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ESSAYS ON ECONOMICS OF THE FAMILY

A Dissertation
Presented to
the Graduate School of
Clemson University

In Partial Fulfillment
of the Requirements for the Degree
Doctor of Philosophy
Economics

by
Yinlin Dai
May 2022

Accepted by:
Dr. Jorge Luis García, Committee Chair
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Abstract

This dissertation is comprised of two essays on family economics.

In the first chapter, I explore the role of gender discrimination in children's investments by parents. I specifically examine whether parents in urban China devote more household resources, specifically education, health, and total expenditures, to girls than to boys. The empirical literature has extensively examined trends in the sex ratio at birth and the effect of gender on the extensive margin of fertility. Much less of the existing literature has explored the impact of gender on the intensive margin of parental inputs mainly because of lack of individual child-level data, especially in urban China. This is an important area for economic research to help disentangle child gender bias and provide insight on the well-being of Chinese children. To answer my research question, I use unique data, Chinese Child Twin Survey (CCTS) that includes family expenditure information for individual children within the family.

Estimating the causal effect of child gender on parental investments requires boys and girls to live in families with similar observable and unobservable characteristics. This assumption may be violated in China for two reasons. First, some families use ultrasound technology to engage in sex selection at birth, which may indicate that parents who have boys are different from those who have girls. Second, if families have a preference for sons and parents base fertility decisions on the gender of previous children, girls will end up in larger families on average. Since larger families have fewer resources per capita, this implies that girls on average will be allocated fewer resources. To address the empirical challenges, I leverage the randomness of the first child's gender and utilize a twin-fixed effect estimator.

My results suggest that average yearly educational expenditures are 18 percentage (48.9 U.S. dollars in 2020 value) higher for first-born girls than for first-born boys in households with first-born singletons. For households with first-born twins, compared to a male twin sibling, the female

twin sibling received 25 percentage (59.8 U.S. dollars) more in yearly educational expenditures. To further provide insight on how parents allocate resources across their children within the framework of economic theory, I adopt a collective-household model to structurally estimate the total spending on each twin sibling to account for the resource sharing among twins. Consistent with the results of the twin-fixed-effects model, my estimates show that parents allocate more household resources to girls.

The second chapter of this dissertation, which is joint work with Scott Barkowski and Joanne Song McLaughlin, examines the effect of young children on the labor supply of parents during COVID-19. Using the monthly Current Population Survey (CPS), and following a pre-analysis plan, we implement three variations of an event study research design comparing workers with childcare responsibilities to those without. The first compares parents with young children (under age 13) who have childcare needs to those without young children. For a sample limited to parents of young children, the second and third rely on the presence of someone who could provide childcare in the household: a teenager in one and a grandparent in the other (as control groups). We analyze three outcomes: whether parents were “at work” (not sick, on vacation, or otherwise away from his or her job); whether they were employed; and hours worked conditional being employed.

Contrary to our expectation, we find the labor supply of parents with young children was not negatively affected during the pandemic. Instead, some evidence suggests they were more likely to be working after the pandemic unfolded. For the outcomes of being at work and employed, our results are not systematically different for men and women. However, some findings suggest women with young children worked almost an hour longer per week than those without. We provide evidence from questions newly added to the CPS during the pandemic that parents were more likely to work remotely than non-parents, suggesting employer flexibility with regard to telework aided parents in avoiding negative shocks to their labor supply.

Dedication

To my parents, Weimin Dai and Xiaohong Cheng. Thank you for always supporting me to chase my dreams and allowing me to have as much freedom as I want. Thank you for unconditional love!

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Chapter 1

Child Gender and Parental

Investments:

Evidence from Urban China

1.1 Introduction

Many Asian societies have a long-standing preference for sons over daughters (Das Gupta et al., 2003). The most stark manifestation of this son preference is the severe male-biased sex imbalance at birth (Sen, 1990). As a result, according to 2010 data, there were about 500,000 more male births per year in China (Almond et al., 2019). Despite the male bias at birth, in China, as in most modern countries, more girls enroll in college than boys (Jayachandran, 2015). In 2010, China's female to male tertiary enrollment ratio reached 1.04 (about 380,000 more girls), and the percentage of female tertiary enrollment continues to increase (see Figure 1.1). Based on the female to male tertiary enrollment ratio, it might seem apparent that parents devote more household resources to daughters. However, their partiality for sons makes it challenging to measure the true relationship between child gender and parental inputs.

Estimating the causal effect of child gender on parental investments requires boys and girls to live in families with similar observable and unobservable characteristics. This assumption may be violated in China for two reasons. First, some families use ultrasound technology to engage in

sex selection at birth (Y. Chen et al., 2013). In 2010, the sex ratio at birth (male to female) was approximately 1.19 in China, while the biologically normal sex ratio at birth is 1.05.¹ The male-biased sex ratio at birth may indicate that parents who have boys are different from those who have girls. Prior literature suggests that child sex decisions may be related to family income (Almond et al., 2019). If low-income families are more likely to abort girls on average, it will appear as if sons receive fewer household resources.

Second, if families have a preference for sons and parents base fertility decisions on the gender of previous children, girls will end up in larger families on average (Lee, 2008; Yamaguchi, 1989).² Since larger families have fewer resources per capita, this implies that girls on average will be allocated fewer resources. Because of this son-biased fertility stopping rule, it is difficult to disentangle the effect of family size and differential parental treatment.³

This paper explores whether parents in urban China devote more household resources, specifically education, health, and total expenditures, to girls than to boys.⁴ To address this question, I use a sample of twin and non-twin households from the Chinese Child Twins Survey (CCTS), which contains detailed expenditure data for each child within a household. The singleton households are the comparison group for the twin households, and the only observable difference between these households is twinning.

I address potential selection bias from endogenous fertility decisions in two ways. First, I follow Dahl and Moretti (2008) and Choi and Hwang (2020) by exploiting the randomness of the first child's sex. The sex ratio at birth of the *first child* is close to the natural sex ratio, and the gender of the first child can be viewed as random (Almond et al., 2019; Y. Chen et al., 2013; Ebenstein, 2010; Garcia, 2020). My findings indicate that yearly educational expenditures are on average 18% (48.9 U.S. dollars in 2020 purchasing power parity) higher for first-born girls than for first-born boys in households with first-born singletons.

Second, I propose a novel empirical strategy to address unobserved parental characteristics that affect children's gender and investments. I use boy-girl twins to deal with both gender selection

¹ According to the World Bank Development Indicator, the world average for the sex ratio at birth was around 1.07 in 2010. Few countries had a sex ratio at birth that exceeded 1.10.

² Ebenstein (2010) has found that even under urban China's one-child policy (where parents pay a fine in order to have another child), first-born girls remain more likely to have younger boy siblings. Garcia (2020) shows that couples respond inelastically to the prices associated with the policy when their first-borns are girls.

³ Barcellos et al. (2014) argued that after controlling for family size, the child's gender is no longer exogenous and might be correlated with parental preferences.

⁴ Studies on the rural-urban education disparity in China find that children from rural areas are less likely to enter college due to limited educational resources (Chi and X. Qian, 2016). It is evident that urban and rural areas are at different stages of economic development. For the purpose of this study, I will only examine urban areas.

and the son-biased fertility stopping rule.⁵ A female twin sibling and a male twin sibling share the same parents and are born at the same time. By employing a within-twin fixed effects estimator, I can simultaneously address sex-selective abortions and the son-biased stopping rule. Prior research has also shown that parents are unlikely to abort twins by using ultrasound technology (Guo and J. Zhang, 2020).⁶ After controlling for unobserved parental characteristics by employing a twin-fixed effects model, I find that compared to a male twin sibling, the female twin sibling receives 25% (59.8 U.S. dollars) more in yearly educational expenditures.

To further investigate the intra-household distribution of resources and provide insight on how parents allocate resources across their children, I adopt a collective-household model (Chiappori, 1988; Chiappori, 1992) to structurally estimate each twin sibling’s intra-household share of resources (defined as each child’s share of total household spending on children). In the model, I allow for economies of scale in consumption among twin siblings. Resource shares are identified by comparing Engel curves of clothing that is consumed exclusively by each twin sibling (Dunbar et al., 2013). Even if twins share resources, I am able to identify the total expenditure allocated to each of them. This structural approach of studying resource allocation allows me to obtain estimates interpretable within the framework of economic theory. After accounting for the resource sharing among boy-girl twins, I find that a female twin sibling commands 54% of household resources devoted to children, while a male twin sibling commands 46% of those. Both the structural estimates and the reduced-form model (with the twin fixed-effect estimator) consistently show that parents spend more on girls (although the standard errors of the structural estimates are relatively large).

The contribution of this study is threefold. First, most of the empirical literature related to the gender effect on parental behavior focuses on the extensive margin of fertility, rather than on the intensive margin of parental monetary investments in China. Although several studies have examined sex ratio at birth trends in China, only a few studies have examined the effect of gender on parental inputs due to the lack of available individual-level data.⁷ To obtain a complete picture

⁵ Twins have been widely used in empirical research as an identification strategy to examine the returns to education (Ashenfelter and Krueger, 1994) and birthweight (Black et al., 2007). To my knowledge, I am the first to use boy-girl twins to examine the differential parental treatment based on gender after birth.

⁶ If Chinese parents engage in selection with regards to having twins, this would make it exceedingly difficult to use my twin estimates to generalize to non-twin parents.

⁷ To the best of my knowledge, Yueh (2006) has performed the only research on the effect of child’s gender on parental human capital investments in urban China. Results from her paper can only be viewed as descriptive rather than causal. Additionally, she utilized a household survey which was conducted in 1995. Since then, the Chinese economy has shifted toward a service economy, where white collar employment predominates (Rosenzweig and J. Zhang, 2013). Women have a comparative advantage in “brain-based” jobs, since they do not require physical strength. Thus, it is essential to examine if parental expenditures on female education in urban China have responded to increased opportunities for women.

of the impact of child gender on parental behavior in Chinese households, this study uses a unique child dataset and complements the current literature by analyzing if the traditional son bias is also reflected in the intra-household allocation of resources.

Second, to my knowledge, my exploration of the randomness of the first child’s gender and the use of boy-girl twins are new to the literature examining boy-girl differences in parental inputs in China.⁸ Under normal circumstances, child gender can be considered random at conception, so a direct comparison of expenditures by gender is sufficient to detect a gender bias in the intra-household allocation of resources. However, preference for sons could lead to boys and girls being born in different families. Few researchers take this problem into account when they examine the effect of gender on parental spending in China. Prior studies deal with the endogeneity of gender by employing a sibling-fixed effect model (Aslam and Kingdon, 2008; Azam and Kingdon, 2013; Kingdon, 2005). However, there are concerns of birth order effects in these models. Children born early may have fewer resources than children born later in the parents’ life cycle, which may affect differences in resource allocations at every stage of each child’s life cycle. A twin-fixed effects estimator is a potentially powerful way of purging heterogeneity across parents and avoiding problems from birth order effects.

Third, I extend the literature by studying resource sharing within the household. This phenomenon is typically studied in the context of consumption goods—examples include Deaton and Paxson (1998) and Browning et al. (2013). Although some include children, they focus on the total amount that all children consume in the household (Dunbar et al., 2013) or they treat children as consumption goods for parents (Blundell et al., 2005; Cherchye et al., 2012) because of theoretical or empirical limitations. This does not allow for studying the sources generating differences in parental investments across siblings. A collective household framework makes it possible to quantify the share of household resources parents spend on each child and offers insights into how parents allocate resources within an economic framework.

The rest of this paper is organized as follows. Section 1.2 provides an overview of the related literature and discusses the unique contributions of this paper. I describe the data in section 1.3. Section 1.4 presents the reduced-form results and establishes a causal link between child gender and

⁸ Li and Wu (2011) and Choi and Hwang (2015) utilize the randomness of the first child’s gender in the context of Asia. Li and Wu (2011) examine the effects of the gender of the first child on the mother’s bargaining power in China, while Choi and Hwang (2015) study the impact of gender on parental monetary inputs in Korea. Bharadwaj, De Giorgi, et al. (2016) implement a twin-fixed effects model to analyze the gender gap in mathematics in Chile.

parental inputs. Section 1.5 discusses the collective household model, the identification of resource shares, and the structural estimation results. Section 1.6 concludes and summarizes the main results of the paper.

1.2 Related Literature

1.2.1 Prior Empirical Evidence

This paper is related to two strands of literature: the relationship between child gender and parental behaviors, particularly on the intensive margin (i.e., parental investments after birth), and collective household behaviors in which children are taken into account in the allocation of resources.

There is sizeable evidence of the effect of child gender on parental behaviors. On the extensive margin (i.e., fertility decisions), parents may engage in sex-selective abortions and follow son-biased stopping rules. The existing literature has documented these son-biased fertility behaviors in China (Almond et al., 2019; Y. Chen et al., 2013; Ebenstein, 2010; Garcia, 2020; N. Qian, 2008).⁹ These seminal works have provided ample evidence that girls and boys might be born into different families, which outlines the empirical challenges in estimating the effects of gender on parental spending.

There is also a growing literature on gender differences on the intensive margin (i.e., parental investments after birth), particularly concerning educational inputs. There are three waves of literature that examine the boy-girl differences in educational spending. For the first wave, most studies of gender discrimination in parental inputs are hampered by the lack of available individual-level data. Given the prevalence of aggregate household-level data, researchers have often used the Engel curve approach (Deaton, 1997) to detect boy-girl differences in educational spending within households.¹⁰ However, the Engel curve approach has failed to identify the boy-girl differences in educational expenditures, while the gender inequality in educational outcomes is pervasive (Kingdon, 2005).

⁹ The male-biased sex ratio at birth in China has been linked to different factors, such as low female bargaining power (N. Qian, 2008), the interaction of the one-child policy with boy preferences (Ebenstein, 2010; Garcia, 2020), the availability of sex-selective abortion technology that reduced the costs of sex selection during the 1980s (Y. Chen et al., 2013), and land-reform that spurred remarkable growth in agricultural output and made sex selection more affordable (Almond et al., 2019). The pre-existing boy preference is the ultimate cause of the male-biased sex ratio, and is triggered by cultural, political, and economic forces.

¹⁰ In the Engel curve approach, the household budget share of the educational goods is regressed on the log per-capita total expenditures, log household size, the shares of various age-sex groups, and other relevant household characteristics. If there is gender bias in intrahousehold allocation, then the coefficient on the share of male members of a particular age group should be significantly different from the coefficient on the share of female members of the same age group (Zimmermann, 2012).

The failure of the Engel approach to identify these differences could be attributed to three reasons. First, the household-level analysis mutes the gender effects. Second, the pro-male bias in education could occur for two different decisions: whether to enroll both sons and daughters in school, and conditional on enrolling both genders, how much to spend on their educational goods. Since there is a pro-male bias in the enrollment decision and no bias (or even a slight pro-female bias) in the conditional education expenditure decision, aggregating these two decisions might mask the boy-girl differences in educational spending. Third, boys and girls are born into families of different characteristics, as implied by the male-biased sex ratio at birth (Aslam and Kingdon, 2008; Barcellos et al., 2014; Kingdon, 2005; Zimmermann, 2012). Thus, the first wave of literature that utilizes household-level data should be interpreted as descriptive, rather than causal.

The second wave of literature employs individual-level data and examines the two parental decisions separately. These studies find pro-male biases in both the enrollment decision as well as the decision of how much to spend conditional on enrollment (Aslam and Kingdon, 2008; Azam and Kingdon, 2013; Kingdon, 2005). These studies employ a family fixed effects approach to eliminate the potential endogeneity of gender. These papers indeed find stronger evidence of gender differences in educational inputs at the individual level. Boys are more likely to enroll in schools and parents spend more on sons' education. The concern with comparing siblings within a household is that parents might have different resources at different stages of their life-cycle. Additionally, child gender remains non-random, as parents might base fertility decisions on the gender of previous children.

Recent work by Choi and Hwang (2020) has leveraged the randomness of the first child's gender to examine the differences in parental monetary inputs by the child's sex in South Korea. Even though sex ratio at birth in South Korea has reached the natural range, there is still evidence that parents practice sex-selective abortions at the higher parity birth and follow son-stopping fertility rules (Choi and Hwang, 2015; Choi and Hwang, 2020). They find that first-born boys receive more financial support for private education and are expected to obtain higher education in South Korea than first-born girls. However, the gender gaps in educational inputs have narrowed substantially over the past two decades in South Korea.

There is a diverse literature on intrahousehold resource allocations based on the collective household framework introduced by (Chiappori, 1988; Chiappori, 1992). This framework models households as a collection of individuals, each with their own preferences. The key assumption of the model is that the household is Pareto efficient in its allocation of resources. This means that

the household resource allocation problem is equivalent to each household member's maximization of their own utility function after receiving a fraction of the household resources.

Identification of individuals' resource shares, defined as the fraction of the total household expenditure that is devoted to each household member, is challenging because expenditure data is typically collected at the household level. It is difficult to observe each individual's consumption level of different goods. Further, the household members might share certain goods. Specifically, Dunbar et al. (2013) use Engel curves of one private assignable good for each individual and impose semi-parametric restrictions on the preferences for this good.¹¹ Using data from Malawi, they find that there is gender asymmetry in consumption within the household. Further, if the proportion of boy children increases, mothers sacrifice more of their resources.

While the methods used by Dunbar et al. (2013) identifies the intra-household resource allocations between men, women, and children, the existing method is often unable to uncover inequalities among each child within the same household. This limitation is due to the nature of consumption surveys, which include expenditures on goods that can be assigned to all children, but not goods that can be assigned to individual children. In their work, Dunbar et al. (2013) identify resource shares for children as a whole, rather than for each individual child. The data used in this study contain detailed expenditure information for each child, which allows for the examination of gender asymmetry in Chinese households.

1.2.2 Education in Urban China

Educational expenditure is one of the most important parental investment measures in developing countries and is known to affect a child's human capital and well-being. In China, parental investments in children's education have grown rapidly in urban areas (Attané, 2016). Additionally, the increasing costs of education are primarily borne by households (Chi and X. Qian, 2016; Heckman and Yi, 2012).

In urban China, the educational system is comprised of primary school (six years), middle school (three years), and high school or professional school (three years) before college. The compulsory schooling includes primary and middle school. Both at the high school level and the compulsory level (primary and middle school), there are differences in school quality and higher quality schools

¹¹ Other literature that identifies the level of resource shares includes Cherchye et al. (2011), Browning et al. (2013), and Dunbar et al. (2021).

tend to be more expensive (for both public and private institutions) (Yueh, 2006). In urban areas, the total household educational expenditures have continued to rise due to increased out-of-school expenditures on private tutoring and extracurricular activities (Chi and X. Qian, 2016).

In urban China, there are no large gender differences in educational enrollment (Yueh, 2006). Schools are typically mixed-sex and the direct costs of schooling are equal for girls and boys. In addition, price discrimination against girls does not exist for out-of-school education (Shu, 2004). Thus, it is difficult to argue that gender-specific requirements drive the observed differences in educational spending.

1.3 Data

The empirical analysis in this study uses the Chinese Child Twins Survey (CCTS). In late 2002 and early 2003, the CCTS was conducted by the Urban Survey Unit (USU) of the National Bureau of Statistics in the Kunming metropolitan area of China. Kunming is the capital city of Yunnan Province and has a total population of approximately 5 million people. Yunnan is located at the southwestern corner of China and is one of China's relatively underdeveloped provinces. In 2002, the average per capita GDP in Yunnan was RMB 5800, while the average in China was RMB 10,000.

To my knowledge, the CCTS is the first census-type survey on Chinese households with twin children. There are two important features of the unique sampling and survey design of the CCTS. First, the USU identified these twin households according to whether the children had the same birth year and relationship with the household head in the 2000 population census. The census office then obtained the addresses of these eligible twin households and verified their status with an investigator visit. The survey successfully identified 1,694 households with twins ages 6 to 18 years living in Kunming in 2002. The average age of children in the sample is 11 years old. There were also 1,693 households with non-twin children in the same age group. For every twin household interviewed, the fourth household on the right-hand side of the same block was chosen to locate a non-twin household. If the fourth household did not have at least one child in the age range of 6 to 18, interviewers would continue going to the fifth, sixth, and so on (Rosenzweig and J. Zhang, 2009).

Second, the survey includes information about parental spending on each child separately.

Parents were asked to report how much they spent on each child on education, health, and clothes in the past 12 months. Educational expenditures include household spending on school tuition, books, stationary, private home tutors, and tutoring classes. Both educational expenditures and health expenditures include the amount paid by the parents, excluding any government subsidies or support from extended family. In addition, the survey contains a wide range of demographic, social, and economic information about the parents.

I select a sample of 439 first-born non-twins in urban areas for my analysis (see Table 1.1 for more details about the sample restriction). Since gender selection is mainly observed from the second birth or later (see Figure 1.2) (Almond et al., 2019; Y. Chen et al., 2013; Ebenstein, 2010), I leverage the randomness of the first child's sex for non-twins. The sex ratio (male to female) of first-born singletons in my sample is 1.02, which is considered natural.

As an empirical comparison group, I also choose 432 first-born twins (see Table 1.1 for more details about the sample restriction). From the survey design, parents of twins are similar to parents of singletons in terms of observable characteristics. Table 1.2 presents parental summary statistics by twinning. I conduct t-tests of the means of parental educational attainment and age at the time of the survey. I do not find that parents of twins are statistically significantly different from parents of non-twins. Consistent with existing empirical studies (Black et al., 2007; Rosenzweig and J. Zhang, 2009; Royer, 2009), the mother's age at first birth is statistically different for twin and non-twin households. The sex ratio of first-born twins is 1.04, which suggests that parents in my sample did not conduct sex-selective abortions of twins (Guo and J. Zhang, 2020).

In urban Kunming, as in the rest of urban China, the One-Child Policy created a pricing system in which permits allowed every woman and her partner to have their first child for free. Permits to have a second or third child had a price (Garcia, 2020). Nine households with first-born non-twins have another singleton child in my sample. The sample also includes 11 households with first-born twins that go on to have another singleton child. Parents from my sample do not seem to follow a son-preference fertility-stopping rule in urban Kunming.

The main empirical concern of sex-selective abortions is that parents who choose girls might be different from parents who choose boys. Table 1.3 presents parental descriptive statistics by child gender. If a child's gender is truly random, the characteristics of the parents with boys and girls (but not necessarily the children) should be similar. This expectation is confirmed in Table 1.3: t-tests of the means of parental educational attainment, age, and mother's age at first birth indicate

non-rejection of the hypothesis of equality across parents with boys and girls.

Tables 1.4 and 1.5 present summary statistics for non-twins and twins. As shown in Table 1.4, average yearly educational expenditures are 364.45 Yuan (approximately 64 U.S. dollars in 2020 purchasing power parity) higher for first-born girls than for first-born boys. Overall, girls' parents spend about 408.53 Yuan (approximately 73 U.S. dollars) more on their child than parents of boys. Total expenditures include spending on education, health, and clothing. In order to adjust for heterogeneous family income, I also compute the budget shares for different categories of expenditures as a percentage of total household expenditures. Educational spending on girls represents 32.25 % of total household expenditures, while educational spending on boys represents 28.18 % of total expenditures. This difference is statistically significantly different ($p = .03$). Sons receive a slightly higher budget share of health investments. Table 1.5 shows that parents of boy-girl twins spend more on the girl siblings' education, health, and clothes on average. I find little statistical differences in the budget shares of different expenditures between female twin siblings and male twin siblings though female budget shares for education and total expenditures are consistently higher.

1.4 A Reduced Form Analysis of Gender and Parental Investments

1.4.1 Empirical Strategies

Boy-girl differences in parental inputs can be measured by

$$y_i = \beta_0 + \beta_1 Boy_i + \beta_2 \mathbf{X}_i + \epsilon_i, \quad (1.1)$$

where y_i is the parental yearly expenditures (education, health and total) for child i , Boy_i is an indicator that equals one if the child is a boy and zero otherwise, \mathbf{X}_i is a vector of family characteristics, and ϵ_i is an error term. The ordinary least squares (OLS) estimate of β_1 captures the average effect of a child's gender on parental investments if the gender is randomly distributed in the population or exogenous conditional on covariates. Although it is natural to assume that child gender is randomly determined at conception, the randomness assumption will be violated for two

reasons.

First, as implied by the male-biased sex ratio at birth in urban China, where sex-selective abortions are accessible, girls are more likely to be born into families with less strong preference. The characteristics of parents might be related to both a child's gender and children's resource allocation. Almond et al. (2019) document that after major land reform in rural China, parents who benefited from the reform and became wealthy were more likely to select the gender of children. If gender-biased families who abort daughters are wealthier on average, it will appear that sons receive more investments than girls. Controls extensively for observed parental characteristics may reduce the omitted variable bias.

However, unobserved parental characteristics like the mother's bargaining power will be hard to measure. The division of resources between children depends on the preferences of the parents. If mothers have relatively less son preference, then that would be reflected in their bargaining power and could affect the chances of giving birth to a son in the first place. In this case, the child's gender will not be orthogonal to bargaining power. N. Qian (2008) shows that an increase in mother's bargaining power—measured by exogenous increase in female-specific income in the households increases the survival rates for girls and educational attainment of all children. The unobserved heterogeneity of parents will bias estimates of gender.

Second, parents base fertility decisions on the gender of previous children. Even though there is a price associated with having a second or third child in urban China (Garcia, 2020), some parents with strong son preference choose to have an additional birth if the first birth is a girl. Ebenstein (2010) and S. Chen (2020) both have found that first-born girls are more likely to have a sibling. This fertility behavior implies that on average, girls will live in larger families than boys, and larger families have fewer per capita resources. Even in the absence of any differential treatment of sons and daughters, girls will tend to have worse outcomes than boys merely as a result of larger family size (Jensen, 2003). It is important to note that the selection bias resulted from the son-biased stopping rules would become more severe if I control for the number of children in the regression because it is partly determined by the gender of previously born children (Barcellos et al., 2014; Choi and Hwang, 2020).

To overcome these challenges, I implement two approaches to identify the relationship between gender and parental investments. First, to address the endogeneity of gender, I follow Dahl and Moretti (2008) and Choi and Hwang (2020) by exploiting the randomness of the first child's

sex for non-twins. Empirical study shows that the sex ratio at birth of the first child is close to the natural sex ratio in different nationally representative samples of China (Almond et al., 2019; Y. Chen et al., 2013; Ebenstein, 2010; Garcia, 2020). Gender of the first child can be viewed as exogenous. Additionally, the 1990 population census of Yunnan shows that the sex ratio at first birth in urban Yunnan is normal and I do not find the male-biased sex ratio at first parity from my sample (see Figure 1.2). Then, Boy_i in (1.1) represents whether the first child is male and the estimation sample only includes first-borns.

Note that only 9 households give additional births after their first-born children in my sample. Therefore, subsequent fertility decisions that may depend on the gender of the first-born child (son-biased fertility stopping rules) do not seem to be a big concern in my estimation. Additionally, estimation relying on the first child's gender consistently estimates the total effect of child gender on parental expenditures, including any indirect effects through later fertility choices (Bharadwaj, Dahl, et al., 2014).

