IS BELONGINGNESS THE KEY TO INCREASING STUDENT WELLNESS

AND SUCCESS? A LONGITUDINAL FIELD STUDY OF A

SOCIAL-PSYCHOLOGICAL INTERVENTION

AND A UNIVERSITY'S RESIDENTIAL

COMMUNITIES

by

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DISSERTATION ABSTRACT

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Institutions of higher education are replete with programs designed to position incoming undergraduate students to successfully persist toward a degree and to do and be well along the way. This longitudinal field study of incoming students' transitional year focused on outcomes associated with two common types of program: bridge programs and living-learning programs. Bridge programs are intended to boost achievement and persistence of structurally disadvantaged (e.g., low-income) students to close the gap between them and their more advantaged peers, usually with some combination of financial and academic support. Living-learning programs are intended to generally promote achievement and persistence through the intentional formation of communities in which groups of students live together in wings of residence halls and engage in curricular and/or cocurricular activities together. Social-psychological interventions have been inspired by critiques that such programs inadequately support students who are at a structural disadvantage. Specifically, critiques have argued that financial and academic support are insufficient, that students also need psychological support. To strongly test that claim, I replicated one of these interventions within a bridge program and examined whether it affected students' wellness and success at the end of their transitional year, over and above the bridge program itself. I also examined whether living-learning programs contributed to students' wellness and success over and above living in conventional residence halls, and whether either of those two types of residential groups differed from students living off-campus.

iv

Results from the intervention did not fit the theoretical framework on which it was based, the same framework contextualized in the bridge program, or an alternative framework on which other similar interventions are based. Results regarding residential groups suggest that living-learning communities did not augment wellness or success, at least at the particular institution under study. Rather, living on campus generally is associated with a greater sense of social-belonging, higher life satisfaction, more extracurricular activity, and taking advantage of campus resources. Practical advice and recommendations for administrators and researchers are outlined in the Discussion.

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For my mom: I remain convinced to this day that the single best thing she ever did for me was to not give me answers whenever I asked deep or difficult questions, but to instead ask me, "what do you think?" This project is a tangible result of the effect that had on me.

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TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

CHAPTER I

INTRODUCTION

"The desire for interpersonal attachment may well be one of the most far-reaching and integrative constructs currently available to understand human nature."
Baumeister and Leary (1995, p. 522)

Two decades after publication of their seminal article on the need to belong, with ever-increasing citations-per-year and over 500 last year alone (Web of Science, July, 2016), Baumeister and Leary's (1995) concluding statement may be bold, but is unlikely hyperbolic. Indeed, the need to belong is so fundamental to our nature that variation in our subjective sense of belonging – e.g., having positive relationships with others (Walton & G. L. Cohen, 2011) – has been linked to variation in essentially everything that is important in life: psychological and physical wellness (Cacioppo, Hughes, Waite, Hawkley, & Thisted, 2006; Caspi, Harrington, Moffitt, Milne, & Poulton, 2006; DeWall, Deckman, Pond, & Bosner, 2011; Dickerson, Gruenewald, Kemeny, 2004; Hackett, Hamer, Endrighi, Brydon, & Steptoe, 2012; Leary, Kelly, Cottrell, & Schreindorfer, 2013; Mellor, Stokes, Firth, Hayashi, & Cummins, 2008; Van Orden et al., 2008; Walton & G. L. Cohen, 2011) and academic and work success (Cook, Purdie-Vaughns, Garcia, & G. L. Cohen, 2012; Good, Rattan, & Dweck, 2012; Holleran, Whitehead, Schmader, & Mehl, 2011; Lewis & Hodges, 2015; Lewis, Stout, G. L. Cohen, Pollack, Finkelstein, & Ito, submitted; Smith, Lewis, Hawthorne, & Hodges, 2014; Walton & G. L. Cohen, 2007, 2011; Walton, Logel, Peach, Spencer, & Zanna, 2015).

Belongingness, Structural Disadvantage, and Demographic Disparity

Across institutions of higher education, programs abound to position incoming undergraduate students to successfully persist toward a degree and to do and be well along the way. Two common types of program are bridge programs and living-learning programs. Bridge programs are intended to boost achievement and persistence of structurally disadvantaged (e.g., low-income) students to close the gap between them and their more advantaged peers, usually

1

with some combination of financial and academic support. Living-learning programs are intended to generally promote achievement and persistence through the intentional formation of communities in which groups of students live together in wings of residence halls and engage in curricular and/or cocurricular activities together. In addition, recent interest has been generated around brief social-psychological interventions (e.g., Aronson, Fried, & Good, 2002; Cook et al., 2012; Good et al., 2012; Schnabel, Purdie-Vaughns, Cook, Garcia, & G. L. Cohen, 2013; Walton & G. L. Cohen, 2007, 2011; Walton, et al., 2015) that reduce gaps between structurally advantaged and disadvantaged groups (e.g., White and Black students, male and female students) on real-world outcomes (e.g., GPA, retention and graduation rates) that represent constructs (e.g., achievement, persistence) perennially studied by psychologists and educators (e.g., Reilly, Neumann, & Andrews, 2014; Robbins, Lauver, Le, Davis, Langley, & Carlstrom, 2004).

The dominant approach to these interventions follows the Identity Engagement Model (G. L. Cohen & Garcia, 2008), which proposes that when people think that an identity of theirs could be the basis for negative judgment or treatment, they become vigilant for environmental cues that confirm or disconfirm such threat to their identity. Confirmation and disconfirmation can have differential effects on identity-relevant task performance. If the threat is disconfirmed, performance depends on task-relevant factors (e.g., feedback) and individual-relevant factors (e.g., self-efficacy), and current levels of performance can be sustained or improved. If cues confirm the threat, however, normal performance can be inhibited. If people have the ability and/or desire to cope with the threat, current levels of performance can be sustained or improved, but if they do not, a decrement in performance follows. There are two points in this stream at which it is possible to intervene that are supported by field research: when people become vigilant to identity-relevant cues (Walton & G. L. Cohen, 2007, 2011; Walton et al., 2015) or when people experience and appraise identity threat (G. L. Cohen, Garcia, Apfel, & Master, 2006; G. L. Cohen, Garcia, Purdie-Vaughns, Apfel, & Brzustoski, 2009). Intervention at the vigilance phase is the focus of the proposed research.

Interventions at the vigilance phase have been aimed at reducing students' tendency to interpret their experiences related to 'fitting in' as being threatening to their social identities (G. L. Cohen & Garcia, 2008). Through sharing of upper-year students' college transition stories, incoming students are encouraged to see the uncertainty and adversity experienced during their college transition, not as evidence that they or people like them do not belong at college, but as a temporary part of a normal process that everyone experiences and that lessens over time (Walton & G. L. Cohen 2007; 2011). This is not to say that the approach involves sweeping diversity or adversity under the rug by ignoring or downplaying their significance. In fact, the method explicitly acknowledges and validates adversity and does not explicitly emphasize the importance of similarities or minimize the importance of differences, which has a known potential to backfire (Purdie-Vaughns, Steele, Davies, Ditlmann, & Crosby, 2008). Downplaying diversity is not the same as subtly smoothing over diversity. Thus, the approach attempts to normalize adversity through students *seeing*, not being *told* that there is, similarity across diversity.

Owing much to closing GPA gaps between black and white students, this social-belonging approach (Walton & G. L. Cohen, 2007, 2011) has had large academic and practical impacts. Results have been published in leading journals and enjoy healthy citation rates,[1] and they have spurred creation of a nonprofit organization designed to work with college administrations in implementing large-scale interventions (College Transition Collaborative, n.d.). This dominant approach, however effective, is the product of one research group's particular perspective.

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[1] Walton and G. L. Cohen (2007), published in *Journal of Personality and Social Psychology*, which has a 5-year Impact Factor of 7.4 and Hirsh Index of 271, has 242 citations to date. Walton and G. L. Cohen (2011), published in *Science*, which has a 5-year Impact Factor of 35.3 and Hirsch Index of 915, has 158 citations to date. Impact Factors retrieved from http://www.apa.org/pubs/journals/psp and https://www.sciencemag.org/about/science-1997-current-issue, Hirsch Indices retrieved from http://www.scimagojr.com/index.php, and citation counts retrieved from http://apps.webofknowledge.com, July 14, 2016. See http://wokinfo.com/essays/impact-factor for calculation of Impact Factor and Hirsch (2005) for calculation of Hirsch Index.

Another approach (Stephens, Hamedani, & Destin, 2014), which is in many ways similar but in one central way is conceptually opposite, recently appeared in another high-impact journal.[2] It aims to inform disadvantaged incoming students how their particular backgrounds matter in college transition. In so doing, upper-year first-generation and continuing-generation students' transition stories were *contrasted*. Similar to how the social-belonging intervention worked for Black students compared to White students, the difference-education intervention increased students' sense of belonging and reduced the GPA gap between first-generation and continuing-generation students.

Both interventions share a theoretical root: that people's senses of who they are and their place in the world derives in part from their group memberships (e.g., social class; Tajfel & Turner, 1979). Both have the same purpose: reduce achievement gaps between structurally advantaged and disadvantaged students by increasing achievement of disadvantaged students. Both are inserted at the same phase of the Identity Engagement Model (G. L. Cohen & Garcia, 2008): the vigilance phase. Both are delivered through the same medium: sharing college transition stories of demographically-diverse, upper-year students with incoming students. Both appear to shift students' thinking toward the same general psychological state: an unthreatened identity. Both affect the same variables in the same directions: belonging, social identity threat, GPA, etc. However, the interventions are tactically opposite. The social-belonging approach, refined by work on stereotype threat (Steele & Aronson, 1995) and implicit theories of intelligence and personality (Dweck, 1999), presents transition stories that are *similar* across upper-year students' demographic characteristics, whereas the difference-education approach, refined by work on multicultural education (Gurin & Nagda, 2006), presents stories that *differ* by upper-year students' demographic characteristics.

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[2] Stephens et al. (2014), published in *Psychological Science*, which has a 5-year Impact Factor of 6.3 and Hirsch Index of 188, has 24 citations to date. Impact Factor retrieved from https://us.sagepub.com/en-us/nam/impact-factor-ranking-results, Hirsh Index retrieved from http://www.scimagojr.com/index.php, and citation counts retrieved from http://apps.webofknowledge.com, July 14, 2016.

The mechanisms through which the interventions are proposed to operate are also very different. The social-belonging intervention is proposed to increase achievement of disadvantaged students by uncoupling the contingency of their sense of belonging on adverse or negative experiences (Walton & G. L. Cohen, 2011), whereas difference-education is proposed to increase achievement of disadvantaged students by increasing their tendency to seek college resources for help and support (e.g., going to the writing center; Stephens et al., 2014). These are not mutually exclusive mechanisms, suggesting potential for synthesis. Further, Walton and G. L. Cohen (2007) suggested that the social-belonging intervention "might benefit at least some White students—such as first-generation college students wondering whether they belong in an elite institution [as did difference-education]" (p. 88), which further suggests that the social-belonging intervention and the difference-education intervention may work the same way for the same people despite opposite messaging tactics. However, Stephens and colleagues' (2014) challenged the notion that "difference-blind approaches [such as the social-belonging approach] are the optimal way to reduce threat" (p. 950), a contention that may not be entirely warranted given all of the parallels noted above and the potential that both might be explained by a common set of mechanisms.

Why Are Social-Psychological Interventions Needed?

The short answer is that achievement and persistence gaps between structurally advantaged and disadvantaged groups of college students still exist in the face of programs aimed at reducing them (Stephens et al., 2014). Indeed, Stephens et al. (2014) modeled their control condition on such bridge programs, which tend to focus on academic skills like studying for exams and choosing majors, referring to it as the "standard" approach to convey its generality across institutions of higher education. Therefore, an appropriate and strong test of the intervention is to conduct it within a bridge program.

Further, Stephens et al. (2014) include in the standard approach, efforts like living-learning programs (LLPs), citing Inkelas, Daver, Vogt, and Leonard (2007), which is a multi-

5

institution study of LLPs focusing on the benefits they confer to first-generation college students. This categorization of LLPs may not be the entirely fair. To be sure, LLPs do focus on academics, but there is also a social component that should not be overlooked. In particular, LLPs are "communities in which students not only pursue a curricular or cocurricular theme together but also live together in a reserved portion of a residence hall" (Inkelas, Vogt, Longerbeam, Owen, & Johnson, 2006, p. 40). These programs thus afford students the opportunity to forge lasting friendships on shared interests and common goal pursuits.

Although there may be some unintended negative consequences that generally arise from group dynamics (e.g., division into cliques, socializing during class time; Jaffee, Carle, & Phillips, 2008), evidence points to positive effects. Students who participate in LLPs may be more academically and socially integrated (Inkelas & Weisman, 2003; Jaffee et al., 2008) and have smoother transitions (Inkelas, et al., 2007; Inkelas & Wiseman, 2003) compared to peers in conventional residence halls, and may be more likely to persist, have higher grades, engage in campus events and activities, and interact with peers and faculty (Inkelas & Weisman, 2003; Pike, 1999; Pike, Schroeder, & Berry, 1997; Soldner, Rowan-Kenyon, Inkelas, Garvey, & Robbins, 2012; Stassen, 2003). Moreover, structurally disadvantaged groups like first-generation students also seem to benefit (Inkelas et al., 2007).

The hedging of these claims of benefit is important and alludes to an eminently fair critique of research on LLPs: It is not at all clear whether such programs confer benefits to students or the students who participate in them are just different from students who do not, given that students self-select into the programs. What is clear is that bridge programs are the legitimate target of Stephens and colleagues' (2014) critique of the standard approach. These programs are founded on the assumption that structurally disadvantaged students may lack the financial means and academic preparation necessary to persist through college and to do and be well along the way; and the solution they offer is, straightforwardly, financial and academic support. Among

other things, the present research will test Stephens and colleagues' (2014) claim that this is insufficient; that students also need psychological support.

Overview of the Present Research

I conducted a longitudinal field study of incoming first-year undergraduate students' transitional year (2015-16) with four measurement waves: one pre-matriculation and three post-matriculation in the latter half of each academic quarter. I directly replicated and preregistered on Open Science Framework[3] Stephens and colleagues' (2014) difference-education intervention. Alterations to the intervention procedure itself were minimal; however, I implemented several extensions insofar as measurement and analysis. All of these are described in detail in Appendix A. One key extension to the intervention is that it was conducted within a bridge program: the target of Stephens and colleagues' (2014) critique of the standard approach to closing achievement gaps. The intervention was delivered through a set of panel-discussion workshops at the beginning of the year, where upper-year bridge-program students shared their transition stories with incoming bridge-program students. This is a strong test of the intervention's efficacy to increase students' wellness (psychological and physical) and success (engagement, achievement, and persistence), because it explicitly tests whether the psychological support it ostensibly provides will benefit students over and above the financial and academic support already provided by the bridge program. I also tested plausible causal chains stemming from the intervention, focusing on improving model specification of and synthesizing the proposed mechanisms from the difference-education (Stephens et al., 2014) and social-belonging (Walton & G. L. Cohen, 2007, 2011) frameworks. Additionally, I investigated how existing university residential programs contribute to variance in the same outcomes that were examined in testing the intervention. This portion of the study compared students who lived on campus who participated in one of two types of LLPs (i.e., both structured community and shared curricular/cocurricular experiences), students who lived on campus but did not participate in an

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[3] https://osf.io/n4t75/?view_only=28d11f3bbf9449e491eb536d948e2206

LLP (i.e., structured community experience but not shared curricular/cocurricular experiences), and students who lived off-campus (i.e., neither structured community nor shared curricular/cocurricular experiences).

Hypotheses for the Intervention

Key constructs measured by Stephens et al. (2014) and Walton and Cohen (2011) were measured in the same or similar fashion along with additional constructs of interest. Generally, the difference-education intervention should affect outcomes related to student wellness and success in normatively positive ways (e.g., higher life satisfaction, fewer instances of illness, etc., for intervention vs. control). However, the extent to which treatment condition might interact with generation status across outcomes is unclear for two reasons. First, Walton and G. L. Cohen (2011) and Stephens et al. (2014) reported different findings for the same or similar constructs. Specifically, Walton and G. L. Cohen (2007, 2011) found interactions between treatment and race on several psychological and physical wellness outcomes, such that Black and White students did not differ in the treatment condition but Black students had poorer health than did White students in the control condition. However, Stephens et al. (2014) found no interactions between treatment and generation status on similar or identical outcomes, only main effects of treatment. This discrepancy could be for a number of reasons: the interventions operate differently, the demographic groups studied respond to the interventions differently, the intervals of time between intervention and follow-up differed, etc. For example, Stephens et al. (2014) report assessments of psychological wellness variables gathered at the end of students' first year and Walton and G. L. Cohen (2011) report assessments from the end of students' third year. Perhaps the interventions operate similarly, but the effects start out as main effects of treatment condition and interactions between treatment condition and demographic groups emerge over time; the processes involved are theorized to be recursive, after all (G. L. Cohen & Garcia, 2008; G. L. Cohen et al., 2009; Yeager & Walton, 2011; Yeager, Walton, & G. L. Cohen, 2013).

Second, the present research involves implementation of the difference-education intervention within the kind of bridge program that Stephens et al. (2014) argue is insufficient for closing achievement gaps between more and less advantaged students. However, students in the bridge program (described in the Method section below) are, in one key respect, all at a structural disadvantage relative to their demographic counterparts: they are all from low-income families, which is the central criterion for admittance to the program. Moreover, the intervention was delivered by panelists who are upper-year peers to incoming bridge-program students. Given those facts, it might be reasonable to expect only main effects of treatment across outcomes, even though the intervention condition contrasted transition stories of first-generation and continuing-generation students.

Hence, the intervention should produce, at minimum, main effects of treatment condition, and possibly theoretically consistent interactions with generation status. If interactions occur, first-generation students in the intervention condition should have, on average, higher life satisfaction, lower frequency of illness, higher GPA, etc. compared to first-generation students in the control condition, and they should not differ (or at least differ less) from continuing-generation students in either condition. In terms of main effects, students in the intervention condition (compared to control) should, by the end of their transitional year, have:

- Psychological Wellness
    - higher life satisfaction
    - higher self-esteem
    - lower sadness
    - lower stress
- Physical Wellness
    - better general health
    - lower frequency of illness
    - lower body mass index
- Success
    - greater engagement
        - greater academic enjoyment
        - more extracurricular activity
        - greater resource seeking

9

- higher achievement[3]
- greater persistence[4]
- Belongingness
  - greater sense of social-belonging[5]

Mechanisms. The matter of mechanism is a complicated one. Sufficiently bracketing the causal chain is no easy task and is not the primary purpose of the present research. However, this study is a replication and Stephens et al. (2014) tested a putative mechanistic model: the difference-education intervention increases first-generation students' tendency to seek resources for support, which leads to increased achievement. In addition, a focal point of the present research is on belongingness being central to improving student wellness and success and Stephens did not propose or test this as a mechanism. However, if the difference-education intervention and the social-belonging intervention operate similarly, albeit with tactically opposite messaging, then belonging may yet be involved in the causal chain.

Resource seeking as mechanism. Stephens et al. (2014) explained their intervention's effect on achievement by arguing that it increased first-generation students' tendency to seek college resources (e.g., going to the writing center), which in turn boosted their GPA. Specifically, at the end of the academic year post-intervention, they measured students' tendency to seek support from campus resources and services during the previous year. In supplemental analyses, they reported that none of the other constructs they measured, again at the end of the year, mediated the effect. Stephens and colleagues' (2014) mechanism is easy to understand and measurement of it is easy to implement. In practice, the findings could serve to legitimize the creation, existence, or augmentation of various student resources and services.

