Abstract

This paper uses variation in the timing of parental layoff to identify the effect of parental job loss on higher education enrollment. Unlike research that compares laid-off workers to workers who do not lose their jobs, all families in our analysis experience a layoff at some point. The treatment group (layoff when child is 15-17) and control group (layoff when child is 21-23) have statistically indistinguishable initial characteristics, but substantially different higher education enrollment rates. We find that parental job loss between ages 15 and 17 decreases college enrollment by 10 percentage points.
1. Introduction

Classical models of education investment predict that, in the absence of credit constraints, investment decisions should be solely determined by the rate of return to education relative to other investment opportunities (Becker, 1962). These models predict that college enrollment should be independent of family resources, a prediction that is at odds with the large college enrollment gaps between rich and poor families. While some point to the relationship between family income and college enrollment as evidence of credit constraints (Lochner and Monge-Naranjo, 2012; Belley and Lochner, 2007; Goodman, 2010), others argue that the relationship between family income and college enrollment could be due to unobserved correlates of parental income (Carneiro and Heckman, 2002; Cameron and Taber, 2004). This latter literature points out that children from lower-income families may face larger costs or lower benefits to enrolling in higher education, thus even in the absence of credit constraints, one would expect a relationship between family income and college enrollment. Finally, to the extent that higher education is a normal consumption good, we would expect a relationship between family income and college enrollment.

We provide new evidence in support of the notion that parental labor market outcomes causally impact higher educational enrollment. Specifically, we use the PSID to compare families who all experience layoffs, but where the timing of the layoff differs with respect to their children’s ages. We find strong, robust evidence that enrollment in higher education differs sharply depending on whether a parent is laid off before or after
the college enrollment decision. Under the assumption that future parental job loss has no
direct effect on past college enrollment, the control group represents the population that
eventually experiences parental job loss, but whose enrollment decisions are not
influenced by the displacement.

This research contributes to a literature on the intergenerational effects of layoff. It is
well documented that involuntary job loss leads to large decreases in an individual’s
Oreopoulos et al. (2008) shows that parental job loss also has intergenerational impacts on
earnings. One important pathway through which parental job loss may impact children’s
earnings is through investment in higher education. Though several studies have
examined the relationship between parental layoff and college enrollment (Shea, 2000;
Page, Stevens and Lindo, 2009; Kalil and Wightman, 2011), previous research has relied
on being able to sufficiently control for differences between laid-off workers and workers
who experience no layoff. Past research has used plant closures and industry variation to
help create comparable treatment and control groups; however, we view our empirical
approach as a cleaner test of credit constraints since we can control for unobservable
factors that might lead certain types of individuals to enter declining industries or firms.

Our identification strategy is similar to the idea behind the falsification test in Coelli
(2011). Coelli (2011) uses Canadian longitudinal data and finds evidence of large
negative effects of parental job loss on post-secondary enrollment of youth. To test the
exogeneity of parental job loss, the author estimates a model that includes an indicator
denoting job losses that occur when the youth is aged 18-19 (after the educational
decision). We expand the idea further, taking advantage of the long panel of the PSID to
carefully test the exogeneity of the timing of parental job loss. For example, our analysis
considers whether cohort effects, birth order, parental age or other covariates act to
systematically bias estimates based on the timing of layoff. Furthermore, we provide
evidence against the notion that our results are driven by an anticipation of future parental
layoff or manipulation of the timing of layoff.

In a working paper written in parallel with our own, Hilger (2013) uses a similar
identification strategy to examine the impact of layoff on college enrollment. While
Hilger (2013) finds that college enrollment declines as a result of layoff, the magnitude of
his estimate is much smaller than that found in our paper. Given the similarities in
methodology, the divergence between our results is likely due to either measurement or
data differences. Our paper uses survey data and measures college enrollment based on
the completion of a full year of higher education whereas Hilger (2013) uses
administrative data and defines college enrollment based on tax filings from the
university.

Given that the change in enrollment rates caused by parental layoff is not attributable
to unobservable differences across families, there are several mechanisms through which
the causal impact might operate. First, if families have limited access to credit, changes
in family resources at the time of the college decision can reduce college enrollment.
Second, if higher education is partly a consumption good, then wealth effects could lead
to decreased higher education enrollment following the drop in wealth caused by a layoff.

