How Financial Aid Affects Persistence

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The Pell Grant program is the largest means-tested financial assistance available to postsecondary students across the United States. Students from all types of degree granting post-secondary institutions can apply for Pell grants. In 2000-01, The federal government awarded almost $8 billion in Pell grants among more than 5 million students, roughly one-third of all colleges students, (DOE 2002a), and President Bush's 2003 budget allocates over $10.9 billion dollars for an estimated 4.5 million Pell grant recipients (DOE 2002b). Yet despite this continued expansion of Pell, researchers have only limited evidence on the effects of these grants.

Most Pell-related research focuses on the effects of Pell Grants on enrollment decisions, specifically focusing on initial enrollment and choice amongst colleges. The general consensus in this research is that Pell grants have not raised enrollment rates among low-income students and minorities although they have likely affected college choice (Kane 1999, Ehrenberg and Sherman 1984, Leslie and Brinkman 1987). Recent work by Seftor and Turner (2001), however, suggests that Pell grants may have improved access for "non-traditional" students. The conclusion that Pell grants have been largely ineffective in improving access does not necessarily mean that students are insensitive to price. For example, MacPherson and Shapiro (1999) provide estimates of the elasticity of demand for higher education, and Dynarski (2002) shows evidence that enrollment and graduation rates for beneficiaries of the Social Security Survivors Benefit program dropped when the generosity of the benefits declined.

While educational researchers have often studied Pell grants and their effects on these enrollment decisions, there is surprisingly little research on how Pell grants affect student outcomes in college. Regardless of whether Pell grants affect initial enrollment patterns, Pell grants may independently affect student retention. Moreover, Pell-eligible students are more likely to be on the margin of "stopping-out."[1] At Ohio 4-year colleges in 1999-2000, 18 percent of full-time freshmen who were *not* eligible for Pell Grants withdrew from college by the next year while 28 percent of students who were eligible for a Pell grant did not enroll the following year. It is an open question whether Pell grants and other need-based aid programs affect these margins.

This paper focuses on the relationship between need-based aid and student retention. Using unique student data from Ohio, this paper tests whether Pell Grants and other need-based financial aid programs affect student retention.

There are a number of reasons why so little research has investigated the effects of Pell grants on outcomes. One reason is that researchers have difficulty distinguishing between the effects of family characteristics and Pell grants. Pell grants are a means-tested program. Comparisons between Pell recipients and non-Pell recipients (e.g. Wei and Carroll 2002) may be difficult to interpret since Pell recipients are poorer and may be more likely to drop-out even in the absence of need-based aid. To correct for such bias, researchers must sufficiently control for family characteristics.

Additionally, identifying the effects of Pell grants is difficult since much of the variation in the size of students' Pell grants is correlated with students' college enrollment decisions. For example, college choice and the size of a students' Pell grant are directly connected. Students who attend more expensive (and often higher quality) schools are eligible for larger Pell grants than students at other colleges or universities. Moreover, Pell grants are more generous for full-time rather than part-time students.

[1] "Stopping-out" refers to students who withdraw from school after their first year. These students are not "drop-outs" since many of these students do not leave school permanently and their undergraduate credit

Even in the absence of Pell grants, students who benefit most from college are more likely to attend more expensive schools and to attend full-time than other students. However, since Pell grants awards are systematically higher for these same students, it may be difficult to identify the effects of the Pell grant separate from college enrollment effects. To avoid this bias, researchers must exploit variation in Pell grants that is independent of college choice (e.g. discontinuities in the Pell formula).

A final reason that researchers have been unable to identify the effects of Pell grants on outcomes is the absence of accurate data, in particular, the absence of accurate persistence and detailed financial data. Some researchers have measured persistence at a particular university; however, in these data, researchers cannot distinguish between a student who transferred to another school and one who withdraws from college. Other survey-based data rely on students' self-reports of their college experience. However, students may not recall or do not wish to report small periods of time when they withdrew from college.

Financial data is equally as difficult to obtain. Most of the students who receive Pell grants do not attend elite, expensive institutions, nor do they have substantial family support. Pell recipients (and much of the variation in their awards) typically come from less expensive colleges and their family contributions are much smaller. Moreover, the variation in Pell grants is typically small. Exact financial data are necessary both to identify the small variations in Pell grants and to employ creative identification strategies. Survey data (e.g. High School and Beyond, National Educational Longitudinal Survey) do not offer the level of detail necessary to identify accurately the level of students' Pell grants.

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hours do not "expire." I use these terms interchangeably throughout the paper.

To examine the effects of Pell, this paper presents evidence from data gathered by the Ohio Board of Regents (OBR). These data do not have the shortcomings of other datasets and offer a level of detail on both persistence and financial variables that is not available in other data. Since 1998, OBR has collected comprehensive data on college enrollment in Ohio's public 2- and 4-year colleges. As a result, the OBR data tracks students within and across schools. With the data, researchers can distinguish between students who withdraw from school and students who transfer to other Ohio schools. Moreover, through collaborative agreements, OBR has expanded the data to include students' ACT scores and data from the Free Application for Federal Student Aid (FAFSA). The FAFSA data are the exact data used by institutions to determine the amount of students' Pell grant eligibility.

The level of detail in the financial data also facilitates the use of statistical tools that are impractical using other data. In particular, the level of detail allows the researcher to identify small discontinuities in the Pell grant formula. These discontinuities may be exploited to identify the effects of the voucher. While this paper may not completely resolve biases from college choice/enrollment or family background, the discontinuity analysis may be the best available method for dealing with such biases.

