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Abstract

This quasi-experimental study examined the effectiveness of a one-time emergency financial relief program among Pell Grant eligible undergraduate students in Spring 2015 pursuing their first bachelor's degree across academic and financial outcomes. The academic outcomes included retention to the next semester, degree completion, attempted credit hours, and grade point average. The financial outcome captured whether students received a stop registration hold due to an unpaid financial balance in the semester after receiving the emergency relief. The results reveal that financial relief applied to low-income students' accounts can improve their retention and graduation rates. The financial relief was most effective among first-generation college students, resulting in a complete elimination of the retention gap for first-generation students. The emergency relief did not improve GPA or substantially change the number of credits earned. A concerning finding was that students receiving this emergency support were more likely to receive a financial hold in a subsequent semester and that effect was stronger among students of color (Black/African American, Hispanic/Latine, Asian, Multiracial, American Indian/Alaska Native), males, and first-generation college students.

Keywords: emergency funds, financial relief, low-income, first-generation college students, retention

The Role of Emergency Financial Relief Funding in Improving Low-Income Students' Academic and Financial Outcomes Across Demographic Characteristics

College students are increasingly unable to cover necessary expenses (Goldrick-Rab et al., 2022; Grodsky & Jones, 2007; Page & Scott-Clayton, 2016), and most college students report feeling stressed about finances (Heckman et al., 2014). In addition to the expected expenses (e.g., tuition, fees, textbooks), unexpected and emergency costs can be devastating to students' progress (Evans et al., 2019), sometimes leading to college departure (Krueger et al., 2016; Lowman et al., 2021). More than 40% of first-time enrolled students in four-year institutions do not complete a bachelor's degree within six years (NCES, 2020), and millions will not complete their degree at all (Engle & Tinto, 2008; Shapiro et al., 2017). Some institutions have tried to assist students by providing emergency relief with emergency federal (Goldrick-Rab et al., 2021) or institutional funds (Evans et al., 2020; Krueger et al., 2016), but little empirical research exists to determine whether such efforts are effective in preventing future financial crises for students or improving outcomes such as time to degree (Anderson et al., 2020; Lowman et al., 2021), and specifically for students of color, low-income, and first-generation college students.

Students of color, low-income, and first-generation college students experience additional barriers related to financial needs and support. Specifically, more than one third of Hispanic students were unaware of any financial aid source (O'Connor et al., 2010), and Black students—especially those who are first in their families to attend college—are less likely to know about financial aid options (Ramirez-Mendoza & Jones, 2020). Similarly, students from low-income families are less likely to receive high-quality information about financial aid opportunities and are less likely to file an application for financial aid (Goldrick-Rab et al., 2009; Long, 2008). Given rising college costs, disadvantages in accessing financial aid information could mean the difference between degree completion or attrition (Chen & DesJardins, 2010).

As word of mouth is still the primary method for disseminating financial aid information (Krueger et al., 2016), students who need emergency funding can easily miss information and deadlines. The recent pandemic, while setting back learning for all students, exacerbated hardships for low-income and students of color (Harper, 2020). Economic hardships, such as job losses, have left students of color and low-income students struggling to pay for necessities and academic needs (Amour, 2020; Curs et al., 2022). Some universities responded with small emergency financial awards; Canadian institutions in Ontario, for example, offered awards of $500-$1,500 through the Bursar's office (El Masri & Sabzalieva, 2020). Therefore, it is necessary to investigate whether emergency financial aid is helpful in improving postsecondary outcomes for students, particularly by race and income.

During the 2015 summer semester, the University of Missouri delivered a one-time financial relief program that waived eligible small outstanding financial balances for students. Eligible outstanding balances included expenses related to direct academic expenses (e.g., tuition, fees, course materials) but did not include other non-academic expenses (e.g., clothing purchases at the MU Bookstore). Students were unaware of this program and there was no application process as the balance waivers were automatically applied to eligible student's accounts. In total, the university waived $214,308 in outstanding balances for 797 students. The average waived balance was $269 and ranged from $10 to $4,000.

Specifically, our research questions are:

1) Was receiving a balance waiver through this one-time financial relief program associated with subsequent academic and financial outcomes?

2) Was the association between the financial relief program and student outcomes different across demographic characteristics, such as race, sex, first-generation status, or year in school?

This study contributes to our understanding of financial relief programs that institutions might implement themselves, and this research informs the current CARES Act (Coronavirus Aid, Relief, and

Economic Security) and HEERF (Higher Education Emergency Relief Funds) funding that serves a similar function on a larger scale (Schmidt & Weissman, 2021)

Relevant Literature

Students have increasing financial, unmet, and emergency needs. The rising cost of college is a major concern for students and their families (Mitchell et al., 2019; Zaloom, 2019). An increasing proportion of students are experiencing unmet need—where the expected cost of college exceeds what families can pay or cover through financial aid scholarships, grants, or loans (Walizer, 2018). Nearly three in four students have an unmet need and in public, four-year colleges the average size of that gap is $9,000 (Walizer, 2018). The unmet need among students of color and low-income students is even greater (Walizer, 2018; Lopez, 2013). Black students, regardless of the institution type that they attend, are found most likely to have unmet need compared to their peers from other racial/ethnic groups (Walizer, 2018).

Institutional relief funding programs to support students in financial need, which more than 70% of higher education institutions have, can take on different forms (e.g., vouchers, scholarships, loans, food access, restricted and unrestricted grants). These programs serve a variety of objectives, such as humanitarian aid, removing barriers to success, removing financial holds so that students can graduate, or to retain students (Krueger et al., 2016). Institutions are most likely to rely on donors and their own operating budget to procure these funds, but students' needs tend to exceed funding availability (Krueger et al., 2016).