Third, parental layoff during high school could directly impact high school performance, which in turn might lower a student’s propensity to enroll in higher education. Finally, it is possible that layoff increases family stress and conflict and leads to an environment less supportive of higher educational investments. While we cannot differentiate between these explanations, our estimates are unchanged when controlling for factors such as geographic mobility, divorce, and parental self-reported health, suggesting that these are unlikely channels.

2. Higher Education Financing in the United States

The direct cost of higher education varies considerably across institutions, with public two-year colleges averaging $3,131 per year and private four-year institutions averaging $29,056 per year (Payea, Baum & Kurose, 2013). While these fees represent a substantial portion of median wealth, few students pay these costs up-front because there are many avenues through which students can access need- and merit-based grants.

Despite the fact that few students are required to pay the full sticker price of higher education, students may rule out going to college based on their perception that it is unaffordable. Based on this concern, the 2008 Higher Education Opportunity Act included a requirement that colleges make “net price calculators” available to prospective students by 2011. During the time frame of our study, these calculators were not available, but a well-informed student would have been able to calculate their own net
price or reach out to prospective universities to obtain this information. That said, even since net price calculators became available, a majority of students still report that they have ruled out colleges based solely on the sticker price alone, without considering financial aid (studentPoll, 2012).

Though net prices are well below sticker prices, parents and students still borrow an average of $4,410 per year to pay for higher education (Payea, Baum & Kurose, 2013). Students and parents share both this debt burden and the up-front cost of higher education, with students paying for approximately 30% of the total cost and parents paying for approximately 40% (Sallie Mae & Ipsos, 2011). While the majority of these loans come from federal programs such as the Stafford Loan Program, nearly 10% of the loan amount came from private or state loans.

Though the federal government provides fairly easy credit for financing the direct costs of education, there are several reasons why credit constraints may still exist. First, loan amounts are capped and loans cannot be used to support family member living expenses (Lochner and Monge-Naranjo, 2012). While private loans can be used to finance other expenses, these loans are credit rated, which presents a major obstacle for young adults with little to no credit. Second, the federal financial aid application process is complicated and work intensive, which may discourage families from filling out the necessary paperwork to qualify for a loan. Bettinger et al. (2012) shows that the barriers created by the complicated FAFSA are substantively important for college attendance
since randomly providing families with help in filling out the FAFSA is found to increase college enrollment by 8 percentage points.

3. Identification Strategy

Figure 1 illustrates both the main idea of the paper and previews the key result. The figure is restricted to only families in which the head is laid off at some point. The x-axis shows the age of the child when their parent is laid off. The y-axis is the rate of enrollment in higher education.\(^1\) This figure clearly demonstrates that layoffs that occur when the child is ages 15-17 are associated with much lower college enrollment compared to layoffs that occur at later (or earlier) ages. We confirm the implications of this figure using a regression analysis and find that parental layoff decreases higher education enrollment by 10.1 percentage points.

For several reasons, we argue that this 10.1 percentage point difference is likely the causal impact of parental layoff on higher education enrollment. First, we show that the timing of layoff relative to enrollment decisions is unrelated to the observable characteristics of the families. Second, since our analysis is restricted to families that experience a layoff, the empirical design accounts for unobservable differences between families that experience a layoff and families that do not.

\(^1\) Figure 1 defines enrollment based on the assumption that students proceed immediately from high school to higher education. The exact definition of higher education enrollment is somewhat complicated because some students enroll at older ages. This creates a problem for our context since we rely on layoffs at older ages having no impact on enrollment. We discuss this issue extensively in the data section and show robustness across a wide variety of data choices.
Furthermore, we find evidence against several potential threats to causal identification. First, if families anticipate future layoffs, this has the potential to bias our estimates towards zero. This interpretation is inconsistent with Figure 1, since enrollment rates when a parent is laid off at 18-20 are no lower than enrollment rates when a parent is laid off at ages 21-23. To limit concerns that families anticipate layoffs, and also to address ambiguity with respect to the exact timing of enrollment decisions, our preferred specification compares layoffs that occur at ages 15-17 to layoffs that occur at ages 21-23.