The paper presents evidence using both panel and cross-sectional variation. The panel specifications suggest that need-based financial aid did not affect students' stop-out behavior. However, complicating these results is the fact that most students who withdraw from college do not file their FAFSA and consequently the change in their Pell grant is unobserved. I simulate the changes in Pell grants for these students. The results suggest that need-based aid likely reduced stop-out rates. The paper also presents evidence relying on cross-sectional variation. The paper estimates the effects of Pell grants close to existing discontinuities in family size and the number of students attending college. The results based on discontinuity approaches are not robust to alternative specifications and are sensitive in both sign and magnitude to changes in the sample.

Section 1 of this paper presents a simple economic model of student persistence under uncertainty. Section 2 of the paper explains the OBR data in greater detail. Section 3 of the paper presents the empirical strategies and results. Section 4 discusses the results and concludes.

I. Economic Model

Economists often model educational attainment as investment in human capital. Even basic economics classes teach that students will choose an education level that maximizes the expected present discounted value (PDV) of future wage payments less the expected PDV of educational costs. There have been a number of permutations to this model – factoring in scholarship aid, allowing the returns to education to vary, and showing how predicted education levels vary with expectations (Clotfelter 1998). This paper investigates the relationship between financial aid and outcomes. Rather than use a traditional human capital model, the paper models students' dropout behavior using a multi-stage investment model.

Multi-stage investment models are particularly useful in cases where the agent must reevaluate the project after an initial period of time. For example, Myers and Majd (1984) investigate the case where a construction firm can abandon a new project at any point. They find an optimal abandonment rule under which the firm may elect to start the project but discontinue it at a later date. Dixit and Pindyck (1994) review other examples of multi-stage investments.

The phenomenon of interest – students' stop-out behavior – is similar to these multi-stage investments. In the initial period, students must decide whether or not to attend the first year of college. After completion of the first year, students must then reevaluate whether or not to complete the next year. About 20 percent of first-time freshmen withdraw from 4-year colleges after the first year.

To formalize the model, let person i's wage at time t (w_{it}) be modeled as a function of years of college (s_t) and ability (a_i) which is not perfectly known to the student. Let the cost of education at time t (c_t) be the difference between announced tuition (T_t) and financial aid. Financial aid contains two components: the need-based component is a function of initial wealth (I_0) and the number of children attending college at time t (n_t); the merit-based component is a function of perceived ability at time t. Let $E_t[]$ denote the expectation operator conditional on information at time t.

$$w_{it}=f(s_{it,},a_i) \tag{1}$$

$$c_t= T_t - g(I_0,n_t) - h(a_i) \tag{2}$$

A student will attend a 1st year of college if the expected value of increased lifetime earnings for that year exceeds the cost of attending college (including foregone earnings).

$$E_0(\sum_{t=2}^{T} R^{t-1}[f(s_{it}=1,a_i)-f(s_{it}=0,a_i)]) > E_0(f(s_{i1}=0,a_i)+T_1-g(I_0,n_1)-h(a_i)) \tag{3}$$

At the start of the first year, a student will indicate an intention to attend a second year as well so long as

$$E_0\left(\sum_{t=3}^{T} R^{t-2}[f(s_{it}=2,a_i)-f(s_{it}=1,a_i)]\right) > E_0(f(s_{i2}=1,a_i)+T_2-g(I_0,n_2)-h(a_i)) \qquad (4)$$

We could solve the decision rules for the maximum tuition level that a student would be willing to pay. For simplicity, let's assume that tuition is fully known one year in advance.

$$E_0(T_1^*)=T_1^*=E_0\left(g(I_0,n_1)+h(a_i)-f(s_{i1}=0,a_i)+\sum_{t=2}^{T} R^{t-1}[f(s_{it}=1,a_i)-f(s_{it}=0,a_i)]\right) \quad (5)$$

and

$$E_0(T_2^*)=E_0\left(g(I_0,n_2)+h(a_i)-f(s_{i2}=1,a_i)+\sum_{t=3}^{T} R^{t-2}[f(s_{it}=2,a_i)-f(s_{it}=1,a_i)]\right) \quad (6)$$

These tuition levels are likely the formulae that students use to make any decisions about the second year of school that must be made during the first year. For example, a student wanting to transfer to another university must file that application during the first year. Also, students who want financial aid in their second year must file applications during their first year. With these types of decisions in mind, there are a few of insights that come from comparing these two tuition values:

1. For a given level of ability, if the returns to schooling are linear (or even concave) in schooling and scholarship aid does not change, then the maximum tuition that a student will pay falls over time. Hence, many students may rationally choose to get only one year of school.

2. Even if the returns to schooling are convex and scholarship aid does not change, then the maximum tuition a student is willing to pay may still decrease over time leading to more planned attrition.[2]

3. Even expected changes in financial aid can alter the maximum that students would be willing to pay leading to students to plan on withdrawing or transferring.

4. Since students must apply for 2nd year financial aid during their first year, they will do so only if they perceive that their benefits exceed costs in both periods.[3]

There are also a number of decisions about the second year that can be made after the first year – for example, the decision to withdraw from college altogether. Students make these decisions after gaining another year of information by which to base their decisions. The student will choose to attend another year if the expected value of the increase in lifetime earnings for the 2nd year exceeds the cost of attending college that year (including foregone earnings).

$$E_1\Big(\sum_{t=3}^{T} R^{t-2}[f(s_{it}=2,a_i)-f(s_{it}=1,a_i)]\Big) > E_1\big(f(s_{i2}=1,a_i)+T_2-g(I_0,n_2)-h(a_i)\big) \qquad (7)$$

We could rewrite this decision rule solving for the maximum tuition levels that the student would be willing to pay in order to actually attend a given year of college:

$$T_2^* = E_1\Big(g(I_0,n_2)+h(a_i)-f(s_{i2}=1,a_i)+\sum_{t=3}^{T} R^{t-2}[f(s_{it}=2,a_i)-f(s_{it}=1,a_i)]\Big) \qquad (8)$$

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2 Holding scholarship aid constant and as T→∞, the maximum tuition rises only if the following inequality is satisfied:

$$(1-r)E_0[f(s_{it}=2,a_i)-f(s_{it}=1,a_i)] > E_0[f(s_{it}=1,a_i)-f(s_{it}=0,a_i)].$$

If in the extreme case, there is a "sheepskin" effect of a degree (i.e. returns only to a 2- or 4-year degree), then the inequality is always satisfied. Typical models of sequential investment show that the willingness to pay increases over time. The key difference is the usability of capital. Students may be able to drop out of colleges and use a year of college to their best interest.

