of parents in daycare spent with them in 1997 (Table 2.7), suggesting that these parents spent less time with their children in 2002 as well.

Column 3 of Table 2.8 adds the risk tolerance measure. Again, adding the risk tolerance measure does not markedly change the coefficients on the other controls between columns 2 and 3. Parents who are one standard deviation more risk tolerant spend about 3.8 more hours with their children in early childhood compared to late childhood. This effect is statistically significant at the 5% level, and suggests that more risk tolerant parents are investing relatively more heavily in early childhood than in late childhood, in order to take advantage of the high productivity of early childhood investments. This is investment profile is tilted 25% more steeply than the average of 14.8 hours per week. By comparison, the difference between families with college and high school graduate mothers is 5.5 hours per week in column 3. The difference between the investments of two families who differ in risk tolerance by about a one and a half standard deviations is the same as the estimated difference between families where the mother has a college diploma compared to a high school diploma.

Columns 4 through 6 present a similar analysis for the difference in educational time between 1997 and 2002. In column 5, the estiamted coefficient on father's age, at $-0.2$, is statistically significant, but it is opposite in sign of the estimated coefficient on mother's age and marriage is highly assortative on age, so this coefficient is hard to interpret. Column 6 adds risk tolerance to the regressions of time differences between 1997 and 2002. More risk tolerant families spend 0.5 additional hours per week with their children in early childhood compared to late childhood. This coefficient is marginally statistically significant at the 10% level. The average difference between educational time in early and late childhood in Table 2.1 is only 0.2 hours, though, so a one standard deviation increase in risk tolerance more than doubles the slope of the age-investment profile, or the amount that parents tilt their time investments towards early childhood. The positive coefficient on risk tolerance in column 6 of Table 2.9 is consistent with the theory that more risk tolerant parents should have steeper age investment profiles in order to take greater advantage of productive investments in younger children. The results imply that children of parents who are one standard deviation more risk tolerant will start kindergarten (reach age 6) with about 165 additional hours of time with their parents in educational activities.[32] By comparison, we know that parental education is an important

[32]This suggests that examining the relationship between risk tolerance and child test scores would be a fruitful avenue of research to pursue.

determinant of children's future human capital and children with college educated mothers will start kindergarten with about 125 more hours of (between 30 and 220 hours with 95% confidence) educational time children of high school educated mothers. This is not to suggest that risk tolerance necessarily is equally important as parental education for other types of parental investments, or eventual human capital endowments, as parental education. But risk tolerance is clearly an important determinant of parental time investment patterns over childhood, and its importance for time investment is comparable in magnitude to parental education.

Columns 7 through 9 of Table 2.9 present the results for recreational time. As mentioned previously, recreational time also likely develops human capital but is perhaps a less intensive investment than educational time. Families with college educated mothers have more steeply tilted time investment profiles in recreational time than families with high school educated mothers, by about 5 hours per week. The average child has a slope of the investment profile of only 3.8 hours per week. Introducing risk tolerance in column 9 once again changes the coefficients on other variables relatively little, but has a large and statistically significant effect on the difference in recreational time between early and late childhood. A family that is a single standard deviation more risk tolerant will spend 2.1 more hours in recreational activities in early childhood than in late childhood. This is more than half of the mean slope. A two standard deviation difference in risk tolerance has the same effect on the slope of recreational investments as the difference between families with college and high school educated mothers. However, the same caveat as above, that this does not suggest that risk tolerance and parental education are equally important either for overall investment or for children's human capital, still applies. Again, to the extent that recreational time is one measure of parental investment in children's human capital, this provides evidence in favor of the theoretical prediction that more risk tolerant families have more steeply tilted time investment profiles and thus invest more heavily in early childhood.

A falsification test is once again provided by columns 10 through 12 of Table 2.9 using time in basic care. If time spent in basic care is relatively unimportant for the uncertain human capital that parents invest in, then there is no reason for the age-investment profile in basic care to slope downward more steeply with risk tolerance. Indeed, this appears to be the case, as the coefficient in column 12 is very small and not statistically significant.

The results in Tables 2.7 through 2.9 are very consistent with the theoretical prediction from Section 2.2 that more risk tolerant parents will tilt their time investment profiles more steeply towards early childhood. There is evidence that parents invest more in early childhood. While the point estimates on total (1.9) and recreational time (0.7) use are positive but not statistically significant, the estimated coefficient on educational time (0.56) is positive, large compared to the mean sample average, and statistically significant. More risk tolerant parents choose more steeply tilted time investment profiles by substituting their time towards early childhood by 3.8 hours per week overall, 0.5 in educational time, and 2.1 in recreational time. By contrast, they do not tilt their time investment profiles in basic care time, which is less related to children's future skill development.[33]

[33]Appendix Tables B1, B2, and B3 present parallel specifications to those in Tables 2.7, 2.8,

It is worth a brief discussion of other potential explanations that the empirical patterns in these few tables have ruled out. First, consider a simple model of parental investment with only a single period of childhood. The relationships between time use and risk tolerance are very different across childhood. Indeed, Table 2.9 can be considered a test of whether the estimated coefficients on each control variable in 1997 and 2002 are the same. The coefficients for risk tolerance are not. Another explanation that is rejected by the data is that risk tolerance is picking up a latent measure of socioeconomic status. In Table 2.4, it is clear that risk tolerance is not related to observable socioeconomic indicators, so for risk tolerance to pick up latent socioeconomic status, it would have to be related to the portion of an unobserved factor that is also uncorrelated with income, race, age, and education. Tables 2.7 through 2.9 provide additional indirect evidence of this. Adding risk tolerance in the third of each group of columns has a small effect on the other coefficients. This suggests that risk tolerance is not correlated with the same unobserved predictors of time investment which are correlated with the other control variables. Many other likely determinants of parental investents would be correlated with these family level characteristics.

It is harder to test the prediction from the theory that more risk tolerant parents invest more in their children overall. The patterns that emerge in the preceding regressions suggest that more risk tolerant parents tilt their time investment profile

and 2.9, but present a different set of controls along with risk tolerance. These tables repeat the first column in each set. The second column in each set of specifications in Appendix A.2 adds risk tolerance but contains only the predetermined controls, omitting factors that might be jointly determined with parental time investment. Though these tables are not discussed in depth in the text, the point estimates for risk tolerance are relatively similar to the third column.

toward early childhood but do not necessarily spend more time altogether with their children throughout childhood (with the exception of educational time). However, as suggested by the theory, on the margin, shifting an hour of time from late childhood to early childhood will have a net positive effect on expected children's human capital. Thus parents who spend the same total number of hours over childhood, but who spend more of the time in early childhood will realize greater levels of children's human capital. By the same argument, even if parents substitute slightly less than one hour toward early childhood and away from one hour in late childhood, they could still be increasing the expected level of children's human capital. To know more about whether more risk tolerant parents choose higher overall levels of investment, we need to know more about the technology of child development.

For realistic values of the marginal productivity of investment in early and late childhood, it is likely that more risk tolerant parents are investing in higher levels of human capital. There is considerable evidence that early childhood interventions are more effective at building skills and remedying gaps than late childhood interventions. This suggests that, given current investment levels, the marginal benefit of an extra unit of investment is larger in early than in late childhood. If risk tolerant parents appear to spend roughly the same total number of hours over childhood, as suggested by the roughly equal but opposite coefficients on risk tolerance in column 3 of Tables 2.7 and 2.8, they are investing more heavily in their children overall.[34]

[34]This is of course only a rough comparison. Investment can take many forms. It could be that the marginal productivity of investment from parental time in early childhood is relatively lower than the productivity of the basket of investments represented by early childhood education programs.

#### 2.3.2.3 The Robustness of Risk Tolerance and Time Use Measures

The analysis to this point has used a particular mapping between responses to the risk tolerance question and expected risk tolerance bins. This measure was based on correcting for measurement error and status quo bias as they exist in the Health and Retirement Study (Kimball, Sahm, and Shapiro (2008)). These authors find that in the context of how risk tolerance predicts the share of portfolio that households hold in stocks in the HRS, correcting for individual level measurement error increased the strength of the relationship between the cardinal risk tolerance measure and portfolio allocations. Since the PSID only has a single measurement of risk tolerance per household, Kimball, Sahm, and Shapiro (2009a) take the measured magnitudes of measurement error and status quo bias from the HRS and use these parameters in the maximum likelihood estimation for risk tolerance in the PSID. It is this maximum-likelihood measure that I use in Sections 2.3.1.1 and 2.3.2.2.

I relax the assumptions on the form of the utility function, measurement error, and status quo bias in Table 2.10 by using alternative measures of risk tolerance. I present the coefficients on risk tolerance measures from regressions like those in Columns 3, 6, 9, and 12 of Tables 2.7, 2.8, and 2.9. Each set of three columns presents the risk tolerance coefficient from regressions of a given type of parental time in 1997, in 2002, and the difference between 1997 and 2002 on the measure of risk tolerance indicated by the row. Note that for each of the first three rows, I take each risk tolerance measure and standardize it to have mean zero and standard deviation 1. Therefore, the coefficients in each of the first three rows of Table 2.10

Table 2.10: Exploration of alternative measures of risk tolerance

| | All | | | Education | | | Recreation | | | Basic Care | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1997 | 2002 | 1997 - 2002 | 1997 | 2002 | 1997 - 2002 | 1997 | 2002 | 1997 - 2002 | 1997 | 2002 | 1997 - 2002 |
| Risk Tolerance (KSS) | 1.861 | -1.951+ | 3.761* | 0.558** | 0.0373 | 0.531+ | 0.715 | -1.322+ | 2.069* | -0.262 | 0.0182 | -0.198 |
| | (1.561) | (1.123) | (1.586) | (0.208) | (0.186) | (0.286) | (1.009) | (0.701) | (0.897) | (0.572) | (0.306) | (0.625) |
| Risk Tolerance (LJ) | 0.928 | -1.401 | 2.249+ | 0.474+ | -0.0301 | 0.457 | 0.434 | -0.975 | 1.409+ | -0.246 | 0.188 | -0.413 |
| | (1.515) | (1.102) | (1.331) | (0.241) | (0.147) | (0.289) | (0.963) | (0.631) | (0.754) | (0.554) | (0.255) | (0.577) |
| Risk Tolerance (BJKS) | 2.058 | -2.277* | 4.314** | 0.507** | 0.103 | 0.438 | 0.797 | -1.565* | 2.326* | -0.0987 | -0.0539 | 0.0885 |
| | (1.506) | (1.148) | (1.605) | (0.191) | (0.225) | (0.318) | (1.031) | (0.773) | (0.978) | (0.567) | (0.349) | (0.612) |
| Accept 1/10 Loss | 3.175 | -2.445 | 6.028 | 0.952** | 0.195 | 1.040+ | 1.611 | -1.500 | 3.629 | -0.856 | -0.207 | -0.396 |
| | (3.677) | (2.850) | (4.328) | (0.323) | (0.472) | (0.561) | (2.413) | (1.884) | (2.536) | (1.395) | (0.865) | (1.727) |
| Accept 1/5 Loss | 0.920 | -6.135** | 7.393* | 0.576 | 0.181 | 0.732 | 0.345 | -4.882** | 5.280* | -1.175 | -0.491 | -0.165 |
| | (3.029) | (2.281) | (3.273) | (0.357) | (0.408) | (0.580) | (2.037) | (1.618) | (2.216) | (1.069) | (0.769) | (1.228) |
| Accept 1/3 Loss | 6.031+ | -4.405+ | 10.42** | 0.881* | 0.138 | 0.833 | 2.779 | -3.011+ | 6.077** | -0.148 | -0.527 | 0.429 |
| | (3.095) | (2.340) | (3.227) | (0.357) | (0.444) | (0.569) | (2.169) | (1.557) | (2.169) | (1.114) | (0.722) | (1.182) |
| Accept 1/2 Loss | 6.026+ | -2.633 | 8.286* | 1.339** | 0.196 | 0.998 | 1.824 | -1.227 | 2.787 | 0.636 | 0.380 | 0.325 |
| | (3.432) | (2.792) | (3.668) | (0.471) | (0.551) | (0.780) | (2.259) | (1.747) | (2.149) | (1.337) | (0.861) | (1.388) |
| Accept 3/4 Loss | 0.990 | -4.358 | 4.733 | 1.557 | -0.358 | 1.660 | 0.193 | -3.163 | 3.258 | -1.445 | 0.866 | -2.403 |
| | (6.066) | (4.677) | (4.805) | (1.044) | (0.563) | (1.235) | (3.899) | (2.602) | (2.953) | (2.264) | (1.040) | (2.350) |
| N | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 |
| Mean of Dep. Var. | 50.67 | 35.90 | 14.77 | 1.973 | 1.777 | 0.195 | 18.78 | 14.97 | 3.813 | 15.50 | 8.037 | 7.468 |

*Standard errors in parentheses:* $+\ p < 0.10$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

*Note:* These tables present regressions of the time in 1997, 2002, and the difference in time in 1997 minus time in 2002 that PSID CDS children spend in activities where either the mother or father are listed as individuals “with whom” the child does the activity between the 1997 and 2002 time diaries. For a discussion of the sample selection, see the footnote to Table 2.1. The first row replicates the coefficients on risk tolerance in Table 2.7, 2.8, and 2.9. Each subsequent row replicates the same 12 regressions using different measures of risk tolerance. For information on the sample selection and variable definitions in the controls, see the notes of Tables 2.7, 2.8, or 2.9.