However, there are several features of Stephens et al.'s design and measurement that could be improved. First, the timing/interval of measurement was questionable. Measurement followed a simple pre-post design with baseline measurements of some constructs taken pre-

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[4] Achievement and persistence are measured by grade-point average and enrollment data described in the Student Records section of the Method.
[5] This hypothesis was not specified in the preregistration, but it made sense to treat it as an outcome and the predicted direction of the effects hypothesized is rather straightforward.

intervention and measurements of different post-intervention outcomes measured at the end of the academic year. This means that measurement of the mediator occurred at the end of the growth period under examination, along with the focal outcome, cumulative first-year GPA. Second, this design feature meant asking students to recall the number of times they sought resources over the entire year. Third, there were only three resources about which students were asked to recall, but three items make a sparse set of behavioral indicators for neither being referenced in the literature as representative of the intended construct nor pretested by the researchers. Fourth, the time frames of recall were different for different items. One item asked: "Since you started at [university name], in a typical month, how many times have you emailed a professor to ask a question?" The other two items asked: "Since you started at [university name], how many times have you met with a professor outside of class?" and "Please estimate how many times you have gone to the writing center since you started at [university name]." (p. 10 Supplemental Material). Presumably the reason for the difference is that the frequency of the first behavior is likely to be higher than the frequencies of the second and third behaviors and the researchers wanted the behaviors to be on the same truncated scale from 0 to 5. Fifth, the items were on an arbitrarily truncated count scale. I addressed these methodological issues by (a) using an expanded, 20-item measurement of resource seeking, which included the original three items adapted to the local context of the university where my research was conducted (the University of Oregon), (b) collected at multiple points throughout the transitional year that (c) asked students for non-truncated frequency estimates regarding (d) a shorter, one-month period of recall.

There are two additional issues, one analytical and one theoretical. The analytical issue is that Stephens et al. reported a mediated moderation model where resource seeking mediated the interaction between generation status and treatment condition affecting GPA. However, their model appears to have been specified as GPA regressed on resource seeking and the treatment by generation interaction term and resource seeking regressed on the interaction term. That is, treatment condition and generation status may not have been included in the model, only their

11

interaction term. If this is what they did, their model is incorrectly specified; the terms that form the interaction term also need to be included in the model.

The theoretical issue is that resource seeking, being self-reported behavior, does not address whether something psychological, such as change in sense of belonging or an uncoupling of belonging from adversity, occurred before students took advantage of services and possibly motivated or encouraged them to do so. Indeed, the difference between the intervention and control conditions on sense of social-belonging measured at the end of the year was one of the largest differences reported by Stephens et al., suggesting that there is room for substantial change throughout the transitional year. Might disruption of the belonging-adversity contingency be the main psychological consequence of the difference-education intervention, as it was of Walton and G. L. Cohen's (2011) social-belonging intervention?

Belonging-adversity contingency as mechanism. Walton and G. L. Cohen (2011) used an intensive data collection method (one week of twice-daily experience sampling immediately following intervention) and a sophisticated technique (correlating daily level of experienced adversity with composite same-day and subsequent-afternoon sense of social-belonging) to create a mechanism metric that illustrates the contingency belonging has on adversity and the intervention's efficacy to disrupt that contingency. Specifically, belonging was not contingent on adversity for White students regardless of treatment condition and for Black students in the intervention condition (i.e., the correlation between belonging and adversity was near-zero), but belonging was contingent on adversity for Black students in the control condition. Thus, the intervention reduced the GPA gap between Black and White students by eliminating the effect of experienced adversity on sense of belonging.

There are actually two conditional indirect effects packed into this set of ideas. One of them was fully represented analytically, but seems tangential to the theoretical argument and the other was not fully represented analytically, but seems central to the theoretical argument. Specifically, the analysis represented the idea that the relation between contingency and GPA was

moderated by race, but it did not represent the idea that the relation between belonging and adversity was moderated by intervention. It is unclear why race was specified as a second-stage moderator (Edwards & Lambert, 2007; Kline, 2011) rather than treatment, given that their argument structurally implies that the intervention altered one causal path (race → GPA) by altering another causal path (adversity → belonging). Moreover, there are in fact five variables, not four, at play in Walton and G. L. Cohen's model: race, treatment, belonging, adversity, and GPA. What was expressed as one variable, viz. contingency (i.e., the relationship between belonging and adversity), is where the conditional part of the conditional indirect effect crucial to the theoretical argument is supposed to occur.

In addition, Walton and G. L. Cohen found that neither mean-level adversity nor mean-level belonging predicted achievement in terms of GPA; the relation (or lack thereof) between adversity and belonging did. Together, the lack of evidence for mean-level belonging predicting GPA and evidence for mediation by the *relationship between* belonging and adversity seem to suggest that belonging on its own is not exactly the mechanism. However, it is well known in longitudinal research that timing of measurement is crucially important for understanding developmental processes (Gollob & Reichardt, 1987; Selig & Preacher, 2009). Walton and G. L. Cohen (2011) measured their mediator during the week after intervention, which does not allow much time for change to occur. Yet sense of belonging has been shown to change over time after exposure to belongingness-relevant events (e.g., Cook et al. 2012; Good et al., 2012). Moreover, Walton and Cohen's outcome (GPA) was measured at a very distal three years following the intervention and measurement of the mediator. This gap in measurement cannot account for incremental growth in constructs of interest.

Thus, there are two points of potential model misspecification in Walton and G. L. Cohen's (2011) analysis: interval/timing of measurement and expression of relationships between variables. To address these issues, I estimated indirect effects of generation status on first-year

13

GPA through resource seeking conditioned on treatment, representing all of the variables relevant to the theoretical propositions.

Common mechanisms? My aims are to (a) test Stephens and colleagues' (2014) mechanism model, (b) test whether the difference-education intervention has the same *psychological* consequence as Walton and G. L. Cohen (2011) theorized for their social-belonging intervention, and (c) synthesize the mechanism models proposed by Stephens et al. (2014) and Walton and G. L. Cohen (2011) by adding resource seeking as a behavioral consequence of the uncoupling of the belonging-adversity contingency. In plain language, the theoretical process is this: First-generation students, on average, have a steeper increase in stress over their transitional year than do continuing-generation students. Steeper increases in stress are associated with steeper decreases in sense of belonging. The intervention condition disrupts this pattern for first-generation students. That is, first- generation students exposed to the intervention treatment no longer interpret difficulties as indications that they do not belong at college (i.e., change in stress does not affect change in belonging), which encourages seeking resources for help. Over time, getting help boosts GPA.

Stephens and colleagues' and Walton and G. L. Cohen's models complement and strengthen each other. Walton and G. L. Cohen's model specifies a mental state, but lacks a behavioral event. Stephens and colleagues' model specifies a behavioral event, but lacks a mental state. Their ultimate outcome, GPA, is neither a mental state nor a behavioral event; it is an aggregation, a coarse function of a river of mental states and behavioral events. On their own, mental states say little about what students did that produced changes in GPA. On their own, behavioral events say little about what treatments did to students. Without specification of both mental states and behavioral events, explanations of variation in aggregate outcomes like GPA, while nonetheless practically useful, boarder on meaninglessness in terms of theoretical understanding.

Hypotheses for the Residential Groups

Living-learning programs at the University of Oregon (University of Oregon Housing, Residence Life, 2014a) are overlaid onto a more general program implemented by Residence Life: The Residential Learning Model (RLM; University of Oregon Housing, Residence Life, 2014b). Even this general program entails very social aspects. For example, the RLM involves a deliberate effort to construct communities of students, clustered around resident assistant nuclei (i.e., upper-year, undergraduate student leaders who live in the residence halls along with incoming students), intended to foster, among other things, a sense of belonging. Indeed, resident assistants facilitate students' generation and implementation of community events, and they are even taught to check in with students whom they directly advise, through semi-structured one-on-one conversations during students' transitional year, specifically about how connected students feel to their communities. On top of the basic RLM, there are two general types of LLPs: Academic Residential Communities (ARCs) and Freshman Interest Groups (FIGs). ARCs represent partnerships between Residence Life and academic divisions, colleges, and departments (Carnegie Council for Ethics in International Affairs, Center for Applied Second Language Study, Center for Multicultural Academic Excellence, Charles H. Lundquist College of Business, College of Arts and Sciences, College of Education, Department of East Asian Languages and Literatures, Department of Ethnic Studies, Department of Geography, Department of German and Scandinavian, Department of Romance Languages, Department of Women's and Gender Studies, Division of Undergraduate Studies, Environmental Studies Program, First-Year Programs, Office of Sustainability, Robert D. Clark Honors College, School of Music and Dance), as well as other parts of the Division of Student Life (e.g., the Office of the Dean of Students collaborates on an LGBTQ Cohort–Social Justice ARC), that integrate curricular and cocurricular learning models and in-classroom and out-of-classroom experiences.

Throughout their first year, students in ARCs live together in residence hall/floor clusters and take classes together that are either specialized for the ARC (e.g., there are "Living the

15

Language" ARCs in Chinese, French, German, Japanese, and Spanish) or are part of college-specific first-year curricula (e.g., Clark Honors College). There are two types of FIGs: residential (rFIG) and non-residential (nFIG). Because they more accurately represent the idea of an LLP, rFIGs are the focus of the proposed research. All FIGs represent partnerships between Undergraduate Studies' First-Year Programs and faculty from various disciplines and rFIGs, in particular, add Residence Life to the partnerships. rFIGs consist of small groups of students (up to 25) who live on the same or adjacent floors of residence halls who co-enroll in two introductory, general lecture courses (e.g., Anthropology's "World Cultures" and Psychology's "Mind and Society") and a specialized "College Connections" seminar limited to the small group that connects the two disciplines. Like ARCs, FIGs may also use cocurricular or out-of-class pedagogies. Both ARCs and FIGs also emphasize interaction with faculty and formation of study groups.

To the extent that the university's basic RLM operates in theoretically similar ways as do social-psychological interventions insofar as promoting wellness and success, and to the extent that rFIGs and ARCs do that better than do the more traditional residential communities, predictable differences should emerge. That is, students living in rFIGs and ARCs should score normatively higher on metrics of wellness and success than students living in conventional residence halls, in the same ways described in the previous section about intervention outcomes. And students living in conventional residence halls should, in turn, score higher than students living off-campus. Specifically, students living in rFIGS nad ARCs (compared to students living in conventional residence halls and to students living off-campus) should, by the end of their transitional year, have higher life satisfaction and self-esteem, lower sadness and stress, better general health, lower frequency of illness and body mass index, greater academic enjoyment, more extracurricular activity, greater resource seeking, higher achievement, greater persistence, and greater sense of social-belonging. And students living in conventional residence halls (compared to students living off-campus) should, by the end of their transitional year, have higher

life satisfaction and self-esteem, lower sadness and stress, better general health, lower frequency of illness and body mass index, greater academic enjoyment, more extracurricular activity, greater resource seeking, higher achievement, greater persistence, and greater sense of social-belonging.

CHAPTER II

METHOD

Participants

Five-hundred forty-six incoming, first-time, full-time students responded at least partially to the pre-matriculation survey and/or attended panel-discussion workshops (delivery of intervention and control treatments) at the beginning of the year. Two-hundred forty-seven of these students were in a bridge program and 299 were not. One-hundred fifty-eight bridge-program students participated a workshop (difference-education: $n = 93$, standard-control: $n = 65$), 38 of whom did not participate in the pre-matriculation survey prior to attending a workshop (difference-education: $n = 25$, standard-control: $n = 13$). An additional 89 bridge-program students participated in the pre-matriculation survey but not in a workshop. Students who did not participate in the pre-matriculation survey and did not participate in any post-matriculation wave of surveying, as well as students who participated in the pre-matriculation survey but did not participate in any post-matriculation wave ($n = 162$), were not considered participants. Students who were less than 18 ($n = 24$) and more than 19 years old ($n = 3$) were subsequently excluded. Other analysis-specific quality control exclusions were made on the basis of attention checks, which are described below where relevant.

Before attention-check exclusions, there were ultimately four groups of students that formed conditions for investigating the intervention. There were two treated groups in the bridge program: difference-education ($n = 58$, among them, $n = 31$ first-generation and 1 of unknown status) and standard-control ($n = 36$, among them, $n = 20$ first-generation), comprised of students who participated in a workshop and in post-matriculation surveying. And there were two untreated groups, program-passive-control ($n = 51$, among them, $n = 27$ first-generation and 2 of unknown status), comprised of bridge-program students who did not attend a workshop but participated in post-matriculation surveying, and nonprogram-passive-control ($n = 212$, among them, $n = 42$ first-generation and 5 of unknown status), comprised of students who were not in

18

the bridge program who participated in post-matriculation surveying. How generation status was classified is described in the next section. For investigating residential groups, there were $n = 124$ students living in residential freshman interest groups or academic residential communities (FIGARCs), $n = 260$ students identified as living in a conventional residence hall, and $n = 102$ students living off-campus. Sixty-seven (12%) students did not consent to releasing residential records; information on seven of these was recovered with survey data. How residential groups were classified is also described in the next section. Excluding students who did not participate in post-matriculation waves of surveying and students under 18 or over 19 years old, the sample ultimately consisted of $n = 91$ students living in FIGARCs, $n = 167$ students living in conventional residence halls, and $n = 75$ students living off-campus. However, data pre-processing procedures, described in the last section of the Method, made the groups in analysis models smaller.

Compensation. Students in the bridge program were given $15 Amazon gift codes for participating in workshops/wave 0 and nonbridge-program students were given $10 Amazon gift codes for participating in wave 0. All students were given $10 Amazon gift codes for participating in wave 1 and were entered into a drawing for a set of Amazon gift codes of variable amounts ranging from $10 to $100.

Measurement

Student records. Data on several variables were obtained from student records and matched to survey data. From the Registrar's Office, quarterly GPA (fall, winter, spring) and enrollment (summer, fall of second year) data were obtained for measuring achievement and persistence outcomes, respectively. Cumulative first-year GPA was computed by averaging quarterly GPAs, which is not how it is done in actual practice, but is sufficient for the purposes of the research. If a student had a GPA for only one quarter, that was their cumulative GPA. If a student had no GPA, that meant they dropped out before they could earn any GPA hours.

Generation status (first, continuing) was obtained for the purpose of examining treatment by generation interactions. However, there was a substantial amount of missingness in this record; students can choose whether or not they report their parents' education level in their application. This is what generation status is based on, defined as *both* parents not holding a four-year college degree. I also collected data on parents' education level in the pre-matriculation survey (see Survey procedures and materials for measurement information). Because the survey data had fewer missing data points and because students may have been more inclined to respond honestly in a low-stakes survey situation than in submitting a college application by which their worth was to be judged and future determined, I used survey data for defining generation status. And because missingness was non-overlapping between survey and student records data, I filled in missing survey values with student records values.

Age, gender, race/ethnicity (federal coding), high school GPA, high school class rank, best SAT/ACT scores (ACT converted to SAT scale), number of advanced placement credits transferred, quarterly number of credit hours attempted and earned, high schools from which students graduated, international student status, and transfer status were obtained for potential control/auxiliary covariates, not all of which were used in the present analysis of the data. Age was an exclusionary criterion, as mentioned previously, and gender (coded as 0 = *female*, 1 = *male*), race/ethnicity (recoded as 0 = *American Indian or Alaska Native*, *Black or African American*, *Hispanic or Latino*, *Native Hawaiian or Other Pacific Islander*, and *Two or More Races* [disadvantaged]; 1 = *White* and *Asian* [advantaged], high school GPA, SAT/ACT, and AP credits (recoded as binary: 0 = *did not transfer any AP credits*, 1 = *transferred AP credits*) were all used in various ways (see Results section).

From the Financial Aid Office, indicators of whether or not (1, 0, respectively) students' received (a) a Pell grant, (b) a student loan, (c) work-study, and (d) a scholarship were obtained. These variables were also potential control/auxiliary covariates; all were used in some way.

20

Finally, from University Housing, residence hall and data on participation in residential communities were obtained to form residential groups. Students were classified into three groups: 1) students who were participating in a residential freshman interest group or an academic residential community (FIGARC),[6] students who were not participating in FIGARCs but were living on campus, and students who had no records in housing data were necessarily students living off-campus, unless they did not consent to releasing their residential records. Seven students who did not consent to releasing residential records indicated in-survey that they lived off-campus. Survey data were used to classify these students, otherwise they were treated as missing.

Students consented with yes/no responses to releasing all of these records in discrete, specific chunks at the beginning of the pre-matriculation survey and again on paper if they attended a panel-discussion workshop: (a) educational records (quarterly GPA, enrollment, and credit hours attempted/earned), (b) background records (all of the other variables from the Registrar's Office), (c) financial need records (Pell, loan, work-study, scholarship), and (d) residential records (residence hall and grouping data).

Survey procedures and materials. Incoming students were invited to participate in four waves of survey measurement, administered using Qualtrics (an online survey platform), throughout their transitional year. Bridge-program students and nonbridge-program students were invited to participate in the survey portion of the study in separate emails sent by the Admissions Office on my behalf. The pre-matriculation survey (wave 0) was initiated later than intended, at the beginning of Week of Welcome orientation activities, one week prior to the start of fall classes. For bridge-program students this survey necessarily had to precede the intervention. So, it was closed the day that panel-discussion workshops started (Tuesday of the first week of classes).

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[6] Differences between rFIGs and ARCs would have been explored, but University Housing decided to phase out rFIGs during the year in which the present research was conducted. As a result, there were only nine students in the sample, after initial exclusions, who were living in an rFIG. Therefore, rFIGs and ARCs were combined into one group.

Because there was slightly greater leeway insofar as timing for nonbridge-program students, wave 0 was left open for them until the end of the second week of classes. A little over half (54%) of these students completed the survey before the first day of classes and 97% had completed it by the start of the second week of classes. The other three waves (1-3) were initiated by me via email mid-quarter (sixth week of fall, winter, and spring) and were closed at the end of the last (tenth) week of the quarter before finals week.

All measurement items can be found in Appendix B. Because the pre-matriculation survey (wave 0) is partly intended to capture and control for baseline, pre-transition levels of measured constructs, inclusion and some wording of items in wave 0 differed from inclusion and wording of items in post-matriculation waves. Unless otherwise indicated, items measuring constructs appeared in and were identical in all waves. Several scales used, in their original form, agree/disagree anchors of various numbers and with various modifiers. These measures were streamlined to use the same 7-point scale with the following anchors: *strongly disagree*, *moderately disagree*, *slightly disagree*, *neutral*, *slightly agree*, *moderately agree*, *strongly agree* (henceforth referred to as "agree/disagree anchors"). Generally, higher numbers indicate higher levels of the relevant construct, which may be normatively negative (e.g., stress) or positive (e.g., life satisfaction).