Notice that the difference between equation (8) and (6) is the information set. Students have a chance to update their expectations with information from their first year of school. As the model stands, the updating comes in terms of ability. Similar to the model in Clotfelter (1998), students discover their ability by attending college. Knowing the ability then changes the willingness to pay.

We could have also changed this model by introducing uncertainty in the financial aid formula. Unexpected changes in financial aid might lower the maximum tuition price that students might be willing to pay. For example, if a students' expected financial aid offer falls, the maximum that a student would be willing to pay declines. The student may wish to transfer to a cheaper school or drop-out altogether.

A simple insight of the model is that changes in financial aid matter. Previous work on the effects of financial aid has looked at both changes and levels. Recent work by Wetzel, O'Toole, and Peterson (1999) look at changes in financial aid for students at Virginia Commonwealth University. They find that increases in need-based financial aid likely improved student retention. Other work by Singell (2001) looks at the effects of the level of financial aid in the first year. He finds that the higher students' levels of need-based financial aid, the more likely the student is to graduate.

While the model in this paper suggests that decisions about enrollment in the second year rely on financial aid changes rather than levels, there may be reasons that the level of financial aid in the first year matters. If the level of financial aid in the first year creates some inertia or helps to shape expectations about the financial aid offer in the second period, the level may affect the student in the next year. One example of this type

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[3] There may be a small group who apply for financial aid even though they expect not to attend the second year. There is an option value to applying for financial aid since ability is not known perfectly.

of effect is the application for 2nd year financial aid. The higher a students' Pell grant in the first year, the more likely the student will apply for a 2nd year award. If students expect to get a low 2nd year award, they may never even apply for financial aid.

The level of financial aid may even have deleterious consequences on the student. The model implicitly assumes that financial aid does not change students' behavior in other ways. But for example, if a student receives financial aid, he or she may be more "detached" from college. The student may not fully engage and take college seriously since his/her money is not "on the line." This student may perform worse. The model may capture some of this through the "updates" on students' abilities. The empirical results will investigate this hypothesis more fully.

Although they are outside of the scope of this paper, there are other outcomes in which we might be interested that are related to either the level or change in financial aid. For example, we might be interested in how financial aid affects the number of credits that a student successfully attempts. Students without financial aid may be reluctant to take loans and may spend more time working on the side. On the other hand, students without financial aid may want to "cram" in more credits per semester to try to reduce the number of semesters they have to attend (and as a result the total cost of college). We might also be interested in knowing how GPA's vary with financial aid. In particular, if the level of financial aid affects hours attempted, completed or grade point averages, it might also affect students' perception of their abilities and, in the context of the model above, affect their likelihood of completing college.

II. Data

The data for this project come from the Ohio Board of Regents (OBR). Through a collaborative agreement with the OBR, the OBR has allowed me to access anonymous student data from Ohio's public institutions. The data are provided by the respective institutions to the OBR and include information on student demographics, enrollment, credit hours completed, and grade point averages.

OBR has collaborative arrangements with other agencies that allow them to expand the data. For example, OBR links the student records to ACT and SAT records. Most Ohio students take the ACT exam, and the ACT records include the highest test score of the student and the most recent responses to the ACT survey (which includes student-reported data on high school performance). OBR also links students to their respective FAFSA. The FAFSA data include detailed information about the finances of both students and their families. From the FAFSA, the variable of most interest is the "estimated family contribution" that colleges use to award grants based on financial need.

One important limitation of the data is that they only include information about need-based financial aid. From FAFSA data, we know students' eligibility for federal grants and loans. We also know students' eligibility for Ohio's Instructional Grant program, a state-run need-based financial aid award. The data do not include information about merit-based financial aid. Ohio institutions are reluctant to divulge merit-based awards since these rewards are central to their recruitment strategies. While I do not observe merit aid, I observe students' GPA's once in college, their ACT scores, and their (self-reported) high school GPA's. If these variables adequately control for student ability and if colleges determine need- and merit-based awards separately, then not knowing students' merit-based awards should not affect the estimated results.

Another limitation of the data is that they only include students attending Ohio public universities. Students from Ohio that attend universities in other states, including the nation's elite schools, and students that attend private schools in Ohio are excluded from the sample.[4] These exclusions are both a weakness and a strength of these data. Excluding elite students makes it so the results may not be generalizeable to all college students; however, excluding elite students gives us the opportunity to describe how financial aid affects students at non-elite schools. These non-elite schools educate the majority of college students and may be places where financial constraints are more binding.

I focus entirely on the incoming freshman class in 1999-2000 school year. These are the first students for whom FAFSA data are available through the OBR. I include students who enrolled in any college, including community colleges, for the first time in 1999, and I track these students through the 2000-2001 school year.

Table 1 provides summary statistics for the sample. At 4-year institutions, about 10 percent of incoming full-time freshman are from other states, and students are much less likely to be commuter students than at 2-year colleges. At 2-year colleges, which include local and state-run community colleges and technical colleges, about 2 percent of all students live on campus. Similar to other national surveys, the average age of first time freshman at 2-year colleges is considerably higher than at 4-year colleges, and

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[4] Ohio State University is the top ranked public university in Ohio. In the 2002 version of US News and World Reports' college rankings, it ranks in the second tier (53rd-131st) of national universities with doctoral programs. Other high ranking institutions in Ohio (e.g. Oberlin) are private colleges.

students complete fewer semester hours in their first year (13 at 4-year colleges as compared to 11 at 2-year colleges.