In order to validate results from OLS estimation and better control for unobserved family backgrounds, I employ a novel empirical strategy—the incidence of boy-girl twins to examine the gender on parental investments. A female twin sibling and a male twin sibling share the same parents and are born at the same time. By employing a within-twin fixed effects model, I am able to address the sex-selective abortions and son-biased stopping rule simultaneously. I illustrate the within-twin-differences as following. The baseline model for each twin sibling $\{1,2\}$ in a household $\{j\}$ is given by

$$Y_{1j} = \lambda_j + \alpha_{1j} + \rho Boy_{1j} + \mathbf{X}_{1j}\beta + \epsilon_{1j}, \quad (1.2)$$

and

$$Y_{2j} = \lambda_j + \alpha_{2j} + \rho Boy_{2j} + \mathbf{X}_{2j}\beta + \epsilon_{2j}. \quad (1.3)$$

Taking the difference within twin pairs, I estimate

$$\Delta Y_{12,j} = \Delta \alpha_{12,j} + \rho \Delta Boy_{12,j} + \Delta \mathbf{X}_{12,j}\beta + \Delta \epsilon_{12,j}, \quad (1.4)$$

where Y_{ij} is the parental investment of child i born in family j and Boy_i is a dummy variable

that equals one if the child is a boy and zero otherwise. The parameter, λ_j , represents the shared family characteristics among twins and α_i shows the individual characteristics of each twin sibling. Outcome variables include educational expenditures (tuition, fees for tutors, books, stationery and tutoring classes) (shares), health investment (shares) and total investment. With twin fixed effects, all parental and some child’s characteristics are differenced out, except for birthweight. Parents might allocate more resources to children with heavier birthweights (Rosenzweig and J. Zhang, 2009). I control for birthweight in my twin-fixed effect model.¹² Each twin sibling might still be subject to an individual shock, such as health shocks (Yi et al., 2015). As long as an individual shock is not correlated with gender, the twin fixed effects estimator of ρ is still consistent.¹³

1.4.2 Estimation Results

In this section, I first present estimation results on the effect of gender on educational expenditures for both singletons and twins. Then I present results on the impact of gender on health and total investments.

Table 1.15 presents the OLS estimation results for parental educational investments. The fourth column shows that parents whose first child is male spend about 18 % less (48.9 U.S dollars in 2002 purchasing power parity) yearly for their eldest child’s education compared to those whose first child is female. Specifically, parents of daughters spend more on their child’s school tuition. This indicates that parents are more likely to send their daughters to better-quality schools. Additionally, daughters receive more financial support for private tutors and tutoring classes. Table 1.7 reports the findings by using educational shares as an outcome variable. Educational shares are computed as a percentage of total household expenditures. Parents spend a 3.15 % greater share of their total household expenditures on girls’ education than on boys’ education.

To validate my OLS results, I next present findings by examining within-twin differences. The impact of gender on the educational expenditures and shares estimated from the twin-fixed effects estimator is reported in Table 1.8 and Table 1.9. By utilizing the twin-fixed effects, I could only estimate the effect of gender by using twins of a different gender. The results show that compared to a male twin sibling, the female twin sibling received 25% (59.8 U.S dollars) more in

¹² I measure birthweight using z-scores.

¹³ The medical literature has established that fertility treatments increase the probability of a twin birth (Huber, 2015). Fertility treatments only became widely available in China from the 2000s onwards (Wahlberg, 2016). The occurrence of twins in my sample should be as natural as random.

yearly educational expenditures. Yearly educational shares are on average 2.15% higher for female twin siblings than male twin siblings. In sum, estimation results from both first-born singletons and twins show that boys and girls are not treated equally on education. Girls, on average, receive more monetary support for education. Unobserved parental characteristics do not seem to be a major concern in my OLS estimation.

While parents spend more on girls' education, I do not find strong evidence that parents invest more in girls' health or clothing than boys. Results for health expenditures (shares), clothing (shares), and total expenditures (shares) for singletons are presented in Table 1.10 and Table 1.11. Although parents spend slightly more on boys' health, the effect of gender is not statistically significant. Overall, parents spend 12.5% more on girls in non-twin households and this is driven by education spending. Table 1.12 and Table 1.13 present results from within-twin comparison. Consistently, I find a child's gender remains unimportant in predicting health and clothing expenditures after accounting for unobserved family-specific characteristics. Parents spend 16.8% more on girls in boy-girl-twin households.

There are two limitations of reduced-form analysis. First, I am not able to interpret my reduced-form results in the framework of economic theory. Second, there could be economies of scale in consumption between twin siblings so that I am not able to identify how parents allocate resources between their children. One of the household goods that appear to be especially "public" for twins is educational goods. Twins might share books, stationery or private tutors. Time spent by tutors providing homework assistance to each child has strong economies of scale—the marginal minute spent providing assistance has a higher pay-off in terms of total child quality when there are two children compared with one, especially for children of the exact same age. Presumably, parents do not fully discount money spent on their children if those expenditures are shared between twin siblings. Even though health spending tends to be more private for each child, parents might still purchase nutrition products or medicines for twins to share.

To understand resource allocation within the household in an economic framework and account for resource sharing among twin siblings, I introduce a structural estimation.

1.5 A Structural Analysis of Parental Decisions

I study how parents allocate resources among twin siblings using a collective model (Chiappori, 1988; Chiappori, 1992). Parents purchase a bundle of goods to maximize the weighted sum of utility function of each twin sibling. A linear technology that describes economies of scale in consumption maps purchased goods into the “after-sharing,” or “private-equivalent” consumption of the twin siblings. Observing assignable, private goods is useful because these goods are exclusively consumed by each twin and their Engel curves have closed forms after imposing fairly general parameterizations. The system formed by these Engel curves for each twin enables identifying and estimating the structural parameters that dictate the within-household distribution of resources.

1.5.1 The Parental Problem

Consider the boy-girl twin households and boys and girls are characterized by $t \in b, g$. Let e denote parental total spending on their children. Each household consumes K types of goods with prices $p = (p^1, \dots, p^k)$. Let $z = (z^1, \dots, z^k)$ be the vector of observed quantities of goods purchased by each household and $x_t = (x_t^1, \dots, x_t^k)$ be the vector of unobserved quantities of goods consumed by a twin sibling (i.e., her private good equivalents). Following Dunbar et al. (2013), I allow for economies of scale in consumption that characterizes a Gorman (1976) linear consumption technology, which converts purchased quantities by the household, z , into private good equivalents, x_t . This technology assumes the existence of a non-singular $K \times K$ matrix A and is the same for both twin siblings in a household.

Let $x = x_b + x_g$ be the sum of the inputs the twin siblings use. When they share resources, x double counts inputs. For example, let the number of books be in the first entry of x , x_g , and x_b . If both children read the same book, then the first entry in x is equal to 2. Hence, x differs from the number of inputs the parent buys for the household, z . In the example of the books, the first entry in z is equal to 1. The scale technology maps x to z , i.e., $x = A^{-1}z$. In other words, the consumption technology maps the inputs the parent buys for the household, z , to the sum of the inputs that each of the twin sibling consumes, $x_b + x_g$. If an element of the diagonal is 1 and all off-diagonal elements of row or column are equal to zero, then there is no sharing of the corresponding good and the good is private.

Each twin has a monotonically increasing, and strictly quasi-concave utility function over

goods. Let $U_t(x_t)$ denote the consumption utility of a twin sibling t over the vector of goods x_t . The parent of a twin chooses x_b and x_g that maximize the weighted sum $U_b(x_b) + \mu U_g(x_g)$, where $\mu = \mu(p/y)$ is the Pareto weight. The larger the value of Pareto weight, the greater the resulting private equivalent quantities of x_t will be. The parental resource allocation problem is

$$\max_{x_b, x_g} \tilde{U}[U_b(x_b), U_g(x_g), p, y] \quad (1.5)$$

subject to

$$z = A[x_b + x_g] \quad (1.6)$$

and

$$e = z'p, \quad (1.7)$$

where \tilde{U}_s can be interpreted as a social welfare function for a twin household (Browning et al., 2013). The solutions to this household program yield the vector of private good consumption to each twin sibling, x_g and x_b . Pricing these goods at the Lindahl (1958) type shadow prices, $A'p$, gives the resource share η_t , that is, the fraction of household spending on children devoted to each twin t . Within a household, each child faces the same shadow prices. Browning et al. (2013) show that there exists one-to-one correspondence between Pareto weight and resource share and the magnitude of former will depend on the arbitrary cardinalizations of the utility functions. Thus, I will measure the resource share of each twin.

Further, I assume that the intrahousehold allocation among twins is Pareto efficient by the standard construction of collective models. By the Second Theorem of Welfare Economics, the household program can be decomposed into two steps: first, parents allocate resources optimally across each child (to pin down η_t). Second, each twin chooses bundles of x_t to maximize her own utility function subject to a Lindahl-type shadow budget constraint, $\eta_t \cdot e = x_t \cdot A'p$.

I define a private assignable good that is consumed exclusively by one known twin sibling and there is no economies of scale in consumption. Note that, whether a private good is assignable depends on if and only data are collected on such information and provided for analysis. To get simple forms of household demand functions, I only examine the optimal allocation for private assignable goods following Dunbar et al. (2013) and Calvi (2020).¹⁴ Since a private good does not have any economies of scale in consumption (A_s will be an identity matrix), the price for a private

¹⁴ Demand functions for goods that are not privately assignable have more complex forms. See more details in Dunbar et al. (2013).

assignable good is the same as the shadow price for that particular good. Therefore, the household demand functions for private assignable goods of twins can be written as

$$\Omega_b(e, p) = \eta_b(e, p) \cdot \omega_b(\eta_b(e, p) \cdot e, p) \quad (1.8)$$

and

$$\Omega_g(e, p) = \eta_g(e, p) \cdot \omega_g(\eta_g(e, p) \cdot e, p), \quad (1.9)$$

where ω_t is the demand function of private assignable goods for each twin sibling in a household given her personal shadow budget constraint.¹⁵ Note that one cannot just use Ω_t as a measure of η_t , because different twin siblings may have very different tastes for their private assignable good. For example, in a boy-girl twin household, a female twin sibling might consume fewer household resources than her brother but still consume more clothes than him because she derives more utility from clothing consumption than her brother does. Following a methodology developed by Dunbar et al. (2013), I estimate clothing Engel curves for each twin sibling. Then, given Ω_t and e , I implicitly invert these Engel curves to solve for resource shares.

1.5.2 Identification and Estimation of Resource Shares

Resource shares are identified using Engel curves of assignable clothing. Clothing is presumably mostly a private good, especially when twin siblings are of a different gender. In Table 1.13, the regression results show that parents spend differently on each twin sibling's clothing. An Engel curve describes the relationship between budget share and total expenditure, holding prices constant. Dunbar et al. (2013) demonstrate that resource shares are identified under observability of private assignable goods, semiparametric restrictions on individual preferences for private assignable goods, and assumptions that resource shares are independent of expenditure on children.¹⁶

I focus on the commonly used price-independence generalized logarithmic demand function for each twin sibling (their preferences are PIGLOG).¹⁷ This functional form is general enough to accommodate the Almost Ideal Demand System of Deaton and Muellbauer (1980) and also conveniently generates Engel curves that are linear in the logarithm of total spending on children.

¹⁵ This is also analogous solution for the hypothetical individual problem.

¹⁶ Menon et al. (2012) show that resource shares do not exhibit much dependence on household expenditure for Italian households. Dunbar et al. (2021) find support for this independence assumption. Dunbar et al. (2013) argue that this restriction allows to identify resource shares in an Engel-curve framework without exploiting price variation and also verify that resource shares can be independent of household expenditure.

¹⁷ See Dunbar et al. (2013) for a more general discussion of identification using Engel curves of private assignable goods.

In a slight abuse of notation, the demand functions for clothing for each twin can be written in Engel curve forms as

$$\Omega_b(e) = \underbrace{\eta_b(\alpha_b + \beta_b \cdot \ln \eta_b)}_{\text{Intercept}} + \underbrace{\eta_b \cdot \beta_b}_{\text{Slope}} \cdot \ln e \quad (1.10)$$

and

$$\Omega_g(e) = \underbrace{\eta_g(\alpha_g + \beta_g \cdot \ln \eta_g)}_{\text{Intercept}} + \underbrace{\eta_g \cdot \beta_g}_{\text{Slope}} \cdot \ln e, \quad (1.11)$$

where Ω_b and Ω_g are the budget shares spent on each twin sibling's assignable clothing and e is the total household expenditure on both children in a household. The parameters of α_b , α_g , β_b , and β_g are combinations of underlying preference parameters, while η_b and η_g are the share of resources devoted to each twin sibling, respectively.

Identification of resource shares is implemented by imposing similarities of preferences for private assignable goods across twin siblings within the same household. In particular, provided that $\beta_b = \beta_g = \beta$, the slopes of the Engel curves in equation (8) and (9) are identified by linear regressions (OLS) of the assignable clothing budget shares on \ln parental spending on children ($\ln e$). Estimates of the slopes and the identity, $1 = \eta_b + \eta_g$, form a system of three equations with three unknowns η_b , η_g , and β . The full-rank assumption satisfies and allows me to identify the parameters in equations (8) and (9).

In summary, my identification strategy following (Dunbar et al., 2013) uses information on individual-level spending on non-shareable assignable goods such as clothing to “back out” the resource share. Importantly, the fact that a male twin sibling has less clothing expenditure (or share) than the female twin sibling in the boy-girl twin households does not imply that he has a smaller resource share. Instead, the link between individual clothing share and resource share is driven by the response of the former to the total spending on children. If a male twin sibling's clothing share responds more to a change in the total spending than does a female one's, then he has larger household resources than she does.

Empirically, I estimate the system using non-linear seemingly unrelated regression (NLSUR) by adding an error to each equation (8) and (9).

1.5.3 Estimation Results

Table 1.14 presents estimates for resource shares for boys and girls separately. All estimated values of the coefficients on the constant term in β_t are statistically significantly different from zero. It is reassuring that nonzero latent slopes are satisfied for the identification of the resource shares. In boy-girl twin households, the resource share for boys is lower than that for girls: girls absorb 54 % of household resources devoted to Children while boys command 45 % of those. After accounting for resource sharing among twin siblings, I still find consistent evidence with the twin-fixed effects estimator that parents spend more on girls. Additionally, I conduct structural estimation by different age groups. I find that parents allocate equal amount of household resources to their children before age 14 and girls receive significantly more resources after.

1.6 Discussion and Conclusion

Parents' favoritism toward boys could be shown by wanting to have sons more than daughters or choosing to invest more in sons than daughters. These two dimensions of favoritism often operate simultaneously, but they are not identical (Jayachandran, 2015). Although the empirical literature has extensively examined trends in the sex ratio at birth and the effect of gender on the extensive margin of fertility, much less of the existing literature has explored the impact of gender on the intensive margin of parental inputs. This lack of research is mainly due to the lack of individual child-level data, especially in urban China.

While gender is randomly determined at conception, parents' desire for sons might distort the sex ratio at birth. This, in turn, may bias simple attempts to measure the relationship between child gender and parental investments. If girls are more likely to be born with parents with less strong son preference, the practice of sex-selective abortion might lead to enhanced well-being of girls who were not aborted (Lin et al., 2014). In this case, there is a spurious relationship between child gender and parental spending.

This study uses two empirical methods to establish a causal link between gender and parental spending to shed light on intrahousehold resource allocation within urban Chinese households in Kunming, Yunnan. To deal with the endogeneity of gender, I first exploit the randomness of the first child's gender following Dahl and Moretti (2008). Second, I utilize a twin-fixed effects estimator

to further address the unobserved characteristics of parents. Both analyses indicate that parents spend more on girls' education. In addition, unobserved family attributes do not seem to bias the effects of gender from the OLS estimation of first-born singletons. While both specifications illustrate that girls receives more expenditures in education, I do not find conclusive evidence on health expenditures. Motivated by the existence of a causal link, I adopt a collective household model (Chiappori, 1988; Chiappori, 1992) and structurally estimate the total spending on each twin sibling to account for the resource sharing among twins. Consistent with the results of the twin-effects model, my estimates show that parents allocate more household resources to girls.

Future research could address the limitations of this analysis. First, the self-reported nature of the parental expenditures on education, health and clothing in the CCTS sample may lead to concerns of misreporting errors. Second, while my finding indicates that girls receive more financial support from parents, uncovering the mechanisms through which the boy-girl differences in parental investments exist is crucial. Identifying these mechanisms will allow policymakers to fully understand the relationship between child gender and parental behavior and design effective policies.

1.7 Extension

Since CCTS was sampled from Kunming, Yunnan, which was one of the few provinces with a normal sex ratio at birth (according to the 1990 China Population Census). It would be more informative to utilize data from various provinces to determine if the results hold in other parts of China where the sex ratios at birth were male-biased. In addition, boy-girl differences in expenditures might be transitory by utilizing cross-sectional data. The CCTS was sampled in 2002 and there could be underlying changes due to recent more rapid economic development in China which experiences a trend of declining sex ratio at birth since 2010.¹⁸ This decreasing trend might indicate the weakening son preference on the extensive margin of fertility. Since the gender discrimination could persist after birth in terms of parental investments (Barcellos et al., 2014; Choi and Hwang, 2015; Jayachandran and Kuziemko, 2011), will the weakening son preference also be manifested on the intensive margin? I take advantage of recent household survey, Chinese Family Panel Study (CFPS) and exploit the longitudinal nature of the CFPS, offering both the short-term and long-term impact of child gender in the context of China.

¹⁸ The data are obtained from the World Bank Development Indicator.

To my knowledge, the only work that has sought to address the boy-girl differences in expenditures across is by Choi and Hwang (2020). They have also explored the randomness of the first child's gender to examine the child gender effect on the intensive margin of parental spending on education across time in South Korea. Even though sex ratio at birth in South Korea has reached the natural range, there is still evidence that parents practice sex-selective abortions at the higher parity birth and follow son-stopping fertility rules (Choi and Hwang, 2015; Choi and Hwang, 2020). They find that first-born boys receive more financial support for private education and are expected to obtain higher education in South Korea than first-born girls. However, the gender gaps in educational inputs have narrowed substantially across time in South Korea.

I use three waves of data (2010, 2012, and 2014) from the China Family Panel Study (CFPS).¹⁹ The CFPS was launched in 2010 by the Institute of Social Science Survey at Peking University, China. It surveys 25 out of 33 provinces in China and is the largest near-nationwide, longitudinal survey in China (Xie and Lu, 2015). The survey follows members in 14,960 households and any children born to these households over four years. For my analysis, I utilize both the child and household questionnaires. These two surveys provide a unique opportunity to construct a sample of firstborn children who are linked to their mothers. Furthermore, for every child under 16 years at the time of the survey, a questionnaire is administered to their parents, which contains direct and detailed measures of educational investment. These features make the CFPS an ideal source for studying effects on parental investment trajectories over time.

The outcome variable analyzed is parental educational expenditures, defined as the total amount parents have spent on the child's education in the past 12 months. Educational expenditures include school tuition, miscellaneous school fees, spending on private tutors, extracurricular activities, books and supplies, and other out-of-school education-related expenses. For children under primary school age, educational expenditures include tuition and miscellaneous fees charged by daycare and kindergarten, as well as expenses on other early childhood education activities. Educational expenditures include only the amount spent by the household, excluding any government subsidies or donations from extended family. Control variables include the gender of the child, age of the child at the time of the survey, child's age squared, mother's age at the time of the survey, mother's age squared, urban/rural residence at the time of the survey, and mother's educational attainment (less than primary, primary, middle school, high school and above).

¹⁹ The CFPS design follows the Panel Survey of Income Dynamics (PSID) in the United States.

Each child survey contains around 8600 observations. As educational expenditure data are collected only for children older than 1 year, the analytic sample is restricted to include children aged 1 to 15 years. After linking mothers with their first-born children and ensuring the completeness of the outcome variable and covariates, I discard first-born children who are twins or triplets. These restrictions result in a sample containing 5605, 5194, and 4544 child-mother pairs in each wave of survey (2010, 2012, and 2014). For the fixed-effect analysis, I apply the same restriction criteria and obtain 2800 child-mother pairs across three years.

To examine whether the child gender gap changes over time, I modify (1.1) so that the coefficient on Boy_i can vary across years. This is shown by

$$y_i = \beta_0 + \sum_t \beta_t(Boy_i \times \{Year_i = t\}) + \beta_2 \mathbf{X}_i + \epsilon_i, \quad (1.12)$$

where $Year_i$ is survey year of child i , and $\{Year_i = t\}$ is an indicator function for the survey years (2010, 2012, 2014). Both (1.1) and (1.12) are estimated by ordinary least squares (OLS), which captures the average effect of a child's gender on parental investments, if the gender is randomly distributed in the population or exogenous conditional on covariates.

Following Dahl and Moretti (2008), I leverage the randomness of the first child's gender. Empirical research has established the fact that the sex ratio at birth of first children is not distorted in China (Almond et al., 2019; Y. Chen et al., 2013; Ebenstein, 2010; Garcia, 2020). This fact holds in the sample that I analyze. In my analytic sample, the sex ratios (male to female) at first birth are 51.81%, 52.03%, and 51.74%, respectively, in three waves of children surveys. The natural sex ratio found in the sample verifies the randomness of the first child's gender.

In (1.1), Boy_i represents whether the first child is male and the estimation sample only includes first-borns. It is important to note that even though CFPS is not a twin-survey, my twin-fixed effects model in the above analysis demonstrates the consistent result as OLS regression only including first-borns in the estimation. Thus estimation relying on the first child's gender yields consistent estimates and also estimates the total effect of child gender on parental expenditures, including any indirect effects through later fertility choices (Bharadwaj, Dahl, et al., 2014).

In addition to this OLS estimation, I take advantage of the longitudinal nature of CFPS and present a fixed-effects model. This accounts for some unobservable factors and provides further evidence of how boy-girl differences vary over time. The regression model is

$$y_{it} = \beta_0 + \beta_1 Boy_i + \beta_2 Boy_i \times Year_{12} + \beta_3 Boy_i \times Year_{14} + \alpha_i + \alpha_t + \alpha_p + \epsilon_{itp}, \quad (1.13)$$

where α_i is an individual fixed effect, α_t represents a year fixed effect, and α_p indicates a province fixed effect. Since child gender is a time-invariant variable, I can only identify interaction terms of the treatment variable and survey year. The time-variant control variable in the fixed-effects model is the urban/rural residence.

Figure 1.3 presents the average effects of child gender on parental educational spending across time. Table 1.15 displays that compared with a firstborn girl, a firstborn boy receives about 4 percentage points more monetary investments on education in 2010, while parents spend more on girls' education in 2012 and 2014. The gender effect is statistically significant at the 95% level in 2012 and parents spend around 410 Yuan more on girls (60 dollars in 2020 purchasing-power parity)²⁰. Boy-girl differences in educational investments have declined over time and the reduction in the gender gap is also supported by the individual-fixed effect model in Table 1.16.

Next, I investigate whether the gender gaps in parents' monetary inputs vary significantly by urban/rural residence, mother's educational attainment, and child's age. As shown in Table 1.17, the coefficient on *Boy* is negative and statistically significant. This coefficient indicates that on average, parents spend 25% more on girls' education in urban areas in 2012. The impact of child gender is more statistically pronounced across mother's education level. Table 1.18 shows that uneducated mothers are more likely to spend on boys' education, while girls receive 30% more financial support for education from more educated mothers. As indicated in Table 1.19, I do not find strong statistical evidence that a child's gender affects educational investments across different age groups.

1.8 Why Boy-Girl Differences in Spending Exist?

The observed differences in educational spending could be due to several reasons. In terms of non-economic returns, the gender gap in the intrahousehold resource allocation may reflect parental favoritism towards girls. Apart from non-economic returns, a strand of literature provides evidence

²⁰ I report the coefficient from the state-fixed effect model

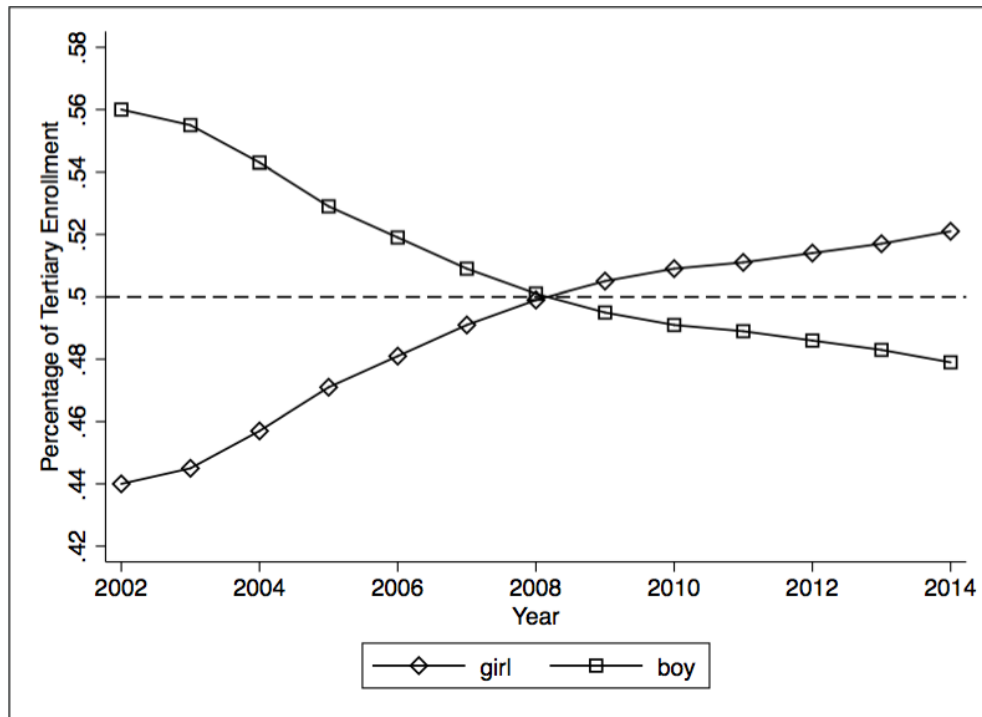
that lower effort costs of learning and greater economic benefits for females might incentivize parents to invest more in daughters. Girls tend to earn better grades in middle or high school and parents expect daughters to obtain a higher level of educational attainment.²¹ Parents spend more on girls' education to reinforce their success in learning. One potential female advantage in learning is due to the higher incidence of boys having disciplinary and behavioral problems. They might spend far fewer hours studying (Goldin et al., 2006).

In addition to lower non-pecuniary costs, investments in girls might confer them more benefits in both the future labor market and the marriage market. With the economic development, the female employment rate might rise faster than the male employment rate. There are several reasons to believe this is the case. As economic activity shifts away from agriculture and manufacturing toward services, women have a comparative advantage in "brain-based" work, so the female employment rises (Pitt et al., 2012; Rosenzweig and J. Zhang, 2013). Reductions in fertility, better control of the timing of fertility, technology of home production improved, marriage delay, and resurgence of feminism are other channels that free up women's time so that their life-cycle labor force participation rate increases (Jayachandran, 2015). The relative rise in female employment in urban China has also been accompanied by a higher rate of return to female schooling (J. Zhang et al., 2005). Additionally, discrimination against women appears to be lower at higher levels of education (Chiappori et al., 2009). With more job opportunities and higher returns to education, forward-looking families will invest more in girls. Another reason parents spend more on girls is to increase their competitiveness in the marriage market. Marriage-market returns to education typically increase much more for women than for men (Chiappori et al., 2009). With educated men being in short supply, as implied by the reversal of the college gender gap in China, educated men are more likely to marry educated women and less likely to marry uneducated women because of the excess supply of educated women. Parents might be induced to invest more in girls in competition for the scarce quality males.

Lastly, parents invest more in girls, indicating that they save more when they have boys. Due to the male-biased sex ratio in the premarital cohort, families with sons raise their savings in response to rising pressure in the marriage market because the newlywed's home is usually supplied by the groom's family (Wei and X. Zhang, 2011). Households with daughters might be induced to reduce their savings to free ride on a future son-in-law's savings.