For quality control purposes, attentiveness was measured using the six-item infrequency subscale of the Attentive Response Scale (Maniaci & Rogge, 2014), which uses three items that are almost universally endorsed (e.g., "I don't like getting speeding tickets.") and three items that are almost universally not endorsed (e.g., "I enjoy the music of Marlene Sandersfield.")[7] attached the following five anchors: *not at all true*, *a little true*, *somewhat true*, *mostly true*, and *very true*. Responses to items were coded from 0 to 4, the three frequently endorsed items were reverse keyed, and scores from the six were summed. Generally, students who had complete data on these

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[7] Of note, "I enjoy the music of Marlene Sandersfield" is quite confusing to people who are paying attention, as she does not exist, at least not as a musician any renown. I received several comments on it in the feedback form at the end of the survey.

items with scores exceeding 7.5 were excluded from analyses, but practical use varied by wave(s) of data used and analysis conducted. Half of the items (mixture of frequently and infrequently endorsed) were presented randomly in the first half of the survey and the other half of the items were presented at the end of the survey. The position of these two halves of the scale in the survey, first half or end, was alternated every other wave.

Otherwise, each wave's survey consisted of six blocks of measurement items representing related superordinate (e.g., psychological wellness) and subordinate constructs (e.g., self-esteem). Within blocks, sets of items were randomly presented at the construct level and presentation of individual items were randomized within constructs, except for items that logically follow or depend on responses to other items. For example, if a student indicated that they were employed, they were subsequently asked a set of followup questions about their employment. Blocks 1-4 were randomly ordered; Blocks 5 and 6 followed Blocks 1-4 in that order, because Blocks 5 and 6 could possibly prime students to respond in particular ways to items in preceding blocks, contaminating measurement of more primary constructs. Superordinate and subordinate constructs arranged by Blocks are as follows:

Block 1: Psychological wellness. Life satisfaction (outcome variable) was measured with the five-item Life Satisfaction Scale (Diener, Emmons, Larsen, & Griffen, 1985) using the agree/disagree anchors. Items were coded 1-7 and averaged to form a composite. Internal consistencies (αs) among scale items and correlations between their composites across survey waves can be found in Table 1.

Global self-esteem (outcome variable) was measured with the Single-Item Self-Esteem Scale (SISE, Robins, Hendin, & Trzesniewski, 2001), consisting of one item: "I have high self-esteem." The original SISE scale used end-point anchors of "not very true of me" and "true of me." Given that I prefer not to use numbers or meaningless radio buttons to signify scale points and that Donnellan, Trzesniewski, and Robins (2014) argue that changes to wording, number of

scale points, or anchor choices, do not have an appreciable effect on the functionality of the SISE,

I used the agree/disagree anchors coded 1-7.

Table 1
Internal Consistencies of Scale Items and Between-Wave Correlations of Composites for Primary Constructs

Construct	Wave	α	*SD*	*n* Items	*n* Students	Between-Wave *r*s		
Life Satisfaction	0	.82	.09	5	333	wave 0	wave 1	wave 2
	1	.87	.08	5	306	.67		
	2	.85	.11	5	271	.57	.63	
	3	.87	.10	5	229	.56	.56	.65
Sadness	0	.90	.09	5	332	wave 0	wave 1	wave 2
	1	.92	.09	5	307	.55		
	2	.92	.09	5	270	.44	.51	
	3	.92	.07	5	228	.45	.42	.52
Stress	0	.74	.11	4	333	wave 0	wave 1	wave 2
	1	.76	.10	4	307	.47		
	2	.73	.09	4	271	.50	.48	
	3	.71	.09	4	228	.43	.39	.52
General Health	0	.68	.15	4	335	wave 0	wave 1	wave 2
	1	.72	.11	4	305	.64		
	2	.76	.12	4	272	.49	.62	
	3	.75	.12	4	227	.56	.66	.71
Academic Enjoyment	0	.83	.09	4	334	wave 0	wave 1	wave 2
	1	.87	.09	4	305	.33		
	2	.88	.08	4	272	.23	.34	
	3	.91	.05	4	227	.30	.47	.33
Social-Belonging	0	.82	.10	3	335	wave 0	wave 1	wave 2
	1	.78	.10	3	307	.55		
	2	.81	.10	3	273	.50	.69	
	3	.81	.12	3	227	.48	.65	.76

Feelings of sadness (outcome variable) during the past month were measured with five

emotion descriptors ("sad," "blue," "downhearted," "alone," and "lonely") from the Positive and

Negative Affect Schedule – Expanded Form (PANAS-X; Watson & Clark, 1999) anchored by

five subjective frequencies (*very slightly or not at all*, *a little*, *moderately*, *quite a bit*, and

extremely). Items were coded 1-5 and averaged to form a composite.

The extent to which students felt stressed/experienced adversity (outcome/mechanism variable) during the past month was measured by the four-item short form of the Perceived Stress Scale (S. Cohen, Kamarck, & Mermelstein, 1983) anchored by subjective frequencies (*never*, *almost never*, *sometimes*, *fairly often*, and *very often*). Responses were coded 1-5 and averaged to form a composite and two of the items were reverse-scored before averaging.

Block 2: Physical wellness. Students' subjective perceptions of their general health (outcome variable) were measured with four items comprising the general health portion of RAND's Medical Outcomes Survey Short-Form (Ware & Sherbourne, 1992), using true/false anchors (*definitely false*, *mostly false*, *don't know*, *mostly true*, and *definitely true*). Items were coded 1-5 and averaged to form a composite and two of the items were reverse-scored before averaging.

To roughly estimate frequency of illness (outcome variable), students were asked to report how many times, with a numeric response, they went to the doctor during the past month (Walton & G. L. Cohen, 2011) for mental health and physical health reasons (separately) and how many times they got sick (e.g., caught a cold or flu) but did not go to the doctor. The two physical health frequencies were summed and then transformed using the natural logarithm plus a constant (ln + 1) to improve the shape of the distribution.

To measure body mass index (BMI; outcome variable), students were asked to report, with numeric responses, their current height in feet and inches and weight in pounds. BMI was computed using the following formula: $((pounds*703) / ((feet*12) + inches)^2)$. The data for this measurement were somewhat messy; fifteen cases required interpretation. A description of this treatment of the data can be found in Appendix C.

To estimate exercise habits (control/auxiliary variable), students were asked to report, using a dropdown menu with response options of 0 to 7 days, about how many days during the past week they engaged in moderately-intense aerobic exercise (e.g., walking briskly) for at least 30 minutes, vigorously-intense aerobic exercise (e.g., running) for at least 20 minutes, and 8-10

strength training exercises (e.g., weightlifting) for at least 8-12 repetitions (American College Health Association, 2014). These items were used individually, as their internal consistency was insufficient to create a composite.

Block 3: Success (engagement) and other followup questions. Four items (agree/disagree scale) measured how much students enjoy academic pursuits (outcome variable) such as talking about interesting ideas (Asher & Weeks, 2012). Items in wave 0 referred to high school and items in waves 1-3 referred to the student's current term at the university. Responses were coded 1-7 and averaged to form a composite.

Similar to an item used by Walton and G. L. Cohen (2011), students were asked to estimate to the nearest half-hour, with a dropdown list of options truncated at 20+ hours, about how much time in an average week they spend studying outside of class (control/auxiliary variable). The item in wave 0 referred to high school and the item in waves 1-3 was phrased in the present tense.

Measurement of extracurricular activity (outcome variable) was based on Stephens and colleagues' (2014) assessment of extracurricular engagement, but was adapted to the local context and expanded to include a larger range of activities. Students indicated whether they have or have not (0 = *no*, 1 = *yes*) done each activity listed (e.g., joined a student group) at some point during high school for wave 0 and during the current term for waves 1-3. Three items/activities presented in waves 1-3 were irrelevant to and thus not presented in wave 0: "joined a fraternity or sorority," "participated in events specific to your residence hall," "participated in other events on campus." Responses to items were summed to form a composite.

Measurement of the tendency to seek support services and resources (outcome/mechanism variable) is based on Stephens and colleagues' (2014) assessment of resource seeking, which they used as a mediator of the effect of intervention on GPA. Like extracurricular activity, it has been adapted to the context and expanded to sample a broader range of resources that includes the original three items from Stephens et al. (2014; Stem: "In the

past month, how many times have you…" "…emailed a professor to ask a question?" "…met with a professor outside of class?" "…gone to the Writing Lab at the Teaching and Learning Center?") with the third item adapted to the local context. Students were asked to report, with a numeric response, how many times they had sought each resource during the past month. These items appear only in measurement waves 1-3, as they are specific to university resources. Responses were summed to form two composites: one representing the original three-item measurement and one representing the expanded 20-item measurement. Sums were then transformed (ln+1) to improve the shapes of the distributions. Other engagement-related questions and followup questions were asked in this block of the survey that were not subjected to analysis in the present research. They are presented in Appendix B, however.

 Block 4: Psychological mechanisms. Although all of the measurements in this section are potential psychological mechanisms, the present research will focus only on sense of social-belonging; all others are described here because they are all used as control/auxiliary variables in analyses (items are in Appendix B and internal consistencies and between-wave correlations are in Appendix D). Sense of social belonging at the university (mechanism/outcome variable) was measured by three items (agree/disagree anchors) from Walton and G. L. Cohen's (2007, 2011) Social Fit Inventory, which have been used with success in predicting achievement and persistence outcomes (Lewis et al., submitted). Items were adapted in two ways. First, two of the three were framed to elicit judgments of *uncertainty* about fitting in socially, to be as similar as possible to measurements of academic belonging (see below). Second, items in wave 0 are prospective and refer generally to "college" (Walton, n.d.), whereas items in waves 1-3 are judgments of current uncertainty about social-belonging and refer specifically to "the UO." Responses were coded 1-7. The two uncertainty-framed items were reverse-scored before averaging them with the third to form a composite, for which higher values represent greater sense of belonging.

Sense of academic-belonging was measured by four items (agree/disagree anchors) from Lewis and Hodges' (2015) Academic Uncertainty Scale, which have been used with success by Lewis and colleagues in studying persistence (K. L. Lewis, personal communication, January 21, 2015). Items were adapted to be prospective judgments of academic uncertainty and refer generally to "college" in wave 0 and judgments of current academic uncertainty and refer to specifically to "the UO" in waves 1-3. Responses were coded 1-7. The three uncertainty-framed items were reverse-scored before averaging them with the fourth to form a composite, for which higher values represent greater sense of belonging.

Individual differences in the need to belong were measured only in wave 0 by the Need to Belong Scale (Leary et al., 2013), which consists of ten items (agree/disagree anchors).

Feelings of loneliness was measured with the Three-Item Loneliness Scale (Hughes, Waite, Hawkley, & Cacioppo, 2004). Because it was developed for use in telephone surveys, it has a simplified 3-point scale (*hardly ever*, *some of the time*, and *often*). Additionally, wording from item to item varies slightly to be conversational and includes the scale anchors (e.g., "How often do you feel isolated from others? Is it hardly ever, some of the time, or often?"). The "how often" parts were retained. To increase variability and because the present survey was administered online, the three anchors were replaced by the four anchors (*never*, *rarely*, *sometimes*, and *often*) from the Revised UCLA Loneliness Scale (R-UCLA: Russell, Peplau, & Cutrona, 1980). The R-UCLA and Three-Item Loneliness Scales are highly correlated ($r = .82$; Hughes et al., 2004). Responses were coded 1-4 and averaged to form a composite.

Three dimensions of perceived availability of social support were measured using the appraisal, belonging, and tangible subscales of the Interpersonal Support Evaluation List (ISEL) short-form (S. Cohen, Mermelstein, Kamarck, & Hoberman, 1985). The appraisal subscale measures perceived availability of support in dealing with personal or familial issues or worries (e.g., advice about handling family problems). The belonging subscale measures perceived availability of social support in doing fun activities with others (e.g., going on a day trip to the

mountains). The tangible subscale measures perceived availability of social support in getting help from others doing tasks (e.g., finding help in moving to a new apartment). The tangible subscale was altered, however. The short-form was based on the general-population long-form ISEL, but two of the items are somewhat irrelevant to college students: "If I were sick, I could easily find someone to help me with my daily chores" and "If I had to go out of town for a few weeks, it would be difficult to find someone who would look after my house or apartment (the plants, pets, garden, etc.)." Hence, two items were taken from the tangible subscale of the college-student long-form ISEL to replace the other two: "I know someone at school or in town who would bring my meals to my room or apartment if I were sick" and "I don't know anyone who would give me some old furniture if I moved into my own apartment." All items used four true/false anchors: *definitely false*, *probably false*, *probably true*, and *definitely true*. Responses were coded 1-4. Two items in each four-item subscale were reverse-scored before averaging them together to form composites.

Feelings of self-assurance during the past month were measured with six emotion descriptors ("proud," "strong," "confident," "bold," "daring," and "fearless") from the Positive Affective and Negative Affective Schedule-X (PANAS-X; Watson & Clark, 1999) anchored by five subjective frequencies (*very slightly or not at all*, *a little*, *moderately*, *quite a bit*, and *extremely*). Responses were coded 1-5 and averaged to form a composite.

Self-efficacy was measured with three items (agree/disagree anchors) used by Lewis et al. (submitted), which refer to events of varying temporal distance and assess students' confidence in meeting the goals implied by the events (e.g., earning a degree). Responses were coded 1-7 and averaged to form a composite.

Perceived threat to social identities (i.e., stereotype threat) was measured with four items (agree/disagree anchors) adapted from Walton and G. L. Cohen (2011), which referred specifically to racial identity. The items were adapted to refer to students' background/identity in

a general way (e.g., "At the UO, I worry that people will draw conclusions about people like me based on my performances."). Responses were coded 1-7 and averaged to form a composite.

   Block 5: Religiosity and socioeconomic status. Religiosity (control/auxiliary variable) was measured with two items from the Centrality of Religiosity Scale (Huber & Huber, 2012), asking about participation in religious services and prayer/meditation, using seven frequency anchors: *never*, *less than once a month*, *once a month*, *several times a month*, *weekly*, *several times a week*, and *daily*. The economy of this measurement is based on reasoning that these two items, the second of which is actually a combination of the basic and interreligious versions (i.e., prayer and meditation; Huber & Huber, 2012), (a) are centrally important to measurement of religiosity and (b) this sort of religious *devotion* measure appears to be one of the most consistent and substantial religiosity predictors of psychological (Hackney & Sanders, 2003) and physical (Powell, Shahabi, & Thoresen, 2003) wellness. Unfortunately, in wave 0 and half of wave 1, the anchors of these items were misrepresented. "Several times a month" appeared twice, once where it was supposed to appear and once where "several times a week" was supposed to appear. When a student informed me of this mistake in the feedback form at the end of the survey, I corrected it. The distributions are right-skewed, for participation in religious services more than for prayer/meditation. So, the misrepresented response option would not have been used by very many students. Moreover, correlations of the individual items with each other and with psychological and physical wellness composites are consistent across waves. So, I inferred that students interpreted the duplicated anchor approximately as it should have been and averaged responses to create a composite.

   Estimated annual family income (control/auxiliary variable) was measured in wave 0 only with the following item: "Although you do not need to be exact, what is your best guess as to your family's income in an average year?" Response options were ordinal dollar amount ranges truncated at "more than $200,000." Subjective social status (control/auxiliary variable), or where students see their families in society (wave 0 only) and themselves in their communities

(wave 0: high school, waves 1-3: the UO), was measured by a version of the MacArthur Subjective Social Status Scale developed for adolescents (Goodman, Alder, Kawachi, Frazier, Huan, & Colditz, 2001), which conceptualizes a ladder as a society/community and asks respondents to place themselves/their family on the ladder with a response from 1 to 10, which correspond to the bottom and top rungs, respectively.

Students were asked to specify, in wave 0 only, the highest level of education attained by their mothers and fathers, separately, using the following response options: *did not finish high school*; *high school diploma, GED*; *some college experience, but did not finish*; *2-year technical /Associate's degree*; *4-year college/university degree*; *graduate degree (Masters, Doctorate, Law)*; and *don't know/not applicable*. As mentioned in the Student Records section above, these measurements were primarily used to classify students in terms of generation status. If students' responses for both their mothers and fathers were less than "4-year college/university degree," they were classified as first-generation. If one or both responses was "4-year college/university degree" or "graduate degree (Masters, Doctorate, Law)," they were classified as continuing-generation. If one response was "don't know/not applicable," but the other was a level of education, they were nonetheless classified as just described. If both responses were "don't know/not applicable," generation status was treated as missing. Missing values due to classification and non-response were filled in as much as possible with student records.

Block 6: Affective experience and feedback. Similar to the method used in daily diary portions of Walton and G. L. Cohen (2011) and Walton et al. (2015) to measure experienced adversity, students were asked to list four positive recent events, rating the positivity (anchors: *neutral*, *slightly positive*, *moderately positive*, and *very positive*) and importance (anchors: *not at all important*, *slightly important*, *moderately important*, and *very important*) of each, and to list four negative recent events, rating the negativity (anchors: *neutral*, *slightly negative*, *moderately negative*, and *very negative*) and importance of each. Wave 0 measurement refers to the summer and Waves 1-3 refer to the current term and the UO. The sum total importance of negative events

listed represents a measurement of adversity that will be used in exploring mechanisms outside of the scope of the dissertation. After the affective experience portion, an "essay" text box was provided for students to comment on the study if they so desired.

Intervention Procedure and Additional Measurements

Stephens and colleagues' difference-education intervention was directly replicated, following their method as closely as possible given situational constraints, and preregistered on Open Science Framework.[8] The bridge program in which the intervention was conducted admits state residents who graduated from high school with at least a 3.4 GPA (4.0 scale) no more than two years prior to enrolling at the university as first-time freshmen and who are eligible for a Pell grant. If resources are available, which was the case for the admission year studied in the present research, students whose high school GPA is below 3.4 are considered. The program ensures four years of covering students' tuition and fees and provides academic advising and access to support services (e.g., tutoring), special orientation sessions, peer-mentoring opportunities, and workshops. Historically, there have tended to be more first-generation students ($M_{2008-2013} = 45\%$) and students of color ($M_{2008-2013} = 36\%$) in the bridge program compared to those not in the bridge program who also submitted an application for federal student aid (first-generation: $M_{2008-2013} = 19\%$, students of color: $M_{2008-2013} = 21\%$).

Eight panelists (five first-generation, two male, three Asian, two Hispanic/Latino, three White) were selected from a pool of 55 upper-year bridge-program students who applied at the end of the 2014-15 academic year. Panelists were paid $9.25 per hour through the bridge program and I bought them dinner/lunch during workshop days.

At the beginning of "Week of Welcome" orientation activities, one week prior to the start of fall classes, I emailed incoming bridge-program students ($n = 750$) to invite them to attend one of sixteen panel-discussion workshops held in the evenings during the first week of classes (Tuesday, Wednesday, and Thursday) and in the daytime on Sunday at the end of the week.

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[8] https://osf.io/n4t75/?view_only=28d11f3bbf9449e491eb536d948e2206

Students indicated any and all times for which workshops were scheduled that they could attend, were asked to block those times off on their schedules, and were notified at the beginning of the first week of classes which workshop time they had been assigned. Students who indicated availability for only one time were assigned that time. Students who indicated availability for multiple times were assigned times so that group size was roughly balanced, with most workshop sessions having 13 attendees.

Of course, group size was not at all balanced in actual practice. According to the schedule, attendance was supposed to be highest at the first session on Tuesday evening, with 30 attendees (there was a preponderance of students with single-time availability for that time), but ended up being the first session on Wednesday evening, with 23 attendees. Attendance was lowest on Sunday; two sessions were canceled when only 1 and 3 students showed up (both standard-control, which resulted in greater participation in the difference-education condition than in standard-control). Otherwise, total attendance on Sunday was 25 across four sessions. Attendance also suffered on the panelist side of workshops on Sunday, as one of the Latina panelists did not show up. Overall, group size ranged from 5 to 23, averaging 11 across the fourteen valid sessions.