Broadly, research on emergency financial aid programs has not found strong evidence of effects on student outcomes. Evans et al. (2019, 2020) studied institutional relief for low-income community college students and found that emergency aid, even when paired with a case management coaching and mentoring program, did not significantly improve student success (e.g., course enrollment, credits earned, GPA, or degree completion). A recent 11-institution study of public universities using

completion grants to assist students with unmet need through randomly applied small grants ($1,200 on average), resulting in a modest decrease in credit card usage to pay for college but no change in graduation rates (Goldrick-Rab et al., 2022). The state of Wisconsin tried a lottery-based, random assignment of one-time funds to increase degree completion and graduate school enrollment, which the program failed to improve (Anderson et al., 2020). There was some reduction in students' time-to-degree and a slight increase in the number of STEM degrees earned during the program (Anderson et al., 2020). Among students attending HBCU institutions, there is preliminary evidence that emergency funding is most helpful in reducing stress, helping with educational expenses for class, and maintaining enrollment, according to students' own perceptions (Dahl et al., 2022).

The initial evidence about one-time relief programs is that these funds do not significantly predict student persistence into the following semester (Lowman et al., 2021). Low-income students are particularly impacted by emergency events, such as COVID-19 (McKinnon-Crowley, 2023), and can particularly benefit from emergency aid (Rodríguez-Planas, 2020). Most students surveyed about receiving these emergency funds reported how this aid was helpful to them personally and academically, communicated care and concern from their institution, and believed that the money would increase their chance of graduating (Goldrick-Rab et al., 2021). Administrators have similar perceptions about the benefits of such funds helping students stay enrolled (Taylor & Melidona, 2021).

The financial relief program we are studying is small (i.e., awards were less than $4,000) and was a surprise to students. Thus, we will utilize the broad concept of scarcity as a theoretical framework for our research study. Generally, when people are faced with a scarcity of important resources (e.g., money, time), they are more likely to focus on immediate needs and have reduced bandwidth for other productive activities (Mullainathan & Shafir, 2013). In our study, students who received relief funds for small financial balances may be less likely to worry about finances and might then be able to focus their attention on their academic success. Previous research has shown that financial stress is increasing and

pervasive (Heckman et al., 2014) and has negative associations with academic outcomes (e.g., Baker & Montalto, 2019; Goldrick-Rab, 2016; Joo et al., 2008).

Research Methodology

In this section we describe the research methodology we utilized to estimate the relationship between receipt of the financial relief program and academic and financial outcomes. We begin by describing the data sources and variables utilized in our analysis. Next, we describe the target population and restrictions made to create the analytical sample - Pell Grant eligible students at the University of Missouri that were enrolled in the spring 2015 semester. We then describe our preferred empirical strategy, propensity score matching, and the decisions we made within the estimation process. We also discuss alternative estimation strategies utilized to demonstrate the robustness of our approach. Within our analysis, we also test for differential responses to the financial relief program across student demographic characteristics (e.g., race/ethnicity, sex, first-generation status, and year in school).

Data Sources and Analytic Variables

The data utilized for this study come from University of Missouri administrative records. Demographic information was collected from application records. Academic outcomes were collected from semester-level academic records. Treatment status was collected from financial aid records. Finally, registration holds were collected from a financial transaction data file that contains all records of official university correspondence related to student financial issues (e.g., past due balances and/or registration holds). All data sources were merged using a student-level unique identifier created for research purposes to protect the anonymity of the student records. Descriptive statistics of all variables utilized in the analysis for the analytic sample are provided in Table 1.

Dependent Variables

We investigated the effect of receiving the financial relief award on five dependent variables. The first dependent variable was *spring to fall semester retention*. Specifically, we classified a student as having been retained if they attempted and completed at least one 3-credit course in the subsequent fall semester. For the analytical sample, the mean spring to fall retention rate was 91%.

The second dependent variable was *completion of a bachelor's degree*. Specifically, we classified students as completing their bachelor's degree if they received their bachelor's degree from the University of Missouri by the Spring 2022 semester. Thus, our completion variable is censored at seven years beyond the financial relief treatment. For the analytical sample, the mean degree completion rate was 83%.

The third dependent variable was the number of *credit hours attempted* in the fall 2015 semester, the semester immediately after the implementation of the financial relief program. For the analytical sample (removing those that were not retained into the fall 2015 semester), the mean number of credit hours attempted in the fall 2015 semester was 13.2. This average credit load implies that the average student was completing a full-time course load (defined as 12 credits or more) but was not attempting enough credits to maintain a 4-year graduation pace (15 per semester).

The fourth dependent variable is the *grade point average* for fall 2015 coursework. The mean (removing those that were not retained into the fall 2015 semester) fall 2015 grade point average was 3.0, on a 0.0 to 4.0 scale.

The fifth dependent variable measures whether a student receives a *stop registration hold* due to financial delinquency during the fall 2015 semester. Stop registration holds are placed on a student's account if they have an outstanding financial balance that is greater than 60 days past due. Until the outstanding balance is taken care of, students face penalties that include the prevention of future class enrollment, requests for transcripts, and receipt of a diploma. For the analytical sample (removing those

that were not retained into the fall 2015 semester), 21% of students received a stop registration hold during the fall 2015 semester.

Treatment Variable

The treatment indicator variable represents whether a student *received the financial relief award*. Within the analytical sample, 6% of the students received this award. Within the analytic sample, the average balance that was waived (for those that received an award) was $198 and ranged from $10 to $4,000.

Covariates

We include covariates in our regression and matching models to control for factors related to prior academic achievement, prior financial factors, and how many years the student has been enrolled at the institution. Our rationale for covariate selection is described below when we describe our empirical strategy.

With respect to prior academic achievement, we include the student's cumulative grade point average through spring 2015, their spring 2015 grade point average, and their spring 2015 number of credit hours attempted. With respect to financial factors, we include information about the student's 2014-2015 academic year financial aid packaging including: their expected family contribution (EFC), and indicator variable as to whether they received institutional needs-based financial aid, and an indicator variable as to whether they received institutional merit-based financial aid. To capture prior financial troubles, we include indicator variables for whether they had a stop registration holds in spring 2015 or fall 2014 semesters, and variables that measure the balance sizes associated with those holds. Finally, we include indicator variables for whether the spring 2015 semester was their second, third, or fourth year at the institution (first year serves as the reference).

