# Reducing Income Inequality in Educational Attainment: Experimental Evidence on the Impact of Financial Aid on College Completion[1]

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Income inequality in educational attainment is a long-standing concern, and disparities in college completion have grown over time. Need-based financial aid is commonly used to promote equality in college outcomes, but its effectiveness has not been established, and some are calling it into question. A randomized experiment is used to estimate the impact of a private need-based grant program on college persistence and degree completion among students from low-income families attending 13 public universities across Wisconsin. Results indicate that offering students additional grant aid increases the odds of bachelor's degree attainment over four years, helping to diminish income inequality in higher education.

The polarization of American society according to family income is sharper and more apparent today than at any point since the 1920s. The share of

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income going to families in the top decile is close to 50%, and the top 1% holds most of those resources (Saez and Piketty 2014). Contemporary income inequality is a significant concern because of its substantial magnitude and its causes, which include the rapid accumulation of advantages by the very elite (McCall and Percheski 2010; Reardon and Bischoff 2011). It is not the result of a deterministic process but rather stems from stratifying forces pushing for rising or shrinking inequality (Saez and Piketty 2014). Historically, the American strategy for addressing those forces and reducing poverty has focused on the educational system, and consequently many are concerned with the contribution that education now is making to burgeoning inequality (Bowles and Gintis 2011; Duncan and Murnane 2011; Torche 2011; Corak 2013; Katz and Rose 2013). In particular, as college attainment has become more important for life chances, researchers and policy makers have renewed their focus on disparities in higher education (Alon 2009; Bailey and Dynarski 2011; Hout 2012).

In the middle of the 20th century, the United States made substantial investments in expanding postsecondary education to create opportunities for people unable to find work in the labor market and provide more spaces for those seeking a college education, often perceived as a promising pathway to social mobility (Rosenbaum 2001; Rosenbaum, Deil-Amen, and Person 2006; Attewell and Lavin 2007; Shavit, Arum, and Gamoran 2007; Torche 2011; Hout 2012). In 1965, public policy makers crystallized a specific set of ambitions for higher education policy, aiming to reduce class stratification by facilitating college opportunities for the children of low-income families to obtain college degrees (Kerr et al. 1960; Parsons 1970; Treiman 1970; Goldrick-Rab, Schudde, and Stampen 2014). The inaugural Higher Education Act created a grant program that led to the signature federal program known as the Pell Grant. At the time, both Senator Claiborne Pell and American Sociological Association President William Sewell gave speeches emphasizing the importance of making college more affordable in order to rapidly attenuate the link between family income and college attainment (Sewell 1971; Goldrick-Rab, Schudde, and Stampen 2014).

But sociologists have long been concerned with the contribution of the educational system to inequality, with many positing that it creates as much

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inequality as it mitigates (Bourdieu 1973; Willis 1977; MacLeod 1987; Coleman 1988; Raftery and Hout 1993; Shavit and Bloesfeld 1993; Lucas 1999; Bowles and Gintis 2011). The debate over the role that higher education plays in inequality is fueled by stark evidence that despite major college initiatives and significant spending on financial aid over the last 40 years, the relationship between family income and college attainment is stronger than ever (Ellwood and Kane 2000; Haveman and Smeeding 2006; Alon 2009; Bastedo and Jaquette 2011; Roksa 2012). Today just 30% of children born to families in the bottom income quartile are expected to enroll in college, compared to 80% from the top income quartile. The completion gap is even more substantial: students from high-income families are six times more likely than those from low-income families to complete a bachelor's degree by age 25 (Bailey and Dynarski 2011).

One major challenge is that many students are starting college but leaving without degrees (Turner 2004; Rosenbaum et al. 2006; Deil-Amen and DeLuca 2010; Goldrick-Rab 2010; Attewell, Heil, and Reisel 2011; Bailey and Dynarski 2011). Some are doing this after attending multiple colleges and accruing debt (Goldrick-Rab 2006; Goldrick-Rab and Pfeffer 2009). Nationally, 11% of Pell Grant recipients entering public universities do not enroll for a second year of college, and about 80% do not receive a bachelor's degree within four years (only another 20% earn that degree over six years; authors' calculations).[2] This is problematic, especially since research suggests that the students most at risk of noncompletion would stand to benefit the most from holding college degrees (Brand 2010; Brand and Xie 2010; Brand and Davis 2011; Hout 2012). At the same time, government, philanthropic, institutional, and employer spending on grant aid for college has reached an all-time high of more than $115 billion a year (College Board 2013).[3] As a result, many researchers and policy makers are posing a critical question: Is financial aid an effective strategy for addressing income inequality in higher education by increasing college completion rates among students from low-income families?

This article presents results from the nation's first experimental analysis of need-based financial grant aid, examining the impacts of a program distributing grants to students from low-income families. In selecting among first-year undergraduates beginning college at 13 public universities across Wisconsin, the private program used a lottery to select eligible students. The

[2] These statistics are based on the nationally representative Beginning Postsecondary Students (BPS) Longitudinal Study (National Center for Educational Statistics; https://nces.ed.gov/surveys/bps/)of 2003–4.

[3] In 2012–13, this included $47 billion in federal grants (including the $32 billion on Pell Grants), $44 billion in institutional grants, $14.5 billion in private and employer grants, and $9.7 billion in state grants. Some but not all of these grants were distributed on the basis of financial need (College Board 2013).

impacts of that program are estimated for three cohorts of undergraduates, focusing on changes in term-by-term enrollment, credit completion, grades, and degree completion. Variability in the program's effects are explored according to how much additional income students received from the grant, and differences based on their demographic, family, and academic characteristics and where they began college are explored as well. The findings provide rigorous empirical evidence that need-based financial grant aid can improve bachelor's degree completion rates among students from low-income families.

## FAMILY INCOME, FINANCIAL AID, AND COLLEGE DEGREE ATTAINMENT

Only 14% of children from poor families reach the top two quintiles of the income distribution if they do not earn a bachelor's degree, but if they do their chances of attaining that status are almost three times greater (Haskins, Holzer, and Lerman 2009). College-educated people enjoy a range of advantages when it comes to employment, health, economic stability, the marriage market, and the tasks associated with parenting (Attewell and Lavin 2007; Lleras-Muney and Cutler 2010; Torche 2011; Hout 2012; Oreopoulos and Petronijevic 2013; Schwartz 2013). But while college degrees continue to be associated with social mobility, lower family income is implicated in limited prospects for college completion. Both direct and indirect effects of lower income over the short and long term reduce the chances of college attendance and persistence to degree completion. For example, low-income families are less likely to reside in communities with strong and effective schools that offer opportunities for the advanced coursework necessary for college success (Roderick, Nagaoka, and Coca 2009; Reardon and Bischoff 2011; Long, Conger, and Iatarola 2012), transmit the forms of social and cultural capital required to obtain college knowledge (Bourdieu 1973; Coleman 1988; Plank and Jordan 2001; Lareau and Cox 2011), purchase the assistance in test preparation and college applications increasingly needed to secure admission to the best schools (McDonough 1997; Klasik 2012), and have the knowledge, beliefs, and dispositions necessary to navigate and benefit from the financial aid system (Conley 2001; Luna de la Rosa 2006; McDonough and Calderone 2006; Grodsky and Jones 2007; Bettinger et al. 2012; Goldrick-Rab and Kelchen 2015). Thus, even if they gain admission to higher education, many children from low-income families are less equipped to succeed in completing college degrees.

Ability does not diminish the difficulties associated with covering college costs. The most talented students possessing strong cultural and social capital still must be able to cover the costs of attendance in order to register for college each year. Families need access to financial capital on an ongoing ba-

sis if their students are to gain academic momentum and persist until degree completion (DesJardins and Toutkoushian 2005; Deil-Amen and DeLuca 2010; Attewell et al. 2012; Harris and Goldrick-Rab 2012). Need-based financial grants are supposed to make college possible by discounting the costs of attendance and thereby encouraging students to enroll for more years of college and to complete degrees (Dynarski 2003; Goldin and Katz 2008; Bowen, Chingos, and McPherson 2009; Goldrick-Rab, Harris, and Trostel 2009; Deming and Dynarski 2010). Government, philanthropy, private business, and educational institutions have invested large sums of money in this strategy, which in theory should be effective as long as students' remaining short-term out-of-pocket costs are sufficiently low enough to be manageable while they pursue higher education (Leslie and Brinkman 1987; Heller 1997; Goldrick-Rab et al. 2009; Feeney and Heroff 2013; Goldrick-Rab, Schudde, et al. 2014; Kelly and Goldrick-Rab 2014).

There are several pathways through which additional financial grants might improve rates of college completion for students from low-income families. A rational choice model predicts that individuals invest in their human capital to the point where the marginal benefits equal the marginal costs, and grants may increase those investments by reducing the costs—a substitution effect (Becker 1964; Manski and Wise 1983; Leslie and Brinkman 1987; Hechter and Kanazawa 1997). Alternatively, grants may provide income that allows students to allocate more time to school activities instead of work, which is especially important given that more than 70% of undergraduates are employed at least part-time (Bozick 2007; Clydesdale 2007; Roksa and Velez 2010, 2012). It is also possible that the income from grants enables students to purchase books and other supplies that enhance their academic performance in school, which both contributes to "academic momentum" and degree progress (Attewell et al. 2012) and increases the likelihood that they will meet the "satisfactory academic progress" standards required in order to retain financial aid (Schudde and Scott-Clayton 2014). These income effects may be especially large for students without college-educated parents since these students tend to work longer hours at lower wages and are also more likely to work at night (Benson and Goldrick-Rab 2011). Income effects may also be stronger for students with weaker levels of high school academic preparation who struggle to balance work with study time and need extra resources and support (Bozick 2007). In other words, whenever a student lacks the know-how (conferred by social and cultural capital) to navigate the complexities involved in attending and financing college, relieving financial constraints through grant aid may be an especially effective way to boost the odds of degree completion.

Apart from substitution and income effects, it is also possible that financial aid promotes college attainment by conferring nonpecuniary benefits. Research on undergraduates from marginalized backgrounds suggests that

they struggle to find their place in higher education and often feel that they do not belong or are unwanted (Goldrick-Rab et al. 2009; Stuber 2011; Armstrong and Hamilton 2013; Lehmann 2014). These feelings are especially common among racial/ethnic minorities, students who are the first in their family to attend college, and those who worry that they are underprepared for the college experience (Charles et al. 2009; Espenshade and Radford 2009; Goldrick-Rab 2010; Schudde 2013; Wilkins 2014; Kirst and Stevens 2015). The social meaning conveyed by the money invested in their education may propel a student's success by signaling the commitment and belief of others (Zelizer 1995; McDonough and Calderone 2006; Goldrick-Rab et al. 2009; Goldrick-Rab and Kendall 2014).

In prior work, we conducted a thorough review of these theoretical frameworks and discussed the corresponding evidence (Goldrick-Rab et al. 2009). Our conclusion is that despite strong priors suggesting that effects of grants ought to be positive, many studies fail to find evidence to support those hypotheses. One reason appears to be that while financial grants are often treated as equivalent to money, in practice they are delivered via a complicated financial aid system that reduces the likelihood that students receive funds and alters the messages that accompany them. Moreover, since navigating bureaucracies requires stamina and specialized knowledge, the students who may be most likely to respond to grant aid may be the least likely to actually receive it (Dynarski and Wiederspan 2012; Kelchen and Jones 2015). Another critical issue, discussed later in this article, is that many analyses are compromised by selection bias (Alon 2005).

Despite the lack of clarity on whether and precisely how financial aid serves to increase college attainment, federal and state spending on need-based grant aid has risen substantially, although not nearly as fast as college prices have (College Board 2013).[4] At the start of the Great Recession, spending on the Pell Grant grew by over $10 billion a year due to policy changes that expanded program eligibility, growth in college enrollment, and economic conditions that increased unemployment and reduced family financial strength. Today virtually every state in the nation funds a financial aid program of some kind, with total spending topping $9.6 billion (NASSGAP 2014). That investment is the result of a significant upward trend over time in state support of aid programs, both in absolute terms and as a percentage of state funds devoted to higher education. Compared to 30 years ago, states are spending about three times as much (after adjusting for inflation), and about 1.6 times as much per student, on need-based grant aid (Jacobs and

[4] Between 2008 and 2012, the period of the current study, the average amount of grant aid per full-time-equivalent undergraduate increased from just over $5,000 to just over $7,000, while the average loan grew from just under $4,000 to almost $5,000 (College Board 2013).

Whitfield 2012). But these investments do not match demand. While federal government expenditures on financial aid have nearly doubled since 2009, state programs are not keeping up with federal expansions or even with growing demand for existing programs—during the Great Recession about half of the states reduced need-based aid, while overall college enrollment expanded (Bettinger and Williams 2014). The effective purchasing power of the Pell Grant declined as well: in the early 1970s, the Pell covered almost 75% of the costs of attending a public four-year college or university; today, it covers less than 33%. It would seem, therefore, that in order to improve college attainment rates and reduce income inequality, further increasing the availability of fairly simple forms of need-based grant aid would be an important priority (Goldrick-Rab and Kendall 2014).