Two treatment conditions were implemented in the panel-discussion sessions: difference-education and standard-control. In addition, because invitations to participate in the survey and workshop portions of the research were separate, and because bridge-program and nonbridge-program students were also sampled separately, two passive control groups were available for comparison: one in the bridge program and one not.[9] Thus, there are four groups: intervention

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[9] In my preregistration of the replication I noted that a passive control condition was unlikely and that I would more likely sample from my nonbridge-program data for passive control comparison. I thought this because I had not yet separated survey and workshop recruitment as such. I separated them because the pre-matriculation survey was dangerously close to not being launched in time for participation to occur before the intervention needed to happen. Making the point entirely moot, however, both survey and workshop invitations ended up going out at the same time. The separation probably promoted the generation of a passive control within the bridge program. The result is that I have both program and nonprogram passive-control data. And because there are also sample size issues, I used all of the data in one combined set.

treatment (difference-education condition), control treatment (standard-control condition), passive control within the bridge program (program-passive-control condition), and passive control not in the bridge program (nonprogram-passive-control condition). Of note, the continuing-generation students in the nonprogram-passive-control condition represent more structurally advantaged students. The levels of achievement and persistence among these students are the target goals of bridge programs and social-psychological interventions like those presently under study insofar as boosting achievement and persistence of structurally disadvantaged students.

Half of the panel-discussion sessions were designated as difference-education treatment and the other half as standard-control treatment. The order of treatments was pseudo-randomized across all sessions so that there was never a day on which only one type of treatment was administered. Two moderators conducted the discussions. Like panelists, moderators were also upper-year students, but were not in the bridge program. Because of scheduling difficulties, the two moderators of the first evening's panel-discussions (both male, one Asian, one Hispanic/Latino) were different than the two of the subsequent sessions (both white female, one of whom was an undergraduate psychology research assistant). The research assistant received course credit and other moderators were paid $15 per hour; all were bought dinner. One of the moderators facilitated the workshops following the script in Appendix E and the other posed the questions in Table 2 to panelists during discussions. After a welcome introduction to the workshop by the facilitating moderator, panelists introduced themselves by saying their name, year, major, and where they were from. Panelists responded to questions in different fixed, random orders for each question; orders were the same across conditions. Panel discussions were video-recorded in order to provide a record of the manifestation of conditions should post-hoc coding of discussion content, panelist attributes, etc. become necessary or useful; however, technical difficulties did incur some information loss diminishing their utility.

34

Table 2

Question Prompts for Panelists in the Panel-Discussion Workshops by Condition

Condition	Question Prompts
Difference-Education	1. People come to college for many different reasons. What did coming to college mean to you?
	2. Students can have a wide variety of experiences when they transition to college and come from many different backgrounds. Thinking back, what was the transition to the UO like for you?
	3. Now we'd like you to share some specific challenges about coming to college. Can you provide an example of an obstacle that you faced when you came to the UO and how you resolved it?
	4. Did your decision to attend the UO affect your relationships with your friends and family at home? If yes, how?
	5. What advice do you have for students with backgrounds similar to your own?
	6. What experiences that you had prior to starting at the UO prepared you to excel in ways that you wouldn't have anticipated at the time?
Standard-Control	1. Trace your path for finding your major.
	2. What were some of the experiences that led you to your major and what were some challenges?
	3. What has been your favorite class and least favorite so far and why?
	4. What do you do to be successful in your classes? For example, how do you plan your courses and what are some strategies for being successful in those courses?
	5. How do you study for midterms and final exams? What are some challenges that you encountered?
	6. What are some options that you are considering as a future career path? How did you come to recognize those options? What are the advantages and/or disadvantages of the different paths you are considering?

Note: Prompts adopted from Stephens et al. (2014) materials with minor edits. Difference-education question 5 was originally "What would you advise other students to do with backgrounds similar to your own?" In difference-education question 6, "starting at" was inserted before the university name.

The content of stories shared in panel-discussions was themed by condition and controlled by working with panelists to prepare responses to questions during late summer, four of which were part of their application to be panelists and were main selection criteria (see Appendix F for responses). Themes differed in that responses to questions in the difference-education condition tended to be about background-relevant experiences transitioning to college and responses in the standard-control condition tended to be general advice (e.g., how to study for tests). Without me prompting them, the first-generation students on the panel spontaneously wrote responses to difference-education questions that were about how that particular identity has shaped their experiences. Continuing-generation students, not having such experiences, tended to respond differently. For example, one of the difference-education questions was "What advice do you have for students with backgrounds similar to your own?" One of the first-generation students responded: "As a first generation college student, my word of advice is to not be afraid to ask questions or for help! When I came here I was so afraid of asking any questions to anybody at all. I was so afraid of being judged; however, thanks to my RA, I lost that fear! She helped me out with resources when I had issues, I would ask professors or friends for help on certain things or I would simply go to my advisor for some help." One of the continuing-generation students responded: "I advise students to make friends with people who live in their dorm, because it is so easy to meet people and find friends that way. I would suggest going to many of the events the school puts on throughout the year because they are a lot of fun and are good for meeting new people. I would also recommend going in to financial aid frequently so you have a good grasp on how financial aid works, what loans to take out, and what you may be eligible for. Understanding my financial aid fully has been extremely beneficial for me during my college career." One issue with the manifestation of these responses was that, in line with what I mentioned in the introduction regarding the bridge-program population, panelists' backgrounds were similar in that they were all low-income, which is illustrated in the response above. So, although first-generation students did tend to express experiences that were qualitatively different than those expressed by

36

continuing-generation students, there was a common social-class thread regardless of the attempt to contrast first-generation and continuing-generation backgrounds.

After the panel-discussion portion of the sessions, attendees were asked to (a) generate a video "testimonial," using handheld Vivitar camcorders, articulating and explaining the main ideas communicated by the panel to leverage the saying-is-believing effect (Aronson et al., 2002; Higgins & Rholes, 1978; Stephens et al., 2014; Walton & G. L. Cohen, 2011; see Table 3 for testimonial preparation prompts), and (b) complete a brief, one-page set of paper-and-pencil measures, slightly revised from Stephens and colleagues' materials, assessing perceptions of the program content, the panelists, and the testimonial exercise (see Appendix B for full materials). Six items (agree/disagree anchors) assessed how useful, interesting, enjoyable, etc. the panel-discussion was to attendees ($\alpha = .94$, $SD = .08$). Responses were coded 1-7 and averaged to form a composite. Two items assessed the extent to which attendees liked the panelists (agree/disagree anchors; $\alpha = .83$). Responses were coded 1-7 and averaged to form a composite. Two final items assessed how enjoyable and effortful the video testimonial activity was to attendees, using the following five anchors: *none/not at all*, *a little bit*, *a moderate amount*, *quite a bit*, and *a great deal* ($\alpha = .63$). Responses were coded 1-5 and averaged to form a composite.

Table 3
Testimonial Preparation Prompts

Instructions	Please create a short 1 to 2 minute video that communicates the main lessons or insights that you gained from the student panel today. **When you begin recording your video, please say the number that appears in the upper right corner of this sheet.**
	The purpose is to create materials to distribute to future UO students to help them learn what college will be like. Hearing your voice and your ideas is an essential part of the Project.
	To brainstorm for your video testimonial, please jot down notes to the questions below. Outline format is fine. Keep in mind that there are no right or wrong answers. We want to know about your perspective and experience.

Table 3 (continued).

Prompt Questions	What are the top three lessons you learned from the student panel today?
	If you were going to advise future incoming students based on what you learned today, what would you say?
	What did you find most personally relevant and meaningful?
	Was this information helpful? How will you use it as you begin your UO experience?

Note: Instructions and questions were directly adopted from Stephens et al. (2014) materials with added content in bolded text.

Propensity-Score Matching of Residential Groups

Controlling for confounders of differences between residential groups on outcomes of interest has been a necessity from inception, because students "choose" their living arrangements (i.e., they are not randomly assigned). Of course, the "choice" to live off-campus or on campus or in a living-learning community has inputs, which poses a well-known inferential problem (Rosenbaum, 1984; Rubin, 1991). The problem can be expressed as one of "endogeneity" (Duncan, Magnuson, & Ludwig, 2004). Endogeneity of a grouping variable means that some other variable or variables systematically influence group membership; the grouping is an endogenous, as opposed to an exogenous, variable in a conceptual/statistical model. This is a serious inferential problem when variables that influence group membership also influence the outcome on which the groups are being compared (i.e., when they are confounded). Successful random assignment to conditions eliminates endogeneity, which is why experimental work uses that method, and is why it considered the "gold standard" when trying to draw causal inference. Even when random assignment is possible it is often quite difficult to achieve in practice (see preexisting differences between *randomly assigned* conditions in the Results section). Unfortunately, random assignment cannot even be attempted for some conditions (e.g., residential groups). Under such circumstances, there are methods available and for attempting to correct the

problem by breaking, or at least reducing, the link between exposure to the quasi-experimental grouping and variables that determine that exposure in order to infer causation.

A close-to-ideal method for addressing the endogeneity problem is direct propensity-score matching (Oakes, 2004; Oakes & Johnson, 2006; Rosenbaum & Rubin, 1983; for modern practical use see, e.g., Coman, et al., 2013; Do & Kincaid, 2006). The original intention of the present research was to attempt direct propensity-score matching, which is a very intuitive idea. It is the idea on which matched-samples designs are based, whereby individuals are matched one-for-one or within some very small distance on one or two characteristics to tightly control for those characteristics when comparing two existing (i.e., not randomly assigned) groups on some outcome. The only conceptual difference with direct propensity-score matching is that, instead of matching raw scores on one or two characteristics, probabilities of group membership are matched within a very small distance. This is because probability of group membership is conditioned on a set of variables too large to match participants on each variable. Unfortunately, direct propensity-score matching is nearly as difficult as achieving a well-balanced matched-samples design and it is practically impossible when samples are small and covariate vectors are large. It was thus practically impossible in the present research.

Fortunately, the practice is preprocessing and there is no harm in exploration and trying different methods at that stage so long as the comparison of the groups on the outcome of interest is not consulted in the decision process. The decision process involved scouring the data for variables (pre-enrollment characteristics from student records and baseline survey measurements) that were related to exposure to residential groups and to the fifteen outcomes of interest separately. All of the variables described above, including those in the Psychological Mechanisms section that are outside the scope of this treatment of the data *per se*, were considered for use as auxiliary variables. Relationships were estimated based on complete-case analyses and classified as related to exposure, related to the outcome, related to both exposure and the outcome, or related to neither exposure nor the outcome on the basis of effect sizes (rs > .1, ds > .2, ORs >

1.4). Variables that were related to both exposure and the outcome (i.e., confounders) and variables that were related to the outcome (i.e., outcome controls) were selected for building propensity-score models (see Appendix G). Intuitively, variables that were related to neither exposure nor the outcome were not selected because they have no bearing on the analysis models. Variables that were related to exposure but not the outcome were not selected, because they tend to decrease precision (i.e., increase variance) while not decreasing bias, whereas variables related to the outcome but not exposure also tend have no effect on bias, but do increase precision (Brookhart, Schneeweiss, Rothman, Glynn, Avorn, & Stürmer, 2006). Variables considered and classified as described above can be found for each outcome in Appendix G.

The *MatchIt* package in *R* (open-source statistical software), was used to build the propensity-score models and match the residential groups. The propensity-score model is a logistic regression of two groups on the selected covariates. Three propensity-score models were built for each of the fifteen outcomes, one for each group comparison (students living off-campus vs. students living in conventional residence halls, students living off-campus vs. students living in residential freshman interest groups or academic residential communities [FIGARCs], and students living in conventional residence halls vs. students living in FIGARCs). Direct matching (called exact matching in *MatchIt*) was attempted for the first couple of models; it quickly became clear that it was not a viable method, given the data, resulting in no matched units. Subclassification, optimal, and nearest neighbor (with and without replacement) methods were also explored. The method that worked best for maximizing matched units and minimizing the distances between propensity scores was nearest neighbor matching with replacement. "Nearest neighbor" means that treatment units are matched to control units with the most similar propensity scores (Ho, Imai, King, & Stuart, 2007); "with replacement" means a single control unit can be matched to multiple treatment units.

Also, switching the definition of treatment and control groups helped because of their unequal sizes and how the matching process works, which is by taking treatment units (coded as

1) and selecting control units (coded as 0) to match them. Again, when selection is done with replacement, that means control units can be selected multiple times to be matched with different individual treatment units. Hence, if the groups are unequally sized, it is better to have the smaller of the two be the treatment group and the larger be the control. It is debatable which of the residential groups is "treated" and which is "control." Student Affairs professionals and practitioners would like to think that living on-campus or living in an academic residential community as a treatment, because their programming is intended to have a positive influence on students. However, it is entirely plausible that the action of any putative effect is actually in the experience of living off-campus, and this is an issue that will not be resolved by the present research. In order to maximize my use of the data, I specified students living in conventional residence halls as the control group when comparing them to the other two groups, because they comprised the largest group ($n = 167$ before deletion due to missing data), and I specified students living in FIGARCs ($n = 91$ before deletion due to missing data) as the control group when comparing them to students living off-campus ($n = 75$ before deletion due to missing data).

However, group sizes varied by propensity-score model because *MatchIt* requires complete data, and using multiply imputed data would require conducting the matching in all of the data sets generated and combining the results, which is an intractable though not completely impossible analytic strategy. In addition, matching results in discarded cases and data were excluded from students who scored ≥ 7.5 on the infrequency scale (i.e., the attention check) in waves 0, 1, or 3, but this information loss is not a bad thing *per se*, because the point of both is to refine the information so that it is actually more informative. The bottom line is that, feeding into the matching process, I had to delete cases with missing data on student records and wave 0 survey and cases of attention check failure in waves 0, 1, and 3, and this varied by propensity-score model because different groups and different covariates comprised different models.

Information on the twelve out of fifteen outcomes measured by the final wave 3 survey (i.e., everything but achievement and persistence to the second year and in summer quarter) was

also lost due to attrition from survey participation. This meant two things. First, I did not include those wave-3 survey-measured outcomes in the data sets that fed the relevant propensity-score models. Doing so would be usual procedure, because after the propensity-score model (balancing groups on propensity scores) comes the analysis model (comparing groups on the outcome). I did not do so, because missingness in wave 3 was the greatest, which would have resulted in a great many deleted cases and consequently severely limited the data set. I also did not conduct a typical post-processing analysis like the average treatment effect or the average treatment effect on the treated (Ho et al., 2007; Oakes & Johnson, 2006). Instead, I used a sample weighting strategy. I saved the data sets produced by *MatchIt*, which included matched cases (i.e., discards are discarded) with the data that fed into the process, plus their propensity scores and a vector of weights. I then joined the grouping and weights variables to the variables that fed multiple imputation models, which are described in the Results section, and conducted weighted and unweighted regressions after imputation. See Table 4 for group sizes before and after matching.

Table 4
Sizes of Residential Groups before and after Propensity-Score Matching

		Comparison 1		Comparison 2		Comparison 3	
		CRH^C	FIGARC^T	CRH^C	OC^T	FIGARC^C	OC^T
Life Satisfaction	initial	127	71	127	61	71	61
	matched	40	71	25	61	23	61
Self-Esteem	initial	128	73	128	61	73	61
	matched	40	73	30	61	26	61
Sadness	initial	138	78	138	64	78	64
	matched	45	78	38	64	30	64
Stress	initial	137	74	137	62	74	62
	matched	44	74	30	62	24	62
General Health	initial	132	73	132	61	73	61
	matched	46	73	31	61	29	61
Illness	initial	140	78	140	64	78	64
	matched	48	78	40	64	36	64
Body Mass Index	initial	128	71	128	59	71	59
	matched	38	71	32	59	28	59
Academic Enjoyment	initial	140	78	140	64	78	64
	matched	52	78	40	64	29	64

Table 4 (continued).

		Comparison 1		Comparison 2		Comparison 3	
		CRH^C	FIGARC^T	CRH^C	OC^T	FIGARC^C	OC^T
Extracurricular Activity	initial	132	74	132	63	74	63
	matched	48	74	37	63	29	63
Resource Seeking (Original)	initial	140	77	140	64	77	64
	matched	51	77	44	64	32	64
Resource Seeking (Expanded)	initial	130	73	130	61	73	61
	matched	51	73	39	61	30	61
Achievement	initial	120	68	120	56	68	56
	matched	42	68	31	56	26	56
Persistence (2nd Year)	initial	123	69	123	56	69	56
	matched	43	69	38	56	31	56
Persistence (Summer)	initial	123	68	123	56	68	56
	matched	43	68	39	56	32	56
Social-Belonging	initial	148	84	148	66	84	66
	matched	54	84	39	66	28	66

Note: CRH = students living conventional residence halls, FIGARC = students living in residential freshman interest groups or academic residential communities, OC = students living off-campus. Superscripts ^{C} and ^{T} indicate conditions specified as control and treatment, respectively, in propensity-score matching.

CHAPTER III

RESULTS

Checking the Intervention Manipulation

Stephens et al. (2014) checked their manipulation by coding students' video testimonials on two themes: (a) "People's different backgrounds matter," and (b) "people with backgrounds 'like mine' can succeed." Sample responses for (a) were "People from different backgrounds have different expectations of college" and "College means very different things to different people." Sample responses for (b) were "People have come from a background like mine" and "I feel like I'm in the right place because students from backgrounds like mine understand the stresses I have" (p. 946). However, looking at their coding scheme, provided to me by Stephens' lab manager, the latter was a bit different. It was labeled "My Experience is Normal/I'm Not Alone," defined as "any statement conveys the feeling or recognition that the student is not alone," with the following examples: "everyone is uncomfortable or struggles at first," "there are other people like me," "other people have the same challenges as me," "the student panel is like me," and "other students of similar backgrounds." I refer to this category, which looks like things one might code for a belongingness intervention, as "normal." The scheme for (a) above, which I refer to as "background," was labeled as "People's backgrounds matter," as in the published work, defined as "Any statement conveys an understanding that there is not just one right way to do things; there are multiple ways of being a person or student; if person acknowledges others' points of view OR if person holds different points of view OR if understand that backgrounds are influencing people," with the following examples: "there are different ways to be a student," and "there are multiple ways to be."

Two undergraduate research assistants (RAs), blind to hypotheses and condition, and I, blind to condition, used the coding scheme provided to code whether each coding category, *normal* and *background*, was present or absent in students' video responses ($n = 138$; 2 videos were lost due to camera malfunction and 18 workshop attendees did not make a video). Being

three coders, we deviated from Stephens and colleagues' use of two. The first round of coding

revealed some agreement challenges. Agreement on *normal* was adequate (percent agreement

between pairs of coders: 78%, 81%, and 82%). Moreover, disagreement was roughly evenly

distributed among coders. I was in the minority 7% of the time, RA1 was in the minority 12%,

and RA2 was in the minority 11% of the time. However, although agreement was adequate on

background 80%, 80%, and 91%, disagreement among coders was more unevenly distributed

than disagreement on *normal*. RA1 and I were in the minority 5% and 4% of the time,

respectively, while RA2 was in the minority 15% of the time. The three of us met to discuss the

potential disparity and learned that RA2 was coding a bit more liberally than RA1 and I were,

taking any mention of "background" as "present," which RA1 and I thought insufficiently fit the

definition. RA2 agreed with our perspective and recoded *background*. Agreement with RA2 and I

improved from 80% to 88% and 85% with RA1 and me, respectively. I was in the minority 6% of

the time, RA1 was in the minority 3% of the time, and RA2 was in the minority 9% of the time.