Seventy-five percent of incoming freshman at Ohio's 4-year colleges took the ACT exam while only 45 percent of students at 2-year colleges did as well. The 4-year college students performed better than the 2-year college students. Throughout the paper, I will at times restrict the sample to students who took the ACT exam. Not only do I know these students test scores, but I also have additional (self-reported) data on these students' high school experiences.

Throughout the paper, I will also restrict the sample at times to those students who filed a FAFSA in both Fall 1999 and Fall 2000. About 65 percent of 4-year students and 46 percent of 2-year students submitted FAFSA's in 1999; however, only 49 percent and 40 percent respectively filed FAFSA's in 2000. As explained below, not observing FAFSA data for many applicants leads to substantial biases in the results using panel identification. The average, uncovered financial need is small across all students, but conditional on it being positive, the uncovered financial need is slightly greater than \$1400 for students at 4-year schools.

Table 2 shows some basic least-squares regressions of student stop-out behavior on the level of students' financial aid. These regressions are not meant to show the "causal" effect of Pell grants, but rather to demonstrate associations between the stop-out behavior, financial aid awards and other covariates. These regressions are also useful in understanding the types of biases present in the data. Comparing the various specifications will help identify important biases.

Column 1 shows a regression of whether a student drops out or not regressed on the student's financial aid award. The regression includes fixed effects that control for the school that the student attends. The estimated coefficient is positive and significant suggesting that larger awards are positively associated with drop-out behavior. As mentioned before, however, these coefficients are significantly biased for a number of reasons. For example, when we include controls for an individual's socio-economic background and personal characteristics in Columns 2 and 3, the estimated relationship drops significantly. The estimated relationship drops dramatically (from .033 to .006) and is still significant in Column 2. In Column 3, I focus only on students who took the ACT exam, including controls for a student's high school performance and entrance exam scores. The estimated coefficient is similar to Column 2 but is no longer significant. The other rows in Columns 2 and 3 suggest that wealthier students are less likely to stop-out; out-of-state students are more likely to drop-out; older students and men are more likely to withdraw; and students living on campus and students who took the ACT (and performed well on it) are less likely to stop-out.

In Column 4, I add controls for students' grades during their freshman years of school. As previously mentioned, Pell may have had a negative effect since students with Pell may have had less of a financial commitment to schooling and may not have worked as hard. Including grade point average should control for these students and may further weaken the estimated relationship between financial aid and stop-out behavior. As shown in Table 2, the estimated relationship is smaller than in Columns 1-3. The result is also not statistically different than zero. In Column 5, I estimate a similar regression for students who took the ACT exam. Again, the estimated relationship is indistinguishable from zero.

In Column 6, I estimate the relationship controlling for personal and family characteristics and grades during a student's first year. I exclude the fixed effects for students' campuses of attendance. These fixed effects control for differences in quality, price, and other unobservable campus characteristics (e.g. the strength of a campuses freshman intervention programs). These fixed effects control for the fact that students attending lower quality schools (who also receive smaller financial aid awards since tuition is smaller) are more likely to withdraw than students attending better schools (who receive higher financial aid awards for similar reasons). Without these fixed effects, we would expect the estimated relationship between financial aid and student stop-out behavior to be even smaller and maybe even negative. This is exactly what Column 6 of Table 2 shows. The estimated relationship suggests that higher financial aid awards are negatively associated with student stop-out behavior.

III. Empirical model & Results

There are three sources of variation that economists can use to identify the effects of Pell grants: time series, panel, and cross-sectional. In this section, I discuss the feasibility of each of these identification strategies using the Ohio Board of Regents data. I also present the basic empirical results for each identification strategy.

A. Time Series Identification

One way to identify the effects of Pell grants is to compare changes in students' outcomes after systematic changes in Pell grants occur. For example, the Pell grant program began in 1973. Previous work by Kane (1996) compares low-income student enrollment rates before and after the Pell program was established. Kane finds that college rates grew about 2.6 percentage points slower for low-income students than other groups, suggesting that the Pell grant had little effect. Other systematic changes in Pell grant formulae are described in Mortenson (1988). For the study at hand, I am presenting evidence for a single cohort, so time-series variation will not be useful in identifying the effects of Pell.

B. Panel Identification

Another way to identify the effects of Pell grants is to look at changes in students' Pell grants over time. While this seems like a promising strategy since the OBR data contain two years of data for a single cohort, there are a number of reasons that this strategy might be limited.

To see the limitations and possibilities of this identification strategy, we need to understand how variations over time are generated for a single individual. There are really three basic reasons that a students' Pell grant would change from one year to the next. First, the generosity of the Pell grant may change. This could be the result of systematic changes in the Pell formula or by a change in college tuition. Such changes are likely to be exogenous, and if they generate enough variation, they may help researchers to accurately identify the effects of Pell grants. Unfortunately, there is little variation over time in the period of time that the OBR data are available. From the 1999-2000 school year to the 2000-2001 school year, the maximum Pell grant increased from

$3125 to $3300, a 5.6% increase. Over the same time, tuition at Ohio schools increased by 5% across the board (OBR 2001).

Another source of variation comes from changes in students' college choices. Students may transfer to another school after the first year. The corresponding change in tuition will generate variation in students' Pell grants. Unfortunately, this source of variation does not help identify the effects of Pell grants. Students who transfer may have differences in ability than those students who do not transfer. For example, a student with high ability may transfer from a two- to a four-year college to gain access to more opportunities. Moreover, the size of students Pell grants may affect transfer behavior making it even more difficult to identify the effects of Pell grants using variation caused by student transfer behavior. Thus, changes in Pell grants resulting from transfer decisions will not generate variation in Pell grants that can be legitimately used to identify the effect of Pell grants alone.

