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ABSTRACT

Many male and first-generation college-goers struggle in their first year of postsecondary education. Mentoring programs have been touted as a potential solution to help such students acclimate to college life, yet causal evidence on the impact of such programs, and the factors that influence participation in them, is scant. This study leverages a natural experiment in which peer advisors (PAs) were quasi-randomly assigned to first-year university students to show that 1) male students were significantly more likely to voluntarily meet with their assigned PA when the PA was also male and 2) these compliers were significantly more likely to persist into the second year of postsecondary schooling. We find no effect of being assigned to a same-sex PA on female students' use of the PA program, nor do we find any evidence that the PA program affected subsequent academic performance (GPAs).

JEL Classification Codes: I21, I23, I28

Key Words: higher education; peer advising; mentoring; gender gap; demographic mismatch; retention

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Females now attend and complete college at significantly higher rates than males, and a similar gap exists between students from high- and low-income backgrounds (Bailey and Dynarski 2011; Bound and Turner 2011). These gaps concern policymakers who desire equal educational, social, and economic opportunities for all students (e.g., Rodríguez-Planas 2012b), as mounting evidence suggests that education, particularly postsecondary education, improves a host of long-term socioeconomic outcomes, including earnings (Blundell, Dearden, and Sianesi 2005; Card 1999), civic engagement (Dee 2004a; Milligan, Moretti, and Oreopoulos 2004), health (Grossman 2006), and crime (Lochner and Moretti 2004; Machin, Marie, and Vujic 2011). Moreover, there are social benefits (i.e., positive externalities) to a more educated population (Moretti 2004a,b).

It is important, then, to identify the cost-effective interventions available to policymakers and college and university administrators that might close sociodemographic gaps in postsecondary educational success, particularly once students have matriculated, since male and first-generation college students often struggle in their first year of postsecondary education. Indeed, conditional on matriculation, there are similar sociodemographic gaps in college completion (Bound and Turner 2011). Advising and mentoring programs constitute one class of potentially beneficial interventions, as such programs that provide information, guidance, and general support to students who lack these resources in their familial and social networks (Angrist, Lang, and Oreopoulos 2009; Avery and Kane 2004; Bettinger, Boatman, and Long 2013; Deming and Dynarski 2009; Dynarski 2016; Rodríguez-Planas 2012b). Indeed, mentoring and advising programs offered to high school seniors have been shown to increase high school graduation and college matriculation rates among socioeconomically disadvantaged populations (Castleman and Page 2015; Castleman, Page, and Schooley 2014; Stephan and Rosenbaum

2013), and colleges and universities now offer a wide array of voluntary and nonvoluntary advising and support services (Carlstrom and Miller 2013).

However, there is relatively little causal evidence on the efficacy of postsecondary voluntary advising and mentoring interventions, and most observational studies are plagued by endogenous take-up of support services (Bettinger, Boatman, and Long 2013).[1] Moreover, the handful of credibly designed studies that randomize access to support services yield mixed evidence (Rodríguez-Planas 2012b). One plausible explanation for these mixed results is low takeup rates of voluntary advising, mentoring, and support services (Angrist, Oreopoulos, and Williams 2014). Interventions that proactively engage students tend to have larger impacts on students' academic success than those that do not, which supports this hypothesis: for example, Bettinger and Baker (2014) investigate InsideTrack, a program in which coaches repeatedly reach out to nontraditional college students by phone, email, text, and social media, and find that the program significantly increased retention and degree completion.

An open question of first-order importance to both the design and evaluation of such programs, then, is what malleable, cost-effective policy levers that affect students' take-up and engagement of on-campus advising, mentoring, and support programs are available to postsecondary institutions? Currently, little is known about the causal determinants of student take-up of such programs. We contribute to this gap in knowledge by providing novel evidence on how assignment to a same-sex peer advisor affects students' engagement with a voluntary peer-advising program at a selective, midsized, private, non-for-profit university.[2] We do so by

[1] The directions of selection and the resulting bias are ambiguous, as there could be both positive and negative selection into advising.

[2] This analysis is motivated by, and also contributes to, an extensive literature that documents the impact of student-instructor demographic match on student success in both the K–12 (Dee 2004b, 2007) and postsecondary contexts (e.g., Bettinger and Long 2005; Carrell et al. 2011; Fairlie et al. 2014; Hoffman and Oreopoulos 2009).

exploiting the quasi-random assignment of peer advisors to first-year, first-semester university students and find that male students assigned to male peer advisors are significantly more likely to engage their peer advisor than are males assigned to female peer advisors.

A related policy-relevant question is whether students induced to engage their assigned peer advisor benefit from the encounter. We address this question using an instrumental variables strategy to provide causal estimates of the local average treatment effect (LATE) of meeting with the peer advisor on students' grade point averages (GPAs) and retention rates.[3] Specifically, our identification strategy exploits quasi-random assignments of peer advisors to students, which creates exogenous variation in assignment to same-sex advisors. These quasi-random assignments to same-sex peer advisors can then be used as instruments for students' participation in the peer advising program. Intuitively, analyses of the impact of peer advisors' sex on students' take-up of the peer advising program described above constitute valid first-stage regressions for instrumental variables analyses of the relationship between meeting with the assigned peer advisor and student outcomes. Therefore, the identification of credible estimates of the effect of a voluntary peer-advising program on students induced to participate by being assigned to a same-sex peer advisor is a second contribution of the current study.

We address these research questions using rich administrative data from two cohorts of first-year undergraduate students at American University. We find evidence of a strong first-stage relationship—primarily driven by the behavior of male students—between being assigned a same-sex peer advisor and students' participation in the peer advising program. Similarly, we find evidence of a causal reduced-form relationship between being assigned a same-sex peer advisor and students' second-year retention rates, but no such impact on GPAs in the following

[3] In future work, we will estimate the impact on degree completion, once the analytic sample's cohorts reach the necessary four- and six-year milestones.

spring semester. The reduced-form findings are strongly suggestive of a positive, causal impact of the peer advising program on compliers' persistence at the university, as there is no channel through which the sex of the randomly assigned peer advisor should affect outcomes other than from engagement with the peer advising program. Indeed, this is confirmed by 2SLS and bivariate-probit estimates that show meeting with assigned peer advisors significantly increased compliers' likelihood of persisting at the university. Qualitative evidence from exit surveys of program participants comport with the quantitative results and provide additional insights into students' reasons for engaging the peer advisor and the channels through which peer advising increased student persistence.

The paper proceeds as follows: The next section briefly reviews the relevant theoretical and empirical literatures on the efficacy of advising and mentoring programs and the role that student-instructor demographic mismatch plays in the education production function. The third and fourth sections describe the institutional details and data, respectively. The fifth section describes the identification strategy. The sixth section presents the first stage and reduced-form estimates of the impact of being assigned a same-sex peer advisor on students' participation in the peer advising program, spring quarter GPA, and retention. The seventh section presents instrumental variables estimates of the impact of meeting with a peer advisor on educational outcomes. The eighth section briefly reviews two sources of qualitative data on students' perceptions of the program. The ninth section concludes.

THEORETICAL BACKGROUND AND LITERATURE REVIEW

Peer advising might be a particularly important form of mentoring intervention for addressing sociodemographic gaps in postsecondary success, for several reasons. First, peer advisors are cheaper than professional full-time advisors, so they might be particularly cost-effective (Karcher et al. 2006; Sanchez, Bauer, and Paronto 2006; Self 2008). Second, peer advising might serve as a gateway that increases students' engagement with the full array of advising and support services on campus, and with the postsecondary institution more generally (Colvin and Ashman 2010; Habley, Bloom, and Robbins 2012). Finally, peer advisors might be especially well-positioned to improve the academic performance and engagement of male students and students from socioeconomically disadvantaged backgrounds, who often struggle in their first year of postsecondary schooling, since peer advisors provide an informal, low-stakes environment for students to openly address their concerns, study habits, and expectations (Shook and Keup 2012). Arguments about the likely importance of peer effects in postsecondary settings make similar points about how high-quality peers might act as role models and affect college students' study habits and time use (Stinebrickner and Stinebrickner 2006).

The current investigation of the determinants and impacts of participation in a voluntary peer-advising program contributes to two distinct literatures. The first is made up of first-stage analyses of how the sex match between students and assigned peer advisors contributes to the literature on the relationship between student-instructor demographic mismatch and student outcomes. Seminal studies by Dee (2004b, 2005, 2007) use a variety of data sources and identification strategies to show that, on average, when primary and secondary school students are assigned to teachers of different races and genders, students perform worse on standardized exams and teachers have lower perceptions of student performance and behavior. Ouazad (2014) finds similar effects on teachers' perceptions of ability in a nationally representative survey of U.S. kindergarteners.

Similar effects of student-instructor demographic mismatch have been documented in the context of postsecondary education. For example, Hoffman and Oreopoulis (2009) find positive effects of having a same-sex professor on a variety of academic outcomes among undergraduates at the University of Toronto, and Carrell, Page, and West (2010) find that this is particularly true for undergraduate female science and math students. Fairlie, Hoffman, and Oreopoulos (2014) find similar effects of being assigned a same-race instructor on several measures of minority students' academic success, including course grades, future course selection, and degree completion, at a community college in Northern California.