*Measures of Risk Tolerance:* The first row uses the measure of risk tolerance recommended by Kimball, Sahm, and Shapiro (2009a). The second row uses the naïve calculation without correction for measurement error done by Luoh and Stafford (1997). The third row uses the error correction calculation done by Barsky, Juster, Kimball, and Shapiro (1997). All measures are standardized to mean 0 and standard deviation 1. The next rows present binary measures of risk tolerance based on whether an individual would accept a gamble with a $50-50$ chance of either doubling their permanent income or losing the fraction presented in the table. Thus as one reads down each row, additional families are reclassified as risk averse rather than risk tolerant.

can be compared directly as the effect of a one standard deviation increase in family risk tolerance on parental time. The first row is the measure of risk tolerance from Kimball, Sahm, and Shapiro (2009b), which is the preferred specification and was used earlier in Section 2.3.2.2. The coefficients on total parental time, in 1997, 2002, and the difference between them, were 1.9, −2.0, and 3.8, respectively. The second row presents a mapping of responses to risk tolerance which does not account for measurement error, which Luoh and Stafford (1997) estimate by simply assuming that risk tolerance is log-normally distributed, fitting the mean and variance of the distribution, and then assigning each bin to its expected value using the fitted distribution and given that the response is in that bin. Thus the measure in the second row does not correct for measurement error at all. Using the uncorrected risk tolerance measure, the effect on total time is somewhat attenuated, dropping from 1.9 hours to 0.9 hours in 1997, and from −2.0 hours to −1.4 hours in 2002. There is now a reallocation of 2.2 hours towards early childhood, down from 3.8 hours using the error corrected measure. This suggests that, consistent with the literature on the use of the risk tolerance measure, measurement error is important. The effects on educational and recreational time are also attenuated somewhat, and there is still no effect of risk tolerance on time with children in basic care.

The third row of Table 2.10 uses a naïve error correction method where the bins are centered based on the error correction in the Health and Retirement Study done by Barsky, Juster, Kimball, and Shapiro (1997). Instead of using the estimated magnitude of measurement error from the HRS to fit a maximum likelihood model in the PSID, these estimates simply use the mapping from survey responses to cardinal risk tolerance directly from the HRS. Because this method will correct for at least some of the measurement error, I would expect these coefficients to be attenuated only slightly from those in the first row of Table 2.10. Using this error correction method, the results are in fact slightly larger and more statistically significant for total parental time (2.1, −2.3, and 4.3 instead of 1.9, −2.0, and 3.8 in 1997, 2002, and the difference, respectively) and recreational time (0.8, −1.5, and 2.3 instead of 0.7, −1.2, and 2.1 for each of the three time measures). The results are slightly less strong for educational time (0.5, 0.1, and 0.4 instead of 0.6, 0.0, and 0.5, respectively). In none of the first three rows is basic care time related to risk tolerance. It is encouraging that even though shifting the placement of response bins matters modestly for the magnitudes of the estimates, the signs and overall patterns don't change. That rows 1 and 3, which use corrections for measurement error, have larger point estimates than the second row suggests that measurement error in risk preferences is again important.

The next several rows of Table 2.10 further relax distributional assumption on the risk tolerance parameter. These rows present binary risk tolerance measures that are equal to one if the respondent would accept a gamble with a 50% chance of doubling or losing the presented fraction of future income. The rows proceed in order from defining as risk tolerant anyone who would accept the risk of potentially losing 1/10 of income to anyone who would accept a risk of losing 3/4 of their income. Because these measures of risk tolerance are binary, the regression coefficients in these cases have the interpretation of the difference in parental time allocation between the "risk averse" and the "risk tolerant" group. Thus, the magnitudes of

Table 2.11: Parental time with children by intensity and risk tolerance

| | 1997 | 2002 | 1997 - 2002 |
|---|---|---|---|
| Primary | 1.810 | -2.057+ | 3.765* |
| | (1.517) | (1.102) | (1.542) |
| Secondary | -3.135* | -0.723 | -2.136 |
| | (1.264) | (1.145) | (1.468) |
| Primary + Secondary | -1.326 | -2.780 | 1.629 |
| | (2.043) | (1.713) | (2.284) |
| N | 298 | 298 | 298 |

*Standard errors in parentheses:* $+\, p < 0.10$, $^{*}\, p < 0.05$, $^{**}\, p < 0.01$, $^{***}\, p < 0.001$

*Note:* The first row of this table presents regressions of the time that PSID CDS children spend in activities where either the mother or father are listed as individuals "with whom" the child does the activity. This is the same measure of time use used in Tables 2.7, 2.8, and 2.9. The second row of the table aggregates time that children spend with parents when they are not "with whom" the child was doing an activity but are the answer to the question "who else was there". The third row of the table takes a simple sum of these two measures. For a discussion of the sample selection, see the footnote to Table 2.1. The controls are the same as those reported in Tables 2.7, 2.8, and 2.9. The first column of the table uses 1997 time use as the outcome variable, the second column uses 2002 time use data, and the third uses the difference between 1997 and 2002 time use.

these coefficients are not directly comparable to those in the first three rows. More risk tolerant parents still spend more time with their children early in childhood and less later in childhood, and the most statistically significant result is on the time reallocation coefficient. Though there are differences in coefficients as the "risk tolerant" cutoff becomes more stringent, the general patterns are fairly robust across a range of definitions of risk tolerance.

Another useful check that the effect of parental risk tolerance is due to investment behavior, rather than other unobserved differences between families, is presented in Table 2.11. This table takes advantage of the question on the CDS time diaries which, in addition to asking "with whom" the child did an activity, also asks "who else was there". This allows the measurement of parents' time spent generally supervising but not actively engaged with their children. While time spent as "someone else who was there" is an important parental responsibility, it is unlikely to lead to the same sort of human capital accumulation as time spent actively engaged with children. The first row of Table 2.11 presents the by now familiar main regression for total time that children spent in a primary activity with their parents.[35] The second row presents the regression using the total amount of time that either parent was listed as also present. One standard deviation more risk tolerant parents spend 3.1 *fewer* hours per week "around" their children in 1997, 0.7 fewer hours per week around them in 2002, and altogether have secondary time investment profiles that are 2.1 hours per week *less steep*. Interestingly, the second row suggests that more risk tolerant parents spend *less* time around but not engaged with their children in early childhood. Indeed, parents appear to shift their supervisory time towards *later* childhood.

The third row presents the regression on the sum of the two measures of time investment. The estimated coefficients on risk tolerance in these regressions using the sum of primary and secondary time as a dependent variable are −1.3 in 1997,

[35]This table does not present breakdowns by activity because the connection between what children are doing when parents are around but not actively involved and parental investment is less clear. It is true that it might increase a child's human capital to spend time on homework by herself, but it is less obvious how to think about this decision reflects parental investment.

−2.7 in 2002, and 1.6 on the difference, though these are all imprecisely estimated. This pattern, where risk tolerance is not strongly related to the overall time spent either actively or passively with children, is consistent with the analysis of Guryan, Hurst, and Kearney (2008). Guryan, Hurst, and Kearney briefly analyze a measure in the ATUS of time that parents spend counting their children as "with whom" an event took place. They show that while there is a strong education gradient in the amount of time that parents spend in primary child care activities with their children, the gradient goes away when including the "with whom" time as well. This suggests that the total number of hours that parents spend "around" their children is a fairly fixed quantity. But parents who want to invest more in human capital spend less of this time passively supervising their children and more time doing activities with them. That total time with children around is relatively fixed is consistent with the observed pattern that more educated families both spend more time in child care and more time in the labor force.[36]

## 2.4 Conclusion

When parents make the decision to invest in their children, they must face uncertainty as to the effects of their actions. I show that PSID risk tolerance supplement measure of risk preferences matter for PSID CDS time diary measures of

[36]At first glance, the implication that parents have a fixed pool of time with their children "around" which they allocate between active and passive child care seems at odds with my illustrative modeling framework in Section 2.2 where parents allocate their time between child care and market work, whose outputs can be borrowed and saved. But if time that parents spend in passive child care is also time spent in home production, the illustrative modeling framework is appropriate.

parental time investments. More risk tolerant parents appear to spend more time with their children in early childhood and less in late childhood than their more risk averse peers. I explain this apparent substitution of investments over childhood using an illustrative model of parental investment in children's human capital under uncertainty. While early childhood investments are more productive, they are also more risky. Investments in late childhood are less risky but also less productive. I show that more risk tolerant parents will, when compared to their less risk tolerant peers, both invest more throughout childhood and tilt their time investment profiles towards early childhood.

I test the implications of this illustrative model using the PSID CDS and Risk Tolerance Supplements. The rich time diary data in the CDS allow me to isolate portions of parental time use that are related to children's human capital, such as recreational and educational time, as well as portions of time that are less likely to represent investment in children's futures. I find that one standard deviation more risk tolerant parents invest 25% more time (0.6 hours per week) in educational activities during early childhood. I also find that one standard deviation more risk tolerant parents tilt their time investment profiles of total, recreational, and educational time towards early childhood by 3.8, 0.6, and 2.1 hours per week, respectively. These reallocations make children of risk tolerant parents receive investments that are slanted towards early childhood by 25%, 250%, and 50% more the respective sample averages. While imprecisely estimated, these effects are statistically different from zero. The effects seem proportionately stronger in parental activities that are more consistent with investments which will affect children's future human capital, though again the estimates are noisy. These patterns are consistent with risk tolerant parents reacting to the uncertainty that preparing young children for the future represents. More risk tolerant parents do not tilt their time investment profiles in basic childcare time, time which is less likely to represent investment in children's uncertain futures. Because risk tolerance is virtually uncorrelated with other family level variables, these differences appear to be related to actual underlying parental risk tolerance and not the result of correlations between risk tolerance measures and other unobserved differences.

There are gaps in achievement between children that open up early in childhood (Cunha and Heckman (2007)). These gaps are persistent, relatively large, and not fully understood. Yet as of eighth grade, test score gaps can explain a substantial portion of the wage differential (Neal and Johnson (1996)). While these gaps may be in part related to genetic factors, they are also likely related to differences in investing behavior between parents. Risk tolerance is largely uncorrelated with other parental characteristics, yet is strongly related to investment patterns. The strong effects of risk tolerance on the level of early childhood time investment in education time in particular suggest that risk and uncertainty might play a significant role in explaining some of the gaps in abilities and skills between children that open up in early childhood. This suggests that risk tolerance could explain some differences in child achievement.