A second potential threat to identification is that parents who are laid off when their child is 15-17 are younger than parents who are laid off when their child is 21-23. If the nature of layoff changes with age, then this has the potential to bias estimates. Specifically, if parents who are laid off at younger ages are more negatively selected than parents who are laid off at older ages, then this could explain our findings. However, this explanation is inconsistent with the pattern shown in Figure 1. If parents who are laid off at younger ages are more negatively selected, then one would expect that enrollment rates would be smoothly increasing with age of layoff. Figure 1, however, shows that children of parents with a layoff at 15-17 have lower enrollment rates than children of parents laid off at ages 12-14, and there is no broad increase in enrollment rates as age at parental layoff increases. Regardless, our regression controls non-parametrically for parental age.

Third, even if layoff timing is as good as randomly assigned, sample attrition has the potential to bias estimates. In particular, in our data we observe more individuals at ages
15-17 compared to ages 21-23. If unobservable characteristics that lead to college attendance also decrease the likelihood of attrition from the sample, then our estimates could be biased. We find this explanation unlikely since sample attrition is a smooth increasing function of age, while Figure 1 shows that the relationship between enrollment and parental layoff shifts discontinuously after age 18. Furthermore, sample attrition monotonically increases with age of parental layoff from age 18 to 23, but the enrollment probabilities are essentially flat.

4. Data

The data in this analysis is from the Panel Study of Income Dynamics (PSID), which began in 1968 with a sample of more than 5,000 families in the United States. The PSID interviewed the original members and the children born into study families annually from 1968 to 1996 and biennially from 1997 to 2009. The original PSID sample was drawn from two independent samples: one is a low-income families sample from the Survey of Economic Opportunity (the “SEO sample”) and the other is a nationally representative sample designed by the Survey Research Center (the “SRC sample”).

The PSID interviews sample members and non-sample members. Sample members include members of the original 1968 families and children of sample members. Non-sample members are the people who live with the sample members. Their information is collected as part of the interview of the household. The major distinction in treating sample members and non-sample members is how they are followed. The PSID
tries to follow all sample members no matter where they go, but does not follow non-sample members once they move out of the households of sample members. Thus, the attrition rates for sample members and non-sample members are very different. To avoid systematic attrition problems, we limit our analysis to sample members.

Since 1985, the PSID has documented detailed information about job displacement, including the reason and time of displacements. The information on layoffs, along with the information on month of birth, allows us to construct a dataset recording the age in months when parental job loss occurred. For our primary specification, we require data on layoffs that occur at ages 15-17 and 21-23 and we thus restrict this analysis to include the sample members born between 1970 and 1985. To create a consistent panel over this time period, we use only biennial surveys starting in 1985, since the latter half of the data has no information for even-numbered years. For analyses that consider long-run outcomes such as wages at age 30, we include the 2011 wave to allow for the inclusion of an additional cohort.

While the PSID has exact information regarding the timing of parental layoff, the information on higher education enrollment is more limited. First, the biennial survey structure prevents us from distinguishing between enrollment at age 18 versus enrollment at age 19. This complicates the empirical design, since the timing of layoff relative to enrollment is the key feature of our analysis. Second, for the majority of participants, the PSID has no information on contemporaneous college enrollment. Like past research
(Kalil and Wightman, 2011; Lovenheim, 2011), we infer enrollment status based on a participant reporting 13 years of completed education.2

The two data issues discussed above make it difficult to predict whether a layoff that occurs at 18, 19, or 20 should have an impact on our measure of enrollment. While a layoff at 19 likely has no impact on enrollment decisions made at age 17, the layoff can directly impact dropout during the first year or delayed enrollment, thus impacting our measure of enrollment. Since our measure of higher education makes it difficult to discern whether a layoff at age 18, 19, or 20 should or should not have an impact on enrollment, we show the robustness of our results to a variety of choices regarding how treatment and control are defined. In particular, our preferred specification compares enrollment rates for layoffs that occur at ages 15-17 to layoffs that occur at ages 21-23, thereby avoiding dating ambiguity. Regardless, the results are very similar when using a control group of 18-20, reflecting the fact that dropout in the first year and delayed enrollment are relatively uncommon. In the remainder of the text, we will refer to families that experience a layoff at ages 15-17 as the treatment group, and families that experience a layoff at ages 21-23 as the control group. Since there are families that experienced a layoff in both periods, the two groups overlap and we include a direct control for these dual layoff families in all specifications. When we use the terms “treatment group” and

2 For simplicity, we use the term “enrollment” throughout the text, but more precisely we are measuring the completion of one year of higher education. If layoff causes dropout during the first year, then our estimates include this effect. The one exception is that if a student directly reports that they are residing at an educational institution, we count them as enrolled, even if they do not complete 13 years of higher education.
“control group” we refer to the children that experience parental layoff in only one of the periods.