There is also growing evidence that student-instructor demographic match improves students' engagement with school: Holt and Gershenson (2015) find significant effects of having a white primary-school classroom teacher on black students' likelihood of being chronically absent or suspended from school, and Lusher, Campbell, and Carrell (2015) find similar effects of having a racially mismatched teaching assistant (recitation leader) on college students' attendance at optional discussion sections and office hours. The latter result is particularly germane to the current study because teaching assistants are similar to peer advisors in that they are relatively close in age and have had similar experiences to undergraduate students, and because engaging in the peer advising program is optional, as is attending office hours. For these reasons, we hypothesize that students will be more likely to engage with same-sex peer advisors.[4] In testing this hypothesis, we contribute to the literature on the impacts of student-instructor demographic mismatch by providing evidence that increased engagement is likely one channel through which student-instructor demographic mismatch affects academic achievement. Moreover, to the extent that there are benefits of peer advising, the finding that take-up is higher

[4] There is insufficient variation in peer advisors' race to study the effects of racial mismatch on engagement.

when students are matched to a same-sex peer advisor has implications for the optimal design and implementation of voluntary peer-advising programs, as well as for similar support services.

Second, by providing causal estimates of the impact of engagement with a peer advisor, the current study furthers our understanding of the efficacy of such programs and more generally of the sorts of interventions that might improve postsecondary success. Academic advising has long served the administrative and clerical function of helping students with the mechanics of scheduling and registering for courses (Frost 2000), and student development theory posits that academic advising can provide high-quality on-campus interactions for students (Light 2004; Tinto 1999; Wyckoff 1999). High-quality, frequent interactions with staff, faculty, and peers likely increase student satisfaction and persistence (Astin 1993; Bean 1980; Tinto 1987).

The fastest growing source of on-campus advising has been from professional advisors, as institutions are becoming increasingly aware of the extent and depth of the nonacademic issues students contend with while in college, and as faculty members face increased demands that limit their ability to advise and mentor students (Hemwall 2008; Kennedy and Ishler 2008; Self 2008). This increase in mentoring and advising services spurred research on the effectiveness of such interventions. However, much of this research is correlational and fails to adequately account for selection bias (Bettinger, Boatman, and Long 2013; Crisp and Cruz 2009). A handful of exceptions to this critique, which exploit experimental or quasi-experimental research designs, generally find positive, modest impacts of academic advising services, at least for certain student populations.

For example, Kot (2014) uses a propensity score matching procedure to compare observationally similar first-year university students who engaged with a centralized academic advising program to those who did not and finds positive effects on retention and grades. Of course, these estimates may be biased by selection on unobservables, which several authors avoid by conducting randomized experiments. Bettinger and Baker (2013) examine one such experiment designed to evaluate the effectiveness of a unique type of advising. The authors find that students who are assigned individualized student coaching from a private firm are more likely to remain enrolled at the university throughout the treatment period and one year after coaching had concluded. In reviewing the literature, Rodriguez-Planas (2012b) notes that most credible evaluations of formal postsecondary advising programs find small, positive gains for females but no such gains for males. One plausible explanation for this discrepancy is that females are more likely to engage with such programs, a point to which we return below.

Just as increased demands on faculty led to professional advisors, so too have increased demands on professional advisors led to the establishment of peer-advising programs. However, rather than replacing professional advising, peer advising is usually supplementary to core offerings (Carlstrom and Miller 2013; Self 2008). Students likely benefit from interacting with peer advisors, whose experiences more closely resemble those of students (Newton and Ender 2010). Additionally, peer advisors are a cost-effective way to maintain support in response to increased student demand or cuts to funding (Shook and Keup 2012).

Despite the growing popularity of peer-advising offerings at postsecondary institutions in the United States and abroad, literature examining the impact of such programs is sparse. Two important exceptions are experimental studies of programs implemented at a less-selective Canadian public university. The first study, Angrist, Lang, and Oreopoulos (2009), finds that female students randomized into a combined treatment condition of peer advising and financial incentives have higher GPAs and better academic outcomes than females who received only the financial incentive, and that this effect persists for two years. Importantly, the authors also find suggestive evidence that student participation in the program is positively correlated with students who happened to be matched with a same-sex peer advisor, although peer advisors were not randomly assigned to students. The second study, Angrist, Oreopoulos, and Williams (2014), offers a variation of the most effective treatment arm from Angrist, Lang, and Oreopoulos and examines whether the chance to earn merit aid, combined with the offer of peer advising, improves academic outcomes. While their main findings regarding financial incentives are outside the scope of the present discussion, it is notable that the intervention intentionally and unanimously matched students to peer advisors of the same sex. Over 75 percent of students interacted with the peer advisor over the course of the year, which suggests a benefit to same-sex matches between peer advisors and students, as there were fewer overall student interactions with peer advisors in the 2009 study, which did not strictly enforce same-sex pairings, particularly among mismatched pairs.

The current study contributes to the literature on the take-up and efficacy of postsecondary academic advising programs, particularly of peer-advising programs, in three general ways. First, we extend the analysis of Angrist, Lang, and Oreopoulos (2009) and Angrist, Oreopoulos, and Williams (2014) to a new context: a selective, private university in the United States, which serves a quite different student population than the less-selective commuter campus of a large Canadian public university. Second, we provide novel causal evidence on a low-cost, malleable determinant of students' engagement with peer advisors: the sex-match between students and assigned peer advisors. Third, we provide novel causal evidence on the impact of a voluntary peer advising program, in the absence of financial incentives, on postsecondary student outcomes.

INSTITUTIONAL DETAILS

The current study investigates student behavior at American University—a midsized, selective, private, nonprofit university in the Mid-Atlantic Region. It enrolls between 1,500 and 1,700 first-year, first-time college students each fall. The university offers doctoral, masters, and undergraduate programs. For the two-year period of this study, the university had federally reported first-year retention rates of nearly 90 percent and six-year graduation rates of about 80 percent. The university is made up of five academic units, the largest of which is the College of Arts and Sciences (CAS).

Distinctly from the other four units, CAS has offered peer advising to its first-year undergraduate students since 2008, though administrative data are only available for the 2013 and 2014 cohorts.[5] The program provides free one-on-one support services during students' first semester on campus. Peer advisors aim to bridge the developmental gap from high school to college and increase the first-to-second-year retention rates of students.[6] Peer advisors complete a competitive application process, receive extensive training on working with students and handling sensitive issues, must maintain a GPA of 3.0 or higher, and commit to working the entire academic year. During the period under study, eight unique peer advisors (four in each year) staffed the program, each working about 8–10 paid hours a week.

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[5] Data from 2012 are available, but there was no variation in peer advisor sex in this year. We occasionally use 2012 data in sensitivity analyses, though not in the main analytic sample. The peer-advisor assignment mechanism changed in 2015, so our identification strategy cannot be applied to this cohort.

[6] Crisp and Cruz (2009) note that much current research about mentoring and advising fails to provide operational definitions of the service(s) provided by the programs under study. In reviewing program planning documents, peer selection criteria, and training materials, the peer advising program of interest has the goals of *psychological and emotional support*, *degree and career support*, and *academic subject knowledge and support*. It is not explicitly noted that the program addresses the fourth goal, *existence of a role model*; however, it could be that many students view their peer advisor in this way. The program does not provide academic support services.

A full-time professional academic advisor coordinated the program as part of her assigned job responsibilities. The coordinator created advising caseloads by sorting new first-year students alphabetically by surname, dividing the alphabetized list into four similarly sized groups, and then arbitrarily assigned one peer advisor to each quartile (group) of students.[7] Importantly, the letters included in each quartile changed from year to year, meaning that there was within-letter variation in quartile assignment. While students may choose whether or not to meet with their assigned peer advisor, they do not choose which advisor they meet with. In fact, the program's coordinator confirmed that that there were no instances of students meeting with (or attempting to meet with) nonassigned peer advisors. In practice, the software students use to schedule appointments enforces this restriction. As a result, conditional on student sex and the first letter of his or her surname, this assignment mechanism creates exogenous variation in assignment to same-sex peer advisors.

Students learn about the program via two emailed invitations, seemingly sent from their assigned peer advisor. However, it is the coordinator who schedules and sends the emails from a central account after verifying the roster of eligible students and splitting them into the caseloads described above. These email invitations, regardless of which peer advisor they were "sent" by, were sent to all students at the same time in September, during the third or fourth week of the fall semester. The only variation between emails is the assigned peer advisor's first name, surname, and contact information.

[7] The coordinator did not split letters between advisors. For example, in 2013, each of the four peer advisors was assigned to one of the following portions of the alphabetically sorted list: A–E, F–L, M–R, and S–Z.

Our identification strategy, formalized below, assumes that students are able to infer the sex of the assigned peer advisor from the name provided in the email invitation.[8] This assumption is likely valid for at least two reasons. First, a well-established literature in phonology provides convincing descriptive and experimental evidence that English language speakers predictably and reliably identify names they read as male or female (Barry and Harper 1995; Cassidy et al. 1999; Slater and Feinman 1985).[9] Second, the names of the peer advisors in 2013 and 2014 are unambiguously sex-specific. All seven unique peer-advisor names are approximately among the top 500 most common names for their sex for the 1995–2013 birth cohorts, and none appear among the top 1,000 most common opposite-sex names; that is, there are no names such as Morgan, which appears among the most common male and female names.[10] The two male names are in the top 10 and top 100 most common male names, respectively, and are similar to names such as Thomas and Nathan. One top-10 female name appears twice among the group and is similar to a name such as Natalie. The other female names are in the range of the top 50, top 100, or top 500 most common female names and are comparable to names such as Sara, Tiffany, and Alyssa, respectively.