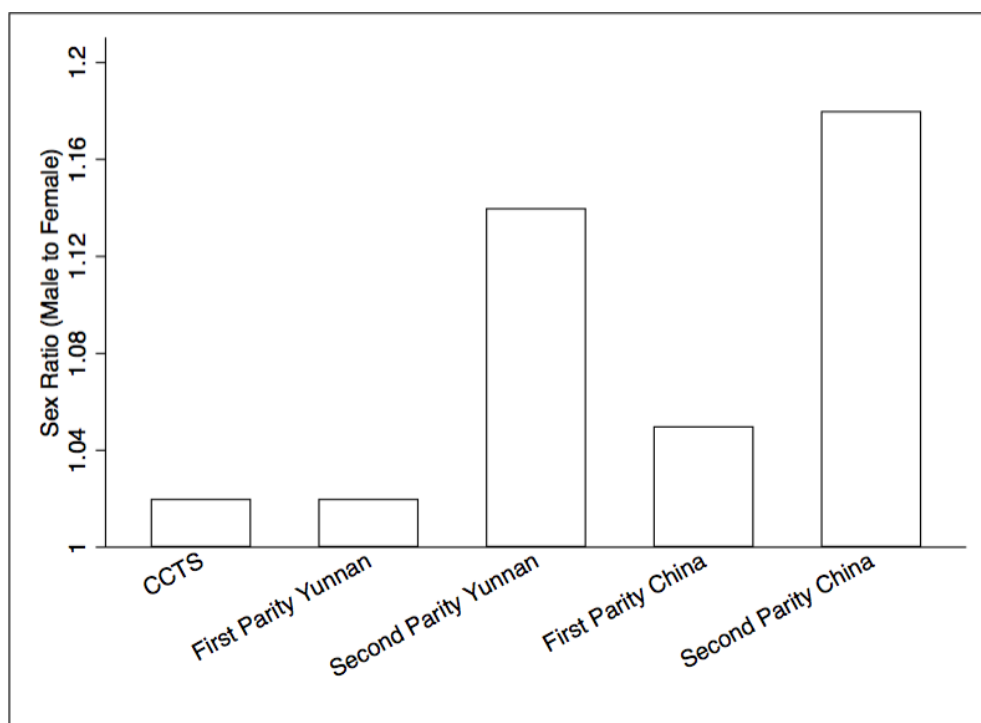
²¹ Girls' better educational attainment is observed from my data

Figure 1.1: Percentage of Tertiary Enrollment by Gender in China



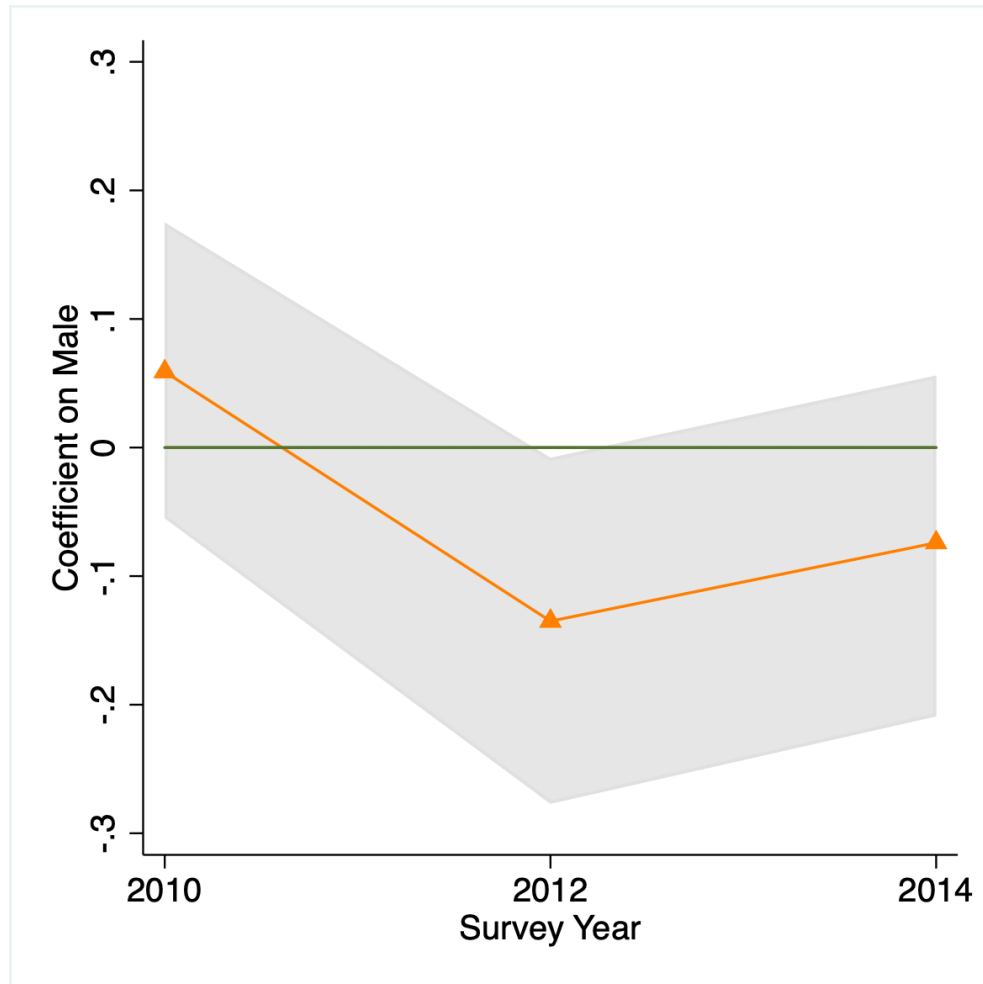
Notes: The data are obtained from the Ministry of Education of the People's Republic of China.

Figure 1.2: Sex Ratio at Birth by Parity in Urban China



Notes: The sex ratio at birth (male to female) for urban Yunnan and urban China is collected from 10 percent Sampling of the 1990 Yunnan Population Census and 1990 China Population Census.

Figure 1.3: Coefficient on Male Across Time



Notes: OLS results for educational investments, by child's gender. I report the coefficient with state-fixed effect and shaded areas around each estimate represent the 95% confidence interval. All estimations use survey weights.

Table 1.1: Sample Construction

Criteria	Singletons	Twins (Pairs)
Raw Data	491	469
First Borns	482	444
No-missing Controls	459	440
No-missing Outcomes	439	432

Notes: After restricting my analysis to the urban sample, there are 491 singletons (including first-borns and second-borns) and 469 pairs of twins.

Table 1.2: Descriptive Statistics by Twinning: Parents

	Non-Twin	Twin	T-test	
	Parents		Difference	P-Value
Mother's Years of Schooling	10.54 (2.98)	10.21(3.51)	0.33	[0.15]
Father's Years of Schooling	10.96 (3.40)	10.90 (3.48)	0.06	[0.80]
Mother's Age	36.41 (4.21)	36.88 (4.74)	-0.47	[0.13]
Father's Age	38.94 (5.03)	39.32 (5.51)	-0.38	[0.29]
Mother's Age at First Birth	25.00 (3.04)	25.52 (3.40)	-0.52	[0.02]
Number of Households	439	432		

Notes: I report the mean of each variable and standard deviations are in parentheses.

Table 1.3: Descriptive Statistics by Gender: Parents of Singletons

	Girls	Boys	T-test	
			Difference	P-value
Father's Age	39.18 (4.877)	38.70 (5.174)	0.48	[0.319]
Mother's Age	36.39 (4.103)	36.43 (4.331)	-0.04	[0.915]
Mother's Age at First Birth	24.95 (2.884)	25.06 (3.202)	-0.12	[0.684]
Father's Years of Education	10.97 (3.340)	10.95 (3.474)	0.02	[0.955]
Mother's Years of Education	10.60 (2.902)	10.47 (3.070)	0.13	[0.65]
N	220	219		

Notes: I report the mean of each variable and standard deviations are in parentheses.

Table 1.4: Descriptive Statistics by Gender: Non-Twins

	Girls	Boys	Girls	Boys	T-test	
	Expenditures (Yuan/RMB)		Budget Shares		Difference	P-Value
School Tuitions	1016.20 (2048.3)	761.58 (1195.4)	19.28 (16.13)	17.25 (15.05)	2.03	[0.17]
Books and Stationary	281.08 (241.4)	249.20 (226.1)	6.54 (5.427)	5.81 (4.267)	0.73	[0.12]
Private Tutors and Tutoring Classes	414.03 (782.8)	336.07 (809.5)	6.43 (10.53)	5.12 (9.315)	1.32	[0.17]
Education Investment	1711.30 (2412.5)	1346.85 (1629.5)	32.25 (19.81)	28.18 (18.36)	4.07	[0.03]
Health Investment	285.59 (1069.9)	262.64 (819.1)	4.92 (8.588)	5.34 (9.807)	-0.424	[0.63]
Clothing	397.66 (312.1)	376.53 (288.6)	9.07 (6.270)	9.10 (6.039)	-0.03	[0.96]
Total Children's Investment	2394.55 (2892.8)	1986.02 (1956.5)	46.23 (22.77)	42.62 (22.59)	3.61	[0.09]
N	220	219	220	219		

Notes: I report the mean of each variable and standard deviations are in parentheses. Educational investments include school tuition and money spent on purchasing books and stationary, hiring home tutors, and attending tutoring classes. Educational investments, health investments, and clothing are included in the total investments. The expenditures are measured in 2002 RMB and the exchange rate for RMB to US dollars was 8.28. 100 dollars in 2002 are equivalent to 145.43 dollars in 2020. Budget shares are computed as a percentage of total household expenditures and are multiplied by 100.

Table 1.5: Descriptive Statistics by Gender: Different-gender Twins

	Girls	Boys	Girls	Boys	T-test	
	Expenditures (Yuan/RMB)		Budget Shares		Difference	P-Value
School Tuitions	973.99 (1134.9)	890.47 (1001.0)	16.48 (12.24)	15.22 (10.32)	1.26	[0.46]
Books and Stationary	214.90 (273.2)	199.13 (248.3)	3.80 (4.109)	3.55 (4.073)	0.25	[0.68]
Private Tutors and Tutoring Classes	235.93 (614.1)	182.64 (489.8)	2.36 (4.877)	1.72 (3.824)	0.64	[0.33]
Education Investment	1424.82 (1603.4)	1272.24 (1369.1)	22.64 (12.84)	20.49 (11.11)	2.15	[0.23]
Health Investment	194.23 (508.7)	117.09 (154.7)	3.56 (7.080)	2.90 (4.471)	0.66	[0.45]
Clothing	317.18 (340.9)	304.76 (301.9)	5.72 (3.922)	5.64 (3.933)	0.08	[0.90]
Total Children's Investment	1936.23 (1828.1)	1694.09 (1526.3)	31.92 (13.48)	29.03 (12.56)	2.89	[0.14]
N	91	91	91	91		

Notes: I report the mean of each variable and standard deviations are in parentheses. Educational investments include school tuition and money spent on purchasing books and stationary, hiring home tutors, and attending tutoring classes. Educational investments, health investments, and clothing are included in the total investments. The expenditures are measured in 2002 RMB and the exchange rate for RMB to US dollars was 8.28. 100 dollars in 2002 are equivalent to 145.43 dollars in 2020. Budget shares are computed as a percentage of total household expenditures and are multiplied by 100.

Table 1.6: OLS Estimates: Effects of Gender on Educational Expenditures

Outcome Variables (log)	School Tuitions	Books and Stationery	Private Tutor and Tutoring Classes	Education Investment
Male	-0.238** [0.103]	-0.007 [0.085]	-0.212* [0.273]	-0.181** [0.077]
Elementary School (dummy)	2.23 [1.778]	2.311* [1.210]	0.265 [1.694]	2.637 [1.885]
Middle School (dummy)	2.686 [1.777]	2.815** [1.208]	0.286 [1.692]	3.012 [1.884]
High School (dummy)	3.604** [1.779]	2.811** [1.216]	0.333 [1.704]	3.651* [1.888]
Birthweight (z-score)	0.081 [0.060]	0.105** [0.052]	0.208 [0.160]	0.121** [0.048]
Mother's Age	0.126 [0.150]	-0.031 [0.137]	0.351 [0.346]	0.141 [0.120]
Father's Age	-0.037 [0.052]	0.111 [0.074]	-0.168 [0.141]	0.012 [0.038]
Mother's Age Squared	-0.001 [0.002]	0.001 [0.002]	-0.004 [0.005]	-0.002 [0.002]
Father's Age Squared	-0.00005 [0.001]	-0.002* [0.001]	0.002 [0.002]	-0.0004 [0.001]
Mother's Years of Education	0.028 [0.021]	0.046** [0.018]	0.220*** [0.058]	0.051*** [0.017]
Father's Years of Education	-0.017 [0.018]	0.056*** [0.017]	0.229*** [0.054]	0.042*** [0.015]
Mean	889.18	265.18	375.13	1529.49
Number of Children	439	439	439	439
R-squared	0.228	0.217	0.206	0.328

Note: Robust standard errors clustered at the household-level are shown in parentheses. Regressions are based on the sample of first-born singletons observed in urban areas of Kunming. The birthweight is measured in terms of z-score. The educational investments include school tuitions, expenditures on books, stationery, private tutors, and tutoring classes. The average for different educational investments is measured in Yuan (or RMB). The expenditures are measured in 2002 RMB and the exchange rate for RMB to US dollars was 8.28. 100 dollars in 2002 are equivalent to 145.43 dollars in 2020.

Table 1.7: OLS Estimates: Effects of Gender on Educational Shares

Outcome Variables (Budget Shares)	School Tuitions	Books and Stationery	Private Tutor and Tutoring Classes	Education Investment
Male	-1.39 [1.326]	-0.544 [0.455]	-1.218 [0.894]	-3.152* [1.697]
Elementary (dummy)	9.013* [5.314]	4.859*** [0.768]	-2.91 [6.735]	10.962 [10.446]
Middle School (dummy)	13.170** [5.272]	6.336*** [0.766]	-3.506 [6.720]	12.33 [10.410]
High School (dummy)	24.496*** [5.557]	5.055*** [0.949]	-3.983 [6.721]	25.568** [10.515]
Birthweight (z-score)	-1.459** [0.712]	-0.302 [0.307]	1.108* [0.580]	-0.654 [0.976]
Mother's Age	3.149 [1.935]	0.203 [0.793]	0.198 [1.113]	3.551 [2.324]
Father's Age	0.043 [0.810]	0.308 [0.407]	-0.118 [0.465]	0.232 [1.190]
Mother's Age Squared	-0.037 [0.028]	-0.003 [0.011]	-0.003 [0.016]	-0.043 [0.033]
Father's Age Squared	-0.00044 [0.012]	-0.002 [0.005]	0.005 [0.007]	0.002 [0.017]
Mother's Years of Education	-0.107 [0.324]	0.005 [0.109]	0.434** [0.199]	0.332 [0.394]
Father's Years of Education	-0.831*** [0.269]	-0.043 [0.093]	0.648*** [0.201]	-0.226 [0.356]
Observations	439	439	439	439
R-squared	0.221	0.049	0.138	0.162

Notes: Robust standard errors clustered at the household-level are shown in parentheses. The birthweight is measured in terms of z-score. Budget shares are computed as a percentage of total household expenditure. Budget shares are multiplied by 100.

Table 1.8: Fixed-Effect Estimates: Effects of Gender on Educational Expenditures

Outcome Variables (log)	School Tuitions	Books and Stationery	Private Tutor and Tutoring Classes	Education Investment
Male	-0.201 [0.128]	-0.170** [0.086]	-0.27 [0.169]	-0.250* [0.135]
Birthweight (z-score)	0.05 [0.042]	-0.015 [0.054]	0.079 [0.116]	0.063 [0.050]
Mean	932.23	207.02	209.29	1348.53
Number of Children			864	
Paris of Twins			432	

Notes: Robust standard errors adjusted for within-twin-pair correlation are shown in parentheses. The coefficient of Male is identified by the different-gender twins. The coefficient of Birthweight is identified by all twins. Regressions are based on the sample of twins observed in urban areas of Kunming. The birthweight is measured in terms of z-score. The average for different educational investments is measured in Yuan (or RMB). The expenditures are measured in 2002 RMB and the exchange rate for RMB to US dollars was 8.28. 100 dollars in 2002 are equivalent to 145.43 dollars in 2020.

Table 1.9: Fixed-Effect Estimates: Effects of Gender on Educational Shares

Outcome Variables (Budget Shares)	School Tuitions	Books and Stationery	Private Tutor and Tutoring Classes	Education Investment
Male	-1.265 [1.067]	-0.251** [0.120]	-0.635** [0.289]	-2.151** [1.085]
Birthweight (z-score)	-0.11 [0.719]	0.012 [0.034]	0.248* [0.149]	0.15 [0.760]
Number of Children			864	
Pairs of Twins			432	

Notes: Robust standard errors adjusted for within-twin-pair correlation are shown in parentheses. The coefficient of Male is identified by the different-gender twins. The coefficient of Birthweight is identified by all twins. Regressions are based on the sample of twins observed in urban areas of Kummung. The birthweight is measured in terms of z-score. Budget shares are computed as a percentage of total household expenditure. Budget shares are multiplied by 100.

Table 1.10: OLS Estimates: Effects of Gender on Other Expenditures

Outcome Variables (log)	Health Investment	Clothes Expenditures	Total Expenditures
Male	0.053 [0.242]	-0.037 [0.065]	-0.126* [0.065]
Age	-0.791*** [0.301]	-0.055 [0.069]	-0.310*** [0.089]
Age Squared	0.026** [0.013]	0.004 [0.003]	0.015*** [0.004]
Birthweight (z-score)	-0.225 [0.150]	0.088** [0.042]	0.109** [0.042]
Mother's Age	0.113** [0.048]	0.001 [0.010]	0.022* [0.012]
Mother's Years of Education	0.085* [0.044]	0.061*** [0.011]	0.077*** [0.012]
Mean	274.14	387.12	2190.75
Observations	439	439	439
R-squared	0.058	0.093	0.225

Notes: Robust standard errors clustered at the household-level are shown in parentheses. Regressions are based on the sample of first-born singletons observed in urban areas of Kunming. The birthweight is measured in terms of z-score. The total investment is calculated by the sum of educational investment, health investment and cloth expenditures. The average for different investments is measured in Yuan (or RMB). The expenditures are measured in 2002 RMB and the exchange rate for RMB to US dollars was 8.28. 100 dollars in 2002 are equivalent to 145.43 dollars in 2020.

Table 1.11: OLS Estimates: Effects of Gender on Other Expenditure Shares

Outcome Variables (Budget Shares)	Health Investment	Clothes Expenditures	Total Expenditures
Male	0.469 [0.860]	-0.072 [0.588]	-3.401 [2.096]
Age	-3.203** [1.529]	1.196* [0.689]	-4.917* [2.598]
Age Squared	0.105* [0.063]	-0.052* [0.028]	0.215** [0.106]
Birthweight (z-score)	-0.469 [0.547]	-0.064 [0.303]	-1.286 [1.210]
Mother's Age	0.395** [0.164]	0.051 [0.106]	1.330*** [0.320]
Mother's Years of Education	-0.176 [0.176]	-0.248** [0.097]	-0.219 [0.362]
Observations	439	439	439
R-squared	0.045	0.021	0.087

Notes: Robust standard errors clustered at the household-level are shown in parentheses. Regressions are based on the sample of first-born singletons observed in urban areas of Kunming. The birthweight is measured in terms of z-score. Budget shares are computed as a percentage of total household expenditure. Budget shares are multiplied by 100.

Table 1.12: Fixed-Effect Estimates: Effects of Gender on Other Expenditures

Outcome Variables (log)	Health Investment	Clothes Expenditures	Total Expenditures
Male	-0.156 [0.190]	-0.064 [0.062]	-0.168** [0.082]
Birthweight (z-score)	-0.420** [0.162]	0.014 [0.039]	0.005 [0.046]
Mean	155.65	321.14	1815.15
Number of Children		864	
Pairs of Twins		432	

Notes: Robust standard errors adjusted for within-twin-pair correlation are shown in parentheses. The coefficient of Male is identified by the different-gender twins. The coefficient of Birthweight is identified by all twins. The birthweight is measured in terms of z-score. The total investment is calculated by the sum of educational investment, health investment and cloth expenditures. The average for different investments is measured in Yuan (or RMB). The expenditures are measured in 2002 RMB and 100 RMB in 2002 are equivalent to 157.38 RMB in 2021. The exchange rate for RMB to US dollars was 6.45 in 2021.

Table 1.13: Fixed-Effect Estimates: Effects of Gender on Other Expenditure Shares

Outcome Variables (Budget Shares)	Health Investment	Clothes Expenditures	Total Expenditures
Male	-0.674 [0.740]	-0.075 [0.154]	-2.901** [1.271]
Birthweight (z-score)	-1.077 [0.698]	-0.001 [0.088]	-0.927 [0.983]
Number of Children		864	
Pairs of Twins		432	

Notes: Robust standard errors adjusted for within-twin-pair correlation are shown in parentheses. The coefficient of Male is identified by the different-gender twins. The coefficient of Birthweight is identified by all twins. The birthweight is measured in terms of z-score. The total investment is calculated by the sum of educational investment, health investment and cloth expenditures. Budget shares are computed as a percentage of total household expenditure. Budget shares are multiplied by 100.

Table 1.14: Structural Estimation: Resource Shares

	All Samples	Age 6-9	Age 10-13	Age 14-18
Girls	0.5407 [0.074]	0.4942 [0.036]	0.5008 [0.002]	0.6626 [0.2048]
Boys	0.4593 [0.073]	0.5058 [0.033]	0.4992 [0.002]	0.3374 [0.2043]
Pairs of Twins	91	21	27	43

Notes: Nonlinear seemingly unrelated regression estimates are based on data from Chinese Child Twins Survey. Robust standard errors are clustered at the household-level.

Table 1.15: OLS Estimates: Effects of Gender on Educational Expenditures

	2010 (1)	2010 (2)	2012 (1)	2012 (2)	2014 (1)	2014 (2)
Child's characteristics						
Male	0.03986 [0.060]	0.05986 [0.059]	-0.15552** [0.070]	-0.13585** [0.069]	-0.07466 [0.069]	-0.07461 [0.068]
Age	1.32164*** [0.033]	1.32685*** [0.032]	1.30766*** [0.041]	1.31524*** [0.041]	1.48608*** [0.041]	1.48974*** [0.041]
Age squared	-0.05895*** [0.002]	-0.05922*** [0.002]	-0.06267*** [0.002]	-0.06248*** [0.002]	-0.06866*** [0.002]	-0.06851*** [0.002]
Mother's characteristics						
Mother's age	0.23259*** [0.048]	0.20824*** [0.048]	0.38896*** [0.056]	0.32500*** [0.056]	0.21013*** [0.056]	0.18316*** [0.056]
Mother's age squared	-0.00319*** [0.001]	-0.00288*** [0.001]	-0.00488*** [0.001]	-0.00411*** [0.001]	-0.00265*** [0.001]	-0.00241*** [0.001]
Less than primary	-1.63996*** [0.110]	-1.19068*** [0.114]	-2.16761*** [0.126]	-1.54528*** [0.130]	-1.84128*** [0.117]	-1.36677*** [0.121]
Primary	-0.85695*** [0.102]	-0.72414*** [0.103]	-1.41286*** [0.108]	-1.08202*** [0.109]	-1.05646*** [0.103]	-0.82533*** [0.105]
Middle school	-0.59394*** [0.093]	-0.51761*** [0.093]	-0.75074*** [0.096]	-0.56346*** [0.097]	-0.70690*** [0.096]	-0.55298*** [0.098]
Urban	0.78989*** [0.068]	0.60124*** [0.070]	0.12747*** [0.038]	0.11983*** [0.036]	0.05498 [0.035]	0.04735 [0.035]
Province fixed effect	No	Yes	No	Yes	No	Yes
Observations	5,605	5,605	5,194	5,194	4,544	4,544
Mean dependent variable	1354	1354	2735	2735	3265	3265

Notes: Standard errors, shown in parentheses, are clustered at the household level. Educational investments are measured in Yuan (RMB).

Table 1.16: Fixed Effect Estimates: Effects of Gender on Educational Expenditures Across Time

Variables	Educational Investment (ln)
Male * Year 2012	-0.053 [0.128]
Male * Year 2014	-0.207 [0.151]
Urban	0.222*** [0.054]
Observations	8,400
Mean dependent variable	2600

Notes: Standard errors, shown in parentheses, are clustered at the household level. Educational spending is measured in Yuan (RMB).

Table 1.17: OLS Estimates: Effects of Gender on Educational Expenditures: by Urban/Rural Residence

	2010	2010	2012	2012	2014	2014
	Urban	Rural	Urban	Rural	Urban	Rural
Child's characteristics						
Male	0.02963 [0.095]	0.08099 [0.074]	-0.24808** [0.103]	-0.06268 [0.091]	-0.03123 [0.099]	-0.06648 [0.094]
Age	1.49942*** [0.054]	1.23582*** [0.039]	1.27239*** [0.068]	1.34851*** [0.050]	1.52949*** [0.060]	1.44528*** [0.055]
Age squared	-0.07126*** [0.003]	-0.05240*** [0.002]	-0.06506*** [0.004]	-0.06045*** [0.003]	-0.07314*** [0.003]	-0.06349*** [0.003]
Mother's characteristics						
Mother's age	0.12966 [0.081]	0.19759*** [0.061]	0.50351*** [0.089]	0.15394** [0.071]	0.25712*** [0.081]	0.0723 [0.077]
Mother's age squared	-0.00175* [0.001]	-0.00277*** [0.001]	-0.00615*** [0.001]	-0.00205** [0.001]	-0.00328*** [0.001]	-0.00108 [0.001]
Less than primary	-1.44746*** [0.188]	-0.66231*** [0.184]	-1.12687*** [0.213]	-1.13690*** [0.190]	-1.52364*** [0.206]	-0.82856*** [0.197]
Primary	-0.91940*** [0.149]	-0.22499 [0.175]	-0.99529*** [0.167]	-0.61709*** [0.175]	-0.94335*** [0.146]	-0.28759 [0.185]
Middle school	-0.57316*** [0.114]	-0.06291 [0.173]	-0.54772*** [0.124]	-0.19045 [0.168]	-0.61067*** [0.119]	-0.10977 [0.183]
Observations	2,396	3,209	2,150	2,994	2,073	2,422
R-squared	0.43	0.491	0.383	0.394	0.501	0.45

Notes: Standard errors, shown in parentheses, are clustered at the household level.