# Chapter 3

# Asymmetric Information and Worker Productivity

## 3.1 Introduction

The information that employers have about employees in the labor market could be of first order importance for wage determination. But the information sets of employers and workers are very hard to measure accurately. Gibbons and Katz (1991) measure the importance of one particular role for information in the labor market: the negative signal associated with a worker being laid off. Gibbons and Katz measure this signal (which they call the "lemons effect") by comparing the wage change of workers who are laid off to the wage change of similar workers whose plant was shut down. Conditional on workers' prior characteristics and reason for displacement, laid off workers suffer worse reemployment outcomes because of the negative signal.

Gibbons and Katz (1991) and numerous other papers which follow it imagine a world in which employers learn about a worker's uni-dimensional "ability," which is equally applicable to any potential job. But a range of recent work argues that rather than being specific to a single job at a single firm or generally applicable across any job, worker productivity might be specific to an industry (Neal (1995)), occupation (Kambourov and Manovskii (2009)), or the tasks performed in an occupation (Poletaev and Robinson (2008); Lazear (2009)). Many skills and abilities might reasonably be expected to have different importance in different situations. For example reading comprehension might be most important for a lawyer, still very important for a business analyst, and least important for a short order cook. If incumbent employers learn about their employees' productivity, as in Gibbons and Katz (1991), then it is an interesting question whether this information pertains to task specific or to general productivity.

I explore this issue by examining how the lemons effect varies with the similarity of the tasks performed in the previous and current jobs. I use measures of task similarity between occupations from Autor (2013) to classify the task overlap between workers' pre- and post-displacement jobs. I show that the measured lemons effect is larger in magnitude for workers who perform different tasks in their new jobs. This pattern provides new, if suggestive, evidence on the nature of asymmetric information in labor markets.

Next, I show that what Gibbons and Katz term the lemons effect is in fact, in the form that they consider, driven by a recall bias which is correlated with wage loss. Workers who were laid off are more likely than workers displaced by plant closing to forget their job loss. And the measured lemons effect is largest for those workers reporting displacement longest ago. This suggests that the method of comparing the reemployment wages of laid off and exogenously displaced workers does not give consistent point estimates of the population conditional average difference in reemployment wages between laid off and exogenously displaced workers i.e. (the true "lemons effect").

The rest of this chapter proceeds as follows. Section 3.2 introduces the CPS

Displaced Workers Supplement data and presents the evidence that the lemons effect is larger among workers who find reemployment in jobs requiring a different set of skills. In Section 3.3, I show that the measured level of the lemons effect is due to recall bias. Subsection 3.3.4 reconciles these two pieces of evidence, and Section 3.4 concludes.

## 3.2 The "Lemons Effect" and Task Specificity in the Labor Market

### 3.2.1 The CPS Displaced Workers Supplement

In this chapter, I use the CPS Displaced Worker Supplement (DWS) from the years 1996-2008. The DWS is a supplement to the monthly CPS, fielded every two years, that asks workers who have been displaced from a job in any of the past three years a series of extra questions about their displacement and their previous employment. Thus, I observe workers who were displaced in the years 1993-2007. The data include information on displaced workers' previous employment and, crucially, their reason for displacement. Following Gibbons and Katz (1991), I restrict my sample to male workers aged 21-60 who are displaced from private, full-time, non-union, non-farm employment outside the construction industry and are currently reemployed full time at a wage of at least $75 (2006 dollars) per week.[1] Also following the literature, I consider workers who report displacement because their "plant or company closed down" to be the comparison group and workers who report displacement due to "insufficient work" or "position or shift abolished" to be laid off.

[1]Gibbons and Katz (1991) use a wage cutoff of $40, which is roughly the equivalent in 1986 dollars.

Note that following this literature, all workers in my sample *were displaced*.

Simple summary statistics for the CPS DWS data are reported in Table 3.1. Displaced workers on average find reemployment at wages that are nearly 7% lower. This is consistent with work by Jacobson, LaLonde, and Sullivan (1993) and von Wachter, Song, and Manchester (2009) which show large and persistent wage losses for displaced workers.[2] This wage loss is also skewed, since the median wage loss for displaced workers is only 2%. Displaced workers have an average of nearly 5 years of tenure in this sample, and a median of 2.5 years of tenure. Displaced workers have lower tenure than males as a whole, whose median tenure has ranged from 3.9 to 4.7 between 2002 and 2012 (Bureau of Labor Statistics (2012)). The average and median education of displaced workers are both about 14 years.

Table 3.2 presents sample averages broken out by the reason for workers' displacement. The standard error of the mean is presented in parentheses below each mean. Workers who are laid off were much less likely to be notified of their displacement: 26% compared to 49%. Laid off workers were modestly more educated. They also had lower tenure by 5.5 to 4.2 years on average. As discussed later, there is evidence of variation in the extent of recall bias by reason for displacement in this table. Workers displaced by plant closing report their layoffs longer ago (2.0

[2]Jacobson, LaLonde, and Sullivan and von Wachter, Song, and Manchester find substantially larger earnings losses (of about 25 to 30%) and document that these losses are persistent. These point estimates are not directly comparable to those from the DWS for at least three reasons. First, they analyze "mass-layoffs," where firms with at least 50 employees lay off at least 30% of their work force. These workers likely experience displacement in a worse local economy than workers in the DWS. DWS workers who are laid off or who experienced the closing of a small plant might be located in relatively healthy local economies. Second, the DWS measures the average weekly wage upon reemployment, while the work on mass-layoffs uses measures of quarterly earnings. To the extent that part of the effect of a displacement comes on the hours margin, this would only be reflected in the wage losses in the mass-layoffs framework.

Table 3.1: Summary of CPS Displaced Worker Supplements from 1996-2008

| | Mean | St. Dev. | Median | Min | Max |
|---|---|---|---|---|---|
| Log(Previous Wage) | 6.63 | 0.63 | 6.59 | 4.787 | 8.199 |
| Log(Current Wage) | 6.56 | 0.63 | 6.51 | 4.523 | 8.149 |
| Log Wage Change | -0.067 | 0.45 | -0.023 | -2.725 | 2.331 |
| Notice of Displacement | 0.35 | 0.48 | 0 | 0 | 1 |
| Non-White | 0.12 | 0.32 | 0 | 0 | 1 |
| Married | 0.65 | 0.48 | 1 | 0 | 1 |
| Education (yr.) | 13.9 | 2.41 | 14 | 0 | 20 |
| Pot. Exp. at Displacement (yr.) | 17.2 | 10.1 | 17 | 0 | 45.50 |
| Tenure at Displacement (yr.) | 4.75 | 5.95 | 2.50 | 0 | 38 |
| Years Since Displacement | 1.94 | 0.82 | 2 | 1 | 3 |
| Skill Overlap | 0.60 | 0.51 | 0.82 | -0.943 | 1 |
| Observations | 4501 | | | | |

*Note:* This table presents summary statistics of the sample of male workers in the 1996-2008 CPS Displaced Workers Supplement as defined in the text.

compared to 1.9) years ago on average.

In order to show that my sample is similar to the earlier DWS, I reproduce Gibbons and Katz (1991)'s main results for my sample and compare my results to those in their paper in Table 3.3. In this table, the previous, current, and change in log wages are regressed on a set of controls for marital status, race, region, previous occupation and industry, schooling, years since displacement dummies, a quadratic in potential experience and a cubic spline in previous tenure with breaks at 1, 2, 3, and 6 years. The coefficients presented in the table are the coefficient on the layoff dummy in regressions where the sample group (the whole sample or only white or blue collar workers) changes in the rows and the outcome variable (log of previous, current, or relative changes in wages) changes in columns. In their sample, Gibbons and Katz (1991) find an effect of 4.0% for the whole sample and 5.5% among white collar workers on wage changes. By comparison, I find an effect of 2.5% for the

Table 3.2: Summary of CPS Displaced Worker Supplements by reason for displacement

| | Plant Closing | Layoff | Total |
|---|---|---|---|
| Log(Previous Wage) | 6.612 | 6.646 | 6.632 |
| | (0.0146) | (0.0122) | (0.00935) |
| Log(Current Wage) | 6.549 | 6.575 | 6.565 |
| | (0.0147) | (0.0121) | (0.00934) |
| Log Wage Change | -0.0630 | -0.0705 | -0.0675 |
| | (0.0100) | (0.00888) | (0.00667) |
| Notice of Displacement | 0.485 | 0.260 | 0.351 |
| | (0.0117) | (0.00846) | (0.00711) |
| Non-White | 0.123 | 0.115 | 0.118 |
| | (0.00771) | (0.00617) | (0.00482) |
| Married | 0.661 | 0.635 | 0.645 |
| | (0.0111) | (0.00929) | (0.00713) |
| Education (yr.) | 13.63 | 14.01 | 13.86 |
| | (0.0575) | (0.0458) | (0.0359) |
| Pot. Exp. at Displacement (yr.) | 17.51 | 17.03 | 17.22 |
| | (0.238) | (0.193) | (0.150) |
| Tenure at Displacement (yr.) | 5.517 | 4.225 | 4.746 |
| | (0.151) | (0.107) | (0.0887) |
| Years Since Displacement | 2.004 | 1.891 | 1.936 |
| | (0.0194) | (0.0156) | (0.0122) |
| Skill Overlap | 0.613 | 0.588 | 0.598 |
| | (0.0134) | (0.0108) | (0.00839) |
| Observations | 1815 | 2686 | 4501 |

*Note:* This table presents averages of the characteristics of male workers in the CPS Displaced Workers Supplement as defined in Gibbons and Katz (1991). The sample is the Displaced Workers Supplements from 1996-2008. Standard errors of the estimate of the mean are presented in parenthesis below each average.

whole sample and 4.1% among white collar workers. Most of my coefficients are within sampling error of their counterparts from Gibbons and Katz (1991)'s paper.

### 3.2.2 Task Specificity of Human Capital: Recent Work

In seminal work, Becker (1962) argued that there are two types of human capital: human capital which is "general" and can be applied to any job, and human capital which is "specific" and useful only when applied to a specific job. More recent work has refined this concept significantly. Neal (1995) showed that workers who were displaced from their jobs had much more favorable job outcomes if they were able to find new employment in the same industry. This suggests that human capital is not just specific to a firm, but is specific to all firms in the same industry. Kambourov and Manovskii (2009) show that experience in the same occupation is relatively more important for explaining wage profiles than overall tenure or tenure in the same industry.

Lazear (2009) refines this idea further, arguing that rather than being generalizable between jobs or specific to a particular job, human capital consists of "skills," which represent the effectiveness with which a worker performs "tasks," or aspects of a particular job. Different jobs require different combinations of tasks, so workers' productivity at a particular job is determined by their productivity in the particular combination of tasks required at the job. This approach yields strong predictions for wage changes as workers progress through their careers. When workers change jobs, their wage at a new job depends on their productivity at the bundle of tasks

Table 3.3: The wage effect of a layoff compared to a plant closing in two different samples

(a) 1996-2008 DWS Sample

| | Current Sample | | |
|---|---|---|---|
| | Previous | Current | Change |
| Whole Sample | 0.038* | 0.013 | -0.025 |
| | (0.015) | (0.016) | (0.014) |
| White Collar | 0.031 | -0.014 | -0.045* |
| | (0.021) | (0.022) | (0.018) |
| Blue Collar | 0.028 | 0.038 | 0.010 |
| | (0.021) | (0.021) | (0.021) |

(b) 1984 and 1986 DWS Sample

| | Gibbons and Katz | | |
|---|---|---|---|
| | Previous | Current | Change |
| Whole Sample | 0.017 | -0.021 | -0.040** |
| | (0.014) | (0.017) | (0.017) |
| White Collar | -0.0094 | -0.064* | -0.055* |
| | (0.024) | (0.029) | (0.028) |
| Blue Collar | 0.022 | 0.0023 | -0.024 |
| | (0.017) | (0.021) | (0.022) |

*Note:* All regressions include controls for marital status, race, region, previous occupation and industry, schooling, a quadratic in potential experience and a cubic spline in previous tenure with breaks at 1, 2, 3, and 6 years. Regressions of wage change and current wage include dummies for years since displacement. These sample in these tables includes workers who are aged 21-60, were displaced from private, full-time, non-union, non-farm employment outside the construction industry and are currently reemployed full time at a wage of at least \$75 (2006 dollars) per week. Panel (a) is from the author's calculations for the 1996-2008 DWS samples, while Panel (b) is reproduced from Table 3 of Gibbons and Katz (1991).
*Standard errors in parentheses:* $+\,p < 0.10$, $^{*}\,p < 0.05$, $^{**}\,p < 0.01$, $^{***}\,p < 0.001$

at the new job. Their wages will be lower than their wages at the old job to the extent that each job requires different tasks.