Still disagreement was rampant. As a group, we would have had to discuss 66 codes (41 for

normal and 25 for *background*) to come to consensus. If just the two RAs were official coders,

they would have had to discuss 59 codes (32 for *normal* and 27 for *background*). Because the

situation presented us with a great deal of prospective effort with promising little return, I used a

tie-breaking tactic to resolve disagreement rather than consensus. When the RAs' codes agreed,

that coding was used; when they did not, my codes were used (22% of cases for *normal* and 12%

of cases for *background*).

The difference-education condition did not differ from the standard-control condition on

either category: one-tailed with Yates' continuity correction for *normal*, $\chi^2(1) = .94$, $p = .166$; for

background, $\chi^2(1) = .03$, $p = 435$. *Normal* was present in 49% of video testimonials in the

difference-education condition and 39% of them in the standard-control condition. *Background*

was present in 11% of the video testimonials in the difference-education condition and 12% of

them in the standard-control condition.

Assessing Panel-Discussion Workshop Perceptions

Following Stephens et al. (2014), composite variables representing panel usefulness, panelist liking, and video activity enjoyment/effort were regressed on condition (0 = standard-control, 1 = difference-education). Thirty-three students did not complete the questionnaire. There were no condition differences on panel usefulness, $t(123) = -.28$, $p = .780$, or panelist liking, $t(123) = .23$, $p = .819$, but students in the difference-education condition ($M = 3.34$, $SD = .93$) enjoyed and/or put more effort into their video testimonials than did students in the standard-control condition ($M = 2.98$, $SD = .81$), $t(116) = 2.22$, $p = .028$.[10]

Missing Data

Most survey missingness was due to participating in wave 0 (i.e., the baseline survey) and not in subsequent waves (127 of 511 students who at least partially participated in wave 0: 24.8%); this was construed as non-participation rather than attrition, given that it was the baseline survey. An additional 38 bridge-program students (of 158: 22%) participated in the workshops but not in wave 0. Contact information was eventually obtained for 33 of these students to invite them to participate in wave 3; 7 responded (5 of them completely). Three-hundred eighty-four students (379 who responded for more measurement waves than just wave 0 plus the 5 invited later who responded completely to wave 3) were considered respondents. Among them, 205 (53.4%) responded for waves 0, 1, 2, and 3; 68 (17.7%) responded for waves 0, 1, and 2 then dropped out; 43 (11.2%) responded for wave 1 then dropped out; 1 (0.3%) consented to participate in wave 0, did not actually participate, was included in the panel, and ended up participating in waves 1, 2, and 3; 13 (3.4%) responded for waves 0, 2, and 3; 16 (4.2%) responded for waves 0, 1, and 3; 14 (3.6%) responded for waves 0 and 2; 19 (4.9%) responded for waves 0 and 3; and the previously mentioned 5 (1.3%) responded for wave 3. Partial responses (i.e., stopping participation mid-wave) among these students were minimal (wave 0: 3.6%, wave

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[10] Degrees of freedom differ because seven students (five in the difference-education condition and two in the standard-control condition) who had otherwise complete data did not complete these two items at the end of the one-page post-intervention questionnaire.

1: 1.8%, wave 2: 1.6%, wave 3: 4.2%), as was item non-response (wave 0: 2.0%, wave 1: 1.1%, wave 2: 1.2%, wave 3: 3.1%).

Using the *BaylorEdPsych* package in *R*, which accepts a maximum of 50 variables as input, Little's MCAR test (Little, 1988) was conducted. After exclusions were made (including the attention check in all four waves), tests were conducted on composite variables of the survey-measured outcomes from each of the four waves of measurement, as well as subjective social status (self), which was always presented toward the end of the survey. Tests were conducted for each wave individually (11 variables for wave 0, 12 variables for waves 1, 2, and 3 – only one version of resource seeking was used because they have the exact same missing patterns being computed from the same data), and on all waves combined (47 variables). There was no evidence that data were not missing completely at random within waves 0, 1, and 3 (wave 0: $\chi^2(70) =$ 65.69, $p =$.624, 12 missing patterns; wave 1: $\chi^2(53) = 46.61$, $p = .720$, 9 missing patterns; wave 3: $\chi^2(52) = 37.27$, $p = .939$, 9 missing patterns). However, data from wave 2 may have not been missing completely at random, $\chi^2(27) = 44.16$, $p = .020$, 6 missing patterns. The missing at random assumption is at least somewhat plausible, however, given that there were baseline predictors of wave 2 missingness.[11] Bridge-program and nonbridge-program students differed somewhat insofar as what significantly predicted missingness. For bridge-program students, having a student loan, wave 0 need to belong, and subjective social status (family) predicted missingness in wave 2. For nonbridge-program students, high school GPA, SAT/ACT scores, having received a Pell grant, having received a Scholarship, and wave 0 life satisfaction predicted missingness in wave 2. Across all waves, data were not missing completely at random, $\chi^2(1,232)$ = 172,718.4, $p < .001$, 47 missing patterns, predominantly suggesting that attrition was not

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[11] Briefly, there are three types of missingness: missing completely at random (MCAR), missing at random (MAR), and missing not at random (MNAR). MAR is when missingness depends on observed but not unobserved data, MCAR is a special case of MAR, where missingness does not depend on observed data either, and MNAR is when missingness depends on unobserved data. MCAR is testable. MAR is not testable. It is an assumption that one hopes is plausible. For detailed yet accessible explanations, see Graham (2009) and Schafer and Graham (2002).

completely random. Group differences in attrition were investigated and are described in the next section.

To restore power lost due to attrition, to reduce potential bias produced by range restriction, and to improve the plausibility of the missing at random assumption, a multiple imputation model was built for each of the 12 survey-measured outcomes, including both the original and expanded versions of resource seeking. For achievement and persistence (to second year and to summer), the analysis models were based on complete cases, because (a) data were mostly complete unless students did not consent to releasing the records that went into the models and (b) when GPA was missing, that meant that the student dropped out before they could earn any GPA hours, which is not something that should be imputed. Prior to imputation and analysis, data from students who scored ≥ 7.5 on the infrequency scale (i.e., the attention check) in waves 0, 1, or 3 were excluded. This resulted in a total sample size of $N = 344$ for intervention/control comparisons. For residential group comparisons, initial sample sizes were $n = 250$ for conventional residence hall vs. FIGARC, $n = 232$ for conventional residence hall vs. off-campus, and $n = 160$ for FIGARC vs. off-campus.

Imputation models typically include predictors of missingness and variables with sizable relationships with the target variable (Dong & Peng, 2013; Graham, 2009; Schafer & Graham, 2002). Although predictors of missingness were examined, very few strong predictors emerged. Furthermore, variables that are highly related to the target variable are effective at reducing bias of even models where data are missing not at random regardless of their relationship to missingness (Enders, 2011; Graham, 2009) and especially when those variables are the same measurements from previous waves in a longitudinal study (Graham, 2009). In addition, including predictors of only missingness may not add any benefit over and above auxiliary variables related to the target variable (Spratt et al., 2010). Auxiliary variables should not have a great deal of missingness relative to the target (van Buuren, Boshuizen, & Knook, 1999). And there is a trade-off between imputation model size and sample size (Asendorpf, van de Schoot,

Denissen, & Hutteman, 2014). For these reasons, imputation models included measurements of the same variable from waves 0 and 1 as well as other variables from waves 0 and 1 that had the largest complete-case relationships with a given outcome in wave 3, but not necessarily predictors of missingness, although in some cases there was overlap between these sorts of variables (e.g., life satisfaction). To maximize utility, all of the variables described in the Method, including those in the Psychological Mechanisms section that are outside the scope of this treatment of the data *per se*, were considered for use as auxiliary variables. Also included in the imputation models were variables to be used in analysis models. For intervention/control comparisons predictors were generation status and the three condition dummies using difference-education as the reference, and covariates included were gender (binary: male vs. female), race/ethnicity (binary: advantaged vs. disadvantaged), high school GPA, and SAT/ACT score. For residential group comparisons the predictor was a single binary grouping (e.g., students living in conventional residence halls vs. students living off-campus) and the covariate was the vector of weights produced by propensity-score matching.

Not all outcomes were fortunate enough to have a rich set of predictors for their imputation models (see Appendix H for variables included in each). For example, very little was related to the physical wellness constructs (general health, illness, and BMI) or resource seeking. Illness was not even very related to itself at different measurement occasions. In contrast, BMI benefited greatly from a strong relationship with itself. On the other end of that spectrum, for example, social-belonging was related to a great many other variables in the data, especially itself. Because of that, it and self-esteem had the richest imputation models of any outcome, but generally, the psychological outcomes had richer models than did other outcomes. Hence, some imputation models are better than others and, therefore, some inferences are stronger than others.

One-hundred data sets (50 iterations) were imputed for each model using predictive mean matching with the *mice* package in *R*, which uses Markov chain Monte Carlo simulation to sample values iteratively across conditional densities for each variable with missing data. Plots of

49

missingness proportions and patterns were generated using the *VIM* package in *R* (Appendix I).

The interaction terms between condition and generation status were not directly included in the imputation models, but were specified in the process so that *mice* would treat their dependencies appropriately (i.e., passive imputation; see van Buuren & Groothuis-Oudshoorn, 2011).

Preexisting Group Differences

Before addressing analyses of outcomes, exploratory data analysis revealed some differences that should be noted between students in intervention and control conditions on attrition, consent to release records, records themselves, and baseline survey measurements; between first-generation and continuing-generation students on consent to release records, records, and baseline survey measurements; and between residential groups on attrition, records, and baseline survey measurements. Analyses of survey measurements excluded data from students who scored ≥ 7.5 on the infrequency scale (i.e., the attention check) in wave 0.

Intervention/control differences. Three sets of three dummy variables were constructed to compare four groups: difference-education (intervention treatment within the bridge program), standard-control (control treatment within the bridge program), program-passive-control (untreated control within the bridge program), and nonprogram-passive-control (untreated control not in the bridge program). The three sets of dummy variables used difference-education, standard-control, and program-passive-control as reference groups, respectively.

Multinomial logistic regressions of students with complete data (reference group), students who dropped out, and students with intermittent data on sets of condition dummies showed that students in the nonprogram-passive-control condition were marginally more likely than were students in the standard-control condition to drop out of the study than to have complete data ($p = .052$). All other coefficients were non-significant, ps ≥ .106. Binary logistic regressions of consent to release a) educational, b) residential, c) background, and d) financial need records ($0 = no$, $1 = yes$) on the sets of dummies, showed that students in the difference-education condition were less likely than were students in the nonprogram-passive-control

condition to consent to releasing educational records (*p* = .049) and financial need records (*p* = .008), and that students in the program-passive-control condition were less likely than were students in the nonprogram-passive-control condition to consent to releasing financial need records (*p* = .046). All other coefficients were non-significant (*p*s ≥ .115).

In terms of records themselves, binary logistic regressions of generation status (0 = *continuing-generation*, 1 = *first-generation*), gender (0 = *female*, 1 = *male*), race/ethnicity (0 = *disadvantaged*, 1 = *advantaged*), AP credit (0 = *did not transfer any AP credits*, 1 = *transferred AP credits*), student loan receipt (0 = *did not receive*, 1 = *received*), work-study receipt (0 = *did not receive*, 1 = *received*), non-residential freshman interest group participation (0 = *not participating*, 1 = *participating*); multinomial logistic regressions of residential group (*off-campus* [reference], *conventional residence hall*, *freshman interest group/academic residential community*), and linear regressions of high school GPA and SAT/ACT scores on sets of condition dummies were conducted. Although data were available, regressions were not conducted for residential or non-residential honors college participation – there were less than five students in the entire data set who were participating in non-residential honors college and virtually zero students in the bridge-program conditions were participating in residential honors college, compared to about 2% of students in the nonprogram-passive-control condition. Neither were regressions conducted for Pell grant or scholarship receipt; virtually all bridge-program students received a Pell grant and a scholarship, which is almost definitionally part of participation in this program, whereas many fewer nonprogram students received a Pell grant (10%) or scholarship (63%).

Most of the differences were between bridge-program conditions and the nonprogram condition (see Appendix J). Compared to each of the three bridge-program conditions, there were fewer first-generation students, more students transferring AP credit, and fewer students who received loans and work-study in the nonprogram-passive-control condition. Additionally, students in the nonprogram-passive-control condition had higher SAT/ACT scores, on average,

51

than did students in all three bridge-program conditions. These differences were not unexpected, given that most students in this condition are more structurally advantaged than are students in the bridge-program conditions. The only significant difference among bridge-program conditions on these variables was on work-study. More students in the difference-education condition received work-study than did in the program-passive-control condition, and although the difference-education condition did not differ significantly from the standard-control condition, the portion of students in standard-control who received work-study was nearly the same as that of students in the program-passive-control.

Two other marginally significant differences among bridge-program conditions were observed. There were more students of an advantaged racial/ethnic group (i.e., White or Asian) in the standard-control condition than there were in the difference-education condition, which made the standard-control condition statistically indistinguishable from the nonprogram-passive-control condition in this respect, where there were significantly more students of an advantaged racial/ethnic group in the nonprogram-passive-control condition than there were in either the difference-education condition or the program-passive-control condition. In addition, there were more students living in conventional residence halls (compared to off-campus) in the difference-education condition than there were in the program-passive-control condition. The larger portion of students in the difference-education condition living in conventional residence halls made them statistically indistinguishable from students in the nonprogram-passive-control condition in terms of that comparison to living off-campus. However, there were more students living in conventional residence halls and in residential freshman interest groups or academic residential communities (compared to off-campus) in the nonprogram-passive-control condition than there were in either of the bridge-program control conditions.

Two other differences were observed: First, there were marginally more students participating in non-residential freshman interest groups in the nonprogram-passive-control condition than there were in the standard-control and program-passive-control conditions, but the

nonprogram-passive-control and difference-education conditions did not differ. Second, students in the nonprogram-passive-control condition had significantly higher high school GPAs, on average, than did students in the difference-education condition; had marginally higher high school GPAs, on average, than did students in the program-passive-control condition; and did not differ significantly from students in the standard-control condition.

Linear regressions of baseline measurements of outcomes of interest and auxiliary/control variables on sets of condition dummies showed mostly nonsignificant or marginally significant differences between conditions (Appendix J). Although there were also several significant differences, none of them were among bridge-program conditions. Of note, in comparison to bridge-program conditions, nonbridge-program students had consistently higher life satisfaction, family income, and subjective social status (family), further supporting the idea that they are more structurally advantaged. The marginally significant differences among bridge-program conditions were that students in the difference-education condition had higher scores on general health than did students in the standard-control condition, and had higher self-esteem and lesser sadness than did students in the program-passive-control condition. Additionally, students in the standard-control condition reported studying for more hours outside of class in high school than did students in the program-passive-control condition, and students in the program-passive-control condition had a greater need to belong than did students in the standard-control condition.

Generation status differences. A multinomial logistic regression of students with complete data (reference group), students who dropped out of the study, and students with intermittent data on generation status ($0 = $ *continuing-generation*, $1 = $ *first-generation*) yielded no evidence that first-generation students were more or less likely to have complete data than to dropout of the study ($p = .183$) or have intermittent data ($p = .385$). Binary logistic regressions of consent to release a) educational, b) residential, c) background, and d) financial need records on generation status showed that first-generation students were more likely to consent to releasing financial need records than were continuing generation students ($p = .008$); other ps $\geq .139$.

53

The same regressions conducted of records and baseline survey measurements on conditions above were conducted using generation status as the predictor. In addition, regressions were conducted for Pell grant and scholarship receipt (0 = d*id not receive*, and 1 = *received*) and residential honors college participation (0 = *not participating*, 1 = *participating*) and a binary logistic regression was conducted of bridge-program participation (0 = *not participating*, 1 = *participating*) on generation status.

First-generation students differed significantly from continuing-generation students on nearly all of the records variables (Appendix J); the exceptions were gender, participation in a non-residential academic community, and residential group, though there were marginally more first-generation than continuing generation students living in residential freshman interest groups or academic residential communities (compared to off-campus). Otherwise, there were significantly more first-generation than continuing-generation students who were participating in the bridge program, had received a Pell grant, had received a student loan, had received work-study, and had received a scholarship. There were significantly fewer first-generation than continuing-generation students who were of an advantaged racial/ethnic group, had transferred AP credit, and were participating in the residential honors college. First-generation students also had significantly lower high school GPAs and SAT/ACT scores, on average, than did continuing-generation students.

Most of the differences on baseline survey measurements were non-significant, but there were a few differences of note (Appendix J). On average, first-generation students were less satisfied with life, got less enjoyment out of academic pursuits in high school, had a lower sense of anticipated academic belonging at the university, had lower annual family income, perceived their families as having lower social status, and had marginally lower self-esteem compared to continuing-generation students.

Residential group differences. Two sets of two dummy variables were constructed to compare three groups to each other: students who were living off-campus, students who were

living in conventional residence halls, and students who were living in a residential freshman interest group or an academic residential community (FIGARC). The two sets of dummy variables used students living off-campus and those living in conventional residence halls as reference groups, respectively.

Multinomial logistic regressions of students with complete data (reference group), students who dropped out, and students with intermittent data on sets of condition dummies showed that students living in conventional residence halls were marginally more likely than were students living off-campus ($p = .050$) and students living in FIGARCs ($p = .081$) to have intermittent data than to have complete data. All other coefficients were non-significant, ps $\geq$ .101. Binary logistic regressions of consent to release a) educational, b) residential, c) background, and d) financial need records on residential group dummies yielded no evidence that any group was more or less likely to consent to releasing any of the types of records, ps $\geq$ .262.

Mostly the same regressions conducted of records and baseline survey measurements on generation status above were conducted using residential group dummies as the predictors. However, regressions were not conducted for residential honors college participation; the residential honors college is considered an ARC and so would be confounded with FIGARC. In addition, a multinomial logistic regression was conducted of intervention condition (*nonprogram-passive-control* [reference group], *program-passive-control*, *standard-control*, and *difference-education*) on residential group dummies, reflecting the reverse regression from the intervention/control differences section.

There were four significant differences on records between students living in conventional residence halls and students living in FIGARCs (Appendix J). There were more first-generation students living in conventional residence halls than there were living in FIGARCs and, unsurprisingly, there were more students living in conventional residence halls who were also participating in a non-residential freshman interest group compared to students living in FIGARCs. Additionally, students living in FIGARCs had higher high school GPAs and

55

SAT/ACT scores, on average, than did students living in conventional residence halls; they differed to the same extent on these variables from students living off-campus who did not differ from students living in conventional residence halls.

Students living off-campus differed in many other respects from both students living in conventional residence halls and students living in FIGARCs. The overlapping differences were all conceptually interconnected: there were more students living off-campus who were participating the bridge program, who had received Pell grants and, reflecting the reverse regression in the intervention/control differences section, who were in the standard-control and program-passive-control conditions compared to the nonprogram-passive-control condition. There were also significantly fewer students living off-campus who had transferred AP credit, though the difference between them and students living in conventional residence halls was marginally significant. Two other differences between students living off-campus and those living in conventional residence halls emerged: there were fewer male students and more students who had received scholarships living off-campus. Finally, one other marginally significant difference emerged between students living off-campus and those living in FIGARCs: there were more first-generation students living off-campus.