Table 1.18: OLS Estimates: Effects of Gender on Educational Expenditures: by Mother's Education

	2010				2012				2014			
	Less than primary	Primary	Middle school	High school and above	Less than primary	Primary	Middle school	High school and above	Less than primary	Primary	Middle school	High school and above
Child's characteristics												
Male	0.25231** [0.128]	0.18772* [0.112]	-0.13501 [0.097]	0.10389 [0.148]	-0.14092 [0.177]	-0.22517 [0.138]	-0.02977 [0.103]	-0.31256** [0.158]	0.30802* [0.159]	-0.12272 [0.133]	-0.10915 [0.114]	-0.32097** [0.149]
Age	1.00587*** [0.078]	1.22893*** [0.062]	1.39325*** [0.052]	1.62617*** [0.080]	1.01213*** [0.113]	1.27899*** [0.087]	1.50259*** [0.061]	1.40153*** [0.091]	1.17497*** [0.109]	1.46602*** [0.088]	1.52580*** [0.067]	1.61481*** [0.085]
Age squared	-0.04141*** [0.004]	-0.05206*** [0.003]	-0.06297*** [0.003]	-0.08157*** [0.004]	-0.04200*** [0.006]	-0.05828*** [0.005]	-0.07321*** [0.003]	-0.07461*** [0.005]	-0.04650*** [0.005]	-0.06607*** [0.005]	-0.07156*** [0.003]	-0.08137*** [0.004]
Mother's characteristics												
Mother's age	0.23348** [0.100]	0.0653 [0.093]	0.19510** [0.079]	0.24369** [0.118]	0.29840** [0.116]	0.29154** [0.126]	0.22004** [0.092]	0.58994*** [0.127]	0.0873 [0.130]	-0.08481 [0.127]	0.26864*** [0.087]	0.42001*** [0.132]
Mother's age squared	-0.00314** [0.001]	-0.00092 [0.001]	-0.00294*** [0.001]	-0.00297** [0.002]	-0.00347** [0.001]	-0.00382** [0.002]	-0.00290** [0.001]	-0.00741*** [0.002]	-0.00123 [0.002]	0.00099 [0.002]	-0.00368*** [0.001]	-0.00489*** [0.002]
Urban	0.30579* [0.177]	0.31893** [0.142]	0.56435*** [0.106]	1.17256*** [0.187]	0.70043*** [0.227]	0.10756 [0.096]	0.07123 [0.050]	0.09929* [0.058]	0.03182 [0.197]	0.11921** [0.052]	0.01628 [0.041]	0.04727 [0.081]
Observations	1,150	1,317	2,087	1,050	861	1,161	2,048	1,123	764	1,110	1,596	1,074
R-squared	0.373	0.467	0.517	0.528	0.326	0.369	0.468	0.442	0.39	0.423	0.495	0.568

Notes: Standard errors, shown in parentheses, are clustered at the household level.

Table 1.19: OLS Estimates: Effects of Gender on Educational Expenditures: by Child's Age

	2010			2012			2014		
	Age 1-5	Age 6-10	Age 11-15	Age 1-5	Age 6-10	Age 11-15	Age 1-5	Age 6-10	Age 11-15
Child's characteristics									
Male	0.18769 [0.164]	0.00274 [0.092]	0.05908 [0.075]	-0.14632 [0.178]	-0.12687 [0.094]	-0.13882 [0.104]	-0.01756 [0.197]	0.00293 [0.092]	-0.14231* [0.084]
Mother's characteristics									
Mother's age	0.87076*** [0.119]	0.11168 [0.113]	0.30827*** [0.117]	1.31759*** [0.154]	0.14025 [0.092]	0.51772*** [0.150]	1.18851*** [0.187]	0.06349 [0.092]	0.28002*** [0.123]
Mother's age squared	-0.01160*** [0.002]	-0.00153 [0.002]	-0.00394*** [0.001]	-0.01719*** [0.002]	-0.00187 [0.001]	-0.00623*** [0.002]	-0.01527*** [0.003]	-0.00087 [0.001]	-0.00344** [0.002]
Less than primary	-0.96317*** [0.312]	-1.01458*** [0.180]	-1.05447*** [0.145]	-2.01160*** [0.354]	-1.47877*** [0.195]	-0.96375*** [0.190]	-0.77535* [0.400]	-1.48811*** [0.193]	-1.12730*** [0.133]
Primary	-0.27463 [0.288]	-0.59870*** [0.147]	-0.65048*** [0.129]	-0.86470*** [0.303]	-0.84126*** [0.150]	-0.68329*** [0.169]	0.26297 [0.303]	-0.73240*** [0.137]	-0.86756*** [0.122]
Middle school	0.02237 [0.239]	-0.48966*** [0.130]	-0.52899*** [0.118]	-0.19976 [0.225]	-0.43824*** [0.133]	-0.30584** [0.151]	0.39035 [0.244]	-0.47369*** [0.129]	-0.59503*** [0.110]
Urban	1.08815*** [0.199]	0.68671*** [0.110]	0.30037*** [0.085]	0.16701** [0.070]	0.04631 [0.037]	0.14580* [0.078]	0.0604 [0.083]	-0.01777 [0.037]	0.08480* [0.046]
Observations	1,619	1,745	2,241	1,560	1,620	2,014	1,432	1,477	1,635
R-squared	0.152	0.216	0.225	0.207	0.204	0.134	0.146	0.185	0.178

Notes: Standard errors, shown in parentheses, are clustered at the household level.

Chapter 2

Young Children and Parents’ Labor Supply during COVID-19

2.1 Introduction

The onset of the COVID-19 pandemic prompted unprecedented policy responses from American state and federal governments to implement social distancing, including broad orders for closures of schools and childcare providers. While these closures may have helped reduce disease spread, they induced concerns over their potential impacts on parents’ ability to work, particularly for women. Would the sudden need to provide childcare for children no longer in school or daycare prohibit workers from continuing to work or from finding new jobs? In the immediate aftermath of the school and childcare provider closures, several analysts suggested this could be the case (Alon et al., 2021; Bayham and Fenichel, 2020; Dingel et al., 2020; Rojas et al., 2020).

At first glance, there is a strong inclination to think school and daycare center closures will lead to parents being unable to work since they raise the cost of childcare. This presumably leads to parents substituting their own time to care for children. Indeed, the literature on the effects of childcare costs on labor supply of mothers has typically, although not always, found that higher costs are associated with lower labor force participation (D. Blau and Currie (2006) provide thorough reviews). The context surrounding the unfolding of the COVID-19 pandemic, however, makes the theoretical prediction less straightforward. Rapid spread of the disease led to schools and childcare

providers closing with minimal warning, giving parents little flexibility in finding childcare. At the same time, stay-at-home policies and business closures might have resulted in family or neighbors being available to help provide care. Availability of such informal sources of childcare has been noted in the literature as potentially blunting the reliance on formal sources (D. M. Blau and Robins, 1988; Heckman, 1974). The extent of such availability in the COVID-19-economy is unclear, though, given social distancing efforts among the population. Finally, as the pandemic unfolded, many employers implemented remote working policies and technologies, allowing parents to work from home much more than in the past. In sum, while there is a straightforward substitution mechanism suggesting COVID-19 school and daycare closures could negatively affect labor supply, the pandemic creates a situation with unique features that could augment or limit the impact of that mechanism.

In this paper, we consider the effects of childcare responsibilities during COVID-19 on the labor supply of parents with young children. Following a pre-specified analysis plan, we use data from the monthly Current Population Survey (CPS) to implement three variations of an event study research design that compares parents with childcare needs to those without (or with lesser needs) to disentangle the effect of childcare responsibilities from the aggregate demand shock the COVID-19 pandemic brought to the labor market. In the first research design, our comparison is of parents with young children to those with no such children. In the second, limiting our sample to those with young children, we compare parents without a teenager in the house to those with one. In the third, we further limit the sample to parents with young children, but no teenager, and use the presence of a grandparent in the house as our basis for comparison. For each of these, we analyze three outcomes of interest: (1) whether a parent was actually working (not sick, on vacation, or otherwise away from his or her job) for an employer during the survey reference week, (2) whether the parent was employed during that week, and (3) the number of hours worked conditional on working. Since the literature on childcare has typically focused on labor supply of mothers, we also perform our analysis separately for men and women. In our pre-specified analysis, we find that, contrary to expectation, the labor supply of adults was not negatively affected by having young children during the COVID-19 pandemic, a finding that holds for each of our research design variations and outcomes. In contrast, we find some evidence that parents with childcare responsibilities were more likely to be working than those without after the onset of the pandemic, a result that is not systematically different for men and women. Furthermore, some estimates suggest working parents of young children worked more hours per week than those without young children, and that this effect was concentrated among

women. This result is consistent with mothers compensating for lost productivity due to childcare demands by working longer hours. Using new questions added to the CPS during the COVID-19 pandemic, we also show that the ability to work remotely was used more often by those with childcare needs, suggesting that employer flexibility with respect to working at home aided parents in avoiding negative impact to their labor supply. These new questions also provide evidence that our results were not due to an increase in labor demand for parents of young children relative to those who are not. In post-hoc analysis of sub-group heterogeneity, we find our main results appear to be driven by five groups: white respondents, high school drop-outs, college graduates, urban residents, and those whose urban or rural status is not known. Additionally, among the sub-groups, the only statistically significant negative estimates we obtain are for the likelihood of being at work for both single mothers and fathers, and for the number of hours worked (conditional on working) for both single women and black respondents. In each case, however, these significant negative estimates are only found using one research design. In the context of the literature on childcare, findings like ours are not entirely unprecedented. As we noted above, childcare costs typically have been found to have negative effects on labor supply, but this was especially true in the early studies. In more recent work, findings of little or no impact have become more common (Fitzpatrick, 2010; Fitzpatrick, 2012; Lundin et al., 2008). Such results are consistent with general trends of falling labor supply elasticities for women over time (F. D. Blau and Kahn, 2007; Heim, 2007). Nevertheless, that some of our estimates show a positive effect on labor supply is unusual in this literature, suggesting the COVID-19 pandemic created a unique childcare and work environment for parents.

In addition to contributing to the broad literature on childcare and labor supply, this paper also adds to the literature on the American labor market and COVID-19 as one of the first to study the labor supply of parents early in the pandemic, and the only one to use a publicly-available pre-analysis plan. The pre-specification of our research designs, outcome variables, sample, and regression specifications before the post-period data became publicly available increases the credibility of our results and assures they are not a result of data mining. This paper is also the first to make use of new questions in the CPS about the response to the pandemic to provide context for its findings on labor supply, and important component for understanding how parents responded to the rapidly changing childcare situation. Finally, we note that our research designs have an important advantage in a pandemic environment: they are not subject to policy endogeneity. An alternative approach of using government policies—such as state-level or school-district-level school-closures—has

the drawback that we know these policies were adopted in response to the spread of COVID-19. Since the labor market was also impacted by disease spread and other pandemic policies, this policy endogeneity could potentially cause bias. Our use of treatment and control groups based on ages of children, however, is not subject to this critique, and our use of CPS data allows us to provide a long pre-period to evaluate pre-trends between the two groups. Since the nature of the pre-existing differences between our groups is not related to the disease, this provides important evidence on their comparability. On the other hand, COVID-19 policy responses are specific to this outbreak, so pre-period comparison of geographical areas with different policy responses is less informative about how reasonable those comparisons are during the pandemic. Thus, our approach provides a credible alternative to policy-based research designs.

2.2 Literature Review

The literature on the effects of childcare responsibilities on the labor supply of American parents during the COVID-19 pandemic is quickly growing. Our article is part of the first wave of papers that began circulating publicly by early July of 2020, which also includes Collins, Landivar, et al. (2021), Heggeness (2020), Kalenkoski and Pabilonia (2020), and Rojas et al. (2020).¹

Compared to our study, these others all use empirical approaches that differ greatly from ours and investigate very different populations. Using state-level variation in school closings to study the general population, Rojas et al. (2020) analyze new unemployment insurance benefit claim filings, while Heggeness (2020) studies unemployment and other labor market outcomes using CPS data. Investigating unincorporated self-employed workers, Kalenkoski and Pabilonia (2020) use CPS data to compare workers across various demographic characteristics, including comparing parents with children to those without (an approach similar to our first research design). Collins, Landivar, et al. (2021) uses CPS data and an individual-level fixed effect strategy to compare the pandemic’s effect on married mothers versus fathers.

Rojas et al. (2020) do not find school closures affected filings, but their estimates are imprecise. Similarly, Heggeness (2020) finds no effects for most outcomes, including unemployment,

¹ Rojas et al. (2020) was distributed as an NBER working paper on May 11th, 2020. Our pre-analysis plan was posted on OSF Registry two days later. Heggeness (2020) was posted on the website of the Minneapolis Federal Reserve Bank on June 15th, 2020, while our draft was made available on SSRN on June 19th, 2020. Collins, Landivar, et al. (2021) appeared on the website of the journal *Gender, Work & Organization* on July 2nd, 2020 and Kalenkoski and Pabilonia (2020) was made available on SSRN on July 7th, 2020.

but she does find some evidence of an increased likelihood of one category of employment, being employed but temporarily not at work, and an increase in the number of hours worked by women. The interpretations of results for both of these studies are complicated by the fact that states across the country issued school closure orders within a matter of days. This leaves little variation through which estimates based on differential timing of closures could be identified.² More importantly, since they are identified based on policies adopted in response to the pandemic, their results could be affected by the above described policy endogeneity.

In their study of self-employed workers, Kalenkoski and Pabilonia (2020), like us, rely on variation across individuals, reporting results that vary across research design implementations. In contrast with our findings, however, they do obtain some large and statistically significant negative estimates for differences in employment and hours worked between parents and non-parents. Surprisingly, these estimates are concentrated among fathers of children over six years old. Collins, Landivar, et al. (2021) find mothers reduced their hours 4 to 5 times more than fathers in April of 2020. However, their comparison of men and women does not account for gender differences in jobs and industries in which parents work.

Like the initial set of papers, a second wave of research tended to find mixed results. These papers also usually focus on mothers and the comparison of women to men, though in some cases parts of their analyses provide comparisons within gender, which we focus on here. Zamarro and Prados (2021) rely on an internet-based “pulse” survey, finding college-educated women with school-aged children were more likely than those without kids to report having reduced their hours worked, but also find women without college were less likely to be so affected.³ Amuedo-Dorantes et al. (2020) used school closure policies to compare across states, finding reduced work hours for both men and women, but no effect of school closings on employment or not working the previous week.⁴ Russell and Sun (2020) compare responses of women with and without children under 6 years old to state-level child care center closure or capacity limit policies. They find some evidence of an increase in unemployment for women with kids under 5 versus those without, but no effects on hours or labor force participation. Fabrizio et al. (2021) closely follow our first research design, though focus on subgroups by education and race. They argue women with kids were less likely to be employed

² Heggeness (2020) also treats all 2020 data as post-period, including January and February, further complicating interpretation.

³ They do not report the coefficient estimator covariances needed to determine whether the differences between those with and without young children are statistically significant or not.

⁴ Hours losses for women were larger than for men in their main specification, but it is not clear if the difference was statistically significant.

than those without, but do not account for differences in employment for these groups before the pandemic. As our analysis shows, once these previous differences are accounted for, there is little evidence of an effect on women.

Petts et al. (2021) use an online survey taken in late April 2020, and measure levels of hours of childcare lost as the basis for their analysis. They report that mothers of children under 6 who lost more than full-time childcare were less likely to work than mothers who lost less than 10 hours of childcare, and that mothers helping educate their school-aged children were less likely to work than those who were not.⁵ In papers that fully focus on comparison of men and women, Collins, Ruppanner, et al. (2021) and Alon et al. (2021) argue childcare responsibilities increased gender gaps. Collins, Ruppanner, et al. (2021) report that the gender gap in labor force participation grew in 2020 versus that in 2019 in states with remote school, whereas in hybrid or in-person states saw less of a gap increase. They do not report whether the difference in change across instruction type was statistically significant. Finally, Alon et al. (2021) argue the employment and hours gender gap in the USA grew more for parents with school-aged children than for those with younger kids or no kids. However, in their analysis that is most similar to our approach in research design 1, their estimates appear to be similar between parents with kids and those without, which would suggest a similar result to ours.⁶

2.3 Data

We base our analysis on data from the basic monthly Current Population Survey (CPS), which has important advantages for our question of interest. It is the basis for the government’s official labor market statistics, has a large sample size, has a relatively high frequency as a monthly survey, and makes data available to researchers quickly. These features allow us to provide timely analysis on the performance of the labor market and the impact of school and daycare closure policies during the pandemic.

Our sample, obtained from IPUMS CPS, includes data for each month from January 2018 through June 2020, and for all non-military, non-student adults ages 21 to 59. Table 1 presents sample averages for our outcomes and selected demographic characteristics for each research design

⁵ They do not address the concern that work and hours lost to childcare during the pandemic and contributing to kids’ educational content are jointly determined outcomes.

⁶ They do not provide the estimator covariances needed to determine if the estimate differences across parent types are statistically significant.

and for both before and after the onset of the pandemic. Since our treatment and control groups are based on differences in children or whether respondents live with their parents, our groups naturally have average differences for some demographic measures. However, as we show below, the groups exhibit parallel trends before the pandemic for our outcomes of interest. Additionally, our preferred specifications of our models include numerous, flexible controls for observable demographic characteristics. Therefore, we do not view the reported differences in some sample averages as being of critical concern for our analysis.

2.4 Empirical Models

We used two separate plans to pre-specify our analyses. Our main analysis was pre-planned in Barkowski et al. (2020a), while a sub-analysis using new COVID-19 variables added to the CPS after the onset of the pandemic was pre-specified in Barkowski et al. (2020b). Both were developed before post-period data for our main analysis, and all the data for the sub-analysis, were available publicly. While it is unusual to pre-specify analyses when using publicly available government surveys, it is not unprecedented Neumark (2001). These publicly posted plans limit our ability to perform specification searches, increasing the credibility of our results.

In our main analysis, we focus on three primary labor supply outcomes. The first is a dummy variable indicating whether individuals' employment status is "at work." A worker categorized as at work is employed and actively working. This outcome is related to formal employment but excludes individuals who are employed but not working for reasons such as vacation and illness. This outcome has the advantage of measuring the extent to which individuals were able to perform their job duties whether from home or to leave the house (if necessary) to work, activities that childcare responsibilities might be inhibit. The second outcome is a dummy variable for employment, a more standard measure of labor market activity. Employed individuals are either at work or are temporarily absent from their jobs, so employment is a broader measure of attachment than being at work. Given this, employment smooths out some of the volatility seen in the at work outcome, but may be misleading on the impact of the pandemic for individuals who are using vacation or sick leave to allow them to stay home with children. Finally, we also analyze the number of hours worked during the reference week, conditional on being at work. This allows us to observe whether workers' availability was affected, even if work was not entirely precluded by the need to provide childcare.

An important issue arising with the CPS survey during the pandemic is that the BLS has reported that some respondents may have been misclassified as employed but absent from work instead of unemployed. Such misclassification could influence our employment outcome, but our “at work” outcome is not affected. This is an added benefit to our use of this outcome, even if (as we noted above) the primary reason for our interest in this variable was based on the context of our analysis, not considerations of data issues.

We study these outcomes of interest using three variations in specifying treatment and control groups. These depend on the ages of respondents’ children (or lack of children) and whether a grandparent also lives in the household (that is, a parent of an adult and grandparent of a young child needing care). In the first variation, which we call research design 1, individuals with children under age 13 (“young children”) are the treatment group. The rationale for this cutoff is that such children are less likely to stay home alone while schools are closed, and previous research has suggested the labor supply effect of children ends by the time they are 13 (Angrist and Evans, 1996). Respondents without young children are then taken as the control group, since they are less likely to be constrained in supplying labor by the need to provide childcare. Formally, for this part of our analysis we define the dummy variable, $treat$, to differentiate these groups, where $treat = 1$ if a worker’s youngest own child is under 13 years old, and $treat = 0$ otherwise.⁷

Our second approach of defining treatment and control groups—research design 2—narrows the population of study to only those who have a young child. To separate individuals constrained by childcare needs from those who are not, we use the presence of older children. We reason here that older children—teenagers and very young adults—can help provide childcare while schools are closed. Thus, we define the control group for research design 2 ($treat = 0$) as individuals whose oldest own child is 13 to 21 years old. In contrast, the treatment group should not have an older child to help provide childcare, implying more childcare restrictions. Therefore, given the definition of the control group, we consider treated individuals ($treat = 1$) as those whose eldest own child is not 13 to 21 years old.⁸

Our third approach, research design 3, further restricts our sample to those with young children but whose oldest children are not 13 to 21 years old. That is, the people who fall into both

⁷ Since all relationships are not made clear in CPS data, there might be some cases of own children that are not identified in the data. We address this via a post-hoc robustness check discussed below.

⁸ Note that this leaves the possibility that an individual with an oldest child who is above 21 and a middle child who is 13 to 21 could be included in the treatment group. We address this via a post-hoc robustness check we discuss below.

treatment groups for the first two research designs. In this case, we use the presence of a parent of the worker (grandparent of the child needing care) to define the groups. Since a worker without a parent nor an older child to provide childcare for the young child is constrained in his or her ability to work, individuals in this situation form our treatment group. Conversely, those who have parents in their houses, who could provide care for the workers' children, form the control group. Thus, for this part of our study we define $treat = 1$ if a worker does not have a parent living with him or her, and $treat = 0$ otherwise. This approach has an important weakness compared to the first two since the share of the sample with a parent in the house is only about six percent. This results in less precision, but we argue this research design still provides a helpful complement to the other two approaches in our analysis.

To implement these research designs, we use the following econometric model:

$$y_{it} = \sum_{j=jan2018}^{jan2020} \beta_j treat_i \times 1_j(t) + \beta_{Feb2020} treat_i + \sum_{k=Mar2020}^{June2020} \beta_k treat_i \times 1_k(t) + \alpha' X_{it} + \mu_{it} \quad (2.1)$$

Here i and t index CPS respondents and month, respectively, and y_{it} represents one of the three outcomes of study discussed above. As already noted above, the treatment group identifier is represented by $treat_i$, while indicator function $1_m(t)$ identifies observations for month m . Finally, X_{it} is a column vector of controls, all implemented as sets of dummy variables, α is a column vector of parameters for those controls, and u_{it} is the error term.

We estimate several versions of the above model for each research design and outcome combination. These include weighted and unweighted versions of the model, with the unweighted version representing our preferred approach (as stated in our pre-specification) given its relative ex-ante efficiency. Within the weighted and unweighted categories, we estimate three versions of the model. The first version has minimal controls, with only a set of year-month dummy variables included, while the second adds state dummies.⁹ Finally, the last adds dummy variables for gender, age, race, marital status, metro-area status, CPS month-in-sample, veteran status, foreign/domestic nativity, Hispanic ethnicity, education, and disability status. Additionally, to investigate whether the effect of the pandemic response differs by gender, we estimate each of our model variations for both men and women separately, in addition to the combined sample.

The primary coefficients of interest for our analysis are the β s for the months of March 2020

⁹ Our pre-analysis plan specifies for the second regression to add calendar-month dummies, but these are redundant given the year-by-month dummies already included.

and after. These represent the difference between the treatment group and control group (treat – control) relative to the difference that existed in February 2020, the final month before societal responses to COVID-19 began occurring. In determining our post-period, some judgement was necessary since the national response began occurring in March 2020. Most schools in the country were formally closed by state-level orders the week beginning March 15th, though some districts closed sooner than that. The March CPS survey took place from March 8th through the 14th, so there is reason to think it would miss the full effect of the virus response. This point is underscored by the resulting unemployment rates reported by the Bureau of Labor Statistics (BLS) based on the CPS surveys. For March, the BLS reported a rate of 4.4 percent, almost a one percentage point increase from 3.5 percent in February. This suggests that some early effects of the pandemic had begun to appear by the time of the March survey, but they were much smaller compared to the measured impact for April, when the BLS reported a rate of 14.7 percent. Accordingly, we consider April to be the beginning of the full post-period of our analysis, but our model measures the effect in March as well, representing the very early effects of the pandemic. Graphs reporting our estimates identify both March and April for clarity.

For all of our models, the underlying identification assumption necessary to identify the labor supply effect on parents is that the labor demand drop that occurred during the pandemic was the same between our treatment and control groups. We provide evidence supporting this assumption in two forms below: we show a long period of similar time trends before the start of the pandemic and provide results from new questions that asked directly during the crisis about how employer demand for workers and respondents' job search efforts were affected.

2.4.1 Post-Hoc Analyses

We perform several post-hoc analyses that are not specified in our pre-analysis plan to provide insight into the character and robustness of our results. To determine if the differences we find between treatment and control groups could be driven by industries or occupations, we estimate an additional version of our model that includes industry and occupation fixed effects.¹⁰ To check whether imbalance by age of young children needing care could be driving our results, we also estimate a version of our model with dummy variables for the age of the youngest child for our second and third research designs, in which the samples are limited to adults with young children.

¹⁰ A fixed effect is also included for those that do not have an occupation or industry in the data.

In addition to adding the above controls, we also perform our analysis using redefined treatment and control groups. In our main analysis, these are based on variables created by IPUMS CPS identifying respondents youngest and eldest own children in the household and their ages. However, the CPS survey does not conclusively identify all relationships between individuals in households, and the use of youngest and eldest children overlooks other children in households of more than two. To address these issues, we redefine the groups on the basis of ages for all children in a household. Hence, for research design 1, the treatment group is those in a household with a child under 13-years-old, while the control is those who are not. In the second design, the groups are defined on whether any child in the household is 13- to 21-years-old. Moreover, the samples for the second and third research designs are limited using these alternative bases for identifying children in the household.

In another robustness check, we examine whether the pandemic’s impact on CPS response rates could be influencing our results. The Bureau of Labor statistics has noted that the response rate for respondents of the CPS survey has been dramatically lower since March. In April 2020, the overall response rate was 70 percent, 13 percentage points less than April 2019 and 12 percentage points lower than February 2020 (IPUMS CPS 2020). The fall in response rate is driven by the Census Bureau dropping in-person interviews beginning in March. These in-person interviews usually occur for households just entering or re-entering the sample (months-in-sample one and five), while households after those points are interviewed by phone. As a result, April response rates were lowest for the first two months after entering or re-entering the sample: 47, 64, 69, and 73 percent for months-in-sample one, two, five, and six, respectively. For those in months-in-sample three, four, seven, or eight, however, April response rates were much closer to normal: 76 percent in months three and seven and 78 percent in four and eight (IPUMS CPS 2020). To check for the effect of the low response rates on our results, we re-estimate the variations of our model using only data for months-in-sample three, four, seven, and eight. Differences in the estimates for this variation of our analysis from our main ones would suggest the low response rates affect our main results.

To examine heterogeneity in our estimates, we also report results obtained by limiting our sample by demographic characteristics, including marital status, race, education level, and setting of residence. To facilitate presentation of these results, we use a standard DD (non-dynamic) version of equation 2.1 that produces only one post-period coefficient estimate per regression, given by

$$y_{it} = \beta_1 treat_i \times post_t + \beta_2 treat_i + \alpha' X_{it} + \mu_{it} \quad (2.2)$$

Here $post_t$ is a dummy indicating the post-period, April 2020 or later, and other variables are as defined above. Since we are not estimating a separate coefficient in this model for each month, and the March survey potentially only reflects the very early stages of response to the pandemic, we drop data for the month of March to avoid diluting our estimates for the full post-period effects.

Finally, to show how parental labor supply was affected through the summer and into the fall, when some schools began opening again, we also produce estimates of equation 2.1 using data through November of 2020. Though our pre-analysis plan stated that we would possibly include data beyond June of 2020, we treat these results as post-hoc since we did not specify a definitive end date.

2.4.2 New COVID-19 CPS Questions

As part of the May survey, six new questions were added to the CPS questionnaire that are intended to help measure the impact of the COVID-19 pandemic. We use three of the new questions that provide context to our main results. Our analysis of these new variables was fully pre-specified, except for one post-hoc specification we discuss at the bottom of this sub-section. These questions ask the following:

1. “At any time in the LAST 4 WEEKS, did you telework or work at home for pay BECAUSE OF THE CORONAVIRUS PANDEMIC?”
2. “At any time in the LAST 4 WEEKS, were you unable to work because your EMPLOYER CLOSED OR LOST BUSINESS due to the coronavirus pandemic?”
3. “Did the coronavirus pandemic prevent you from looking for work in the LAST 4 WEEKS?”

The first question provides insight as to whether employers differentially provided the two groups work flexibility, or the workers differentially took advantage of it. The second provides some evidence towards whether employer factors, rather than childcare issues, could have influenced the groups differently. Finally, the last question provides additional evidence of whether the groups differed in their ability to look for work, which is relevant since young children presumably could impede parents’ search efforts.