Our sample consists of 4,030 children, 640 of whom have a parent who experience a layoff when they are between 15-17 or 21-23. Table 1 presents descriptive statistics. Though our analysis is focused on the 640 children whose families experience a layoff, we include non-laid-off families in some analyses to improve the precision of our estimates of the coefficients for other student and family characteristics.

Column (1) shows that approximately 50% of children in the full sample complete at least one year of higher education. Columns (2) and (3) show the same descriptive statistics but restrict the sample to children whose parents were laid off, split based on the timing of the layoff. Comparing column (2) to column (3), we see that the enrollment rate is 11 percentage points lower for families that experience the layoff when their child is ages 15-17 compared to ages 21-23. Table 1 also shows that other observable characteristics do not statistically differ between these two groups. This suggests that the higher education enrollment rate of children who experience a parental layoff at ages 21-23 is likely a good counterfactual for the higher education enrollment rate of children who experience a parental layoff at ages 15-17. Appendix Table A1 shows the same information from Table 1, but instead considers families that experience a layoff at ages 18-20 as the control group.

3 An additional 55 families experienced a layoff both when their child was 15-17 and when their child was 21-23.
Though the observable characteristics are statistically indistinguishable between treatment and control, the sample sizes are very different between the two groups. Nearly 150 more observations experience a layoff when their children are 15-17 compared to when their children are 21-23. This reflects the fact that the parents of 21-23 year olds are approximately 6 years older than the parents of 15-17 year olds, and the PSID sample is skewed towards younger ages. Appendix Figure A1 shows that many household heads in the PSID sample are under 40 and thus unlikely to have children aged 21-23.

Despite differences in sample size, so long as the timing of layoff is exogenous to underlying college enrollment propensity, then Table 1 alone provides an unbiased estimate of the effect of parental job loss on college enrollment. However, in the regression analysis, we control for all covariates shown in Table 1, because adding controls allows for more precise estimation.

5. Empirical Model

We formalize the results from Figure 1 and Table 1 by placing the analysis in a regression context. Our regression model includes only children who experience a layoff, either at ages 15-17 or at ages 21-23.\(^4\) Using this sample, we estimate:

\[ Y_i = \beta L_{i,\text{before}}^{\text{before}} + \alpha L_{i,\text{both}}^{\text{both}} + \beta X_i + \epsilon_i \]  

\(Y_i\) is an indicator for higher education enrollment. \(L_{i,\text{before}}^{\text{before}}\) is an indicator that is unity if a child’s parent is laid off at ages 15-17. The variable \(L_{i,\text{both}}^{\text{both}}\) is an indicator that is unity for

\(^4\) This empirical specification uses layoff at ages 21-23 as the control group; however, results are robust to using different control groups.
parents who are laid-off both when their child is 15-17 and also when their child is 21-23. We control for being laid-off in both periods since we expect that families that are laid-off twice are likely a select group.

The coefficient of interest for our study is \( \theta \). This coefficient captures the difference in conditional enrollment rates between families where layoff occurs at ages 15-17 and families where layoff occurs at ages 21-23. \( X_i \) is a vector of covariates that includes child demographics, child birth order, parental age, parental education, family income, family homeowner status, year of birth, and a constant term. In some specifications, we augment this baseline model by adding family fixed effects to account for unobserved time-invariant familial characteristics.

Though the outcome variable is binary, we estimate (1) using a linear probability model (LPM) for simplicity. The average marginal effects from the same specification estimated as a probit are nearly identical to the LPM results presented in the text. Furthermore, the LPM is reasonable in this context because empirically the LPM yields predicted probabilities between zero and one for the vast majority of observations.

6. Results

6.1 Main Estimation Results

Column (1) of Table 2 shows baseline results with no covariate controls. The coefficient of interest is “Parent laid-off at ages 15-17.” The estimated coefficient implies a statistically significant 11.1 percentage point reduction in higher education attendance. This coefficient reflects the difference between the enrollment rates for families where the
parent is laid off at 15-17 versus 21-23. Column (1) also shows that families that experience layoffs when their child is 15-17 and when their child is 21-23 have even lower college enrollment rates. Given that a layoff at 21-23 cannot impact enrollment decisions at 18, this latter coefficient should not be interpreted causally and simply reflects that families that experience two layoffs during a short period of time are a select group.