A final reason that students' Pell grants may change is due to changes in students' circumstances. Some changes may be legitimate sources of variation. For example, a family of four with one child in college may have a second child come of college age causing the existing college student's Pell grant to increase. Similarly, a change in family size (e.g. birth of another child or separation) may increase a student's Pell grant. Even the natural aging of parents should increase students' Pell grants although only slightly. However, there are other changes in family circumstances which may not be legitimate sources of variation. For example, changes in income due to unemployment or health shocks may reduce family income and consequently increase students' Pell grants from year to year. These sources of variation may also affect the likelihood that a student persists in college.

Table 3 contains some basic results using panel identification. Columns 1 and 2 show the basic panel results. The dependent variable in these regressions is the likelihood that a student drops out after his or her first year. The key regressor is the increase in a student's Pell grant from one year to the next measured in thousands of dollars. For students who withdrew or transfer, I impute the Pell grant students would have received if they remained at the same institution as their initial enrollment. Column 1 shows the results without covariates. Column 2 shows the results with additional covariates for gender, age, campus living conditions, whether the student took the ACT exam, and grade point average in the students' first years. The regressions also include fixed effects for school of attendance. The only students included in these regressions are the students who filed FAFSA forms for both the 1999-2000 and 2000-2001 school years.

As Column 1 shows, students whose Pell grants increase are more likely to dropout. Without covariates, the coefficient suggests that a $1000 increase in a student's Pell grants leads to a 1.4 percentage point increase in the likelihood that the student withdraws. With covariates, the estimated coefficient implies that a $1000 increase in a student's Pell grant corresponds to a 1.0 percentage point increase in the likelihood that students withdraw. These effects seem counterintuitive. They seem to suggest that students with increases in their Pell grants are more likely to withdraw. However, it is not as if these students are reducing their effort as a result of the increase in the Pell grant. The students who withdraw never actually receive the extra money. They leave school before the school awards it.

These results are likely the result of shocks to students' families. Students with the largest increases in their Pell grants are likely to be the students who had the largest shocks to the family finances. For example, a parent may have lost a job or had significant losses in wages due to illness. Because of the change in students' circumstances these students may be more likely to withdraw even in the absence of changes in Pell grants.

For panel identification strategies to be successful, variation in the Pell grants must come from sources that are exogenous from changes in students' stop-out behavior. As mentioned above, the most legitimate changes come from changes in the Pell formula, changes in tuition, and changes in family size or sibling attendance. I can use changes from these "legitimate" sources as instruments for actual changes in financial aid.

Constructing the instrument from changes in the Pell formula and tuition is straightforward. I simply estimate what students Pell grants would have been during the 2000-2001 school year assuming that their financial and family information is unchanged from the 1999-2000 school year. The revised 2000-01 Pell grant does not include variation from changes in students' circumstances. It only includes variation arising from changes in the Pell grant formula and tuition.

Column 3 of Table 3 shows instrumental variable estimates when changes in the number of siblings in college and changes in the Pell grant arising from tuition and the Pell grant formula are used as instruments for actual changes in Pell grants.[5] The sample in Column 3 is the set of students who filed FAFSA's in both 1999-2000 and the 2000-2001 school year. The estimated coefficient on the change in the Pell grant is now 0.050 with a standard error of 0.045. Although the coefficient is still positive, it is statistically indistinguishable from zero. The estimates suggest that need-based financial aid may not have affected college persistence decisions.

One difficulty in the panel identification strategies shown thus far is that it includes only students for whom financial data are available for both years. Unfortunately, about 1/3 of students filing FAFSA's in the 1999-2000 school year did not do so in the next year. This group includes 12,143 students and I will hereafter refer to them as the "non-filers." These non-filers are not a random subset of all students. These non-filers include 2/3 of all students who withdrew from college after the 1999-2000 school year. For these individuals, I am missing financial data and information about changes in their siblings' college attendance for the 2000-2001 school year.

In Column 3 of Table 3, I presented IV estimates where I used students' financial information from 1999-2000 to estimate what students' Pell grants would have been with both tuition changes and changes in the financial need formulae during the 2000-2001 school year. I can construct a similar estimate of students' need-based financial aid awards for the non-filers. In Column 4 of Table 3, I regress student drop-out behavior on the simulated change in the value of students' Pell grants. In Column 4, I report the reduced form for all students, including those who filed a FAFSA only in their first year. In Column 5, I report the reduced form when I restrict the sample to those students who filed FAFSA's in both years.

As Column 4 shows, the estimated effect of this simulated change is negative and significant. A $1000 increase in a student's financial aid award corresponds to a 9 percent reduction in the likelihood that the student withdraws from school. In Column 5, I restrict the sample only to those students who filed FAFSA's in both years. Now the

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[5] Note that the first stage regression appears in Appendix Table 1.

estimate is positive and significant. A $1000 increase in a student's financial aid corresponds to a 2 percentage point increase in the likelihood that a student withdraws. I include the estimate of Column 5 to provide some hint of what the bias may be from excluding the "non-filers" in the previous columns. When we include the "non-filers," we get significant, negative relationships between increases in students' Pell grants and the likelihood that students drop out; however, when these students are omitted, the estimates are consistently positive and often significant.