Demographic Variables

We utilize three demographic factors to test whether the treatment differentially affects students. First, we test for differences between students of color and white students. Specifically, we define students of color as students who self-reported their race/ethnicity on their application for admissions as Black/African American, Hispanic/Latine, Asian, Multiracial, or, American Indian/Alaska Native. Unfortunately, sample sizes for treated groups restricts the statistical power to analyze treatment effects for more segmented race/ethnicities.

Second, we test for differences across sex/gender. At the time of the financial relief program, in the application for admissions the University of Missouri asked students for their self-reported *gender* but provided *sex* (male or female) categories as the response options. Thus, we utilize the binary sex categories of male and female in our statistical reporting.

Third, we test for differences across first-generation status. The University of Missouri classifies a student as a first-generation college student based upon their response to a question on their admissions application asking, “Prior to your 18th birthday, did you either reside with or receive support from a parent who has a bachelor’s degree from a college or university?” Students responding “no” were classified as first-generation.

Fourth, we test for differences across the number of years a student was enrolled at the University of Missouri. We categorize students into years based upon when they first enrolled at the University of Missouri. Thus, we define a first-year student as someone who first enrolled at the University of Missouri during the academic year (fall 2014 to summer 2015) of the treatment (Summer 2015). A second year is defined as having started during the academic year prior to the treatment (fall 2013 to summer 2014). Third- and fourth-year students are similarly defined as second-year students.

Sample Characteristics

The base population for this study includes all Pell Grant eligible undergraduate students who were enrolled at the University of Missouri during the spring 2015 semester (n = 12,287). To create an analytic sample appropriate to analyze the effect of the financial relief program, we made a few sample restrictions. First, we removed all students who were ineligible for the financial relief treatment as they graduated during the spring 2015 or summer 2015 semesters (n = 1,772). Second, we removed students who started their bachelor's degree program prior to 4 years before the spring 2015 semester (n = 347). Thus, the potential analytical sample for this study was 10,168 Pell Grant eligible students who were within four years of starting their degree program. The final analytic sample 9,653 after the trimming procedure described in the following section.

Empirical Strategy

Our preferred estimation strategy employs a propensity score matching estimator to estimate the effects of the treatment on academic and financial outcomes. This quasi-experimental procedure allows us to make causal estimates of the treatment effect under two primary assumptions: the unconfoundedness assumption and the overlap assumption (Caliendo & Kopeinig, 2008; Imbens, 2015). The following three steps describe our approach to making the decisions necessary to estimate the propensity score matching treatment effect within the context of the two primary assumptions.

Step One – Choose the Covariates and Estimate the Propensity Score

Following the guidance of Imbens (2015), the first step of our propensity score matching strategy is to choose the set of covariates to include in the propensity score model. Our choice of covariates was guided by the unconfoundedness assumption, which states "given a set of observable covariates X which are not affected by treatment, potential outcomes are independent of treatment assignment" (Caliendo & Kopeinig, 2008, p. 35). Thus, all constructs that affect both treatment status and the outcome must be observed and accounted for by the researchers. This assumption is inherently

untestable, and scholars have stated that the most credible strategies for satisfying this assumption come through the richness of the data and detailed understanding of the treatment selection process (Blundell et al., 2005; Imbens, 2015; Steiner et al., 2010).

We utilized the detailed administrative longitudinal data on both academic constructs (i.e., cumulative GPA, spring 2015 GPA, spring 2015 credit hours passed) and financial outcomes (e.g., EFC, spring 2015 registration hold indicator and amounts, fall 2014 registration hold and amounts, and indicators for receipt of institutional need- and merit-based grants) to account for factors likely to be related to both treatment status and subsequent academic outcomes. We also include indicators of whether a student is in their second, third, and fourth year at the University of Missouri (students in their first year are the reference group).

Table 2 presents estimation of the propensity score model for the full sample (column 1) and the sample used for treatment effects after the trimming process in step 2 (column 2). With respect to academic factors, the probability of receiving treatment was positively associated with cumulative GPA and the number of credit hours passed in spring 2015 (the semester preceding the financial treatment). With respect to financial factors, the treatment was negatively related to a student's EFC and positively related to students having received a stop registration hold in the spring 2015 and fall 2014 semesters. First year students were more likely to receive the treatment when compared to second-, third-, or fourth-year students.

Step Two – Trim the Sample

The overlap assumption "ensures that persons with the same X values have a positive probability of being both participants and nonparticipants" (Caliendo & Kopeinig, 2008, p. 35). After estimating the propensity scores on the full sample, the overlap assumption can be tested through inspection of the overlap of propensity scores for the treated and untreated groups. Figure 1 presents the propensity score distributions for the treated and untreated groups. The vertical dashed lines

represent the boundaries for which there is an overlap between the propensity scores for the treated and untreated groups. Following guidance from the propensity score literature (e.g., Caliendo & Kopeinig, 2008, Imbens, 2015) we removed observations with propensity scores outside of this range of overlap. Specifically, we removed 158 observations whose propensity score was less than 0.0062 and 357 observations with propensity scores above 0.3757. Propensity scores were then re-estimated (presented in column 2 of Table 2) on the final analytic sample of 9,653 observations.

Step Three – Estimate the Treatment Effects

After choosing the covariates for the propensity score model and trimming the sample, we estimated the treatment effects using a propensity score matching estimator. Our strategy utilized a single nearest neighbor from the comparison group to match to each treated observation. Specifically, average treatment effects using propensity score matching were estimated using the *teffects psmatch* command in Stata SE 17 (StataCorp, 2021).

Figure 2 presents the standardized differences for each covariate used in the analysis between the treated and untreated groups. Circles represent the standardized differences in the raw data and triangles for the matched samples. For the raw data, concerning standardized differences (i.e., greater than 0.25) were observed for variables that measured a student's EFC and whether they had a registration hold in spring 2015. After matching, the differences were reduced (all less than 0.1) providing evidence that the matching process reduced potential concerns of the validity of the unconfoundedness assumption.