Columns (2) through (4) of Table 2 sequentially add individual controls, family controls and year-of-birth fixed effects. Individual and family controls are included to account for any observable differences between families that experience layoff at different times. Year-of-birth fixed effects are included to address the concern that economic fluctuations combined with changing enrollment over time could create a spurious correlation between the timing of layoff and educational enrollment. For example, the 2001 recession may make the probability of parental layoff at ages 15-17 relatively high for children born in 1985 since the recession occurred when these children were 16. Similarly, the 2001 recession could increase the probability of parental layoff at ages 21-23 for children born in 1979. If other cohort-based factors make the 1979 cohort more likely to attend higher education, then this could lead to biased estimates. In practice, concerns of cohort effects biasing estimates appear to be unfounded.

In total, adding covariates reduces the estimate from 11.1 percentage points to 10.1 percentage points, but these estimates are statistically indistinguishable from one another. The fact that the $R^2$ increases as covariates are added suggests that the covariates used are
meaningfully correlated with the outcome, and thus the stability of our estimates across specifications is not simply the result of using irrelevant covariates.

While there is little evidence that observable characteristics explain the relationship between layoff timing and enrollment, it remains possible that unobservable factors are correlated with the timing of layoff. To address this concern, Column (5) of Table 2 adds family fixed effects so that identification is based on comparisons of siblings who experience parental layoff at different ages. Column (5) shows that the coefficient when controlling for family fixed effects is slightly smaller in magnitude than the other specification, but it is statistically indistinguishable. With family fixed effects included, however, the standard error increases substantially, which is not surprising given the small amount of variation used to identify these estimates.

To improve the precision of the family fixed effect specification, we remove the restriction that the control group must be exactly ages 21-23. Importantly, this specification is identified by a much broader set of families compared to the previous specification. Specifically, the estimates are identified by families who have one child ages 15-17 and at least one other child (regardless of the age of the other child). Column (6) shows that when we include all siblings, the family fixed effect specification is very similar to estimates that exclude the family fixed effects and the estimates are statistically significant. In light of the large standard error on the Column (5) estimate, we take the specification that excludes the family fixed effect as our preferred specification, but it is reassuring that unobserved family-specific factors cannot explain our main result.
Our preferred estimates are in a similar range compared to past work investigating the impact of parental layoff on college enrollment in the United States, suggesting that the covariates used in past work were sufficiently rich so that unobservable differences between laid-off and stable workers did not drive past estimates. Kalil and Wightman (2009) estimate that parental layoff reduces college enrollment by approximately 10 percentage points. Using data on Canadian families, Coelli (2011) estimates that parental layoff reduces college enrollment at age 18 by approximately 13.7 percentage points. Coelli (2011) also estimates a falsification test and finds that parental job losses that occur from ages 18 to 19 have a statistically insignificant impact on college enrollment (point estimate of -0.031 with a standard error of 0.042). Consistent with the pattern for layoff at early age shown in Figure 1, Page et al. (2009) find no statistically significant relationship between layoff at ages zero to sixteen and subsequent college enrollment, though their coefficients are imprecisely estimated.

6.2 Robustness Checks

Though our analysis up until this point has compared layoffs at ages 15-17 (treatment) to layoffs at ages 21-23 (control), we can, in principle, make other choices regarding the definition of the treatment and control groups. As previously discussed, using layoffs that occur at ages 21-23 has the advantage of avoiding ambiguity regarding the dating of enrollment decisions and placates concerns that anticipation of future layoffs impacts college enrollment. That said, using layoffs that occur at ages 18-20 as the control
group has the advantage of comparing families who experience layoffs at more similar times.

To ensure that our results are not driven by our definition of treatment and control, Table 3 shows the estimated coefficients from 15 different regressions, each of which uses a different treatment and control group combination. The regressions’ specifications are identical to that estimated in Column (4) of Table 2, and the only thing that changes across specifications is how treatment and control are defined. Table 3 shows that the results are broadly similar across specifications. Though none of the estimates are statistically significantly different from one another, there are larger estimated effects when we restrict the treatment group to layoffs that occur at age 17.