Instead, many policy makers are questioning whether means-tested grants are an effective way to boost college attainment (Kelly and Goldrick-Rab 2014). Legislators, policy analysts, and articles in the media have begun to brand Pell Grant recipients as unmotivated, undeserving, and fraudulent (McCluskey 2008; Field 2011; Terkel 2011; Cheston 2013; Nelson 2013), even though there is little evidence that widespread abuse exists (Institute for College Access and Success 2011). This behavior is consistent with the perception of other means-tested programs (Piven and Cloward 1993; Shaw et al. 2006; Katz 2013). Rarely discussed is the possibility suggested by prior research that these concerns are raised about the effectiveness of financial grant programs partly because they are targeted to poor people rather than universally available (Bruch, Ferree, and Soss 2010; Soss, Fording, and Schram 2011). In other words, not only has means testing, often central to the process of distributing grant aid higher education, created political challenges to these programs, but it contributes to the perception that they are difficult to access and unfair in their allocation; this is a sharp contrast to how the public has historically viewed other aid programs such as the G.I. Bill (Mettler 2005; Dynarski and Scott-Clayton 2007). Funding for financial aid has increasingly shifted toward merit-based or performance-based scholarships, which provide support for students only if they meet narrowly defined criteria of academic ability or performance (Kelly and Goldrick-Rab 2014). This is consistent with a broader movement away from the basic premises of mass public higher education (Attewell and Lavin 2007; Mettler 2014).

## EVIDENCE ON THE EQUITY EFFECTS OF NEED-BASED AID

There is very little rigorous research directly testing the theory that means-tested financial aid effectively reduces college prices to the point that students are more likely to complete their degree, and the dearth of compelling research evidence on the effectiveness of need-based grants is often noted

in policy discussions (Bettinger 2011; Lederman 2011; Owen and Sawhill 2013; Sawhill 2013; Kelly and Goldrick-Rab 2014).[5] Studies vary widely in the extent to which they address selection bias, whether they isolate impacts on college completion from effects on college enrollment, and whether they consider the potential for effect heterogeneity. The empirical challenge is that, as with all means-tested programs, students eligible for financial grants are different from ineligible students. There are many reasons having little to do with grant aid as to why students from low-income families might not complete college, given that they disproportionately receive weaker K–12 preparation, come from homes where college-going is rarely normative, and receive less social support in their efforts to pursue degrees (e.g., by having attended schools with fewer resources). At the same time, recipients of financial aid have successfully navigated a complex system and thus may be more motivated or possess more social and cultural capital than their peers. Together, these selection processes mean that a simple correlation between the receipt of grant aid and college completion may substantially overstate or understate the true benefits of that aid, partly depending on whether the estimates are based on aid eligibility or aid receipt among other factors (Alon 2005; Cellini 2008; Goldrick-Rab et al. 2009; Harris and Goldrick-Rab 2012; Castleman and Long 2013). Most financial aid research uses basic regression techniques to control for observable differences between students, an approach that fails to specify appropriate counterfactuals to financial aid receipt (Morgan and Winship 2007). There is also a growing number of studies using quasi-experimental techniques, usually propensity score analysis and regression discontinuity designs, or taking advantage of natural experiments. But to date there have not been any experimental studies.

Given that social contexts often moderate decision making, it is reasonable to anticipate heterogeneity; estimates of aid's impacts may vary across studies on the basis of the composition of the students and the colleges or universities under examination. They could also vary depending on whether the study estimates the effect of grants on whether students enroll in college, remain for a year, or complete degrees. These represent distinct educational decisions, and short-term income constraints may exert different effects at each point. The rate at which college attendance is transformed into degree completion has declined over time, especially for younger students like those in this study (Turner 2004).

[5] There are many studies on the impacts of merit-based financial aid programs distributed on the basis of students' academic preparation for college or their tested abilities; since these are not based on family income, they are not considered here. The mechanisms and impacts of merit- and need-based programs are thought to be quite different.

But the effects of aid on college attendance and effects on college persistence are often melded together in analyses (e.g., McPherson and Shapiro 1991; Kane 1994, 2007; Light and Strayer 2000; Bound and Turner 2002; DesJardins, Ahlburg, and McCall 2002; Paulsen and St. John 2002; Seftor and Turner 2002; Van der Klaauw 2002; Stinebrickner and Stinebrickner 2003; Singell 2004; Singell and Stater 2006; Stater 2009; for notable exceptions, see Bettinger 2004; Turner 2004). It is possible that reducing the costs of college attendance by providing aid may induce more students to attend college yet do little to help them finish. According to the most rigorous and relevant studies, the impact of a $1,000 increase in grant aid on rates of college retention (annual enrollment after initial entry) ranges from 1.5 percentage points (Alon 2011), 1–5 percentage points (Singell 2004), 2–3 percentage points (Bettinger 2010), to 3.6 percentage points (Dynarski 2003).[6] Very few studies observe students for enough time to consider impacts on degree completion. The study most relevant to the current analysis uses a regression-discontinuity framework to estimate impacts of a Florida state grant program over six years. The authors find that an additional $1,300 in grant aid eligibility (covering 57% of average costs of tuition and fees at public universities in that state) increased the probability of earning a bachelor's degree within six years by 4.6 percentage points, or 22% (Castleman and Long 2013). However, given the limitations of the research design, the authors could only produce those estimates for a subsample of the students eligible for the grant, and the estimates may still suffer from bias.

The price of college are clearly not the same for all students, and thus it is important that studies consider variation in the effects of financial grants across different types of students. Some research has identified effect heterogeneity according to race/ethnicity, gender, or precollege academic preparation (Kane 1994; Heller 1997, 1999; Ellwood and Kane 2000; Linsenmeier, Rosen, and Rouse 2006; Dowd 2008; Dynarski 2008; Angrist, Lang, and Oreopoulos 2009; Angrist, Oreopoulos, and Williams 2010; Chen and DesJardins 2010; Crockett, Heffron, and Schneider 2011; Castleman and Long 2013). For example, in a difference-in-difference analysis of an Ohio need-based grant program, Bettinger (2010) found that an unexpected in-

[6] Authors tend to report on the impacts of dollars of aid receipt even though, as Castleman and Long (2013) point out, aid programs and policies make aid available to students but cannot assure that all eligible students receive it. Thus when considering the effects of programs or policies, it is best to focus on students offered aid rather than only those receiving it. This is the approach taken in this article. There are other studies that examine the impact of aid on persistence; however, the methods employed do not address the likely selection bias and thus are not considered among the most rigorous (e.g., Murdock 1987; Perna 1998; St. John, Hu, and Tuttle 2000; McCready 2001; St. John, Hu, and Weber 2001; Dowd 2004; DesJardins and McCall 2010).

crease in aid for a group of less advantaged students generated a small average positive effect in first-year persistence rates for that group, while the same policy change reduced aid for a more advantaged group—but did not result in a reduction in persistence. In addition, grants may be more or less effective according to the depth of familial poverty students face, the degree to which their academic barriers make college success more or less possible, their levels of social capital related to parental education, or the costs or financial resources of their schools that they attend (Goldrick-Rab et al. 2009; Roksa and Potter 2011). This variation could reduce, or enhance, the degree to which grants affect inequality in outcomes. Using data from the BPS, Alon (2011) exploited a discontinuity created by the number of siblings attending college and identified much larger positive benefits of need-based grants (including federal, state, and institutional) on first-year persistence accruing to students in the bottom half of the income distribution and virtually no benefits accruing to students in the top half. Effects on completion were not estimated in either study. But to increase program effectiveness—and promote equity—Alon recommended focusing the Pell Grant on poorer families by adjusting the targeting of that program.

Only a few studies have been able to consider whether the effectiveness of grant aid depends on the extent to which it increases students' income and thus reduces out-of-pocket costs for college (Leslie and Brinkman 1987). As noted earlier, rapidly rising costs of college attendance have outpaced increases in need-based grant aid, resulting in a rising net price (Goldin and Katz 2008; Bowen et al. 2009). Because of those changes, the purchasing power of programs such as the Pell Grant has declined precipitously. In addition, while state and federal spending on higher education has increased over time, so has enrollment, and thus per-student subsidies have declined. As a result, even though spending on need-based financial aid is over $40 billion a year, poor families must spend as much as 75% of their annual income in order to send their children to college (Goldrick-Rab 2013; Goldrick-Rab and Kendall 2014). Thus, even with financial aid, students' short-term out-of-pocket costs (the difference between their calculated financial need and all forms of financial aid) can continue to be unmanageable, causing them to leave college. This would be a reason why aid is insufficient at ameliorating inequality. It is therefore particularly important to attend to these costs and consider how they are affected by grant aid when analyzing the impacts of grant programs.

## RESEARCH QUESTIONS

This article builds on prior theory and research by presenting the first-ever experimental test of a need-based financial grant program. Can offering students from low-income families more grant aid reduce inequality in col-

lege attainment by increasing degree completion rates among those students? The creation and implementation of a new private program made it possible to examine this important question critical to scholars of stratification, education policy researchers, and practitioners and policy makers throughout the country. We first consider the average impacts of the grant program on students' retention rates, academic achievement, and on-time (four-year) bachelor's degree attainment. Next, we ask whether impacts varied depending on the extent to which the grant added to students' income and reduced students' short-term out-of-pocket costs instead of simply substituting for loans during the first year of college. Then, we investigate whether the aid was more or less effective on the basis of students' ascribed characteristics (race, gender, immigrant status, family income, level of parental education), high school preparation, and tested ability. Finally, we examine variation in impacts according to the type of university that students attended. In this way, we explore the capacity of grant aid to reduce income inequality in college persistence and degree completion, as well as other sources of inequities among students from low-income families.

## THE INTERVENTION: A STATEWIDE FINANCIAL AID PROGRAM

The Wisconsin Scholars Grant (WSG) is a privately funded grant initiated in 2008 and supported by a $175 million endowment from the Fund for Wisconsin Scholars (FFWS), making it one of the largest need-based grant programs in the state (Pope 2010).[7] This article describes a study of the program's first cohort, with some additional data from the cohorts of 2009 and 2010.

While there has been a proliferation of more complicated programs attaching academic requirements to financial aid as incentives to improve student performance (Patel and Richburg-Hayes 2012; Kelly and Goldrick-Rab 2014), most federal and state financial grant programs remain need based and straightforward, with only modest academic requirements. For example, the federal Pell Grant program simply requires students to enroll in college full-time (12 credits) in order to receive the full amount of the grant and stipulates that students must make "satisfactory academic progress" (SAP) each term in order to retain the aid (typically a C average). The WSG is similarly structured.[8]

The WSG program offers students a $3,500 grant per year, which is renewable for up to five years, with a total potential maximum award of

[7] More information on the FFWS is available at http://www.ffws.org.

[8] However, the Pell Grant is prorated for students attending college less than full-time, while the WSG is not.

$17,500 per student.[9] On average, for students in the entering class of 2008, this amounted to 20% of the estimated costs of attendance (defined as tuition and fees, room and board, books, transportation, and other expenses), including 56% of tuition and fees at the median university. Since all students offered the WSG were already receiving other aid, it is also worth noting that the WSG amount was equivalent to 85% of the remaining short-term out-of-pocket costs they faced in September when beginning college.[10]

Students were eligible for the WSG if they were Wisconsin residents who attended and graduated from a state public high school within three years of matriculating to one of the state's 13 public universities, where they enrolled for at least 12 credits (full-time), completed the Free Application for Federal Student Aid (FAFSA) and qualified for a federal Pell Grant, while still possessing unmet need (excluding loans) of at least $1.[11]

In many experiments, researchers recruit participants by describing the potential benefits of the intervention, seeking consent for research participation, and then using random assignment to determine who is assigned to the treatment or comparison conditions. If employed here, this process could have led students to feel coerced into research participation or created disappointment if they did not receive the WSG. Instead, the FFWS created a process in which eligible participants were identified, randomly assigned, and then only notified of the program if chosen to receive the WSG offer. Data for this research study were obtained independently from the program, in order to avoid any possible interaction effects that could compromise the research or the program.

In early September of each academic year, financial aid officers at each university identified eligible students using administrative records and sent

[9] A student is eligible to receive the Pell Grant if his or her expected family contribution (EFC), as determined by completion of a federal aid application and a need analysis methodology, is below a certain value ($4,041 in the 2008–9 academic year). For more details, see Dynarski and Scott-Clayton (2007). The WSG was transferable among all public colleges and universities in Wisconsin. Students were still eligible if they switched to a Wisconsin public two-year college, but the grant amount declined to $1,800 per year.

[10] We would prefer to provide the reader with information comparing this amount (85% of out-of-pocket costs) to those of grants in other studies, but this information is unfortunately rarely if ever reported. This is a flaw in the research literature that needs to be corrected so that analysts can better understand the treatment (grant aid) and its effects. However, we can report that the FFWS grant distributed to students at two-year colleges was much smaller, covering at most 40% of their out-of-pocket costs, and elsewhere (Anderson and Goldrick-Rab 2015), we report correspondingly smaller impacts.

