TIME TO GROW UP? ADULT CHILDREN AS DETERMINANTS OF PARENTAL LABOR SUPPLY

Breno Braga
Urban Institute, United States

Olga Malkova
University of Kentucky, United States

Abstract
As children transition to adulthood, do they remain a major determinant of parental labor supply? To answer this question, we examine how college costs affect the labor supply of mothers and fathers by exploiting the roll-out of nine generous state merit aid programs in the United States from 1993 to 2004, which made college more affordable. Mothers of college-age children decreased their annual hours of work after the introduction of these state-wide programs, while fathers did not adjust their labor supply. Mothers of college-going children were entirely responsible for the decline in hours of work, where mothers of children who did not go to college experienced no change in hours of work. The decline in labor supply was mainly due to adjustments among high-income, married, more educated, and white mothers, whose labor supply was more elastic to college costs. (JEL: I22, J13, J22)

Teaching Slides
A set of Teaching Slides to accompany this article is available online as Supplementary Data.

1. Introduction

Children are widely acknowledged as one of the most important determinants of female labor supply. The academic literature has focused on the effects of the birth

The editor in charge of this paper was Giovanni Peri.

Acknowledgments: We are grateful to David Agrawal, Martha Bailey, Sandy Baum, John Bound, Celeste Carruthers, Matthew Chingos, Raj Darolia, David Frisvold, Brad Hershbein, Steven Lugauer, Terra McKinnish, Melinda Pitts, Ken Troske, Sarah Turner, Elizabeth Handwerker, Jim Ziliak, and seminar participants at the Association for Education Finance and Policy Conference, Southern Economic Association Meetings, Washington Area Labor Economics Symposium, Population Association of America, Society of Government Economists, Society of Labor Economists, Oberlin College, University of Colorado Boulder, University of Tennessee, and the University of Kentucky for helpful comments. A previous version of this paper circulated with the title Hope for the Family: The Effects of College Costs on Maternal Labor Supply. Braga and Malkova are affiliated with IZA.

E-mail: bbraga@urban.org (Braga)

Journal of the European Economic Association 2024 22(1):230–262
https://doi.org/10.1093/jeea/jvad051

of a child and finds that women—but not men—experience substantial and persistent declines in employment after childbirth.[1] These declines may be influenced by the time requirements of young children, availability of family friendly policies, childcare provision, and gender norms and culture. However, little is known about the effect of a child's transition to adulthood on parental employment. Yet parents continue to transfer a great deal to their adult children: college expenses, co-residence, rent, and expenses related to major life events such as weddings, buying a house, and transfers that cushion against negative financial shocks are increasingly important parental transfers.[2] Once they grow up, do children *stop* being a critical determinant of labor supply for mothers, and/or do they *become* a critical determinant for fathers? The labor supply decisions of parents with adult children are fundamentally different from those with young children because parents are at a more advanced stage in their career and closer to retirement, and adult children generally require less time-intensive and mother-specific care.

In this paper, we examine how parental labor supply varies as children transition into adulthood and, eventually, out of the household. Causal inference within this framework is challenging due to difficulties in finding exogenous changes in the costs of adult children. This study takes advantage of the staggered state roll-out of nine generous merit aid programs from 1993 to 2004, which made college more affordable to a substantial number of families in the United States. Since the early 1990s, many states have established state-sponsored merit aid programs for residents who have maintained a modest grade point average in high school and enrolled in a post-secondary institution in their state of residence.

Theoretically, merit aid may substantially decrease parental labor supply due to two channels: (1) "income-effect" due to an increase in the disposable income of families, because parental income is the primary source of funding for college expenses,[3] and (2) "time-transfer" due to a change in time-transfers from parents to children. The "income-effect" channel is important because families expect to receive merit aid for many years: in every year that each child attends college and is eligible. Moreover, merit aid may lead to lower tuition because students may shift enrollment from higher-cost out-of-state colleges toward lower-cost in-state colleges (Zhang and Ness 2010). The "time-transfer" channel is important because of potential effects on the opportunity cost of parental time—merit aid may induce children to attend college closer to home, which may increase parental time-transfers to the child. On the other hand, merit aid may increase labor supply among parents of children who would not attend college without the aid, if they need to finance college expenses not covered by the aid.

To the best of our knowledge, this is the first study to causally link costs of adult children and parental labor supply by focusing on the specific example of lower college

1. See: Kleven, Landais, and Søgaard (2019), Cortés and Pan (2020).

2. See: Kaplan (2012), Andersen, Johannesen, and Sheridan (2020), and McGarry (2016). Published tuition and fees at public 4-year universities rose by about 200% from 1989 to 2019 (College Board 2018), and 36% of young adults ages 18–31 lived with their parents in 2012 (Pew Research Center 2013).

3. In 2018, parents paid on average 49% of college costs at 4-year institutions (Sallie Mae 2018). The average in-state tuition net of aid at a public 4-year institution is 26% of the income of a median household with a child in college (Radwin and Wei 2015).

costs. We exploit the variation in the years of merit aid program establishment, using a two-way fixed effects difference-in-differences (TWFE DiD) framework to causally estimate both short-run and dynamic effects on the labor supply decisions of mothers and fathers who could have an eligible child. We pool data from the Panel Study of Income Dynamics (PSID) using years 1988–2015; its unique structure allows us to construct samples of parents with college-age and college-going children. We also exploit variation in merit aid spending across states to estimate the effect of merit aid per dollar of spending on paternal and maternal labor supply, where we instrument for merit aid spending with the introduction of strong merit-aid programs.

We focus on parents with a potentially eligible child by performing the analysis separately on two samples of parents: (1) with any children ages 18–22 (we call these "college-age children") and among those (2) with college-going children. One concern of using the sample of parents with college-going children is the potential effect of merit aid on college enrollment, which may change the composition of this sample. However, we find no evidence that merit aid affected the probability that a mother in our sample has a college-going child or that merit aid affected the composition of mothers of college-going children.

The introduction of merit aid programs resulted in a decline in hours of work among mothers but no adjustment among fathers. The decline in hours of work among mothers was mostly due to adjustments at the intensive margin, while we find mixed evidence of adjustments along the extensive margin. Moreover, the decline was entirely due to adjustments among women with college-going children; we find no adjustments among women without college-going children (a placebo group). A 10% increase in spending on merit aid programs per undergraduate student is associated with a 1.3% reduction in hours of work among mothers of college-going children. These reductions are consistent with a correlational literature, suggesting a link between college expenses and parental outcomes. For instance, parents are more likely to work while their children attend college (Handwerker 2011), while increasing rates of college attendance predict increases in foreclosure rates (Faber and Rich 2018). Finally, financial aid based on assets is connected with lower savings rates.[4]

We provide many robustness checks for the validity of our TWFE DiD model. First, we show empirical and anecdotal evidence that the timing of merit aid introduction is conditionally random. Second, our results are robust to alternative estimators mitigating concerns over bias in TWFE DiD models due to differential treatment effects across time and states.[5] Third, we provide evidence of parallel pre-trends using the test developed in Borusyak, Jaravel, and Spiess (2021). Finally, we find no decline in hours of work among mothers of children who are older than college-age, who were unlikely to receive merit aid.

Do effects of merit aid programs differ across mothers? First, the decline in labor supply is almost entirely due to advantaged mothers, who are married, more educated,

4. See: Feldstein (1995), Dick and Edlin 1997, and Long (2003). Ding, Lugauer, and Bollinger (2022), link increases in both college-going and savings rates in China.

5. See: Cengiz et al. (2019), de Chaisemartin and d'Haultfœuille (2020), Borusyak, Jaravel, and Spiess (2021), Callaway and Sant'Anna (2021), and Goodman-Bacon (2021).

white, and have higher family incomes. Even though advantaged mothers may adjust their labor supply the most because their children are disproportionately eligible for merit aid (Dynarski 2004a), we find evidence that this is also due to their more elastic labor supply. Our findings are consistent with the literature, showing that married mothers are more responsive to transfers than single mothers (Eissa and Hoynes 2004; Koebel and Schirle 2016; Feinberg and Kuehn 2018; Powell 2020). Second, the decline in labor supply is larger for mothers with multiple children in college, consistent with these families receiving merit aid for multiple children.

Taking advantage of the longitudinal structure of the PSID, we investigate the dynamic effects of college costs on labor supply. We find suggestive evidence that mothers decrease their labor supply 1 to 2 years before their first child attends college, and that this decrease persists 1 to 2 years after the last child leaves college. However, these declines are substantially smaller than estimates while children are in college and are not statistically significant.

To conclude, this study demonstrates that child-related costs continue to be a determinant of maternal labor supply, even when children grow up. Thus, this study identifies a previously unexplored determinant of maternal labor supply—costs of adult children. However, adult children do not become a determinant of paternal labor supply, even though they no longer require mother-specific care. Given the sizable magnitudes of parental transfers to adult children (Haider and McGarry 2018), it is important to understand the potential effects of such transfers on parents.

This study also provides novel insights on potential effects of policies that make college more affordable to families. In response to merit aid programs, the group that reduced their labor supply primarily consisted of advantaged mothers. As tuition fees have continued to grow, and the college affordability debate has been gaining importance in political and policy discussions,[6] it is important to evaluate the effect of college costs on parental economic outcomes.

2. Background on Merit Aid Programs

Since the early 1990s, many states have established merit aid programs. The typical program, such as Georgia's HOPE Scholarship, awards grant amounts to young residents who have maintained a modest grade point average in high school. In all states, students may use merit aid grants to cover tuition and fees, while in some states merit aid also covers additional expenses, such as room and board, books, or lab equipment.[7] Some programs also have thresholds for students' SAT or ACT scores or class rank. Many require a high school grade point average (GPA) of 3.0 or above, which is not a particularly high threshold. Virtually all state merit aid programs also require students to maintain a certain GPA in college to renew the award for subsequent

6. See: Baum and Turner (2019) and Harris (2019).

7. The Georgia Hope grants can only be applied toward undergraduate tuition, while the Florida Bright Scholarship can also be used to cover additional expenses.

years, although the required GPA may differ across states.[8] There is generally no means test for eligibility, and award amounts do not differ by family income. Most merit aid programs are "last-dollar" programs, meaning that other forms of financial aid, such as Pell grants, displace the merit aid grant, and students can only receive the difference between the merit aid grant they qualify for and the grant from other financial aid. However, a few programs, such as the Louisiana TOPS Scholarship, are "first-dollar" programs, meaning that other forms of financial aid do not displace the state merit aid, for students eligible for multiple types of aid.

Among many goals, these programs aim to improve the quality of education in the state by providing an incentive for students to perform better in high school and college. They also encourage high-achieving high school students to attend college in-state. Finally, they may allow low-income, high-achieving students who cannot afford college to attend college.

Previous literature has mixed findings on the effect of merit aid programs on college enrollment and attainment. For college enrollment, previous literature finds modest positive (Dynarski 2004a; and Cornwell, Mustard, and Sridhar 2006) or no effects (Goodman 2008; Bruce and Carruthers 2014; Gurantz and Odle 2022). For college attainment, it finds positive (Dynarski 2008; and Scott-Clayton 2011), null (Sjoquist and Winters 2015b; and Fitzpatrick and Jones 2016), or even negative effects (Cohodes and Goodman 2014). Minority and low-income students are less likely to be eligible for merit aid (Dynarski 2004a). As a result, merit aid programs subsidize many students who would have gone to college anyway (Carruthers and Özek 2016; and Fitzpatrick and Jones 2016).