[8] Of course, it is also possible that students searched for their assigned peer advisor's name on social media or the Internet and inferred the peer advisor's sex from a photograph or other additional information. Nonetheless, such behavior does not violate the key identifying assumption.

[9] This is one of several reasons we exclude international students from the main analytic sample, as international students may be less familiar with common male and female names in the U.S. context.

[10] We go back to the 1995 birth cohort because this is, on average, when the students in the analytic sample were born. The Social Security Administration provides lists of the most popular first names for boys and girls by birth year at https://www.ssa.gov/oact/babynames.

DATA

The current study relies on administrative data on first-year domestic CAS students in two consecutive cohorts (Fall 2013 and 2014).[11] Table 1 summarizes these students. Column 1 shows that overall, a little more than half of students voluntarily met with their assigned peer advisor, and 87 percent were retained by the college into their second year. Students performed well in the spring semester of their first year, earning an average GPA of 3.26 (on a 4.0 scale). About one-quarter of these students were male, which is slightly smaller than the university's campus-wide sex ratio of about 1:3. About one-quarter of these students were assigned a male peer advisor; this is consistent with the fact that each year only one of the four peer advisors was male and about one-quarter of students were uniquely assigned to each peer advisor. Given that the majority of both students and peer advisors are female, it is unsurprising that almost two-thirds of students were assigned a same-sex peer advisor. This is a majority white student body, though a variety of other race and ethnic groups are represented. Students have strong admissions credentials, which is unsurprising given the university's selectivity, and tend to come from wealthy zip codes.

Columns 2 and 3 of Table 1 summarize students separately by sex. Consistent with past research (e.g., Angrist et al. [2009, 2014]), females were marginally more likely to meet with their peer advisor and had marginally higher retention rates and spring GPAs. Males and females were about equally likely to be assigned a male advisor and were approximately evenly distributed across quartiles of the alphabet, which reassures us that the peer advisor assignments

[11] We exclude about 50 international students because a separate support system is in place for these students. However, international students are eligible for peer advising and do receive the email invitation. The main results are robust to including these students, but we exclude them from the main analytic sample because they are not the target population for peer advising.

were in fact exogenous. Nonetheless, we conduct a formal balancing test below, which provides further assurances. Finally, males and females had very similar preadmission credentials, including similar SAT/ACT scores, though females had slightly higher high school GPAs.

Finally, columns 4 and 5 of Table 1 compare students who did meet with their assigned peer advisor to those who did not. Those who did attend advising were more likely to be retained and had higher spring GPAs, though these differences could be driven by positive selection into advising. Overall, students who met their peer advisor were significantly more likely to have been assigned a same-sex peer advisor, which is consistent with our hypothesized first-stage relationship. We delve into this relationship in greater detail below. Regarding selection on observables into meeting with the peer advisor, students who did and did not meet with their assigned peer advisor tend to have similar sociodemographic characteristics and preadmissions credentials, though those who did meet with their peer advisor had marginally higher high-school GPAs.

Table 2 compares the mean characteristics of students who were and were not assigned a same-sex peer advisor, separately by students' sex. Pooling the two sexes in one test of covariate balance yields similar results, but we perform this analysis separately by student sex since there are some fundamental differences between the sexes and females are overrepresented among both students and peer advisors. No mean differences are statistically significant at the 95 percent confidence level. Importantly, none of the mean differences in preadmissions proxies for student ability and work ethic (SAT and GPA) between students in the treatment (same-sex peer advisor assignment) and control (different-sex peer advisor assignment) conditions are even marginally statistically significant, nor are differences in students' home zip code socioeconomic characteristics. Two differences are marginally statistically significant for males: black students and students from New England. While the marginal significance of these differences is likely spurious given the number of hypothesis tests conducted in Table 2, the fact that these differences are in demographic characteristics that might predict the first letter of the surname, and thus might be associated with the sex of the assigned peer advisor, merit further attention. We address this concern by estimating models that explicitly control for students' racial and ethnic backgrounds, home region fixed effects (FE), and "first letter of surname FE." The main results are robust to conditioning on these covariates.

Table 3 provides suggestive evidence of unconditional first-stage and reduced-form effects of being assigned to a same-sex advisor on meeting with the peer advisor, and subsequent educational outcomes, respectively. Regarding the first-stage relationship between being assigned a same-sex peer advisor and the likelihood of meeting with the assigned peer advisor, there is a statistically significant positive effect that appears to be entirely driven by the response of male students. Specifically, male students assigned to a male peer advisor are, on average, 16 percentage points more likely to meet with their assigned peer advisor than are males assigned to a female peer advisor. From the base take-up rate of males assigned to a female peer advisor, this represents a substantively large 38 percent increase in the likelihood of meeting with the peer advisor.

A similar pattern appears in the next panel of Table 3, which shows the unconditional reduced-form effect of being assigned a same-sex peer advisor on retention the following fall. Specifically, males assigned to a male peer advisor are 9.3 percentage points (11 percent) more likely to be retained than males assigned to a female peer advisor. This difference is marginally statistically significant and consistent with the first stage results described above. Indeed, because it is hard to imagine how an email that provides the name of the assigned peer advisor could affect retention in any way other than through inducing the student to meet with his or her peer advisor, this reduced-form result strongly suggests a causal relationship between meeting with the assigned peer advisor and persistence at the university. Interestingly, however, the final panel of Table 3 provides no evidence that the sex of the assigned peer advisor affected the probability that the student earned a spring GPA of 2.0 or higher, which is the threshold for academic probation.[12] The fifth section formalizes an empirical strategy for identifying and interpreting these suggestive effects and for testing the identifying assumptions.

ECONOMETRIC MODEL AND IDENTIFICATION

The current study contributes to our understanding of the role of voluntary peer-advising programs in the postsecondary education production function. Specifically, we provide causal evidence on the factors that influence participation in peer-advising programs and on the impact of participation on student outcomes. A generalized Roy Model (Roy 1951; Heckman 2010), in which students choose whether to meet with their assigned peer advisor based on the expected net benefits of doing so, articulates these contributions and motivates the econometric model. Returns to meeting with the peer advisor are intermediate, in the sense that they are measured in terms of human capital accumulation and not wages (e.g., GPA [academic performance] and persistence and graduation [attainment]).[13] Rational students voluntarily meet with their assigned

[12] Nor is there evidence of an effect on any other measure of performance (i.e., a GPA of 3.0 or higher (B average), actual GPA on the continuous 0.0-4.0 scale). This is likely because the peer advising program was designed to facilitate peer connections and provide academic advising, not to provide tutoring services. Peer advisors were trained to direct academic issues to the university's academic support office and tutoring centers.

[13] We assume that individuals prefer more human capital to less, but remain agnostic as to why. There are many reasons why they might, as educational attainment is associated with higher earnings (Blundell, Dearden, and Sianesi 2005; Card 1999) and better health (Grossman 2006). Data on graduation are unavailable at the time of writing this draft in 2016, as members of the earliest cohort are currently in their third year at the university, but we will update the analysis to include graduation results when it is possible to do so.

peer advisor (select into treatment) if the expected net benefit of doing so is positive. This decision rule is characterized by three random variables: two potential outcomes (Y^1, Y^0) and the cost of meeting with the peer advisor (C). Formally,

$$(1) \quad \Pr\left(A=1 \mid \Omega\right)=\Pr\left(Y^{1}-C>Y^{0}\right),$$

where A is a binary indicator equal to 1 if the student met his or her peer advisor, and 0 otherwise. Of course, only one of the two potential outcomes is actually observed, so we follow the common approach of modeling each potential outcome as a unique linear function of observed student characteristics (X) and additively separable stochastic errors.[14] Finally, in addition to the opportunity cost of meeting with a peer advisor, which is also a function of X, we hypothesize that C is a function of the sex match between student i and assigned peer advisor j.[15] Specifically, we hypothesize that students, particularly male students, are more likely to interact with same-sex peer advisors, based on empirical evidence that students are more likely to attend the office hours of demographically matched teaching assistants (Lusher et al. 2015) and the fact that Angrist et al. (2009, 2014) find higher rates of interaction with peer advisors when the students were assigned to same-sex peer advisors.

We operationalize Equation (1) by estimating linear and probit models of the form

$$(2) \quad \Pr\left(A_{ij}=1 \mid \Omega_{ij}\right)=\beta_{0}+\beta_{1} X_{i}+\beta_{2} male_{i}+\beta_{3} male_{j}+\delta male_{i} \times male_{j}+u_{ij},$$

where X contains observed student characteristics, including a cohort (year) indicator, sociodemographic background, admissions profile, and “first letter of surname” FE; *male* is a

[14] As in the additive random utility model (ARUM) (Cameron and Trivedi 2005, p. 477).