Since these are new questions, there is no pre-period data, so we compare each of our treatment and control groups using only May and June post-period data. For each of the three

questions, we present regression adjusted group differences ones for our full sample and for men and women separately.¹¹ We also produce them for our robustness check sample that includes only month-in-sample observations three, four, seven, and eight to examine whether our results were affected by low survey response rates due to the pandemic.¹²

Regression estimates are based on the following model:

$$y_{it} = \beta_{May20}treat_{it} \times 1_{May20}(t) + \beta_{Jun20}treat_{it} \times 1_{Jun20}(t) + \alpha'X_{it} + \mu_{it} \quad (2.3)$$

We include the same set of controls as in our primary analysis and again report standard errors clustered at the state level. The outcomes, y_{it} , are dummy variable versions of the answers to the three questions, where yes if coded as one and no as zero. The primary coefficient estimates of interest from this model are the β s for the months of May and June, which represent the (regression adjusted) differences between the treatment group and control group (treat – control).

We perform one post-hoc analysis for these COVID-19 outcomes, focusing on the teleworking question. Teleworking opportunities could be driven by specifics of an individual’s job, since not all occupations and industries are equally amenable to remote work. To investigate whether this possibility influences our results, we estimate a model that includes fixed effects for industry and occupation.

2.5 Results

Before discussing the details of our estimates, we note here that our use of event-study models for the basis of our analysis tends to obscure an important empirical fact of the pandemic. As the sample averages in Table 1 show, the pandemic created a substantial drop in the likelihood of being at work or employed in our sample, with workers being about seven percentage points less likely to be at work in the post-period as compared to the pre-period. As we discuss our results, the reader should be aware that, at all times, our results are relative to those that have less needs for childcare.

¹¹ As per our pre-specification, we also estimated unadjusted, group sample average differences, but we omit them from the paper to save space. They are available from the authors on request. We present sample averages for each variable and for each treatment and control group for May and June combined in Table 1.

¹² This analysis was pre-specified for these questions even though our use of this sample was not pre-specified for our main analysis. This is due to the data for the new COVID-19 questions being released later than the data for the main analysis. During the delay we recognized that the response rate had been affected and added this sample to the plan, which could not be done for the main analysis since that data was already available.

Keeping the above in mind, we turn to Figures 1 through 3, which plot our estimates for our preferred specification of equation (1) that includes full demographic controls based on our full sample. These results are also presented numerically for March 2020 and onward in Table 2, along with estimates for women and men separately. In the plots, the shaded area represents 95 percent confidence intervals and the green dashed and solid lines indicate March and April 2020, respectively. Figures are grouped by outcome, presenting results for all three research designs together.

The figures show that, despite that the treatment and control groups are based on differences in household composition, pre-period trends are reasonably parallel across outcomes and research designs. Most of the small handful of statistically significant (at five percent) differences between groups in the pre-period occur for the at work outcome (Figure 1), where a slight decreasing trend is exhibited in the four months immediately leading up to the pandemic start in research design 1. Given this trend is reversed upon the start of the pandemic, we do not consider it to be an influence on our findings. Hence, we find the pre-trends overall suggest our control groups provide credible comparisons for our treatment subjects.

Our pre-analysis plan stated that our expectation was to find the pandemic caused a negative shock on the labor supply of parents with young children relative to those without. However, our unweighted main results in Figures 1 to 3 and Table 2 suggest this negative shock did not occur. On the contrary, some estimates suggest parents of young children were more likely to be at work after the onset of the pandemic. Our full sample results for research design 1 on being at work suggest parents of young children were about one percentage point more likely than those without in April ($p=0.068$), May ($p=0.037$), and June ($p=0.070$). Research design 2 estimates put the increase at about two percentage points for the same months ($p=0.020$, 0.035 , and 0.12), and for March ($p=0.022$) also, despite that the pandemic response was still at its early stages. Research design 3 returns similar, positive point estimates for April, and May, and a positive but smaller estimate for June, but with much larger standard errors that result in zero-effect null hypotheses not being rejected for all three months. We find results for employment that are substantially similar to these for being at work, while for hours worked we obtain positive point estimates in most cases but only one instance of an estimate exhibiting a conventional-level of statistical significance: April for research design 2 (10% level, $p=0.067$).

Our separate estimates for men and women on the outcomes of being at work and employed do not suggest our full sample results are driven by only one gender. As panels B and C show, our

estimates for research design 1 appear to be driven by men but those for design 2 predominantly reflect women. In contrast, for the hours worked outcome we find some evidence via research design 1 that women with young children worked more than those without in May and June by more than half-an-hour of work per week ($p=0.003$ and 0.038), a result not reflected in the estimates for men. However, for the other two research designs we do not obtain any statistically significant effects for either men or women. On net, we find limited evidence that gender response differed, but our results by gender do show clearly that the pooling of men and women does not obscure any large negative labor supply shocks for either gender.

A corresponding set of estimates to those in Table 2, but calculated using sampling weights, is presented in Table 3. Overall, our weighted results largely conform to our unweighted ones, but these estimates tend to be larger in magnitude and often exhibit higher levels of statistical significance. Additionally, the weighted results magnify the partial evidence in Table 2 that hours worked may have increased for some female parents of young children. As Table 3 shows for the hours worked outcome, we obtain estimates for May and June from research design 1 that are positive and statistically significant ($p=0.066$ and 0.015) for the pooled sample. These estimates suggest parents of young children worked about half-an-hour more per week than parents without such children, conditional on working. The breakdown of these results by men and women shows that they are driven by women, whose estimates are also statistically significant ($p=0.0004$ and 0.016), while those for men are not (and are, in fact, negative in March through May). The significant estimates for women suggest their weekly number of hours worked exceeded those of women who did not have young children by almost an hour. Additionally, the weighted estimate for April from research design 2 is also positive and significant ($p=0.093$), though in this case the result seems to have been driven equally by men and women. We note, though, that the hours worked outcome for research design 3 here is also where we find one of our very few statistically significant ($p=0.071$) negative estimates for women for the month of June. This estimate suggests women worked an hour-and-a-half less due to childcare that month.

Taking results from both Tables 2 and 3 into consideration, for the outcomes of being at work and being employed, we do not find evidence that the response to the pandemic was different for men and women. We also do not find evidence overall that hours decreased, with some evidence suggesting that, conditional on being at work, women with young children responded to the pandemic by working about a half-hour to an hour longer than women without young children. This result is

not evident in all research designs, though, and is counterbalanced to some degree by the negative estimate we noted above.

In addition to the estimates for our preferred specification that are reported in Tables 2 and 3, we also estimated model variations with fewer controls. Across these variations we obtain similar results to those found in Table 2. Full results are available upon request, but Appendix Table 2 reports estimates from when our model includes minimal controls.

2.5.1 Post-Hoc Analyses Results

Tables 4 through 7, Appendix Tables 3 and 4, and Appendix Figures 1 to 6 present results from our post-hoc analyses of our main labor supply outcomes. The first of these, Table 4, reports estimates from models with additional fixed effects for industry and occupation added, controls which were not pre-specified.¹³ Their addition shows whether differences in the composition of the treatment and control groups across industries and occupations could be hiding negative labor supply shocks. Here we find estimates for being at work or employed that are typically smaller than in our preferred specification, particularly for research design 2, which are much smaller and not significant. Our estimates for hours worked, however, are quite similar to our main results, even when broken down by gender. Across the table, the most notable difference from our main results is our estimates for men being employed based on research design 2, which are negative and statistically significant in May and June. These are a stark contrast from the positive and significant estimates produced by research design 1 for the same outcome. Given this inconsistency, and the lack of any other negative and significant estimates, the evidence overall from Table 4 does not suggest childcare needs caused a negative labor supply shock for parents during the COVID-19 crisis. Instead, they show that some of our findings of positive effects come from comparison of treatment and control individuals across industries and occupations.¹⁴ One possible explanation for this is parents of young children may have been working in jobs relatively more sheltered from the impact of the pandemic, resulting in them being less likely to be away from their work.

A second set of results from adding controls that were not pre-specified are reported in

¹³ Workers' industries and occupations could be endogenously influenced by the onset of the COVID-19 pandemic, which is why these controls were not included in our pre-analysis plan. Our unexpected results, however, prompted us to consider the influence of industries and occupations. Nevertheless, the threat of endogeneity should be considered when interpreting these results.

¹⁴ Movement of workers between industries and occupations in response to the pandemic could also be part of the explanation here. Whether such movement occurred would be an interesting question for future research.

Appendix Table 3. These estimates come from models that include fixed effects for the age of each parent’s youngest own child. This addresses potential concerns that the treatment and control groups could have important differences in child ages that influence their childcare needs. We calculate these estimates only for research designs 2 and 3 since some control group individuals in research design 1 do not live with or have a child, and we find results that are very similar to our main estimates, undermining differences in youngest child ages as a potential explanation for our results.

Table 5 reports results when we redefine the treatment and control groups using the presence of any children of the relevant ages in worker households instead of the IPUMS constructed variables used in our main analysis. In this case, for the outcomes of being at work and being employed, we find that our estimates are smaller in this alternative framework in research design 1, but similar in design 2, and much larger and positive in research design 3. For hours worked, the alternative approach produces estimates that are typically more positive and more likely to be statistically significant. Across all regressions, we obtain two statistically significant, negative estimates, with both occurring for men. One is found for our employment result for June from research design 2 ($p=0.096$). This mimics our estimate for the same design, outcome, and month in Table 4. The other is for May in research design 1 for hours worked ($p=0.023$). These negative estimates are not consistent across research designs, as is the case for all of the few instances of negative effects we obtain in this project. So, despite these two negative estimates, the overall impression of these results is not drastically different from our main results, and there is little to suggest a negative labor supply shock response to the pandemic.

Next, we consider whether the reduced response rate to the CPS survey could be influencing our results. To preface this, we note that in our table of sample averages, Table 1, we report both pre-period and post-period sample means.¹⁵ Review of these shows that despite the response rate changes, there is relatively little change across periods, suggesting the types of individuals represented in the survey over time are not changing meaningfully. Nevertheless, we go further and re-estimate equation (1) using individuals with higher values of the month-in-sample variable (as described above), which had higher response rates. These results are found in Table 6 for our full sample. Here we obtain point estimates that, in most cases, are similar to our main results.

¹⁵ These are not calculated using sampling weights for two reasons. First, our goal is to compare the various subsamples themselves for similarity, not the populations from which they are drawn (which the weights are intended to enable). Second, to the extent that response rates do affect the sample, it is not clear that the adjustments to the weights made by the Census Bureau for demographic factors are appropriate or accurate given they were not developed for use in periods of viral pandemic when response rates are significantly affected.

Differences include smaller, but still positive, estimates from research design 1 for the at work and employment outcomes, and estimates that are nearly all not statistically significant across all designs and outcomes, even when point estimates are similar or larger, due to larger standard errors. Overall, though, like our main results, these estimates do not provide any evidence for a negative labor supply effect.

Our next post-hoc analysis examines heterogeneity across demographic characteristics via a simpler version of our main model, equation (2). The results of this analysis are presented in Table 7. To provide a point of comparison for estimates based on sample sub-groups, we report estimates of equation (2) using our full-sample in the first-row. These conform to our main results, suggesting being at work or employed increased from about one (research design 1) to two (research design 2) percentage points. In the following rows of the table, the sample for each regression is limited to the sub-group indicated in the far-left cell of each row. We find most estimates across groups are positive or not statistically significant. Results for white respondents, high school non-graduates, college graduates, and residents in urban areas or those where metropolitan status is unknown are significant and positive in some research designs, suggesting these groups are important drivers of our estimates for the main, pooled sample.

In terms of negative estimates, we find only a few, most notably among single respondents. Via research design 1, we estimate they were less likely to be at work by 1.7 percentage points ($p=0.015$), a result that appears to be driven about equally by men and women. None of the other estimates for single individuals are statistically significant except in the case of hours worked for women, via research design 3. That estimate is significant at the 10 percent level and suggests single women with young children and no grandparent in the household worked 1.3 hours fewer each week ($p=0.078$) than single women with young children and a grandparent in the house. The only other statistically-significant (at the 10 percent level) negative estimate we obtain is also for hours worked by black respondents via research design 3, an estimate that suggests they worked almost 2 fewer hours per week, conditional on working ($p=0.066$). This result is contrasted, however, by the positive and significant estimate we obtain for black individuals via research design 2 that suggests they worked 1.2 hours more ($p=0.033$). Considering our results for all sub-groups, we find little evidence of negative labor supply effects. The strongest evidence of such effects is found for single individuals, though this result is not robust across specifications.

We present graphical results for our final post-hoc analysis on our labor supply outcomes

in Appendix Figures 1 through 6, with numerical results for these plots presented in Appendix Table 4. These results come from extending our sample through November 2020 and estimating a version of equation (1) that allows for the longer post-period. We include results for our full sample and for men and women separately. These findings are similar to our results through June only, though they suggest positive differences in work between groups during the early summer months faded towards zero in the fall. We do not obtain any statistically-significant negative estimate for any month, research design, outcome, or gender in these results, nor do we observe any systematic gender difference.

2.5.2 New COVID-19 Questions Results

In Table 8, we report estimates of equation (3) where the newly added survey questions related to the COVID-19 pandemic are used as dependent variables. The first outcome is whether the respondent reported having worked remotely (“teleworked”) because of the pandemic, a question that can help us understand the mechanism behind our results. Here we obtain positive estimates in all cases but one, where a positive means parents with childcare responsibilities were more likely to report teleworking. Most point estimates fall in the one to three percentage point range, with our sample in Panel D, which is based on the highest response rate months-in-sample, producing the largest estimates overall. Additionally, and perhaps surprisingly, we find positive and significant estimates for both women and men, depending on the research design. Moreover, as we report in Appendix Table 5, we obtain very similar estimates from a post-hoc specification that included occupation and industry fixed effects. Given that our results for the labor supply outcomes suggested childcare did not reduce how much parents worked, this result offers a possible explanation: that employer flexibility towards remote work helped parents continue working despite their increased responsibilities for childcare. This finding is also consistent with the stylized fact that parents with children were more likely to work at home before the pandemic (Woods, 2020).

In addition to providing evidence on the mechanism behind our results, these new questions also help address the fact that the main outcomes we study – being at work or employed and hours worked – are equilibrium outcomes, and reflect the interplay of both the labor supply and demand curves. The next two outcomes allow us to provide some direct measurement on the movements of these underlying curves. The first of these outcomes, asked of all respondents, inquires whether respondents were unable to work because their employers lost business due to COVID-19. This

question provides evidence on the nature of labor demand, and helps us address a theoretical possibility that our results could still reflect a reduction in the labor supply curve despite the fact that we do not find a reduction in our labor outcomes. While it is clear that overall labor demand fell in the pandemic, if labor demand for parents with childcare responsibilities rose (that is, shifted rightward in a standard labor supply and demand model) relative to that of those without childcare responsibilities, then the labor supply curve could shift to the left (that is, a reduction in labor supply) and still leave the equilibrium quantity of labor unchanged or positive.

In our analysis, negative estimates would be evidence towards the relative labor demand curve of employers shifting rightward for those with childcare needs. We find, however, mostly small and insignificant estimates for being unable to work. Those estimates that are significant are positive in all cases. This suggests that labor demand either shifted equally for those with and without childcare needs, or downward (leftward) for those with childcare needs relatively more than for those without. This result implies our findings of null or positive effects on the equilibrium in labor quantity are not consistent with a reduction in labor supply.

The final outcome is whether respondents claim they are prevented from looking for work by the pandemic. This question represents a direct measure of the labor supply curve for the part of the market that was out of the labor force (since it was only asked of those who were not in the labor force). In this question, positive estimates mean those with childcare responsibilities were more likely to claim they were prevented from looking for work by the pandemic than those without. Thus, a positive estimate implies a reduction in labor supply for those with childcare needs. Unfortunately, for this outcome we obtain results that are contradictory across research designs. In research designs 1 and 2 we find mostly negative estimates, most of which are statistically significant at various levels. In research design 3, however, we find all positive estimates, some of which are significant. Moreover, estimates from research design 3 are very large in magnitude, with the significant ones suggesting those with childcare needs were about 10 percentage points more likely to have been prevented from looking for work. Thus, our estimates for this outcome are not consistent, but suggest some sub-populations may have experienced a relative reduction in labor supply, while others experienced an increase. Combined with the results for the unable to work question, which was asked of many more respondents, we interpret the overall evidence for the full labor market from these direct measures of the labor supply and demand curves as suggesting little change in labor supply *on net* for parents with childcare responsibilities.

2.6 Discussion

The COVID-19 pandemic created an extraordinary labor market environment in which social distancing followed by government orders to stay home induced a massive shock to labor demand. As it unfolded, concerns were raised that the closing of schools and daycare centers across the country would compound the labor market shock for parents of young children, who suddenly had to provide childcare for their children, and cause them to reduce their labor supply. The school closings indeed created a severe childcare concern, as Sevilla and Smith (2020) report that families with young children in the UK increased their childcare provision by about 40 hours per week after the pandemic onset. Nevertheless, we find that the concerns about negative labor supply shocks were unfounded, as we fail to find much evidence of a labor supply reduction for parents of young children of either gender. Instead, we find some evidence that they were more likely to work than those without young children or those that had other childcare options in their households. We estimate that parents with young children were about one percentage point more likely to be working than adults without young children in their households after the pandemic began (based on research design 1). Per our CPS data, about 46.7 million adults in the country have young children, so our one-percentage-point estimate corresponds to about 467,000 workers. We also find that among workers with young children in their households, those without a teenager oldest child are more likely to be at work by about two percentage points than those who do have a teen. Again, per our CPS data, the population of parents of young children whose oldest child is not a teen is about 34 million people, which implies our estimate of two-percentage-points corresponds to 680,000 parents nationwide. Taking these together, roughly half a million more parents were at work after the COVID-19 pandemic began as compared to those with fewer childcare obligations.

Additionally, we find that men and women did not have systematically different responses to the pandemic for two of our outcome variables, being at work and being employed. While surprising, this is consistent with findings that gender differences in childcare provision narrowed at least slightly during the COVID-19 pandemic (Sevilla and Smith, 2020). For our third outcome, the number of hours worked conditional on being at work, we find some evidence that women of young children may have worked nearly an hour more per week in response to the pandemic. While it is surprising parents did not substitute away from hours worked given their sudden additional child care demands, this finding could be rationalized if we consider that children could have reduced the productivity

of parents – and women in particular – resulting in them working more hours to complete assigned tasks. This interpretation is also consistent with Gibbs et al. (2021), who show Asian IT workers worked more hours at lower productivity-levels when they had children at home after the onset of the pandemic. Nevertheless, we note here that this finding of increased working hours was not consistent across our three research designs, though it was consistent across our post-hoc robustness checks.

In post-hoc sub-group analysis, the most evidence we found for possible negative effects came from single parents of young children, where we found they were about 1.5 percentage points less likely to be at work than single parents without young children. This effect was present for both men and women, but it was not consistent across research designs. This inconsistency was also found by Kalenkoski and Pabilonia (2020). In specifications that differed in the way they controlled for seasonality, they estimated a large negative effect on single fathers' employment in one specification but none in another. Their estimates for single mothers' employment were not statistically significant.

Our main findings run counter to our pre-stated expectations but are consistent with some studies of the relationship between the cost of childcare and labor supply that have found little evidence of effects (Fitzpatrick, 2010; Fitzpatrick, 2012; Lundin et al., 2008). Our results are also broadly consistent with some findings in the recent literature on childcare during the COVID-19 pandemic: Rojas et al. (2020) and Heggeness (2020) find no effect on unemployment, while Amuedo-Dorantes et al. (2020) find no effect on employment, and Russell and Sun (2020) find no effect on labor force participation. Moreover, Heggeness (2020) also finds some evidence for women that hours worked increased during the pandemic.

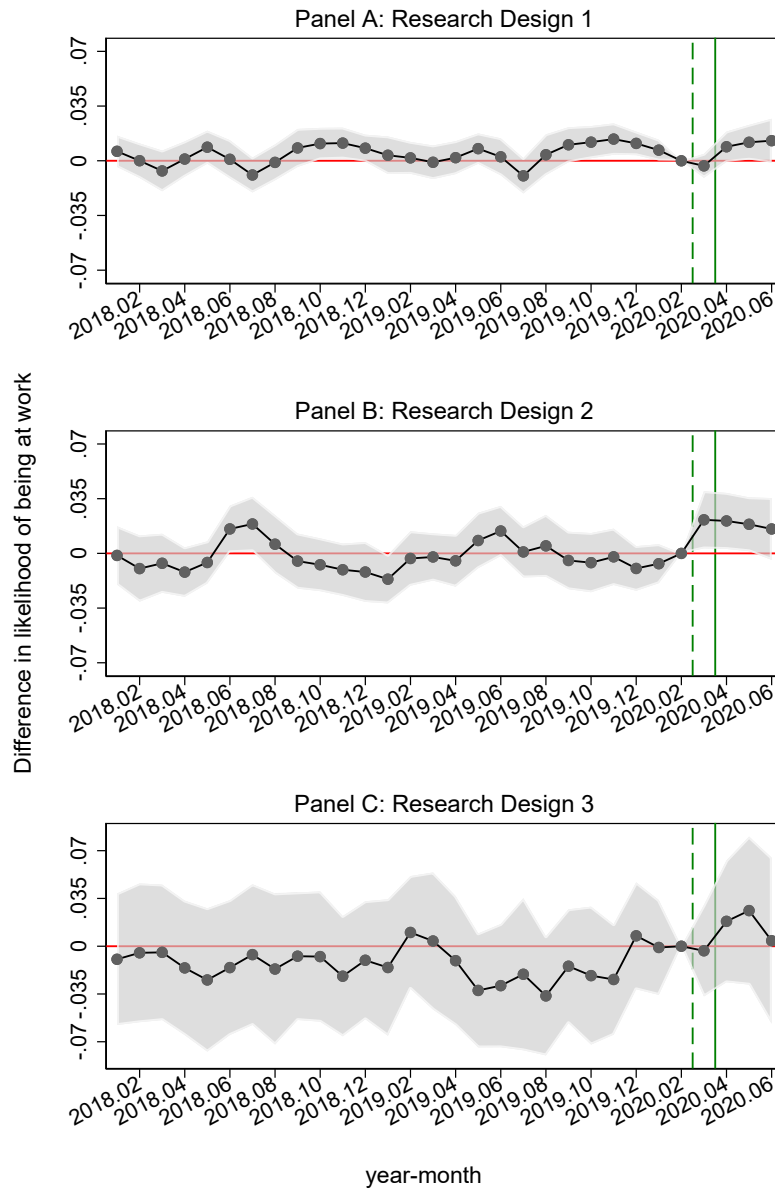
We argue our findings are suggestive of the importance of employer responses during the pandemic to increase employees' flexibility to work at home, and of the critical role informal sources of childcare play in parents' employment. One of the ways employers increased flexibility during the pandemic was to allow employees to work from home, and we showed that those with childcare needs were more likely to report working remotely during the pandemic. Moreover, (Brynjolfsson et al., 2020) found that about half of employees in the USA who were employed before the pandemic were working from home as it unfolded.¹⁶ Other dimensions of flexibility are possible, though, such

¹⁶ This includes individuals who were working from home before the pandemic. Brynjolfsson et al. (2020) also report that more than a third of those who commuted to work before the pandemic were working from home after.

as allowing workers to complete job tasks during hours outside the typical day schedule. Important insights could be gained by future research, perhaps with time-use surveys or mobility data, into the dimensions of work flexibility during the pandemic.

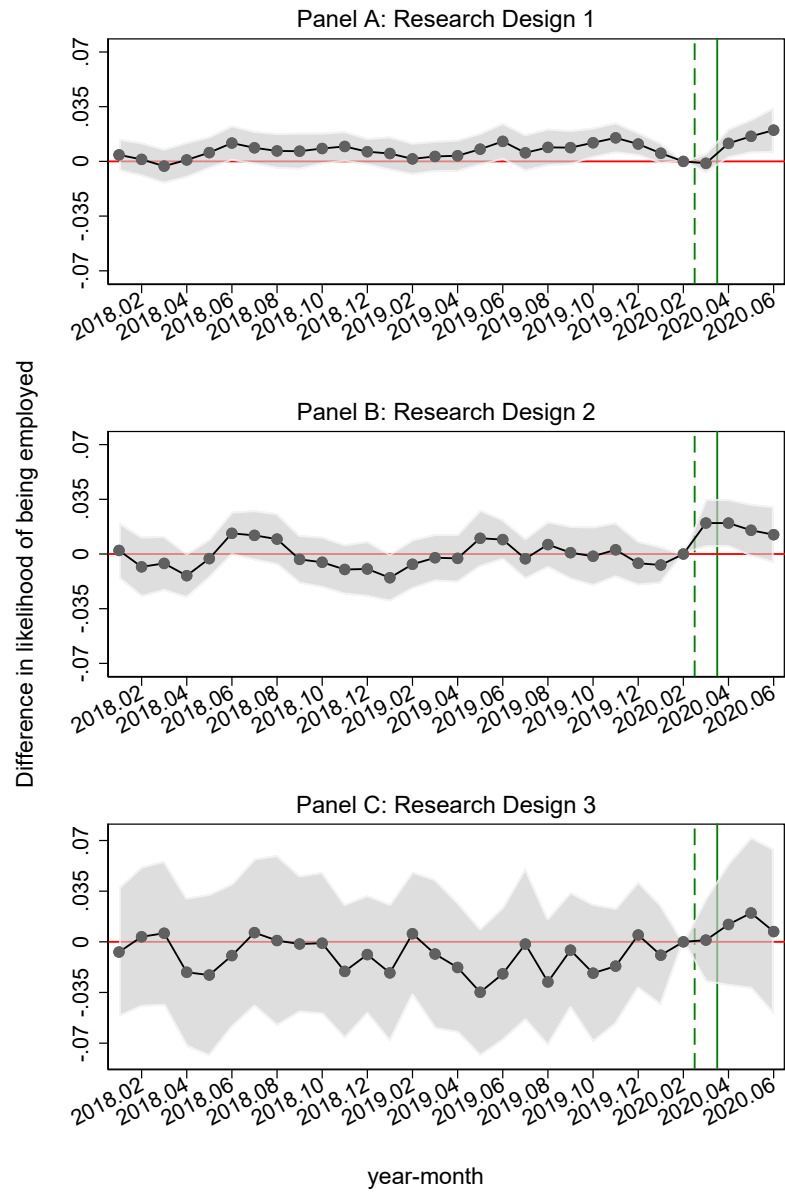
Finally, while we argue that labor supply did not fall for parents with childcare needs, this finding does not mean their welfare was not negatively affected by the sudden closure of schools and childcare centers. Instead, the fact that working parents can absorb an additional 40 hours per week of additional childcare in their schedules without a major labor supply shock attests to how important parents' jobs are to them. Policies that assist parents obtain childcare or encourage work flexibility could, therefore, potentially improve the welfare of parents significantly. Research into the potential costs of such policies could offer important insights.