Finally, merit aid affects college readiness, employment, and mobility of students. First, merit aid improves college readiness (Pallais 2009). Second, merit aid decreases teenage employment (Frisvold and Pitts 2018), and it decreases military enlistment among men (Barr 2016). Third, merit aid decreases the "brain drain" resulting from the migration of talented students and workers to other states (Zhang and Ness 2010; and Sjoquist and Winters 2014). After college, merit aid recipients are more likely to own a home, and less likely to have adverse credit outcomes (Scott-Clayton and Zafar 2019).

3. Conceptual Framework

Right after its introduction, merit aid is an unexpected positive shock to the disposable income of families whose children would attend college even without merit aid, which we call the "income effect" channel. This has implications for consumption, family net worth, and labor supply of these families. First, positive income shocks lead to an increase in the consumption of normal goods. Consistent with this theory, the introduction of merit aid is associated with an increase in automobile purchases

8. The minimum GPA for renewal is typically 2.75–3.0 but is as low as 2.3 for the first year in Louisiana and 2.5 in subsequent years (Sjoquist and Winters 2015a).

(Cornwell and Mustard 2007) and alcohol consumption in college (Cowan and White 2015). Second, income shocks increase family net worth, with parents and children using the extra resources to increase savings and decrease debt. In fact, qualifying for merit aid programs lowers the student loan burden of college graduates by $5,800–$7,200 (Chapman 2015). Finally, because leisure is a normal good, income shocks resulting from merit aid may also lower parental labor supply.

Merit aid may have a substantial effect on the finances and parental employment for several reasons. First, families may have multiple children receiving this annual grant (mothers of college-going children have on average 2.8 children in our sample) and could benefit from it for several years while their children attend college. Second, given the structure of the program, students tend to shift their enrollment from higher-cost out-of-state state colleges toward lower-cost in-state colleges (Zhang and Ness 2010). As a result, families in merit aid states may save much more than the merit aid transfer on college costs, when enrolling their children in-state.

However, merit aid may have a different effect on families whose children attend college because of merit aid. Their labor supply may go up to help cover the higher financial expenses from college costs. Because we do not find evidence that merit aid changes the probability of having a child attend college (see Section 4.6), we expect merit aid to mainly affect behavior among families whose children always attend college.

What are the dynamic effects of merit aid on parental labor supply? A canonical dynamic life-cycle labor supply model predicts that families smooth consumption and labor supply decisions overtime in response to expected future positive income transfers, as long as future consumption is discounted and families have no barriers to borrow and save. Thus, parental labor supply may adjust in years before and after the children enroll in college; this response depends on expectations about the benefits of merit aid, intertemporal preferences, and the costs to borrow.

Labor supply responses to merit aid may differ depending on whether it was unexpected (in the first few years after the introduction) or anticipated (becoming the case several years after the introduction). Parents of college-age children may adjust their labor supply by more, in response to an unexpected relative to an anticipated receipt of merit aid, because they could not smooth labor supply over time. Thus, even if merit aid becomes more generous over time, labor supply responses may not increase over time, because parents have more time to smooth their labor supply, as more years pass since the start of programs.

Merit aid may also affect parental labor supply through a “time-transfer” channel if it induces children to attend college closer to home and leads to an increase in parental time-transfers to the child. Then, merit aid may change the parental opportunity cost of leisure. As time with the child increases, the net-wage (earnings—cost of time with the child) may fall.[9] Empirically, time transfers from parents to adult children are

9. Using time-use data from the 2017 PSID, Online Appendix Table A1 shows that while child care time drops as children get older, mothers of 18–22-year-old children still spend a significant amount of time on child care.

important and are more likely when parents and children live in geographic proximity (Compton and Pollak 2015). Thus, changes in proximity between parents and adult children may affect parental time use and parental labor supply.

4. Data

Our principal data source is the PSID, because it allows us to construct samples of parents with college-age (ages 18–22) and college-going children (children who are both college-age and enrolled in college) and identifies parental state of residence. Specifically, the PSID allows us to match children to their parents regardless of where the children reside. The PSID is a longitudinal survey launched in 1968 with a nationally representative sample, interviewed annually from 1968 to 1997, and every other year thereafter. We pool data from the 1988 to 2015 PSID waves for our analysis.[10] We also use parental state of residence to identify whether they lived in a state with a strong merit aid program. We use the birth years of each child from the "Childbirth and Adoption History" supplement to construct our sample of parents who have any college-age children.

We examine three main labor market outcomes for parents: annual hours of work, employment status, and annual hours of work if employed.[11] These variables are measured as of last year, so we adjust all other variables in our analysis accordingly. For instance, a parent with a child ages 18–22 in the year when employment outcomes are observed has a child ages 19–23 in the PSID survey year.[12] We construct the employment status variable using annual hours of work in the past calendar year.[13] We restrict our sample to heads of household or spouses, because we have employment data for this group.

The PSID does not provide a direct measure of college attendance that is available for everyone in our sample of years. As a result, we define college attendance by combining several variables, following an approach similar to Lovenheim (2011) and based on correspondence with experts at the PSID.[14] Because we are often unable

10. We have data for 1988–1997 in every year, and every other year for 1999–2015.

11. While the effects of merit aid on maternal earnings are qualitatively consistent with the effects on hours of work, we do not report them because they are sensitive to the exclusion of outliers in the top 1% of the earnings distribution. Although merit aid may affect consumption and wealth, the PSID does not collect data on consumption and only includes questions on savings in the wealth module in a few selected years.

12. Marital status, household headship, and state of residence are measured as of the current year, because individuals are observed every other year starting from 1998.

13. A person is employed if she works at least 52 hours a year.

14. First, we define someone as a high school graduate if they obtain at least 12 years of education. Next, we define someone as in college if they are a high school graduate and satisfy any of the following conditions: (i) answer "student" to the "employment status" question; (ii) answer "student" to the "whether a student" question; (iii) increase completed years of education across waves of the PSID; and (iv) list themselves as attending college in the Transition to Adulthood sample. See Online Appendix B for more details.

TABLE 1. Characteristics of parents of college-age children.

	(1) Parents of college-age children	(2)	(3) Mothers of college-age children	(4)
Analysis vriables	Mothers	Fathers	College-going children	Not college-going children
Annual hours of work	1,416	2,030	1,465	1,336
Employed (%)	80.8	91.6	83.6	76.0
White, non-Hispanic (%)	75.4	82.7	78.2	70.5
Age	46.9	49.0	47.3	45.7
Number of children	3.0	2.9	2.8	3.2
Some college (%)	42.2	49.8	52.8	22.6
Head of household (%)	31.9	100	24.8	45.2
Observations	13,907	9,832	7,890	5,162

Notes: Columns (1) and (2) use a sample of mothers and fathers with any children ages 18–22, which we define as "college-age children." Columns (3) and (4) use a sample of mothers of college-age children by whether ever had a college-going child. Statistics are weighted by the individual weights provided in the PSID. Source: PSID.

to measure college attendance due to missing information on the child whose parent is present in a particular year, we define a child as college-going if we observe the child enrolled in college anytime within the age range of 18–22.[15] While our "college-attendance" variable may be misclassified or missing, we only use it to separate our sample into parents with college-going children and without college-going children; our results are consistent across these samples. The analysis using parents with college-age children does not rely on the "college-attendance" variable.

Table 1 shows that mothers of college-age children (panel A) are less likely than fathers to be employed, work fewer hours, are less likely to be white, and college-educated. Mothers are on average 47 years old, and fathers are 49—therefore, still far from approaching retirement age. Parents in our sample have on average three children. Mothers of college-going children (panel B) are more likely to be employed and work more hours; be white, older, more educated, and married; and have fewer children than mothers of non-college goers.

4.1. *Strong Merit Aid Programs*

Our analysis focuses on nine generous merit aid programs (we call these "strong") where both a large share of students is eligible and a large share of tuition and fees is covered by aid, following the convention in previous literature (Sjoquist and

15. The "college-attendance" variable is likely measured with error due to data limitations. If this measurement error is independent of college attendance, our estimates suffer from attenuation bias (Bound, Brown, and Mathiowetz 2001).

TABLE 2. States with strong merit aid programs.

State	First year	Program name	Annual state merit grant aid per full-time-equivalent undergraduate student in 2012	% of 18–22 undergraduate students receiving state merit aid in 2011	Maximum merit aid as percentage of tuition and fees in public 4-year institutions in 2012
Florida	1997	Florida Bright Futures Scholarship	$482	24.1	72.0
Georgia	1993	Georgia HOPE Scholarship	$2,538	34.5	73.0
Kentucky	1999	Kentucky Educational Excellence Scholarship	$921	50.7	29.0
Louisiana	1998	Louisiana TOPS Scholarship	$1,616	26.5	112.0
Nevada	2000	Nevada Millennium Scholarship	$375	25.8	42.0
New Mexico	1997	New Mexico Lottery Success Scholarship	$1,364	29.2	80.0
South Carolina	1998	South Carolina LIFE Scholarship	$2,641	41.2	68.0
Tennessee	2004	Tennessee HOPE Scholarship	$1,814	35.0	95.0
West Virginia	2002	West Virginia PROMISE Scholarship	$753	20.6	80.0

Notes: The table presents details on strong merit aid programs in nine states that implemented them between 1993 and 2004. Arkansas started a strong merit aid program in 2010 but is not included in our analysis due to the short length of observation of the post-implementation period in our data. Sources: Sjoquist and Winters (2015a), Frisvold and Pitts (2018), NASSGAP annual reports, IPEDS, and the (NPSAS: 12).

Winters 2015a). In Table 2, we use data from several data sources to construct three state-level variables to describe the intensity of programs in strong merit aid states.[16] First, to construct the annual merit grant aid spending per full-time equivalent (FTE) undergraduate student, we use the information on non-need-based grant aid awarded to undergraduate students from the annual reports of the National Association of State Student Grant & Aid Programs (NASSGAP) and FTE undergraduate student enrollment by state from the Integrated Postsecondary Education Data System (IPEDS). Second, to construct the share of 18–22-year-old undergraduate students receiving merit aid, we use the National Postsecondary Student Aid Study (NPSAS).

16. We exclude Arkansas, which introduced a strong merit aid program in 2010, from our sample because we would need to restrict our analysis to a much shorter post-merit-aid period. We correct the start date to be 2004 in Tennessee from Sjoquist and Winters (2015a).

Finally, to construct maximum merit aid as a percentage of tuition and fees in public 4-year institutions we use data compiled by Frisvold and Pitts (2018).

Seven out of nine strong merit aid states are in the South. Spending on "strong" merit aid programs grew in their early years, but it tapered off as they became more established (Online Appendix Figure A1). The most generous programs are in Georgia and South Carolina, where states spend on average more than $2,500 per FTE undergraduate student. Kentucky has the most eligible students, where 51% of 18–22 years old undergraduates received merit aid in 2011. Finally, Louisiana's package is most generous, with the maximum aid covering more than 110% of the average tuition and fees of a public 4-year institution in the state. States without "strong" programs can also have merit aid programs, but they are not generous enough to be classified as "strong." Indeed, "strong" states spend substantially more on merit aid per full-time-equivalent (FTE) student: $1,398 in "strong" states relative to $69 in all other states.