Alternative Estimation Strategies. Imbens (2015) suggests providing estimates from alternative treatment effects estimators to check for robustness across estimation techniques. Thus, we also present estimates from two alternative treatment effects estimation strategies. First, we present results using a linear regression with covariate adjustment strategy. All covariates described in the covariates section above will be included in the linear regression specification.

Second, a nearest neighbor matching design using a Mahalanobis distance allows us to compare similar students based upon observable characteristics who did and did not receive the financial relief treatment. Specifically, we forced matches to be exact based upon the year of enrollment for each student (first, second, third, or fourth year of enrollment) to make sure that treated students were matched to non-treated students who were in similar places in their academic careers. Second, we included all the covariates previously described within our matching process to help predict whether a student received the treatment. The nearest neighbor matching estimates were obtained through the *teffects nnmatch* command in Stata SE 17 (StataCorp, 2021).

Results

Table 3 presents the results with respect to our first research question, what was the effect of receiving a balance waiver through this financial relief program on subsequent academic and financial outcomes? Results are presented for all five dependent variables for three analytical treatment effects methods; (a) propensity score matching (PSM); (b) linear regression with covariate adjustment (OLS); and (c) nearest neighbor matching (NNM). Interpretation of coefficients will be focused on our preferred estimation strategy – propensity score matching, with comparison to the other two specifications (OLS and NNM) when relevant.

The spring 2015 to fall 2015 retention rate for the comparison group (i.e., students who did not receive financial relief treatment) was 91.0%. The financial relief treatment was estimated to be positively associated with spring-to-fall retention for all three empirical specifications. The estimate from the PSM specification indicates that students who received the financial relief treatment were 4.2 percentage points ($p < .01$) more likely to be retained (i.e., their likelihood of retention increased from 91.0% to 95.2%). Across our three specifications, the financial relief treatment effect estimate was robust, ranging from 3.7 (OLS) to 5.3 (NNM) percentage points.

The financial relief treatment was also positively associated with the likelihood of graduating with a bachelor's degree from the University of Missouri. The graduation rate for the comparison group (i.e., students who did not receive the financial relief treatment) was 82.6%. The PSM treatment effect estimate indicates that receipt of the financial relief treatment increased a student's likelihood of graduation by 5.3 percentage points ($p < .01$), to an estimated graduation rate of 87.9%. The treatment effect on graduation ranged across our empirical specifications from 2.9 (OLS) to 5.8 (NNM) percentage points.

For the final three dependent variables, the analytical sample was reduced to students who were retained (and thus enrolled) during the fall 2015 semester ($n = 8,814$). The fall 2015 credit hours attempted for untreated students was 13.2 credit hours. The estimate from our preferred specification (PSM) was positive (0.21) but not statistically significant ($p > .1$) indicating that the financial relief treatment was not associated with credit hours attempted in the subsequent semester. The other two specifications produced positive (0.27 for OLS and 0.34 for NNM) and statistically significant ($p < .05$) effects of the financial relief treatment on credit hour attempted. However, the practical significance of these effects estimates was small, as they indicate a change in credit hours attempted of less than a third of a credit hour.

The financial relief program was not found to be practically (i.e., point estimates are very small) or statistically ($p > .1$) associated with a student's GPA in the following semester for all three specifications. The comparison group's mean fall 2015 semester GPA was 3.04 and the treatment effect point estimates ranged from -0.03 (OLS) to 0.03 (NNM).

The financial relief treatment was positively associated with the likelihood a student receives a registration hold due to a past due balance of at least 60 days in the fall 2015 semester. For context, the registration hold rate for students who did not receive treatment was 20%. The PSM treatment effect estimate indicates that receipt of the financial relief treatment increased a student's likelihood of a fall

2015 registration hold by 4.7 percentage points ($p < .05$), or an estimated registration hold rate of 24.7%. The treatment effect on registration holds rates ranged across our empirical specifications from 3.8 (OLS) to 5.1 (NNM) percentage points.

Heterogeneity in Treatment Effect Estimates by Demographic Factors

Tables 4 and 5 present the results with respect to our second research question, was the effect different across demographic characteristics, such as race/ethnicity, sex, first-generation status, or year in school? Results of heterogeneous treatment effects are presented for all five dependent variables for when samples were separated by demographic factor. For brevity, the results are only presented for the propensity score matching estimation strategy.

Race/Ethnicity

Table 4, panel A, presents the propensity score estimates of the treatment effect for students of color and white student separately. Point estimates indicate that the financial relief program had a smaller association with retention for students of color students (3.7 percentage points, $p < .1$) compared to white students (4.8 percentage points, $p < .01$). However, the 95% confidence intervals for each coefficient overlapped indicating that these estimates may not be statistically significantly different. Similarly, the estimated effect sizes on bachelor's degree receipt were larger for white students (5.2 percentage points, $p < .05$) when compared to students of color students (3.8 percentage points, $p > .1$). Combined, the heterogeneity in treatment effects would indicate that the financial relief program was beneficial for both students of color and white students with respect to retention and graduation. However, the point estimates would indicate that the program may exacerbate already existing gaps in retention and graduation between these groups.

Consistent with the results from the full sample, the financial relief program's estimated effects on academic outcomes were either practically small or statistically insignificant when separated by race/ethnicity. The financial relief program had a larger association in increasing credit hours attempted

for white students (0.39, $p < .05$) than students of color (0.12 hours, $p > .1$), although the effect sizes remain practically insignificant given their small sizes (i.e., less than a third of a credit hour for white students). There were no statistically significant estimates with respect to fall 2015 GPA for either students of color or white students.

The financial relief program was positively related to registration holds for students of color students (11.0 percentage points, $p < .05$) but not for white students (2.3 percentage points, $p > .1$).