[15] It is possible that students expect greater returns to meeting with a same-sex peer advisor, although Online Appendix Table A.1 shows no evidence that the impact of A on the ex post outcome Y varies by the type of student-peer advisor sex match. Nonetheless, nothing is lost by assuming that the sex-match indicators enter the linear equation for Y^1 as well.

binary indicator equal to one if male, and zero if female; and u is an idiosyncratic error term.[16] The parameter of interest in the linear probability model (LPM) is δ, which represents the differential effect of being assigned to a male advisor for male students (i.e., same-sex advisor match) on propensity to voluntarily meet with the peer advisor.[17] The identifying assumption is that conditional on a student's sex and his or her surname's place in the alphabet, assignment to a same-sex advisor is random. As discussed in the third section, this is very likely true, since the peer advisors were arbitrarily assigned to subsets of the alphabetically sorted student list and the balancing test provided in Table 2 finds no evidence of systematic differences in the observable characteristics of students in the treatment and control conditions. Moreover, the alphabetical quartile cutoffs varied over time, so that students with surnames starting with E, F, K, and L were assigned to male peer advisors in some years and to female peer advisors in others.[18] Thus OLS estimates of δ in Equation (2) and MLE estimates of the analogous probit model can be given causal interpretations. These results contribute to the literature on the impact of demographic mismatch on student outcomes.

The exogenous variation in assignment to a same-sex peer advisor has two additional implications for Equation (2). First, Equation (2) can be interpreted as the first stage in an instrumental variables analysis of the impact of A on ex post outcome Y, in which the sex and sex

[16] Peer advisor sex can also be replaced by a peer advisor fixed effect, which also subsumes the cohort indicator. Sensitivity analyses show that there are no statistically significant differences between the six female peer advisors, nor between the two male peer advisors.

[17] In the probit model, the parameter of interest is the corresponding average partial effect. The degree of student-peer advisor sex match in Equation (1) could be equivalently characterized by a set of four mutually exclusive categorical variables, one of which must serve as the omitted base category: male student, same sex match; female student, same sex match; male student, nonmatch; female student, nonmatch. We use these definitions in the probit model to ease the interpretation of partial effects.

[18] Variation in exposure to treatment within individuals whose surnames begin with the same or adjacent letters is arguably exogenous, as randomization in many high-profile randomized control trials was conducted by sorting units alphabetically and assigning treatment to every other, or every third, unit in the list (e.g., Glewwe et al. [2004]; Miguel and Kremer [2004]).

match of the randomly assigned peer advisor instrument for *A*. Second, the reduced form causal effect of being assigned a same-sex peer advisor on *Y* can be estimated using variants of Equation (2) that take *Y* as the dependent variable. These two implications are related, as documenting such reduced-form effects is a useful diagnostic check of the instruments' validity (Angrist and Krueger 2001; Angrist and Pischke 2009; Chernozhukov and Hansen 2008).

We proceed by jointly modeling Equation (2) and a parameterized version of the education production function (outcome equation):

$$(3) \quad E\left(Y_{ij} \mid \Omega_{ij}\right) = \alpha_0 + \alpha_1 X_i + \alpha_2 male_i + \tau A_i + \varepsilon_{ij}.$$

When Equations (2) and (3) are linear, they can be estimated by 2SLS or LIML and τ is the parameter of interest, which represents the local average treatment effect (LATE) of meeting with the peer advisor on compliers' outcomes (Imbens and Angrist 1994).[19] Since the educational outcome *Y* is binary (i.e., an indicator of retention to the second year of postsecondary schooling, of graduation from college, or of GPA being above a certain threshold), it is natural to jointly model Equations (2) and (3) as a bivariate probit model, though a system of two LPMs may well provide reasonable approximations of the parameters of interest (Wooldridge 2010).[20] We discuss additional sensitivity analyses and tests for heterogeneity in the results section.

[19] Compliers are students induced into treatment (meeting with their peer advisor) by the assigned peer advisor's sex.

[20] Of course, *Y* could also be a continuous measure of GPA that is censored from above at 4.0. Thus Equations (2) and (3) can be jointly modeled as a system of probit and tobit equations (Roodman 2011). We find no evidence that peer advising affected spring GPA, regardless of how GPA is measured, so we do not report estimates of the probit-tobit two-equation system.

IMPACT OF ASSIGNMENT TO A SAME-SEX ADVISOR

Main Results

Panel A of Table 4 presents baseline OLS estimates of linear first-stage and reduced-form versions of Equation (2).[21] Columns 1 and 2 report specifications of the first-stage regression that condition only on cohort (year) FE, and on the full set of covariates described in the fourth section (including a full set of "first letter of surname" FE), respectively.[22] Point estimates on the male-student and male-student×male-advisor interaction terms are statistically significant, of the expected signs, and remarkably similar in magnitude across the two specifications.[23] This provides further evidence that same-sex peer advisors (the treatment) were as good as randomly assigned. Specifically, males assigned to a female peer advisor are about 13 percentage points less likely than female students to meet with their assigned peer advisor. That the male-advisor indicator is statistically indistinguishable from zero indicates that female students were equally likely to meet with their peer advisor, regardless of the assigned peer advisors' sex. Male students assigned to a male (same-sex) peer advisor, however, were 18 percentage points more likely to meet with their peer advisor than males assigned to a female peer advisor. Since only about half of male students met their peer advisor, this is a substantively large increase of about 40 percent.

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[21] Probit coefficients are reported in Online Appendix Table A.2. Probit average partial effects, which are comparable and qualitatively similar to OLS estimates, are reported in Online Appendix Table A.3.

[22] Gelbach (2016) shows that the only relevant comparison is between the base (unconditional) and fully specified models, so we eschew the common practice of sequentially adding covariates to the model.

[23] Importantly, the interaction terms remain at least marginally statistically significant ($p < 0.10$) after making inference robust to clustering by peer advisor (eight clusters) using the wild bootstrap procedure proposed in Cameron, Gelbach, and Miller (2008). The main results are similarly robust to including a full set of advisor indicators.

Columns 3 and 4 of Table 4 similarly report estimates of baseline and fully specified reduced-form linear retention models, respectively.[24] As in the first-stage results described above, the reduced-form point estimates are robust to the inclusion of covariates and “first letter of surname” FE, again suggesting that same-sex peer advisors were conditionally randomly assigned. Consistent with evidence of a gender gap in educational attainment, male students are marginally less likely to persist into their second year than females, but males assigned to a male peer advisor are 10 percentage points more likely to persist into their second year than male students who were assigned to a female peer advisor. Given that students were informed of their peer advisor’s sex indirectly by the name included in an email at the start of the fall term, it is difficult to imagine how this information could have affected retention in any way other than through students’ engagement with the peer advising system. In other words, the sex of the assigned peer advisor and the interaction between student and peer-advisor sex are unlikely to enter Equation (3) and therefore satisfy the exclusion restriction. Given this exclusion restriction, the reduced-form estimates presented in columns 3 and 4 of Table 4 are highly suggestive of a causal impact of meeting with the peer advisor on persistence into the second year of postsecondary education for male students induced to meet with their peer advisor by the same-sex match (Angrist and Krueger 2001; Angrist and Pischke 2009; Imbens and Angrist 1994).

That the sex of a randomly assigned peer advisor can significantly affect not only student engagement with the peer advising program but, ultimately, student persistence at the university is striking. Since each cohort was exposed to only one male peer advisor, one possible explanation is that these results were driven by one particularly popular, charismatic, or effective male peer advisor. We investigate this hypothesis in Panel B of Table 4 by reporting estimates of

[24] Grade-threshold and GPA-reduced forms are not reported because, as shown in Table 3, there is no reduced-form effect of being assigned a same-sex advisor on spring semester grades.

an augmented model that includes interaction terms that allow the effects of student and peer-advisor sex to vary across cohorts. The estimated coefficients in columns 1 and 2 of Panel B on the male-student indicator are nearly identical to those in Panel A, and the corresponding male-student interaction term in Panel B is both small and statistically insignificant. Together, these two results indicate that male students assigned to female peer advisors were equally likely to engage their peer advisor across the two cohorts. The male-advisor coefficient and interaction term are similarly insignificant in columns 1 and 2 of Panel B, which indicates that female students were indifferent to the sex of their assigned peer advisor in both cohorts. Finally, in Panel B of Table 4, the male-student/male-advisor/2014-cohort triple interaction term is statistically indistinguishable from zero, while the male-student/male-advisor interaction term is positive and similar in magnitude to that in Panel A, indicating that male students were more likely to engage male peer advisors than female peer advisors in both cohorts. In sum, the estimates reported in columns 1 and 2 of Panel B of Table 4 suggest that the male preference for male peer advisors was not unique to one specific cohort or peer advisor. Given that these cohort interaction terms are jointly insignificant at traditional confidence levels, we prefer the more parsimonious baseline specifications reported in Panel A. The reduced form estimates reported in columns 3 and 4 of Panel B reinforce the general finding that while average effects of being assigned a same-sex male peer advisor were larger in 2013 than in 2014, the differences are not statistically significant at traditional confidence levels. And, as in the first-stage regressions presented in columns 1 and 2, the cohort interaction terms are jointly insignificant in the reduced-form regressions reported in columns 3 and 4 of Table 4. Since the main results are unlikely to have been driven by one unique peer advisor, subsequent analyses focus on the baseline model that assumes constant effects across cohorts in the interest of parsimony and statistical power.