Though layoff most directly impacts family resources, it is possible that layoff can also impact investment in higher education by influencing parental divorce, geographic mobility, health or time availability. While we are unable to completely rule out the possibility that layoff effects operate through these alternative channels, we find that our estimate remains fairly stable (-0.081 with a standard error of 0.041) when we add controls for parental divorce, geographic mobility, and self-reported parental health. Importantly, since these controls could themselves be outcomes of layoff, we add these controls to investigate potential mechanisms—not in an attempt to reduce bias. Although our estimate is robust to including controls for these alternative mechanisms, it is important to note that controlling for intermediate covariates to explore mechanisms is only valid in a context where the coefficients on the intermediate covariates are consistently estimated.
Furthermore, a variety of other changes may occur in laid-off families, such as increased stress levels, and we have no data to investigate these alternative mechanisms.

6.3 Effect Heterogeneity

Given the relatively small sample, our ability to split the sample to explore heterogeneity is somewhat limited. Nevertheless, looking at heterogeneity of the layoff effect is useful because it can provide suggestive evidence regarding the interpretation of the estimated impacts.

In Column (1) of Table 4 we explore heterogeneity in the impact of layoff depending on whether either parent has completed higher education. Our hypothesis is that those with less education might be less aware of financial aid opportunities and therefore more credit constrained. While statistically insignificant, the interaction effect shows that children from families with more educated parents appear to be less negatively impacted by layoff compared to children from families with less educated parents.

To explore whether the layoff effect differs with wealth, we explore heterogeneity by homeowner status. While this is only a proxy for wealth, for most US households, home equity represents the vast majority of wealth and during the time period considered, it was relatively easy for families to borrow against this equity (Lovenheim, 2011). Column (2) of Table 4 shows that the impact of layoff on higher education enrollment is driven entirely by families that do not own their home. This might suggest credit constraints since families that own their homes can take out a home equity loan to compensate for
their temporary lack of income due to layoff, whereas renters have no such option. We cannot, however, rule out the possibility that layoffs disrupt the high school performance of renters but not homeowners. Similarly, we cannot rule out the possibility that layoff disrupts the high school performance of both renters and homeowners, but perhaps renters are more likely to be on the margin with respect to college enrollment.

In Column 3, we interact layoff with family income (measured at age 15, prior to any layoff). We find little evidence of heterogeneity along the income dimension. While this result seems at odds with the notion of credit constraints, it is worth noting that given that a layoff has occurred, families must rely on their wealth, more than their income, to support higher education.

Since the consequence of layoff may depend in part on the generosity of unemployment benefit, in Column 4 of Table 4, we explore whether laid-off workers in states with more generous benefits are less impacted than workers laid-off in states with less generous benefits. Though statistically insignificant, the estimated coefficient is consistent with smaller impacts of layoff in more generous states. Naturally, states that are more generous in their unemployment benefits may be different along many other dimensions and we cannot rule out the possibility that state characteristics correlated with unemployment generosity drive this heterogeneity.

Column 5 examines whether families living in states with higher tuition levels appear to be more impacted by layoff compared to families living in states with lower tuition levels. To implement this analysis, we use IPEDS data to measure cross-state net price
differentials (as of 2008) and examine heterogeneity across states according to the median of net two-year college price in each state. Although the estimate is quite noisy, the coefficient suggests that the consequences of layoff are more severe in states with higher tuition levels. The main effect on college tuition is strongly positive, which most likely reflects the fact that demand for college is positively correlated with tuition (as opposed to being evidence of an upward sloping demand curve). While this clearly biases estimates of the impact of tuition on enrollment, if the bias is similar for parental layoff at 15-17 versus 21-23, the interaction term could potentially be unbiased. That said, we are cautious in placing much emphasis on the interaction result, because in addition to being imprecisely estimated, the coefficient may be biased if tuition levels are correlated with other factors that cause heterogeneity in the consequences of layoff.

While layoff reduces family resources on average, there exists substantial heterogeneity in the labor market consequences of layoff across families. To investigate whether this heterogeneity in income loss relates to the importance of layoff for enrollment, we consider whether families that experience more severe income losses have larger drops in college enrollment.