Figure 2.1: Difference in likelihood of being at work (treated group minus control)



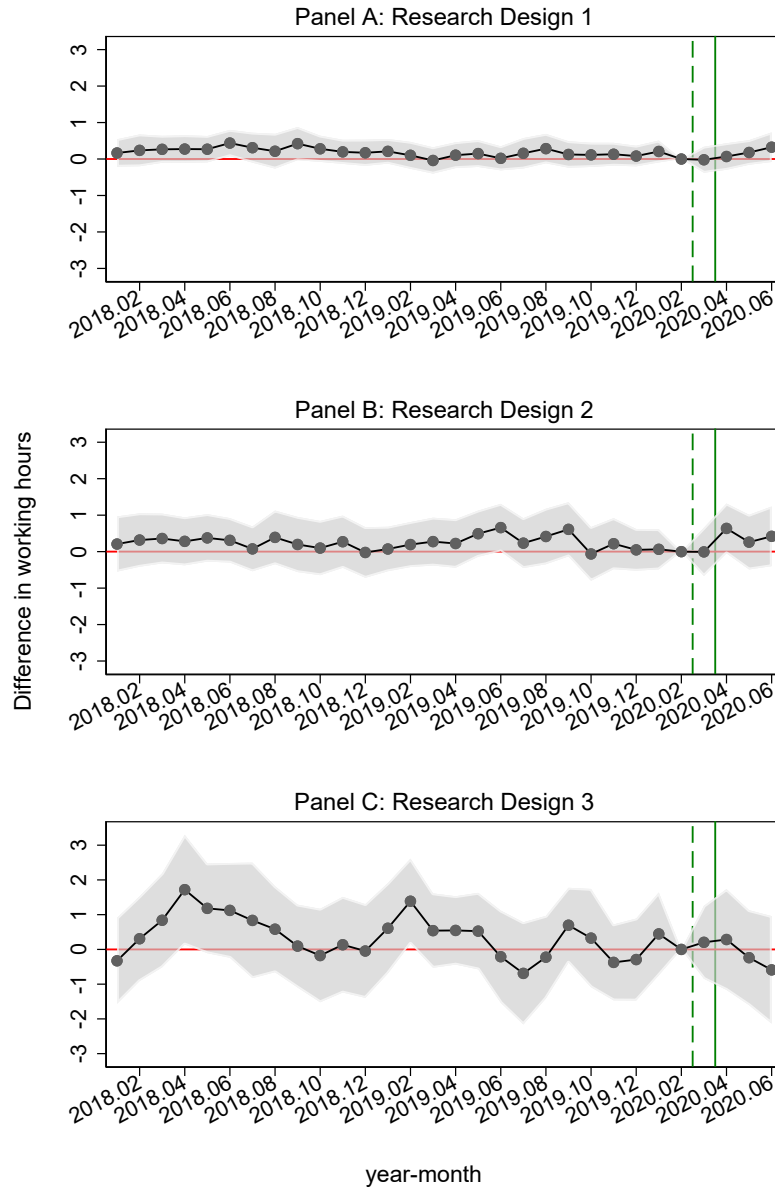
Notes: Shaded area represents 95% confidence intervals based on state-level clustered standard errors. Dashed, green vertical line indicates early pandemic stages (March 2020). Solid, green vertical line represents the start of the post-period (April 2020). Sample is basic monthly CPS for Jan 2018 – June 2020, including non-military, non-student respondents ages 21–59. Estimation performed without sampling weights while including controls for year-month, calendar-month, state, age, race, ethnicity, gender, marital status, education, metropolitan area, month-in-sample, veteran status, foreign birthplace, and disability. “At work” means individuals are employed and actively working. Research design 1 defines treat=1 if a worker’s youngest own child is under 13 years of age, and 0 otherwise. Research design 2 defines treat =1 if a worker’s eldest own child is not 13 to 21 years of age, and 0 otherwise. Research design 3 defines treat =1 if a worker does not have a parent living with him or her, and 0 otherwise.

Figure 2.2: Difference in likelihood of being employed (treated group minus control)



Notes: Notes to Figure 1 apply, except employed workers include those temporarily absent from their jobs (e.g., sick, vacation).

Figure 2.3: Difference in hours worked (treated group minus control)



Notes: Notes to Figure 1 apply, except hours worked are conditional on being at work.

Table 2.1: Selected sample averages

Variables	Period	Research Design 1		Research Design 2		Research Design 3	
		Treatment	Control	Treatment	Control	Treatment	Control
At work	Pre	0.769	0.764	0.768	0.774	0.773	0.687
	Post	0.702	0.690	0.707	0.690	0.713	0.607
Employed	Pre	0.799	0.787	0.799	0.800	0.804	0.712
	Post	0.747	0.727	0.753	0.732	0.760	0.651
Hours at work	Pre	40.276	40.735	40.202	40.471	40.329	37.839
	Post	39.243	39.611	39.145	39.505	39.244	37.321
Age	Pre	37.511	42.921	36.026	41.455	36.250	32.334
	Post	38.014	43.009	36.583	41.750	36.804	33.108
Age of oldest child	Pre	9.626	21.275	7.284	15.848	7.325	6.595
	Post	9.654	21.311	7.267	15.890	7.290	6.899
Age of youngest child	Pre	5.277	19.375	4.362	7.707	4.353	4.502
	Post	5.316	19.313	4.376	7.770	4.362	4.603
Female	Pre	0.550	0.497	0.549	0.552	0.540	0.699
	Post	0.543	0.499	0.544	0.542	0.535	0.689
White	Pre	0.800	0.799	0.800	0.801	0.807	0.686
	Post	0.801	0.800	0.804	0.794	0.812	0.676
Black	Pre	0.093	0.110	0.090	0.102	0.085	0.164
	Post	0.088	0.107	0.086	0.092	0.082	0.162
Hispanic	Pre	0.187	0.138	0.170	0.232	0.165	0.246
	Post	0.170	0.135	0.154	0.210	0.149	0.243
Married	Pre	0.775	0.475	0.766	0.801	0.792	0.333
	Post	0.803	0.482	0.793	0.829	0.820	0.376
Divorced or separated	Pre	0.082	0.151	0.074	0.102	0.068	0.176
	Post	0.072	0.141	0.067	0.085	0.061	0.173
Single	Pre	0.137	0.355	0.155	0.090	0.135	0.479
	Post	0.121	0.360	0.136	0.081	0.116	0.442
High school dropout	Pre	0.092	0.081	0.076	0.134	0.073	0.115
	Post	0.079	0.070	0.062	0.121	0.061	0.086
High school	Pre	0.251	0.301	0.248	0.259	0.239	0.393
	Post	0.235	0.288	0.230	0.247	0.220	0.382
Some college	Pre	0.261	0.266	0.257	0.269	0.255	0.302
	Post	0.255	0.268	0.251	0.267	0.247	0.320
College	Pre	0.243	0.236	0.255	0.210	0.262	0.138
	Post	0.260	0.247	0.275	0.220	0.283	0.148
COVID-19 related:							
Unable to work due	Post	0.095	0.098	0.094	0.096	0.093	0.103
Teleworked	Post	0.181	0.165	0.187	0.165	0.191	0.127
Prevented looking for work	Post	0.061	0.075	0.062	0.061	0.061	0.063
Observation Count	Pre	447,851	998,343	325,364	122,487	306,760	18,604
	Post	54,589	124,900	39,479	15,110	37,124	2,355

Notes: Sample and research design definitions described in the notes to Figure 1. Calculated without sampling weights. Post-period includes March to June 2020. Samples for hours at work and child ages are limited to those at work and those with children. COVID-19 related variables are available only for May and June 2020.

Table 2.2: Regression adjusted differences between treatment and control groups

Dependent variable:	At Work		Employed		Hours Worked	
	1	2	1	2	1	2
Research Design						
Panel A: All	n=1,625,683	n=502,440	n=1,625,683	n=502,440	n=1,231,552	n=382,952
March 2020	-0.00365 (0.00381)	0.0215** (0.00910)	-0.00120 (0.00326)	0.0198** (0.00764)	0.00120 (0.0144)	-0.00701 (0.329)
April 2020	0.00898* (0.00482)	0.0215** (0.00892)	0.0182 (0.0222)	0.0198** (0.00736)	0.0120 (0.0211)	0.637* (0.340)
May 2020	0.0118** (0.00552)	0.0186** (0.00857)	0.0261 (0.0270)	0.0152* (0.00835)	0.0199 (0.0261)	0.261 (0.373)
June 2020	0.0129* (0.00697)	0.0156 (0.00984)	0.00404 (0.0303)	0.0124 (0.00899)	0.00710 (0.0285)	0.419 (0.407)
Panel B: Women	n=834,217	n=275,819	n=834,217	n=275,819	n=200033	n=179,982
March 2020	-0.00476 (0.00486)	0.0345*** (0.0123)	-0.00527 (0.0215)	0.0307** (0.0119)	0.00392 (0.0200)	0.151 (0.372)
April 2020	0.00572 (0.00676)	0.0374*** (0.0116)	-0.000477 (0.0257)	0.0390*** (0.0110)	0.00107 (0.0245)	0.748 (0.497)
May 2020	0.00440 (0.00761)	0.0414*** (0.0136)	0.0279 (0.0325)	0.0379*** (0.0137)	0.0329 (0.0330)	0.697 (0.535)
June 2020	0.00550 (0.00954)	0.0318** (0.0134)	0.0105 (0.0340)	0.0278* (0.0139)	0.0290 (0.0304)	0.353 (0.568)
Panel C: Men	n=791,466	n=226,621	n=164,810	n=226,621	n=164,810	n=202,970
March 2020	0.000561 (0.00581)	0.00519 (0.0122)	-0.00552 (0.0263)	0.00624 (0.00844)	-0.0115 (0.0245)	-0.140 (0.431)
April 2020	0.0149*** (0.00537)	0.00108 (0.0155)	0.0629* (0.0364)	-0.00281 (0.0108)	0.0377 (0.0326)	0.562 (0.381)
May 2020	0.0227*** (0.00615)	-0.00970 (0.0136)	0.0281 (0.0412)	-0.0129 (0.0106)	-0.000401 (0.0394)	-0.0953 (0.475)
June 2020	0.0251*** (0.00824)	-0.00492 (0.0146)	-0.0181 (0.0443)	-0.00733 (0.0117)	-0.0391 (0.0457)	0.447 (0.482)

Notes: Estimates of equation (1). State-level, clustered standard errors reported in parentheses. Statistically significant estimates for two-tailed tests at the one, five, and ten-percent levels are indicated ***, **, and *, respectively. Sample and research designs as described in the notes to Figure 1.

Table 2.3: Regression adjusted differences between treatment and control groups, weighted regressions

Dependent variable:	At Work			Employed			Hours Worked		
	1	2	3	1	2	3	1	2	3
Research Design									
Panel A: All	n=1,625,683	n=502,440	n=364,843	n=1,625,683	n=502,440	n=364,843	n=1,231,552	n=382,952	n=277,680
March 2020	-0.00486 (0.00491)	0.0232* (0.0128)	-0.00989 (0.0200)	0.000205 (0.00454)	0.0226** (0.0109)	-0.00321 (0.0151)	-0.0937 (0.231)	0.196 (0.378)	0.158 (0.527)
April 2020	0.0131** (0.00565)	0.0199 (0.0126)	0.0210 (0.0271)	0.0139** (0.00601)	0.0197* (0.0110)	0.0160 (0.0233)	-0.00622 (0.200)	1.029** (0.407)	0.369 (0.770)
May 2020	0.0162*** (0.00507)	0.0233** (0.00956)	0.0346 (0.0335)	0.0188*** (0.00526)	0.0201** (0.00930)	0.0242 (0.0301)	0.357* (0.190)	0.233 (0.355)	-0.388 (0.849)
June 2020	0.0183** (0.00731)	0.0152 (0.0121)	0.0138 (0.0351)	0.0234*** (0.00817)	0.0162 (0.0110)	0.00918 (0.0360)	0.594** (0.235)	0.485 (0.411)	-0.708 (0.769)
Panel B: Women	n=834,217	n=275,819	n=200033	n=834,217	n=275,819	n=200033	n=580,015	n=179,982	n=129,848
March 2020	-0.00799 (0.00484)	0.0365* (0.0190)	-0.0127 (0.0257)	-0.00516 (0.00453)	0.0349* (0.0184)	-0.00288 (0.0199)	-0.102 (0.201)	0.394 (0.436)	-0.134 (1.040)
April 2020	0.0138** (0.00682)	0.0440*** (0.0135)	0.0105 (0.0328)	0.0103 (0.00782)	0.0453*** (0.0135)	0.0135 (0.0299)	0.134 (0.290)	1.027* (0.599)	0.246 (0.979)
May 2020	0.00904 (0.00649)	0.0539*** (0.0163)	0.0553 (0.0375)	0.0108 (0.00736)	0.0520*** (0.0160)	0.0544 (0.0355)	0.833*** (0.218)	0.493 (0.508)	-0.279 (1.076)
June 2020	0.0114 (0.00907)	0.0396** (0.0170)	0.0252 (0.0347)	0.0172 (0.0112)	0.0404** (0.0164)	0.0349 (0.0350)	0.778** (0.312)	0.474 (0.577)	-1.536* (0.833)
Panel C: Men	n=791,466	n=226,621	n=164,810	n=791,466	n=226,621	n=164,810	n=651,537	n=202,970	n=147,832
March 2020	-0.000544 (0.00823)	0.00712 (0.0150)	-0.0217 (0.0307)	0.00652 (0.00660)	0.00774 (0.0115)	-0.0141 (0.0270)	-0.120 (0.292)	0.00244 (0.435)	0.558 (0.857)
April 2020	0.0148** (0.00553)	-0.00749 (0.0218)	0.0373 (0.0428)	0.0200*** (0.00620)	-0.00927 (0.0159)	0.0280 (0.0302)	-0.162 (0.302)	1.003** (0.413)	1.049 (1.281)
May 2020	0.0266*** (0.00647)	-0.0131 (0.0188)	0.00169 (0.0521)	0.0299*** (0.00608)	-0.0177 (0.0136)	-0.0184 (0.0420)	-0.113 (0.242)	-0.00937 (0.506)	-0.129 (1.131)
June 2020	0.0302*** (0.00824)	-0.0153 (0.0174)	-0.00647 (0.0557)	0.0329*** (0.00793)	-0.0141 (0.0154)	-0.0375 (0.0534)	0.370 (0.267)	0.452 (0.549)	0.755 (1.155)

Notes: Notes to Table 2 apply except sampling weights are used.

Table 2.4: Regression adjusted differences between treatment and control groups, with industry and occupation controls

Dependent variable:	At Work			Employed			Hours Worked		
	1	2	3	1	2	3	1	2	3
Research Design									
Panel A: All	n=1,625,683	n=502,437	n=364,837	n=1,625,683	n=502,437	n=364,837	n=1,231,552	n=382,948	n=277,673
March 2020	-0.00116 (0.00335)	0.00742 (0.00606)	-0.00611 (0.0114)	0.000754 (0.00197)	0.00521 (0.00394)	-0.00185 (0.00850)	-0.0267 (0.163)	0.0806 (0.332)	0.207 (0.526)
April 2020	0.00771* (0.00412)	0.00515 (0.00784)	0.0274* (0.0144)	0.00985*** (0.00333)	0.00424 (0.00642)	0.0222* (0.0128)	0.0612 (0.172)	0.635* (0.358)	0.570 (0.729)
May 2020	0.00390 (0.00457)	-0.00355 (0.00670)	0.0302 (0.0204)	0.00752* (0.00387)	-0.00795 (0.00521)	0.0265 (0.0165)	0.112 (0.162)	0.401 (0.386)	0.301 (0.679)
June 2020	0.00177 (0.00448)	0.000135 (0.00711)	0.00935 (0.0156)	0.00849* (0.00429)	-0.00391 (0.00556)	0.0130 (0.0136)	0.307 (0.187)	0.464 (0.387)	-0.294 (0.781)
Panel B: Women	n=834,214	n=275,805	n=200,016	n=834,214	n=275,805	n=200,016	n=580,010	n=179,966	n=129,829
March 2020	0.000646 (0.00440)	0.0112 (0.00732)	-0.0121 (0.0150)	0.00198 (0.00310)	0.00646 (0.00597)	-0.00396 (0.0124)	-0.0944 (0.164)	0.234 (0.377)	-0.263 (0.848)
April 2020	0.00979 (0.00616)	0.0103 (0.00889)	0.00694 (0.0190)	0.00811 (0.00497)	0.0121* (0.00706)	0.00865 (0.0168)	0.214 (0.238)	0.721 (0.509)	0.333 (0.863)
May 2020	0.00250 (0.00615)	0.00579 (0.00852)	0.00285 (0.0224)	0.00568 (0.00584)	0.00148 (0.00703)	0.0109 (0.0177)	0.552** (0.207)	0.798 (0.526)	0.0975 (0.924)
June 2020	-0.000251 (0.00579)	0.0122 (0.00822)	-0.0118 (0.0192)	0.00729 (0.00591)	0.00774 (0.00613)	0.00611 (0.0159)	0.525** (0.266)	0.337 (0.548)	-1.477 (0.942)
Panel C: Men	n=791,465	n=226,612	n=164,800	n=791,465	n=226,612	n=164,800	n=651,536	n=202,958	n=147,819
March 2020	-0.00243 (0.00468)	0.00168 (0.00906)	-0.00466 (0.0220)	-0.000336 (0.00271)	0.00300 (0.00534)	-0.00149 (0.0170)	0.0375 (0.226)	0.0368 (0.416)	0.886 (0.885)
April 2020	0.00643 (0.00522)	-0.000890 (0.0134)	0.0553* (0.0281)	0.0126** (0.00486)	-0.00425 (0.00880)	0.0441** (0.0218)	-0.142 (0.256)	0.554 (0.377)	1.495 (1.345)
May 2020	0.00649 (0.00550)	-0.0142 (0.0124)	0.0699* (0.0395)	0.0105** (0.00469)	-0.0172** (0.00834)	0.0427 (0.0359)	-0.342 (0.232)	0.0235 (0.480)	1.172 (1.162)
June 2020	0.00633 (0.00617)	-0.0131 (0.0112)	0.0276 (0.0325)	0.0104* (0.00528)	-0.0149* (0.00885)	0.00602 (0.0316)	0.0380 (0.222)	0.499 (0.470)	1.695 (1.263)

Notes: Notes to Table 2 apply except industry and occupation fixed effects are added to the model.

Table 2-5: Regression adjusted differences between treatment and control groups, alternative group specification

Dependent variable:	At Work			Employed			Hours Worked		
	1	2	3	1	2	3	1	2	3
Research Design									
Panel A: All	n=1,625,683	n=564,914	n=383,292	n=1,625,683	n=564,914	n=383,292	n=1,231,552	n=423,971	n=288,878
March 2020	-0.00433 (0.00392)	0.0208** (0.00911)	0.00752 (0.0188)	-0.00199 (0.00325)	0.0172** (0.00776)	0.0110 (0.0159)	-0.133 (0.161)	0.221 (0.270)	0.0187 (0.600)
April 2020	-0.000953 (0.00433)	0.0265*** (0.00788)	0.0757*** (0.0238)	0.00232 (0.00485)	0.0224*** (0.00711)	0.0722*** (0.0217)	0.0181 (0.189)	0.749** (0.326)	1.004 (0.694)
May 2020	-0.000412 (0.00494)	0.0277*** (0.00844)	0.0742*** (0.0270)	0.00450 (0.00494)	0.0266*** (0.00848)	0.0614** (0.0252)	-0.00592 (0.171)	0.576* (0.303)	-0.0872 (0.657)
June 2020	0.00458 (0.00592)	0.000898 (0.00935)	0.0473* (0.0261)	0.0123* (0.00621)	0.00253 (0.00850)	0.0484** (0.0238)	0.185 (0.193)	0.549 (0.357)	-0.117 (0.757)
Panel B: Women	n=834,217	n=309,081	n=209,328	n=834,217	n=309,081	n=209,328	n=580,015	n=199,890	n=135,356
March 2020	-0.00465 (0.00503)	0.0256** (0.0111)	0.00494 (0.0237)	-0.00279 (0.00387)	0.0237** (0.0108)	0.0157 (0.0194)	-0.129 (0.161)	0.317 (0.343)	-0.443 (0.966)
April 2020	-0.00162 (0.00595)	0.0368*** (0.0102)	0.0480* (0.0272)	-0.00261 (0.00597)	0.0381*** (0.00999)	0.0550** (0.0238)	0.279 (0.258)	0.768* (0.399)	1.298 (0.824)
May 2020	-0.00985 (0.00668)	0.0533*** (0.0115)	0.0640* (0.0352)	-0.00525 (0.00725)	0.0548*** (0.0114)	0.0628* (0.0334)	0.601*** (0.220)	0.910** (0.385)	0.157 (0.765)
June 2020	-0.00230 (0.00805)	0.0154 (0.0130)	0.0521 (0.0335)	0.00611 (0.00893)	0.0195 (0.0129)	0.0679** (0.0298)	0.511** (0.244)	0.628 (0.449)	-0.441 (0.937)
Panel C: Men	n=791,466	n=255,833	n=173,964	n=791,466	n=255,833	n=173,964	n=651,537	n=224,081	n=153,522
March 2020	-0.00197 (0.00606)	0.0154 (0.0119)	0.0129 (0.0235)	0.000457 (0.00508)	0.00955 (0.00902)	0.00616 (0.0200)	-0.142 (0.224)	0.182 (0.350)	0.488 (0.901)
April 2020	0.00253 (0.00559)	0.0150 (0.0144)	0.128*** (0.0289)	0.0101* (0.00576)	0.00423 (0.0108)	0.109*** (0.0296)	-0.284 (0.248)	0.773* (0.415)	1.038 (1.209)
May 2020	0.0128** (0.00556)	-0.00211 (0.0118)	0.105*** (0.0311)	0.0177*** (0.00520)	-0.00634 (0.0109)	0.0771** (0.0292)	-0.599** (0.256)	0.267 (0.425)	-0.0341 (0.972)
June 2020	0.0161** (0.00745)	-0.0152 (0.0116)	0.0551 (0.0337)	0.0212*** (0.00688)	-0.0163* (0.00960)	0.0380 (0.0316)	-0.195 (0.235)	0.449 (0.449)	0.631 (0.932)

Notes: Notes to Table 2 apply except treatment and control groups are redefined to account for all children in a household, as described in the text.

Table 2.6: Regression adjusted differences between treatment and control groups, sample restricted to respondents in 3rd, 4th, 7th, or 8th month in the CPS sample

Dependent variable: Research Design	At Work			Employed			Hours Worked		
	1	2	3	1	2	3	1	2	3
	n=829,721	n=256,599	n=186287	n=829,721	n=256,599	n=186287	n=627,888	n=195,380	n=141,647
March 2020	-0.00864 (0.00666)	0.0163 (0.0143)	-0.0112 (0.0249)	-0.00838 (0.00506)	0.0148 (0.0126)	-0.0136 (0.0232)	0.0641 (0.238)	-0.164 (0.445)	-0.252 (0.675)
April 2020	0.00229 (0.00692)	0.0235 (0.0148)	0.0280 (0.0305)	0.00275 (0.00641)	0.0204* (0.0131)	0.0112 (0.0319)	-0.0182 (0.278)	0.635 (0.434)	0.0183 (1.122)
May 2020	0.00120 (0.00910)	0.0168 (0.0145)	0.0850** (0.0347)	0.00303 (0.00777)	0.00983 (0.0127)	0.0727** (0.0341)	0.145 (0.276)	0.164 (0.481)	-0.0426 (0.890)
June 2020	0.0139 (0.00927)	0.0220 (0.0148)	0.0357 (0.0314)	0.0169* (0.00900)	0.00743 (0.0139)	0.0210 (0.0316)	0.228 (0.301)	0.115 (0.650)	-0.163 (0.969)

Notes: Notes to Table 2 apply except the samples are restricted to include only respondents whose month-in-sample is 3, 4, 7, or 8.

Table 2.7: Sub-group heterogeneity of effects using a standard difference-in-differences model

Dependent variable:	At Work			Employed			Hours Worked		
Research Design	1	2	3	1	2	3	1	2	3
	n=1,578,417	n=487,895	n=354,310	n=1,578,417	n=487,895	n=354,310	n=1,195,624	n=371,883	n=269,608
Full Sample	0.00724* (0.00370)	0.0205*** (0.00644)	0.0290 (0.0176)	0.00935** (0.00363)	0.0173*** (0.00540)	0.0228 (0.0186)	0.000503 (0.126)	0.195 (0.205)	-0.565 (0.485)
Single	-0.0174** (0.00693)	0.0110 (0.0155)	-0.0131 (0.0215)	-0.00575 (0.00732)	0.00581 (0.0159)	-0.00314 (0.0218)	-0.236 (0.224)	-0.0612 (0.494)	-0.845 (0.642)
Women only	-0.0192** (0.00934)	0.00783 (0.0184)	-0.0197 (0.0251)	-0.00914 (0.00934)	0.0160 (0.0178)	-0.00733 (0.0244)	-0.287 (0.208)	0.0514 (0.644)	-1.291* (0.717)
Men only	-0.0245* (0.0134)	0.0207 (0.0308)	0.00713 (0.0366)	-0.0113 (0.0140)	-0.0163 (0.0280)	0.0148 (0.0382)	-0.321 (0.400)	-0.633 (0.909)	0.592 (1.320)
Married	-0.00134 (0.00460)	0.0255*** (0.00840)	0.0346* (0.0204)	-0.000203 (0.00438)	0.0220*** (0.00765)	0.0205 (0.0216)	-0.0189 (0.140)	0.253 (0.243)	-0.711 (0.656)
Women only	0.00345 (0.00633)	0.0533*** (0.0111)	0.0189 (0.0317)	0.00342 (0.00624)	0.0483*** (0.0112)	0.0152 (0.0274)	0.268 (0.219)	0.0824 (0.442)	-0.797 (1.066)
Men only	-0.00543 (0.00499)	-0.00372 (0.0102)	0.0471* (0.0251)	-0.00291 (0.00423)	-0.00597 (0.00821)	0.0218 (0.0278)	-0.193 (0.192)	0.343 (0.257)	-0.597 (1.069)
White	0.00840* (0.00433)	0.0203*** (0.00645)	0.0310 (0.0222)	0.0110*** (0.00408)	0.0167*** (0.00607)	0.0244 (0.0227)	-0.0417 (0.140)	0.0355 (0.228)	-0.529 (0.630)
Black	-0.00161 (0.0112)	0.0170 (0.0264)	-0.00936 (0.0467)	0.00116 (0.0112)	0.0161 (0.0218)	-0.00650 (0.0390)	-0.0351 (0.292)	1.239** (0.566)	-1.937* (1.032)
Hispanic	0.00736 (0.0108)	0.0271 (0.0166)	0.00298 (0.0374)	0.00636 (0.00960)	0.0184 (0.0184)	-0.0222 (0.0404)	0.114 (0.283)	0.728** (0.357)	-0.405 (0.691)
High school non-grad	-0.0178 (0.0166)	0.0666*** (0.0193)	0.0416 (0.0582)	-0.0140 (0.0160)	0.0580*** (0.0162)	0.0490 (0.0561)	-0.654 (0.509)	-0.540 (0.609)	0.467 (1.813)
High school grad	-0.00497 (0.00722)	0.00606 (0.0129)	-0.0109 (0.0309)	0.00170 (0.00714)	0.00602 (0.0128)	-0.00723 (0.0290)	0.0871 (0.202)	0.0676 (0.424)	-0.997 (0.657)
Some college	-0.00700 (0.00889)	0.00184 (0.0110)	0.00646 (0.0211)	0.000112 (0.00890)	-0.00796 (0.00952)	-0.00263 (0.0243)	0.217 (0.237)	0.00622 (0.410)	-0.670 (0.879)
College grad	0.0231*** (0.00542)	0.0123 (0.00910)	0.0519 (0.0324)	0.0209*** (0.00542)	0.0160* (0.00871)	0.0503 (0.0343)	0.0696 (0.214)	0.360 (0.313)	-0.463 (0.725)
Urban resident	0.0117 (0.00723)	0.0326** (0.0126)	0.0101 (0.0375)	0.0126* (0.00699)	0.0265** (0.0119)	-0.00152 (0.0362)	0.291 (0.257)	0.505 (0.532)	-0.780 (0.838)
Suburban resident	0.00634 (0.00503)	0.0192 (0.0118)	0.0451** (0.0213)	0.00664 (0.00528)	0.0191* (0.0101)	0.0318 (0.0233)	0.0261 (0.194)	0.278 (0.294)	-0.264 (0.652)
Rural resident	-0.00180 (0.00804)	0.0114 (0.0133)	0.0205 (0.0468)	0.00577 (0.00876)	-0.00115 (0.0118)	0.0297 (0.0450)	-0.495 (0.297)	0.365 (0.536)	-1.759 (1.391)
Metro unknown	0.0116 (0.00860)	0.0271* (0.0138)	0.0143 (0.0515)	0.0149* (0.00858)	0.0295** (0.0142)	0.0209 (0.0477)	-0.0490 (0.282)	-0.613 (0.509)	-0.305 (1.246)

Notes: Notes for Table 2 apply except estimates are of equation (2), data for the month of March 2020 is excluded from all samples, and each sample is limited to the relevant sub-population. For estimates by race we limit to only those respondents that specify one race. Sample sizes at the top of each column are for the full sample.