Heterogeneous Effects

Of course, there could also be heterogeneity along other dimensions, such as students' sociodemographic backgrounds, since policy debates surrounding the importance of peer effects hypothesize that socioeconomically disadvantaged and underrepresented minority students stand to benefit the most from positive peer effects (Stinebrickner and Stinebrickner 2006). Table 5 investigates four possible sources of heterogeneity in both the first-stage and reduced-form impacts of the sex of assigned peer advisors. Columns 1 and 2 test for heterogeneity by the educational attainment of the parents of students. The idea is that first-generation college-goers may benefit more from advising and mentoring interventions than the children of college-educated parents, since college-educated parents are more familiar with postsecondary institutions, but first-generation students may also be more apprehensive about engaging with advisors. Indeed, these dual concerns are the motivation for informal peer-advising programs (Shook and Keup 2012). Columns 1 and 2 provide no evidence of significant differences by students' first-generation status in the relationship between peer-advisor sex and student outcomes, as the first-generation interaction terms are individually and jointly statistically insignificant. Columns 3 and 4 of Table 5 report similar analyses for another measure of students' socioeconomic status: whether or not the student received a Pell Grant.[25] These estimates are generally similar to those for first-generation status, with one exception: the male-

[25] Pell grants were established in 1972 to provide federal funds directly to college students with financial need. Pell grants depend on the individual student's financial need, cost of attendance, and enrollment status. Maximum awards were $5,645 and $5,730 in 2013 and 2014, respectively (Dynarski and Scott-Clayton 2013).

advisor/Pell-recipient interaction in column 3 is large, positive, and statistically significant. This suggests that female Pell recipients assigned to male peer advisors are significantly more likely to meet with the peer advisor than are non-Pell females assigned to male peer advisors. This result is reflected in a marginally significant bump in retention for Pell-recipient females assigned to male peer advisors, seen in column 4. Columns 5 and 6 similarly allow for heterogeneous effects by student race. Specifically, race is measured by a crude "nonwhite minority" indicator that equals 1 if the student is black or nonwhite Hispanic and 0 otherwise. There is no evidence of significant differences by students' race in the relationship between peer-advisor sex and student outcomes, as the nonwhite interaction terms are individually and jointly statistically insignificant.[26]

Together, the estimates in columns 1 to 6 of Table 5 indicate that the sizable male-student/male-peer-advisor match effect documented in Table 4 does not vary by students' socioeconomic or racial backgrounds. Nonetheless, it is interesting that the point estimates on the male-male-X triple interaction terms are systematically negative and similar in magnitude to the corresponding point estimates on the male-male interaction terms, suggesting that the male-male match effect is marginally larger among relatively advantaged white students. Finally, columns 7 and 8 of Table 5 test for heterogeneity by students' home locales, since students far from home might be more likely to utilize campus support services when they feel homesick or have less access to friends and family. We find no evidence for this, however, regardless of how distance from home is operationalized. In sum, Table 5 suggests a general lack of heterogeneity in the impact of male-student/male-peer-advisor sex match on take-up of the program and retention.

[26] The lack of significant differences by race is robust to allowing for race-specific differences (e.g., a full set of black, Hispanic, Asian, and Other categorical indicators).

IMPACT OF PEER ADVISING ON ACADEMIC SUCCESS

Main Results

Table 6 reports estimates of Equation (3). Columns 1 and 2 report naïve OLS and probit estimates of the relationship between meeting with the peer advisor and the probability of persisting into the second year of postsecondary education. The models in column 1 condition only on student sex and cohort FE, while the models in column 2 condition on the full set of covariates and "first letter of surname" FE. These estimates show a modest, positive, marginally significant association between engagement with the peer advising system and retention. However, even the estimates in column 2 cannot be given a causal interpretation, as they are likely biased by unobserved factors that jointly determine retention and meeting with the peer advisor.[27] We therefore prefer instrumental variables (IV) approaches that jointly estimate Equations (2) and (3), which are reported in columns 3 and 4 of Table 6.

We prefer the linear IV and bivariate probit estimates of the fully specified model reported in column 4 of Table 6 to the unconditional models reported in column 3 for two reasons. First, conditioning on the exogenous student-background controls (e.g., race, SES, preadmissions academic performance) increases precision, particularly in the bivariate-probit models. This is useful, given the relatively small sample size and the general imprecision of IV estimators. Second, controlling for the "first letter of surname" FE is particularly important in the IV setting, since it is possible that the spot in the alphabet for a student's surname is weakly correlated with academic success, perhaps because of the advantage early-letter students receive from being assigned to the front row of seating charts. Still, while we focus our discussion on the results in column 4, it is reassuring that the estimates in column 3 are quite similar.

[27] Control-function endogeneity tests in columns 3 and 4 of Table 6 suggest that OLS estimates are biased.

The first panel of column 4 reports linear (2SLS) IV estimates. The 2SLS estimate is positive, quite large, and marginally statistically significant. It suggests that for students who were on the margin of meeting their peer advisor, and ultimately did because their assigned peer advisor was of the same sex, meeting with the peer advisor increased their probability of persisting into the second year of postsecondary schooling by about 67 percentage points. This is perhaps implausibly large, given that the average retention rate is between 80 and 90 percent. Given the LATE interpretation of this estimate, it could be that compliers had very low ex ante likelihoods of persisting and the peer advisors really helped these students. An alternative interpretation is that this point estimate is imprecise: the lower end of the 90 percent confidence interval is an impact of only about 5 percentage points, which also happens to be nearly identical to the naïve OLS estimate. While dramatically lower than 67, a 5 percentage point increase in the probability that a student is retained is arguably policy-relevant, as it is similar in size to the impact of merit awards (e.g., West Virginia's PROMISE Scholarship) and the Open Doors bundle of small scholarships and enhanced student services (Deming and Dynarksi 2009).

We also report the LIML estimate of the same linear specification, which has better small-sample properties than 2SLS and might perform better given our relatively small sample size (Angrist and Pischke 2009). The LIML estimate is nearly identical to the 2SLS estimate, which in addition to relaxing concerns about finite sample bias, suggests there is no weak IV problem (Angrist and Pischke 2009). Together, the linear estimates reported in the top panel of column 4 of Table 6 suggest that meeting with peer advisors had a positive impact on retention rates, at least for compliers who were induced to meet with their peer advisor by the randomly assigned peer advisor's sex. The magnitude of this effect is less clear, however, both because of the imprecision of the 2SLS and LIML estimates and the fact that both the endogenous and outcome variables are binary.

The bivariate-probit models estimated in the bottom panel of Table 6 address both concerns, as the bivariate probit estimates are more efficient and restrict predicted probabilities to the unit interval (Wooldridge 2010). Indeed, the bivariate-probit coefficient estimate and associated average partial effects (APE) in column 4 are both statistically significant, and the APE, which is directly comparable to the linear estimates, has a standard error that is about one-third the size of the linear 2SLS and LIML standard errors. The APE is smaller, but still positive, and suggests that compliers who met with their peer advisor were about 28 percentage points more likely to be retained. Once again, the bivariate probit estimates strongly suggest that, for at least a subset of students, voluntary peer advising programs can significantly increase retention.

Heterogeneous Effects

For the same reasons that the first-stage and reduced-form effects discussed in the sixth section might vary by students' sociodemographic backgrounds, so too could the impact of actually meeting with the assigned peer advisor vary with observed student characteristics. We test this hypothesis by augmenting Equation (3) to include interactions between A and X, and by using interactions between the instruments and *X* as additional instruments. As in Table 5, Table 7 tests for heterogeneity along four observable dimensions: first-generation, Pell recipient, underrepresented minority (black or Hispanic), and distance from home (Mid-Atlantic region). For each of these outcomes, naïve OLS and IV 2SLS estimates are reported, and in all but one case the control function is statistically significant, indicating a significant difference between the OLS and 2SLS estimates. None of the 2SLS estimates of the interaction terms are statistically significant at traditional confidence levels, though the first-generation and Pell-recipient interaction terms are negative and relatively large in magnitude. Overall, like the reduced-form results in Table 5, Table 7 suggests that there is little variation by observable student characteristics in compliers' benefits of meeting with the assigned peer advisor.

QUALITATIVE EVIDENCE

In order to better understand why students utilized the peer-advising program and the potential channels through which peer advising affected students, program staff conducted two qualitative surveys of program participants. First, all students who attended peer advising during the 2013 fall semester were invited to participate in an online, anonymous survey. Second, 10 of these students were randomly selected to participate in in-depth discussions about their experiences in the program with program staff. Both surveys were subject to severe limitations that prevent us from making causal inferences from these data: low (less than 50 percent) response rates, only program participants were sampled, and individual student responses cannot be linked to the administrative data analyzed above. Still, these data shed some light on students' reasons for participating in the program, and on what they got out of it. We summarize the results below.

Online Survey

All students who met with a peer advisor during the 2013 fall semester were emailed a link to take an exit survey on www.surveymonkey.com at the end of the semester. Only 71 (about 32 percent) of eligible students responded, of whom 69 answered every question.[28] Unfortunately, the anonymous survey did not ask about students' demographic backgrounds, so

[28] Each of the 71 entries did come from a unique IP address.

we cannot examine the representativeness of the respondents, nor can we investigate heterogeneity in responses. However, the survey did ask about the student's major, and 31 percent of respondents reported being undecided. This is similar to the overall percentage (27 percent) of 2013 cohort CAS students who had yet to declare a major at the end of the fall 2013 semester, which suggests that, at least on this dimension, survey respondents resemble the full 2013 CAS cohort.

The survey contained 13 multiple choice questions and concluded with an option to write an open-ended comment. Responses to the first five multiple-choice questions are summarized in Panel A of Table 8. These five questions ask students about their perceptions of their peer advisor. Respondents' perceptions of the peer advisors were unanimously positive, as about one-quarter agreed and three-quarters strongly agreed with each positively framed statement.