[11] The WSG could not have affected college entry in the first cohort, and it is very unlikely to have affected the initial enrollment decision of later cohorts. While the program was first announced about one year before the awards were made (December 2007), program details were not public until September 2008 and even then received little publicity. Because of this, we think the estimated impacts are purely on persistence and not on the initial decision to enroll in college.

their names to the state agency overseeing the distribution of grant and loan programs. Using a lottery, students were drawn at random from this pool, thereby receiving an offer of the WSG. An award notification letter was sent to the chosen students at the end of September.[12] To receive the grant, eligible students had to receive, sign, and return that form to the FFWS by December, when the first checks were distributed to universities. After that, students could continue receiving the grant for up to five academic years if they maintained Pell eligibility and enrolled at a Wisconsin public university or two-year college, were full-time (at least 12 credits) at the start of each term, and made SAP. Of course, not all students offered the WSG received the grant, initially or throughout college, since receipt depended on their actions. Therefore, when presenting analyses of the grant's impacts in each term, we also present the fraction of students receiving the grant in that term.

It is important to recognize that the programmatic annual cost of the WSG ($3,500) did not always translate into an equivalent reduction in students' actual out-of-pocket costs for attending college. Instead, the cost reduction achieved by the WSG varied on the basis of students' financial aid packages at the time of the award. This happens to all grants and scholarships received after the start of the academic year and is the result of federal regulations capping the amount of financial aid that students on need-based aid may have.[13] In brief, the amount of aid cannot exceed their budget for that academic year, which is closely related to the school's cost of attendance.[14] Three types of financial aid are available to cover that need: nonrepayable grants and scholarships, loans, and work-study. Most students do not receive sufficient grant aid to cover all need, and therefore they must make decisions about whether to use loans or work-study. If they take all federal loans and attend a school with a generous work-study program, some Pell recipients have a relatively "full" package of financial aid (with few out-of-pocket costs in the short term) that cannot accommodate a new grant like the WSG without first reducing loans to accommodate it. The law requires that subsidized and unsubsidized loans are removed first, followed by work

[12] For the cohorts described in this article, the letter was sent in October. Students were also sent e-mail from their financial aid officer verifying the legitimacy of the grant and told to watch for documents in the mail.

[13] This is a common occurrence, as financial aid elements arrive at different times during the semester according to when funds become available. Private grants are often distributed after government grants.

[14] An individual student's budget is usually the same as the cost of attendance at the college or university but may be adjusted by a financial aid office under special circumstances. The budget depends on whether the student lives on campus or off campus and whether the off-campus residence is with family. For more information on how costs of attendance vary according to living arrangements, see Kelchen, Hosch, and Goldrick-Rab (2014).

study and then state grants.[15] In cases like this, this displacement process means that a grant like the WSG may not transfer much, if any, new income to students.[16] Other students have more room in their financial aid package because they do not accept loan offers or cannot access federal work-study; these students face higher out-of-pocket costs, and the WSG fits easily into their financial aid package, increasing their income and reducing those costs.

Consider an illustration. Sara and Robert attend the same university and have similar family backgrounds characterized by a lack of financial strength and thus no expected family contribution to the costs of their education. They each need $8,000 per year to cover the costs that remain after their Pell and other need-based grants. Sara elects to take all available federal loans (amounting to $5,500) and also secures a work-study job that covers her remaining need. Robert accepts a subsidized federal loan ($3,500) but declines the unsubsidized federal loan and is unable to locate a work-study job. He faces out-of-pocket costs of $4,500 that he must cover in order to pay for his first year of college. But one month later, both Sara and Robert are selected to receive the WSG. After it is processed through their school's financial aid office, Robert receives a check for $3,500 to help cover those costs, while Sara does not receive a check—instead, her loan balance is simply reduced, and she no longer has a subsidized loan to repay.

If the effects of the WSG operate via increased income, we might expect Robert and Sara to respond very differently. They faced different out-of-pocket costs when the grant was awarded, and only one of them (Robert) now has more money (thanks to the WSG) to use to substitute for working or buy his books. The correlation between out-of-pocket costs and the additional income students received from the WSG is strong at 0.63. While it is possible that students like Sara might benefit in other ways from having their loans reduced—for example, it might improve their postcollege prospects in terms of purchasing a home or beginning a family—it is unlikely that this will enhance the WSG's impacts on their odds of college completion.[17]

## EDUCATIONAL SETTING: WISCONSIN'S PUBLIC UNIVERSITIES

Wisconsin has a diverse set of public postsecondary institutions led by two systems: the University of Wisconsin (UW) System and the Wisconsin Tech-

[15] Institutional aid is also frequently removed, especially when government aid is available (NSPA 2013; Turner 2013). However, because the FFWS prohibited this practice, it did not occur with the WSG.

[16] The effects of aid displacement are rarely documented or examined by researchers (Amos et al. 2009; NSPA 2013).

[17] A follow-up study is tracking the impacts of the WSG on student debt and postcollege outcomes.

nical College System (WTCS). The UW System includes 13 universities and 13 two-year branch campuses, while the WTCS has 16 technical college districts (many with multiple campuses). More than 80% of the state's undergraduate enrollment is in the public sector (nearly 45% of students attend public four-year colleges, while another 39% attend public two-year and technical colleges). In 2008, Wisconsin's total undergraduate enrollment in public universities was approximately 136,000 students, ranging from about 2,400 to 30,000 per school. As table 1 indicates, the median undergraduate enrollment per university was just over 8,600 students.

Non-Hispanic white students predominate among public university students in the state. The UW System continually aims to increase its enrollment of targeted minority groups—African-American, Latino, Southeast Asian (primarily Hmong), and Native American—but in 2008–9 the total number of students from these racial/ethnic backgrounds comprised just over 10% of the undergraduate student body on average. Women outnumbered men among undergraduates (54% vs. 46%), and almost half of all undergraduates did not have a parent holding a bachelor's degree. Across the 13 universities, about one in five students received a Pell Grant.

During the decade before the start of this study, tuition increased substantially in Wisconsin, a state historically known for its low tuition (Higher Educational Aids Board 2010; Mianulli 2010). At the 11 comprehensive (nonresearch) universities, it nearly doubled between 2000 and 2009 (from $2,594 to $5,084) and more than doubled at UW—Milwaukee and Madison.[18] In 2008–9, the cost of full-time attendance (including tuition and fees, books and supplies, room and board) at Wisconsin's public universities ranged from approximately $13,300 per year to about $19,000, with a median cost of $14,509. Full-time attendance required 12 credits, and the costs per credit were the same from 12 up to 18 credits.[19]

Even after taking financial aid into account, the share of family income needed to pay for college in Wisconsin was substantial. In 2008–9, Wisconsin resident undergraduates received a total of $799.1 million in need-based aid from all sources (including loans), and yet 50,000 students had unmet need totaling $675.2 million (Pope 2010). Apart from the Pell Grant, the Wisconsin Higher Education Grant (WHEG) was the largest source of need-based aid for residents and contributed 15% of all need-based aid received. But the state's allocation for the WHEG failed to meet demand—during the period of this study over 7,000 UW students each year found themselves without a WHEG despite being eligible. Moreover, institutional aid was scarce, representing just over 1% of need-based aid provided to students in the UW. The median amount of institution-funded grant

[18] See http://www.uwsa.edu/budplan/tuition.

[19] However, costs accrued on a per-credit basis at one of the 13 universities.

TABLE 1
DESCRIPTIVE CHARACTERISTICS OF THE 13 UNIVERSITIES IN THE UNIVERSITY OF WISCONSIN SYSTEM

| | Mean | SD | Median | Min | Max |
|---|---|---|---|---|---|
| Undergraduate enrollment:* | | | | | |
| Total undergraduate enrollment ($N$) | 10,576 | 7,904 | 8,641 | 2,440 | 30,362 |
| Wisconsin resident (%) | 79 | 16 | 82 | 50 | 97 |
| Pell Grant recipients (%) | 21 | 6 | 21 | 10 | 34 |
| Female (%) | 54 | 7 | 54 | 34 | 62 |
| First generation (% no parent with BA degree)† | 49 | 9 | 48 | 26 | 60 |
| Race/ethnicity (%) | | | | | |
| Non-Hispanic white | 88 | 8 | 90 | 68 | 94 |
| African-American | 3 | 4 | 1 | 1 | 14 |
| Hispanic | 2 | 2 | 1 | 1 | 8 |
| Southeast Asian | 2 | 1 | 2 | 0 | 3 |
| Other Asian | 1 | 1 | 1 | 0 | 3 |
| Native American/Pacific Islander/ Alaskan native | 1 | 1 | 1 | 0 | 3 |
| UW System targeted students of color | 11 | 7 | 7 | 6 | 31 |
| Financing: | | | | | |
| Cost of attendance ($) | 15,171 | 1,619 | 14,509 | 13,258 | 18,973 |
| Tuition and fees ($) | 6,523 | 572 | 6,220 | 6,037 | 7,584 |
| Instructional expenditures per undergraduate ($) | 6,055 | 2,030 | 5,399 | 4,652 | 12,466 |
| Institutional grant aid per undergraduate ($)‡ | 279 | 312 | 124 | 77 | 1,140 |
| Average debt of graduates ($)§ | 16,480 | 2,271 | 15,365 | 13,949 | 19,956 |
| Enrollment management: | | | | | |
| Selectivity (% of applicants admitted) | 79 | ... | 88 | 59 | 99 |
| Composite ACT score | 23 | ... | 22 | 20 | 28 |
| One-year retention rate (non-Pell recipients) | 81 | ... | 76 | 64 | 94 |
| One-year retention rate (Pell recipients) | 76 | ... | 75 | 66 | 91 |
| Four-year BA completion rate (all undergraduates) | 30 | ... | 25 | 9 | 55 |
| Six-year BA completion rate (non-Pell recipients) | 68 | ... | 65 | 35 | 84 |
| Six-year BA completion rate (Pell recipients) | 55 | ... | 56 | 30 | 77 |

NOTE.—Data are from the UW System reports, except instructional expenditures (Integrated Postsecondary Education Data System). All characteristics are for the 2008–9 academic year, except for first generation, which is 2009–10. The UW System's definition of "targeted students of color" excludes East Asian students. Means for the enrollment management section are enrollment weighted; others are institutional averages. "BA" here indicates a bachelor's degree across a wide spectrum of types (BA, BS, AB, etc.).

* Characteristics are for first-year students only, with the exception of total enrollment.

† Wisconsin residents only.

‡ Calculated by dividing discretionary grant aid controlled by institutions by the number of undergraduate students.

§ Unconditional on having accepted loans.

aid available per student was just $124 a year (although the range was substantial, from $77 to $1,140 per student). Thus, at the time, 69% of Wisconsin residents who earned a bachelor's degree from the UW System left with debt, with a per-person average of $16,480 (see table 1).

Wisconsin is typical in its struggles to improve educational attainment and close achievement gaps while confronting declines in state support and affordability (Goldrick-Rab and Harris 2011). Among new freshmen enrolling in public universities full-time in fall 2008, students not receiving Pell Grants were 5 percentage points more likely to be retained to the second year of college than students receiving Pell (table 1). Moreover, at the time there was a 13-percentage-point gap in six-year bachelor's degree completion rates at the average institution. On average, only 55% of first-time, full-time freshman Pell Grant recipients who entered a Wisconsin public university earned a bachelor's degree within six years, compared to 68% of nonrecipients.[20] That completion rate varied across universities, ranging from 30% to 77%.

## SAMPLE AND DESCRIPTIVE STATISTICS

The study focuses on estimating impacts for the WSG's first cohort of students, since the most detailed information is available for that sample. However, some estimates are also computed for students beginning college in fall 2009 and fall 2010. We include estimates from these cohorts because it provides for a greater sense of the reliability of the estimates and also allows for the possibility that as the program matured, its effectiveness may have changed, perhaps improving.[21] The number and characteristics of students eligible for the WSG changed over time, in part due to the changing economy and shifting financial conditions facing families that result in Pell eligibility, and as the program matured, financial aid administrators became better equipped to follow program rules for identifying students meeting the grant's criteria. In 2008 there were 3,157 students in the eligible pool, and that number grew each year. The number of grants the WSG offered also varied slightly according to the program's endowment, ranging from 550 to 600 per year. For comparison purposes, the control group includes all nonselected students, except for the first cohort, for which a stratified random sample of 900 students (instead of the full pool) serves as the comparison

[20] Six-year degree completion rates are based on the entering class of 2003. The gap for the entering class of 2006 (the most recent available) is larger, with 47% of Pell recipients and 62% of non-Pell recipients completing degrees (University of Wisconsin System 2013).