5. Effect of Merit Aid on Parental Labor Supply

5.1. Two-Way Fixed Effects Difference-in-Differences (TWFE DiD)

To estimate the effect of strong merit aid programs, we exploit their roll-out in nine states from 1993 to 2004 within a TWFE DiD framework,

$$Y_{i,s,t} = \alpha + \gamma_t + \delta_s + \sum_{\tau=-3}^{-1} \theta_\tau D_s 1\left(EY = \tau\right) + \sum_{\tau=1}^{7} \pi_\tau D_s 1\left(EY = \tau\right) + X_{s,t} + Z_{i,t} + C_{s,t} + \varepsilon_{i,s,t} \tag{1}$$

where $Y_{i,s,t}$ is an employment outcome of parent i residing in state s in year t who has a college-age child in year t; γ_t are year fixed effects; δ_s are state fixed effects; and D_s is a dummy that equals one if a state has a strong merit aid program, and zero if a state does not (our sample includes states with and without strong merit aid programs). Because PSID was done once every two years after 1997, we construct paired event years, EY, to keep our sample of states balanced within each paired event year.[17] Thus, $1(EY = \tau)$ is a dummy that represents paired event years, which we define as paired years relative to the start of merit aid within a state. The paired event years, EY, range from -2 to 6 for a balanced set of states.[18] We group values of event years that are not based on a balanced set of states: values of $\tau < -2$ to be equal to -3 and values of $\tau > 6$ to be equal to 7. Following the literature (Bailey, Malkova, and McLaren 2019), while these unbalanced event years are included in the regression, we only present results

17. Not all states are in the PSID 2 years after merit aid started, but all states are 2 to 3 years after. An event year is the year of observation minus the year of merit aid start.

18. *EY* equaling -2 and -1 represent 5 to 6 and 3 to 4 years before merit aid started, respectively. *EY* equaling 1, 2, 3, 4, 5, and 6 represent 0 to 1, 2 to 3, 4 to 5, 6 to 7, 8 to 9, and 10 to 11 years after merit aid started, respectively.

for a balanced set of paired event years. We expect that merit aid programs may affect employment in the year of the program start, because parents may adjust their labor supply as soon as they find out that their child is eligible for merit aid. Thus, we omit the dummy for 1–2 years before merit aid started ($EY = 0$).

In addition, the equation includes individual-level covariates, $Z_{i,t}$: race, years of education, age, and marital status fixed effects, number of children, and household headship. It also includes state educational controls by year, $C_{s,t}$: need-based aid spending to undergraduate students per FTE undergraduate student from NASSGAP and IPEDS, and average tuition and fees for FTE undergraduate students separately in public 4-year and 2-year degree institutions, also from IPEDS. These covariates control for other state-level higher education policy changes that might occur during the introduction of a merit aid program. The equation also includes state-level economic covariates from the University of Kentucky Center for Poverty Research, $X_{s,t}$: the unemployment rate, log state government revenue, minimum wage, whether the governor is a democrat, the poverty rate, number of AFDC/TANF recipients, and number of food stamp/SNAP recipients. These covariates control for changes in the economy and political environment that could be correlated to the introduction of merit aid. We cluster standard errors at the state level to account for potential spatial correlation within a state and weight using the individual longitudinal weights.[19]

Our coefficient of interest (π_τ) measures the effect of merit aid on parental outcomes τ paired years after the start of merit aid. Because we do not restrict the sample to families receiving merit aid, the coefficient should be interpreted as an intent to treat effect. Finally, we summarize our results in a difference-in-differences specification. The paired event year dummies, $1(EY = \tau)$, are replaced with dummies for paired event years -3 and below, 1–6, and 7 and above. The coefficient on paired event years 1–6 measures the average effect over 11 years after merit aid program implementation.

5.2. Testing the Internal Validity of the Empirical Framework

Institutional evidence supports the assumption that the year of merit aid start is conditionally random (Athey and Imbens 2022). The literature suggests that states were mostly experimenting with a new higher education policy rather than responding to economic or educational shocks (Dynarski 2004a). Governor Zell Miller introduced the Georgia HOPE program as a visionary act to promote higher education, which set an example that many other states have followed. Merit aid programs were unexpected, because most programs were created with an unanticipated source of revenue, such as state lotteries (Georgia and Kentucky) or tobacco lawsuit (Nevada) settlements and were implemented shortly after their announcement.[20] Lottery sales and tobacco

19. We include all states for which we have data in the PSID, resulting in 49 clusters.

20. See Heller and Marin (2004). In 1992 Georgia voters approved a ballot measure authorizing a lottery to fund the merit aid program. In June 1993 the first lottery ticket was sold, and in the fall 1993

settlements are less likely to be affected by economic and political changes within a state.

Empirical evidence also supports this assumption. First, Online Appendix Table A2 shows small and statistically insignificant coefficients on the relationship between the year of merit aid start and employment outcomes of mothers ages 35–64 before the start of merit aid programs. Second, the inclusion of educational, economic, and political controls does not affect our main results, suggesting robustness to omitted variable bias (Altonji et al. 2019).

Finally, we show evidence of parallel pre-trends of parental employment outcomes before merit aid start. First, pre-trend coefficients (θ) are on a flat trend and not statistically different from zero in our event study figures. Second, a test that separates testing of parallel trends from estimation (Borusyak, Jaravel, and Spiess 2021) provides further evidence of parallel pre-trends. This test mitigates concerns that pre-trend coefficients (θ) are correlated with the estimates of treatment effects obtained from the same specification (Roth 2022).

5.3. Results: Hours of Work of Parents of College-Age Children

Figure 1 displays event-study estimates of the effect of merit aid on hours of work of mothers and fathers of college-age children. This figure includes individuals who are employed and unemployed, where annual hours of work equal zero for those who did not work. It presents estimates of our preferred model that includes the full set of covariates in equation (1). Estimates to the left of the vertical axis present paired years before merit aid started and estimates to the right present paired years after merit aid started, where each paired event year actually includes two years. Thus, the graph extends to 6 years before and 11 years after merit aid started. Dashed lines plot 95%, point-wise confidence intervals. Online Appendix Tables A3 and A4 (column (4)) present the point estimates for mothers and fathers respectively, while Table 3 summarizes the estimates in Figure 1 for mothers in a difference-in-differences specification and presents the overall effect over 11 years after implementation.

We show evidence in support of parallel pre-trends. First, the coefficients (θ) on negative paired event years are small and statistically insignificant. Second, the p-value is 0.59 of the test whose null is that the pre-trend coefficients are jointly zero (Borusyak, Jaravel, and Spiess 2021).

Figure 1 (panel (a)) shows that mothers of college-age children work significantly fewer hours after the start of merit aid. This effect appears in a notable drop in coefficients, evidenced by negative and statistically significant coefficients on positive paired event years. Table 3 (panel A) shows that annual hours of work dropped by 194 hours, representing a 12.4% decline relative to the pre-treatment mean. While there is suggestive evidence that the effect of merit aid becomes stronger overtime, we

semester, HOPE awarded its first scholarship. Nevada's program, funded by Nevada's settlement with tobacco companies, was first announced by Governor Guinn in 1999 and passed by the legislature that year and began distributing funds in the fall of 2000 (McDonough, Calderone, and Purdy 2007).

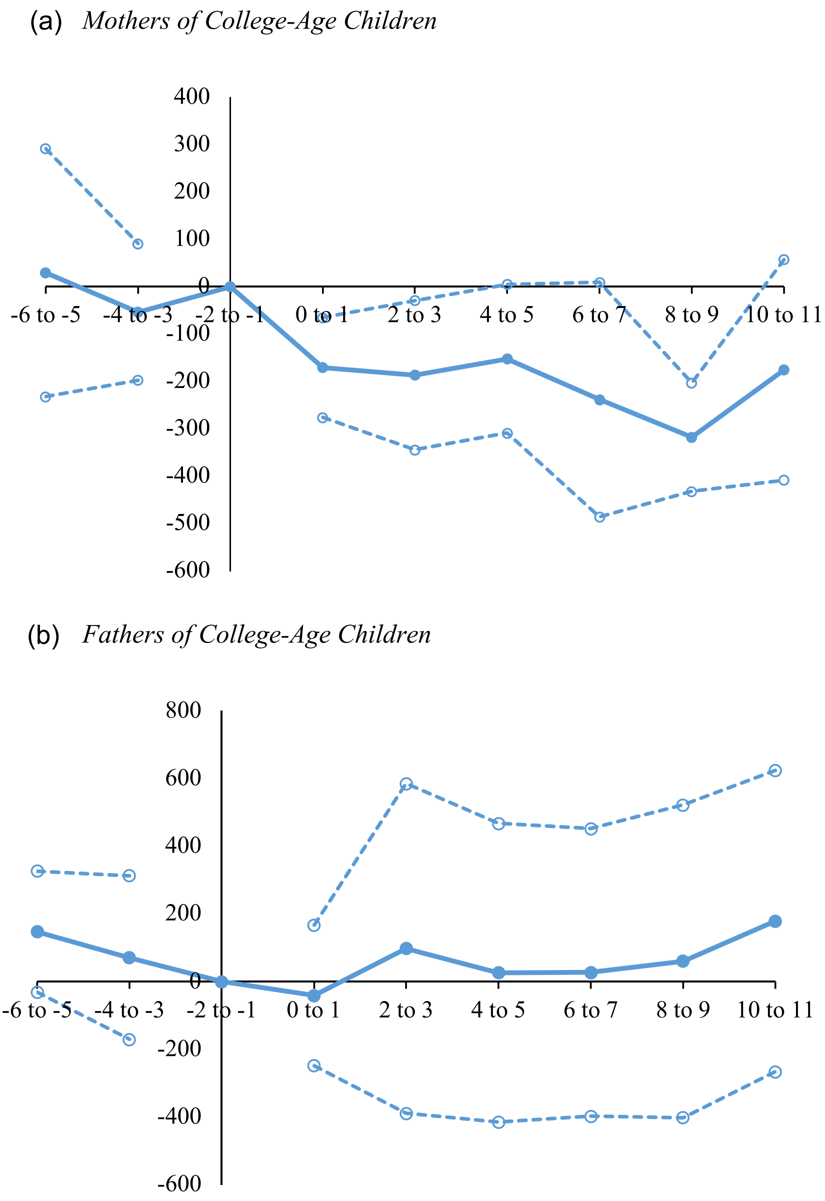


FIGURE 1. Effect of merit aid on parental annual hours of work. The graphs present the evolution of annual hours of work before and after merit aid programs for mothers (panel (a)) and fathers (panel (b)) with college-age children using equation (1). The *x*-axis represents paired event years—years since merit aid introduction—that include two event years. Event year 0 corresponds to the year a merit aid program is introduced, and we expect that parents will become treated that year, because that is when they find out that their child is eligible to receive merit aid. The estimates are from model (4), which is our baseline specification that includes all covariates. Regressions are weighted using longitudinal weights. Standard errors are clustered at the state-level and used to construct 95%, point-wise confidence intervals (dashed lines). Sources: PSID, NASSGAP, IPEDS, University of Kentucky Poverty Center.

TABLE 3. Effect of merit aid on employment outcomes of mothers with a college-age child.