Sex/Gender

The financial relief treatment was estimated to increase retention (5.1 percentage points, $p < .01$) and bachelor's degree recipients (3.5 percentage points, $p < .05$) for female students. However, for male students the financial relief treatment was not associated with changes in retention ($p > .1$) or graduation ($p > .1$). The financial relief treatment was estimated to increase credit hours attempted for male students (0.54 hours, $p < .05$) while not significantly increasing credit hours attempted for female students ($p > .1$). There were no statistically significant estimates with respect to fall 2015 GPA for either female or male students. A positive association with fall 2015 registration holds was found for male students (10.0 percentage points, $p < .05$) but not female students ($p > .1$).

First-Generation

The positive effect of the financial relief program on retention was the largest for first-generation students (7.6 percentage points, $p < .01$), almost triple the size when compared to continuing-generation students (2.4 percentage points, $p < .1$). In fact, the treatment was associated with a complete elimination of the retention gap for first-generation students. For untreated students, first-generation students had a spring 2015 to fall 2015 retention rate of 88.7% compared to a 92.5% for continuing-generation students. With the financial relief treatment, the retention rates were 96.3% for first-generation students and 94.9% for continuing-generation students. The treatment effect of the

financial relief program on graduation rates was similar for first-generation students (6.0 percentage point, $p < .1$) and continuing-generation students (5.8 percentage points, $p < 0.1$).

The financial relief treatment was estimated to increase credit hours attempted continuing-generation students (0.46 hours, $p < .05$) while not significantly increasing credit hours attempted first-generation students (0.42, $p > .1$). There were no statistically significant estimates with respect to fall 2015 GPA for either first-generation or continuing-generation students. A positive association with fall 2015 registration holds was found for first-generation students (10.7 percentage points, $p < .05$) but not continuing-generation students ($p > .1$).

Year in School

Table 5 presents the results for the propensity score matching specification separately by the year of enrollment at the University of Missouri. The estimates of the effect of the financial relief program on retention were found to be largest for fourth-year students (i.e., those that had finished their fourth year and were transitioning into their fifth year) with an estimated increase in retention of 7.3 percentage points ($p < .1$), followed by second-year students (6.1 percentage points, $p < .1$), first year students (5.8 percentage points, $p < .01$), and third-year students (3.4 percentage points, $p < .05$). The estimates of the financial relief treatment on graduation were similar across academic years, ranging from 3.6 percentage points for second-year students to 4.8 percentage points for third-year students, although the points estimates were only statistically significant for third-year students ($p < .01$) and fourth-year students ($p < .1$).

The estimated financial relief treatment effects on fall 2015 credit hours attempted across academic year show quite a bit of variation. The treatment was associated with increased credit hours for first-year students (0.5 credit hours, $p < .01$) and third-year students (0.9 credit hours, $p < .01$). For third-year students this translates into almost a third of a typical 3-credit course. Alternatively, receipt of

the financial relief treatment was associated with a 1.2 credit hour reduction for fourth-year students. The variance in treatment effects across academic years is puzzling.

Consistent with the previous findings, the financial relief treatment was not found to be related to GPA for any of the academic year subgroups. Interestingly, there were no statistically significant treatment effects on fall 2015 registration holds for any academic year subgroups.

Heterogeneity in Treatment Effects by Treatment Amounts

Given the relative robustness of the findings regarding the treatment effect for retention, we wanted to explore whether the treatment effect varied by the dollar amount. To estimate whether the treatment effect of the financial relief program varied by the size of the financial relief award we estimated dose response functions (Bia & Mattei, 2008; Hirano & Imbens, 2005). The dose response function estimates the average treatment effect for a particular treatment level (i.e., ATE(t)) within a generalized propensity score framework. We estimated the dose response function using the same set of control variables as our PSM models using the *ctreatreg* function in Stata 17 (Bia & Mattei, 2008).

For retention, the dose response function approach estimated an average treatment effect of 0.51 ($p < .01$), which falls within the range of the PSM (0.42) and NNM (0.53). Figure 3 presents the dose response function, in which the solid line can be interpreted as the average treatment effect for a given treatment level and the dashed lines as the 95% confidence interval of those estimates. The dose response function would indicate the average treatment effect of the financial relief program on retention was near zero for treatment levels below about $800. At $800 we see an increasing average treatment effect that peaks at about $2,600 and starts to decline at about $3,200. This would suggest that the financial relief program was most successful at influencing retention for students that received between $800 and $3,200 in their relief award. However, the 95% confidence intervals include zero for all values treatment likely due to the small sample size of the treated group. Thus, caution should be used when interpreting the relative effectiveness of the financial relief award across treatment values.

Limitations

One limitation of the study is the limited scope of a unique, one-time program at one institution. The program is relatively low-cost and easy to implement, which institutions might find beneficial. Still, additional research is needed to determine whether these results mirror other large-scale financial relief programs or are replicable on other campuses.

A second limitation is that we were not involved in the program's administration and do not know if students were notified of the financial relief awards or what those notifications may have said, which may have influenced how the students felt about the awards. Without qualitative data, we are unable to speak to their reactions or states of mind about the relief awards or how they perceived their financial situations in general.

A third limitation is that in the semester after this financial relief funds were dispersed, the University of Missouri experienced protests against racism and inequitable resources for students of color, particularly Black and African American students, resulting in national attention and major administrative leadership changes (American Council on Education, 2018). If the events of fall 2015 differentially affected students who did and did not receive the treatment, this would be a concern with our design. To minimize some of these concerns, we also present estimates separately by race to account for the fact that students of different races may have experienced the events in fall 2015 differently.

Finally, as with any quasi-experimental research methodology, we rely upon the assumptions of the model to allow causal interpretations of the treatment effect estimates. In our case, unconfoundedness assumption is the key assumption to whether our estimates are internally valid. Our approach relies upon the richness of the administrative data utilized in this study, including detailed academic and financial records to satisfy the requirement that sufficient observed factors can remove selection on the unobservable factors.