[21] For cohorts other than 2008, only student-level information on treatment status, university attended, and outcomes was provided to the researchers; thus, these samples cannot be characterized with the level of detail available for the cohort of 2008.

group.[22] In selecting that comparison group, the list of nonrecipients was blocked by university in order to facilitate the collection of an oversample of nonwhite students. Thus, that group is 50% larger than the treatment group and contains more students attending racially and ethnically diverse institutions. In analyses, inverse probability weights are employed because of unequal assignment probabilities among students across schools.

The analyses for the first cohort involve three samples of students, depending on the required data sources. Table 2 provides information on (*a*) the full sample, (*b*) an "administrative data sample" that is used to analyze average treatment effects for selected academic outcomes and heterogeneous effects according to student demographic and institutional characteristics, and (3) a "financial aid sample" used to examine how impacts varied by reduction in out-of-pocket costs. In each case the sample is labeled according to the data required to conduct the analysis—as described later, we have administrative records for a subsample of the full sample and financial aid records for a subsample of the administrative data sample. As the information in the table demonstrates, there are few meaningful differences across these subsamples.

Given the program requirements, all students in the sample were Pell Grant recipients who graduated from a Wisconsin public high school, regardless of which university they initially attended. The average age was just over 18, and nearly all were classified as dependents of their parents for financial aid purposes. Women constituted the majority (57%), and students of color were overrepresented when compared to the general student body: 27% were members of a racial/ethnic minority group (table 2).[23] Three groups predominated among students of color, including African-Americans, Hispanics, and Southeast Asians, of whom the vast majority were Hmong. Twelve percent of students in the sample were either first-generation immigrants or children of immigrants. According to student surveys, the students had an average of three siblings, with two siblings being the modal response.

Almost two in five students in this study did not have a parent who completed any education after high school, and almost four in five did not have a parent with a bachelor's degree. In 2007, the average adjusted gross income of the parents was just under $30,000, and the average calculated EFC based on the FAFSA was $1,631. Just over one-third of the sample

[22] Data could not be obtained for the entire group of nonrecipients ($N = 2{,}557$) in the first cohort because of the initial data agreements and data collection costs, but note that there are diminishing statistical returns to control group size with a fixed treatment group (Bloom 2005).

[23] Racial/ethnic minority groups include African-Americans, Native Americans, Hispanics, Southeast Asians, and multiracial students who are from at least one of these groups. Information on race was obtained from a student survey and administrative records, as it is not included in the FAFSA and as such is only available for about 80% of the full sample.

TABLE 2
DESCRIPTIVE CHARACTERISTICS OF COHORT 1 SAMPLES

| Characteristic | Full Sample | Admin Sample | Financial Aid Sample |
|---|---|---|---|
| *N* | 1,500 | 1,167 | 639 |
| Assigned to treatment (%) | 40 | 41 | 41 |
| Demographic characteristic: | | | |
| Wisconsin resident (%) | 100 | 100 | 100 |
| Pell Grant receipt (%) | 100 | 100 | 100 |
| Financially dependent for aid purposes (%) | 97 | 97 | 97 |
| Age (% 19 or younger) | 97 | 98 | 97 |
| Female (%) | 57 | 58 | 61 |
| Parental education (%): | | | |
| No college (neither parent) | 39 | 40 | 41 |
| Some college or associate's degree (at least one parent) | 38 | 38 | 36 |
| Bachelor's degree or higher (at least one parent) | 23 | 23 | 23 |
| Race/ethnicity (%): | | | |
| Non-Hispanic white | 73 | 75 | 73 |
| African-American | 7 | 7 | 8 |
| Hispanic | 6 | 5 | 6 |
| Southeast Asian | 8 | 8 | 8 |
| Native American | 4 | 4 | 3 |
| First- or second-generation immigrant (%) | 12 | 10 | 16 |
| Number of siblings | 3 | 3 | 3 |
| High school preparation: | | | |
| ACT score (composite) | . . . | 22 | 22 |
| Received ACG (%) | . . . | 81 | 78 |
| Financial resources: | | | |
| Parent(s)' adjusted gross income ($) | 29,918 | 29,567 | 30,644 |
| Below poverty line for family of four (%) | 33 | 34 | 32 |
| Financial aid (pretreatment, start of college): | | | |
| EFC ($) | 1,631 | 1,629 | 1,716 |
| Zero EFC (%) | 31 | 31 | 29 |
| Grants and scholarships ($) | . . . | . . . | 6,666 |
| Unmet financial need (COA − grant aid − EFC) ($) | . . . | . . . | 8,367 |
| Accepted loans (%, if offered) | . . . | . . . | 86 |
| Average loans ($) | . . . | . . . | 3,769 |
| Out-of-pocket costs ($) | . . . | . . . | 4,097 |
| Out-of-pocket exceeding WSG ($3,500) (%) | . . . | . . . | 50 |

NOTE.—Data are from the fall 2008 Wisconsin Scholars Longitudinal Study survey (parental education, number of siblings), UW System (college-level measures, ACG receipt, ACT score), and FAFSA (all other measures). ACG is a federal award based on rigorous high school course completion. Out-of-pocket costs are calculated as the cost of attendance (COA) less all forms of aid received pretreatment and the student's expected family contribution (EFC). COA includes tuition and fees, room, board, books, travel, and miscellaneous expenses. The only differences across samples (at $P < .05$) are female and received ACG (financial aid sample), white and Hispanic (administrative data sample), and first/second-generation immigrant (both samples).

came from families living below the poverty line for a family of four ($22,000 per year in 2008), and nearly all qualified as "working poor" because they earned less than 200% of the federal poverty threshold (Center on Wisconsin Strategy 2010).[24]

Pell Grant recipients qualify for the most need-based grant aid, and students with a zero EFC qualify for the maximum Pell. In this sample, 31% of students fell into this category. When starting college, students in the sample received an average of just under $7,000 in grants and scholarships (including an average Pell of $3,200). Since the average institutional cost of attendance was just over $15,000, this left students with an average $8,367 in unmet financial need (defined as the cost of attendance less grant aid and the student's EFC). But unmet need varied widely; the standard deviation was $3,029, and the range was from negative $7,500 (meaning that either the student received more grant aid than needed or professional judgment was exercised) to $17,900.[25] To put this into context, consider that covering this unmet need directly for the average student would require that his or her family spend an additional 28% of income beyond what was needed to cover the EFC.

Students could take loans to cover that need; at the time they could borrow subsidized Stafford loans amounting to $3,500 or the amount of their unmet need, whichever was less. In addition, they could borrow unsubsidized Stafford loans of up to a total of $5,500 in federal loans. On average, students in this sample accepted about $3,300 in loans (80% of which were subsidized). But 47% of students declined to take at least some of the loans offered to them, with 14% of students declining all loans (Goldrick-Rab and Kelchen 2015). As a result, more than 80% of students had remaining, uncovered out-of-pocket costs (defined as the cost of attendance less any type of financial aid received) when they started college. The average student faced out-of-pocket costs of $4,097, and more than one in four students still needed to cover greater than $8,000 in out-of-pocket costs in order to afford his or her first year of college.[26]

[24] Twenty-seven percent of families in Wisconsin earned less than 200% of poverty in 2010, compared to 30% nationwide (Center on Wisconsin Strategy 2010).

[25] Although students and their families are expected to cover the value of the EFC, this is often not feasible as the EFC may not represent the actual ability to pay. Rather, it represents a rough ranking of which students have the most financial need. Professional judgments occur when financial aid administrators adjust a student's EFC to better reflect the current financial circumstances. For example, an aid administrator can adjust an EFC to account for a parent losing her job midway through the tax year.

[26] In recent years, families have turned to Parent PLUS loans to reduce these out-of-pocket costs. One reason is that a growing number of Wisconsin's universities, like many across the nation, have begun including PLUS loans in students' aid packages rather than waiting for families to request them (Fishman 2014; Goldrick-Rab, Kelchen, and Houle 2014). But at the time of this study, very few students used these loans.

## DATA

The state of Wisconsin does not have a student unit record data system for higher education. Therefore, in order to examine the college outcomes of students offered the WSG, data agreements were required between the research team and the state agency that possesses financial aid information, the UW System, each of the 13 public universities in that system, and the FFWS. Over time, data agreements changed, creating variation in data availability across cohorts.

Two data sources provide information on whether and where a student is enrolled in college each semester. For all three cohorts, data from the UW System record enrollments at the 13 universities and 13 two-year branch campuses in that system. In addition, for the first cohort, data from the National Student Clearinghouse (NSC)—a centralized reporting system that collects publicly available directory information obtained from the colleges and universities attended by 92% of American undergraduates—are used to estimate impacts on transfer. All public universities in Wisconsin participate in the NSC.[27] Combining data from these two sources, enrollment and on-time (four-year) bachelor's degree completion information is available for all students in the study.

For all cohorts, the UW System measures credits and grades, but these data are available for different lengths of time.[28] This information is available for 78% of students in the first cohort (in table 2, see the administrative data sample) and all students in the second and third cohorts. If students offered the WSG left the UW System at different rates than other students, these analyses might be subject to bias, but estimates based on the first cohort suggest that there was no impact of the WSG on transfer rates outside of the system (analyses not presented but available on request). Impacts on the total number of credits earned are considered along with estimates of impacts on completion of 12 or more credits per term since the WSG required full-time enrollment. The cumulative GPA is reported by term for enrolled students, and for students who are not enrolled the GPA from the last term enrolled is reported, following Scott-Clayton (2011), while rec-

[27] Only 12 colleges in Wisconsin who participate in the Integrated Postsecondary Education Data System did not participate in the NSC as of 2008–9. The largest of these is Herzing University, a for-profit institution with a student enrollment of under 1,500. Total enrollment at these 12 schools is just over 7,000 students.

[28] In order for us to observe completed credits and grade point average (GPA), a student must have registered for and completed a credit and passed the class with a D or above. Credits for pass/fail classes, which are not included in GPA calculations, are not recorded with this measure. Credits derived from precollege enrollment, including advanced placement tests, are also not included. We observe the first and second cohorts for three years using UW System data, and the third cohort for two years.

ognizing that estimation of causal effects on GPA is not as straightforward as with other academic outcomes.[29] Finally, impacts of the grant on whether students met the requirements for retaining all of their financial aid from term to term are reported, since continual receipt of financial aid may be important for ensuring degree completion.

Financial aid packages are measured and pretreatment unmet need and out-of-pocket costs computed using financial aid packages provided by the universities. The data were difficult to obtain since it required that financial aid officers print screen shots of each student's financial aid package before packaging the WSG.[30] The data are available for 10 of the 13 universities (49% of students in the sample).

Students' precollege characteristics—demographic, academic, familial, and financial—are captured through the use of multiple data sources, including their financial aid application, the academic record provided by their university, and a survey fielded by researchers as students began college. Information on the characteristics of universities in the study is obtained from UW System data reports and the federal Integrated Postsecondary Education Data System (University of Wisconsin System 2008, 2009*a*, 2009*b*, 2010).

## ANALYTIC PLAN

Even in experimental studies, internal validity can be a concern, and thus the first stage of the analysis considers the validity of inferences derived using the experimental and control groups, testing for equivalence in their characteristics before the program began and examining the potential impact of differential attrition in data sources used for analyses. As explained earlier, there are critical differences between WSG offer and WSG receipt, with the former arguably representing the most policy-relevant parameter and the one for which estimations in this study are most free from bias. The experimental analysis focuses on an intent-to-treat framework in which students offered the WSG in their first year of college are compared to students who would have been offered it if selected during random assignment. We report the fraction of students who were offered and actually received the grant in each term so that attrition in receipt can be considered, but the analysis is an intent to treat and does not take duration of treatment into account.

We examine the number of years that students assigned to be offered the WSG actually receive the grant. Additionally, we estimate the impact of WSG

[29] Students can only have grades if they are enrolled; thus, if the grant influences enrollment, then this could give the false appearance that the program influenced GPA when in fact it may be that different students were enrolled and had the grades observed.

[30] This effort was required because some data are overwritten in financial aid systems; thus, some time-specific data had to be captured immediately.

offer on on-time (four-year) bachelor's degree completion for student $i$ in cohort 1 using the following ordinary least squares (OLS) regression:

$$Y_i = \alpha_{0i} + \alpha_{1i}T_i + \alpha_{2i}C_i + \varepsilon_i, \quad (1)$$

where $Y_i$ represents the outcome of interest (graduation), $T_i$ is an indicator for whether the student was assigned to receive the WSG in the first year of college (future assignment is not included), and $C_i$ is a vector of college fixed effects.

The magnitudes of the impacts are reported in the tables according to percentage point differences and standardized mean difference effect sizes; the latter are provided in the text since they allow the reader to understand the impacts in relation to the amount of variation present in the sample (Lipsey et al. 2012). Effect sizes are calculated using OLS regression for continuous outcomes. For binary outcomes, the Cox (1970) method is used, where the difference between treatment and control groups (after including covariates) is divided by 1.65. To aid in assessing whether those effect sizes are small, medium, or large, consider that the most critical outcome in this study, on-time (four-year) bachelor's degree completion, is a low-incidence outcome that is difficult to change. Effect sizes of educational interventions on outcomes like these usually fall well below 0.20 (Harris 2013).