	(1)	(2)	(3)	(4)
A. Dependent variable: annual hours of work				
After merit aid	−160.6	−188.2	−186.2	−194.4
	[70.07]**	[76.06]**	[78.75]**	[83.66]**
Observations	13,907	13,907	13,907	13,907
Pre-treatment mean	1,567	1,567	1,567	1,567
B. Dependent variable: employment status (%)				
After merit aid	−3.125	−3.771	−3.323	−3.483
	[2.391]	[2.451]	[2.465]	[2.503]
Observations	13,907	13,907	13,907	13,907
Pre-treatment mean	81.49	81.49	81.49	81.49
C. Dependent variable: annual hours of work if employed				
After merit aid	−128.1	−159.1	−166.2	−174.8
	[68.60]*	[72.19]**	[72.36]**	[80.32]**
Observations	10,842	10,842	10,842	10,842
Pre-treatment mean	1,923	1,923	1,923	1,923
Covariates	FE: State, Year	FE: State, Year; Xind	FE: State, Year; Xind; Xeduc	FE: State, Year; Xind; Xeduc; Xmacro

Notes: The coefficients come from a difference-in-differences version of equation (1) and measure the effect of strong merit aid programs over 11 years after their start. "After merit aid" is the coefficient on the dummy for paired event years 1–6 corresponding to a balanced set of states. The omitted event years are −1 and −2. The sample includes women with a college-age child; the sample includes all states for which data are available in the PSID (49 states). Regressions are weighted using individual longitudinal weights. Standard errors are clustered at the state-level and presented in brackets under the coefficients. Column (1) includes state and year fixed effects; column (2) adds race, education, age, and marital status dummies, number of children, and household headship; column (3) adds annual state-level education controls: need-based aid spending per FTE student, and average tuition and fees for FTE undergraduates separately in 4-year and 2-year institutions; column (4) includes annual state-level macroeconomic controls: the unemployment rate, log revenue, minimum wage, whether the governor is a democrat, the poverty rate, number of AFDC/TANF, and number of food stamp/SNAP recipients. The pre-treatment mean is calculated in strong merit aid states in event years −1 and −2. Statistically significant at ***0.01, **0.05, and *0.10. Sources: PSID, NASSGAP, IPEDS, University of Kentucky Poverty Center.

cannot reject the null hypothesis that effects are equal for different time periods after implementation in Online Appendix Table A5. Our estimated effects are not sensitive to the exclusion of any one state; Online Appendix Figure A2 shows that estimates are of similar magnitudes and with overlapping confidence intervals when we drop one strong state at a time from the analysis.

Table 3 and Online Appendix Table A3 show the robustness of these estimates to the addition of different control variables. Column (1) includes state and year fixed effects, column (2) adds individual-level covariates, column (3) adds educational covariates at the state and year level, and column (4) adds macroeconomic covariates at the state and year level. The coefficients across specifications are similar; thus, all our results are robust to the inclusion of covariates.[21]

There is no evidence that fathers have changed their work hours after merit aid. Panel (b) of Figure 1 shows small and statistically insignificant coefficients before and after merit aid started.[22] Given the null effect on fathers, the rest of the paper focuses on mothers.

5.4. Robustness of TWFE DiD: Alternative Estimators

The TWFE DiD estimator is potentially biased when the treatment effect varies over time or across states due to comparisons between states that implemented merit aid earlier versus later. To test for this potential bias, we perform the decomposition from Goodman-Bacon (2021), which shows how the overall point estimate is the weighted sum of timing effects and never versus treated groups.[23] Online Appendix Table A8 shows that our TWFE DiD estimates stem almost exclusively from comparisons of states with strong programs to states that never implemented a strong program (treated vs. never treated). Even with treatment effects that vary across time and state, the TWFE DiD estimator performs well when "never treated" units are used as a comparison group to "eventually treated" units (Callaway and Sant'Anna 2021). Thus, our TWFE DiD estimates are unlikely affected by issues raised in Goodman-Bacon (2021).

Nonetheless, we use four alternative estimators that accommodate the possibility of differential treatment effects across time and state, and we avoid the problematic comparisons of the TWFE DiD estimator. Even though the literature has not settled on the best estimator yet, Baker, Larcker, and Wang (2022) show that each alternative estimator recovers the true treatment effect.

Our TWFE DiD estimates are robust to all four alternative estimators. Figure 2 shows the effect of merit aid on hours of work for all mothers of college-age children using the TWFE DiD estimator (equation 1), and the alternative estimators: stacked (Cengiz et al. 2019), Borusyak, Jaravel, and Spiess (2021), Callaway and Sant'Anna (2021), and de Chaisemartin and d'Haultfoeuille (2020). The coefficients across all estimators are on similar trends and are of similar magnitudes. Online Appendix Table A9 shows a drop in hours immediately after merit aid start. Online Appendix Table A10 presents the effect of merit aid 1 to 11

21. Tables A6 and A7 show robustness across specifications for other employment outcomes in this sample.

22. The p-value is 0.419 of the test whose null is that the pre-trend coefficients are jointly zero (Borusyak, Jaravel, and Spiess 2021).

23. The TWFE estimator is a weighted average of all possible two-group and two-period difference in differences estimators. To perform the decomposition, we aggregate data to the state-level, use balanced panels, and exclude covariates.

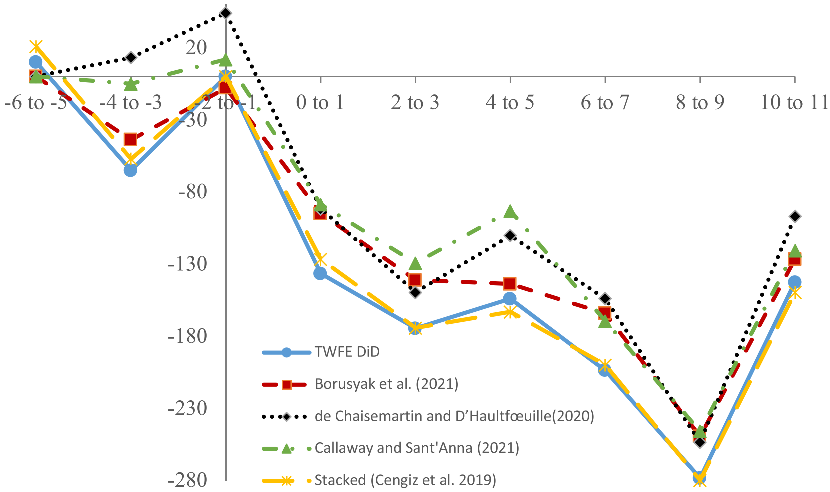


FIGURE 2. Robustness of TWFE DiD: effect of merit aid on annual hours of work of mothers with college-age children. The solid line with circles is the estimate from the TWFE DiD model presented in Figure 1 (panel (a)). The dashed line with crosses is the stacked model (Cengiz et al. 2019). We create a dataset for each state with a strong program and all other states that did not and stack these nine datasets in relative time to calculate an average effect across all nine events using a single set of treatment indicators. We use equation (1) but interact state and year fixed effects with indicators for the specific stacked dataset. The dashed line with squares is the efficient imputation estimator developed in Borusyak, Jaravel, and Spiess (2021). The dashed line with triangles is the two-step estimator developed in Callaway and Sant'Anna (2021). The dotted line with diamonds is the instantaneous treatment effect estimator developed in de Chaisemartin and D'Haultfoeuille (2020). All estimates are from model 1 without covariates. In the Callaway and Sant'Anna (2021) method, we aggregate the individual-level data to be at state-level. The omitted pre-period is event years −6 to −5 in Borusyak, Jaravel, and Spiess (2021), Callaway and Sant'Anna (2021), and de Chaisemartin and d'Haultfœuille (2020), and is event years −2 to −1 in TWFE DiD and the stacked model. Sources: PSID, NASSGAP, IPEDS, University of Kentucky Poverty Center.

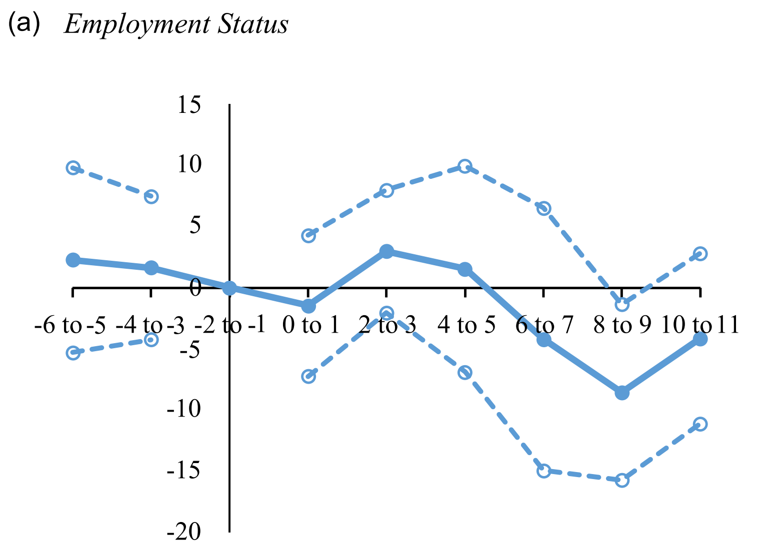
years after its introduction, where treatment effects are remarkably similar across all estimators.[24]

Based on the results of the decomposition from Goodman-Bacon (2021), which are confirmed by the similarity of coefficients across all alternative robust estimators, we continue to use the TWFE DiD estimator for the rest of the paper.

5.5. *Results: Intensive and Extensive Margin Effects*

Next, we examine whether the maternal decrease in hours of work was a result of exit from the labor market (extensive margin adjustment) or a decline in hours of work among mothers who were employed (intensive margin). We find mixed evidence of a change in employment status. The coefficient in Table 3 (panel B, column (4)), even

24. We cannot provide this estimate using the method from de Chaisemartin and d' Haultfœuille (2020), because it can only estimate instantaneous treatment effects.



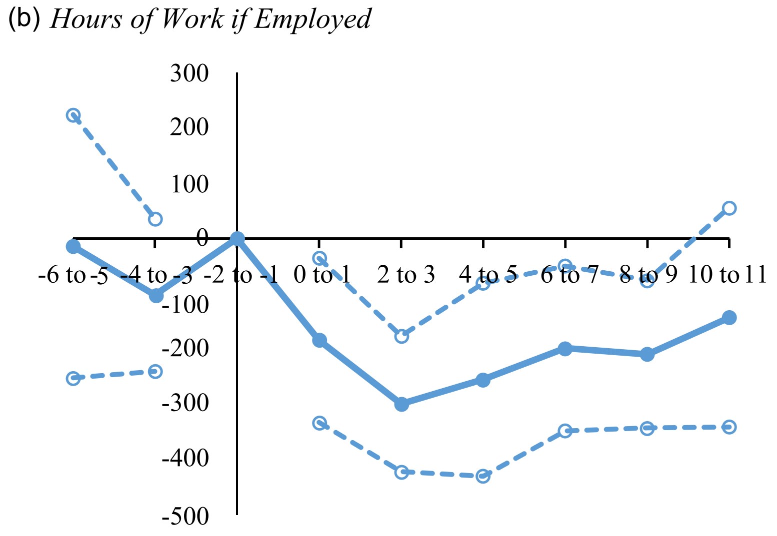


FIGURE 3. Effect of merit aid on employment outcomes of mothers of college-age children. The estimates are from equation (1) using the TWFE model. The x-axis represents paired event years—years since merit aid introduction—that include two event years. Event year 0 corresponds to the year a merit aid program is introduced. Regressions are weighted using longitudinal weights. Standard errors are clustered at the state-level and used to construct 95%, point-wise confidence intervals (dashed lines). Sources: PSID, NASSGAP, IPEDS, University of Kentucky Poverty Center.

though negative, is not statistically significant. In Figure 3, the coefficient is negative and statistically significant for mothers of college-age children 9 to 10 years after merit aid started, which may mean that mothers started also adjusting their employment status after some time passed from merit aid program start. However, the overall pattern of coefficients, their small magnitudes, and their large standard errors mostly point to a lack of convincing evidence for a significant effect on employment status.