Several relatively recent papers operationalize the Lazear approach and find that the task-specificity of human capital is a useful concept for explaining patterns of job change and wage growth in the labor market. Gathmann and Schönberg (2010) measure the task inputs to particular occupations using the German Qualification and Career Survey. This survey asks workers to report the degree to which each of a variety of tasks is used in their job. Gathmann and Schönberg calculate the overlap between two jobs based on the overlap between the vector of task weights for each job. They find that workers typically move to jobs requiring similar skills, and that workers get paid more when they have previous tenure in jobs that required similar skills. They also show that workers are more likely to make more drastic career changes earlier in their careers, before they have accumulated substantial skill at the tasks involved in their job. Poletaev and Robinson (2008) perform a related analysis in the United States. Because there are no large nationally representative data sources with worker-level information on tasks performed in jobs in the United States, Poletaev and Robinson use information on the tasks performed in occupations from from the Dictionary of Occupational Titles. They find that workers who are reemployed in occupations that have different principal task requirements have worse reemployment outcomes.

To the extent workers face a wage cost of changing tasks, those who are most able to avoid this cost will be the most likely to do so. The characteristics of workers who change tasks are relatively unexplored. While it is possible that selection on unobservables related to general productivity is able to explain part of the relationship between task switching and wage loss, this story alone cannot be the only story. With only general and firm specific human capital, the only reason that a worker might would choose to perform particular tasks in a new job would be related to preferences. Wage differentials related to productivity might incentivize the most unobservably productive workers to take jobs performing the same tasks, generating selection which augments the initial wage difference.

### 3.2.3 Employer Learning, Tasks, and the "Lemons Effect"

Gibbons and Katz (1991) argue that if firms have private information on the productivity of individual workers, they will chose to lay off those with the lowest productivity in their jobs. To the extent that outside firms are uncertain about the workers' productivity, they will interpret the layoff as a negative signal about the worker's quality. Gibbons and Katz show that workers who are laid off in the CPS DWS are reemployed at lower wages than their peers who are displaced by a plant closing. To the extent that all workers involved in a plant closing lose their job at the same time, there is no negative signal about the quality of a particular worker. Thus Gibbons and Katz argue that the difference in reemployment wages between workers who were laid off and whose plant closed down is the informational effect of a layoff.

But this model of employer learning has a very specific conception of what employers do not know about employees–employers learn about workers' unobserved one dimensional ability. In light of the work on the task specificity of human capital (discussed in the previous section), it is interesting to explore the meaning of this signal for employers. Are employers learning about a simple measure of employee "ability" which augments productivity in every task? Or are they learning about employee productivity in the range of tasks performed in the previous job?

The lemons effect could conceivably be either a signal about a workers' productivity in any job or a signal about workers' productivity at the tasks performed in the current job. In the latter case, the signal of a layoff should have a different impact on wages depending on the tasks performed at another job. In the case of specific skills, for example, firms might learn how consistent and fast workers are in routine, manual tasks by observing worker productivity over a long period of time (e.g. Mas and Moretti (2009)), or how adaptive, resourceful, and cheerful an employee is in non-routine interpersonal interactions with clients. Employers alternatively might learn about worker's problem solving ability, resourcefulness, or work ethic, which are characteristics that would likely prove useful in any job. Realistically, the signal which the labor market observes might be about some combination of these two types of skills. The differential impact of a layoff versus a plant closing on reemployment wages should then depend on the relative importance of the signal for skills which are either general or task specific as well as the tasks performed at the old and new job.

In the next few paragraphs, building on Greenwald (1986), I lay out a framework for thinking about how the measured lemons effect should depend on the task similarity of the previous and current job. From this, I draw predictions as to how the difference in reemployment wages between workers displaced by layoffs and plant closings should depend on the similarity of tasks performed at the new job and the old job and whether the information contained in a layoff is about general or task-specific productivity. [3]

In the Greenwald model, incumbent employers know workers' individual productivity and offer wages equal to their marginal products. All workers receive an exogenous shock to their utility from staying at the same job. Thus the workers who leave are those who have some combination of a dislike of their current job and those who are low ability and so receive low wage offers. Outside employers know that the workers who change jobs are those who received low wage offers from their previous employers because the previous employer knew that they were unproductive. But outside employers do not know workers' individual productivities, and only observe whether or not each worker was laid off. Since all laid off workers appear identical to the outside firm, each laid off employee receives the same wage offer. Since this offer is equal to the average marginal productivity of movers, it is below the average offer level of stayers.

Now consider a simple case where there are only two types of outside employers. One outside employer requires exactly the same set of tasks as the incumbent employer. The other requires completely different tasks. How does the lemons effect depend on the task similarity in the previous and current job? First consider the situation where the lemons effect pertains to general productivity in any job, so a

[3]While Greenwald (1986) develops a formal model, I discuss only the intuition behind extending this model. I leave a formal extension for future work.

layoff means that the worker will be less productive in any job, on average, than a worker displaced by plant closing. The lemons effect here should not depend on the tasks required.[4] Because the lemons effect speaks to productivity in any job, laid off workers will receive wage offers which are lower than their exogenously displaced counterparts, no matter what tasks are performed. This makes sense if being laid off from a job suggests that a worker lacks motivation, and so would be less productive in any other job, no matter what tasks he performed. Here, the magnitude of the lemons effect will not depend on the similarity of task content between jobs.

The other case to consider is where the information effect of layoffs pertains only to the specific set of tasks performed at the previous job. If workers choose jobs which require completely different tasks, then whether a worker was laid off is irrelevant for wages. Employers hiring for different tasks *know* that the worker displaced by layoff was less productive at the old job compared to her colleague displaced by plant closing. But since the new job requires the performance of completely different tasks, this has no effect on wages. By contrast, if displaced workers choose to take jobs utilizing similar tasks, then the layoff is again a meaningful signal. Here, the lemons effect will be larger in magnitude when workers use similar tasks in their next job than when they use different tasks.

This simple discussion suggests that different underlying mechanisms for the lemons effect have very different empirical implications for how the lemons effect

[4]This requires some assumption about why workers might choose to take each type of outside jobs. I assume that each job will pay the expected marginal product to workers, conditional on their reason for displacement. Workers also have some preference for performing similar or different tasks, so that workers consider aspects other than wages when deciding on which potential job to take.

should depend on the degree of task overlap.[5] If the lemons effect is bigger for workers taking jobs requiring more similar tasks, this is a sign of learning about specific tasks. If the lemons effect is constant across the types of tasks employed, then this can be interpreted as evidence of symmetrical learning about general skills which contribute equally to productivity in any job. The relative importance of learning about specific and general productivity is an empirical question, which I address in the work that follows.

### 3.2.4 Measuring Task Requirements in Occupations

To measure the skill content of occupations, I follow Autor (2013) and Acemoglu and Autor (2011). Acemoglu and Autor consolidate the very large number of aspects of occupations contained in the Occupational Information Network's (O*NET) Work Activities and Work Context Importance scales to only 6 different dimensions of tasks required.[6] They then map these skill requirements to both the Census 2000 and Standard Occupational Classifications. For each occupation, there is a measure of the extent to which jobs require the performance of both routine and non-routine cognitive, manual, and interpersonal tasks, for a total of 6 different

[5] In future work, a formal foundation can be laid for this intuitive discussion.

[6] Autor (2013) breaks tasks down into six categories: non-routine cognitive analytic, non-routine cognitive interpersonal, routine cognitive, routine manual, non-routine manual physical, and non-routine manual interpersonal. Non-routine cognitive analytical tasks include things like creative thinking and interpreting information for others. Non-routine cognitive interpersonal tasks include guiding and motivating others and establishing relationships. Routine cognitive tasks consist of things where there is a high importance of repeating the same task and being exact or accurate. Routine manual tasks are those where workers control machines and spend time making repetitive motions. Non-routine manual physical tasks require a greater degree of physical ability, and include operating vehicles, using tools with one's hands, and spatial orientation. Non-routine manual interpersonal tasks include assisting and caring for others.

dimensions of task intensity.[7] These measures of each dimension of task intensity in each occupation are then standardized to be zero mean and unit variance. I merge these measures of task intensity at the 3 digit occupation level into the CPS DWS sample discussed in Section 3.2.1.

Table 3.4 presents sample averages of skill inputs as well as other variables separately for white and blue collar pre-displacement jobs. The differences between workers of different collars are much as expected. The average blue collar job has 0.25 standard deviations less need for non-routine cognitive skills than the average job. By contrast, the average white collar job has 0.76 standard deviations more need for these skills. This is consistent with the understanding of white collar jobs as requiring a greater degree of intellectual problem solving. White collar jobs also require significantly more non-routine interpersonal tasks, which include things like teaching and explaining concepts to others. While both white and blue collar jobs require similar amounts of routine cognitive inputs, blue collar jobs require much more routine manual as well as non-routine physical tasks. This is very much as expected: blue collar jobs inherently require physical tasks. White collar jobs require more non-routine manual interpersonal tasks.

The overlap between jobs is defined as a simple normalized dot product of the task vectors required at each job. So if the task requirement of the initial job is $\vec{v}_0$, and the task requirement of the current job is $\vec{v}_1$, then the overlap in skill requirements is given by:

[7] It is worth mentioning that there is still considerable heterogeneity which remains within these task definitions. For example, while both lawyers and engineers have jobs intensive in non-routine cognitive analytic tasks, it is likely that transitioning between these two occupations would result in considerable human capital loss.

Table 3.4: Summary of skill measures by worker collar in the CPS Displaced Worker Supplement

| | Blue Collar | White Collar | Total |
|---|---|---|---|
| Log(Previous Wage) | 6.326 | 6.926 | 6.675 |
| | (0.0126) | (0.0131) | (0.0105) |
| Log Wage Change | -0.0541 | -0.0901 | -0.0751 |
| | (0.0114) | (0.00952) | (0.00730) |
| Education (yr.) | 12.41 | 15.05 | 13.95 |
| | (0.0531) | (0.0452) | (0.0406) |
| Pot. Exp. at Displacement (yr.) | 17.51 | 17.37 | 17.43 |
| | (0.274) | (0.209) | (0.167) |
| Tenure at Displacement (yr.) | 4.161 | 5.238 | 4.788 |
| | (0.142) | (0.136) | (0.0991) |
| Years Since Displacement | 1.898 | 1.983 | 1.947 |
| | (0.0211) | (0.0176) | (0.0135) |
| Skill Overlap | 0.621 | 0.582 | 0.598 |
| | (0.0126) | (0.0112) | (0.00839) |
| Non-Routine Cognitive Analytic | -0.248 | 0.755 | 0.336 |
| | (0.0187) | (0.0180) | (0.0154) |
| Non-Routine Interpersonal | -0.370 | 0.468 | 0.118 |
| | (0.0217) | (0.0224) | (0.0173) |
| Routine Cognitive | 0.0819 | -0.0630 | -0.00244 |
| | (0.0196) | (0.0180) | (0.0133) |
| Routine Manual | 0.790 | -0.484 | 0.0484 |
| | (0.0231) | (0.0174) | (0.0174) |
| Non-Routine Manual Physical | 0.922 | -0.468 | 0.113 |
| | (0.0252) | (0.0184) | (0.0189) |
| Non-Routine Manual Interpersonal | -0.660 | 0.0739 | -0.233 |
| | (0.0204) | (0.0181) | (0.0149) |
| Observations | 1515 | 2111 | 3626 |

*Note:* This table presents summary statistics by "collar" of employment in a worker's pre-displacement job, as defined in Gibbons and Katz (1991) for the CPS Displaced Workers Supplement from 1996-2008. Standard errors of the mean are reported in parentheses. The measures of skill content come from the files that are publicly available at `http://economics.mit.edu/faculty/dautor/data/acemoglu`.

$$\text{Skill Overlap} = \frac{\vec{v}_0 \cdot \vec{v}_1}{\left(\|\vec{v}_0\|\|\vec{v}_1\|\right)^{1/2}} \tag{3.1}$$

Gathmann and Schönberg use a different measure of task intensity based on how often workers perform a given task in their job. Thus their overlap measure is strictly positive and has a more natural interpretation as the extent to which the bundle of skills required is similar between the two jobs–this value can be between 0 (indicating no overlap whatsoever) and 1 (indicating complete overlap). By contrast, my measure expresses only a relative amount of overlap and runs from $-1$ to 1.