We estimate treatment impacts on term-by-term persistence and achievement separately for cohort 1 and the combined second and third cohorts using equation (1). For continuous outcomes (such as credit completion), we determine effect sizes by dividing the covariate-adjusted difference in means by the pooled sample standard deviation.

We use interaction models to examine whether treatment effects on retention, credits earned in the fall of the third semester, and on-time graduation rates vary by pretreatment out-of-pocket costs and student characteristics (race/ethnicity, gender, parental education, dependency status, family income, and immigration status). We use continuous and binary measures of out-of-pocket costs (with the cutoff being $3,500 in out-of-pocket costs, as this is the value of the WSG) and ACT scores (where scores are broken into terciles); all other measures are binary. We use the following OLS regression to estimate treatment impacts:

$$Y_i = \alpha_{0i} + \alpha_{1i}T_i + \alpha_{2i}X_i + \alpha_{3i}(T_i \times X_i) + \alpha_{4i}C_i + \varepsilon_i, \quad (2)$$

where $Y_i$ represents the outcome of interest, $T_i$ is an indicator for whether the student was assigned to receive the WSG, $X_i$ represents out-of-pocket costs or the demographic measure of interest, $(T_i \times X_i)$ represents the interaction (continuous or binary), and $C_i$ is a vector of college fixed effects. Effect sizes are determined similar to before, with logistic regression for retention and graduation and OLS regression for credits completed.

Then we use interaction models to examine potential differences in treatment impacts by institutional selectivity (binary), Pell graduation rates (continuous), and institutional aid available per student (continuous). The models are the same as those used earlier with the exception that college fixed effects are excluded since all of the variation is across institutions.

The reader should note that the analyses of effect heterogeneity in tables 5–7 are exploratory since students were not randomly assigned to the characteristics used to stratify the sample. While it is the case that each comparison made (e.g., women vs. men or high vs. low out-of-pocket costs) includes a counterfactual (e.g., the outcomes of men randomly assigned the WSG are compared to those of men not assigned the WSG before being compared to the same contrast conditions for women), and these subgroups were formed before treatment was administered, it is still possible that biases due to unobserved sample selection could affect the impact estimates.

In table A2, we present unadjusted (college fixed effects only) and covariate-adjusted impacts and effect sizes for term-by-term persistence and achievement outcomes. We adjust for race/ethnicity, gender, age, parental education, zero EFC, dependency status, parental income, immigration status, and college fixed effects in the covariate-adjusted model. The models are otherwise similar to equation (1), and all covariates are used to determine effect sizes.

## INTERNAL VALIDITY

The primary threat to the internal validity of treatment impacts in this study stems from the potential for inadvertent nonequivalence in baseline equivalence of the samples, regardless of random assignment, and the potential differential observation of outcomes. Thus, before conducting each analysis, group differences in baseline characteristics are estimated and main and differential attrition examined, following best practices in experimental research (What Works Clearinghouse 2013).

Tables 3 and A1 present the results of regressions predicting student demographic characteristics with the indicator reflecting assignment to treatment. The coefficients from OLS regressions indicate whether and by how much the treatment group differed from the control group. In accordance with field standards, group differences raise concerns when they exceed 0.05 standard deviations, and differences larger than 0.25 standard deviations are especially problematic. The full samples for cohorts 1, 2, and 3 are balanced. However, the treatment group in the cohort 1 administrative data sample is disproportionately Southeast Asian (ES = 0.33), and the treatment group in the cohort 1 financial aid sample has more dependent students (ES = 0.30), students over age 19 (ES = 0.50), and Southeast Asian students (ES = 0.35) when compared to the control group. To address these

TABLE 3
Impact of Student Characteristics on Assignment to Treatment (Cohort 1)

| Characteristic | Full Sample | | Admin Sample | | Financial Aid Sample | |
|---|---|---|---|---|---|---|
| | Coefficient | ES | Coefficient | ES | Coefficient | ES |
| Financially dependent for tax purposes (%) | .4 | .095 | .6 | .145 | 1.4 | .302 |
| | (.9) | | (1.0) | | (1.3) | |
| Age (% 19 or younger) | .8 | .216 | 1.0 | .287 | 2.1 | .501 |
| | (.8) | | (.9) | | (1.4) | |
| Female (%) | 1.6 | .041 | 5.1 | .130 | 3.5 | .091 |
| | (2.7) | | (3.1) | | (4.1) | |
| Parental education (%): | | | | | | |
| No college (neither parent) (omitted) | 7.5* | .196 | 6.7 | .174 | 6.5 | .168 |
| | (3.1) | | (3.6) | | (4.2) | |
| Some college or associate's degree (at least one parent) | −8.6** | −.228 | −7.9* | −.208 | −4.2 | −.111 |
| | (3.1) | | (3.6) | | (4.1) | |
| Bachelor's degree or higher (at least one parent) | 1.1 | .041 | 1.2 | .045 | −2.3 | −.085 |
| | (2.6) | | (3.1) | | (3.5) | |
| Race/ethnicity (%): | | | | | | |
| Non-Hispanic white (omitted) | −.4 | −.014 | −1.2 | −.043 | 1.8 | .059 |
| | (2.7) | | (3.0) | | (3.6) | |
| African-American | −1.8 | −.189 | −1.9 | −.207 | −2.1 | −.200 |
| | (1.4) | | (1.6) | | (2.0) | |
| Hispanic | −.5 | −.048 | −.9 | −.126 | −.5 | −.049 |
| | (1.5) | | (1.5) | | (2.0) | |

| | | | | | | |
|---|---|---|---|---|---|---|
| Southeast Asian | 2.3 | .185 | 3.8 | .327 | 4.1 | .347 |
| | (1.7) | | (2.0) | | (2.3) | |
| Native American | .3 | .043 | .6 | .106 | −.8 | −.155 |
| | (1.2) | | (1.4) | | (1.5) | |
| First- or second-generation immigrant (%) | 2.3 | .139 | 3.1 | .205 | 1.6 | .077 |
| | (1.7) | | (1.9) | | (3.0) | |
| Number of siblings | .1 | .050 | .3 | .119 | .3 | .110 |
| | (.2) | | (.2) | | (.2) | |
| ACT composite score | .1 | .019 | .1 | .019 | .1 | .028 |
| | (.2) | | (.2) | | (.3) | |
| Received ACG (%) | .2 | .009 | .2 | .009 | 1.6 | .065 |
| | (2.6) | | (2.6) | | (3.3) | |
| Parent(s)' adjusted gross income ($) | 1,184 | .066 | 1,332 | .074 | 1,471 | .079 |
| | (1,000) | | (1,127) | | (1,543) | |
| Below poverty line for family of 4 (%) | −2.5 | −.071 | −1.0 | −.029 | −2.3 | −.067 |
| | (2.6) | | (3.0) | | (4.0) | |
| Expected family contribution ($) | 58 | .026 | 38 | .017 | −.136 | −.065 |
| | (127) | | (146) | | (170) | |
| Zero expected family contribution (%) | −2.7 | −.080 | −3.1 | −.092 | −6.9+ | −.221 |
| | (2.5) | | (2.9) | | (3.7) | |
| *N* | 1,500 | | 1,167 | | 639 | |

NOTE.—Data are from the fall 2008 Wisconsin Scholars Longitudinal Study survey (parental education, number of siblings), UW System (college-level measures, ACG receipt, ACT score), FAFSA (all other measures). All estimates are the results of regressions with institutional fixed effects. SEs are in parentheses. Effect sizes (ES) are calculated using OLS for continuous outcomes and logistic regression for binary outcomes.

$^{+}P < .10$.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

potential concerns, college fixed effects are included in all models for the first cohort (in case the observed differences are due to differential representation of colleges across samples) except when testing for differences across institutional characteristics, and the unbalanced covariates are added when estimating impacts with those samples. Also, for the second and third cohorts, baseline equivalence can only be checked using measures of where the students attended college (table A1). That simple check raises no cause for concern, but of course there is still potential for unmeasured bias in estimates based on those samples.

Even when there are no group differences before the start of treatment, differential attrition from those samples can introduce bias. The full sample of cohort 1 has no attrition. The administrative data sample includes 79% of the treatment group and 77% of the control group. The financial aid data sample includes 44% of the treatment group and 42% of the control group. While these differences in attrition by treatment status are small, given the overall magnitude of attrition in the financial aid data sample, significant bias to the estimates could occur, and thus those analyses should be treated as exploratory (What Works Clearinghouse 2013).

## AVERAGE IMPACTS ON COLLEGE ACHIEVEMENT AND ATTAINMENT

As table 4 indicates, the offer of the $3,500 WSG grant generated statistically significant and substantively important increases in on-time (four-year) bachelor's degree completion rates for students in the program's first co-

TABLE 4
Treatment Receipt Rates and Average Impacts on Graduation (Cohort 1)

| | Control Mean | Treatment Impact | Effect Size |
|---|---|---|---|
| Treatment receipt of the WSG, average (%): | | | |
| One year of receipt | 0 | 92.4 | . . . |
| Two years of receipt | 0 | 70.7 | . . . |
| Three or more years of receipt | 0 | 47.4 | . . . |
| On-time (four-year) bachelor's degree completion | 16.3 | 4.7* | .213 |

Note.—Data are from the UW System (WSG receipt) and the NSC (degree completion). Degree completion measure observes students' graduation records, regardless of whether they remained within the UW System.

$^{+}P < .10$.
* $P < .05$.
** $P < .01$.
*** $P < .001$.

hort. While just 16% of students who were not offered the WSG managed to complete a bachelor's degree in four years, about 21% of students offered the grant finished that degree (ES = 0.21, $P < .05$). Data are not yet available to estimate impacts on degree completion for the second and third cohorts of students, but since degree completion stems from a process of academic achievement and attainment after college entry, we next examine impacts on a term-by-term basis across cohorts.

Students were notified of the WSG offer early during their first semester of college. While this followed the registration period, making it impossible for the treatment to change decisions about whether students registered that term or how many credits they took, it is possible that the notice of $3,500 in pending grant aid could have affected how many credits they completed or improved their grades. Funds from the grant reached the students' financial aid packages by the end of that first semester and were received by the start of the second term. After that time, students were eligible to continue receiving the grant during subsequent semesters as long as they continued to enroll in school, maintained Pell eligibility (which required making SAP), and registered for at least 12 credits per term. In table 5 we report impacts on enrollment, credit completion, and grades by semester.

All students were enrolled during the first semester of the study, but about 6% of those students left college after one term, nearly 20% were gone after two terms, and by the end of three academic years (five semesters) after their initial start date, just over 70% of students remained enrolled. As table 4 indicates, the percentage of the treatment group receiving the WSG also diminished over time (partly due to attrition from college but also due to failure to meet the requirements). For example, while 92% of students offered the grant received it in the first year, that fraction dropped to 71% by year two, and just 47% in year three.[31] This is not uncommon—indeed, receipt of the federal Pell Grant also declines over time when students do not renew their FAFSA, do not meet SAP standards, or experience a change in their family's economic circumstances (Bird and Castleman 2014; Schudde and Scott-Clayton 2014; Kelchen 2015). This program attrition is rarely considered in analyses of the effects of financial aid and is an important area for further research.

[31] These changes may be been partly related to shifts in students' family income and Pell eligibility, but that is clearly not the only reason for the decline in the number of students receiving the grant over time. Most students did not see large changes in their household income over three years, as the correlation in parental income between the first and third years of college is 0.59. Eighty-nine percent of continuously enrolled students were eligible to receive the Pell Grant during their second year of college, and 86% during their third year.