Column 6 shows the estimated impact of layoff for four groups of families. The omitted group is families that are re-employed and whose income losses are in the lowest tercile. The coefficient on parental layoff (-0.081) suggests that families that experience a low-level of income loss following layoff see a decline in college enrollment, but this estimate is statistically indistinguishable from zero. The next coefficient in Column 6
(-0.131) is the difference in enrollment rates between families who fail to find re-employment and the reference group. This estimate is statistically insignificant at conventional levels, though the p-value is 0.112. Summing the main effect and the interaction term shows that the total impact of layoff for families who fail to find re-employment is 21.2 percentage points and this effect is statistically different from zero (p-value 0.005).

Among families that find re-employment, those that experience the most severe income losses following layoff have more severe consequences of layoff compared to the reference group, though the estimate is imprecise. Combining the main effect and the interaction term implies that enrollment is reduced by 15.4 percentage points for these families and this estimate is statistically different from zero (p-value 0.026). Families that are in the middle tercile of income loss have slightly better outcomes than the reference group, though the difference is statistically insignificant. In total, the impact of layoff is negative for all terciles of income loss, but only statistically different from zero for families who experience the highest losses and families who remain unemployed at age 18.

While these results are suggestive of the importance of income loss, we are cautious in interpreting them because income loss is likely to be endogenous with respect to unobserved determinants of higher education enrollment. For example, parents who have children aged 15-17 and are dedicated to sending their children to college may alter their search intensity or lower their reservation wage.
6.4 Other Outcomes at Age 18

While our primary outcome of interest is higher education enrollment, there are several mechanisms through which parental layoff might impact child labor force outcomes as well. First, familial income effects can lead to an increase in labor supply for each family member. Second, as school enrollment falls, time constraints are relaxed, which may result in an increase in labor force participation. Since approximately 50% of full-time students work while in college, the relationship between work and school is not mechanical—particularly for students on the margin of college attendance (Planty et al., 2009). That said, if any students view school and work as mutually exclusive, we might expect to see an increase in labor force participation caused by the decrease in college enrollment.

In order to explore the impact of layoff on labor force outcomes, we use an estimation strategy that is identical to the main specification and simply change the dependent variable. The first column of Table 5 shows the estimated effect of familial layoff between ages 15-17 on labor force participation at age 18. Though imprecisely estimated, the point estimate implies that layoff leads to a 6.5 percentage point increase in labor force participation among children. The fact that 6.5 percentage points is considerably less than the 10.1 percentage point decrease in college enrollment is consistent with the notion that college and work are not mutually exclusive. In light of the large standard errors, we cannot rule out that the labor force response is as large as the college enrollment response, nor can we rule out a zero effect.
Mechanically, any increase in labor force participation must lead to either an increase in working or an increase in unemployment. Columns (2) and (3) of Table 5 show the impact of layoff on the probability of employment and unemployment. Though statistically insignificant, the point estimates suggest that a roughly similar fraction of children become unemployed as become employed.

In addition to impacting labor force participation, parental layoff might lead to altered living arrangements for several reasons. First, since many students move to attend college, the decrease in enrollment might lead to an increase in the fraction of students who live at home. More generally, to the extent that familial resources are shared, living at home saves the family money and is therefore particularly attractive following a layoff. Alternatively, children might be more likely to leave home following a layoff if they are viewed as a drain on familial resources or if tension caused by the layoff creates a hostile home environment (Charles & Stephens, 2004). Column (4) of Table 5 shows that layoff increases the probability of living at home by 5.2 percentage points. While this coefficient is not statistically significant at conventional levels, the p-value is 0.101.

6.5 Long-Run Impacts

Our paper focuses on short-run enrollment effects, which we view as the appropriate test for the existence of credit constraints. It is worth exploring, however, whether these short-run enrollment effects persist. In addition to considering the probability that an
individual enrolls by age 30, we also examine the long-run impact of layoff on years of completed education and income at ages 25-30.\textsuperscript{5}

Unlike our main specification, our estimates of the long-run impact do not capture the causal impact of a layoff because both treatment and control experience a layoff prior to age 30. Since our “control” group is defined as experiencing a parental layoff at ages 21-23, our estimates will be biased towards zero if the decision to enroll at ages 22-29 is impacted by these layoffs. In other words, when looking at long-term outcomes, both our treatment and control group experience a form of treatment—just at different times. That said, given that most children are no longer living at home by age 21-23, it is plausible that the layoff at 15-17 will be much more consequential than the layoff at 21-23. In particular, children who plan to enroll for the first time between ages 23 and 29 are less likely to rely on their parents for financial support.