The decrease in hours was mostly a result of adjustment on the intensive margin. Figure 3, panel (b), shows a drop in hours of work among employed mothers after merit aid started.[25] Table 3 (panel C) shows that employed mothers of college-age children dropped their hours of work by 174.8 hours, representing a 9.1% decline relative to the pre-treatment mean.

5.6. Results: Labor Supply by College-Going Status of Children

Because only children of parents of college-going children are eligible for merit aid, we test whether the decline in hours of work is concentrated among mothers of college-going children. We do not expect responses among mothers without college-going children, our placebo group.

One potential concern is that merit aid also affects the college-going decision, resulting in mechanical changes in labor supply due to the change in the composition of parents of college-going children. To evaluate this issue, we first test in our sample whether the likelihood that a mother has a college-going child changes after the introduction of merit aid (Online Appendix Table A11, Panel A). Second, we investigate whether parents of college-age and college-going children are systematically different in terms of their socioeconomic characteristics after merit aid (Online Appendix Table A11, Panel B). We do not find evidence that mothers are more likely to have a child enrolled in college after the introduction of merit aid in our sample. We also do not find evidence that mothers of college-age and college-going children are different in terms of their years of education, race, number of children, and age after the start of merit aid. Given that the effect of these programs on college attendance and parent composition is undetectable in our sample, any bias from the endogeneity of college attendance is likely small.

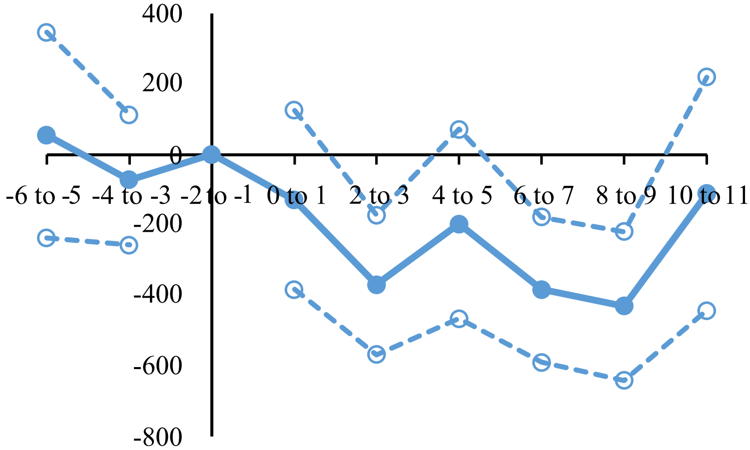
As expected, the decline in hours of work was due to adjustments among mothers who have college-going children. Panel (a) of Figure 4 shows a notable decline in hours of work after merit aid programs started among mothers of college-going children.[26] Table 4 (panel A) shows that annual hours of work dropped by 269.3 hours over 11 years after the start of merit aid programs among mothers of college-going children.[27] Panel B shows no evidence of a change in hours of work among mothers who didn't have a child attending college (our placebo group), providing further evidence of the validity of our estimation strategy. Moreover, Online Appendix Table A13 shows that these results are robust to alternative estimators that take into account heterogeneous treatment effects.

25. We cannot reject the null that there are no pre-trend differences (Borusyak, Jaravel, and Spiess 2021), where the p-value equals 0.57.

26. The p-value is 0.34 for the null that the pre-trend coefficients are jointly equal to zero (Borusyak, Jaravel, and Spiess 2021). Online Appendix Table A12 includes the estimates from Figure 4.

27. Similar to mothers of college-age children, we mostly find that the decline in hours of work happened at the intensive margin for mothers of college-going children (Table 4).

(a) *College-Going Child*



(b) *Not College-Going Child*

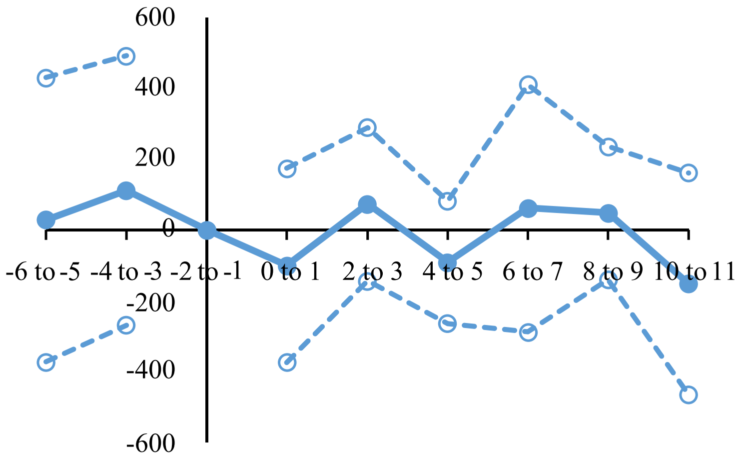


FIGURE 4. Effect of merit aid on maternal annual hours of work by child college-going status. The graphs present estimates using equation (1) for women with children ages 18–22 who have a college-going child (panel (a)), and do not have a college-going child (panel (b)). The *x*-axis represents paired event years—years since merit aid introduction—that include two event years. Regressions are weighted using longitudinal weights. Standard errors are clustered at the state-level and used to construct 95%, point-wise confidence intervals (dashed lines). Sources: PSID, NASSGAP, IPEDS, University of Kentucky Poverty Center.

TABLE 4. Effect of merit aid on employment outcomes of mothers with a college-going child.

	(1)	(2)	(3)	(4)
A. Dependent variable: annual hours of work				
After merit aid	−221.2	−244.0	−248.5	−269.3
	[90.60]**	[90.81]***	[91.83]***	[85.80]***
Observations	7,890	7,890	7,890	7,890
Pre-treatment mean	1,659	1,659	1,659	1,659
B. Dependent variable: employment status (%)				
After merit aid	−5.758	−6.024	−5.597	−5.408
	[4.179]	[3.655]	[3.505]	[3.279]
Observations	7,890	7,890	7,890	7,890
Pre-treatment mean	84.94	84.94	84.94	84.94
C. Dependent variable: annual hours of work if employed				
After merit aid	−134.1	−172.5	−184.4	−210.9
	[92.25]	[94.16]*	[93.70]*	[99.06]**
Observations	6,465	6,465	6,465	6,465
Pre-treatment mean	1,953	1,953	1,953	1,953
Covariates	FE: State, Year	FE: State, Year; Xind	FE: State, Year; Xind; Xeduc	FE: State, Year; Xind; Xeduc; Xmacro

Notes: The coefficients come from a difference-in-differences version of equation (1) and measure the effect of merit aid programs over 11 years after their start. The sample includes women with a college-going child. See Table 3 notes. Statistically significant at ***0.01, **0.05, and *0.10. Sources: PSID, NASSGAP, IPEDS, University of Kentucky Poverty Center.

5.7. Dose-Response Framework

Previously, we estimated intent-to-treat parameters, because we do not observe in the PSID whether children receive merit aid. This section estimates a dose-response model that aims to recover treatment on the treated (TOT) parameters. First, we estimate the likelihood of student merit aid receipt as a function of race, maternal education, and parental marital status in strong merit aid states using data from the NPSAS.[28] Online Appendix Table A14 shows that white students with highly educated and married mothers are more likely to receive merit aid.

Second, using the results from Online Appendix Table A14, we predict for each mother with a college-going child in the PSID a dose of treatment ($M_{i,t}$)—the probability of child merit aid receipt in a strong merit aid state. This dose varies by race, education, and marital status; it is constant within strong merit aid states and over time and equals zero in all other states.[29]

28. The question on state merit aid receipt is only available in a few NPSAS years. We choose the 2012 NPSAS because it is the most recent available year within our period of analysis. Access to the NPSAS microdata is restricted by the National Center for Education Statistics and we use PowerStats remote software for our estimates.

29. Because of sample size restrictions, NPSAS does not allow the estimation of the dose of treatment separately for each of the nine strong merit aid states.

TABLE 5. Effect of dosage of merit aid on annual hours of work of mothers with a college-going child.

	(1)	(2)	(3)	(4)
Dependent variable: annual hours of work				
Dosage of Merit Aid	−318.2	−444.8	−444.2	−473.0
	[263.9]	[252.2]*	[252.5]*	[240.9]*
Observations	7,890	7,890	7,890	7,890
Pre-treatment mean	1,659	1,659	1,659	1,659
Covariates	FE: State, Year	FE: State, Year; Xind	FE: State, Year; Xind; Xeduc	FE: State, Year; Xind; Xeduc; Xmacro

Notes: These coefficients present estimates from the dose-response regression in equation (2). Dosage of merit aid is the estimated probability of having a child receive merit aid in a strong merit aid state based on maternal race, education, marital status, and state of residence. The coefficients present the change in maternal annual hours of work after the increase in the likelihood of a child receiving merit aid from 0 to 100%. Regressions are weighted using individual longitudinal weights. Standard errors are clustered at the state-level and presented in brackets under the coefficients. See Table 3 for covariates definitions. Statistically significant at ***0.01, **0.05, and *0.10. Sources: PSID, NASSGAP, IPEDS, University of Kentucky Poverty Center, National Postsecondary Student Aid Study: 2012 Undergraduates (NPSAS: UG).

Finally, we estimate a dose-response model, where in the summarized version of equation (1) we multiply the post merit aid dummy with our predicted dose of treatment, in the following specification:

$$Y_{i,s,t} = \alpha + \gamma_t + \delta_s + \pi 1\,(1 \leq EY \leq 6)\, D_s M_{i,s} + X_{s,t} + Z_{i,t} + C_{s,t} + \varepsilon_{i,s,t} \tag{2}$$

where $Y_{i,s,\,t}$ is annual hours of work of mother i residing in state s in year t who has a college-going child in year t; D_s is a dummy that equals one if a state has a strong merit aid program, $1(1 \leq EY \leq 6)$ is a dummy for the period of 11 years after merit aid programs have been in place; and $M_{i,s}$ is the predicted dose of treatment representing the probability of merit aid receipt. Other controls are the same as in equation (1). The coefficient of interest is π providing an estimate of the effect of increasing the probability of merit aid receipt from 0 to 100% on hours of work and should be interpreted as a TOT parameter.

An increase in the dose of treatment is associated with a decline in annual hours of work of mothers of college-going children. Table 5 shows that an increase from 0 to 100% in the probability of merit aid receipt in strong states results in a decline of 473 hours of work. Thus, merit aid results in a 29% decline in maternal hours of work relative to the pre-treatment mean.

5.8. Placebo: Mothers with Youngest Child Older than College Age

We also analyze the effect of merit aid on mothers whose children went to college but whose youngest child is older than college age (ages 26 to 33 in our case). Because their children are unlikely to receive merit aid, analysis of this group of mothers serves as a placebo test.[30] Online Appendix Table A15 shows no evidence of an effect on hours of work among mothers whose youngest children are older than college-age. This result bolsters the internal validity of our estimates and the interpretation that the decline in labor supply was due to merit aid.