TABLE 5
Term-by-Term Impacts on College Persistence and Achievement (Cohorts 1–3)

| | Cohort 1 Admin Sample | | | Cohorts 2 and 3 | | |
|---|---|---|---|---|---|---|
| | Control Mean | Treatment Impact | Effect Size | Control Mean | Treatment Impact | Effect Size |
| Semester 1 (treatment begins—91% of treatment group received WSG): | | | | | | |
| Total credits completed | 14.0 | .2 | .059 | 13.8 | .1 | .032 |
| | | (.2) | | | (.1) | |
| % completing 12+ credits | 88.0 | .3 | .021 | 88.7 | 1.1 | .077 |
| | | (1.8) | | | (1.0)** | |
| Cumulative GPA | 2.54 | .08 | .078 | 2.70 | .09* | .099 |
| | | (.06) | | | (.03) | |
| 12+ credits and 2.0 GPA (%) | 71.6 | −.5 | −.017 | 74.5 | 2.7* | .095 |
| | | (2.7) | | | (1.4) | |
| Semester 2 (88% of treatment group received WSG): | | | | | | |
| Enrollment (%) | 93.7 | 1.5 | .176 | 93.9 | 2.2* | .265 |
| | | (1.4) | | | (.7) | |
| Total credits completed | 12.1 | .3 | .067 | 12.7 | .3** | .078 |
| | | (.3) | | | (.1) | |
| % completing 12+ credits | 73.7 | .8 | .029 | 80.0 | 3.4** | .145 |
| | | (2.7) | | | (1.2) | |
| Cumulative GPA | 2.49 | .07 | .076 | 2.65 | .07** | .084 |
| | | (.05) | | | (.03) | |
| 12+ credits and 2.0 GPA (%) | 66.2 | −2.0 | −.059 | 71.4 | 3.5* | .115 |
| | | (2.9) | | | (1.4) | |
| Semester 3 (69% of treatment group received WSG): | | | | | | |
| Enrollment (%) | 80.9 | 2.5 | .110 | 82.6 | 3.0** | .138 |
| | | (2.4) | | | (1.2) | |
| Total credits completed | 10.7 | .3 | .053 | 11.2 | .5* | .080 |
| | | (.4) | | | (.2) | |
| % completing 12+ credits | 65.7 | 2.7 | .080 | 69.2 | 3.0* | .093 |
| | | (2.9) | | | (1.5) | |

| | | | | | | |
|---|---|---|---|---|---|---|
| Cumulative GPA | 2.47 | .06 (.05) | .070 | 2.64 | .08** (.03) | .089 |
| 12+ credits and 2.0 GPA (%) | 61.3 | .8 (3.0) | .023 | 65.3 | 3.1* (1.5) | .091 |
| Semester 4 (64% of treatment group received WSG): | | | | | | |
| Enrollment (%) | 76.0 | 1.8 (2.6) | .067 | 78.2 | 2.2 (1.3) | .084 |
| Total credits completed | 9.9 | −.1 (.4) | −.020 | 10.3 | .5* (.2) | .075 |
| % completing 12+ credits | 61.7 | −4.1 (3.0) | −.113 | 63.9 | 3.5* (1.5) | .101 |
| Cumulative GPA | 2.47 | .06 (.05) | .070 | 2.64 | .08** (.03) | .096 |
| 12+ credits and 2.0 GPA (%) | 58.5 | −4.5 (3.0) | −.121 | 61.5 | 3.1* (1.5) | .086 |
| Semester 5 (49% of treatment group received WSG): | | | | | | |
| Enrollment (%) | 71.2 | −.0 (2.8) | −.000 | 73.3 | 1.9 (2.1) | .063 |
| Total credits completed | 9.1 | .3 (.4) | .043 | 9.7 | .4 (.3) | .059 |
| % completing 12+ credits | 53.8 | 2.6 (3.0) | .071 | 59.4 | 3.7 (2.3) | .101 |
| Cumulative GPA | 2.48 | .06 (.05) | .066 | 2.64 | .06 (.04) | .069 |
| 12+ credits and 2.0 GPA (%) | 52.7 | 1.6 (3.1) | .043 | 57.9 | 3.5 (2.3) | .095 |
| Semester 6 (45% of treatment group received WSG): | | | | | | |
| Enrollment (%) | 69.0 | −1.4 (2.9) | −.041 | 71.1 | 1.7 (2.1) | .055 |
| Total credits completed | 8.8 | −.2 (.4) | −.035 | 9.3 | .1 (.3) | .013 |
| % completing 12+ credits | 55.4 | −2.4 (3.1) | −.065 | 57.7 | .6 (2.4) | .017 |
| Cumulative GPA | 2.49 | .07 (.05) | .074 | 2.65 | .05 (.04) | .063 |

TABLE 5 (*Continued*)

| | Cohort 1 Admin Sample | | | Cohorts 2 and 3 | | |
|---|---|---|---|---|---|---|
| | Control Mean | Treatment Impact | Effect Size | Control Mean | Treatment Impact | Effect Size |
| 12+ credits and 2.0 GPA (%) | 53.9 | −3.0 | −.078 | 56.5 | .4 | .011 |
| | | (3.1) | | | (2.3) | |
| Cumulative outcome (three years cohorts 1 and 2, 2 years cohort 3): | | | | | | |
| Total credits completed | 65.8 | .9 | .032 | 57.6 | 2.1** | .089 |
| | | (1.7) | | | (.7) | |
| Cumulative GPA | 2.49 | .07 | .074 | 2.65 | .08** | .093 |
| | | (.05) | | | (.03) | |
| 12+ credits each semester (%) | 35.1 | .3 | .008 | 45.1 | 2.5 | .066 |
| | | (2.9) | | | (1.6) | |
| 12+ credits and 2.0 GPA each semester (%) | 33.3 | 1.2 | .036 | 43.1 | 3.1 | .082 |
| | | (2.9) | | | (1.6) | |
| Sample size | 692 | 475 | | 7,862 | 1,035 | |

Note.—Data are from the UW System. Standard errors in parentheses. Enrollment includes any of the 13 four-year UW System universities, as well as the 13 two-year UW colleges. If a student was not enrolled in a given semester, the cumulative GPA from the previous semester is reported. There are only four semesters of data for cohort 3. All estimates include university fixed effects. Effect sizes are calculated using OLS for continuous outcomes and logistic regression for binary outcomes. The percentage of students receiving grants in each term may not be the same as the duration of time grants are received (table 3) because some students received the grants in discontinuous terms.

$^{+}P < .10$.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

Looking across impacts for the first three cohorts served by the FFWS program (table 5), it appears that the WSG offer boosted retention rates among university students by 1–3 percentage points per term (translated into effect sizes, these impacts rate from about 0.1 to 0.3 standard deviation improvements). The impact estimates are larger and the standard errors smaller for the second and third cohorts; the latter is unsurprising given the much larger sample used in those estimations. But the trends are generally the same across cohorts, with the largest impacts on retention occurring during the third semester—one term after the receipt of the grant funds—and waning after that point. By the sixth semester after college entry, less than half of students offered the WSG were still receiving the grant, and impacts on retention were indistinguishable from zero.

The students began college registered for at least 12 credits, the minimum threshold for full-time enrollment. While the funds from the grant did not arrive until December or the start of the second semester (in some cases), there is limited evidence that impacts occurred during the semester in which students were first notified. Specifically, table 4 indicates that students offered the WSG finished that term with a slightly higher cumulative GPA (just over a 2.7 rather than a 2.6). The impact estimates are similar across cohorts and hold steady in magnitude (effect size = 0.09) across the first four semesters of college before diminishing slightly and becoming statistically indistinguishable from zero.[32] While an impact of this size is rather small, it may be notable given that the cumulative GPAs of these Pell recipients hovered so close to a C+ average, while continued financial aid receipt hinged on maintaining at least a C (more on this below).

Students receiving the WSG also seem to have earned modestly better grades while completing more credits. On average, the treatment impact on completed credits was about 0.3 to 0.5 credits per term; this includes zero credits for all nonenrolled students. Like the trend for GPA, impacts faded by the start of the third year of college. In total, across the three years for which we can measure credits and grades, the offer of the WSG increased the completed credits by one or two and generated an improvement in GPA of about 0.08.

These modest improvements in credit completion and grades may have contributed to overall educational attainment directly but might have also enhanced on-time degree completion by increasing students' likelihood of re-

[32] It is impossible to determine from the available data whether the estimated effects waned over time because the fraction of students receiving the grant diminished (which clearly occurred) or because students become less financially needy (or less sensitive to financial aid) as they move through school. While it would be informative to know more about variation in the impacts of aid according to timing of delivery (as suggested by Kelchen and Goldrick-Rab 2015), this is a task for future research.

taining their financial aid. Throughout college, students are at risk of losing some or all of their financial aid by shifting from full-time to part-time enrollment or failing to make SAP. This affects the distribution of the Pell Grant and also affected the distribution of the WSG, which required continued Pell receipt and continued full-time enrollment. The results indicate that offering students the WSG increased their chances of making SAP and thus retaining their aid. In this critical sense, money may beget money—in other words students with more financial resources may have the greater support required to complete more credits and earn better grades, thus retaining their aid.[33]

The most important finding in this regard is that large numbers of students do not meet these standards, completing 12 credits while maintaining at least a C average cumulative GPA (table 5). In each term, between 20% and 30% of enrolled students did not meet the academic thresholds required to retain their need-based financial aid. But students offered the WSG were more likely to meet the academic requirements necessary to keep their need-based aid. Estimates from the second and third cohorts suggest that the WSG offer increased by about 3 percentage points the likelihood that students would make SAP (a 2.0 GPA) and complete at least 12 credits per term (ES = 0.08–0.10). These impacts were not apparent for the program's first cohort of students.[34] About one in three students in the first cohort and

[33] It is unlikely that the WSG provided students with an incentive to make SAP based on its requirements, given the evidence from surveys and interviews that many students were unaware of the grant's requirements. Like many government programs, the WSG's program rules were unevenly followed and in some cases misunderstood by students. Students in the first cohort were not regularly reminded about the grant's renewal criteria, and surveys administered to that cohort in the months after the program began and again a year later showed that barely half of students offered the grant knew that it was part of their financial aid package (in contrast 80% of these Pell recipients knew they received a Pell Grant). Some students were also confused about the grant's academic requirements for retention of the funds. On surveys, 83% of students assigned to treatment revealed that they misunderstood the grant's requirements, and recipients of the federal Academic Competitiveness Grant (ACG), which required a 3.0 average, seem to have mistakenly thought that the Wisconsin grant demanded full-time enrollment and a 3.0 average. In addition, the WSG required that students continue to receive the Pell Grant each year, and some students did not understand this and were surprised when their family income changed or they did not refile the FAFSA and thus their WSG was discontinued.

[34] As indicated in n. 33, surveys and interviews conducted with the first cohort provide a possible explanation, indicating that students were confused about the WSG's requirements and thought that the grant required a 3.0 GPA instead of a 2.0. Many of these students also had a now-defunct ACG from the federal government, which did require a 3.0. Students attempting to earn a 3.0 GPA while enrolling full-time (to keep the WSG) may have failed, leading to dropping either credits or getting worse grades. The FFWS consistently increased and improved communications with schools and universities over time, and this problem may have been resolved.

43% of students in the second and third cohorts made SAP each semester they were observed (six semesters for cohorts 1 and 2, four semesters for cohort 3).

## VARIATION IN IMPACTS

### Out-of-Pocket Costs

As explained earlier, the impacts of the WSG offer might vary depending on the out-of-pocket students faced at the start of college. Those out-of-pocket costs were not randomly distributed but were determined before the assignment of the WSG offer. However, the ability to observe the data needed to conduct this analysis of effect heterogeneity does appear to be related to students' treatment status, and thus there is reason to suspect that bias may affect the estimation of the results. The analysis is therefore exploratory.

In general, students (like Robert, described earlier) who had less financial aid and higher out-of-pocket costs when they began college were slightly less likely to persist for a second year of college, and they earned somewhat fewer credits (table 6). Since it was possible to add the WSG to their financial aid package without displacing existing aid (including loans), these students were more likely to gain additional income from the grant. Other students (like Sara, described earlier) saw their loans displaced and thus received less additional income in the short term. For example, consider that

TABLE 6
HETEROGENEOUS IMPACTS ON COLLEGE RETENTION, CREDITS OVER ONE YEAR, AND FOUR-YEAR DEGREE COMPLETION RATES ACCORDING TO PRETREATMENT OUT-OF-POCKET COSTS (Cohort 1 Financial Aid Sample)

| | Retention | Credits | Four-Year BA |
|---|---|---|---|
| Variation by pretreatment out-of-pocket cost: | | | |
| Assigned to treatment | −2.5 | −.7 | 8.8 |
| Out-of-pocket costs ($1,000) | −1.3 | −.2* | .2 |
| Treatment × out-of-pocket cost | 1.5 | .3 | |
| Variation by pretreatment high out-of-pocket cost: | | | |
| Assigned to treatment | −2.3 | −.4 | 8.1 |
| Out-of-pocket cost (over $3,500) | −10.8* | −1.5* | −1.7 |
| Treatment × high out-of-pocket cost | 11.5* | 1.6 | −5.8 |

NOTE.—Data are from the UW System (retention and credits) and the NSC (four-year graduation). Out-of-pocket costs are defined as the cost of attendance less all pretreatment financial aid and the student's expected family contribution. $N = 639$.

$^{+}P < .10$.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

for 89% of students whose out-of-pocket costs at the start of college exceeded $3,500, the WSG offer increased their income by at least $1,000. Just 27% of these students had their loans reduced, on average by about $1,500. In contrast, only 38% of students whose out-of-pocket costs were less than $3,500 when they started college received at least $1,000 in additional income from the WSG. Instead, 69% of these students saw their loans reduced when the WSG was added, with an average reduction of $2,612. In other words, most of those students did not obtain any additional income to help with college costs, even though interviews conducted with a subset of students suggest that they often expected that they would.

Analyses suggest that students with higher initial out-of-pocket costs not only received more income from the WSG, but they also incurred larger benefits in terms of impacts on retention and credits in the second year of college. For each additional $1,000 in out-of-pocket costs students faced as they started college, the additional impact of the WSG offer on retention to the second year of college was 1.3 percentage points ($P < .10$), for a total impact of 4.5 percentage points for a student with $3,500 in out-of-pocket costs. The impacts were even larger for students with out-of-pocket costs exceeding the size of the WSG; students needing to cover at least $3,500 in order to make ends meet received an additional 11.5 percentage point boost in retention to the second year of college ($P < .05$), for a total impact of 14.7 percentage points.[35] However, similar impacts on completion were not observed; it may be the case that reducing large out-of-pocket costs helped students stay in school, but the factors contributing to those higher costs in the first place may inhibit any acceleration in degree completion.