Columns (5), (6) and (7) of Table 5 show the estimated impact of layoff on long-run outcomes.\textsuperscript{6} Though one might expect to find smaller impacts on enrollment at age 30, we

\textsuperscript{5} We use a range of ages to measure income because only one-third of our sample has income measured at age 30 since much of our sample is born after 1981 and therefore is not yet 30 at the last survey. Also, sample attrition and missing data prevents measuring income at age 30 for some participants who are born before 1981. We use income from the oldest reported year, and in order to avoid comparing income measured at 25 to income measured at age 30, we include age at survey dummies when examining long-run outcomes.

\textsuperscript{6} While these long-run outcomes are interesting to explore, looking at outcomes at age 30 requires dropping observations born in later years and those that leave the sample. As such, the long-run analyses are estimated using smaller samples than the analyses of outcomes measured at 18. The one exception is that our measure of enrollment by age 30 does not require observing enrollment status at 30, since a person who is observed enrolling at a younger age is considered enrolled by age 30, even if her education level cannot be observed at age 30.
find surprisingly similar estimates. A layoff at ages 15-17 lowers the probability of enrollment by age 30 by 9.8 percentage points. This finding reflects a combination of factors. First, it is possible that students who are unable to enroll at age 18 end up not enrolling at all. Second, it is possible that some children delay enrollment, but these students are balanced by other children who planned to enroll at age 19 or later, and fail to enroll at all as a result of the layoff at age 15-17.

In Column (6), we show that children whose parents are laid off at age 15-17 end up with 0.74 fewer years of education compared to children whose parents are laid off at ages 21-23. Column (7) shows that the timing of parental layoff also appears to correlate with income at age 30, but this effect is imprecisely estimated. Some might be tempted to divide the income coefficient by the years of schooling coefficient to estimate the returns to education. We view this interpretation as problematic since parental layoff at ages 15-17 is a poor instrument for completed education since it can plausibly have a direct effect on income at age 30.

7. Discussion and Conclusion

This paper adds to the existing literature on the impacts of parental job loss by examining the relationship between the timing of parental layoff and higher education enrollment. While our empirical strategy uses the timing of layoff to identify a credible counterfactual, it is important to note that our estimates speak to the causal impact of layoff—not the relative value of having one’s parents be laid off at different ages. Given
that the benefits of college attendance are well established, it is likely that the children who experience parental layoff at age 21-23 (as opposed to 15-17) benefit from the increase in college enrollment; however, whether their families benefit on net is an open question.

Our finding that layoff impacts higher education enrollment could feasibly be the result of a variety of mechanisms. First, if families are credit constrained, poorly timed reductions in family resources may make investments in college enrollment infeasible. Alternatively, layoff may lower high school performance or increase family instability, making it more difficult for youth to enroll in college. Finally, to the extent that college acts as a consumption good and families experience a reduction in permanent income, income effects will cause optimizing families to reduce their choice of education.

Distinguishing between these possibilities is an important area for future work because the correct policy aimed at reducing the negative impacts of layoff depends strongly on the underlying mechanism. If laid-off families are credit constrained, expanding loan opportunities for those claiming UI benefits could effectively mitigate the intergenerational consequences of layoff. If parental layoff impacts enrollment primarily through other channels, then financial aid policy alone may be insufficient. In any case, it is important to know that the children of laid-off workers are substantially impacted by layoff, because identifying a set of children whose college enrollment is vulnerable provides an opportunity for intervention.
Given that the difference between the control and treatment group in our analysis is simply a matter of the timing of layoff, one would hope that this small difference would not generate long-run consequences. While our identification strategy is best suited to evaluating the short-term impact of layoff, the fact that we find that long-run college enrollment depends strongly on whether an individual’s parents were laid off at 15-17 versus 21-23 suggests that the timing of income is important.

Without fully understanding the mechanism through which layoff impacts enrollment, it is difficult to prescribe precise policies. That said, the importance of layoff timing suggests that policies aimed at helping families smooth job transitions have the potential to mitigate not only the consequences of layoff for individual workers, but also for the future generations. Since policies such as unemployment insurance are costly and create the possibility of moral hazard, it is beyond the scope of this paper to evaluate the net benefits of these policies. In any case, policy makers interested in evaluating the total impact of UI benefits or other job transition programs should consider the potential for substantial intergenerational benefits.

REFERENCES