It is also worth comparing this measure to that of Poletaev and Robinson (2008). They use the same underlying Dictionary of Occupational Titles information as Autor. But instead of making an argument about how to construct the relevant low-dimensional skill vector, they use factor analysis to let the data determine a low-dimensional vector to use. They use two definitions of thresholds for "switching" skill requirements of jobs based on whether the new job uses the same principal skill component most intensively or not and whether or not there was a big change in the magnitude of the importance of the principal component. To the extent that the work of Autor, Murnane, and Levy (2003) was successful in identifying the key components of tasks performed at jobs, the two approaches are likely very similar.

Note that the measure of overlap between pre- and post-displacement jobs is relatively similar for white and blue collar workers. The summary statistics in Table 3.1 show that the sample average overlap in tasks is reasonably high, at 0.60. But some workers do take jobs with completely (or nearly completely) opposite task intensities, as the sample minimum is close to $-1$. Of course, any worker who does not change 3 digit occupations will by definition have a task overlap measure of 1.

Table 3.5 presents summary statistics by whether a worker found reemployment in a job requiring a mix of tasks with greater or less than the median degree of overlap. Workers who find a new job that has low task overlap with their previous job appear to have wage changes upon reemployment that are (statistically significantly) about 6% worse than those who find reemployment in a new job with high task overlap. Other than this wage difference, workers who have high task overlap in their jobs pre- and post-displacement seem quite similar to those with low task overlap. They appear to be modestly positively selected on characteristics related to wages, just on the edge of statistical significance. For example, high overlap workers are about three percentage points more likely to be married, and have about 0.1 additional years of education.

Notice that the degree of task overlap does not seem to be strongly related to either the probability of being laid off or the time since workers report displacement. In light of the evidence discussed below in Section 3.3 this is important. Workers who are laid off are more likely to forget displacement than those displaced by plant closing. And this forgetting appears to drive the measured lemons effect. Fortunately, the task overlap between workers' previous and current jobs does not seem to be strongly correlated with when workers were displaced or their reason for displacement.[8]

[8] A simple linear probability model regressing the probability of being laid off on the degree of

Table 3.5: Summary of worker characteristics by task overlap between pre- and post-displacement jobs

| | Low Skill Overlap | High Skill Overlap | Total |
|---|---|---|---|
| Log(Previous Wage) | 6.607 | 6.743 | 6.675 |
| | (0.0145) | (0.0149) | (0.0105) |
| Log(Current Wage) | 6.501 | 6.699 | 6.600 |
| | (0.0146) | (0.0147) | (0.0105) |
| Log Wage Change | -0.106 | -0.0439 | -0.0751 |
| | (0.0107) | (0.00992) | (0.00730) |
| Laid Off | 0.617 | 0.582 | 0.600 |
| | (0.0114) | (0.0116) | (0.00814) |
| Notice of Displacement | 0.344 | 0.374 | 0.359 |
| | (0.0112) | (0.0114) | (0.00797) |
| Non-White | 0.132 | 0.119 | 0.125 |
| | (0.00794) | (0.00761) | (0.00550) |
| Married | 0.632 | 0.660 | 0.646 |
| | (0.0113) | (0.0111) | (0.00794) |
| Education (yr.) | 13.88 | 14.02 | 13.95 |
| | (0.0534) | (0.0613) | (0.0406) |
| Pot. Exp. at Displacement (yr.) | 16.74 | 18.12 | 17.43 |
| | (0.239) | (0.233) | (0.167) |
| Tenure at Displacement (yr.) | 4.774 | 4.802 | 4.788 |
| | (0.142) | (0.138) | (0.0991) |
| Years Since Displacement | 1.974 | 1.921 | 1.947 |
| | (0.0189) | (0.0194) | (0.0135) |
| Skill Overlap | 0.225 | 0.972 | 0.598 |
| | (0.0112) | (0.00123) | (0.00839) |
| Observations | 1814 | 1812 | 3626 |

*Note:* This table presents summary statistics by the degree of overlap in task requirements between a displaced worker's pre- and post-displacement jobs, as defined by equation (3.1). Standard errors of the mean are reported in parentheses.

### 3.2.5 The Lemons Effect and Task Overlap

Table 3.6 presents the results of estimating the standard lemons effect equation from Gibbons and Katz while allowing the effect to vary by the measure of task overlap discussed in Section 3.2.4. This estimating equation is presented in Equation 3.2.

$$\Delta \log w = \beta_0 \text{Layoff} + \beta_1 \text{Layoff* Skill Overlap} + \beta_2 \text{Skill Overlap} + \gamma X + \varepsilon \quad (3.2)$$

The coefficient on Layoff in the first row of Table 3.6 is $-0.05$ and represents the lemons effect when a worker is reemployed in a job consisting of tasks that are perfectly orthogonal to those of the previous job. The coefficient indicates that laid off workers who find a new job requiring orthogonal tasks will earn 5% lower wages than a similar worker displaced by plant closing making the same transition. This coefficient is large compared to the analogous estimate of the main lemons effect estimates in Table 3.3a of $-0.02$. If workers are reemployed with perfect overlap, the lemons effect is then given by the sum of the coefficients in the first two rows, which is very close to zero. Indeed, the $p$-value from a test that these two coefficients sum to zero is reported in the table, and fails to reject that hypothesis. That there is a positive, significant coefficient on Skill Overlap is intuitive. Decreasing the task overlap between a worker's old and new job through the entire range from 1 to $-1$

overlap has an insignificant coefficient suggesting that workers finding reemployment with complete overlap were about two percentage points less likely to be laid off than those finding reemployment in a job using perfectly orthogonal tasks.

Table 3.6: The effect of a layoff on reemployment wages and degree of task overlap

| | $\Delta$ log($w$ ) |
|---|---|
| Layoff | -0.052* |
| | (0.024) |
| Layoff * Overlap | 0.061* |
| | (0.030) |
| Overlap | 0.049* |
| | (0.024) |
| $p$(Layoff + Layoff * Overlap = 0) | 0.592 |
| N | 3626 |

*Note:* This table presents results of a regression of wage changes on an indicator of whether a worker was laid off interacted with the distance in task overlap between the worker's previous and current occupation of employment. The regression includes controls for marital status, race, region, previous occupation and industry, schooling, years since displacement, a quadratic in potential experience and a cubic spline in previous tenure with breaks at 1, 2, 3, and 6 years. They use data from the 1996-2008 CPS DWS, considering only workers who were displaced from full-time, private sector, non-agricultural, non-self employed jobs paying at least $75 2006 dollars per week and are reemployed in similar jobs.

would result in 10% lower reemployment wages.[9] This is consistent with the work of Neal (1995), Kambourov and Manovskii (2009), Poletaev and Robinson (2008) and others which find that reemployment wages are higher for workers who find jobs more similar to their old jobs, based on whether reemployment is in the same industry or occupation.

The estimated coefficient on the interaction between layoff and task overlap is

[9]This estimate comes from multiplying the point estimate 0.049 by the change in overlap given by $1 - (-1) = 2$.

the main parameter of interest, and the fact that it is positive is quite an interesting result. The estimated coefficient suggests that the lemons effect is more important when workers end up in jobs using different tasks than when they end up in jobs using similar tasks. This is not consistent with the scenario, discussed above, where the layoff is a signal about general productivity. In that situation, we would expect the coefficient to be zero. It is also inconsistent with the scenario where the layoff is a signal about task-specific productivity. In that situation, a lemons effect would not exist when there is no task overlap. But the lemons effect would exist when workers find reemployment performing similar tasks. This would suggest a *negative* coefficient on the interaction term. This presents a puzzle for the simple framework discussed above. Future theoretical work might explain this result. A particularly promising direction might include introducing an informational asymmetry between firms who hire workers utilizing similar and different tasks. An informational advantage might help to explain why firms who reemploy workers using similar tasks pay more: they are better able to tell which workers are lemons. Thus they pay the keep the best lemons and pay them more, while the most severe lemons end up taking jobs using different tasks and receive lower wages.

This empirical analysis is thought-provoking in that it sheds some light on the way that employers learn asymmetrically about specific versus general employee productivity in the labor market. But some caution should be used in drawing conclusions. One reason among many for this caution is that it is possible that recall bias is playing a role in the measured lemons effect. In the next section, I turn to the role of recall bias in the estimation of the lemons effect more broadly.

## 3.3 The Role of Recall Bias in Estimating the "Lemons Effect"

Recall bias, in which a survey respondent answers questions erroneously due either to forgetfulness or cognitive bias, can be crucially important in any study that relies on retrospective survey data. The seriousness of the problem depends on the length of respondent recall periods and on the correlation between forgetting and outcomes of interest. In the case of the CPS DWS, respondents are asked about displacement events that occurred up to five years before the survey date in Supplements fielded earlier than 1996, and up to three years before the survey date in Supplements fielded 1996 or later.

At present, I am interested in measuring the "lemons effect," or the conditional difference in log wage changes between workers who are displaced due to layoffs compared to those displaced due to plant closings. Forgetting is likely to matter here since whether workers remember being displaced is likely related to their pre- and post- displacement wages. The potential problem is that laid off workers who subsequently find relatively well-paying jobs are more likely to forget displacement (and not be in the sample) than are workers who find relatively low-paying jobs. Because workers who are laid off with good outcomes are missing from the sample, the measured lemons effect will be biased towards more negative values. This story is originally due to Robert Topel and mentioned in Gibbons and Katz (1991). It is important to keep in mind that this story is for workers displaced by layoffs relative to plant closings. The implicit assumption here is that workers displaced by plant closings are more likely to have relatively constant memories over time. This is reasonable if a plant closing is a big event for the entire community and not just for the lone displaced worker.

I examine the role of recall bias in the DWS sample years 1996-2008, and find that it plays an important role. I tabulate the number of workers reporting displacement according to the time since their reported displacement and their reason for displacement. This tabulation shows that workers who were laid off appear to forget displacements much more quickly than workers whose plants closed. While this initially appears to be less true among high tenure workers, I show that after attempting to account for selection bias by limiting the sample to workers who are reemployed quickly, high tenure workers also appear to forget displacement at similar rates to low tenure workers.

I check to see whether the lemons effect differs by the number of years since displacement took place. When I allow the lemons effect to vary by years since displacement, I find that the measured lemons effect is indistinguishable from zero for workers displaced one or two years ago, but is around 7% for those displaced three years ago. Allowing the lemons effect differ for high and low tenure workers, as Gibbons and Katz do, I find the same thing: there is no lemons effect for workers displaced one or two years ago, but the effect is 12% for those displaced three years ago.