Discussion

College students are increasingly unable to cover necessary expenses (Grodsky & Jones, 2007; Page & Scott-Clayton, 2016), and 70% of college students report feeling stressed about finances (Heckman et al., 2014). In addition to the expected expenses (e.g., tuition, fees, textbooks), unexpected and emergency costs can be devastating to students' progress (Evans et al., 2020), sometimes leading to college departure (Krueger et al., 2016; Lowman et al., 2021). Students of color and low-income college students experience additional barriers related to financial needs and support (O'Connor et al., 2010; Ramirez-Mendoza & Jones, 2020), including higher incidences of past due financial balances that lead to registration holds and other sanctions (Curs et al., 2022). Some institutions have tried to assist students by providing emergency relief with emergency federal (Goldrick-Rab et al., 2021) or institutional funds (Krueger et al., 2016), but little empirical research exists to determine whether such efforts are effective in preventing future financial crises for students or improving outcomes such as time to degree (Anderson et al., 2020; Lowman et al., 2021), and specifically for students of color, low-income, and first-generation college students.

Despite administrator perceptions of the benefits of emergency funding for students (Taylor & Melidona, 2021), previous research has mainly found that emergency relief does not lead to improvements in degree completion (Anderson et al., 2020; Evans et al., 2019, 2020; Goldrick-Rab et al., 2022) or persistence into the following semester (Lowman et al., 2021). Although students' perceptions of such support can be positive and seen as an indication of institutional care and a boost toward graduation (Goldrick-Rab et al., 2021). Our results revealed that a surprise financial relief program that reduced outstanding balances for Pell eligible students was positively related to students' retention and graduation rates. This contributes to the growing research on whether institutional relief funds (Krueger et al., 2016) have any effect on student outcomes (Anderson et al., 2020; Lowman et al., 2021). Improving retention and graduation rates are longstanding concerns within higher education and these

results offer some promise about being able to improve those outcomes at a relatively small cost. At the same time, waiving past due balances is helping students return to the university and persist, but it is not impacting their ability to afford college and avoid financial holds in future semesters.

A concerning finding related to receiving the emergency funding was that students were more likely to receive a financial hold in a subsequent semester and that effect was stronger among students of color (Black/African American, Hispanic/Latine, Asian, Multiracial, American Indian/Alaska Native), males, and first-generation college students. It is unclear why the emergency aid led to greater financial hardship in a subsequent semester. Future research is needed to determine whether students developed an expectation that future balances would be similarly waived or perhaps they overestimated the benefit of the award and were less careful with their spending, leading to financial challenges in a subsequent semester.

Financial concerns tend to be higher among students of color (O'Connor et al., 2010; Ramirez-Mendoza & Jones, 2020) and low-income college students (Goldrick-Rab et al., 2009; Long, 2008), particularly post-pandemic (Amour, 2020; Harper, 2020). The results of this study offer evidence that the relief intervention had a larger retention and graduation effect among white students when compared to students of color, although both groups benefitted. All the students in the study were low-income, so these results offer evidence of an effective intervention for students who tend to have the highest unmet financial need (Walizer, 2018; Lopez, 2013). The intervention was also more effective on retention and graduation among first-generation college students, with increases of 7.6% in the retention rate to fall semester and 6% in the graduation rate for this group. These results are promising given the high attrition rates among first-generation college students (Ishitani, 2003) and their unique college experiences (Garriott et al., 2023). Nationally, the bachelor's degree attainment rate for first-generation college students is about half that of continuing-generation college students (20% vs. 42%,

respectively), with cost cited as the biggest factor preventing degree completion (Redford & Hoyer, 2017).

Our findings had a negligible relationship to subsequent term academic outcomes, including GPA and credit hours attempted. This is consistent with past findings that also found that emergency institutional relief did not improve GPA or substantially change the number of credits earned (Evans et al., 2017, 2019). These results are contrary to past research on merit aid funding of similar amounts (Curs & Harper, 2012), where students would be notified in advance of their awards and would have an incentive to maintain a certain GPA to retain the award in subsequent years. Since emergency relief funding is a surprise that clears a past due balance and holds no promise of future relief, it might make sense that students' GPAs were unaffected.

Implications for Research and Practice

This financial relief program was effective in improving the retention rates of the full sample of low-income students and was most effective for first-generation college students. The program resulted in a temporary elimination of the retention gap, particularly among first-generation college students. The ease of the program from the students' perspectives, given that they did not have to apply for funding—which low-income students are less likely to do (Goldrick-Rab et al., 2009; Long, 2008)—might be something to consider maintaining in future iterations of the program. Staff may have also found this to be helpful in that applications did not need to be collected or reviewed. Finding ways to continue the program through advancement or other institutional funds would be helpful while also determining whether similar, relatively small, amounts of continued support would help address the graduation gap that persists long-term and is left unaddressed by this short-term program. Donors and administrators deciding how to apply similar funds might be moved by the effectiveness that $214,308 had for nearly 800 students. As a reminder, the average waived balance was only $269 and ranged from $10 to $4,000.

Further research is needed to determine whether there is an ideal range of balance waivers that institutions should consider to maximize the effectiveness of these funds.

Returning to our theory of scarcity and the hope that the program would have allowed students to worry less about finances and improve their academic attention and success (Mullainathan & Shafir, 2013), the results did not find such a connection. Since the program had no meaningful effect on grades or credits earned, this program should not be relied upon for academic outcomes. It may be that the funds were not substantial enough for the students to feel a sense of relief about their finances. Pairing this intervention with other forms of support might be another strategy to address the increased risk of financial trouble these students experienced in a subsequent semester. Related to that, further research is needed to determine why this financial relief led students to be more likely to run into financial difficulties. Qualitative research would help discover students' thought processes and reactions to receiving this support and how their behaviors or decisions changed after receiving the funds. Understanding this from students would help determine what kinds of further support would be helpful (e.g., financial literacy programs, budgeting support, tuition payment plans).