One might be able to further refine the estimates of students' Pell grants in the cases where data are missing by using information about students' siblings. If ages or graduation dates were known or could be approximated, I could include this information in the estimation of what students' Pell grants would have been in the 2000-2001 school year. Unfortunately, little information is available about students' siblings for the non-filers.[6]

What conclusion should be drawn from the panel identification specifications? First, panel identification has only limited power to actually identify the effects of Pell grants. Much of the variation created over time in a student's Pell grant comes from sources which may also affect the probability that the student withdraws from school. It would be inappropriate to use this type of variation to identify the effects of Pell. Second, when instruments are used to isolate "legitimate" sources of variation, the estimated effect is indistinguishable from zero. Finally, the fact that many students,

[6] Some information about the family (parental age, family size in 1999-2000, number of children in college in 1999-2000, parental marital status) may help predict changes in the number of children attending college; however, their predictive power is limited. When I model changes in sibling attendance on these variables, I get a very low R-squared. After rounding the predicted values to the nearest integer, the specification predicts that 0.05% of students who filed FAFSA's in both periods would have had a change in the number of siblings attending college. In reality, 20.5% of students had a change in the number of siblings attending college.

especially those who plan to withdraw from school, do not file FAFSA's in both years limits makes it difficult to estimate the effect of financial aid. When we simulate data for these people, we find estimates suggesting that increases in financial aid reduce the likelihood that students withdraw from college.

C. Cross Sectional Identification

One might also identify the effects of need-based financial aid by comparing the need-based awards of different students at a single moment in time. There are a number of reasons that students may have different need-based awards. Students may differ from each other in terms of personal income and assets, family income and assets, family size, parental age, college of attendance, and enrollment status (full versus part time). All of these differences will lead to differences in students' need-based financial aid. Much of this variation will not be helpful in identifying the effects of Pell grants. These sources of variation will also likely affect students' drop-out behavior independent of need-based awards.

However, there is some variation across individuals that might be useful. In particular, differences in family size and the number of children in college may facilitate identification in a cross-section. The Pell grant formula contains a number of discontinuities, the largest of which is based on the number of students attending college and the family size.

Table 4 shows the changes in Pell Grants that accompany changes in family size. The table shows three different schedules linking family size and the number of children attending college.[7] Each schedule corresponds to a different income level ($40,000, $50,000, or $60,000). For example, among the families with $50,000 in income, a family of 2 with one in college would receive a Pell grant of $975. If both members of the family attended college, then the Pell grants would be $1775 per person. Differences in family size and the number of children attending college can lead to systematic differences in students' Pell grants. These systematic differences create discontinuities that can be exploited to estimate the effect of Pell grants on students.

Assuming that the differences between the number of children in college is unrelated to a student's success in college, comparisons can be made between similar size families with different numbers of children in college. Similarly, assuming that the differences between the overall size of a family is unrelated to a student's success in college, comparisons can be made between families of different sizes who have the same number of children in college. Each of these different comparisons reflects a different discontinuity that can be used to identify the effects of Pell grants on students. However, as Table 4 shows, there is heterogeneity in income (and thus Pell grant) within a given family size and a given number of children in college. For discontinuity analysis to work, the families on either side of the discontinuity should be similar except for the discontinuity. As a result, when making comparisons across groups, we need to stratify the groups so that comparisons are made across relatively homogeneous groups (e.g. people with similar income and assets).

Intuitively, the easiest way to estimate the effect of the Pell grant while taking advantage of this discontinuity is to use a Wald estimator (Wald 1940). To estimate the

[7] The computations assume that the family has no assets and that the students do not contribute to the family's estimated family contribution.

Wald estimator, one must first isolate two groups that are fairly homogeneous. Across the groups, the Wald estimator is estimated by taking the ratio of the differences across groups of the dependent variable (stop-out behavior) and the independent variable (size of the Pell grant). For example, suppose we could identify all people who have low income and few assets and came from two person families. Some of these families have both members attending college while others have only one. Assuming that the number attending college is uncorrelated with an individual's success in college, we could estimate a Wald estimator across these groups. Let y_i be the average withdrawal rate for group i. Group i takes on a value of 1 for the group of students in two person families with one in college and 2 for the group of students in two person families with two in college. Let x_i be the average Pell grant for group i. The Wald estimator between these groups would then be

$$\beta_{Wald} = \frac{y_2 - y_1}{x_2 - x_1}.$$

The denominator should be the expected change in the Pell grant as a result of this discontinuity within this income-asset group. The numerator would be the difference in stop-out rates between these groups.

After computing the first Wald statistic, we could then create a Wald estimator between each income-asset grouping with the set of 2 person families. If we had 10 income-asset groupings, we would have 10 Wald estimators. These Wald estimators can be combined by taking a weighted-average of the estimators (weighted by the number of observations in each group 2). We could similarly create Wald estimators across adjacent groupings of family size and the number of children attending college. For example, among 3 person families, we could estimate a Wald statistic between families with one in college and families with two in college. We could also estimate a Wald statistic between 3 person families with two in college and 3 person families with three in college. Similarly, among families with one person in college, we could estimate Wald statistics between 2 and 3 person families and between 3 and 4 person families and so on. Of course, in the estimation of each Wald statistic we would actually have multiple Wald statistics comparing income-asset groupings across each discontinuity. In Table 4, there are 25 different comparisons that can be made. If there are 10 income-asset groupings with each family size and number of children combination, then we would estimate 250 Wald statistics.

While this approach seems straightforward, other discontinuities in the Pell formula complicate the estimation of Wald statistics. For example, there are some income ranges where students would receive the maximum Pell grant regardless of their family size or the number of children attending college. The Wald statistic would not be defined (or would be greatly inflated) over these ranges. Similarly, the Wald statistic will be similarly not defined for families that would have received no Pell grant regardless of their family size or the number of children in college. Since these groups will likely create additional noise in the estimation, we may want to exclude these groups at times.