Panel B of Table 8 reports each of the 11 responses in the open-comment form. Once again, the responses are uniformly positive. Some of the responses hint at why students engaged with the program. For example, students commented "it's much less intimidating to ask questions . . . of a peer advisor than of a faculty member" and "The informal setting and meeting with your peers is the perfect place for students to let down their guard and get help and learn new things." These sentiments are consistent with theories that predict that the informality and shared experiences of peer advisors make them an attractive source of support. Responses about the efficacy of the program are too generic to provide much insight into the specific channels through which peer advisors affected student outcomes, though the responses seem to indicate that the program was beneficial: five comments included the word "helped" or "helpful." One student suggested that the program be more widely advertised.

In an effort to evaluate the program's efficacy, the online survey also included four pairs of ex post "before" and "after" questions about students' awareness of various campus resources and procedures. Again, these questions are imperfect, because there is no control group of students who did not meet with a peer advisor, and the questions were asked at the same time, after the respondents had met with a peer advisor. Nonetheless, we examine these data to confirm that they are consistent with the reduced-form and IV estimates reported in Sections 6 and 7 that suggest that participating in the program significantly increased retention. Responses to each of the four pairs of questions are reported in transition matrices in Table 9. Diagonal elements measure the percentage of students whose awareness remained the same after participating in the program, and the bold numbers to the right of the diagonal measure those whose awareness increased. Overall, 51, 39, 62, and 48 percent of respondents reported increased awareness along the four dimensions measured by the before-and-after questions, respectively. These significant self-reported increases are consistent with the positive impact on retention documented above.

25-Minute In-Depth Interviews

Motivated by the generally positive results of the online survey and a desire to better understand students' experiences, program staff randomly selected 10 attendees to invite to participate in an in-depth conversation about their experience with the peer-advising program. Four of the invited students agreed to participate in the one-on-one interviews, which lasted about 25 minutes. All four were female, unfortunately, which prevents probing the thought process behind the first-stage effects of sex-match among males. Nonetheless, the interview transcripts reveal a few patterns that generally comport with the exit-interview results. First, students were attracted to the program because it was a safe environment to ask informational and procedural questions about registration and advising that they feared would be judged negatively if asked to a nonpeer. One student commented about reaching out to faculty, “I think that people don’t feel, I guess, worthy, to talk to their professors. Or they don’t feel like they have something to offer.” Another student commented, “I’m not comfortable going to an adult with something I don’t think they think is very important.”

A second draw of the peer advising program was the opportunity to meet with a peer whom they felt could relate to—and help process and navigate—issues they were experiencing as a new college student. One student felt pressure to compete for internships and career-related opportunities. Another felt out of place because he perceived himself to be of a different socioeconomic background from his peers. Another student was struggling socially and noted being frustrated because the student thought “college would automatically be great because it is college.” For these students, the invitation to meet with a peer advisor came as a timely and useful offer of support in a time of need. Overall, the interview transcripts demonstrate that respondents perceived peer advising to be effective, timely, and delivered by a member of the campus community equipped to relate to the experiences of new college students.

DISCUSSION

Using unique administrative records from a selective, midsized, private, not-for-profit university, we leverage quasi-random assignments of peer advisors to first-year arts and sciences undergraduate students to investigate two aspects of a voluntary peer advising program. First, we show that the sex of the randomly assigned peer advisor has a significant and substantively large impact on male students’ engagement with the peer advising program. This result is consistent with a large literature in the economics of education that finds significant effects of student-teacher demographic match on a host of student outcomes and behaviors (e.g., Carrell et al. 2010; Dee 2004, 2007; Fairlie et al. 2014; Lusher et al. 2015). This finding has policy implications as well, given the relatively low take-up rates for voluntary support services, particularly among males, in postsecondary education settings (Angrist et al. 2014). Indeed, manipulating the sex of assigned peer advisors, or providing peer advisors of both sexes, is a scalable, low-cost policy lever available to many postsecondary institutions. To our knowledge, the current study provides the first causal evidence of a malleable policy lever that can increase male students' engagement with university support services. Moreover, we show that engagement with the peer advisor is beneficial, as reduced-form estimates indicate that assignment to a male peer advisor significantly increased male students' retention rates.

Second, using an instrumental variables strategy, we formally show that engaging with an assigned peer advisor significantly increased compliers' likelihoods of persisting at the university. This finding is consistent with qualitative data from online exit surveys and in-depth interviews. Compliers are males who would not have engaged with the peer advising program if assigned to a female peer advisor and were not definitely going to engage regardless of peer advisor assignment. The LATE here is a policy-relevant parameter for administrators, educators, and policymakers seeking to increase retention rates among first-year students, particularly among male students, as the policy lever (assigning a choice or same-sex advisor) is very low-cost and not controversial for institutions already offering voluntary peer advising. Moreover, the "always compliers" who engage with the support infrastructure no matter the peer advisor assignment do better, on average, than compliers in the absence of treatment. This is low-hanging fruit that can be collected in the short run, while institutions and researchers continue to search for the policy tools that might improve the engagement, and ultimately the academic success and persistence, of the "never compliers."

That no effect is found on spring-semester academic performance (GPA) is consistent with the stated mission of the university's peer advising program. This nonfinding is also consistent with the peer effects literature, which generally finds modest effects of high-achieving peers on postsecondary students' academic performance, which Stinebrickner and Stinebrickner (2006) attribute to peer effects primarily operating through the transmission of study habits and time-management skills. Together with social skills, these are exactly the types of skills that comprise the mission of the peer advising program, and are likely to improve retention. Indeed, Stinebrickner and Stinebrickner (2006) find positive, significant peer effects on the probability that female students return for a second year of postsecondary schooling.

One caveat of the current study is that these findings may not generalize to larger public institutions or to less selective institutions. Conducting a similar analysis in other postsecondary contexts would be straightforward and would be useful in furthering our understanding of the factors that influence student engagement with voluntary support services such as peer advising, as well as the impact of such services on compliers who are induced to engage by the demographic representation of mentors, support staff, and peer advisors. It would also be useful to determine whether there are similar race-match effects. Unfortunately, we were unable to investigate the impact of race and ethnicity matching in the current study because there was only one nonwhite peer advisor. We hope to investigate such questions in the future, and to conduct a randomized experiment in which some students are offered a choice between one male and one female peer advisor, and so on, both in arts and sciences and in other units at the institution, as the peer advising program is scaled up to the university level.

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Table 1 Analytic Sample Means

	All students (1)	Male students (2)	Female students (3)	Attended advising (4)	Did not attend advising (5)
Attended advising (%)	55	48	57*	100	--
Retained (%)	87	84	87	89	84**
Spring GPA†	3.26	3.11	3.31***	3.30	3.22*
Spring GPA ≥ 3.0† (%)	76	66	79***	80	71***
Spring GPA ≥ 2.0† (%)	96	95	96	96	95
Male student (%)	25	100	--	22	29**
Male advisor (%)	24	27	23	25	23
Same-sex advisor (%)	64	27	77***	68	60**
List quartile 1 (%)	26	26	26	28	24
List quartile 2 (%)	24	27	23	25	23
List quartile 3 (%)	25	24	25	21	28**
List quartile 4 (%)	25	23	26	26	24
Surname letter	12	11	12	11	12
White (%)	64	64	64	64	64
Black (%)	6	10	5	7	5
Asian (%)	7	7	7	7	8
Hispanic (%)	11	10	11	10	12
Multiracial (%)_	6	5	7	6	7
Not reported (%)	5	5	6	6	4
Pell recipient (%)	19	21	18	20	18
First-generation (%)	11	12	11	11	12
Mid-Atlantic (%)	47	49	46	45	48
Midwest (%)	8	7	8	7	9
New England (%)	18	17	19	18	18
South (%)	12	13	11	13	10
West (%)	14	13	15	15	13
Other (%)	1	0	2	1	1
SAT†	1259	1258	1260	1262	1257
High school GPA†	3.74	3.66	3.76***	3.76	3.71
Home zip income† ($)	89,805	92,480	88,891	88,009	91,966
Home zip college† (%)	27	27	27	26	28**
N	800	200	600	450	350

NOTE: Surname letter is the rank of students' last-name first letter in alphabet (e.g., A = 1, B = 2, and so on). List quartile refers to the quartile of the alphabetically sorted list in which the student's surname falls, which was used to make peer advising assignments.
† This variable is missing for a small share of students.

Table 2 Means by Sex (balancing test)

	Male students		Female students	
	Male adviser (1)	Female adviser (2)	Female adviser (3)	Male adviser (4)
SAT†	1273	1252	1260	1260
High school GPA	3.65	3.66	3.77	3.75
Zip code median income† ($)	91,275	92,930	89,462	86,964
Zip code % college† (%)	27	27	27	27
White (%)	67	63	64	64
Black (%)	4	9*	5	5
Asian (%)	9	6	8	7
Hispanic (%)	7	12	11	10
Multiracial (%)	4	6	6	8
Not reported (%)	9	3	5	6
First-generation (%)	11	13	12	9
Pell recipient (%)	24	20	19	17
Mid-Atlantic (%)	40	42	46	47
Midwest (%)	5	8	8	9
New England (%)	25	14**	19	18
South (%)	13	14	11	10
West (%)	16	12	14	15
Other(%)	0	1	2	0
N	50	150	450	150

NOTE: † This variable is missing for a small share of students. *** p < 0.01; ** p < 0.05; * p < 0.1 indicate the statistical significance of the mean difference between columns 1 and 2, and between columns 3 and 4.