The analyses of baseline survey data showed a similar general pattern (Appendix J). There was one marginally significant difference between students living in conventional residence halls and students living in FIGARCs: the latter had a greater sense of anticipated academic belonging at the university. Some of the differences between students living off-campus and their counterparts living in conventional residence halls or in FIGARCs were marginally significant. That fact notwithstanding, students living off-campus were less satisfied with life, had lower self-esteem, were sadder, more stressed, and were generally less healthy. They had lesser senses of anticipated social and academic belonging, had less social support (belonging), were lonelier, had lower annual family income, and perceived their families to be of lower social status. Compared to just students living in FIGARCs, they reported studying for fewer hours

56

outside of class in high school. And compared to just students living in conventional residence halls, they had less social support (on the appraisal subscale), were less self-assured, and felt greater stereotype threat.

Strategy Moving Forward

Regarding residential group comparisons, these pre-existing differences are precisely what are addressed by propensity-score matching. Regarding intervention/control comparisons, models could easily be overloaded with covariates to control for differences between conditions, mostly between bridge-program conditions and the nonbridge-program condition. Entering the same covariates into models as did Stephens et al. (2014; i.e., gender, race, high school GPA, and SAT/ACT score) is a necessity for replication purposes. Entering baseline measurements of pertinent outcomes is prudent. However, given all of the variables on which bridge-program students differ from nonbridge-program students and how many of those variables are also related to outcomes (i.e., are potential confounders), I would have to enter as many as 20 additional parameters as covariates. This is an unwelcome loss of several degrees of freedom for an already size-challenged sample. Additionally, propensity-score matching is infeasible because of the loss of information on the front end and the small size of treatment conditions. Given that most of the condition differences are between bridge-program and nonbridge-program, and half of the differences between bridge-program conditions are on the outcome constructs themselves, I conducted two regressions per outcome (one with Stephens and colleagues' set of covariates and one with those covariates *and* baseline measurement of the pertinent outcome, with the exception of resource seeking, achievement, and persistence analyses because there is no baseline measurement) and will tolerate the messiness of the comparisons to the passive-control conditions. Importantly, the only difference between the difference-education condition and the standard-control condition is that the standard-control condition had more students of a disadvantaged racial/ethnic group, which is already a covariate.

57

The next three sections describe the results from tests of end-of-year outcomes for intervention and control conditions, tests of intervention mechanisms, and tests of end-of-year outcomes for residential groups.

End-of-Year Outcomes: Intervention/Control Differences

After multiple imputation, the outcomes measured in the wave 3 survey (life satisfaction, self-esteem, sadness, stress, general health, illness, BMI, academic enjoyment, extracurricular activity, resource seeking [original and expanded versions], and social-belonging) were regressed on three condition dummy variables using the difference-education condition as the reference, generation status (0 = continuing-generation, 1 = first-generation), the condition by generation interaction (three dummy variables), and control covariates (gender, advantaged/disadvantaged race/ethnicity, high school GPA, SAT/ACT score) for all 100 data sets. In a separate set of regressions, baseline measurement of the outcome was added as a control covariate. Regressions across data sets were pooled to yield overall tests of coefficients. For achievement (GPA) and persistence outcomes (enrollment in fall of 2nd year and in summer), the same analysis models applied, but on complete-case data and persistence was analyzed with logistic, not linear, regressions. An additional set of regressions were conducted for persistence outcomes that controlled for cumulative first-year GPA.

Because hypotheses regarding comparisons of the difference-education and standard-control conditions and the interaction with generation status on all outcomes except social-belonging were directional and were specified and preregistered prior to conducting the study, those hypothesis tests use one-tailed p-values. Specifically, for a given regression, the test of the coefficient representing the comparison of the standard-control condition to the difference-education condition and the test of the generation status coefficient, which represents the simple main effect of generation in the difference-education condition because of how dummy codes were constructed used one-tailed p-values. All others are two-tailed.

Psychological wellness outcomes. Coefficients are presented in Table 5. There were no significant differences between the difference-education and standard-control conditions on any of the psychological wellness outcomes (all *p*s with and without baseline control ≥ .217). There were no significant simple main effects of generation in the difference-education condition (all *p*s with and without baseline control ≥ .114). There were also no significant differences between the difference-education and nonprogram-passive-control conditions (all *p*s with and without baseline control ≥ .242) and no significant interactions with generation status (*p*s > .71). There were differences between the difference-education and program-passive-control conditions, but no interactions with generation (all *p*s with and without baseline control ≥ .123). Compared to untreated bridge-program students, students in the difference-education condition had marginally higher self-esteem, *t*(252.58) = -1.91, *p* = .057, were significantly less sad, *t*(227.39) = 2.88, *p* = .004, and were significantly less stressed, *t*(226.31) = 2.43, *p* = .016, at the end of their first year at the university. However, the difference between these two groups on self-esteem, *t*(212.17) = -1.31, *p* = .191, was reduced to nonsignificance when baseline self-esteem was entered into the model, while the differences on sadness, *t*(211.74) = -2.86, *p* = .005, and stress, *t*(215.30) = 2.31, *p* = .022, remained significant when baseline sadness and stress were entered into the models.

Physical wellness outcomes. Coefficients are presented in Table 6. With one exception, none of the coefficients in models of physical wellness outcomes were significant (*p*s ≥ .107). The exception was that without controlling for baseline BMI, students in the difference-education condition did not differ from students in the standard-control condition on end-of-year BMI, (*p* = .185), but when controlling for baseline BMI, students in the difference-education condition had significantly lower BMI, on average, *t*(134.54) = 1.88, *p* = .031.

Table 5
Coefficients for Intervention/Control Comparisons on Psychological Wellness Constructs

	Life Satisfaction		Self-Esteem		Sadness		Stress	
		baseline		baseline		baseline		baseline
G	-0.21	-0.23	-0.29	0.08	-0.10	0.10	0.16	0.24
Txi	-0.17	-0.07	-0.30	0.07	0.07	0.06	0.17	0.19
Txii	-0.68	-0.35	-1.04†	-0.57	0.92‡	0.86‡	0.54*	0.49*
Txiii	0.12	-0.10	-0.06	0.14	0.04	0.17	0.13	0.18
TxGi	-0.09	0.01	0.48	0.17	0.15	0.02	-0.03	-0.13
TxGii	0.52	0.28	0.39	0.26	-0.12	-0.26	-0.40	-0.43
TxGiii	-0.10	0.04	0.22	-0.28	0.04	-0.23	-0.05	-0.19

Note: Coefficients are unstandardized. G = generations status (represents additional effect of being first-generation in difference-education condition), Txi = treatment dummy representing comparison of difference-education to standard-control, Txii = treatment dummy representing comparison of difference-education to program-passive-control, Txiii = treatment dummy representing comparison of difference-education to nonprogram-passive-control, TxGi = treatment by generation interaction term representing additional effect of being first-generation in the standard-control condition, TxGii = treatment by generation interaction term representing additional effect of being first-generation in the program-passive-control condition, TxGiii = treatment by generation interaction term representing additional effect of being first-generation in the nonprogram-passive-control condition. Tests of G and Txi are one-tailed. All others are two-tailed. †$p < .10$, *$p < .05$, ‡$p < .01$

Table 6
Coefficients for Intervention/Control Comparisons on Physical Wellness Constructs

	General Health		Illness		Body Mass Index	
		baseline		baseline		baseline
G	-0.07	0.14	0.05	0.04	-1.62	-0.30
Txi	-0.09	0.11	0.11	0.11	1.42	1.22†
Txii	-0.23	-0.04	0.07	0.06	-0.75	0.45
Txiii	-0.15	0.04	0.07	0.06	-1.48	0.46
TxGi	-0.09	-0.17	-0.05	-0.05	-1.90	-0.32
TxGii	0.07	-0.07	-0.01	0.00	0.74	0.32
TxGiii	-0.05	-0.17	-0.03	-0.03	1.28	-0.11

Note: Coefficients are unstandardized. G = generations status (represents additional effect of being first-generation in difference-education condition), Txi = treatment dummy representing comparison of difference-education to standard-control, Txii = treatment dummy representing comparison of difference-education to program-passive-control, Txiii = treatment dummy representing comparison of difference-education to nonprogram-passive-control, TxGi = treatment by generation interaction term representing additional effect of being first-generation in the standard-control condition, TxGii = treatment by generation interaction term representing additional effect of being first-generation in the program-passive-control condition, TxGiii = treatment by generation interaction term representing additional effect of being first-generation in the

Success outcomes. Coefficients are presented in Tables 7 and 8. Except for a marginal difference on achievement, $t(279) = 1.37$, $p = .086$, there were no significant differences between the difference-education and standard-control conditions on any of the success outcomes ($ps \geq$ .165). However, there was a significant simple main effect of generation in the difference-education condition, $t(279) = 2.18$, $p = .030$. First-generation students in the difference-education condition had higher cumulative first-year GPAs, on average, than did continuing-generation students in the difference-education condition. Computing and plotting the marginal means based on this equation revealed an interesting pattern, which was similar but not identical to the raw group means (Figure 1). There was also a marginally significant simple main effect of generation in the difference-education condition on persistence into the second year. First-generation students in the difference-education condition were marginally more likely to be enrolled for fall of their second year compared to their continuing-generation counterparts, $z(279) = 1.51$, $p =$ .066; this effect was reduced to non-significance when cumulative first-year GPA was entered into the model, $p = .258$. Plotting the predicted probabilities from both models, with and without controlling for cumulative first-year GPA, and the raw proportions showed consistent patterns (Figure 2), but both are a bit misleading. Looking at the raw numbers of students enrolled, relative to the number not enrolled by condition and generation status (Figure 3), there is not really anything of note happening within the bridge program. Students are persisting quite well across the board.

There were no significant differences between the difference-education and program-passive-control conditions ($ps > .246$) and no significant interactions with generation status ($ps >$ .243), but two differences did emerge between the difference-education and nonprogram-passive-control condition. Compared to nonbridge-program, bridge-program students in the difference-education condition enjoyed academic pursuits to a greater extent, $t(245.57) = -2.08$, $p = .039$,

and engaged in less extracurricular activity, $t(253.26) = 2.07$, $p = .039$. The difference between

them on extracurricular activity was reduced to nonsignificance when baseline extracurricular

activity was entered into the model, $t(242.25) = 1.51$, $p = .132$. However, the difference on

academic enjoyment remained significant when baseline academic enjoyment was entered into

the model, $t(239.56) = -2.10$, $p = .036$. Additionally, there was a marginally significant condition

by generation status interaction on persistence into the second year, $z(279) = -1.95$, $p = .052$. That

regression term represents the additional effect of being first-generation in the nonprogram-

passive-control condition, meaning that first-generation students in the nonprogram-passive-

control condition were marginally less likely to be enrolled for fall of their second year compared

to their continuing-generation counterparts; this effect was reduced to non-significance when

cumulative first-year GPA was in the model, $p = .223$.

Table 7
Coefficients for Intervention/Control Comparisons on Success Constructs (Engagement)

	Academic Enjoyment		Extracurricular Activity		Resource Seeking (Original)	Resource Seeking (Expanded)
		baseline		baseline		
G	-0.52	-0.42	0.63	0.47	0.07	-0.07
Txi	-0.37	-0.52	-0.34	-0.55	0.06	0.01
Txii	-0.53	-0.50	-0.05	-0.15	-0.07	0.01
Txiii	-0.68*	-0.67*	0.78*	0.56	-0.01	0.00
TxGi	0.08	0.14	0.23	0.43	-0.17	-0.06
TxGii	0.69	0.46	-0.27	-0.11	0.12	0.14
TxGiii	0.02	-0.04	-0.48	-0.32	-0.05	0.06

Note: Coefficients are unstandardized. G = generations status (represents additional effect of being first-generation in difference-education condition), Txi = treatment dummy representing comparison of difference-education to standard-control, Txii = treatment dummy representing comparison of difference-education to program-passive-control, Txiii = treatment dummy representing comparison of difference-education to nonprogram-passive-control, TxGi = treatment by generation interaction term representing additional effect of being first-generation in the standard-control condition, TxGii = treatment by generation interaction term representing additional effect of being first-generation in the program-passive-control condition, TxGiii = treatment by generation interaction term representing additional effect of being first-generation in the nonprogram-passive-control condition. Tests of G and Txi are one-tailed. All others are two-tailed. †$p < .10$, *$p < .05$, ‡$p < .01$

Figure 1. Marginal and raw mean cumulative first-year GPA by condition (DE = difference-education, SC = standard-control, PPC = program-passive-control, NPPC = nonprogram-passive-control) and generation status (FG = first-generation, CG = continuing-generation).
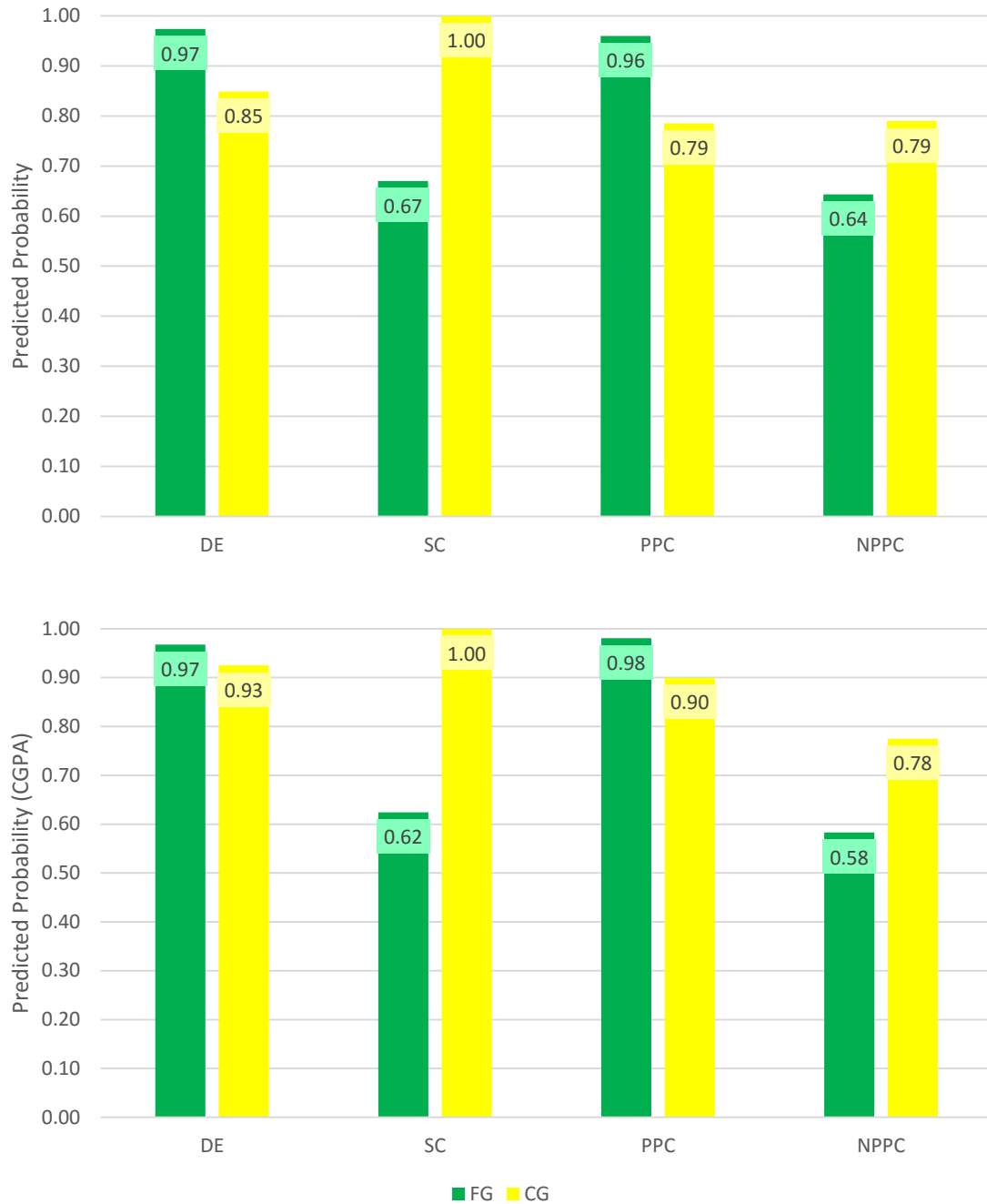
Figure 2. Predicted probabilities, with and without controlling for cumulative first-year GPA (CGPA) of enrollment in fall of second year by condition (DE = difference-education, SC = standard-control, PPC = program-passive-control, NPPC = nonprogram-passive-control) and generation status (FG = first-generation, CG = continuing-generation).

Table 8

Coefficients for Intervention/Control Comparisons on Success Constructs (Achievement and Persistence)

	Achievement		Persistence		
		2nd Year	2nd Year CGPA	Summer	Summer CGPA
G	0.38*	1.89†	0.89	-15.99	-16.15
Txi	0.27†	15.52	15.68	-0.34	-0.47
Txii	-0.15	-0.43	-0.32	0.19	0.26
Txiii	0.20	-0.40	-1.28	0.26	0.17
TxGi	-0.41	-18.42	-18.57	16.68	16.88
TxGii	0.04	-0.01	0.86	15.15	15.07
TxGiii	-0.33	-2.62†	-1.79	16.38	16.52

Note: Coefficients are unstandardized and those for Persistence are in log units. 2nd Year = enrolled for fall of 2nd year, Summer = enrolled for summer, CGPA = controlling for cumulative 1st-year GPA, G = generations status (represents additional effect of being first-generation in difference-education condition), Txi = treatment dummy representing comparison of difference-education to standard-control, Txii = treatment dummy representing comparison of difference-education to program-passive-control, Txiii = treatment dummy representing comparison of difference-education to nonprogram-passive-control, TxGi = treatment by generation interaction term representing additional effect of being first-generation in the standard-control condition, TxGii = treatment by generation interaction term representing additional effect of being first-generation in the program-passive-control condition, TxGiii = treatment by generation interaction term representing additional effect of being first-generation in the nonprogram-passive-control condition. Tests of G and Txi are one-tailed. All others are two-tailed. †$p < .10$, *$p < .05$, ‡$p < .01$

Figure 3. Raw numbers of students enrolled and not enrolled in fall of second year by condition (DE = difference-education, SC = standard-control, PPC = program-passive-control, NPPC = nonprogram-passive-control) and generation status (FG = first-generation, CG = continuing-generation).

Social-belonging. Coefficients are presented in Table 9. There were no significant

coefficients in the social-belonging models with or without baseline control (ps > .214, two-

tailed).