Before estimating the Wald statistics, we need to create the income-asset groupings needed to create comparisons between homogeneous groups. To create groupings, I re-estimate each student's Pell grant assuming that he or she belonged to a 2-person family with only 1 person attending college. I then divide this group into 6 parts based on the revised Pell grant:

1. People whose Pell grant in a 2-person, 1-in college would have been at the maximum of $3125.
2. People with revised Pell grants between $3124 and $2001.
3. People with revised Pell grants between $2000 and $1001.
4. People with revised Pell grants between $1000 and $401.
5. People with revised Pell grants at the Pell grant minimum of $400.
6. People with revised Pell grants equal to zero.

Having uniform groupings across cells makes it much easier to estimate the Wald statistics and their standard errors. Creating groupings around Pell grant values also avoids the problem that wealthier families are more likely to apply if they have more children. These families are identified in group 6. Also, I separate people who would have had the Pell grant minimum ($400) since in the Pell grant formula there is a discontinuity that allows families across a wider range of income to have this value of Pell grant. I use these revised Pell grants only for grouping. When actually computing the Wald statistics, I use the actual Pell grants.

Following Angrist (1991), the efficient combination of Wald estimators is just the instrumental variables estimate of *y* (stop-out behavior) on *x* (size of Pell grant) where dummy variables for each group (i.e. there are 6 groupings defined above for each family size/number-in-college combination) are used as instruments for *x*. The resulting estimates are found in Table 5.

Column 1 of Table 5 shows the instrumental variable estimate when all six groups are included, including those for whom there is no variation in Pell grants. The estimated effect is positive and significant. A $1000 increase in Pell grants stemming from differences in family size corresponds to a 5 percentage point increase in likelihood that a student drops out. This might be in line with the story that students receiving Pell grants do not try as hard in school and consequently are more likely to drop out. However, these estimates include data for all students including those who never received Pell grants. These students who filed FAFSA's but did not receive Pell grants are likely to be wealthier than other students and are less likely to have withdrawn from school.

In Column 2 of Table 5, I include only the students who would have had positive Pell grants less than the maximum.[8] The excluded students should have had not have any type of change in the Pell grants. The estimated effect suggests that a $1000 increase in students' Pell grants leads to a 3.6 percentage point reduction in the probability that a student withdraws. The result is statistically significant and suggests that systematic differences in Pell grants lead to differences in stop-out rates for students. Larger Pell grants reduce students' probabilities of withdrawing.

However, this result is not robust. When we restrict the sample to students at 4-year colleges, we find a positive and significant relationship. An increase of $1000 in Pell grant aid corresponds to a 2.6 percentage point increase in the likelihood that students transfer.

Another way to estimate the effects of Pell is to use an instrumental variable approach where the "delinearized" Pell grant is used as an instrument for the actual Pell grant. To do this, I run a regression of the actual Pell grant on a quartic in the key variables that determine the Pell grant (family income, family assets, family size, and number of children in the family attending college). The residuals from this regression should be primarily made up of discontinuities in the Pell formula along these dimensions. I then use the residual as an instrument for the actual Pell grant in a simple regression of stop-out behavior on students' Pell grants. I restrict the sample to students whose families have less than $150,000 in assets or less than $115,000 in annual income. The results appear in Column 4 of Table 5. Similar to Column 3, I find that a $1000 increase in Pell grants is associated with a 4 percentage point reduction in the likelihood that a student drops out.

In Column 5, I use a similar procedure except that I put campus fixed effects in the regression that predicts students' Pell grants. Again, I use the residuals from this 1st stage as an instrument in the next. No the results drop in magnitude and are statistically indistinguishable from zero.

What conclusions should be drawn? First, the estimates are extremely sensitive to sample selection. Among the infra-marginal students, particularly those at 2-year colleges, the size of Pell grants is negatively related to student stop-out behavior; however, the results for the majority of students and those at 4-year colleges does not have the same relationship. Second, the estimates are sensitive to the income-asset groupings. There is substantial heterogeneity around the discontinuity and efforts to create comparisons among homogeneous groups may not fully account for the heterogeneity. In these discontinuity models, the underlying assumption is that family size and the number of siblings does not have a direct effect on students' performances. However, this assumption may not be valid

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[8] The maximum income in this group is $25,000.

IV. Conclusion

This paper set out to estimate the effects of Pell grants on student retention. Using panel and cross-sectional variation as sources of identification, this paper attempted to estimate the relationship. While the Panel results suggest that the effects of Pell are likely negative, the results from the regression-discontinuity are less compelling and do not provide any conclusive result.

The paper demonstrates that even with superior data the effects of Pell grant are difficult to quantify. On the one hand, results showing positive relationships between Pell grants and drop-out behavior may show that Pell grants have been ineffective; however, they may also be the result of failure to adequately control for heterogeneity amongst Pell students.

Table 1. Descriptive Statistics
Full-time First-time Freshman in 1999-2000

Variable	4-year College	2-year College
Out of State Student	.103	.032
Lives on Campus	.557	.024
Age	18.8	21.0
	(2.5)	(6.2)
Non-White	.134	.173
Hours Completed by Fall 1999	13.4	11.2
	(4.7)	(6.3)
Left Institution After 1 year	.278	.491
Left Higher Education After 1 Year	.201	.431
Took ACT exam	.750	.446
ACT Composite Score (36=max)	21.8	18.9
	(4.3)	
Filed FAFSA for Fall 1999	.653	.628
Uncovered Financial Need ($)	423.0	24.1
	(716.1)	(172.4)
Uncovered Financial Need Cond'l on Being >0	1081.8	82.0
	(773.7)	(310.5)
Filed FAFSA for Fall 2000	.490	.399
Uncovered Financial Need ($)	1261.5	21.3
	(951.4)	(93.1)
Uncovered Financial Need Cond'l on Being >0	1400.2	41.3
	(900.2)	(126.5)
Change in Pell Grant (Cond'l on Pell eligibility in 99 or 00)	1691	881
	(994)	(762)

Standard deviations appear for non-binary variables. Data are for first time college freshman entering Ohio public colleges and universities in Fall 1999. Uncovered financial need equals tuition less the estimated family contribution from the FAFSA less any Pell Grant for which the student was eligible.