Table 2.8: Regression adjusted differences between treatment and control groups for COVID-19 questions

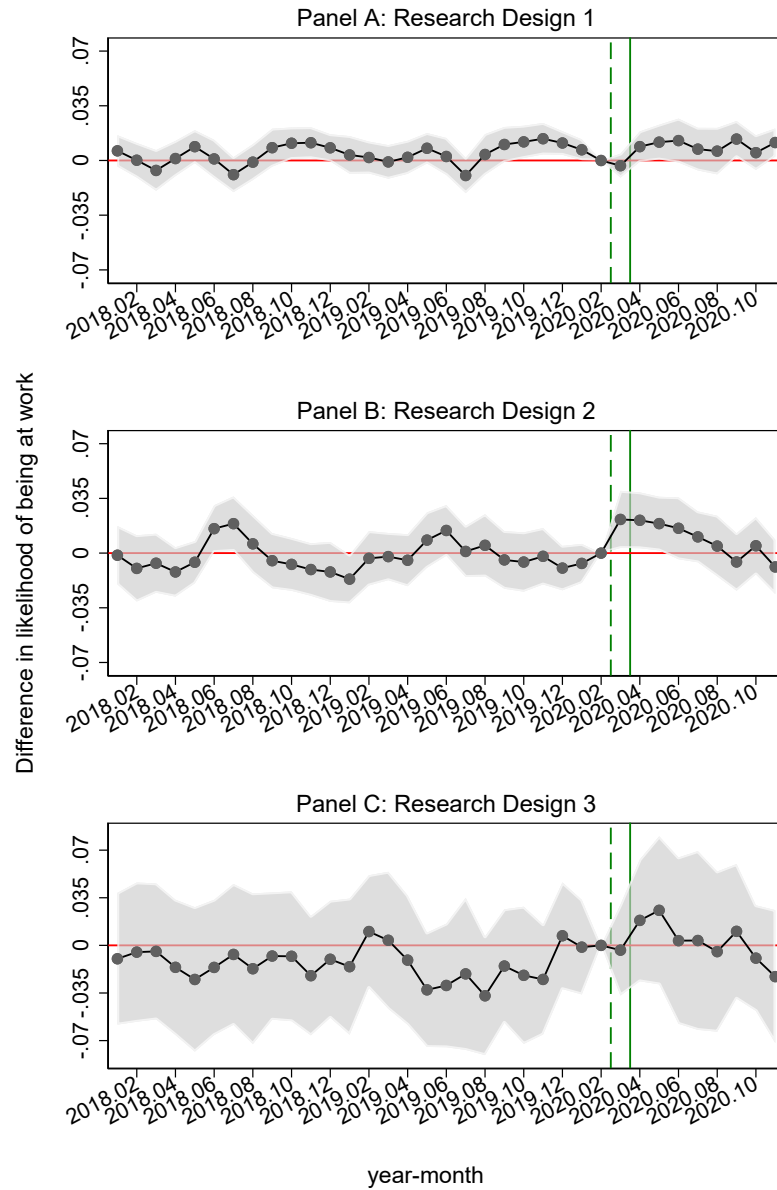
Dependent variable: Research Design	Teleworked			Unable to Work			Prevented Looking for Work		
	1	2	3	1	2	3	1	2	3
Panel A: All	n= 59,198	n=18,094	n=13,149	n=86,451	n=25,999	n=18,780	n= 16,928	n=4,843	n=3,389
May 2020	0.0111* (0.00638)	0.0130 (0.0117)	0.0299 (0.0273)	0.00288 (0.00531)	-0.00121 (0.00756)	0.0235 (0.0236)	-0.0397*** (0.0101)	0.00657 (0.0128)	0.0439 (0.0482)
June 2020	0.0111** (0.00515)	0.0292** (0.0121)	0.00402 (0.0259)	0.00278 (0.00487)	0.00680 (0.00786)	0.0375*** (0.0140)	-0.0354*** (0.00928)	-0.0207** (0.00970)	0.110*** (0.0282)
Panel B: Women	n= 27,455	n=8,118	n=5,955	n=44,254	n=14,072	n=10,173	n= 11,277	n= 4,141	n=2,895
May 2020	0.0188** (0.00912)	0.00624 (0.0186)	0.0366 (0.0357)	-0.00704 (0.00802)	-0.00740 (0.00923)	0.0455* (0.0246)	-0.0501*** (0.0141)	0.000553 (0.0131)	0.0278 (0.0451)
June 2020	0.0235** (0.00930)	0.0262 (0.0180)	0.0133 (0.0335)	-0.00128 (0.00640)	0.00565 (0.0108)	0.0491*** (0.0175)	-0.0421*** (0.0134)	-0.0120 (0.0103)	0.0914*** (0.0336)
Panel C: Men	n=31,743	n= 9,976	n=7,194	n=42,197	n=11,927	n=8,607	n= 5,651	n=702	n=493
May 2020	0.00556 (0.00769)	0.0186 (0.0136)	0.0262 (0.0413)	0.0128* (0.00649)	0.00544 (0.0119)	-0.0167 (0.0381)	0.0436* (0.0224)	0.00701 (0.0412)	0.160 (0.124)
June 2020	0.00142 (0.00708)	0.0311** (0.0136)	-0.00108 (0.0339)	0.00597 (0.00621)	0.00852 (0.00941)	0.0231 (0.0285)	0.0282 (0.0264)	-0.101** (0.0468)	0.161 (0.0981)
Panel D: MIS=3,4,7,8	n= 32,149	n=9,776	n=7,082	n=47,254	n=14,199	n=10,225	n=9,379	n=2,761	n=1,937
May 2020	0.00232 (0.00924)	0.0293* (0.0163)	0.0721* (0.0361)	0.0110 (0.00823)	-0.0120 (0.0118)	0.0222 (0.0314)	-0.0205* (0.0122)	0.00519 (0.0155)	0.0275 (0.0585)
June 2020	0.00448 (0.00826)	0.0383** (0.0179)	0.0357 (0.0299)	-0.00322 (0.00855)	0.00582 (0.0113)	0.0242 (0.0201)	-0.0393*** (0.0125)	-0.0113 (0.0163)	0.0967** (0.0362)

Notes: Estimates of equation (3). State-level, clustered standard errors reported in parentheses. Statistically significant estimates for two-tailed tests at the one, five, and ten-percent levels are indicated ***, **, and *, respectively. Research designs definitions and base sample, limited to May and June of 2020, are as described in the notes to Figure 1. “Teleworked” means that the respondent teleworked or worked at home for pay, and the sample is limited to those who were at work. “Unable to work” means that the respondent was not able to work because of the employer closed or lost business due to COVID-19; the question was asked of the full sample. “Prevented looking for work” means that the COVID-19 prevented the respondent from looking for work, and the sample is limited to those who were out of the labor force.

Appendices

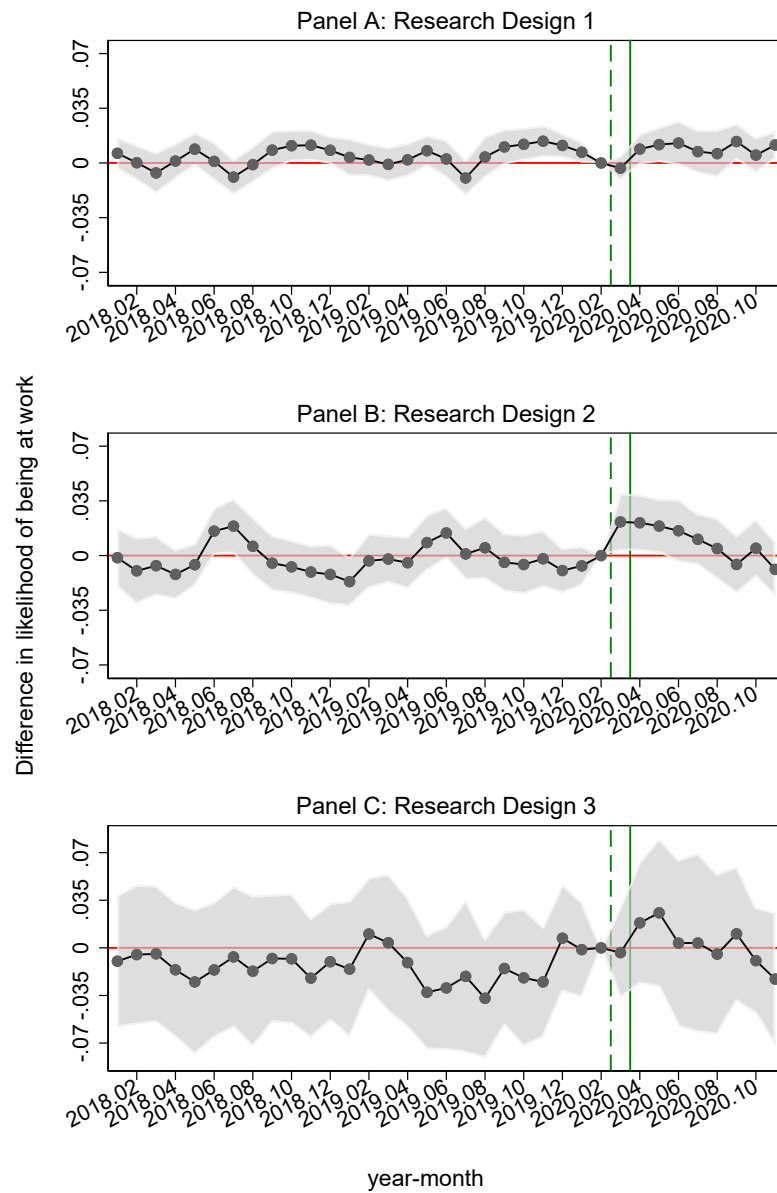
Appendix A Additional Results for Young Children and Parents' Labor Supply During Covid-19

Figure A.1: Difference in likelihood of being at work (treated group minus control) through November



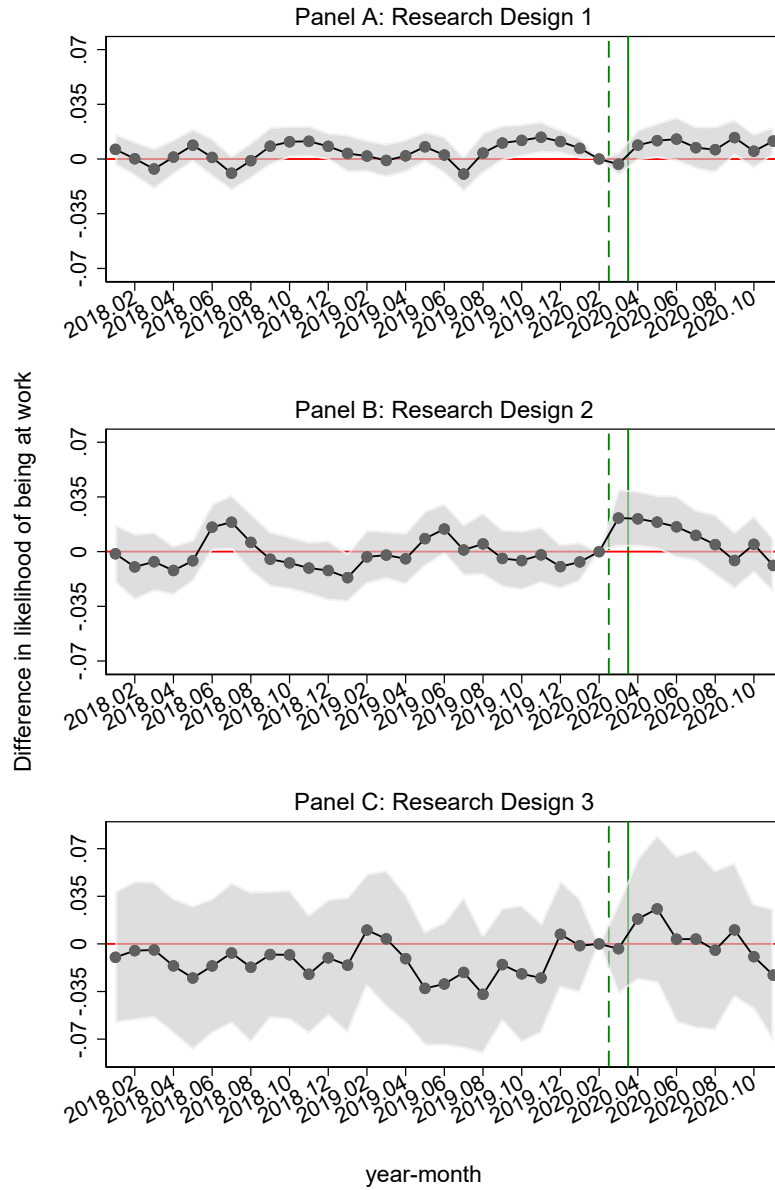
Notes: Notes to Figure 1 apply, except the sample period extends to November 2020.

Figure A.2: Difference in likelihood of being employed (treated group minus control) through November



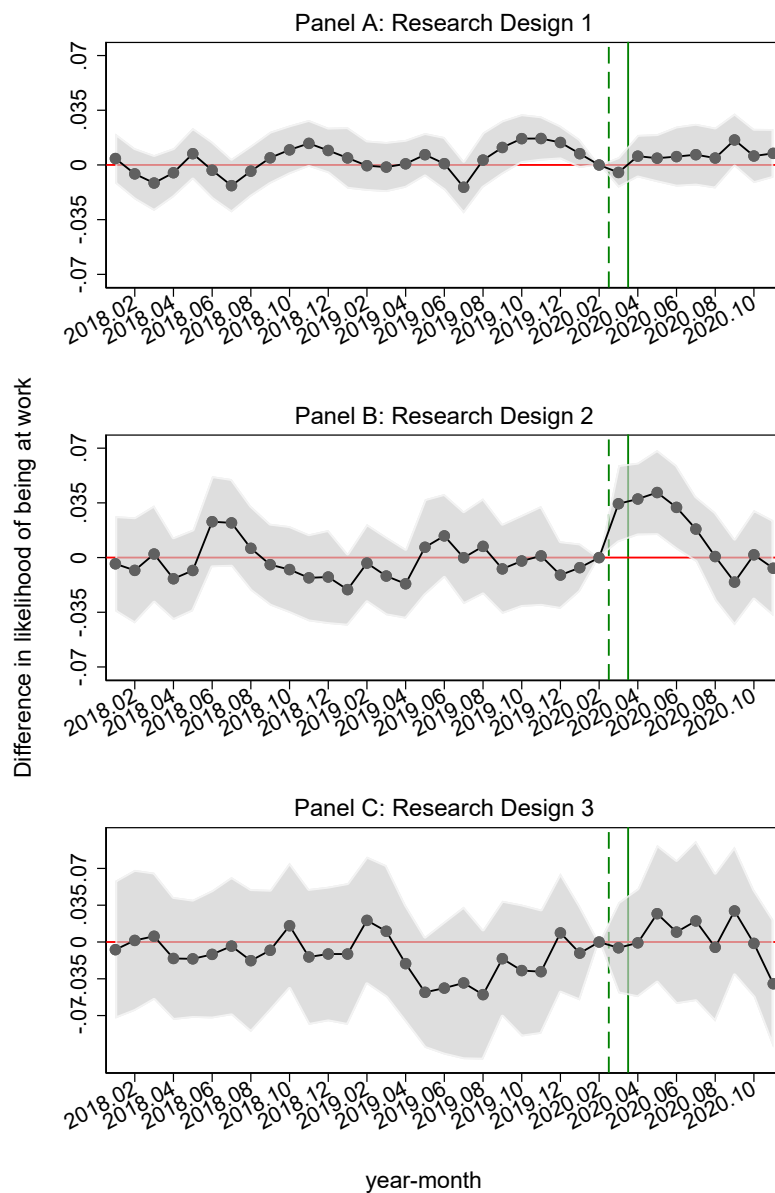
Notes: Notes to Figure 2 apply, except the sample period extends to November 2020.

Figure A.3: Difference in hours worked (treated group minus control) through November



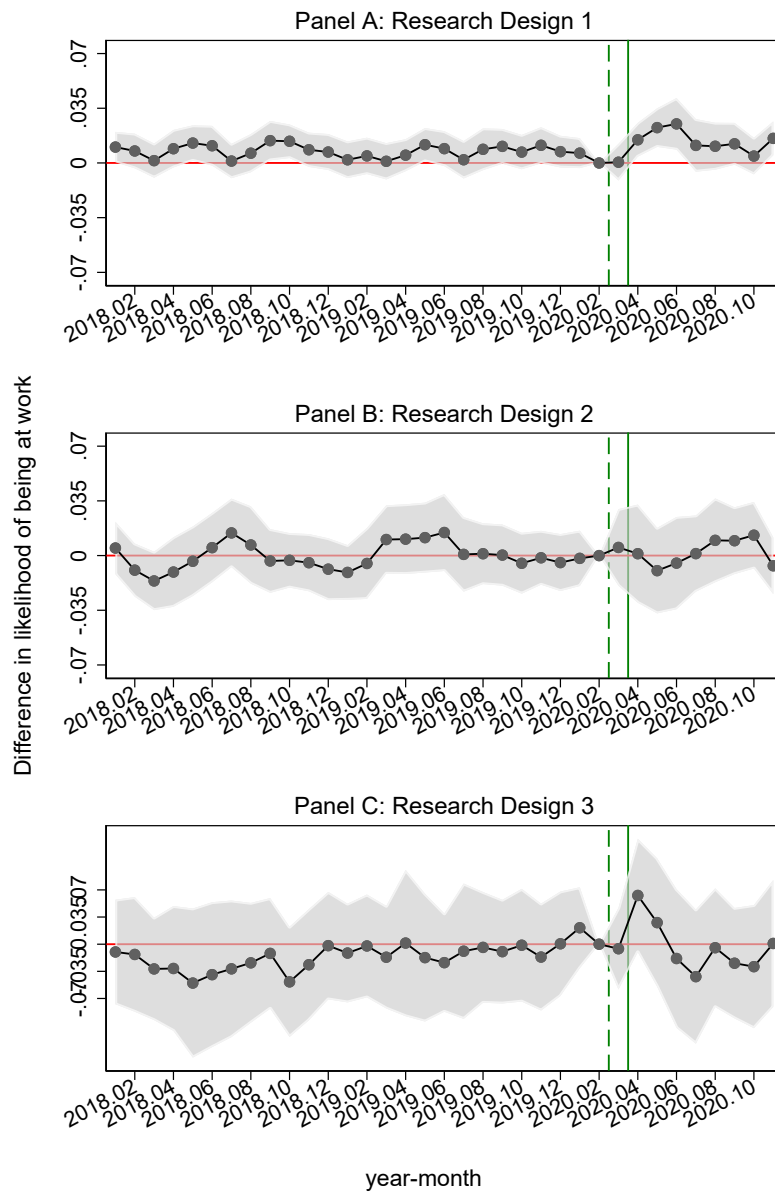
Notes: Notes to Figure 3 apply, except the sample period extends to November 2020.

Figure A.4: Difference in likelihood of being at work (treated group minus control) for Women through November



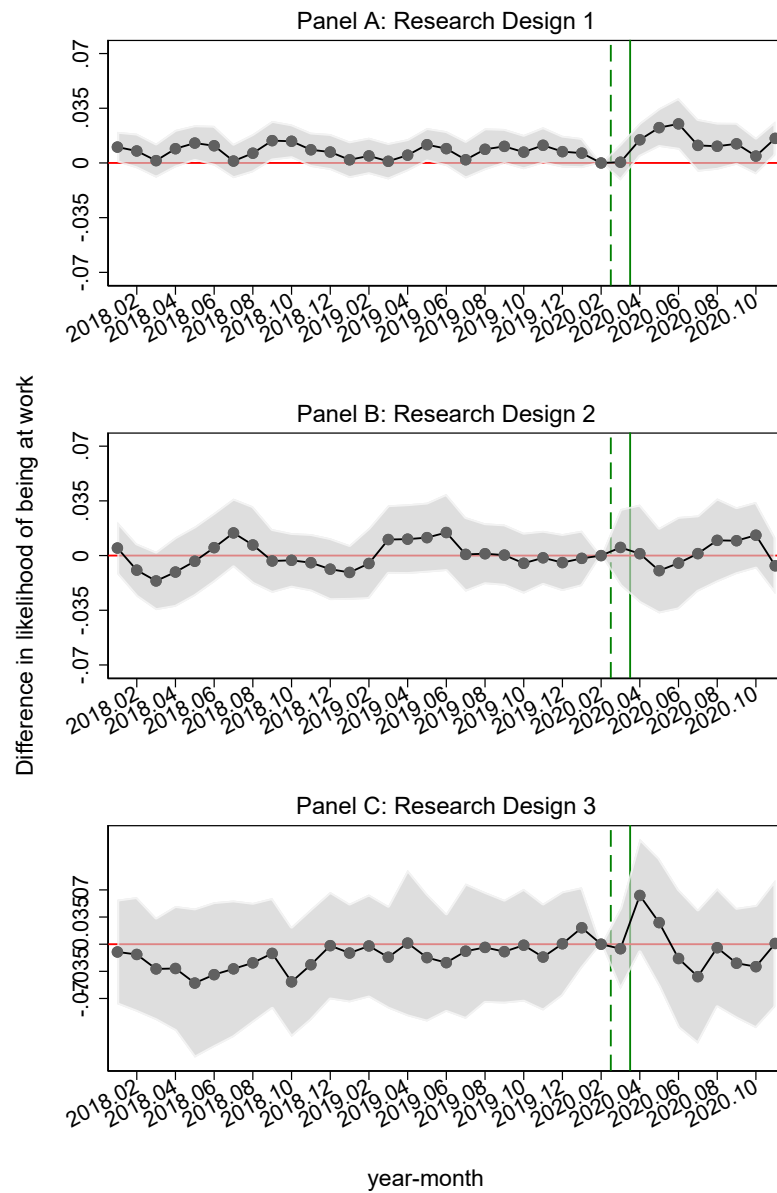
Notes: Notes to Figure 1 apply, except the sample period extends to November 2020.

Figure A.5: Difference in likelihood of being at work (treated group minus control) for Men through November



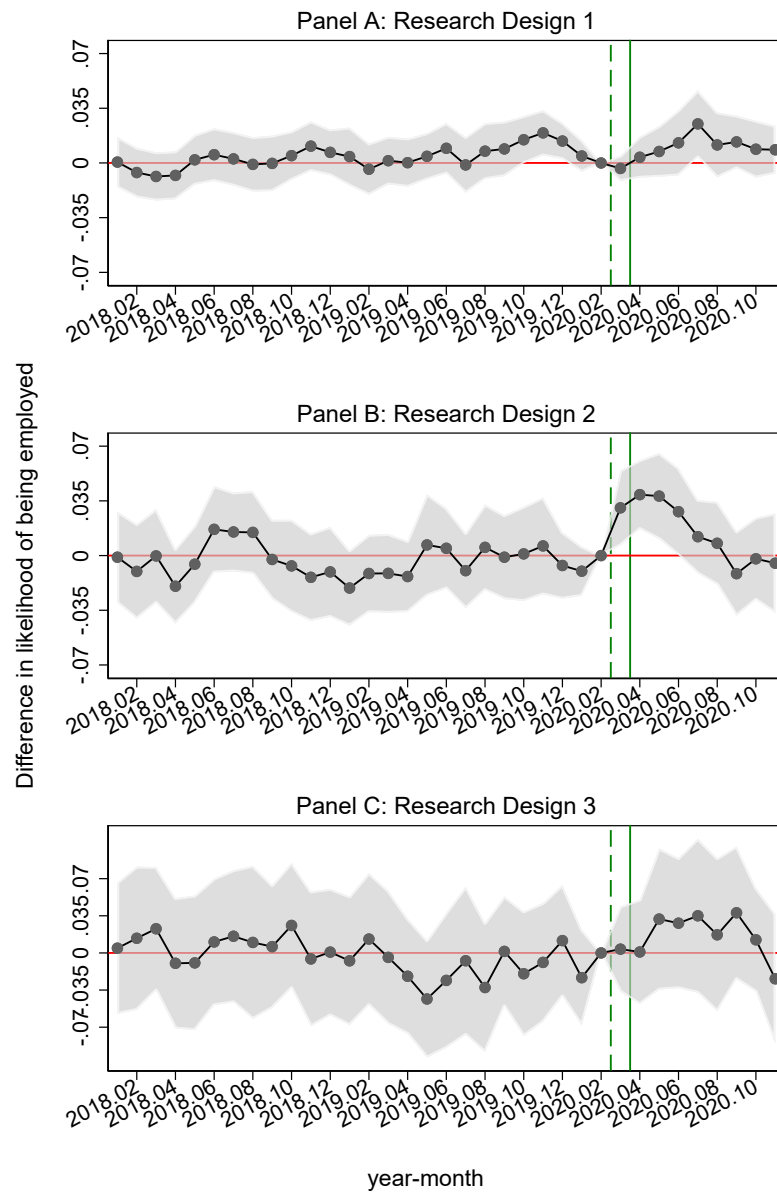
Notes: Notes to Figure 1 apply, except the sample period extends to November 2020.

Figure A.6: Difference in likelihood of being at work (treated group minus control) for Men through November



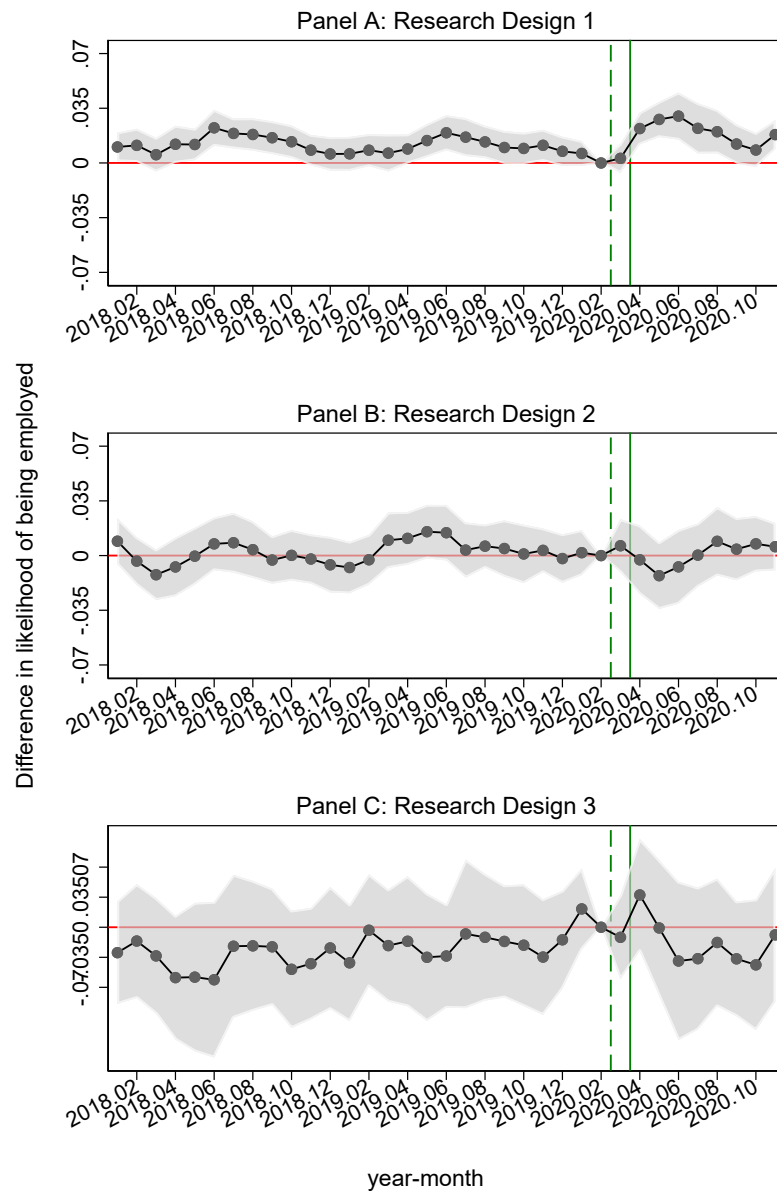
Notes: Notes to Figure 1 apply, except the sample period extends to November 2020.