Table 9
Coefficients for Intervention/Control Comparisons on
Social-Belonging

	Social-Belonging	
		baseline
G	0.38	0.29
Txi	0.27	-0.70
Txii	-0.15	-0.66
Txiii	0.20	-0.13
TxGi	-0.41	0.27
TxGii	0.04	0.27
TxGiii	-0.33	0.02

Note: Coefficients are unstandardized and those for
Persistence are in log units. G = generations status
(represents additional effect of being first-generation in
difference-education condition), Txi = treatment
dummy representing comparison of difference-
education to standard-control, Txii = treatment dummy
representing comparison of difference-education to
program-passive-control, Txiii = treatment dummy
representing comparison of difference-education to
nonprogram-passive-control, TxGi = treatment by
generation interaction term representing additional
effect of being first-generation in the standard-control
condition, TxGii = treatment by generation interaction
term representing additional effect of being first-
generation in the program-passive-control condition,
TxGiii = treatment by generation interaction term
representing additional effect of being first-generation
in the nonprogram-passive-control condition. All tests
are two-tailed, because social-belonging was not
specified as an outcome prior to conducting the study.
†p < .10, *p < .05, ‡p < .01

Intervention Mechanisms

Several things went awry insofar as modeling mechanisms was concerned. First, my

analysis plan proposed group and time invariance testing of measurements. Group invariance

testing was out of the question, given the ultimate group sizes. And neither group nor time invariance testing made much sense to do if limited sample sizes called for limited model parameters, in which case the proposed fully-latent growth model, inclusive of the measurement models, should be reduced to parceled indicators (i.e., composite variables). That is, the measurement model parts of the structural models are rendered irrelevant.

Thus, I attempted estimating growth models using composite indicators (i.e., eliminating the measurement model parts). However, several problems emerged in even the simplest, most basic steps. A simple growth model of resource seeking on its own produced a negative residual variance. It did this for both the original (i.e., Stephens and colleagues' original three-item measurement) and expanded (i.e., my 20-item measurement) versions of resource seeking, as well as when using only the treated conditions (i.e., difference-education and standard-control) and using all bridge-program conditions (i.e., adding program-passive-control). On the positive side, the same simple growth models of stress and GPA did not produce problematic results and fit well too, when using treated conditions only and all bridge-program conditions. Putting both the social-belonging and stress growth models together in a parallel process growth model worked and fit adequately using all bridge-program conditions, as did conditioning growth on social-belonging, but trying to condition the relationship *between* growth on social belonging and growth in stress led to nowhere but error messages.

Other attempts at various models went well and poorly to varying degrees. These could be problems of empirical underidenification (Kenny, 1979). Substantively, is it plausible that, at least with resource seeking, growth *per se* is an unreasonable theoretical expectation. For example, perhaps when students need support, they seek it, and they get it, they do not necessarily need to keep getting it or get more of it. In any case, the problems were extensive enough that I consequently cannot currently test the ideas I set out to test in a growth modeling framework. Therefore, I chose instead to construct path models that represented straightforward tests and that represented the available information appropriately.

67

Replication: Resource seeking. In switching to path modeling for testing Stephens and colleagues' (2014) model, there are two important things to note. First, the growth hypothesis is different than a covariance hypothesis. Specifically, I hypothesized that first-generation students would have a steeper slope in resource seeking than would continuing generation students in the difference-education condition. This would argue for comparing first-generation and continuing-generation students within conditions. In a path model, however, coefficients of paths from exogenous grouping variables to resource seeking represent mean differences. Stephens and colleagues' model predicts *no difference* on resource seeking between first-generation and continuing-generation students in the difference-education condition; the difference should actually occur between the difference-education and standard-control conditions for first-generation students. The same applies to cumulative first-year GPA. However, I observed a pattern of differences on GPA that does not fit this description. Additionally, none of the regression model coefficients for spring resource seeking were significant, but resource seeking may be different in different academic terms. Given the situation, it made the most sense to retain the grouping structure of the regression models.

Second, I needed to do justice to the information contained in the resource seeking variables, which growth modeling would have done. I could not simply use wave 3 resource seeking, because measurement of it referred to only spring quarter. That is, although measurement occurred at the end of the year, Stephens et al. asked students to recall seeking resources across their entire first year whereas I asked students to report on their resource seeking for the term in which measurement occurred, in this case spring term. Entering all three waves of surveying in the present research together in the same model effectively represents the entire

year.[12] Hence, to represent the entire year, I built a path model in *Mplus* (Muthén & Muthén, 1998-2015), regressing cumulative first-year GPA on fall, winter, and spring resource seeking (waves 1, 2, and 3 composites computed using the original three resource seeking items, and then mean-centered) and on generation status (0 = continuing-generation [$n = 42$], 1 = first-generation [$n = 51$]), and regressing fall, winter, and spring resource seeking on generation status, treatment (0 = standard-control [$n = 36$], 1 = difference-education [$n = 57$]), and the treatment by generation interaction (see Figure 4 for path diagram). Additionally, I estimated specific indirect, total indirect, and total effects by condition using the model constraint command and bootstrapping bias-corrected confidence intervals (10,000 replications). Data from students who scored ≥ 7.5 on the infrequency scale (i.e., the attention check) in waves 1, 2, and 3 were treated as missing and missing data in this context is handled by full-information maximum likelihood estimation.
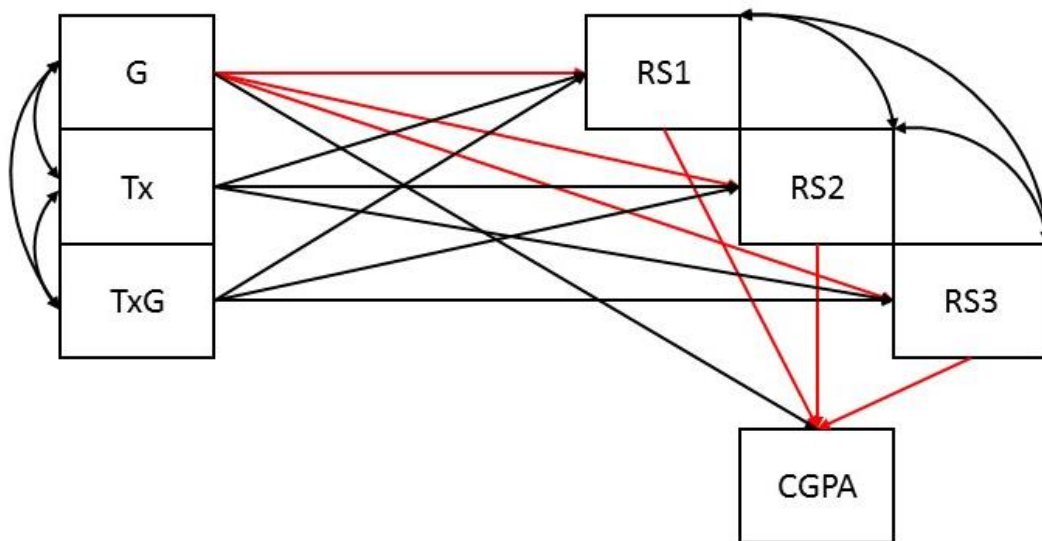
Figure 4. Path diagram of indirect effects of generation status (G) on cumulative first-year GPA (CGPA) through fall, winter, and spring resource seeking (RS1, RS2, and RS3, respectively) with paths from generation to resource seeking conditioned on treatment condition (Tx); TxG = treatment by generation interaction and red direct paths indicate indirect paths.

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[12] This raises a criticism worth noting of my treatment of resource seeking in regression models. I used only spring resource seeking as the outcome. I think this is reasonable given the consistent timing of measurement between wave 3 of the present research and Stephens and colleagues' end-of-year survey, but wave 3 does not represent the whole year. It may or may not be worthwhile to additionally investigate fall and winter resource seeking.

The model did not fit well, $\chi^2(5) = 28.46$, $p < .001$; RMSEA = .23 (90% CI: .15, .31); CFI = .15; SRMR = .15. Allowing the resource seeking variables to correlate made the model fit well, $\chi^2(2) = 1.40$, $p = .50$; RMSEA = .00 (90% CI: .00, .19); CFI = 1.00; SRMR = .02 (see Table 10 for coefficients and confidence intervals). However, none of the indirect paths had confidence intervals that did not include zero, not even 90% confidence intervals. The only significant direct paths were generation → fall quarter resource seeking (i.e., wave 1), $b = -0.80$ (95% CI: -1.36, -0.32) and treatment × generation → fall quarter resource seeking, $b = 0.74$ (95% CI: 0.02, 1.33). Fitting the same model using the full, expanded version of resource seeking measurement did not produce substantively different results.

Despite nonsignificant indirect paths, there were some glimmers that may benefit from a larger sample to reduce residual variance. To explore whether the small sample size was contributing to imprecision, I added the program-passive-control group ($n = 51$, among them $n = 27$ first-generation) and fit roughly the same model, including intercorrelations among waves of resource seeking measurements – the difference being that two dummy-coded terms represented condition (difference-education as the reference) and two dummies represented the condition by generation interaction. The model fit well, $\chi^2(4) = 1.60$, $p = .81$; RMSEA = .00 (90% CI: .00, .08); CFI = .1.00; SRMR = .01 (see Table 11 for coefficients and confidence intervals). The interaction term representing the additional effect of being first-generation in the standard-control condition on fall quarter resource seeking was significant, $b = -0.77$ (95% CI: -1.49, -0.05), reflecting the significant treatment × generation → fall quarter resource seeking path in the previous model. And this time, one of the specific indirect paths (generation → winter resource seeking → cumulative GPA) was marginally significant, $b = -0.19$ (90% CI: -0.57, -0.01; 95% CI: -0.64, 0.02). First-generation students in the standard-control condition sought fewer resources than did continuing-generation students during winter quarter and seeking fewer resources resulted in lower GPA. Interestingly, the coefficients for the specific indirect paths for the difference-education condition were all very small (-0.003, 0.01, and 0.002). Fitting the same model using

70

the expanded version of resource seeking measurement attenuated the coefficient of the specific

indirect path just described to total nonsignificance, but it was still the largest one of the specific

indirect paths, $b = -0.11$ (90% CI: -0.38, 0.02).

Table 10
Coefficients and Confidence Intervals of Two-Group Resource Seeking Model

Path	Lower 2.5%	Lower 5%	Coeff.	Upper 5%	Upper 2.5%
G → RS1	-1.36	-1.27	-0.80	-0.32	-0.24
G → RS2	-0.72	-0.64	-0.18	0.29	0.39
G → RS3	-0.41	-0.36	-0.08	0.26	0.33
G → CGPA	-0.11	-0.05	0.22	0.55	0.61
Tx → RS1	-0.79	-0.70	-0.27	0.18	0.26
Tx → RS2	-0.82	-0.72	-0.20	0.27	0.37
Tx → RS3	-0.23	-0.19	0.04	0.32	0.38
TxG → RS1	0.02	0.12	0.74	1.33	1.45
TxG → RS2	-0.56	-0.45	0.19	0.84	0.95
TxG → RS3	-0.41	-0.34	0.03	0.36	0.42
RS1 → CPGA	-0.10	-0.05	0.20	0.52	0.58
RS2 → CPGA	-0.24	-0.16	0.19	0.54	0.60
RS3 → CPGA	-1.50	-1.38	-0.48	0.49	0.70
G → RS1 → CGPA (DE)	-0.20	-0.16	-0.01	0.04	0.06
G → RS2 → CGPA (DE)	-0.13	-0.10	0.00	0.13	0.17
G → RS3 → CGPA (DE)	-0.08	-0.05	0.02	0.33	0.40
Total Indirect (DE)	-0.17	-0.13	0.01	0.24	0.31
Total (DE)	-0.12	-0.06	0.23	0.57	0.63
G → RS1 → CGPA (SC)	-0.58	-0.50	-0.16	0.01	0.05
G → RS2 → CGPA (SC)	-0.35	-0.29	-0.03	0.03	0.05
G → RS3 → CGPA (SC)	-0.13	-0.08	0.04	0.39	0.48
Total Indirect (SC)	-0.58	-0.50	-0.16	0.07	0.11
Total (SC)	-0.27	-0.21	0.06	0.34	0.39

Note: Coefficients are unstandardized. G = generation status (first-generation = 1, continuing-generation = 0), Tx = treatment condition (DE = difference-education [coded as 1], SC = standard-control [coded as 0]), TxG = treatment by generation interaction, RS1 = fall resource seeking, RS2 = winter resource seeking, RS3 = spring resource seeking, CGPA = cumulative first-year grade point average.

Table 11
Coefficients and Confidence Intervals of Three-Group Resource Seeking Model

Path	Lower 2.5%	Lower 5%	Coeff.	Upper 5%	Upper 2.5%
G → RS1	-0.48	-0.41	-0.05	0.32	0.39
G → RS2	-0.45	-0.37	0.05	0.50	0.58
G → RS3	-0.25	-0.22	-0.03	0.17	0.20
G → CGPA	-0.12	-0.07	0.16	0.40	0.45
Txi → RS1	-0.27	-0.19	0.27	0.70	0.79

Table 11 (continued).

Path	Lower 2.5%	Lower 5%	Coeff.	Upper 5%	Upper 2.5%
Txi → RS2	-0.32	-0.21	0.25	0.76	0.86
Txi → RS3	-0.36	-0.28	0.00	0.23	0.26
Txii → RS1	-0.26	-0.19	0.22	0.61	0.69
Txii → RS2	-0.88	-0.80	-0.38	0.07	0.16
Txii → RS3	-0.35	-0.31	-0.11	0.09	0.13
TxGi → RS1	-1.49	-1.38	-0.77	-0.17	-0.05
TxGi → RS2	-1.00	-0.90	-0.26	0.36	0.48
TxGi → RS3	-0.48	-0.42	-0.09	0.27	0.36
TxGii → RS1	-0.93	-0.80	-0.21	0.40	0.52
TxGii → RS2	-0.42	-0.29	0.32	0.89	1.02
TxGii → RS3	-0.11	-0.05	0.24	0.51	0.57
RS1 → CPGA	-0.15	-0.11	0.06	0.24	0.29
RS2 → CPGA	-0.08	-0.02	0.26	0.51	0.55
RS3 → CPGA	-0.90	-0.74	-0.05	0.61	0.74
G → RS1 → CGPA (DE)	-0.09	-0.07	0.00	0.03	0.04
G → RS2 → CGPA (DE)	-0.10	-0.08	0.01	0.19	0.23
G → RS3 → CGPA (DE)	-0.09	-0.06	0.00	0.11	0.14
Total Indirect (DE)	-0.15	-0.12	0.01	0.18	0.22
Total (DE)	-0.13	-0.08	0.17	0.43	0.49
G → RS1 → CGPA (SC)	-0.29	-0.23	-0.05	0.08	0.11
G → RS2 → CGPA (SC)	-0.64	-0.57	-0.19	-0.01	0.02
G → RS3 → CGPA (SC)	-0.64	-0.48	0.04	0.78	0.97
Total Indirect (SC)	-1.03	-0.84	-0.20	0.18	0.31
Total (SC)	-0.82	-0.66	-0.04	0.39	0.51
G → RS1 → CGPA (PPC)	-0.19	-0.15	-0.02	0.02	0.04
G → RS2 → CGPA PPC)	-0.38	-0.32	-0.04	0.08	0.12
G → RS3 → CGPA (PPC)	-0.27	-0.19	0.01	0.44	0.61
Total Indirect (PPC)	-0.61	-0.47	-0.05	0.16	0.23
Total (PPC)	-0.41	-0.29	0.11	0.42	0.50

Note: Coefficients are unstandardized. G = generation status (first-generation = 1, continuing-generation = 0), Txi = treatment dummy representing comparison of difference-education (DE) to standard-control (SC), Txii = treatment dummy representing comparison of difference-education to program-passive-control (PPC), TxGi = treatment by generation interaction term representing additional effect of being first-generation in the standard-control condition, TxGii = treatment by generation interaction term representing additional effect of being first-generation in the program-passive-control condition, RS1 = fall resource seeking, RS2 = winter resource seeking, RS3 = spring resource seeking, CGPA = cumulative first-year grade point average.

Extension: Belonging-adversity contingency. Using a similar analytic approach to test the idea that the intervention might disrupt the belonging-adversity contingency, I regressed fall, winter, and spring stress and social-belonging (waves 1, 2, and 3 composites, mean-centered) on generations status (0 = continuing-generation, 1 = first-generation); regressed fall, winter, and

spring stress and social-belonging on condition (0 = standard-control, 1 = difference-education) and the condition by fall quarter stress interaction; regressed social-belonging on stress within quarter (e.g., wave 1 social-belonging on wave 1 stress), and allowed fall, winter and spring stress to intercorrelate and fall, winter, and spring social-belonging to intercorrelate (see Figure 5 for path diagram). The condition by fall quarter stress interaction caused 19 cases to be deleted due to missingness on predictors ($N = 75$). I again estimated specific indirect, total indirect, and total effects by condition using the and bootstrapped bias-corrected confidence intervals (10,000 replications).
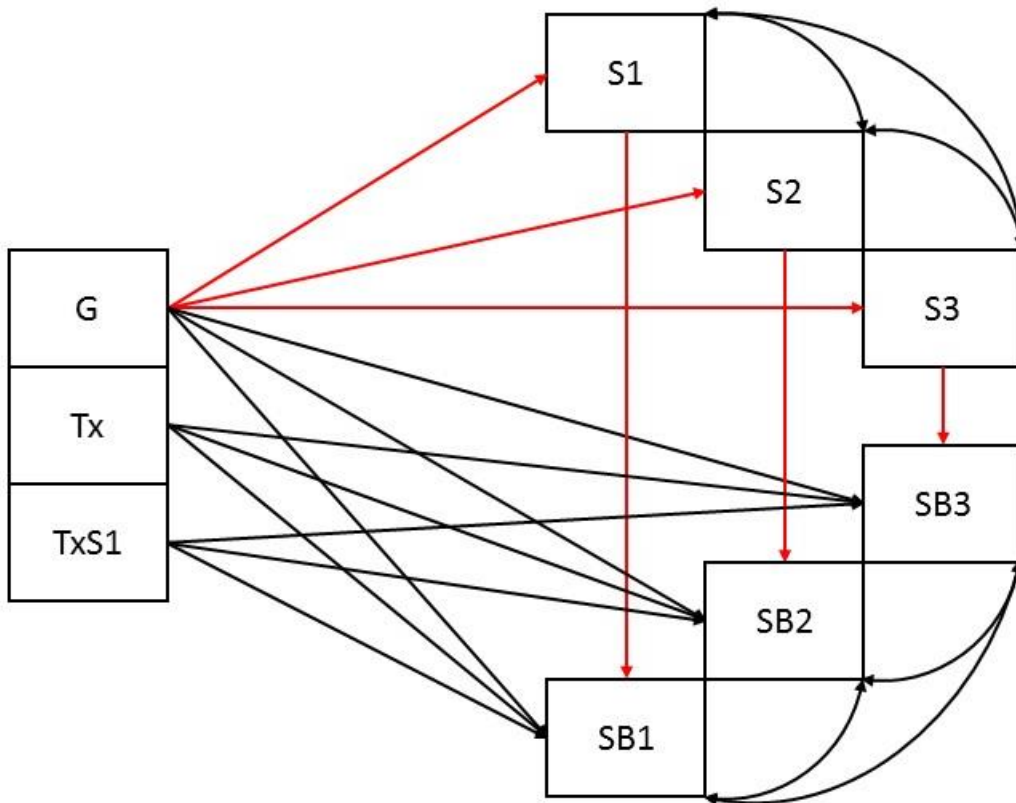
Figure 5. Path diagram of indirect effects of generation status (G) on fall, winter, and spring social-belonging (SB1, SB2, and SB3, respectively) through fall, winter, and spring stress (S1, S2, and S3, respectively) with paths from stress to social-belonging conditioned on treatment condition (Tx); TxS1 = treatment by fall stress interaction and red direct paths indicate indirect paths.