Table 3 Unconditional First Stage and Reduced Form Cross Tabs

	Same-sex advisor (1)	Different-sex advisor (2)	Difference (1) − (2) (3)
% met advisor			
Male student (%)	60.0	43.5	16.5**
Female student (%)	57.2	55.5	1.7
All students (%)	57.5	49.3	8.2**
% retained			
Male student	90.9	81.6	9.3*
Female student	87.7	86.7	0.1
All students	88.1	84.0	4.0
% ≥ C (2.0) GPA			
Male student (%)	94.3	94.6	−0.3
Female student (%)	96.1	96.1	−0.0
All students (%)	95.8	95.3	−0.5

NOTE: $N = 800$. Grade point average (GPA) is on a 4.0 point scale. 2.0 is the cutoff for academic probation. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 4 Effect of Peer Advisor Sex (linear probability models)

	First stage: met advisor		Reduced form: retention	
	(1)	(2)	(3)	(4)
A. Baseline model				
Male student	−0.136	−0.132	−0.061	−0.054
	(0.047)***	(0.048)***	(0.036)*	(0.035)
Male advisor	−0.017	0.094	−0.010	0.092
	(0.049)	(0.134)	(0.033)	(0.105)
Male student × male advisor	0.184	0.183	0.103	0.111
	(0.092)**	(0.096)*	(0.060)*	(0.062)*
Wild cluster p value	[0.094]	[0.074]	[0.038]	[0.038]
Adjusted R^2	0.008	0.006	0.001	0.021
B. Heterogeneity by Cohort				
Male student	−0.132	−0.120	−0.111	−0.106
	(0.069)*	(0.071)*	(0.055)**	(0.053)**
Male advisor	−0.023	0.081	−0.050	0.077
	(0.068)	(0.142)	(0.049)	(0.111)
Male student × male advisor	0.262	0.242	0.183	0.190
	(0.133)**	(0.147)	(0.092)**	(0.099)*
Male student × 2014 cohort	−0.007	−0.024	0.095	0.098
	(0.095)	(0.097)	(0.072)	(0.071)
Male advisor × 2014 cohort	0.011	0.016	0.080	0.023
	(0.098)	(0.103)	(0.066)	(0.070)
Male S × Male A × 2014 cohort	−0.137	−0.101	−0.155	−0.144
	(0.183)	(0.191)	(0.121)	(0.127)
2014 cohort	−0.031	−0.039	−0.026	−0.017
	(0.047)	(0.049)	(0.031)	(0.031)
Adjusted R^2	0.006	0.003	0.001	0.020
Join significance of 2014 interaction terms (p value)	0.80	0.88	0.44	0.55
Cohort fixed effects (FE)	Yes	Yes	Yes	Yes
Sociodemographic controls	-	Yes	-	Yes
High school GPA	-	Yes	-	Yes
Region FE	-	Yes	-	Yes
First letter FE	-	Yes	-	Yes
Zip code controls	-	Yes	-	Yes

NOTE: $N = 800$. Standard errors in parentheses are robust to heteroscedasticity. Square brackets contain p values that are robust to clustering by peer advisor (8 clusters). These p values were computed via 1,000 wild cluster bootstrap replications (Cameron, Gelbach, and Miller 2008). Sociodemographic controls include a set of race FE, a Pell recipient indicator, and a first-generation college student indicator. Zip code controls include median income and percent of adults with a college degree in the student's home zip code. *** p < 0.01; ** p < 0.05; * p < 0.1.

Table 5 Heterogeneity in Effect of Peer Advisor Gender (linear probability models)

X =	First generation		Pell recipient		Black and Hispanic		Mid-Atlantic	
Y =	Met advisor	Retained	Met advisor	Retained	Met advisor	Retained	Met advisor	Retained
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male student	−0.121	−0.061	−0.152	−0.063	−0.117	−0.053	−0.164	−0.027
	(0.052)**	(0.037)*	(0.054)***	(0.038)*	(0.054)**	(0.039)	(0.066)**	(0.050)
Male advisor	0.102	0.094	0.021	0.067	0.089	0.096	0.154	0.127
	(0.135)	(0.107)	(0.136)	(0.108)	(0.137)	(0.108)	(0.141)	(0.105)
Male S × Male A	0.205	0.143	0.220	0.157	0.205	0.121	0.177	0.116
	(0.102)**	(0.062)**	(0.109)**	(0.068)**	(0.104)**	(0.067)*	(0.127)	(0.079)
Male student × X	−0.098	0.046	0.103	0.044	−0.076	−0.010	0.059	−0.056
	(0.139)	(0.113)	(0.121)	(0.090)	(0.119)	(0.092)	(0.097)	(0.071)
Male advisor × X	0.039	0.058	0.311	0.116	0.124	0.011	−0.126	−0.071
	(0.164)	(0.135)	(0.123)**	(0.070)*	(0.140)	(0.096)	(0.098)	(0.068)
Male S × Male A × X	−0.241	−0.300	−0.253	−0.232	−0.236	−0.096	0.009	−0.031
	(0.294)	(0.262)	(0.216)	(0.150)	(0.275)	(0.198)	(0.191)	(0.125)
X	−0.039	−0.105	−0.052	−0.001	−0.046	0.021	−0.022	−0.030
	(0.077)	(0.060)*	(0.065)	(0.042)	(0.074)	(0.048)	(0.157)	(0.087)
Adjusted R^2	0.005	0.017	0.020	0.006	0.005	0.020	0.010	0.021
Join sig. of X interactions (*p* value)	0.51	0.71	0.08	0.28	0.51	0.94	0.44	0.30

NOTE: N = 800. Standard errors in parentheses are robust to heteroscedasticity. All models condition on the full sets of sociodemographic controls, cohort FE, and “first letter of surname” FE. *** p < 0.01; ** p < 0.05; * p < 0.1.

Table 6 The Effect of Peer Advising on Retention

	Naïve	Naïve	IV	IV
A. Linear probability model	(1)	(2)	(3)	(4)
Coefficient estimate	0.049	0.049	0.557	0.665
	(0.025)*	(0.025)*	(0.363)	(0.371)*
First stage instrument: same sex advisor	N/A	N/A	*	*
First stage instrument: male advisor	N/A	N/A		
First stage F statistic (joint significance)	N/A	N/A	2.36*	2.44*
Control function endogeneity test	N/A	N/A	−0.512	−0.621
			(0.301)*	(0.312)**
B. LIML				
Coefficient estimate	N/A	N/A	0.557	0.672
			(0.363)	(0.376)*
C. Probit model				
Coefficient estimate	0.227	0.252	1.016	1.259
	(0.113)**	(0.120)**	(0.611)*	(0.530)**
Average partial effect (APE) estimate	0.048	0.050	0.237	0.275
	(0.024)**	(0.024)**	(0.165)	(0.139)**
First stage instrument: same sex Advisor	N/A	N/A	**	**
First stage instrument: male advisor	N/A	N/A	*	*
First stage χ^2 statistic (joint significance)	N/A	N/A	5.74*	7.85**
Rho statistic	N/A	N/A	−0.50	−0.64
Cohort fixed effects (FE)	Yes	Yes	Yes	Yes
Sociodemographic controls	-	Yes	-	Yes
High school GPA	-	Yes	-	Yes
Region FE	-	Yes	-	Yes
First letter FE	-	Yes	-	Yes
Zip code controls	-	Yes	-	Yes

NOTE: N = 800. Standard errors in parentheses are robust to heteroscedasticity (linear models only). Sociodemographic controls include a set of race FE, a Pell recipient indicator, and a first generation college student indicator. Zip code controls include median income and percent of adults with a college degree in the student's home zip code. LIML = Limited Information Maximum Likelihood; IV = Instrumental Variables; LPM = Linear Probability Model. Control function endogeneity test reports the estimated coefficient and standard error on the first stage residual in the control function. Rho is the correlation between the two error terms in the bivariate probit model. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 7 Heterogeneous Effects of Peer Advising on Retention

X	First generation		Pell recipient		Black and Hispanic		Mid-Atlantic	
Specification	Naïve LPM	IV 2SLS	Naïve LPM	IV 2SLS	Naïve LPM	IV 2SLS	Naïve LPM	IV 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Attended	0.032	0.692	0.046	0.660	0.043	0.632	0.017	0.476
	(0.026)	(0.373)*	(0.028)*	(0.378)*	(0.026)	(0.361)*	(0.031)	(0.389)
Attended × X	0.154	−0.663	0.013	−0.398	0.023	−0.197	0.057	0.319
	(0.090)*	(0.820)	(0.065)	(0.479)	(0.040)	(0.621)	(0.032)*	(0.544)
Net effect of A for X	0.186**	0.029	0.059	0.263	0.0666	0.435	0.074	0.795
	(0.087)	(0.859)	(0.059)	(0.280)	(0.042)	(0.634)	(0.029)***	(0.417)*
First stage 1 (attended)								
IV: Male A	N/A		N/A		N/A		N/A	*
IV: Same sex A	N/A	*	N/A	**	N/A	*	N/A	*
IV: Male A × X	N/A		N/A	*	N/A		N/A	
IV: Same sex × X	N/A		N/A		N/A		N/A	
F-stat	N/A	1.88*	N/A	3.30***	N/A	1.88*	N/A	2.29**
First stage 2 (attended × X)								
IV: Male A	N/A		N/A		N/A		N/A	**
IV: Same sex A	N/A	*	N/A		N/A	*	N/A	
IV: Male A × X	N/A		N/A	**	N/A		N/A	
IV: Same sex × X	N/A		N/A		N/A		N/A	*
F-stat	N/A	0.94	N/A	1.92*	N/A	1.19	N/A	1.62
Control function *p* value	N/A	0.046	N/A	0.085	N/A	0.148	N/A	0.047

NOTE: $N = 800$. Standard errors in parentheses are robust to heteroscedasticity. All models condition on the full sets of sociodemographic controls, cohort FE, and "first letter of surname" FE. IV = instrumental variables; LPM = linear probability model. Control function p value reports the joint significance of the first stage residuals (control function). *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 8 Responses to Online Exit Survey

	Strongly disagree (1)	Disagree (2)	Agree (3)	Strongly agree (4)
A. Please indicate your level of agreement with each statement: (%)				
I felt comfortable talking with my peer advisor.	0	0	25	72
I felt welcome in the peer advising office.	0	0	27	70
My peer advisor listened to my questions/concerns/comments.	0	0	23	75
My peer advisor was knowledgeable about academic advising information and campus resources.	0	0	27	70
Overall, my peer advisor provided me with information that will be helpful in my transition to AU.	0	0	27	70

B. Comments (all)

Comment a little more about how to sign up for classes and plan that out. Other than that the program is really good and it helped me out a lot.