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Table 1

Descriptive Statistics of the Analytic Sample

	Mean	S. D.
Dependent Variables		
Retained (Fall 2015)	0.91	0.28
Graduated (Ever)	0.83	0.38
Credit Hours (Fall 2015) [a]	13.23	3.06
GPA (Fall 2015) [a]	3.04	0.79
Registration Hold (Fall 2015) [a]	0.21	0.41
Treatment Variables		
Treatment Receipt (0,1)	0.06	0.25
Treatment Amount ($) [b]	197.87	413.47
Covariates		
Cumulative GPA	3.03	0.58
GPA – SP15	2.99	0.80
Hours Passed – SP15	12.99	3.34
Expected Family Contribution ($1,000s) – 14/15AY	8.85	8.35
Registration Hold (0,1) – SP15	0.35	0.48
Registration Hold Amount ($1,000s) – SP15	0.33	1.05
Registration Hold (0,1) – F14	0.21	0.41
Registration Hold Amount ($1,000s) – F14	0.37	1.16
Received Institutional Merit Aid (0,1) – 14/15AY	0.39	0.49
Received Institutional Needs-Based Aid (0,1) – 14/15AY	0.34	0.47
First-Year Student	0.38	0.49
Second-Year Student	0.29	0.45
Third-Year Student	0.24	0.43
Fourth-Year Student	0.09	0.28
N	9,653	

[a] Calculated based upon the sample of students that were retained (n = 8,814).

[b] Calculated based upon the sample of students that received the treatment (n = 624).

Table 2

Propensity Score Model Estimates

	Full Sample	Trimmed Sample
Cumulative GPA	0.675***	0.686***
	(0.135)	(0.135)
GPA – SP15	0.016	0.016
	(0.104)	(0.104)
Hours Passed – SP15	0.032**	0.032**
	(0.016)	(0.016)
Expected Family Contribution ($1,000s) – 14/15AY	-0.071***	-0.072***
	(0.007)	(0.007)
Registration Hold (0,1) – SP15	1.189***	1.193***
	(0.098)	(0.098)
Registration Hold Amount ($1,000s) – SP15	0.006	0.005
	(0.039)	(0.039)
Registration Hold (0,1) – F14	0.449***	0.461***
	(0.120)	(0.120)
Registration Hold Amount ($1,000s) – F14	-0.023	-0.024
	(0.040)	(0.040)
Received Institutional Merit Aid (0,1) – 14/15AY	0.119	0.110
	(0.092)	(0.092)
Received Needs-Based Aid (0,1) – 14/15AY	-0.099	-0.104
	(0.093)	(0.093)
Second Year Student	-0.392***	-0.395***
	(0.103)	(0.104)
Third Year Student	-0.757***	-0.758***
	(0.119)	(0.120)
Fourth Year Student	-0.438***	-0.441***
	(0.161)	(0.161)
N	9,814	9,653

Note. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3

Treatment Effects Estimates of the Financial Relief Award by Outcome

	Retained (Fall 2015)	Graduated (Ever)	Credit Hours (Fall 2015)	GPA (Fall 2015)	Registration Hold (Fall 2015)
Propensity Score Matching	0.042***	0.053***	0.205	-0.024	0.047**
	(0.014)	(0.020)	(0.137)	(0.037)	(0.020)
OLS w/ covariates	0.037***	0.029**	0.274**	-0.029	0.038**
	(0.011)	(0.014)	(0.121)	(0.026)	(0.016)
Nearest Neighbor Matching	0.053***	0.058***	0.335**	0.028	0.051**
	(0.008)	(0.014)	(0.141)	(0.031)	(0.023)
Outcome Mean Of Untreated	0.91	0.83	13.22	3.04	0.20
N	9,653	9,653	8,814	8,814	8,814

Note. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4

Heterogeneity of Propensity Score Matching Treatment Effects by Selected Demographics

	Retained (Fall 2015)	Graduated (Ever)	Credit Hours (Fall 2015)	GPA (Fall 2015)	Registration Hold (Fall 2015)
Panel A: Differences across Race and Ethnicity					
Students of Color	0.037*	0.038	0.116	0.082	0.110**
	(0.019)	(0.036)	(0.227)	(0.068)	(0.047)
Untreated Mean	0.907	0.797	13.07	2.841	0.267
N	2,380	2,380	2,171	2,171	2,171
White Students	0.048***	0.052**	0.387**	-0.023	0.023
	(0.011)	(0.022)	(0.156)	(0.043)	(0.026)
Untreated Mean	0.914	0.839	13.27	3.109	0.181
N	7,111	7,111	6,515	6,515	6,515
Panel B: Differences across Sex					
Female Students	0.051***	0.035**	-0.049	-0.084	0.021
	(0.011)	(0.018)	(0.176)	(0.053)	(0.030)
Untreated Mean	0.917	0.851	13.55	3.176	0.189
N	5,099	5,099	4,692	4,692	4,692
Male Students	0.026	0.009	0.537**	-0.005	0.100**
	(0.025)	(0.028)	(0.243)	(0.062)	(0.039)
Untreated Mean	0.905	0.801	12.85	2.894	0.220
N	4,313	4,313	3,913	3,913	3,913
Panel C: Difference across First Generation Status					
First Generation Students	0.076***	0.060*	0.418	-0.012	0.107**
	(0.015)	(0.033)	(0.275)	(0.060)	(0.051)
Untreated Mean	0.887	0.787	13.08	2.953	0.211
N	3,755	3,755	3,349	3,349	3,349
Continuing Gen. Students	0.024*	0.058***	0.458**	-0.031	0.000
	(0.014)	(0.019)	(0.194)	(0.052)	(0.027)
Untreated Mean	0.925	0.851	13.30	3.091	0.196
N	5,900	5,900	5,470	5,470	5,470

Note. All estimates were estimated on subsamples using the propensity score matching estimation strategy. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5