### Other Student Characteristics

We also test for and reveal some variation in the impacts of the WSG offer according to students' demographic characteristics and their levels of precollege academic preparation (table 7). While gender, racial/ethnic, and income variations in effects were not detected (the most common aspects of effect heterogeneity identified in prior research), there were sizable differences in the impacts of the WSG offer according to parental education. Specifically, students who were the first in their family to attend college do not appear to have accrued positive benefits of the WSG offer in terms of degree completion over four years. Those benefits seem to have been limited to students with college-going parents. As discussed earlier, it may be that

[35] Falsification tests available from the authors suggest that the impacts are nonlinear, with greater benefits accruing to students with at least $2,000 of unmet need and accelerating somewhat around the amount of the grant.

these students were less equipped to navigate the financial aid system and derive the potential effects of the grant, or it may be that they carried heavier workloads that could not be adequately diminished by the grants. It is also possible that the impacts of the grant have not yet emerged for these students, who often take longer than educationally advantaged students to complete college.

There is also evident effect heterogeneity based on how prepared students were for college, such that larger positive benefits of the WSG offer are detected for students with less academic preparation. More specifically, positive impacts on retention, credits, and degree completion are larger for students whose high school transcripts did not qualify them for the federal ACG—a program that was designed to give more financial aid to students considered to be academically deserving. Instead, the results presented here indicate that investments in students with lower odds of success may generate greater payoffs. As described earlier, this may be because students with lower levels of academic preparation benefit more from the income provided by grants, especially if it reduces their workload and allows them to focus more of their time and energy on school.

### Institutional Factors

The decision about where to attend college occurred before students came into contact with the WSG, and therefore we consider whether the impacts of the WSG offer varied according to characteristics of the university a student attended (table 8). Specifically, estimates based on institutional selectivity (using median ACT scores), Pell recipient six-year graduation rates, and institutional aid budgets are presented.[36] Impact variation on three outcomes is considered: rates of retention to the second year of college (when the fraction of students offered the WSG who were still receiving the grant was still fairly high), credits obtained by the second year of college, and on-time (four-year) bachelor's degree completion rates for the first cohort of students.

The evidence regarding the interaction between institutional selectivity and the impacts of the WSG offer is not strong. For the first cohort served by the program, it appears that students at less selective institutions may have received somewhat larger positive benefits from the program in terms

[36] Institutions are classified as being more selective if the median ACT score is 25 or higher ($n = 3$) and are compared to the 10 less selective institutions. The control group retention rates are pooled among students in the first three cohorts not offered the WSG. Finally, the institutional aid per student measure is the institutional grant aid budget in the 2008–9 academic year (according to the UW System) divided by the number of undergraduate students in the fall 2008 semester.

TABLE 7
HETEROGENEOUS IMPACTS ON COLLEGE RETENTION, CREDITS OVER ONE YEAR, AND FOUR-YEAR DEGREE COMPLETION RATES ACCORDING TO STUDENT CHARACTERISTICS (Cohort 1)

| | Cohort 1 Admin Sample | | | Cohort 1 Financial Aid Sample | | |
|---|---|---|---|---|---|---|
| | Retention | Credits | Four-Year BA | Retention | Credits | Four-Year BA |
| Variation by gender: | | | | | | |
| Assigned to treatment | −3.1 | −.5 | 4.7 | −.4 | −.2 | 6.8 |
| Female | −5.2 | −.3 | 3.2 | −7.6* | −.5 | 6.7 |
| Treatment × male | 7.2 | 1.0 | .0 | 6.8 | .9 | −3.0 |
| Sample size | 1,163 | | | 639 | | |
| Variation by parental education: | | | | | | |
| Assigned to treatment | 3.8 | .8 | 12.2** | 7.4* | 1.2 | 15.1*** |
| Parent education HS or less | −1.2 | −.4 | 1.2 | 2.4 | −.0 | 5.6 |
| Treatment × parental education | −6.3 | −1.4 | −18.0** | −9.5 | −1.7 | −24.2*** |
| Sample size | 811 | | | 634 | | |
| Variation by race/ethnicity: | | | | | | |
| Assigned to treatment | 1.7 | .2 | 6.8 | 5.6 | .7 | 6.7 |
| Targeted minority | −2.8 | −1.5* | −7.7 | 2.0 | −.7 | −5.3 |
| Treatment × minority | −2.1 | .1 | −5.3 | −7.8 | −1.1 | −5.9 |
| Sample size | 819 | | | 639 | | |
| Variation by immigrant/nonimmigrant: | | | | | | |
| Assigned to treatment | 1.3 | .0 | 4.0 | 4.6 | .4 | 4.8 |
| First/second-generation immigrant | 2.5 | −.1 | −5.9 | 7.8 | .6 | −6.4 |
| Treatment × immigrant | −.8 | 1.0 | 8.8 | −7.3 | −.2 | 2.8 |
| Sample size | 1,167 | | | 639 | | |
| Variation by parental income above sample median ($29,055) (dependents only): | | | | | | |
| Assigned to treatment | 2.1 | .0 | 1.7 | 7.4 | .8 | 6.8 |
| Higher parental income | 5.2 | 1.4* | −.4 | 4.7 | 1.2 | 1.1 |
| Treatment × higher parental income | −2.5 | −.1 | 6.2 | −7.8 | −1.1 | −3.4 |
| Sample size | 1,121 | | | 611 | | |

| | | | | | | |
|---|---|---|---|---|---|---|
| Variation by academic preparation (ACT score): | | | | | | |
| Assigned to treatment | 5.1 | −2.9 | −13.2 | −7.9 | −3.7 | −14.6 |
| ACT score | 1.0 | .2* | 1.2* | .6 | .2 | .9 |
| Treatment × ACT | −.2 | .1 | .8 | .5 | .2 | .9 |
| Sample size | 818 | | | 630 | | |
| Variation by academic preparation (ACT score): | | | | | | |
| Assigned to treatment | 5.3 | .9 | 5.8 | 8.8* | 1.1 | 4.2 |
| ACT score 25+ | 4.4 | .8 | 7.3 | 4.6 | .5 | 2.7 |
| Treatment × ACT 25+ | −11.6 | −1.0 | 4.7 | −10.6 | −.8 | 6.4 |
| ACT score 20 or below | −2.8 | −.8 | −.8 | −.7 | −.9 | −1.3 |
| Treatment × ACT 20 or below | −4.4 | −1.2 | −4.3 | −7.9 | −1.4 | −2.3 |
| Sample size | 818 | | | 630 | | |
| Variation by academic preparation (ACG): | | | | | | |
| Assigned to treatment | 17.0** | 2.0* | 14.0* | 17.8* | 1.7 | 13.1* |
| ACG receipt | 15.0** | 2.9*** | 9.8** | 14.5* | 2.6** | 9.0* |
| Treatment × ACG | −19.5** | −2.3* | −10.5 | −18.3* | −1.8 | −10.1 |
| Sample size | 828 | | | 639 | | |

NOTE.—Data are from UW System (retention and credits), NSC (four-year BA). Four-year graduation data from the NSC are available for cohort 1 only. ACG is awarded to students who completed a rigorous high school curriculum. Targeted minority groups include African-Americans, Latinos, Southeast Asians, Native Americans, and multiracial. "Targeted" refers to a policy of the UW System.

$^{+}P < .10$.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

TABLE 8

HETEROGENEOUS IMPACTS ON COLLEGE RETENTION, CREDITS OVER ONE YEAR, AND FOUR-YEAR DEGREE COMPLETION RATES ACCORDING TO INSTITUTIONAL CHARACTERISTICS (Cohorts 1–3)

| | COHORT 1 ADMIN SAMPLE | | | COHORT 1 FINANCIAL AID SAMPLE | | | COHORTS 2 AND 3 | |
|---|---|---|---|---|---|---|---|---|
| | Retention | Credits | Four-Year BA | Retention | Credits | Four-Year BA | Retention | Credits |
| Variation by institutional selectivity: | | | | | | | | |
| Assigned to treatment | −5.2 | .1 | 10.2 | −1.8 | .7 | 17.2 | 1.2 | .3 |
| Less selective college | −19.5*** | −3.1*** | −12.7*** | −16.8*** | −2.9*** | −13.0* | −12.8*** | −2.7*** |
| Treatment × less selective college | 9.7* | .3 | −6.6 | 6.7 | −.4 | −14.4 | 2.3 | .2 |
| Variation by Pell graduation rate at institutions: | | | | | | | | |
| Assigned to treatment | 13.5 | .3 | −20.1* | 18.9 | 1.2 | −22.5* | 8.9 | 1.5 |
| Pell graduation rate (six year) | .52*** | −.10*** | .32*** | .58*** | .11*** | .32* | .47*** | .11*** |
| Treatment × institutional Pell grad rate | −.21 | −.001 | .47*** | −.29 | −.02 | .52* | −.11 | −.019 |
| Variation by institutional aid per student: | | | | | | | | |
| Assigned to treatment | 1.1 | .0 | 4.5 | 4.1 | .3 | 6.2 | 2.6 | .3 |
| Institutional aid per student ($1,000s) | 6.0 | 1.0 | 15.2** | 6.9 | 1.1 | 15.6* | 8.5*** | 1.3*** |
| Treatment × institutional aid | 6.4 | 1.3 | 2.1 | −.5 | .6 | −2.7 | .8 | .4 |
| Sample size | 1,167 | | | 639 | | | 8,839 | |

NOTE.—Data are from the UW System (retention and credits) and the NSC (four-year graduation). Pell graduation rate is measured in percentage points. Four-year graduation data from the NSC are available for cohort 1 only. Institutional selectivity is determined by median ACT score. Ten of 13 universities had median ACT scores of 23 or below and are classified as less selective.

$^{+}P < .10$.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

of retention to the second year of college, but there is no evidence of differential impacts on degree completion rates or for the second and third cohorts of students served. The point estimate for the impacts on degree completion for the first cohort is negative and not statistically significant, and impacts on degree completion are not measured for the second and third cohorts.

Was the WSG more effective at boosting college persistence and degree completion rates for students attending universities where Pell recipients are generally already doing well? Higher rates of Pell student success could be another proxy for selectivity, but it might also indicate a more supportive environment for these students. The results suggest that for the first cohort of students, the grant offer generated larger impacts on on-time degree completion rates at institutions where the institutional graduation rate (over six years) for Pell recipients was higher. Specifically, for a 10 percentage point increase in a university's six-year degree completion rate for Pell recipients, the impact of the WSG offer on four-year degree completion rates increased by about 4.7 percentage points. But similar impacts are not observed for retention rates or credits, and these estimates cannot be confirmed with the second and third cohorts of students at this point. Moreover, the results provide no evidence of variation in treatment impacts based on the institutional financial aid budget—one factor that might be supportive of higher Pell recipient graduation rates.

## DISCUSSION

College attendance in the 21st century is normative, but college completion is not. Income inequalities in K–12 education are largely reproduced in postsecondary education, generating skepticism about the capacity of tertiary education to do much more than perpetuate stratification. Fifty years ago, federal policy makers began investing in need-based financial aid as a strategy for reducing income inequality in college attainment. While the effectiveness of financial aid is often assessed in terms of college attendance, higher education's ability to affect social mobility hinges in part on students from low-income families completing college degrees. This study provides new experimental evidence indicating that increasing need-based grant aid is an effective approach for inducing current students to remain enrolled in college, earn slightly more credits, and get somewhat better grades, contributing to improved rates of on-time (four-year) bachelor's degree completion. Moreover, grant aid contributes to the attenuation of inequality in college graduation. We find that before the introduction of the WSG, the expected gap in the on-time (four-year) bachelor's degree completion rate between the Pell Grant recipients in this sample (16%) and the average on-time (four-year) degree completion rate in the UW System (30%) was 14 percentage

points, but the offer of $3,500 in additional grant aid raised graduation rates to 21%, cutting that gap to 9 percentage points.[37]

While this study focuses on a group of Wisconsin undergraduates, the point estimates are similar to those obtained elsewhere. For example, in Florida, eligibility for $1,300 of need-based grant aid led to a 22% increase in bachelor's degree completion over six years (Castleman and Long 2013), while in Wisconsin the offer of a $3,500 grant boosted odds of on-time (four-year) degree completion by 29% (4.7 percentage points from a control group mean of 16.3%). In addition, it is worth noting that this study examined a program operated as it would in real life, rather than a trial program created for demonstration purposes. This further helps to enhance the generalizability of the results we obtain (Heckman 2005). It seems reasonable to suggest that the findings indicate that policy makers could improve rates of college completion (and perhaps reduce time to degree) among some students from low-income families by increasing the amount of grant aid offered.