[Female peer advisor] was great and so helpful! I definitely look to her as a resource for CAS specific questions as well as any AU questions in general!

[Female peer advisor] is great!

[Female peer advisor] is super nice!

[Female peer advisor] was fantastic!

I loved meeting with my peer advisor. She was extremely helpful and understanding of my questions and provided in depth and helpful responses.

Make students more aware earlier in the semester that this is available, because it's much less intimidating to ask questions about registration of a peer than of a faculty member all the time.

[Male peer advisor] is a wonderful peer advisor. He is professional, organized, and kind.

The course book was a very helpful gift.

The peer advising program was very helpful for me, I was feeling very alone/lost/unsure, but chatting with my advisor assured me I was on the right track

This resource should really be praised! The informal setting and meeting with your peers is the perfect place for students to let down their guard and get help and learn new things. The advisors are so kind and considerate and I can't wait to go back!

NOTE: *N* = 71. Rows do not sum to 100 percent because two students occasionally skipped questions.

Table 9. Online Exit Survey Pre-Post Analysis (transition matricies, % responding)

Before meeting with peer advisor	After meeting with peer advisor				
	Strongly disagree (1)	Disagree (2)	Agree (3)	Strongly agree (4)	Total (5)
I understood the General Education Program requirements					
Strongly disagree	0	**0**	**0**	**0**	0
Disagree	0	0	**6**	**10**	16
Agree	0	0	13	**35**	48
Strongly agree	0	0	0	36	36
Total	0	0	19	81	100
I was/am aware of campus resources (e.g., Writing Center, Tutoring Lab, Support Center, etc.)					
Strongly disagree	0	**0**	**0**	**0**	0
Disagree	0	1	**3**	**3**	7
Agree	0	0	45	**33**	78
Strongly agree	0	0	0	14	14
Total	0	1	48	51	100
I understood/understand the spring semester registration process (dates, requirements, etc.)					
Strongly disagree	1	**3**	**4**	**1**	10
Disagree	1	6	**20**	**7**	35
Agree	0	0	19	**26**	45
Strongly agree	0	0	0	10	10
Total	3	9	43	45	100
I feel comfortable with the process of exploring and choosing a major					
Strongly disagree	0	**0**	**4**	**0**	4
Disagree	0	1	**7**	**4**	13
Agree	0	0	22	**32**	54
Strongly agree	0	0	3	26	29
Total	0	1	36	62	100

NOTE: $N = 69$. All questions, including those about preprogram feelings, were asked after students met with their peer advisor. Numbers are only reported for students who answered both the pre- and post- questions. Bold cells count the percentage of students who reported higher levels of understanding, awareness, and comfort with campus resources after meeting with their peer adviser.

Online Appendix Table A.1 The Effect of Peer Advising on Retention by Match Status

	Naïve OLS	Naïve OLS	IV 2SLS	IV 2SLS
	(1)	(2)	(3)	(4)
Attended mismatched (IV)	0.013	0.013	0.550	0.774
	(0.035)	(0.039)	(0.499)	(0.586)
Attended matched (IV)	0.066	0.066	0.551	0.748
	(0.026)**	(0.026)**	(0.448)	(0.511)
P-value (mismatched − matched)	0.120	0.175	0.98	0.81
First stage 1 (attended mismatched advisor)				
IV: Male advisor mismatched	N/A	N/A	***	***
IV: Male advisor matched	N/A	N/A	***	***
F-stat	N/A	N/A	140.01***	130.22***
First stage 2 (attended matched advisor)				
IV: Male advisor mismatched	N/A	N/A	***	***
IV: Male advisor matched	N/A	N/A	***	***
F-stat	N/A	N/A	344.61***	150.92***
Control function endogeneity test IV1	N/A	N/A	−0.539	−0.753
			(0.418)	(0.504)
Control function endogeneity test IV2	N/A	N/A	−0.487	−0.691
			(0.371)	(0.442)
Control function endogeneity joint p value	N/A	N/A	0.42	0.28
Cohort fixed effects (FE)	Yes	Yes	Yes	Yes
Sociodemographic controls	-	Yes	-	Yes
High school GPA	-	Yes	-	Yes
Region FE	-	Yes	-	Yes
First letter FE	-	Yes	-	Yes
Zip code controls	-	Yes	-	Yes

NOTE: $N = 800$. Standard errors in parentheses are robust to heteroscedasticity (linear models only). Sociodemographic controls include a set of race FE, a Pell recipient indicator, and a first-generation college student indicator. Zip code controls include median income and percent of adults with a college degree in the student's home zip code. LIML = Limited Information Maximum Likelihood; IV = Instrumental Variables; LPM = Linear Probability Model. Control function endogeneity test reports the estimated coefficient and standard error on the first stage residual in the control function. Rho is the correlation between the two error terms in the bivariate probit model. *** p < 0.01; ** p < 0.05; * p < 0.1.

Online Appendix Table A.2 Effect of Peer Advisor Gender (probit coefficients)

	First stage: met peer advisor		Reduced form: retention	
	(1)	(2)	(3)	(4)
1. Female S and Female A	Omitted	Base	Group	
2. Female S and Male A	−0.044	0.259	−0.049	0.464
	(0.123)	(0.386)	(0.156)	(0.515)
3. Male S and Female A	−0.342	−0.342	−0.260	−0.254
	(0.120)***	(0.124)***	(0.142)*	(0.156)
4. Male S and Male A	0.080	0.389	0.172	0.796
	(0.181)	(0.396)	(0.249)	(0.536)
Test 2 = 3	0.047**	0.124	0.246	0.166
Test 3 = 4	0.035**	0.066*	0.104	0.052*
Test 2 = 4	0.542	0.541	0.418	0.269
Pseudo R^2	0.01	0.04	0.01	0.10
Log likelihood	−541.1	−521.7	−309.7	−279.8
Cohort fixed effects (FE)	Yes	Yes	Yes	Yes
Sociodemographic controls	-	Yes	-	Yes
High school GPA	-	Yes	-	Yes
Region FE	-	Yes	-	Yes
First letter FE	-	Yes	-	Yes
Zip code controls	-	Yes	-	Yes

NOTE: $N = 800$. These specifications are equivalent to those in Table 4 of the main text. Sociodemographic controls include a set of race FE, a Pell recipient indicator, and a first-generation college student indicator. Zip code controls include median income and percentage of adults with a college degree in the student's home zip code. Corresponding average partial effects (APE) reported in Online Appendix Table A.3. *** p < 0.01; ** p < 0.05; * p < 0.1.

Online Appendix Table A.3 Effect of Peer Advisor Gender (probit APE)

	First stage: met peer advisor		Reduced form: retention	
	(1)	(2)	(3)	(4)
1. Female S and Female A	Omitted	Base	Group	
2. Female S and Male A	−0.017	0.098	−0.011	0.091
	(0.048)	(0.146)	(0.033)	(0.101)
3. Male S and Female A	−0.134	−0.129	−0.056	−0.050
	(0.046)***	(0.046)***	(0.030)*	(0.031)
4. Male S and Male A	0.031	0.147	0.037	0.157
	(0.071)	(0.149)	(0.053)	(0.106)
Test 2 = 3	0.046**	0.122	0.246	0.166
Test 3 = 4	0.034**	0.066*	0.104	0.052*
Test 2 = 4	0.542	0.541	0.418	0.269
Pseudo R2	0.01	0.04	0.01	0.10
Log likelihood	−541.1	−521.7	−309.7	−279.8
Cohort fixed effects (FE)	Yes	Yes	Yes	Yes
Sociodemographic controls	-	Yes	-	Yes
High school GPA	-	Yes	-	Yes
Region FE	-	Yes	-	Yes
First letter FE	-	Yes	-	Yes
Zip code controls	-	Yes	-	Yes

NOTE: N = 800. These specifications are equivalent to those in Table 4 of the main text. Sociodemographic controls include a set of race FE, a Pell recipient indicator, and a first-generation college student indicator. Zip code controls include median income and percentage of adults with a college degree in the student's home zip code. These APE are based on probit coefficients reported in Online Appendix Table A.2. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.