### 3.3.1 Measurement of the "Lemons Effect" in Other Studies

In their classic paper, Gibbons and Katz (1991) develop a model which predicts that because of signalling effects, workers who are laid off will experience more adverse wage changes than workers whose company or plant closes. A literature has developed that attempts to measure this "lemons effect" in different countries and using different data. Among these papers are Doiron (1995) for Canada, Stevens (1997) using the PSID, Krashinsky (2002) using the NLSY, Grund (1999) for Germany, and Song (2007) using the 2000 and 2002 DWS.[10]

Because of its large sample size and representativeness, the DWS is a leading candidate to study the lemons effect (indeed, Gibbons and Katz used it in their seminal work), but because of the three year recall period, there is a risk of recall bias. Gibbons and Katz considered this possibility in their work, but concluded that it was unlikely for three reasons: recall bias 1) would not explain the larger effect for white compared to blue collar workers 2) would suggest, counter to their findings, that the lemons effect should grow with time since displacement and 3) would be less of a problem among high tenure workers, for whom they observed the biggest effect. Evans and Leighton (1995) study recall bias directly in the DWS using the overlapping nature of the 1984-1990 samples and find that workers forget displacements at a rate of 17.6% per year. Oyer (2004) attempts a validation of the DWS by asking the DWS survey instruments to workers displaced from a financial services firm and matching their responses the "truth" using payroll data. Though

[10]Gibbons and Katz (1991), Doiron (1995) and Song (2007) use retrospective supplements to cross-sectional surveys, while the other authors use panel data sets that find very few displacements but have more information about the careers of displaced workers.

he does not specifically address the rate of forgetting, he does find that workers recall their pre-displacement tenure and reason for displacement accurately but tend to overstate their earnings.

Song (2007) looks specifically at the question of whether recall bias drives the measured lemons effect in Gibbons and Katz (1991). When restricting the sample to those workers displaced in the last two years, he finds that the lemons effect is the same among white collar workers but grows among blue collar workers.[11] Thus, implicitly Song finds that blue collar workers are more likely to forget displacements that resulted in very bad wage changes, which is at odds with a recall bias story where workers forget displacements that result in mild wage changes. Song (2007) also finds that when the wage-tenure profile at the previous job is allowed to differ by the reason for which the worker was displaced, laid off workers have a steeper wage-tenure profile and the lemons effect disappears.

### 3.3.2 Recall Bias

I begin by demonstrating the existence of recall bias and presenting suggestive evidence that it might play a major role in the estimation of the lemons effect. In Table 3.7 I present simple tabulations of the number of workers reporting displacement one, two, or three years ago and meeting sample selection criteria by whether they were laid off or not. Workers displaced due to plant closings appear to report displacement at roughly equal rates in the last three years: 615, 578, and

[11]The problem with mitigating recall bias in this way is that using workers displaced recently accentuates the selection bias from only considering workers who have found new jobs. Indeed, as a robustness check, Gibbons and Katz consider only workers reporting displacement *longer than two years ago*.

Table 3.7: Displacements reported in 1996-2008 CPS DWS by time since displacement and reason for displacement

| Years Since Displacement | Plant Closing | Layoff | Total |
|---|---|---|---|
| 1 Year Ago | 615 | 1042 | 1657 |
| 2 Years Ago | 578 | 896 | 1474 |
| 3 Years Ago | 622 | 748 | 1370 |
| Total | 1815 | 2686 | 4501 |

*Note:* Tabulation of 1996-2008 DWS by Reported Time Since Displacement and Reason for Displacement. This table includes workers who are aged 21-60, were displaced from private, full-time, non-union, non-farm employment outside the construction industry and are currently reemployed full time at a wage of at least $75 (2006 dollars) per week.

622 workers report being displaced by plant closings one, two, and three years ago respectively. In comparison, laid off workers are more likely to be displaced last year (1042 workers) than three years ago (748 workers).[12] If the true number of workers displaced one and three years ago is actually similar, there are around 300 "missing" workers who were displaced three years ago but have forgotten about it: this is a very sizable portion of the sample. Therefore, Table 3.7 suggests that laid off workers are forgetting displacements at a greater rate than their counterparts

Though it seems that forgetting is taking place, this does not necessarily mean that recall bias is relevant for the estimation of the lemons effect. To show that

[12]Fitting a simple exponential model to the count data with controls for years since displacement, whether a worker was laid off or displaced by plant closing, and the interaction between the two validates this pattern. Workers displaced by plant closing are 0.5% more likely to report a displacement each year since displacement, while laid off workers are 17% less likely to report displacement with each passing year. Assuming that the number of workers reporting displacement a given number of years ago follows a Poisson distribution also allows a direct test of the statistical significance of this relationship. Using the Poisson distribution, the expected value of both the mean and variance are given by the number of counts. Thus there were $1042 \pm 32$ workers reporting displacement last year and $748 \pm 27$ reporting displacement three years ago, a difference which is clearly statistically significant.

recall bias matters, I first see if the measured lemons effect varies by how long ago a worker reports displacement. Finding that the measured effect is higher for workers displaced longer ago is consistent with recall bias driving the results, but recall bias is not the only possible explanation. I consider other possible explanations in Section 3.3.3.

Table 3.8 presents results of regressions which allow the measured lemons effect to vary by the time since displacement. I find that the lemons effect is statistically different from zero only for workers displaced three years ago. In Column 1 of Table 3.8 I repeat the analysis in Table 3.3, but replace the layoff dummy with a set of layoff $\times$ years since displacement interaction dummies. The first column of Table 3.8 shows that laid off workers displaced last year have 2.4% worse wage changes than their counterparts whose plants closed while laid off workers displaced two years ago had 1.5% *better* wage changes. Neither of these estimates is statistically different from zero. By contrast, laid off workers displaced three years ago have 6.8% (and statistically significant) worse wage changes. I reject the joint equality of these three coefficients at the 1% level, but I fail to reject that the lemons effect for workers displaced one or two years ago is zero. The difference in coefficients between workers reporting displacement one and three years ago is particularly striking. Because of the biennial nature of the DWS, workers are reporting displacement in *nearly the same* years, yet the layoff coefficient is quite different.[13]

To attempt to control for recall bias, Gibbons and Katz as well as Song test

[13]Except for the first and last sample years (1993 and 2007), every odd year is both one and three years before a DWS.

whether the lemons effect is different for workers displaced more or less than two years ago using a Chow test. They fail to reject that the coefficients are different. Compared to Song in particular, I am able to reject the analogous measure of equality simply because I have more power. While Song uses only the 2000 and 2002 CPS DWS, I use five additional surveys. My point estimates are relatively similar, but I have smaller standard errors thanks to my larger sample. Thus, it appears that laid off workers workers' wage changes do differ depending on when a worker has reported displacement. While it is possible that recall bias has changed between the sample period in Gibbons and Katz and in this work, the rate of forgetting seems similar between the two periods.[14]

Looking at high tenure workers provides a starker test of recall bias for two reasons. First, Gibbons and Katz find that the lemons effect is larger in magnitude for high tenure workers, so that classical measurement error is relatively less important. Second, high tenure workers should be less susceptible to recall bias than their low tenure counterparts if longer term jobs are more memorable, so any finding of recall bias among these workers is less expected. Column 2 of Table 3.8 presents the layoff coefficients from a wage change regression where the coefficient on layoff is allowed to differ by years since displacement and whether workers had tenure above or below the sample median.[15] The results show a lemons effect three years ago of 12.7%, compared to one last year of 1.7%. Again, only the coefficient for workers displaced three years ago is significant, and I cannot reject the hypothesis

[14]See Evans and Leighton (1995).

[15]There are actually 6 layoff coefficients in this model: one for each years since displacement – tenure level interaction. The coefficients for low tenure workers are all relatively close to zero and are omitted.

Table 3.8: The effect of a layoff on reemployment wages by years since displacement

| | All | High Ten. | High Ten. Sample |
|---|---|---|---|
| Layoff * 1 Year Ago | -0.024 | -0.017 | -0.028 |
| | (0.022) | (0.028) | (0.030) |
| Layoff * 2 Year Ago | 0.015 | -0.006 | 0.014 |
| | (0.023) | (0.028) | (0.031) |
| Layoff * 3 Year Ago | -0.068** | -0.127*** | -0.143*** |
| | (0.025) | (0.029) | (0.031) |
| $p$-Value (Equality of 1, 2, and 3) | 0.045 | 0.003 | 0.001 |
| $p$-Value (Equality of 2 and 3) | 0.013 | 0.002 | 0.000 |
| $p$-Value (Equality of 1 and 3) | 0.171 | 0.005 | 0.007 |
| $p$-Value (1 and 2 Equal 0) | 0.444 | 0.823 | 0.577 |
| $R^2$ | 0.083 | 0.086 | 0.115 |
| N | 4501 | 4501 | 2266 |

*Note:* Estimates of a log wage change regression where the lemons effect is allowed to very by years since displacement. The first column reports results of these regressions for the sample. In the second column, the lemons effect is also allowed to vary by tenure level, but for brevity only high tenure coefficients are reported (all coefficients for low tenure workers are close to zero). The third column estimates the same equation as the first column except that the sample is restricted to only workers with tenure above the median. The controls are the same as those presented in Table 3.6.
*Standard errors in parentheses:* * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

that the lemons effect is zero for workers displaced one or two years ago. I can reject the equality of these coefficients at the 1% level. Column 3 reproduces Column 1 restricting the sample to only high tenure workers with very similar results.

### 3.3.3 Robustness of the Recall Bias Finding

An alternative explanation for the results in Table 3.8 is that workers displaced by plant closings either find stable jobs faster (and accumulate higher tenure and thus receive higher wages there) or are more effective at job search (and hence accumulate better match-specific capital) than their laid off counterparts. Thus

Table 3.9: The effect of a layoff on reemployment wages by years since displacement using additional controls

| | All | High Ten. | High Ten. Sample |
|---|---|---|---|
| Layoff * 1 Year Ago | -0.025 | -0.019 | -0.031 |
| | (0.022) | (0.028) | (0.030) |
| Layoff * 2 Year Ago | 0.017 | -0.005 | 0.016 |
| | (0.023) | (0.028) | (0.031) |
| Layoff * 3 Year Ago | -0.070** | -0.131*** | -0.146*** |
| | (0.025) | (0.029) | (0.031) |
| $p$-Value (Equality of 1, 2, and 3) | 0.031 | 0.002 | 0.001 |
| $p$-Value (Equality of 2 and 3) | 0.009 | 0.001 | 0.000 |
| $p$-Value (Equality of 1 and 3) | 0.164 | 0.004 | 0.007 |
| $p$-Value (1 and 2 Equal 0) | 0.370 | 0.784 | 0.516 |
| $R^2$ | 0.104 | 0.107 | 0.133 |
| N | 4501 | 4501 | 2266 |

*Note:* See notes to Table 3.6. In addition, these regressions include a cubic spline in tenure at a worker's current job with breaks at one, two, and three years. It also includes a set of dummies for the number of different jobs that a worker had since they were displaced.
*Standard errors in parentheses:* * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

differences in human capital between the two groups of workers would get bigger over time and the measured lemons effect would grow. This does not appear to be the case, however. In Table 3.9 I control for a cubic spline in tenure at the current job and the number of jobs held since displacement. The point estimates in this table are largely unchanged from Table 3.8

Another potential explanation is that while Table 3.7 (a tabulation of workers in the sample by the time since displacement and reason for displacement) suggests a role for recall bias among the entire population, high tenure workers are less susceptible to recall bias. No matter whether a worker has a good or bad reemployment outcome, one might expect that a worker will be more likely to remember a job which they held for longer. Yet while recall bias should be less important, the lemons effect certainly seems more prominent for high tenure workers, so the pattern of forgetting in Table 3.7 must still be prominent among high tenure workers. The analogue to Table 3.7, restricted just to workers with tenure above the median is reported in Panel 3.10a of Table 3.10. There appears to be almost no forgetting among high tenure workers: 435 report displacement one year ago compared to 395 three years ago. This suggests a rate of forgetting around 2% per year, much lower than the 17.6% reported in Evans and Leighton (1995). There is also no forgetting among workers displaced by plant closing, of whom 328 report displacement from long-held jobs one year ago and 380 report a similar displacement three years ago

In fact, this apparent uniformity of reports of displacement over time in Table 3.10a masks heterogeneity in recall bias with respect to time spent jobless. In particular, workers are very likely to remember any form of displacement which left them jobless for an extended period of time. This is likely one of the main non-wage determinants of the salience of a job loss. Therefore, no recall bias should be expected for workers who have no job for a year or more (for example). Whether these workers were laid off or displaced by a plant closing, they will likely remember this painful spell of joblessness with great fidelity. Therefore, to the extent that recall bias exists and differs by reason for displacement, this pattern should hold more strongly among workers who find reemployment relatively quickly. Table 3.10b repeats tabulations in Table 3.10a for high tenure workers with jobless spells less than 13 weeks. In Table 3.10b the differential forgetting pattern reemerges for laid off workers. There are 346 high tenure workers laid off one year ago compared to 235 three years ago.