Figure A.7: Difference in likelihood of being employed (treated group minus control) for Women through November



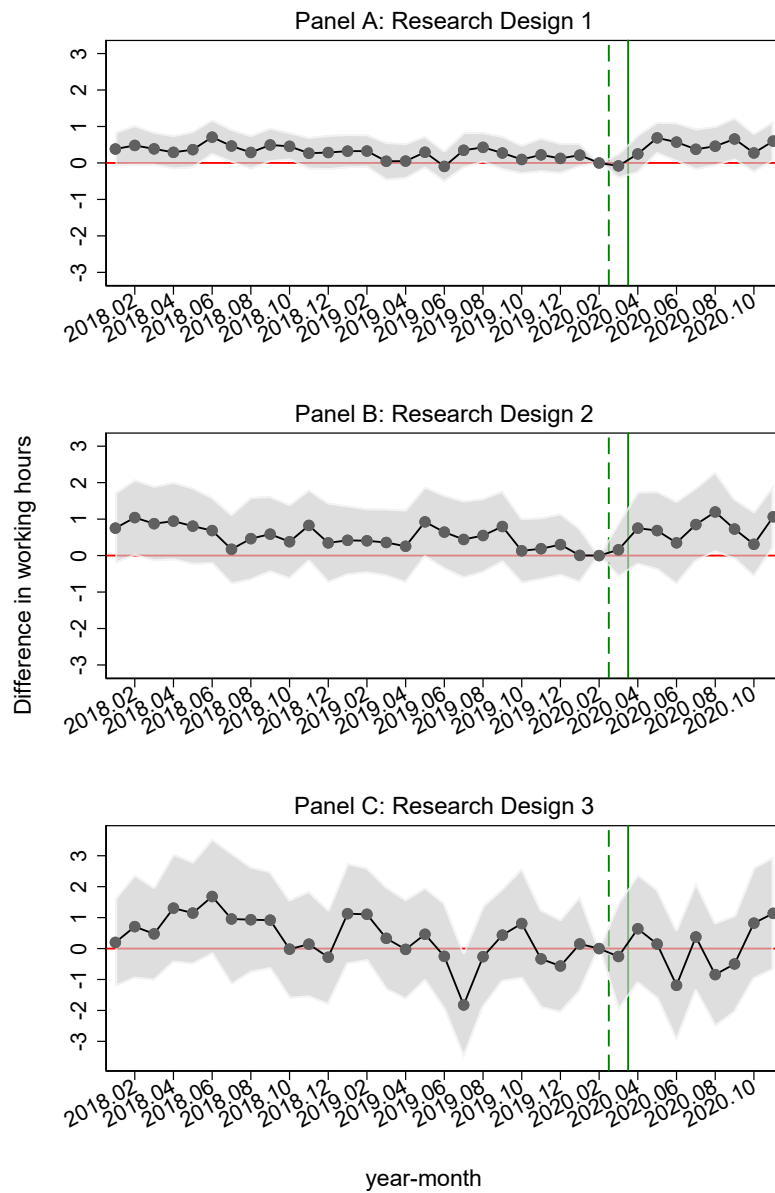
Notes: Notes to Figure 2 apply, except the sample period extends to November 2020.

Figure A.8: Difference in likelihood of being employed (treated group minus control) for Men through November



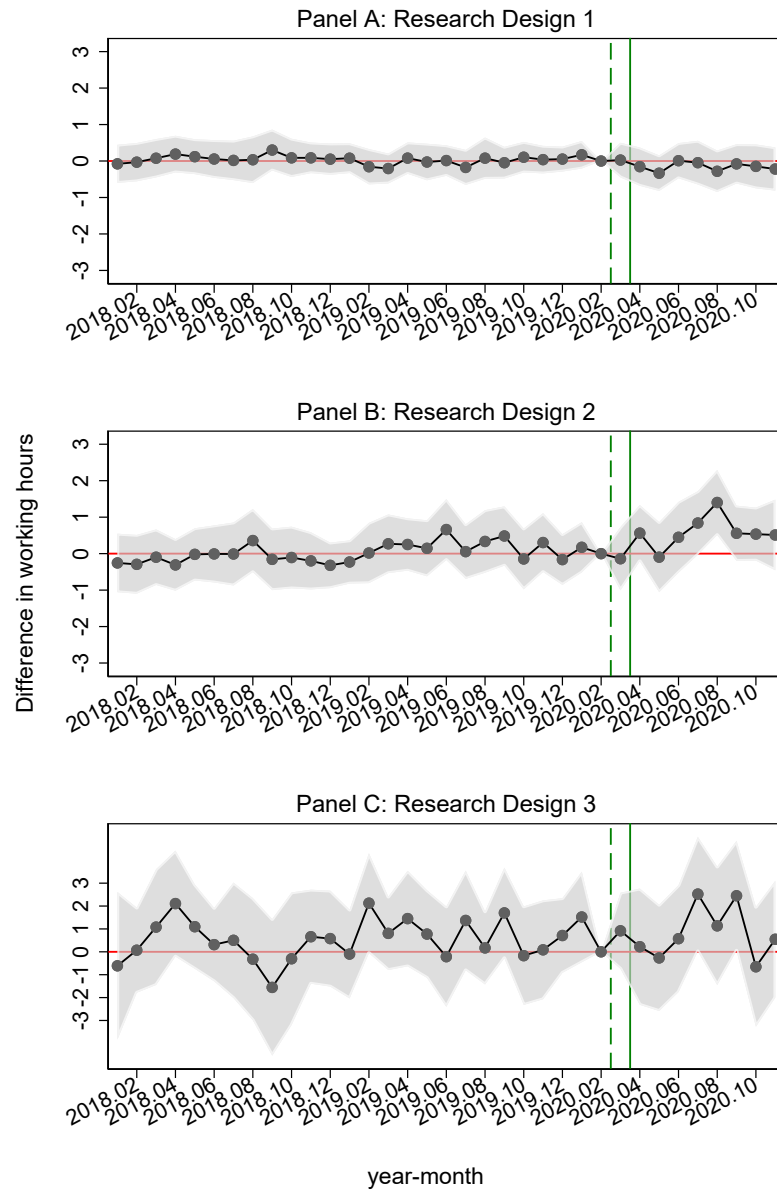
Notes: Notes to Figure 2 apply, except the sample period extends to November 2020.

Figure A.9: Difference in hours worked (treated group minus control) for Women through November



Notes: Notes to Figure 3 apply, except the sample period extends to November 2020.

Figure A.10: Difference in hours worked (treated group minus control) for Men through November



Notes: Notes to Figure 3 apply, except the sample period extends to November 2020.

Table A.1: Regression adjusted differences between treatment and control groups, with partial pre-period estimates

Dependent variable: Research Design	At Work			Employed			Hours Worked		
	1 n=1,625,683	2 n=501,655	3 n=364,843	1 n=1,625,683	2 n=502,440	3 n=364,843	1 n=1,231,552	2 n=382,952	3 n=277,680
September 2019	0.0102* (0.00557)	-0.00453 (0.00915)	-0.0147 (0.0207)	0.00882** (0.00551)	0.000854 (0.00849)	-0.00574 (0.0199)	0.125 (0.181)	0.611 (0.370)	0.697 (0.536)
October 2019	0.0119** (0.00511)	-0.00592 (0.00942)	-0.0215 (0.0252)	0.0119** (0.00480)	-0.00142 (0.00943)	-0.0216 (0.0238)	0.113 (0.165)	-0.0631 (0.367)	0.326 (0.704)
November 2019	0.0139*** (0.00499)	-0.00220 (0.00897)	-0.0244 (0.0201)	0.0151*** (0.00488)	0.00270 (0.00872)	-0.0168 (0.0200)	0.132 (0.155)	0.218 (0.348)	-0.374 (0.544)
December 2019	0.0111*** (0.00375)	-0.00963 (0.00710)	0.00763 (0.0195)	0.0111*** (0.00375)	-0.00590 (0.00708)	0.00472 (0.0184)	0.0807 (0.140)	0.0489 (0.284)	-0.292 (0.588)
January 2020	0.00681** (0.00332)	-0.00661 (0.00620)	-0.000917 (0.0173)	0.00522 (0.00326)	-0.00701 (0.00586)	-0.00924 (0.0173)	0.207 (0.149)	0.0635 (0.274)	0.446 (0.593)
February 2020 – Reference Period									
March 2020	-0.00325 (0.00381)	0.0215** (0.00910)	-0.00333 (0.0167)	-0.00120 (0.00326)	0.0198** (0.00764)	0.00120 (0.0144)	-0.0205 (0.177)	-0.00701 (0.329)	0.204 (0.523)
April 2020	0.00898* (0.00482)	0.0208** (0.00892)	0.0182 (0.0222)	0.0115** (0.00468)	0.0198** (0.00763)	0.0120 (0.0211)	0.0690 (0.181)	0.637* (0.340)	0.285 (0.722)
May 2020	0.0118** (0.00555)	0.0186** (0.00857)	0.0261 (0.0270)	0.0161*** (0.00539)	0.0152* (0.00835)	0.0199 (0.0261)	0.179 (0.168)	0.261 (0.373)	-0.238 (0.680)
June 2020	0.0129* (0.00697)	0.0156 (0.00984)	0.00404 (0.0303)	0.0200*** (0.00720)	0.0124 (0.00899)	0.00710 (0.0285)	0.325 (0.202)	0.419 (0.407)	-0.589 (0.770)

Notes: Notes to Table 2 apply.

Table A.2: Regression adjusted differences between treatment and control groups, with minimum controls

Dependent variable:	At Work			Employed			Hours Worked		
	1	2	3	1	2	3	1	2	3
Research Design									
Panel A: All	n=1,625,683	n=502,440	n=364,843	n=1,625,683	n=502,440	n=364,843	n=1,231,552	n=382,518	n=277,680
March 2020	-0.00359 (0.00415)	0.0209** (0.00920)	-0.00309 (0.0168)	-0.00154 (0.00335)	0.0209** (0.00920)	0.00206 (0.0148)	0.00422 (0.186)	-0.114 (0.332)	0.0286 (0.582)
April 2020	0.00946* (0.00524)	0.0209** (0.00946)	0.0238 (0.0212)	0.0119** (0.00493)	0.0209** (0.00946)	0.0180 (0.0208)	0.179 (0.187)	0.490 (0.331)	0.666 (0.731)
May 2020	0.0119* (0.00693)	0.0184* (0.00931)	0.0273 (0.0262)	0.0162** (0.00679)	0.0184* (0.00931)	0.0219 (0.0251)	0.297* (0.172)	0.0568 (0.359)	-0.160 (0.711)
June 2020	0.0103 (0.00693)	0.0153 (0.0115)	0.00956 (0.0288)	0.0176** (0.00857)	0.0153 (0.0115)	0.0133 (0.0269)	0.414** (0.202)	0.261 (0.410)	-0.386 (0.729)
Panel B: Women	n=834,217	n=275,819	n=200,033	n=834,217	n=275,819	n=200,033	n=580,015	n=179,982	n=129,848
March 2020	-0.00561 (0.00513)	0.0343*** (0.0128)	0.00109 (0.0228)	-0.00411 (0.00436)	0.0307*** (0.0123)	0.0144 (0.0218)	-0.0902 (0.176)	0.169 (0.360)	-0.262 (0.873)
April 2020	0.00290 (0.00683)	0.0395*** (0.0133)	-0.00521 (0.0259)	0.000943 (0.00670)	0.0414*** (0.0129)	-0.00227 (0.0251)	0.274 (0.268)	0.730 (0.506)	0.694 (0.881)
May 2020	0.00136 (0.00884)	0.0447*** (0.0164)	0.0182 (0.0334)	0.00470 (0.00927)	0.0416** (0.0162)	0.0239 (0.0334)	0.724*** (0.223)	0.634 (0.521)	-0.0668 (0.914)
June 2020	0.000683 (0.0110)	0.0366** (0.0161)	0.00353 (0.0351)	0.00791 (0.0123)	0.0331* (0.0166)	0.0230 (0.0309)	0.558** (0.268)	0.374 (0.566)	-1.312 (0.880)
Panel C: Men	n=791,466	n=226,621	n=164,810	n=791,466	n=226,621	n=164,810	n=651,537	n=202,970	n=147,832
March 2020	-0.00202 (0.00609)	0.00676 (0.0119)	-0.00660 (0.0266)	0.000386 (0.00465)	0.00755 (0.0817)	-0.0123 (0.0254)	-0.00338 (0.247)	-0.169 (0.433)	0.794 (0.833)
April 2020	0.0159** (0.00626)	-0.00134 (0.0158)	0.0722* (0.0366)	0.0230*** (0.00536)	-0.00516 (0.0110)	0.0480 (0.0345)	-0.214 (0.264)	0.496 (0.378)	0.201 (1.310)
May 2020	0.0212*** (0.00716)	-0.0123 (0.0134)	0.0314 (0.0418)	0.0265*** (0.00660)	-0.0153 (0.0105)	0.00377 (0.0440)	-0.409 (0.253)	-0.140 (0.493)	-0.171 (1.157)
June 2020	0.0206** (0.00909)	-0.00857 (0.0148)	-0.00395 (0.0454)	0.0257*** (0.00827)	-0.0105 (0.0119)	-0.0233 (0.0464)	-0.0594 (0.245)	0.408 (0.493)	0.811 (1.131)

Notes: Notes to Table 2 apply except the only controls included are year-month fixed effects.

Table A.3: Regression adjusted differences between treatment and control groups, controlling for the youngest child's age fixed effects

Dependent variable:	At Work			Employed			Hours Worked		
	1	2	3	1	2	3	1	2	3
Research Design									
Panel A: All	n=502,440	n=502,440	n=364,843	n=502,440	n=502,440	n=364,843	n=382,952	n=382,952	n=277,680
March 2020	0.0216** (0.00906)	-0.000194 (0.0164)	0.000194 (0.0164)	0.0202** (0.00768)	0.0202** (0.00768)	0.00339 (0.0142)	0.00326 (0.330)	0.00326 (0.330)	0.230 (0.524)
April 2020	0.0217** (0.00934)	0.0193 (0.0220)	0.0193 (0.0220)	0.0209** (0.00782)	0.0209** (0.00782)	0.0128 (0.0207)	0.650* (0.337)	0.650* (0.337)	0.297 (0.726)
May 2020	0.0184** (0.00880)	0.0269 (0.0269)	0.0269 (0.0269)	0.0155* (0.00840)	0.0155* (0.00840)	0.0206 (0.0258)	0.272 (0.370)	0.272 (0.370)	-0.233 (0.679)
June 2020	0.0166 (0.0101)	0.00491 (0.0299)	0.00491 (0.0299)	0.0133 (0.00907)	0.0133 (0.00907)	0.00749 (0.0281)	0.434 (0.409)	0.434 (0.409)	-0.576 (0.770)
Panel B: Women	n=275,819	n=275,819	n=200033	n=275,819	n=275,819	n=200033	n=179,982	n=179,982	n=129,848
March 2020	0.0348*** (0.0121)	0.000635 (0.0208)	0.000635 (0.0208)	0.0314** (0.0120)	0.0314** (0.0120)	0.00832 (0.0193)	0.181 (0.373)	0.181 (0.373)	-0.186 (0.856)
April 2020	0.0399*** (0.0114)	0.00230 (0.0257)	0.00230 (0.0257)	0.0416*** (0.0110)	0.0416*** (0.0110)	0.00357 (0.0241)	0.800 (0.492)	0.800 (0.492)	0.674 (0.880)
May 2020	0.0417*** (0.0131)	0.0290 (0.0325)	0.0290 (0.0325)	0.0390*** (0.0135)	0.0390*** (0.0135)	0.0343 (0.0326)	0.751 (0.540)	0.751 (0.540)	0.133 (0.883)
June 2020	0.0337** (0.0136)	0.00964 (0.0330)	0.00964 (0.0330)	0.0296** (0.0139)	0.0296** (0.0139)	0.0284 (0.0298)	0.378 (0.572)	0.378 (0.572)	-1.203 (0.890)
Panel C: Men	n=226,612	n=226,612	n=164,810	n=226,621	n=226,621	n=164,810	n=202,970	n=202,970	n=147,832
March 2020	0.00168 (0.0906)	-0.00512 (0.0262)	-0.00512 (0.0262)	0.00638 (0.00845)	0.00638 (0.00845)	-0.0113 (0.0245)	-0.146 (0.431)	-0.146 (0.431)	0.923 (0.832)
April 2020	-0.000890 (0.0134)	0.0626* (0.0364)	0.0626* (0.0364)	-0.00261 (0.0108)	-0.00261 (0.0108)	0.0373 (0.0326)	0.557 (0.382)	0.557 (0.382)	0.219 (1.259)
May 2020	-0.0142 (0.0124)	0.0279 (0.0412)	0.0279 (0.0412)	-0.0127 (0.0106)	-0.0127 (0.0106)	-0.0000990 (0.0393)	-0.103 (0.473)	-0.103 (0.473)	-0.290 (1.149)
June 2020	-0.0131 (0.0112)	-0.0179 (0.0443)	-0.0179 (0.0443)	-0.00698 (0.0116)	-0.00698 (0.0116)	-0.0395 (0.0457)	0.437 (0.481)	0.437 (0.481)	0.540 (1.160)

Notes: Notes to Table 2 apply except fixed effects for the age of the youngest child are added to the model. Estimates are not calculated for research design 1 since some members of the research design 1 control group do not have children or are not living with one.

Table A.4: Regression adjusted differences between treatment and control groups through November 2020

Dependent variable:	At Work			Employed			Hours Worked		
Research Design	1	2	3	1	2	3	1	2	3
All	n=1,866,419	n=575,778	n=418,043	n=1,866,419	n=575,778	n=418,043	n=1,405,610	n=436,386	n=316,427
March 2020	-0.00333	0.0215**	-0.00350	-0.00127	0.0198**	0.00101	-0.0193	-0.00667	0.208
	(0.00381)	(0.00910)	(0.0166)	(0.00327)	(0.00763)	(0.0143)	(0.177)	(0.329)	(0.521)
	0.00888*	0.0210**	0.0184	0.0114**	0.0200**	0.0122	0.0719	0.637*	0.299
April 2020	(0.00482)	(0.00889)	(0.0223)	(0.00467)	(0.00759)	(0.0211)	(0.181)	(0.340)	(0.721)
	0.0117**	0.0189**	0.0258	0.0160***	0.0154*	0.0196	0.182	0.253	-0.213
May 2020	(0.00554)	(0.00854)	(0.0271)	(0.00541)	(0.00832)	(0.0261)	(0.168)	(0.374)	(0.680)
	0.0127*	0.0159	0.00350	0.0199***	0.0126	0.00670	0.326	0.414	-0.564
June 2020	(0.00698)	(0.00985)	(0.0304)	(0.00721)	(0.00897)	(0.0286)	(0.202)	(0.408)	(0.769)
	0.00727	0.0104	0.00358	0.0233***	0.00635	0.0102	0.229	0.834**	1.111
July 2020	(0.00678)	(0.00810)	(0.0327)	(0.00665)	(0.00783)	(0.0314)	(0.246)	(0.359)	(0.706)
	0.00598	0.00450	-0.00458	0.0147*	0.00814	0.00501	0.103	1.285***	-0.347
August 2020	(0.00740)	(0.00964)	(0.0293)	(0.00748)	(0.00852)	(0.0299)	(0.233)	(0.371)	(0.619)
	0.0137**	-0.00568	0.0103	0.0124**	-0.00599	0.0125	0.289	0.610*	0.187
September 2020	(0.00573)	(0.00915)	(0.0246)	(0.00554)	(0.00906)	(0.0252)	(0.234)	(0.323)	(0.629)
	0.00501	0.00471	-0.00936	0.00859	-0.0000464	-0.00456	0.0704	0.361	0.224
October 2020	(0.00551)	(0.00911)	(0.0193)	(0.00584)	(0.00833)	(0.0173)	(0.225)	(0.310)	0.224
	0.0115**	-0.00884	-0.0229	0.0133***	-0.00223	-0.0151	0.184	0.674**	0.683
November 2020	(0.00446)	(0.00864)	(0.0243)	(0.00474)	(0.00908)	(0.0251)	(0.238)	(0.332)	(0.659)
Women	n=957,188	n=315,952	n=418,043	n=957,188	n=315,952	n=229,072	n=661,295	n=204,842	n=147,729
March 2020	-0.00480	0.0344***	-0.00350	-0.00349	0.0306**	0.00356	-0.0800	0.159	-0.255
	(0.00484)	(0.0123)	(0.0166)	(0.00402)	(0.0119)	(0.0200)	(0.171)	(0.373)	(0.865)
	0.00562	0.0375***	0.0184	0.00367	0.0390***	0.000936	0.248	0.752	0.640
April 2020	(0.00675)	(0.0115)	(0.0223)	(0.00650)	(0.0109)	(0.0245)	(0.261)	(0.497)	(0.876)
	0.00433	0.0416***	0.0258	0.00735	0.0381***	0.0320	0.689***	0.686	0.145
May 2020	(0.00763)	(0.0135)	(0.0271)	(0.00805)	(0.0137)	(0.0331)	(0.217)	(0.537)	(0.880)
	0.00537	0.0321**	0.00350	0.0129	0.0281**	0.0281	0.572**	0.350	-1.189
June 2020	(0.00955)	(0.0134)	(0.0304)	(0.0103)	(0.0139)	(0.0305)	(0.267)	(0.569)	(0.898)
	0.00659	0.0183*	0.00358	0.0250**	0.0121	0.0350	0.375	0.851*	0.376
July 2020	(0.00981)	(0.0104)	(0.0327)	(0.0107)	(0.0117)	(0.0361)	(0.282)	(0.502)	(0.871)
	0.00442	0.000660	-0.00458	0.0115	0.00788	0.0171	0.459*	1.196**	-0.841
August 2020	(0.00968)	(0.0141)	(0.0293)	(0.0103)	(0.0132)	(0.0359)	(0.273)	(0.549)	(0.836)
	0.0160*	-0.0157	0.0103	0.0135	-0.0115	0.0379	0.657**	0.728*	-0.497
September 2020	(0.00842)	(0.0138)	(0.0246)	(0.00834)	(0.0134)	(0.0311)	(0.291)	(0.413)	(0.781)
	0.00574	0.00178	-0.00936	0.00880	-0.00205	0.0124	0.274	0.310	0.822
October 2020	(0.00863)	(0.0142)	(0.0193)	(0.00901)	(0.0130)	(0.0244)	(0.269)	(0.446)	(0.896)
	0.00743	-0.00683	-0.0229	0.00845	-0.00485	-0.0243	0.600**	1.065**	1.138
November 2020	(0.00774)	(0.0153)	(0.0243)	(0.00760)	(0.0158)	(0.0304)	(0.270)	(0.423)	(0.907)
Men	n=909,231	n=259,826	n=188,971	n=909,231	n=259,826	n=188,971	n=744,315	n=231,544	n=168,698
March 2020	0.000494	0.00521	-0.00580	0.00296	0.00631	-0.0117	0.0261	-0.139	0.919
	(0.00583)	(0.0122)	(0.0262)	(0.00466)	(0.00845)	(0.0244)	(0.236)	(0.432)	(0.827)
	0.0148***	0.00121	0.0628*	0.0220***	-0.00270	0.0376	-0.154	0.562	0.224
April 2020	(0.00537)	(0.0156)	(0.0364)	(0.00506)	(0.0109)	(0.0326)	(0.261)	(0.384)	(1.264)
	0.0226***	-0.00963	0.0279	0.0279***	-0.0129	-0.000823	-0.333	-0.0969	-0.267
May 2020	(0.00614)	(0.0136)	(0.0412)	(0.00561)	(0.0106)	(0.0394)	(0.242)	(0.478)	(1.154)
	0.0250***	-0.00481	-0.0185	0.0300***	-0.00723	-0.0393	0.00975	0.448	0.575
June 2020	(0.00821)	(0.0147)	(0.0445)	(0.00752)	(0.0117)	(0.0459)	(0.240)	(0.483)	(1.162)
	0.0112	0.00127	-0.0418	0.0221***	0.000279	-0.0366	-0.0447	0.840*	2.524**
July 2020	(0.00837)	(0.0121)	(0.0430)	(0.00801)	(0.0101)	(0.0412)	(0.298)	(0.434)	(1.245)
	0.0107	0.00975	-0.00452	0.0200***	0.00913	-0.0177	-0.281	1.402***	1.135
August 2020	(0.00747)	(0.0133)	(0.0381)	(0.00689)	(0.0109)	(0.0370)	(0.283)	(0.444)	(1.288)
	0.0123*	0.00952	-0.0245	0.0121*	0.00407	-0.0368	-0.0783	0.558	2.450**
September 2020	(0.00665)	(0.0106)	(0.0354)	(0.00623)	(0.00989)	(0.0332)	(0.268)	(0.377)	(1.195)
	0.00441	0.0130	-0.0290	0.00828	0.00753	-0.0438	-0.147	0.535	-0.654
October 2020	(0.00582)	(0.0106)	(0.0395)	(0.00540)	(0.00879)	(0.0379)	(0.299)	(0.361)	(1.303)
	0.0157***	-0.00652	0.000971	0.0182***	0.00567	-0.00884	-0.219	0.512	0.550
November 2020	(0.00540)	(0.00903)	(0.0407)	(0.00464)	(0.00754)	(0.0388)	(0.295)	(0.482)	(1.271)

Notes: Notes to Table 2 apply except the sample period extends to November 2020.

Table A.5: Regression adjusted differences between treatment and control groups for telework outcome, including occupation and industry fixed effects

Dependent variable:	Teleworked		
Research Design	1	2	3
Panel A: All	n= 59,192	n=18,065	n=13,108
May 2020	0.0159*** (0.00578)	0.00777 (0.00963)	0.0208 (0.0239)
June 2020	0.0170*** (0.00414)	0.0256** (0.0102)	0.00359 (0.0220)
Panel B: Women	n=27,412	n=8,050	n=5,867
May 2020	0.0177** (0.00810)	0.00468 (0.0157)	0.0121 (0.0306)
June 2020	0.0263*** (0.00836)	0.0167 (0.0182)	-0.00328 (0.0269)
Panel C: Men	n=31,727	n=9,936	n=7,125
May 2020	0.0135* (0.00708)	0.00697 (0.0114)	-0.0125 (0.0333)
June 2020	0.00787 (0.00594)	0.0257** (0.0109)	-0.0167 (0.0276)

Notes: Notes to Table 8 apply, except industry and occupation fixed effects have been added to the models.

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