6. Effects per Dollar of Merit Aid Spending

In the previous section, we estimated the effect of the introduction of a strong state merit aid program on maternal labor supply. In this section, we estimate the effect per dollar of merit aid spending, by using annual data on merit aid spending per FTE undergraduate student in each state as a measure of generosity of merit aid programs. This approach has several advantages. First, it explores differences in generosity of programs across states with strong merit aid programs (Table 2). For example, in 2012, Georgia spent almost seven times more than Nevada in their program per undergraduate student. Second, it allows us to estimate the elasticity of the labor supply response relative to dollar spending that is more easily comparable to potential estimates in other studies. Finally, it allows us to test the robustness of our TWFE DiD estimate using a different estimation strategy.[31]

Relying on the same identification assumptions from our TWFE DiD framework, we use the introduction of merit aid in each strong state as an instrument for merit aid spending. The first-stage regression in this two-stage least squares (2SLS) estimator is a difference-in-differences regression model,

$$Merit_{i,s,t} = \alpha_1 + \alpha_2 * D_s * 1\,(EY \geq 1) + \gamma_t + \delta_s + X_{s,t} + Z_{i,t} + C_{s,t} + \varepsilon_{i,s,t}, \tag{3}$$

where $Merit_{i,s,\,t}$ is merit aid spending per FTE undergraduate student in individual i's state of residence s and year t, $D_s{*}1(EY \geq 1)$, is the instrumental variable equal to 1 in strong states in years after merit aid start, and 0 in strong states in years before merit aid start and in states without strong merit aid programs. Other controls are the same as in equation (1). The coefficient of interest is α_2, providing an estimate of the effect of the introduction of merit aid on spending in the years after the introduction.

The second stage uses the predicted merit aid spending from (3),

$$Y_{i,s,t} = \alpha + \beta \widehat{Merit}_{i,s,t} + \gamma_t + \delta_s + X_{s,t} + Z_{i,t} + C_{s,t} + \varepsilon_{i,s,t}, \tag{4}$$

where the coefficient of interest, β, shows the effect on labor supply ($Y_{i,s,\,t}$) per dollar of merit aid spending per undergraduate student. In all regressions, we report the Olea and Pflueger (2013) F-test for weak instruments.

First, we present evidence for the first-stage relationship between merit aid start and spending. Online Appendix Table A16 presents estimates of α_2 in equation (3). In

30. To ensure that our placebo group is not affected by merit aid, we present effects averaged over 4 years (instead of 12) after merit aid. Our placebo group could have children receive merit aid in the past, the more years have passed since merit aid start.

31. This is consistent with Isen, Rossin-Slater, and Walker (2017), who use both a difference-in-differences and an instrumental variables strategy, where they instrument for changes in air pollution using the introduction of the Clean Air Act of 1970.

TABLE 6. Effect of merit spending per FTE undergraduate student on employment outcomes of mothers of college-going children.

	(1)	(2)	(3)	(4)
A. Dependent variable: annual hours of work				
Merit spending per FTE student	−0.161	−0.190	−0.202	−0.199
	[0.0797]**	[0.0848]**	[0.0881]**	[0.0863]**
Observations	7,890	7,890	7,890	7,890
First-stage F-statistic	12.92	13.62	16.34	16.07
B. Dependent variable: employment status				
Merit spending per FTE student	−0.00424	−0.00506	−0.00491	−0.00421
	[0.00425]	[0.00340]	[0.00328]	[0.00330]
Observations	7,890	7,890	7,890	7,890
First-stage F-statistic	12.92	13.62	16.34	16.07
C. Dependent variable: annual hours of work if employed				
Merit spending per FTE student	−0.098	−0.13	−0.144	−0.155
	[0.0900]	[0.0990]	[0.101]	[0.104]
Observations	6,465	6,465	6,465	6,465
First-stage F-statistic	12.46	13.03	15.24	15.06
Covariates	FE: State, Year	FE: State, Year; Xind	FE: State, Year; Xind; Xeduc	FE: State, Year; Xind; Xeduc; Xmacro

Notes: The estimates present the effect of merit aid spending per full-time-equivalent undergraduate student (in 2015 dollars) for mothers of college-going children using equation (4). We use an indicator for the period after merit aid in strong states as an instrumental variable. Olea and Pflueger F-statistics are reported as a test of the first-stage strength of the instrument. Regressions are weighted using longitudinal weights. Standard errors are clustered at the state level and presented in brackets under the coefficients. See Table 3 for covariates definitions. Statistically significant at ***0.01, **0.05, and *0.10. Sources: PSID, NASSGAP, IPEDS, University of Kentucky Poverty Center.

the sample of mothers with a college-going child, merit aid start corresponded to an average \$1,125 increase in merit aid spending per FTE undergraduate student.

Once we incorporate differences in program size, we still find that increasing merit aid is associated with decreases in labor supply of mothers of college-going children.[32] Table 6 shows that a \$1 increase in spending per undergraduate student is associated with a reduction of 0.199 hours of work among mothers of college-going children. Thus, a 10% increase in spending (\$112.5 = 0.1∗\$1,125) is associated with a 1.35% decline in hours of work (−0.199∗112.5/1,659). However, Online Appendix Table A18 (panel B) shows no evidence of effects of merit aid on hours of work among mothers who never had a college-going child—our placebo test, which is consistent with findings using equation (1) in Online Appendix Table A18 (panel A) and Table A19.

32. Online Appendix Table A17 shows similar results among mothers of college-age children. Online Appendix D shows that these results are robust to using another measure of program generosity.

In sum, we find similar results using the TWFE DiD strategy (equation 1) and the 2SLS strategy (equation 4). For convenience of interpretation, the next sections present effects per dollar of merit aid spending.

7. Heterogeneous Effects of Merit Aid Programs

Did advantaged or disadvantaged mothers of college-going children adjust their labor supply the most? On one hand, advantaged mothers may adjust their labor supply the most because their children are disproportionately more eligible for merit aid (Dynarski 2004a). This is important because the analysis focuses on women with college-going children and not on women whose children receive merit aid. Also, advantaged mothers may have a more elastic labor supply, because of fewer credit constraints, while less advantaged women may experience changes in family net worth. On the other hand, the opportunity cost of leisure among less advantaged mothers is lower due to their lower wages, which may make their labor supply more elastic.

Hours of work mainly decline among advantaged mothers.[33] First, the decline in hours is concentrated among married mothers.[34] A 10% increase in merit aid spending is associated with a 1.6% (−0.262∗0.1∗1,125/1,822) decrease in hours of work among married mothers of college-going children (Table 7, column (2)). We find no evidence of adjustments among unmarried mothers where the coefficient is small and statistically insignificant. Second, the decline in hours is concentrated among more educated mothers. A 10% increase in merit aid spending is associated with a 1.7% (−0.280∗0.1∗1,125/1897) decline in hours of work among mothers of college-going children (Table 7, column (4)). We find no evidence of adjustments among less educated mothers.

Third, the decline in hours of work is concentrated among white mothers. A 10% increase in merit aid spending is associated with a 1.4% (−0.239*0.1*1,125/1,873) decline in hours among white mothers (Table 7, column (6)). We find no evidence of adjustments among non-white mothers. Fourth, the decline in hours is concentrated among mothers in the top tercile of family income.[35] To avoid selection of an endogenous sample, we measure parental income when the oldest child is 18 years old and compare his or her parental income to the income distribution of all families with children who are ages 15 to 20 in the PSID in the same year. Most of the effect of merit aid on hours of work is concentrated among mothers in the top tercile (67 to

33. This finding also holds for mothers of college-age children (Online Appendix Table A20). There is no evidence of a change in the employment status among advantaged and disadvantaged mothers of college-age (Online Appendix Table A21) and college-going (Online Appendix Table A22) children.

34. We compare heads of households to spouses. If a husband is present in the household, the PSID lists him as the head of household and his wife/partner as the spouse.

35. Family income includes the sum of both paternal and maternal income for married couples, and only maternal income for single-headed households. We use the after-tax-and-transfer family income calculated using TAXSIM.

TABLE 7. Heterogeneous effects of merit aid among mothers of college-going children.

	(1) Head of household	(2) Not head of household	(3) High school or less	(4) Some college	(5) Non-white	(6) White
Merit per FTE student	−0.0201	−0.262	−0.0300	−0.280	−0.0613	−0.239
	[0.110]	[0.123]**	[0.139]	[0.171]	[0.0939]	[0.118]**
Observations	2,823	5,067	4,009	3,631	3,830	4,060
Pre-treatment mean	1,199	1,822	1,283	1,897	957.9	1,873
First-stage F-statistic	21.88	15.22	34.57	11.03	27.49	14.38

	(7)	(8)	(9)	(10)	(11)
	Family income (percentiles) when oldest child is age 18			Number of college-going children	
	0–33	34–66	67–100	1 child	2+ children
Merit per FTE student	−0.0872	0.0335	−0.336	−0.101	−0.561
	[0.109]	[0.158]	[0.141]**	[0.0612]*	[0.229]**
Observations	1,902	1,977	2,079	6,171	1,719
Pre-treatment mean	1,151	1,281	2,162	1,621	1,949
First-stage F-statistic	96.3	11.39	16.87	17.17	11.04

Notes: The estimates present the effect of merit aid spending per FTE undergraduate student on annual hours of work among mothers of college-going children using an indicator for the period after merit aid introduction in strong states as an instrumental variable (see equation 4). Each column represents a different sample of mothers. We use after tax and transfer family income, so we subtract state and federal income taxes from family income (columns (7)–(9)). Family income is summed over the heads and spouses and includes income from several sources: wages and salary, non-business and business labor, pension, property, social security, and unemployment. Olea and Pflueger F-statistics are reported as a test of the first-stage strength of the instrument. Statistically significant at ***0.01, **0.05, and *0.10. Sources: PSID, NASSGAP, IPEDS, University of Kentucky Poverty Center.

100) of the family income distribution, with small and insignificant effects for mothers in the bottom two terciles (Table 7, columns (7) to (9)).[36]

We provide evidence that the labor supply of more advantaged mothers is more elastic to merit aid receipt. First, a substantial share of disadvantaged mothers are also eligible: 27.6% of lower educated mothers are eligible, while 34.6% of higher educated mothers are eligible (Online Appendix Table A23). We have performed a dose-response analysis separately for advantaged and disadvantaged mothers that takes into account the differential probabilities of merit aid eligibility and allows a treatment on the treated interpretation. Online Appendix Table A24 shows that even when accounting for differences in the likelihood of merit aid receipt, only married, white, and higher educated mothers experience a decline in annual hours of work.

Finally, we estimate the effect of merit aid by the number of children in college at the same time. Conditional on their total number of children, we expect women with more children in college to decrease their labor supply the most, because they can accumulate multiple merit aid awards. Confirming the benefits of receiving multiple merit aid grants, mothers with two or more children in college adjust their labor supply the most. Among mothers with one child in college, a 10% increase in merit aid spending per undergraduate student is associated with a 0.77% (−0.101∗0.1∗1,125/1467) decrease in hours worked (Table 7, column (10)). Among mothers with at least two children in college, a 10% increase in merit aid is associated with a 3.8% (−0.561∗0.1∗1,125/1660) decrease in hours worked (Table 7, column (11)).

8. Dynamic Effects of Merit Aid on Maternal Labor Supply

Previous sections estimate the contemporaneous effects of merit aid on mothers while their children attend college. This section sheds light on the life-cycle dynamics of responses to merit aid. We conduct our analysis on the main sample of mothers of college-going children used in equation (4).

First, we test if mothers adjust their labor supply even before the first child starts college. We perform a variant of equation (4), where all variables are measured 1 to 2 years before the mother's first child attends college.[37] This is a deviation from equation (4), where all variables are measured as of year *t*—when the mother has a child in college. The coefficient of interest measures the effect of a $1 increase in merit aid spending per undergraduate student 1 to 2 years before the first child enters college on the labor supply of mothers in that year.