Table 3.10: Displacements reported in 1996-2008 CPS DWS by time since displacement and reason for displacement for workers with at least 30 months of tenure

(a) High Tenure Workers

| Years Since Displacement | Plant Closing | Layoff | Total |
|---|---|---|---|
| 1 Year Ago | 328 | 435 | 763 |
| 2 Years Ago | 320 | 408 | 728 |
| 3 Years Ago | 380 | 395 | 775 |
| Total | 1028 | 1238 | 2266 |

(b) High Tenure Workers Reemployed within 13 Weeks

| Years Since Displacement | Plant Closing | Layoff | Total |
|---|---|---|---|
| 1 Year Ago | 290 | 346 | 636 |
| 2 Years Ago | 217 | 270 | 487 |
| 3 Years Ago | 255 | 235 | 490 |
| Total | 762 | 851 | 1613 |

*Note:* Tabulation of 1996-2008 DWS by Reported Time Since Displacement and Reason for Displacement. These tables include workers who are aged 21-60, were displaced from private, full-time, non-union, non-farm employment outside the construction industry and are currently reemployed full time at a wage of at least \$75 (2006 dollars) per week. Table 3.10a further restricts the sample to workers with tenure above the median (2.5 years), while Table 3.10b restricts the sample to high tenure workers who were reemployed within 13 weeks.

While it is possible to question whether a substantial number of workers might forget a job they held for more than 2.5 years, this is not necessarily the only margin along which recall bias might matter. It is also possible that the language asking about displacement with the language "did you lose a job" just does not resonate because the transition might not feel like a job loss in retrospect. A worker who was laid off from a long-held job but found another good job quickly might remember the details of the job but recall it as being a voluntary change rather than as being lost in response to this line of questioning.

Table 3.11: The effect of a layoff on reemployment wages among workers reemployed within 13 weeks

| | All | High Ten. | High Ten. Sample |
|---|---|---|---|
| Layoff * 1 Year Ago | -0.005 | -0.000 | 0.000 |
| | (0.023) | (0.030) | (0.032) |
| Layoff * 2 Year Ago | 0.028 | 0.027 | 0.039 |
| | (0.027) | (0.032) | (0.035) |
| Layoff * 3 Year Ago | -0.031 | -0.100** | -0.119** |
| | (0.029) | (0.034) | (0.037) |
| $p$-Value (Equality of 1, 2, and 3) | 0.332 | 0.016 | 0.006 |
| $p$-Value (Equality of 2 and 3) | 0.140 | 0.006 | 0.002 |
| $p$-Value (Equality of 1 and 3) | 0.479 | 0.024 | 0.015 |
| $p$-Value (1 and 2 Equal 0) | 0.585 | 0.712 | 0.532 |
| $R^2$ | 0.075 | 0.078 | 0.111 |
| N | 3379 | 3379 | 1613 |

*Note:* Estimates of a log wage change regression where the lemons effect is allowed to very by years since displacement with the sample restricted to workers reemployed at the survey date who found reemployment within 13 weeks of displacement. The first column reports results of these regressions for the sample. In the second column, the lemons effect is also allowed to vary by tenure level, but for brevity only high tenure coefficients are reported. The third column estimates the same equation as the first column except that the sample is restricted to only workers with tenure above the median. All regressions include controls for marital status, race, region, previous occupation and industry, schooling, years since displacement, a quadratic in potential experience and a cubic spline in previous tenure with breaks at 1, 2, 3, and 6 years. They use data from the 1996-2008 CPS DWS, considering only workers who were displaced from full-time, private sector, non-agricultural, non-self employed jobs paying at least $75 2006 dollars per week and are reemployed in similar jobs within 13 weeks of displacement.

*Standard errors in parentheses:* + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

So recall bias does appear to exist, even among high tenure workers. But for this to be an important driver of the lemons effect, the results presented in Section 3.3.2 and Table 3.8 should still apply even if the sample is limited to workers who found reemployment quickly. Table 3.11 reproduces the basic results in Table 3.8 while limiting the sample to workers reemployed within 13 weeks. Column 1 of Table 3.11 does not restrict the sample to high tenure workers, but presents the analogue of column 1 of Table 3.8 for workers reemployed quickly. The lemons effect here is no longer discernible from noise. Columns 2 and 3 look at the lemons effect among the high tenure sample. Here the pattern of recall bias driving the lemons effect is again obvious. The lemons effect for high tenure workers reemployed within 13 weeks and displaced three years ago is 10%, while the lemons effects are indistinguishable from zero for those reporting job loss one or two years ago.

Song argues that the measured lemons effect is due to differences in the wage-tenure profile between workers who are laid off and those whose plants close. Song's findings are not incompatible with my finding of recall bias. Song argues that laid off workers have "more to lose" because they have a higher wage-tenure profile. If laid off workers forget displacement from low wage jobs, those in the sample will have overwhelmingly high wage jobs. A recall-biased sample of laid off workers will appear to have "more to lose." At the same time, it would be difficult for a wage-tenure profile story to explain the difference in measured lemons effects by years since displacement. Any difference would have to be due to differences in tenure between workers displaced at different times, but workers displaced at different times appear to have relatively similar tenure characteristics.

A final critique of the results discussed here is that Gibbons and Katz (????) already considered recall bias explicitly in an unpublished version of their paper, so the recall bias that I find is due to differences between our samples. Compared to this unpublished work, I have more power because I have a larger sample size. While Gibbons and Katz might have had enough power to estimate the lemons effect, they might still lack the precision to see how the lemons effect varies by time since reported displacement. I also use seven sample years compared to their two, and have six overlapping sample years compared to three, so my estimates will be less likely to pick up effects that are specific to particular years of the samples.

### 3.3.4 Robustness of Task Specific Relationship to Recall Bias

The evidence from Section 3.3.2 suggests that recall bias has the potential to bias estimates of the lemons effect. If the lemons effect is biased, this makes it difficult to interpret the relationship between the lemons effect and task overlap discussed in Section 3.2. I present evidence that recall bias is not a driver of the relationship between the magnitude of the lemons effect and the similarity of tasks required at the reemployment job. While it is important to keep the threats to estimates from Section 3.3.2 in mind, the results in Table 3.6 provide some thought provoking suggestive evidence on the role of information in labor markets.

Table 3.12 shows the number of workers in the CPS DWS by their time since displacement and the degree of overlap between tasks in their previous and current occupations. While there is some evidence that workers are forgetting displacements

Table 3.12: The relationship between recall bias and task overlap

| Years Since Displacement | Low Skill Overlap | High Skill Overlap | Total |
|---|---|---|---|
| 1 Year Ago | 612 | 695 | 1307 |
| 2 Years Ago | 638 | 565 | 1203 |
| 3 Years Ago | 564 | 552 | 1116 |
| Total | 1814 | 1812 | 3626 |

*Note:* This table presents tabulations of the number of workers reporting displacement in the 1996-2008 CPS Displaced Worker Supplements by whether they found reemployment in occupations requiring tasks that were more or less similar than the median. The task overlap measure is discussed in Section 3.2.4. For details on the sample selection, see the notes to Table 3.4.
A $\chi^2$-test of independence of time since displacement and task overlap rejects independence ($p = .007$), but the relationship is much less important in magnitude than the relationship between time since displacement and the reporting of a layoff versus a plant closing in Table 3.7.

here overall, the forgetting does not appear strongly related to task overlap. If anything, it appears that workers who end up finding jobs requiring similar tasks (those in the "high overlap" column) forget displacement more quickly. However a more formal test, where the number of workers reporting displacement in each cell is modelled with a Poisson regression, fails to reject that high overlap workers are forgetting displacement more quickly at the 5% level.

If recall bias is important for estimating how the lemons effect varies with task overlap then the estimated lemons effect should vary with time since displacement. Table 3.8 allows the lemons effect to vary with time since displacement and shows that it is larger for workers reporting displacement longer ago. The analogous result for the interaction with task overlap is presented in Table 3.13. This table presents a regression of the log wage change on the same set of controls as in Tables 3.6 and 3.8, while allowing the layoff and task overlap terms to vary with time since displacement.

The interaction between the lemons effect and time since displacement takes the value of 0.06 for workers displaced one year ago, 0.10 for those displaced two years ago, and 0.02 for those displaced three years ago. At the bottom of the table, the $p$-test of the joint equality of the interaction coefficients is reported, and equality is not rejected. By contrast, the analogous test for equality of the layoff coefficient by time since displacement is rejected in Table 3.8.

## 3.4 Conclusion: Further Analysis of the "Lemons Effect"

I use measures of the task requirements of occupations to probe how employer learning as embodied in the lemons effect relates to task-specific abilities. I present suggestive evidence that the lemons effect is significantly larger for workers who make large task changes after displacement. Laid off workers switching to jobs using completely opposite tasks face a lemons effect of 12% of wages, and workers making a transition one standard deviation greater than average face a 3.6% penalty. This finding is inconsistent with a simple extension of Greenwald (1986) that would suggest either that the lemons effect would be smaller for workers making large skill transitions, or independent of transition size, depending whether employers learn about, respectively, tasks specific or general productivity.

I also show that conventional estimates of the Gibbons and Katz lemons effect can largely be explained by recall bias. Workers who were laid off are more likely to forget their displacement over time if they have good reemployment outcomes. The entire measured lemons effect in the 1996 to 2008 CPS DWS is driven by those

Table 3.13: The effect of a layoff on reemployment wages by time since displacement and task overlap

| | $\Delta$ log($w$ ) |
|---|---|
| Layoff * Overlap * 1 Year Ago | 0.060<br>(0.048) |
| Layoff * Overlap * 2 Year Ago | 0.095<br>(0.055) |
| Layoff * Overlap * 3 Year Ago | 0.024<br>(0.052) |
| Layoff * Years Since Displacement | X |
| Overlap * Years Since Displacement | X |
| p(Equality of Layoff * Overlap Coefficients) | 0.647 |

*Note:* Estimates of a log wage change regression where the lemons effect is allowed to very by years since displacement. The first column reports results of these regressions for the sample. In the second column, the lemons effect is also allowed to vary by tenure level, but for brevity only high tenure coefficients are reported (all coefficients for low tenure workers are close to zero). The third column estimates the same equation as the first column except that the sample is restricted to only workers with tenure above the median. All regressions include controls for marital status, race, region, previous occupation and industry, schooling, years since displacement, a quadratic in potential experience and a cubic spline in previous tenure with breaks at 1, 2, 3, and 6 years. They use data from the 1996-2008 CPS DWS, considering only workers who were displaced from full-time, private sector, non-agricultural, non-self employed jobs paying at least $75 2006 dollars per week and are reemployed in similar jobs.

reporting displacement three years ago. While the level of the lemons effect is driven by recall bias, the interaction between the lemons effect and the task change after displacement is not.

# Appendix A

# Details on Uncertainty and Parental Investment

## A.1 The Illustrative Model with Quadratic Utility

To see that more risk tolerant parents invest more in their children, notice that the first order condition in Equation 2.5b can be written in the quadratic case as:

$$u'(c_1) \;=\; (A - 2B \cdot E[H]) \cdot E[H_y] - 2B \cdot \text{var}(H) \cdot \frac{\partial \text{var}(H)}{\partial y} \tag{A.1}$$

Note that greater risk tolerance corresponds to a flatter utility function, or a decrease in $B$. As $B$ decreases, the right hand side of Equation A.1 increases. Therefore, the marginal utility of consumption increases, and so this more risk tolerant individual will choose lower consumption $c_1$ and higher investment levels $y$ and $s$.