The estimated differential impacts according to students' out-of-pocket costs before the WSG was awarded, and in turn how much additional income they received from the grant offer, suggest that students from low-income families benefit from having additional resources to cover their costs. Substituting grants for already-accepted loans is associated with smaller changes in academic outcomes than increasing students' income via grants, thus reducing their out-of-pocket costs.

The findings also suggest that the effects of additional economic capital may be mediated by the presence of social or cultural capital. For example, students with college-educated parents appear to have benefited more from the offer of the WSG. It may be that with their greater knowledge about how to navigate college, they were better equipped to strategize about how to translate the increased resources into a shorter time to degree. But, students with less academic preparation appear to have benefited more from the grant offer, perhaps because the impact of purchasing books or supplies with the new resources, or reducing work hours, was more helpful in their academic success. This finding may also suggest that programs with academic merit requirements for needy students may be reducing the effectiveness of their investments, which could be larger if targeted to those who just miss those requirements. This finding is consistent with several other recent studies in Florida (Castleman and Long 2013) and Louisiana (Crockett et al. 2011).

[37] We would prefer to use the on-time (four-year) graduation rate for non-Pell recipients in the UW System, rather than the average student, in this calculation, but that information is unavailable.

The evidence presented in this article also points to the importance of considering how program impacts evolve over time and across cohorts, replicating analyses with additional cohorts of students whenever possible. The short-term effects of the WSG on retention for the first cohort of recipients suggested a much more limited set of impacts that did not reveal the positive benefits for degree completion, while more years of data and comparisons to the results for the second and third cohorts of students indicate that more robust effects took place.[38] While we collected some evidence on program implementation over time, which indicated that compliance with the program rules improved and messages sent by the program to students became clearer, we cannot say for sure whether these explain the cohort variation. Further experimentation with the implementation and messaging of financial aid programs should be undertaken.

As with all studies, the analyses in this article have several limitations. First, there is a possibility of some bias in the analysis of heterogeneous effects since the sample was not blocked by these student characteristics before randomization and there is some differential attrition in the samples used. Second, several of the analyses may be underpowered, particularly for subsamples. Third, the results are based on a group of Wisconsin Pell Grant recipients who began college full-time despite having substantial unmet financial need and who persisted until the end of the term before the WSG arrived. The impacts of the WSG might be stronger if the grant were delivered earlier or was directed to part-time or otherwise needier students. The lead author is currently testing these hypotheses in a new experimental study of need-based grants.

This study also raises critical questions about the mechanisms through which those impacts operate and the factors moderating them (Harris and Goldrick-Rab 2012). The results regarding variability in the impacts of the WSG offer provide the most fertile ground for theory development and empirical testing. It is one thing to identify differential effects of a program like grant aid and quite another to account for them. Effect heterogeneity should be examined within the experimental framework whenever possible, ideally by stratifying the pretreatment sample by subgroup (Brand and Thomas 2013). It is also important to find ways to rigorously examine the potential mediators of effects of grant aid, for example, by considering alternative ap-

[38] The parent project for this study included a mixed-methods data collection strategy, and while analysis of the qualitative data is beyond the scope of this article, there is some evidence that program implementation could have affected the impacts of the grant, especially for the first cohort. Interviews with financial aid officers revealed variation in their understandings of the criteria regarding who was eligible for the grant, the conditions under which it could be renewed, and what messages they were to provide students about the award.

proaches to reducing students' work hours and then estimating impacts on academic outcomes.

Quite apart from the documentation of impacts, the question of how to translate research findings like these into policy recommendations is a very difficult one (Kelly and Goldrick-Rab 2014). While some scholars have encouraged the greater use of targeting of financial aid (Alon 2011), that strategy is often accompanied by significant trade-offs. Means testing creates divisions in political support for programs, and the politics of differentiating among poor people is fraught (Soss et al. 2011). It may be more possible to distribute financial aid to educational institutions on the basis of their admissions policies, to encourage broader access and enhance the achievement of students with lower prospects of graduation (Goldrick-Rab, Schudde, and Stampen 2014). But given the current emphasis of the Higher Education Act on facilitating college choice among all varieties of institutions (public, private, for profit) and debates over entitlement programs, rethinking the rules of aid programs rather than shoring up investments in those programs may be inadvisable. This political economy of financial aid and higher education policy is deserving of far greater attention among sociologists, since it is at least as important to the future of means-tested financial grants as the rigorous estimation of program impacts like that reported here.

Furthermore, while financial grant aid may reduce income inequality in college attainment rates, that does not necessarily imply that in turn income inequality among individuals will be similarly affected (Bowles and Gintis 2011). Since policy ambitions for higher education among political leaders often rest on the latter outcome, but the need for financial grant aid will not diminish if real family incomes do not rise, it is unclear whether investing in need-based financial aid is a sustainable strategy. Deserving of greater consideration are the personal and societal consequences of the current financial aid system, which reflects the norms of today's capitalist economy by using grants as vouchers to discount college costs, relying heavily on individual action and responsibility. Structuring the finance of higher education in this way may exert some positive effects for some students, while exacting broader implications in terms of how college is valued and who is responsible for its success. Alternatives such as providing some form of postsecondary education at no cost to families might be explored both in terms of their benefits for individual education attainment and for the labor market demand and wage premium accruing to college degrees—both of which contribute to income inequality.

## APPENDIX

TABLE A1
Descriptive Statistics and Baseline Equivalence by Institutional Characteristics (Cohorts 1–3)

| Characteristic | Cohort 1 | | | Cohorts 2 and 3 | | |
|---|---|---|---|---|---|---|
| | Sample Mean | Treatment Difference | Effect Size | Sample Mean | Treatment Difference | Effect Size |
| Median ACT score | 22.8 | .0 | .000 | 23.0 | −.0 | −.015 |
| | | (.1) | | | (.1) | |
| Percentage admitted (%) | 83.7 | .0 | .000 | 83.1 | .3 | .027 |
| | | (.6) | | | (.4) | |
| Attending less selective college (%) | 80.3 | .0 | −.001 | 77.0 | 1.1 | .039 |
| | | (2.2) | | | (1.4) | |
| Pell recipients (%) | 20.4 | .0 | .000 | 19.7 | .0 | .002 |
| | | (.3) | | | (.2) | |
| Six-year Pell graduation rate (%) | 53.1 | .0 | .000 | 53.7 | −.2 | −.018 |
| | | (.6) | | | (.4) | |
| Institutional aid/student ($) | 239 | 0 | .000 | 278 | 6 | .019 |
| | | (12) | | | (10) | |
| *N* | 1,500 | | | 8,897 | | |

Note.—Data are from the UW system. All estimates are the results of regressions without institutional fixed effects. Standard errors in parentheses. Cohort 1 had 2,557 students in the control group and 600 in the treatment group, but because of data agreements we are unable to observe the full sample. Effect sizes are calculated using OLS for continuous outcomes and logistic regression for binary outcomes.

TABLE A2

UNADJUSTED AND COVARIATE-ADJUSTED TERM-BY-TERM IMPACTS ON COLLEGE PERSISTENCE AND ACHIEVEMENT (Financial Aid Data Sample)

| | | Unadjusted | | Covariate Adjusted | |
|---|---|---|---|---|---|
| | Control Mean | Treatment Impact | Effect Size | Treatment Impact | Effect Size |
| Semester 1 (treatment began): | | | | | |
| Credits earned | 14.1 | −.0 | −.025 | .3 | .083 |
| | | (.3) | | (.2) | |
| Earned 12+ credits (%) | 88.1 | −.5 | −.033 | −.9 | −.111 |
| | | (2.5) | | (2.4) | |
| Cumulative GPA | 2.67 | .02 | .023 | −.01 | −.009 |
| | | (.08) | | (.07) | |
| 12+ credits and 2.0 GPA (%) | 76.0 | −3.7 | −.122 | −4.8 | −.217 |
| | | (3.6) | | (3.3) | |
| Semester 2: | | | | | |
| Enrollment (%) | 96.0 | −.3 | −.052 | −.1 | −.005 |
| | | (1.7) | | (1.7) | |
| Credits earned | 12.6 | −.0 | −.001 | −.1 | −.022 |
| | | (.4) | | (.4) | |
| Earned 12+ credits (%) | 77.2 | −4.0 | −.137 | −5.0 | −.220 |
| | | (3.5) | | (3.2) | |
| Cumulative GPA | 2.62 | .05 | .006 | .02 | .025 |
| | | (.07) | | (.06) | |
| 12+ credits and 2.0 GPA (%) | 70.0 | −4.8 | −.137 | −5.7 | −.224 |
| | | (3.9) | | (3.6) | |
| Semester 3: | | | | | |
| Enrollment (%) | 85.3 | 3.3 | .162 | −3.1 | .148 |
| | | (2.9) | | (2.9) | |
| Credits earned | 11.3 | .4 | .063 | .3 | .044 |
| | | (.5) | | (.5) | |
| Earned 12+ credits (%) | 69.4 | 4.7 | .135 | 4.0 | .117 |
| | | (3.9) | | (3.7) | |

| | | | | | |
|---|---|---|---|---|---|
| Cumulative GPA | 2.60 | .06 | .070 | .03 | .033 |
| | | (.07) | | (.06) | |
| 12+ credits and 2.0 GPA (%) | 65.0 | 1.9 | .050 | .4 | −.002 |
| | | (4.1) | | (3.8) | |
| Semester 4: | | | | | |
| Enrollment (%) | 79.8 | 1.6 | .061 | 1.7 | .058 |
| | | (3.4) | | (3.4) | |
| Credits earned | 10.4 | −.2 | −.036 | −.2 | −.040 |
| | | (.5) | | (.5) | |
| Earned 12+ credits (%) | 63.9 | −5.1 | −.133 | −5.1 | −.155 |
| | | (4.1) | | (4.0) | |
| Cumulative GPA | 2.60 | .06 | .072 | .03 | .035 |
| | | (.07) | | (.06) | |
| 12+ credits and 2.0 GPA (%) | 61.4 | −5.6 | −.143 | −6.0 | −.185 |
| | | (4.2) | | (4.0) | |
| Semester 5: | | | | | |
| Enrollment (%) | 74.0 | .9 | .028 | .9 | .021 |
| | | (3.7) | | (3.6) | |
| Credits earned | 9.7 | .5 | .069 | .4 | .062 |
| | | (.6) | | (.6) | |
| Earned 12+ credits (%) | 59.2 | 6.9 | .175 | 5.7 | .157 |
| | | (4.2) | | (4.0) | |
| Cumulative GPA | 2.60 | .06 | .074 | .03 | .037 |
| | | (.07) | | (.06) | |
| 12+ credits and 2.0 GPA (%) | 58.2 | 6.5 | .162 | 5.2 | .143 |
| | | (4.2) | | (4.0) | |
| Semester 6: | | | | | |
| Enrollment (%) | 72.1 | −2.4 | −.072 | −2.4 | −.085 |
| | | (3.8) | | (3.7) | |
| Credits earned | 9.2 | −.4 | −.064 | −.5 | −.072 |
| | | (.6) | | (.6) | |
| Earned 12+ credits (%) | 58.4 | −5.1 | −.128 | −5.5 | −.157 |

TABLE A2 (*Continued*)

| | Control Mean | Unadjusted | | Covariate Adjusted | |
|---|---|---|---|---|---|
| | | Treatment Impact | Effect Size | Treatment Impact | Effect Size |
| | | (4.2) | | (4.1) | |
| Cumulative GPA | 2.61 | .06 | .077 | .03 | .039 |
| | | (.07) | | (.06) | |
| 12+ credits and 2.0 GPA (%) | 56.9 | −5.3 | .162 | −5.9 | −.167 |
| | | (4.2) | | (4.1) | |
| Cumulative outcome: | | | | | |
| Credits earned | 68.9 | .6 | .024 | .2 | .009 |
| | | (2.2) | | (2.1) | |
| Cumulative GPA | 2.61 | .06 | .077 | .03 | .039 |
| | | (.07) | | (.06) | |
| 12+ credits each semester (%) | 38.4 | .3 | .007 | −.9 | −.026 |
| | | (4.2) | | (4.0) | |
| 12+ credits and 2.0 GPA each semester (%) | 37.3 | .0 | .001 | −1.2 | −.033 |
| | | (4.1) | | (4.0) | |
| BA completion rates (NSC): | | | | | |
| By semester 8 (%) | 18.1 | 4.9 | .197 | 4.2 | .170 |
| | | (3.3) | | (3.3) | |

Note.—Data are from the UW System, except where noted. Standard errors in parentheses. Retention includes any of the 13 four-year UW System universities, as well as the 13 two-year UW colleges. If a student was not enrolled in a given semester, the cumulative GPA from the previous semester is reported. Control mean is adjusted for university fixed effects only. The unadjusted column has no covariates, and the covariate-adjusted column includes race, gender, age, parental education, zero EFC status, dependency status, parent income, and immigration status. Effect sizes are calculated using OLS for continuous outcomes and logistic regression for binary outcomes. $N = 628$.

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