We find suggestive evidence that mothers are forward looking. Table 8 shows that merit aid has a negative effect on hours of work 1 to 2 years before the first child attends college, where the coefficient on hours of work (−0.125; column (1)) is 60% of the size

36. Online Appendix C also shows that the decline in labor supply is concentrated among mothers in the top tercile of annual hours of work distribution and working in occupations with flexible schedules, consistent with their ability to adjust their hours. Moreover, merit aid increases the probability of part-time work.

37. Given that starting from 1997 the PSID becomes an every-other-year survey, we observe the mother 1 to 2 years before the first child attends college.

TABLE 8. Dynamic effects of merit aid among mothers of college-going children.

	(1) 1 to 2 years before first child in college	(2) 1 to 2 years after last child in college
	A. Dependent variable: annual hours of work	
Merit per FTE student	−0.125	−0.101
	[0.145]	[0.149]
Observations	6,295	5,689
Pre-treatment mean	1,485	1,405
First-stage F-statistic	32.69	13.41
	B. Dependent Variable: Employment Status	
Merit per FTE student	−0.00137	−0.00627
	[0.00574]	[0.00629]
Observations	6,295	5,689
Pre-treatment mean	85.05	80.45
First-stage F-statistic	32.69	13.41

Notes: This table presents the effect of merit aid spending per FTE student 1 to 2 years before the first child enters college (column (1)), and 1 to 2 years after the last child leaves college (column (2)). We use an indicator for the period after merit aid in strong states as an instrumental variable in Section 7.

of the coefficient in the years when a child attends college (Table 7, panel A, column (4)) but is not statistically significant.

Second, we test whether the decline in labor supply while children are in college persists even after the last child leaves college. We perform a variant of equation (4), where merit aid per undergraduate student ($Merit_{i,s,\,t}$) is measured in year t—when the mother has a child in college. However, all other variables are measured 1 to 2 years after the last child leaves college. The coefficient of interest measures the effect of a \$1 increase in merit aid spending per undergraduate student in year t on labor supply 1 to 2 years after the last child leaves college. Importantly, we measure the differential effect of merit aid among treatment and comparison states; even though college expenses decline after the last child leaves college, this happens among parents in both types of states.

We find suggestive evidence that while mothers reduce their hours the most while children are in college, some of this reduction may persist after children leave college. Table 8 shows that merit aid has a negative effect on hours of work 1 to 2 years after the last child leaves college, where the coefficient on hours of work (−0.0971) is roughly half the size of the coefficient when the child attends college (Table 6, panel A, column (4)) but is not statistically significant.

9. Discussion

How do the employment responses among mothers compare to the size of the merit aid transfer? Accounting for the total effect of merit aid on families' finances is challenging due to the lack of information on who receives it in the PSID, the expected number of years and amount of the transfer, and monetary savings from a child not attending a more expensive out-of-state institution. Nonetheless, suggestive back-of-the-envelope calculations in Online Appendix E allow us to estimate the average annual transfer and the average decline in earnings.

Comparing the annual maternal decline in earnings in the years when a child is in college ($7,899) to the annual merit aid transfer ($8,868), we estimate that the maternal decline in earnings accounts for about 88.8% of the transfer. These estimates are consistent with female labor supply being more income elastic historically than male labor supply (Keane 2011, and Bargain and Peichl 2016).[38] How do our suggestive estimates compare to those in the literature on exogenous unearned income shocks? On one hand, our estimates for mothers are in the same range as those of effects of income transfers to mothers during a child's early years; similarly, we do not find responses among fathers.[39] Mothers of young children likely reduce labor supply because young children benefit the most from interactions with their mother. Our results provide novel evidence that child-related transfers continue to play a major role in maternal labor supply, even once children grow up. Our finding of responses *only* among mothers is of note, because income transfers when children transition to adulthood could affect the labor supply of both parents, as adult children require much less mother-specific and time-intensive care than immediately after birth. Our estimates are also in the same range as the estimate of the effect of welfare transfers on the labor supply of older workers, and consistent with stronger effects of welfare transfers on the labor supply of women than men (Giupponi 2019).[40]

Our paper also relates to the literature on early child care subsidies and early pre-K education through the "time-transfer" channel. In response to early child care subsidies, female employment may increase after the decrease in child care costs raises the net-wage (earnings—child care costs). However, at the intensive margin, the effect on hours of work is ambiguous due to both income (decrease hours) and substitution (increase hours) effects (Cascio, Haider, and Nielsen 2015). This literature finds both positive and null extensive margin effects on female employment and limited effects at the intensive margin.[41] In contrast to the early child care literature, merit aid has no detectable effect at the extensive margin but negative effects at the intensive margin.

Even though adult children are not as time-intensive as young children, merit aid may also lower the net-wage of mothers through increased time-transfers to their

38. However, recent work shows that married women's wage elasticities have declined over time in the United States (Blau and Kahn 2007; Heim 2007).

39. Gonzalez (2013; Spain) finds a 2,500 euro benefit at the birth of a child reduces female earnings by 700 euros in the following 12 months. Using estimates from Schirle (2015; Canada), we calculate that receiving $1,200 per year until the child is age 6 reduces female yearly earnings by $1,040 while the child is under age 6. Wingender and LaLumia (2017; United States) find that each additional dollar of tax benefits (December births tax break) reduces earnings in the year after birth by a dollar.

40. Earnings increase one-for-one with survivor benefit losses (Giupponi 2019).

41. Gelbach 2002, Fitzpatrick 2010, Carta and Rizzica 2018.

children who may attend college closer to home. In Online Appendix Table A25, we do not find evidence that the probability of co-residence with a college-age or a college-going child changes after merit aid. However, a more complete study of whether maternal time with adult children changes is beyond the scope of this paper, due to lack of data on how involved the mother remains in the child's life and where a child attends college and is a promising area of future inquiry.

Our estimates for mothers are larger than those found in the literature studying the effects of winning the lottery and receiving the 2008 tax rebate in the United States.[42] Moreover, our lack of an effect for fathers is in contrast to similar responses among men and women to winning the lottery and receiving a tax rebate. Effects of unexpected changes in college costs may be different from an unexpected windfall of money for several reasons. First, families likely plan for future college costs by adjusting savings earlier in life. Second, effects from changes in college costs may be due to the parental "time-transfer" channel, which is different from the "income-effect" channel. Finally, lottery players may not be representative of the overall population, and labor supply responses may differ after increases in other income.

10. Conclusion

This study documents a meaningful link between a child's transition to adulthood and the labor supply of mothers, but not of fathers. Mothers of college-going children decrease their annual hours of work after the start of merit aid programs in their state of residence. Almost the entire decline in labor supply stems from married, more educated, white, and high-income mothers; we show evidence that this is because these mothers have a more elastic labor supply.

This paper provides a novel contribution to the literature relating childbirth to female labor supply. While it is well-known that young children are one of the most important deterrents of female labor supply, this study provides the first causal evidence that adult children influence labor supply later in life. Even though adult children require less time-intensive and mother-specific care, it is of note that in our setting they do not affect the labor supply of fathers.

Our findings suggest that mothers adjust their labor supply in a similar way to adult-child related transfers as to young-child related transfers. Do we expect this? On one hand, the labor supply of mothers with young children may be more responsive, because of greater perceived returns to spending time with their children. Mothers may believe that young children benefit the most from interactions with the mother or may be dissatisfied with the available child care options. On the other hand, the labor supply of mothers with adult children may be more responsive, because

42. Winning 1 million SEK reduces annual earnings by 11,000 SEK (Cesarini et al. 2017); an extra dollar of lottery income reduces pre-tax labor earnings by 50 cents (Golosov et al. 2021); earnings decline by 23 cents per rebate dollar (Powell 2020).

they are closer to retirement and may face lower penalties for career interruptions (Miller 2011).

Finally, this study underscores the importance of considering the potential effects of making college more affordable on the whole family. While previous literature has evaluated the effect of college costs on children's outcomes (Page and Scott-Clayton 2016), it has largely ignored their potential effects on parental labor supply. Effects can be economically meaningful, because college costs represent a major expense for families.

In addition, this study underscores the importance of considering how lower college costs change economic inequalities across groups. In the case of merit aid, more advantaged mothers decrease their labor supply the most. While this leads to the shrinking of the gap in hours of work between more and less advantaged mothers, the gap in consumption and wealth may go up if families do not completely replace the merit aid transfer with lost earnings.

We expect differential effects on labor supply of other programs that make college more affordable, depending on whether these programs target lower-income or higher-income families. For programs that target lower-income families, such as Pell grants (Dynarski 2004b), we expect lower responses in labor supply, because we find limited evidence of responses among disadvantaged families, even though a substantial share of these families receives merit aid. For programs that target higher-income families, such as tax advantaged college savings accounts (Dynarski 2004b), we expect larger responses in labor supply, because we mainly find evidence of responses among advantaged families. Thus, in designing programs that lower college costs, it is important to keep in mind that effects on families depend critically on what types of families benefit from reductions in college costs the most.

References

Altonji, Joseph, Timothy Conley, Todd E. Elder, and Christopher R. Taber (2019). "Methods for Using Selection on Observed Variables to Address Selection on Unobserved Variables." Working paper, Yale University.

Andersen, Asger, Niels Johannesen, and Adam Sheridan (2020). "Bailing Out the Kids: New Evidence on Informal Insurance from One Billion Bank Transfers." Working paper.

Athey, Susan and Guido Imbens (2022). "Design-Based Analysis in Difference-in-Differences Settings with Staggered Adoption." *Journal of Econometrics*, 226, 62–79.

Bailey, Martha, Olga Malkova, and Zoë McLaren (2019) "Does Access to Family Planning Increase Children's Opportunities? Evidence from the War on Poverty and the Early Years of Title X." *Journal of Human Resources*, 54, 825–56.

Baker, Andrew, David Larcker, and Charles Wang (2022). "How Much Should We Trust Staggered Difference-In-Differences Estimates?" *Journal of Financial Economics*, 144, 370–95.

Bargain, Olivier and Andreas Peichl (2016). "Own-Wage Labor Supply Elasticities: Variation across Time and Estimation Methods." *IZA Journal of Labor Economics*, 5, 1–31.

Barr, Andrew (2016). "Enlist or Enroll: Credit Constraints, College Aid, and the Military Enlistment Margin." *Economics of Education Review*, 51, 61–78.

Baum, Sandy and Sarah Turner (2019). "Free Tuition Is the Opposite of Progressive Policymaking." *Washington Post*, May 3.

Blau, Francine and Lawrence Kahn (2007). "Changes in Labor Supply Behavior of Married Women: 1980–2000." *Journal of Labor Economics*, 25, 393–438.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess (2021). "Revisiting Event Study Designs: Robust and Efficient Estimation." Working paper.

Bound, John, Charles Brown, and Nancy Mathiowetz (2001). "Measurement Error in Survey Data." In *Handbook of Econometrics*. Vol. 5, edited by J. Heckman and E. Leamer, pp. 3705–843. Elsevier Science, Amsterdam.

Bruce, Donald and Celeste Carruthers (2014). "Jackpot? The Impact of Lottery Scholarships on Enrollment in Tennessee." *Journal of Urban Economics*, 81, 30–44.

Callaway, Brantly and Pedro Sant'Anna (2021). "Difference-in-Differences with Multiple Time Periods." *Journal of Econometrics*, 225, 200–30.