More risk tolerant parents also invest relatively more in early childhood than their risk averse peers. To see this, look at the quadratic version of Equation 2.5c:

$$\begin{aligned} EV_y &= EV_s \\ (A - 2BE[H])E[H_y] - 2B\text{var}(H)\frac{\partial \text{var}(H)}{\partial y} &= \\ &(A - 2BE[H])E[H_s] - 2B\text{var}(H)\frac{\partial \text{var}(H)}{\partial s} \end{aligned} \tag{A.2}$$

Marginal utilities with respect to each argument are decreasing with the arguments if parents are at an interior solution. If risk tolerance increases (i.e. $B$ decreases), it is possible to see how parents adjust by looking at whether there is a larger effect on the marginal utility of investment in early childhood or the marginal utility of investment in late childhood. If the marginal utility with respect to $y$ increases by more than the marginal utility with respect to $s$, then parents must invest more in $y$ and/or less in $s$ in order for equation A.2 to hold. That is, we need:

$$\begin{aligned} -\frac{\partial EV_y}{\partial B} &> -\frac{\partial EV_s}{\partial B} \\ 2E[H]\cdot E[H_y] + 2\mathrm{var}(H)\cdot\frac{\partial \mathrm{var}(H)}{\partial y} &> 2E[H]\cdot E[H_s] + 2\mathrm{var}(H)\cdot\frac{\partial \mathrm{var}(H)}{\partial s} \\ 2E[H]\cdot(\underbrace{E[H_y]-E[H_s]}_{>0}) + 2\mathrm{var}(\quad H \quad)\underbrace{\left(\frac{\partial \mathrm{var}(H)}{\partial y}-\frac{\partial \mathrm{var}(H)}{\partial s}\right)}_{>0} &> 0 \qquad \text{(A.3)} \end{aligned}$$

Equation A.3 holds because at the margin time with younger children is more productive (an additional unit of time in early childhood increases human capital more than an additional unit of time in late childhood) and because it is more risky (an additional unit of investment in early childhood contributes more to risk than a unit of investment in late childhood). Thus more risk tolerant parents will shift their time investment profile from late childhood towards early childhood.

## A.2 Risk Tolerance and Time Use: Different Controls

Table B1: 1997 parental time with children in the PSID CDS and risk tolerance: additional specifications

| | All | | Education | | Recreation | | Basic Care | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Risk Tolerance (KSS) | | 1.903 | | 0.520** | | 0.717 | | -0.423 |
| | | (1.674) | | (0.195) | | (1.060) | | (0.556) |
| Child Age (Months) | -0.0396 | 0.0286 | 0.129** | 0.147** | -0.0492 | -0.0179 | -0.387** | -0.406** |
| | (0.363) | (0.371) | (0.0437) | (0.0452) | (0.243) | (0.247) | (0.146) | (0.146) |
| Child Age$^2$/1000 | -1.604 | -2.566 | -1.648* | -1.912** | 0.0273 | -0.428 | 3.550+ | 3.831+ |
| | (5.111) | (5.142) | (0.645) | (0.667) | (3.380) | (3.395) | (2.121) | (2.115) |
| Non-White | -7.455+ | -7.134+ | 0.845 | 0.928 | -1.614 | -1.745 | -3.979*** | -3.869*** |
| | (4.252) | (4.210) | (1.019) | (0.960) | (2.713) | (2.816) | (1.015) | (1.032) |
| Male | 0.792 | 0.952 | -0.358 | -0.313 | 0.769 | 0.936 | 0.0136 | -0.0981 |
| | (2.923) | (3.024) | (0.328) | (0.325) | (1.877) | (1.953) | (0.963) | (0.964) |
| Age of Mother | -0.375 | -0.351 | 0.0351 | 0.0420 | -0.640 | -0.606 | 0.177 | 0.153 |
| | (0.627) | (0.675) | (0.0612) | (0.0604) | (0.392) | (0.427) | (0.166) | (0.161) |
| Age of Father | -0.200 | -0.224 | -0.0402 | -0.0471 | 0.0726 | 0.0455 | -0.198 | -0.180 |
| | (0.648) | (0.691) | (0.0547) | (0.0557) | (0.376) | (0.405) | (0.148) | (0.144) |
| Mom Col. Grad | 3.204 | 3.424 | 0.817* | 0.876* | 2.126 | 2.168 | -0.579 | -0.598 |
| | (3.048) | (3.119) | (0.404) | (0.394) | (2.191) | (2.236) | (1.051) | (1.040) |
| Mom HS Grad | 3.573 | 3.133 | 0.862 | 0.739 | 3.631 | 3.319 | -2.460 | -2.257 |
| | (5.863) | (5.867) | (0.569) | (0.529) | (3.504) | (3.561) | (1.701) | (1.666) |
| Male 1996 Respondent | | -1.045 | | -0.300 | | -1.315 | | 0.893 |
| | | (3.263) | | (0.324) | | (2.225) | | (0.935) |
| N | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 |

*Standard errors in parentheses:* $+\ p < 0.10$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

*Note:* These tables present regressions of the time that PSID CDS children spend in activities where either the mother or father are listed as individuals "with whom" the child does the activity. The time that children spend in activities of this sort with their mothers is added to the time they spend in activities with their fathers, as in Folbre, Yoon, Finnoff, and Fuligni (2005). The first two columns present any type of time that parents spend with their children. The second set of two columns use time spent in educational activities, the third set time in recreational activities, and the fourth basic care activities. For the definitions of these activities, see Guryan, Hurst, and Kearney (2008). For a discussion of the sample selection, see the footnote to Table 2.1.

*Variables Definitions:* See the notes to Table 2.7.

Table B2: 2002 parental time with children in the PSID CDS and risk tolerance: additional specifications

| | All | | Education | | Recreation | | Basic Care | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Risk Tolerance (KSS) | | -2.476* | | 0.0137 | | -1.679* | | -0.0640 |
| | | (1.118) | | (0.180) | | (0.708) | | (0.313) |
| Child Age (Months) | -0.181 | -0.608+ | -0.0597 | -0.0910 | -0.136 | -0.263 | -0.0218 | -0.0873 |
| | (0.273) | (0.316) | (0.0503) | (0.0621) | (0.193) | (0.217) | (0.0883) | (0.111) |
| Child Age$^2$/1000 | 1.871 | 8.195+ | 0.921 | 1.462+ | 1.776 | 3.945 | 0.135 | 1.094 |
| | (4.089) | (4.727) | (0.735) | (0.837) | (2.978) | (3.304) | (1.255) | (1.638) |
| Non-White | -3.612 | -5.886 | -0.584 | -0.138 | -4.004 | -5.776* | -1.813+ | -2.061+ |
| | (3.649) | (4.361) | (0.409) | (0.461) | (2.484) | (2.795) | (1.001) | (1.170) |
| Male | 1.675 | 2.516 | -0.200 | -0.0420 | 2.939+ | 3.966* | -0.0927 | -0.139 |
| | (2.326) | (2.382) | (0.462) | (0.421) | (1.529) | (1.603) | (0.688) | (0.729) |
| Age of Mother | 0.288 | 0.403 | -0.0569 | -0.0831 | 0.212 | 0.445 | 0.0000882 | 0.0119 |
| | (0.398) | (0.391) | (0.0734) | (0.0801) | (0.347) | (0.350) | (0.120) | (0.138) |
| Age of Father | -0.698+ | -0.583 | 0.0783 | 0.145+ | -0.400 | -0.421 | -0.0975 | -0.0770 |
| | (0.365) | (0.360) | (0.0711) | (0.0770) | (0.275) | (0.269) | (0.102) | (0.116) |
| Mom Col. Grad | -3.317 | -3.712 | 0.175 | 0.0116 | -3.059+ | -3.692* | 0.361 | 0.560 |
| | (2.540) | (2.433) | (0.477) | (0.418) | (1.753) | (1.846) | (0.726) | (0.785) |
| Mom HS Grad | 10.59** | 8.467+ | 0.543 | 0.407 | 5.423* | 3.059 | 0.206 | 0.00782 |
| | (3.348) | (4.358) | (0.521) | (0.619) | (2.638) | (2.989) | (1.278) | (1.513) |
| Male 1996 Respondent | | -1.987 | | -0.478 | | -1.700 | | -0.336 |
| | | (2.302) | | (0.532) | | (1.567) | | (0.801) |
| N | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 |

*Standard errors in parentheses:* + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

*Note:* These tables present regressions of the time that PSID CDS children spend in activities where either the mother or father are listed as individuals "with whom" the child does the activity. The time that children spend in activities of this sort with their mothers is added to the time they spend in activities with their fathers, as in Folbre, Yoon, Finnoff, and Fuligni (2005). The first two columns present any type of time that parents spend with their children. The second set of two columns use time spent in educational activities, the third set time in recreational activities, and the fourth basic care activities. For the definitions of these activities, see Guryan, Hurst, and Kearney (2008). For a discussion of the sample selection, see the footnote to Table 2.1.

*Variables Definitions:* See the notes to Table 2.7.

Table B3: Differences in parental time over childhood and parental risk tolerance: additional specifications

| | All | | Education | | Recreation | | Basic Care | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Risk Tolerance (KSS) | | 3.671* | | 0.563* | | 2.050* | | -0.198 |
| | | (1.620) | | (0.280) | | (0.927) | | (0.617) |
| Child Age (Months) | 0.141 | 0.725 | 0.188** | 0.273** | 0.0868 | 0.329 | -0.365* | -0.216 |
| | (0.373) | (0.467) | (0.0690) | (0.0859) | (0.258) | (0.338) | (0.148) | (0.191) |
| Child Age$^2$/1000 | -3.475 | -12.26+ | -2.569* | -3.687** | -1.749 | -5.486 | 3.415 | 1.336 |
| | (5.261) | (6.623) | (1.013) | (1.235) | (3.580) | (4.702) | (2.161) | (2.718) |
| Non-White | -3.843 | -0.184 | 1.429 | 0.735 | 2.391 | 5.879* | -2.166+ | -2.273 |
| | (4.010) | (4.495) | (1.208) | (1.164) | (2.840) | (2.852) | (1.308) | (1.653) |
| Male | -0.883 | -1.877 | -0.158 | -0.0523 | -2.170 | -3.350 | 0.106 | 0.237 |
| | (3.083) | (3.343) | (0.578) | (0.547) | (2.052) | (2.190) | (1.048) | (1.105) |
| Age of Mother | -0.663 | -0.482 | 0.0921 | 0.149 | -0.852* | -0.940* | 0.177 | 0.256 |
| | (0.603) | (0.614) | (0.0991) | (0.104) | (0.415) | (0.396) | (0.205) | (0.207) |
| Age of Father | 0.498 | 0.454 | -0.118 | -0.204* | 0.473 | 0.585 | -0.101 | -0.137 |
| | (0.562) | (0.619) | (0.0921) | (0.0902) | (0.350) | (0.363) | (0.157) | (0.170) |
| Mom Col. Grad | 6.521* | 5.338+ | 0.642 | 0.735 | 5.186* | 5.635* | -0.940 | -1.927 |
| | (3.106) | (3.225) | (0.653) | (0.613) | (2.280) | (2.491) | (1.128) | (1.184) |
| Mom HS Grad | -7.013 | -6.055 | 0.318 | 0.122 | -1.792 | -1.480 | -2.666+ | -1.830 |
| | (5.252) | (5.696) | (0.811) | (0.861) | (3.468) | (3.904) | (1.581) | (2.016) |
| Male 1996 Respondent | | -0.240 | | 0.00631 | | -0.0579 | | 0.317 |
| | | (3.354) | | (0.678) | | (2.333) | | (1.097) |
| N | 298 | 298 | 298 | 298 | 298 | 298 | 298 | 298 |

*Standard errors in parentheses:* + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

*Note:* These tables present regressions of the difference time that PSID CDS children spend in activities where either the mother or father are listed as individuals "with whom" the child does the activity between the 1997 and 2002 time diaries. To create the time measures, the time that children spend in activities of this sort with their mothers is added to the time they spend in activities with their fathers, as in Folbre, Yoon, Finnoff, and Fuligni (2005). The first two columns present any type of time that parents spend with their children. The second set of two columns use time spent in educational activities, the third set time in recreational activities, and the fourth basic care activities. For the definitions of these activities, see Guryan, Hurst, and Kearney (2008). For a discussion of the sample selection, see the footnote to Table 2.1.

*Variables Definitions:* See the notes to Table 2.7.

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