Table 2. Association Between Financial Aid and Stop-out Behavior

	Dependent Variable = Level of Financial Aid in 1999-2000					
	(1)	(2)	(3)	(4)	(5)	(6)
Level of Financial Aid (000's)	.033	.006	.005	.0002	.002	-.005
	(.002)	(.003)	(.003)	(.003)	(.003)	(.003)
Log of Parents' Income		-.036	-.030	-.030	-.024	-.042
		(.005)	(.006)	(.005)	(.005)	(.005)
Out of State Student		.072	.365	.055	.165	.041
		(.077)	(.152)	(.073)	(.172)	(.073)
Age		.022	.018	.018	.016	.020
		(.004)	(.005)	(.004)	(.005)	(.004)
Male		.029	.017	.003	-.002	.003
		(.005)	(.005)	(.004)	(.005)	(.004)
Lives on Campus		-.082	-.079	-.057	-.062	-.112
		(.007)	(.007)	(.007)	(.007)	(.004)
Took the ACT		-.113		-.063		-.088
		(.007)		(.007)		(.007)
ACT Score			-.0003		.004	
			(.0007)		(.001)	
Freshman Grade Point Average				-.138	-.132	-.136
				(.003)	(.003)	(.003)
Includes HS GPA controls	No	No	Yes	No	Yes	No
Includes Race FE	No	Yes	Yes	Yes	Yes	No
Includes Campus FE	Yes	Yes	Yes	Yes	Yes	No
R-squared	.108	.112	.091	.204	.169	.192
N	37028	30851	24627	29778	24012	29778

White standard errors are in parentheses. Sample includes first-time freshman who started at a public Ohio 2- or 4-year college in Fall 1999. These students were full-time students in their first semester. Data are from the Ohio Board of Regents.

Table 3. Relationship between Changes in Pell Grants and Stop-out Behavior: Results with Panel Data

	OLS	OLS	IV	OLS	OLS
	(1)	(2)	(3)	(4)	(5)
Actual Increase in Financial Aid (000's)	.014	.010	.050		
	(.002)	(.002)	(.045)		
Imputed Increase in Financial Aid (000's)				-.092	.018
				(.002)	(.008)
Level of Financial Aid in 1999-2000 (000's)		.009	.014	.026	.006
		(.002)	(.007)	(.002)	(.002)
Includes Covs	No	Yes	Yes	Yes	Yes
Includes Campus FE	Yes	Yes	Yes	Yes	Yes
N	24885	24116	20696	35233	24116

Standard errors are reported in parentheses. Samples in all columns except Column 4 include all students who filed FAFSA's for both the 1999-2000 and 2000-2001 school years. The sample in Column 4 also includes all students who filed FAFSA's in 1999-2000. Covariates include an indicator for whether the student was from out of state, age, gender, whether the student lives on campus, whether the student took the ACT exam, students' freshman GPA, and controls for race.

Table 4. Pell Grant by Family Size and the Number of Children in College

Income=$40,000		Number of Children in College			
		1	2	3	4
Number in Family	2	$2175	$2475	--	--
	3	$2875	$2775	$2775	--
	4	$3125	$3125	$2975	$2875
	5	$3125	$3125	$3125	$3075
	6	$3125	$3125	$3125	$3125
Income=$50,000		Number of Children in College			
		1	2	3	4
Number in Family	2	$975	$1775	--	--
	3	$1575	$2175	$2275	--
	4	$2325	$2425	$2575	$2575
	5	$3125	$2825	$2775	$2775
	6	$3125	$3125	$3075	$2975
Income=$60,000		Number of Children in College			
		1	2	3	4
Number in Family	2	$0	$975	--	--
	3	$400	$1475	$1775	--
	4	$1075	$1825	$2175	$2275
	5	$1875	$2275	$2375	$2475
	6	$2675	$2675	$2675	$2675

Calculations assume that the families have zero assets and no student contribution in the computation of the estimated family contribution. Calculations also assume that students attend high-cost institutions.

Table 5. Wald/IV Estimates of Effect of Financial Aid on Stop-out Behavior

Dependent Variable= Stop-out Behavior	Wald Estimator Full Sample	Wald Estimator Sample with max>Pell>0	Wald Estimator max>Pell>0 4-year Students	IV	IV Includes Campus FE
	(1)	(2)	(3)	(4)	(5)
Level of Pell Grant in 1999-2000 (in 000's)	.050	-.036	.026	-.040	-.006
	(.003)	(.009)	(.010)	(.004)	(.004)
N	32068	6398	3838	29166	29166

Standard errors are reported in parentheses. Standard errors correct for heteroskedasticity. In Column 3, the instrument for "Level of Pell Grant" is the residual from a regression of Pell grant on a quartic of the key variables determining Pell grants (family income, assets, family size, number of children in college). The IV column excludes families with income greater than $115,000 or assets greater than $150,000. Campus fixed effects are included in the 1st stage of the specification in Column 5 to control for the fact that different school costs will lead to different levels of tuition.

Appendix Table 1. First-stage Estimates of Family Size and Simulated Pell on Changes in Pell Grants

	Dependent Variable=Change in Pell from 1999-2000 to 2000-2001
Level of Financial Aid in 1999-2000 (in 000's)	-.154
	(.005)
Change in Financial Aid from Formula	.065
	(.074)
Change in Number of Siblings	.096
	(.013)
Age	.015
	(.009)
Took ACT	-.102
	(.020)
Gender	-.019
	(.014)
GPA in 1999	-.026
	(.008)
Race Fixed Effects	Yes
Campus Fixed Effects	Yes
N	20696

Standard errors are in parentheses. Regression also includes controls for whether the student lived on campus or had residency in another state.