Carruthers, Celeste and Umut Özek (2016). "Losing HOPE: Financial Aid and the Line between College and Work." *Economics of Education Review*, 53, 1–15.

Carta, Francesca and Lucia Rizzica (2018). "Early Kindergarten, Maternal Labor Supply and Children's Outcomes: Evidence from Italy." *Journal of Public Economics*, 158, 79–102.

Cascio, Elizabeth, Steven Haider, and Helena Skyt Nielsen (2015). "The Effectiveness of Policies That Promote Labor Force Participation of Women with Children: A Collection of National Studies." *Labour Economics*, 36, 64–71.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer (2019). "The Effect of Minimum Wages on Low-Wage Jobs." *The Quarterly Journal of Economics*, 134, 1405–54.

Cesarini, David, Erik Lindqvist, Matthew J. Notowidigdo, and Robert Östling (2017). "The Effect of Wealth on Individual and Household Labor Supply: Evidence from Swedish Lotteries." *American Economic Review*, 107(12), 3917–46.

Chapman, Stephanie (2015). "Student Loans and the Labor Market: Evidence from Merit Aid Programs." Working paper, Northwestern University.

Cohodes, Sarah and Joshua Goodman (2014). "Merit Aid, College Quality, and College Completion: Massachusetts' Adams Scholarship as an in-Kind Subsidy." *American Economic Journal: Applied Economics*, 6, 251–85.

College Board (2018). "Trends in College Pricing 2018." Washington, DC. https://research.collegeboard.org/media/pdf/trends-college-pricing-2018-full-report.pdf. Accessed May 2023.

Compton, Janice and Robert Pollak (2015). "Proximity and Co-Residence of Adult Children and Their Parents in the United States: Descriptions and Correlates." *Annals of Economics and Statistics*, 117/118, 91–114.

Cornwell, Christopher and David Mustard (2007). "Merit-Based College Scholarships and Car Sales." *Education Finance and Policy*, 2, 133–51.

Cornwell, Christopher, David Mustard, and Deepa Sridhar (2006). "The Enrollment Effects of Merit-Based Financial Aid: Evidence from Georgia's HOPE Program." *Journal of Labor Economics*, 24, 761–86.

Cortés, Patricia and Jessica Pan (2020). "Children and the Remaining Gender Gaps in the Labor Market." *Journal of Economic Literature*, forthcoming. https://www.aeaweb.org/articles?id=10.1257/jel.20221549.

Cowan, Benjamin and Dustin White (2015). "The Effects of Merit-Based Financial Aid on Drinking in College." *Journal of Health Economics*, 44, 137–49.

de Chaisemartin, Clément and Xavier d'Haultfœuille (2020). "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review*, 110(9), 2964–96.

Dick, Andrew and Aaron Edlin (1997). "The Implicit Taxes from College Financial Aid." *Journal of Public Economics*, 65, 295–322.

Ding, Xiaozhou, Steven Lugauer, and Christopher Bollinger (2022). "The Expansion of Higher Education and Household Saving in China." *China Economic Review*, 71, 101736.

Dynarski, Susan (2004). "Who Benefits from the Education Saving Incentives? Income, Educational Expectations and the Value of the 529 and Coverdell." *National Tax Journal*, 57, 359–83.

Dynarski, Susan (2004a). "The New Merit Aid." In *College Choices: The Economics of Where to Go, When to Go, and How to Pay for It*, edited by Caroline Hoxby , pp. 63–97. University of Chicago Press, Chicago.

Dynarski, Susan (2008). “Building the Stock of College-Educated Labor.” *Journal of Human Resources*, 43, 576–610.
Eissa, Nada and Hilary Williamson Hoynes (2004). “Taxes and the Labor Market Participation of Married Couples: The Earned Income Tax Credit.” *Journal of Public Economics*, 88, 1931–58.
Faber, Jacob and Peter Rich (2018). “Financially Overextended: College Attendance as a Contributor to Foreclosures during the Great Recession.” *Demography*, 55, 1727–48.
Feinberg, Robert and Daniel Kuehn (2018). “Guaranteed Nonlabor Income and Labor Supply: The Effect of the Alaska Permanent Fund Dividend.” *The BE Journal of Economic Analysis & Policy*, 18, 13.
Feldstein, Martin (1995). “College Scholarship Rules and Private Saving.” *American Economic Review*, 85(3), 552–66.
Fitzpatrick, Maria (2010). “Preschoolers Enrolled and Mothers at Work? The Effects of Universal Prekindergarten.” *Journal of Labor Economics*, 28, 51–85.
Fitzpatrick, Maria and Damon Jones (2016). “Higher Education, Merit-Based Scholarships and Post-Baccalaureate Migration.” *Economics of Education Review*, 54, 155–72.
Frisvold, David and Melinda Pitts (2018). “State Merit Aid Programs and Youth Labor Market Attachment.” NBER Working Paper No. w24662.
Gelbach, Jonah B. (2002). “Public Schooling for Young Children and Maternal Labor Supply.” *American Economic Review*, 92(1), 307–22.
Giupponi, Giulia (2019). “When Income Effects Are Large: Labor Supply Responses and the Value of Welfare Transfers.” CEP Discussion Papers No. 165, Centre for Economic Performance, LSE, London, UK.
Golosov, Mikhail, Michael Graber, Magne Mogstad, and David Novgorodsky. (2021). “How Americans Respond to Idiosyncratic and Exogenous Changes in Household Wealth and Unearned Income.” NBER Working Paper No. 29000.
Gonzalez, Libertad (2013). “The Effect of a Universal Child Benefit on Conceptions, Abortions, and Early Maternal Labor Supply.” *American Economic Journal: Economic Policy*, 5, 160–88.
Goodman, Joshua (2008). “Who Merits Financial Aid? Massachusetts’ Adams Scholarship.” *Journal of Public Economics*, 92, 2121–31.
Goodman-Bacon, Andrew (2021). “Difference-in-Differences with Variation in Treatment Timing,” *Journal of Econometrics*, 225, 254–77.
Gurantz, Oded and Taylor Odle (2022). “The Impact of Merit Aid on College Choice and Degree Attainment: Reexamining Florida’s Bright Futures Program.” *Educational Evaluation and Policy Analysis*, 44, 79–104.
Haider, Steven and Kathleen McGarry (2018). “Parental Investments in College and Later Cash Transfers.” *Demography*, 55, 1705–25.
Handwerker, Elizabeth (2011). “Delaying Retirement to Pay for College.” *ILR Review*, 64, 921–48.
Harris, Douglas (2019). “The Central Role of Free College and Loan Forgiveness in the Democratic Primary.” Brookings Brown Center Chalkboard.
Heim, Brad (2007). “The Incredible Shrinking Elasticities: Married Female Labor Supply, 1978–2002.” *Journal of Human Resources*, 42, 881–918.
Heller, Donald and Patricia Marin (2004). *State Merit Scholarship Programs and Racial Inequality*. The Civil Rights Project at Harvard University, Cambridge, MA.
Isen, Adam, Maya Rossin-Slater, and Reed Walker (2017). “Every Breath You Take—Every Dollar You’ll Make: The Long-Term Consequences of the Clean Air Act of 1970.” *Journal of Political Economy*, 125, 848–902.
Kaplan, Greg (2012). “Moving Back Home: Insurance against Labor Market Risk.” *Journal of Political Economy*, 120, 446–512.
Keane, Michael (2011). “Labor Supply and Taxes: a Survey.” *Journal of Economic Literature*, 49, 961–1075.
Kleven, Henrik, Camille Landais, and Jacob Søgaard (2019). “Children and Gender Inequality: Evidence from Denmark.” *American Economic Journal: Applied Economics*, 11, 181–209.
Koebel, Kourtney and Tammy Schirle (2016). “The Differential Impact of Universal Child Benefits on the Labour Supply of Married and Single Mothers.” *Canadian Public Policy*, 42, 49–64.

Long, Mark (2004). “The Impact of Asset-Tested College Financial Aid on Household Savings.” *Journal of Public Economics*, 88, 63–88.
Lovenheim, Michael (2011). “The Effect of Liquid Housing Wealth on College Enrollment.” *Journal of Labor Economics*, 29, 741–71.
Mae, Sallie (2018). *How America Pays for College: Sallie Mae’s National Study of College Students and Parents*. Report.
McDonough, Patricia M., Shannon M. Calderone, and William C. Purdy (2007). “State Grant Aid and Its Effects on Students’ College Choices.” *Western Interstate Commission for Higher Education*.
McGarry, Kathleen (2016). “Dynamic Aspects of Family Transfers.” *Journal of Public Economics*, 137, 1–13.
Miller, Amalia (2011). “The Effects of Motherhood Timing on Career Path.” *Journal of Population Economics*, 24, 1071–100.
Olea, José Luis Montiel and Carolin Pflueger (2013). “A Robust Test for Weak Instruments.” *Journal of Business & Economic Statistics*, 31, 358–69.
Page, Lindsay and Judith Scott-Clayton (2016). “Improving College Access in the United States: Barriers and Policy Responses.” *Economics of Education Review*, 51, 4–22.
Pallais, Amanda (2009). “Taking a Chance on College: Is the Tennessee Education Lottery Scholarship Program a Winner?” *Journal of Human Resources*, 44, 199–222.
Pew Research Center (2013). “A Rising Share of Young Adults Live in Their Parents’ Home.”
Powell, David (2020). “Does Labor Supply Respond to Transitory Income? Evidence from the Economic Stimulus Payments of 2008.” *Journal of Labor Economics*, 38, 1–38.
Radwin, David and Christina Chang Wei (2015). “What Is the Price of College? Total, Net, and out-of-pocket Prices by Type of Institution in 2011–12.” National Center for Education Statistics no. 2015165.
Roth, Jonathan (2022). “Pretest with Caution: Event-Study Estimates after Testing for Parallel Trends.” *American Economic Review: Insights*, 4(3), 305–22.
Schirle, Tammy (2015). “The Effect of Universal Child Benefits on Labour Supply.” *Canadian J of Economics*, 48, 437–63.
Scott-Clayton, Judith (2011). “On Money and Motivation: A Quasi-Experimental Analysis of Financial Incentives for College Achievement.” *Journal of Human Resources*, 46, 614–46.
Scott-Clayton, Judith and Basit Zafar (2019). “Financial Aid, Debt Management, and Socioeconomic Outcomes: Post-College Effects of Merit-Based Aid.” *Journal of Public Economics*, 170, 68–82.
Sjoquist, David and John Winters (2014). “Merit Aid and Post-College Retention in the State.” *Journal of Urban Economics*, 80, 39–50.
Sjoquist, David and John Winters (2015a). “State Merit Aid Programs and College Major: A Focus on STEM.” *Journal of Labor Economics*, 33, 973–1006.
Sjoquist, David and John Winters (2015b). “State Merit-Based Financial Aid Programs and College Attainment.”*Journal of Regional Science*, 55, 364–90.
Wingender, Philippe and Sara LaLumia (2017). “Income Effects on Maternal Labor Supply: Evidence from Child-Related Tax Benefits.” *National Tax Journal*, 70, 11–52.
Zhang, Liang and Erik Ness (2010). “Does State Merit-Based Aid Stem Brain Drain?” *Educational Evaluation and Policy Analysis*, 32, 143–65

Supplementary Data

Supplementary data are available at *JEEA* online.

Journal of the European Economic Association 2024 22(1):230–262
https://doi.org/10.1093/jeea/jvad051