Heterogeneity of Propensity Score Matching Treatment Effects by College Year

	Retained (Fall 2015)	Graduated (Ever)	Credit Hours (Fall 2015)	GPA (Fall 2015)	Registration Hold (Fall 2015)
First-Year	0.058***	0.047	0.531***	0.030	0.009
Students	(0.015)	(0.029)	(0.155)	(0.051)	(0.030)
Untreated Mean	0.905	0.758	13.26	3.013	0.183
N	3,663	3,663	3,333	3,333	3,333
Second-Year	0.061*	0.036	-0.424	-0.048	0.018
Students	(0.032)	(0.059)	(0.261)	(0.086)	(0.054)
Untreated Mean	0.917	0.847	13.41	3.043	0.195
N	2,711	2,711	2,495	2,495	2,495
Third-Year	0.034**	0.048***	0.914***	-0.022	0.027
Students	(0.014)	(0.017)	(0.299)	(0.086)	(0.069)
Untreated Mean	0.937	0.902	13.41	3.169	0.211
N	2,161	2,161	2,028	2,028	2,028
Fourth-Year	0.073*	0.047*	-1.202***	-0.072	0.170
Students	(0.043)	(0.025)	(0.407)	(0.245)	(0.122)
Untreated Mean	0.857	0.866	11.84	2.826	0.317
N	724	724	619	619	619

Note. All estimates were estimated on subsamples using the propensity score matching estimation strategy. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 1

Overlap of Propensity Scores

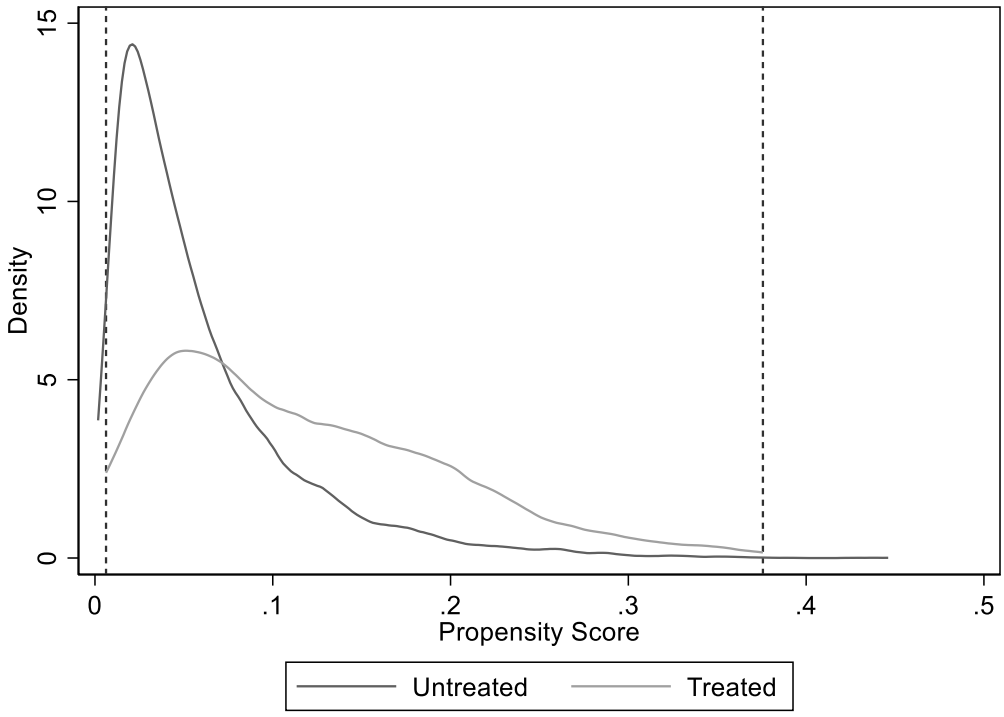

Figure 2

Standardized Differences for Selected Covariates

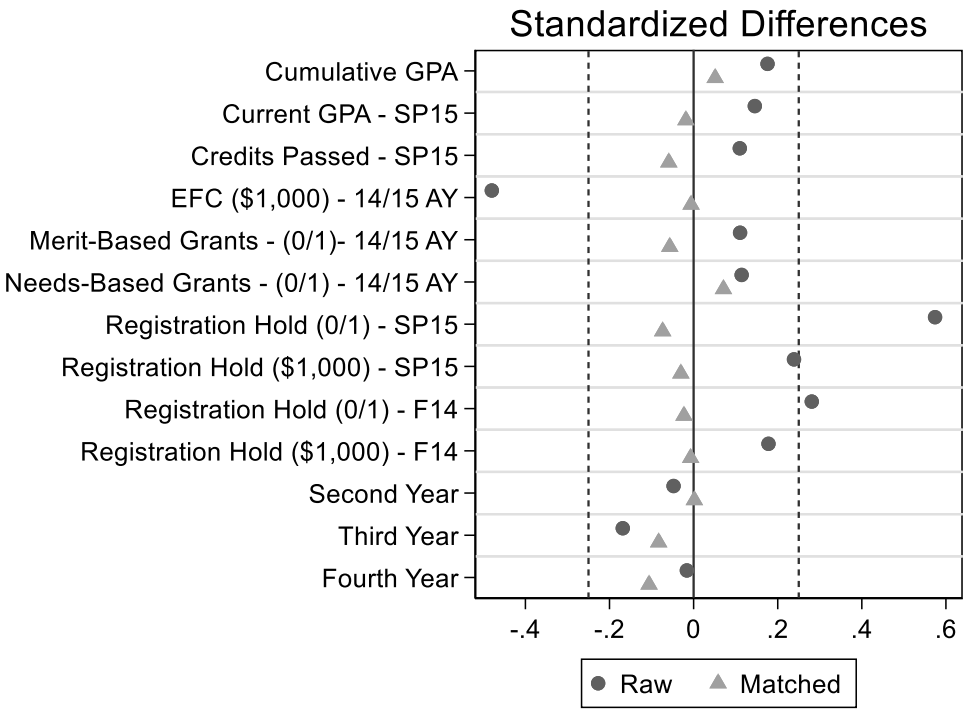

Figure 3

Dose Response Function of Treatment Amount on Retention