The model did not fit well, $\chi^2(12) = 78.63$, $p < .001$; RMSEA = .27 (90% CI: .22, .33); CFI = .71; SRMR = .18 (see Table 12 for coefficients and confidence intervals). Again, none of the indirect paths had confidence intervals did not include zero, not even 90% confidence intervals. There were several significant direct paths; however, none them were generation → stress coefficients and all of which involved regressions of social-belonging on other variables. There were again some glimmers in the indirect paths that made leveraging the sample size increase with the program-passive-control condition a reasonable exploration. I fit roughly the same model after adding that condition with the same changes in specification as in the resource seeking model. The condition by fall quarter stress interaction caused 29 cases to be deleted due to missingness on predictors ($N = 116$). That model also did not fit well, $\chi^2(18) = 183.03$, $p < .001$; RMSEA = .28 (90% CI: .25, .32); CFI = .61; SRMR = .17 (see Table 13 for coefficients and confidence intervals). However, three specific indirect paths were now marginally significant: generation → winter stress → winter social-belonging, for all three conditions (difference-education: $b = 0.16$ [90% CI: 0.01, 0.40]; standard-control: $b = 0.41$ [90% CI: 0.04, 1.00]; program-passive-control: $b = 0.40$ [90% CI: 0.04, 0.86]). First-generation students in each of the bridge-program conditions felt less stressed during winter quarter than did continuing generation students and feeling less stressed led to a higher sense of social belonging.

Adding cumulative first-year GPA to the model as an outcome of the process (i.e., regressing GPA on fall, winter, and spring social-belonging) did not fit well either, in the two-group fitting ($\chi^2[18] = 88.63$, $p < .001$; RMSEA = .23 [90% CI: .18, .28]; CFI = .71; SRMR = .17) or the three-group ($\chi^2[26] = 199.47$, $p < .001$; RMSEA = .24 [90% CI: .21, .27]; CFI = .60;

SRMR = .17).[13] In the two-group fitting, only two unmoderated indirect paths were marginally

significant: generation → winter social-belonging → cumulative GPA (b = 0.22 [90% CI: 0.03,

0.64]) and generation → spring social-belonging → cumulative GPA (b = -0.23 [90% CI: -0.56,

-0.06]). No indirect paths were significant in the three-group fitting of the model (see Tables 14

and 15 for coefficients and confidence intervals of the two-group and three-group models).

Synthesis? Unfortunately, with such a small sample size, adding more parameters to the

model is unwise. Additionally, the belonging-adversity contingency did not translate into

differences in achievement. Moreover, there are likely different processes at work for first-

generation and continuing-generation students that are conditioned differently. Fitting one model

that explains it all may well be possible, but it is not feasible for these data.

Table 12
Coefficients and Confidence Intervals of Two-Group Belonging-Adversity Model

Path	Lower 2.5%	Lower 5%	Coeff.	Upper 5%	Upper 2.5%
G → S1	-0.43	-0.38	-0.13	0.13	0.18
G → S2	-0.52	-0.48	-0.20	0.09	0.15
G → S3	-0.28	-0.23	0.01	0.24	0.29
G → SB1	-0.23	-0.14	0.33	0.84	0.92
G → SB2	0.07	0.17	0.71	1.27	1.38
G → SB3	0.15	0.25	0.74	1.22	1.31
Tx → SB1	-0.18	-0.08	0.44	0.97	1.07
Tx → SB2	-0.32	-0.22	0.37	0.92	1.03
Tx → SB3	-0.03	0.07	0.59	1.08	1.18
TxS1 → SB1	-1.64	-1.52	-0.89	-0.25	-0.14
TxS1 → SB2	-1.92	-1.78	-1.08	-0.49	-0.39
TxS1 → SB3	-1.57	-1.43	-0.77	-0.18	-0.06
S1 → SB1	-1.07	-0.97	-0.50	-0.09	-0.01
S2 → SB2	-1.05	-0.98	-0.64	-0.34	-0.29
S3 → SB3	-1.34	-1.23	-0.62	-0.13	-0.04
G → S1 → SB1 (DE)	-0.24	-0.16	0.18	0.54	0.61
G → S2 → SB2 (DE)	-0.26	-0.15	0.34	0.86	0.98

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[13] I made several attempts at improving fit of these belonging-adversity models (e.g., adding more direct paths from stress to other waves of social-belonging, changing correlations among social-belonging to direct paths, etc.). Nothing attempted substantially improved fit. I suspect it may be that generation status interacts with treatment to affect the process, but that is not what Walton and G. L. Cohen's (2011) theoretical model implies. What they argue is that treatment disrupts the contingency of social-belonging on experienced adversity, which is a second-stage (Edwards & Lambert, 2007) conditional indirect effects model like the ones I estimated. I tested what they said, not what they did. Therefore, I presented the models that represented the most straightforward tests of theoretical propositions despite their misfit.

Table 12 (continued).

Path	Lower 2.5%	Lower 5%	Coeff.	Upper 5%	Upper 2.5%
G → S3 → SB3 (DE)	-0.46	-0.37	-0.02	0.30	0.37
Total Indirect (DE)	-0.58	-0.38	0.50	1.44	1.65
Total (DE)	0.40	0.72	2.28	3.97	4.24
G → S1 → SB1 (SC)	-0.05	-0.03	0.06	0.30	0.36
G → S2 → SB2 (SC)	-0.07	-0.04	0.13	0.41	0.49
G → S3 → SB3 (SC)	-0.24	-0.19	-0.01	0.13	0.18
Total Indirect (SC)	-0.26	-0.17	0.18	0.62	0.74
Total (SC)	0.36	0.61	1.96	3.36	3.65

Note: Coefficients are unstandardized. G = generation status (first-generation = 1, continuing-generation = 0), Tx = treatment condition (DE = difference-education [coded as 1], SC = standard-control [coded as 0]), TxS1 = treatment by fall stress interaction, S1 = fall stress, S2 = winter stress, S3 = spring stress, SB1 = fall social-belonging, SB2 = winter social-belonging, SB3 = spring social-belonging.

Table 13
Coefficients and Confidence Intervals of Three-Group Belonging-Adversity Model

Path	Lower 2.5%	Lower 5%	Coeff.	Upper 5%	Upper 2.5%
G → S1	-0.40	-0.35	-0.13	0.11	0.15
G → S2	-0.57	-0.51	-0.26	-0.01	0.03
G → S3	-0.40	-0.35	-0.08	0.17	0.22
G → SB1	-0.42	-0.35	0.05	0.45	0.52
G → SB2	-0.33	-0.24	0.20	0.66	0.74
G → SB3	-0.11	-0.03	0.40	0.80	0.88
Txi → SB1	-1.11	-1.00	-0.44	0.06	0.16
Txi → SB2	-1.12	-1.00	-0.40	0.14	0.25
Txi → SB3	-1.27	-1.16	-0.62	-0.13	-0.03
Txii → SB1	-0.51	-0.43	0.01	0.44	0.52
Txii → SB2	-0.53	-0.43	0.07	0.56	0.66
Txii → SB3	-0.88	-0.78	-0.29	0.19	0.28
TxSi → SB1	-1.42	-1.22	-0.40	0.36	0.48
TxSi → SB2	-2.01	-1.78	-0.95	-0.34	-0.23
TxSi → SB3	-1.56	-1.40	-0.60	0.02	0.16
TxSii → SB1	-1.09	-0.98	-0.47	0.01	0.11
TxSii → SB2	-1.42	-1.34	-0.92	-0.50	-0.41
TxSii → SB3	-1.42	-1.32	-0.79	-0.21	-0.09
S1 → SB1	-1.20	-1.13	-0.75	-0.33	-0.23
S2 → SB2	-0.94	-0.89	-0.62	-0.38	-0.34
S3 → SB3	-0.99	-0.92	-0.60	-0.25	-0.17
G → S1 → SB1 (DE)	-0.09	-0.06	0.10	0.31	0.36
G → S2 → SB2 (DE)	-0.01	0.01	0.16	0.40	0.45
G → S3 → SB3 (DE)	-0.13	-0.09	0.05	0.25	0.29
Total Indirect (DE)	-0.13	-0.06	0.31	0.76	0.86
Total (DE)	-0.44	-0.22	0.96	2.12	2.36
G → S1 → SB1 (SC)	-0.14	-0.09	0.15	0.53	0.61
G → S2 → SB2 (SC)	-0.03	0.04	0.41	1.00	1.13

Table 13 (continued).

Path	Lower 2.5%	Lower 5%	Coeff.	Upper 5%	Upper 2.5%
G → S3 → SB3 (SC)	-0.28	-0.20	0.10	0.52	0.63
Total Indirect (SC)	-0.24	-0.08	0.66	1.68	1.94
Total (SC)	-0.41	-0.13	1.31	2.71	2.99
G → S1 → SB1 (PPC)	-0.18	-0.12	0.16	0.43	0.49
G → S2 → SB2 (PPC)	-0.03	0.04	0.40	0.86	0.97
G → S3 → SB3 (PPC)	-0.31	-0.23	0.12	0.50	0.59
Total Indirect (PPC)	-0.25	-0.09	0.67	1.56	1.74
Total (PPC)	-0.36	-0.09	1.32	2.74	3.02

Note: Coefficients are unstandardized. G = generation status (first-generation = 1, continuing-generation = 0), Txi = treatment dummy representing comparison of difference-education (DE) to standard-control (SC), Txii = treatment dummy representing comparison of difference-education to program-passive-control (PPC), TxSi = treatment by fall stress interaction term representing additional effect of a one-unit increase in stress during fall quarter in the standard-control condition, TxSii = treatment by generation interaction term representing additional effect of a one-unit increase in stress during fall quarter in the program-passive-control condition, S1 = fall stress, S2 = winter stress, S3 = spring stress, SB1 = fall social-belonging, SB2 = winter social-belonging, SB3 = spring social-belonging.

Table 14

Coefficients and Confidence Intervals of Two-Group Belonging-Adversity Model with Cumulative GPA as Outcome

Path	Lower 2.5%	Lower 5%	Coeff.	Upper 5%	Upper 2.5%
G → S1	-0.43	-0.39	-0.13	0.13	0.18
G → S2	-0.56	-0.51	-0.22	0.07	0.13
G → S3	-0.23	-0.19	0.05	0.29	0.34
G → SB1	-0.24	-0.14	0.33	0.83	0.91
G → SB2	0.08	0.19	0.73	1.32	1.43
G → SB3	-0.13	-0.02	0.58	1.11	1.21
Tx → SB1	-0.18	-0.08	0.44	0.97	1.06
Tx → SB2	-0.36	-0.25	0.35	0.91	1.02
Tx → SB3	-0.05	0.05	0.58	1.11	1.20
TxS1 → SB1	-1.62	-1.50	-0.86	-0.23	-0.11
TxS1 → SB2	-1.97	-1.82	-1.11	-0.49	-0.38
TxS1 → SB3	-1.42	-1.23	-0.52	0.19	0.33
S1 → SB1	-1.10	-1.00	-0.52	-0.12	-0.05
S2 → SB2	-1.08	-1.01	-0.67	-0.37	-0.32
S3 → SB3	-1.48	-1.37	-0.69	-0.14	-0.02
SB1 → CGPA	-0.22	-0.16	0.06	0.26	0.30
SB2 → CGPA	-0.06	0.04	0.29	0.54	0.58
SB3 → CGPA	-0.69	-0.65	-0.40	0.01	0.18
G → SB1 → CGPA	-0.05	-0.02	0.02	0.20	0.24
G → SB2 → CGPA	0.00	0.03	0.22	0.64	0.71
G → SB3 → CGPA	-0.61	-0.56	-0.23	-0.06	0.00
G → S1 → SB1 → CGPA (DE)	-0.03	-0.01	0.01	0.13	0.16
G → S2 → SB2 → CGPA (DE)	-0.03	0.00	0.11	0.48	0.57

Table 14 (continued).

Path	Lower 2.5%	Lower 5%	Coeff.	Upper 5%	Upper 2.5%
G → S3 → SB3 → CGPA (DE)	-0.08	-0.05	0.03	0.21	0.26
Total Indirect (DE)	-0.09	-0.06	0.15	0.50	0.58
Total (DE)	0.19	0.45	1.79	3.12	3.36
G → S1 → SB1 → CGPA (SC)	-0.01	-0.01	0.00	0.07	0.09
G → S2 → SB2 → CGPA (SC)	-0.01	0.00	0.04	0.21	0.26
G → S3 → SB3 → CGPA (SC)	-0.03	-0.02	0.02	0.17	0.22
Total Indirect (SC)	-0.15	-0.12	0.06	0.27	0.33
Total (SC)	0.07	0.32	1.70	3.01	3.25

Note: Coefficients are unstandardized. G = generation status (first-generation = 1, continuing-generation = 0), Tx = treatment condition (DE = difference-education, SC = standard-control), TxS1 = treatment by fall stress interaction, S1 = fall stress, S2 = winter stress, S3 = spring stress, SB1 = fall social-belonging, SB2 = winter social-belonging, SB3 = spring social-belonging, CGPA = cumulative first-year grade point average.

Table 15

Coefficients and Confidence Intervals of Three-Group Belonging-Adversity Model with Cumulative GPA as Outcome

Path	Lower 2.5%	Lower 5%	Coeff.	Upper 5%	Upper 2.5%
G → S1	-0.40	-0.35	-0.13	0.11	0.15
G → S2	-0.57	-0.52	-0.27	-0.01	0.03
G → S3	-0.42	-0.36	-0.09	0.18	0.23
G → SB1	-0.42	-0.35	0.05	0.45	0.53
G → SB2	-0.33	-0.23	0.21	0.67	0.75
G → SB3	-0.11	-0.03	0.40	0.83	0.91
Txi → SB1	-1.11	-1.00	-0.44	0.06	0.17
Txi → SB2	-1.12	-1.00	-0.38	0.16	0.27
Txi → SB3	-1.30	-1.19	-0.62	-0.14	-0.04
Txii → SB1	-0.51	-0.43	0.01	0.43	0.52
Txii → SB2	-0.54	-0.44	0.07	0.57	0.67
Txii → SB3	-0.91	-0.82	-0.28	0.19	0.29
TxSi → SB1	-1.46	-1.28	-0.42	0.34	0.47
TxSi → SB2	-2.00	-1.78	-0.94	-0.34	-0.23
TxSi → SB3	-1.62	-1.44	-0.61	0.00	0.12
TxSii → SB1	-1.13	-1.02	-0.49	-0.01	0.09
TxSii → SB2	-1.51	-1.41	-0.93	-0.51	-0.42
TxSii → SB3	-1.54	-1.40	-0.78	-0.17	-0.04
S1 → SB1	-1.19	-1.11	-0.74	-0.28	-0.19
S2 → SB2	-0.95	-0.90	-0.63	-0.37	-0.32
S3 → SB3	-0.98	-0.91	-0.59	-0.20	-0.12
SB1 → CGPA	-0.17	-0.13	0.06	0.25	0.29
SB2 → CGPA	-0.29	-0.24	0.09	0.36	0.41
SB3 → CGPA	-0.51	-0.46	-0.04	0.37	0.42
G → SB1 → CGPA	-0.05	-0.03	0.00	0.08	0.10
G → SB2 → CGPA	-0.06	-0.04	0.02	0.20	0.24
G → SB3 → CGPA	-0.28	-0.23	-0.02	0.14	0.18

Table 15 (continued).

Path	Lower 2.5%	Lower 5%	Coeff.	Upper 5%	Upper 2.5%
G → S1 → SB1 → CGPA (DE)	-0.01	-0.01	0.01	0.07	0.09
G → S2 → SB2 → CGPA (DE)	-0.04	-0.03	0.02	0.11	0.14
G → S3 → SB3 → CGPA (DE)	-0.08	-0.07	0.00	0.03	0.04
Total Indirect (DE)	-0.17	-0.14	0.02	0.14	0.17
Total (DE)	-0.58	-0.37	0.68	1.78	2.00
G → S1 → SB1 → CGPA (SC)	-0.02	-0.01	0.01	0.01	0.13
G → S2 → SB2 → CGPA (SC)	-0.11	-0.07	0.04	0.27	0.34
G → S3 → SB3 → CGPA (SC)	-0.19	-0.14	0.00	0.05	0.08
Total Indirect (SC)	-0.16	-0.12	0.05	0.20	0.26
Total (SC)	-0.56	-0.35	0.71	1.79	2.00
G → S1 → SB1 → CGPA (PPC)	-0.02	-0.01	0.01	0.09	0.11
G → S2 → SB2 → CGPA (PPC)	-0.10	-0.06	0.04	0.26	0.36
G → S3 → SB3 → CGPA (PPC)	-0.17	-0.13	-0.01	0.06	0.09
Total Indirect (PPC)	-0.16	-0.12	0.05	0.20	0.25
Total (PPC)	-0.582	-0.36	0.70	1.79	2.00

Note: Coefficients are unstandardized. G = generation status(first-generation = 1, continuing-generation = 0), Txi = treatment dummy representing comparison of difference-education (DE) to standard-control (SC), Txii = treatment dummy representing comparison of difference-education to program-passive-control (PPC), TxSi = treatment by fall stress interaction term representing additional effect of a one-unit increase in stress during fall quarter in the standard-control condition, TxSii = treatment by generation interaction term representing additional effect of a one-unit increase in stress during fall quarter in the program-passive-control condition, S1 = fall stress, S2 = winter stress, S3 = spring stress, SB1 = fall social-belonging, SB2 = winter social-belonging, SB3 = spring social-belonging, CGPA = cumulative first-year grade point average.

End-of-Year Outcomes: Residential Group Differences

After multiple imputation, the outcomes measured in the wave 3 survey (life satisfaction, self-esteem, sadness, stress, general health, illness, BMI, academic enjoyment, extracurricular activity, resource seeking [original and expanded versions], and social-belonging) were regressed on individual, binary groupings (0 = conventional residence hall vs. 1 = off-campus, 0 = conventional residence hall vs. 1 = FIGARC, 0 = FIGARC vs. 1 = off-campus) and, in a separate set of regressions, on the groupings and the weights produced by propensity-score matching. Regressions across data sets were pooled to yield overall tests of coefficients. For achievement (GPA) and persistence outcomes (enrollment in fall of second year and in summer), the same analysis models applied, but on complete-case data and persistence was analyzed with logistic not

linear regressions. An additional set of regressions were conducted for persistence outcomes that controlled for cumulative first-year GPA.

Here too, hypotheses regarding comparisons of residential groups on all outcomes except social-belonging were directional and were specified prior to conducting the study.[14] Hence, I used one-tailed tests here, too. However, when using one-tailed tests, two of three marginally significant differences emerged in the direction opposite of that predicted between students living in conventional residence halls and those living in FIGARCs. Because there were so few differences between these groups and most of them should be disregarded or at least taken with a very large grain of salt, I report them and move on to report comparisons to students living off-campus, where most differences were found, all but one of which were in the predicted direction.

In the predicted direction, students living in FIGARCs had a marginally lower frequency of illness than did students living in conventional residence halls when samples were weighted, $t(73.76) = -1.38$, $p = .086$, but not when they were unweighted, $p = .143$. In the unexpected direction, students living in FIGARCs had marginally higher BMI than did students living in conventional residence halls when samples were weighted, $t(101.61) = 1.35$, $p = .090$, but not when unweighted, $p = .206$, and had a marginally lesser tendency to seek resources (original measurement only) when samples were weighted, $t(76.44) = -1.36$, $p = .088$, and unweighted, $t(69.94) = -1.52$, $p = .066$. However, none of those p-values warrant being called marginally significant when multiplied by 2, which should be done because the effects are in the wrong tails.

Psychological wellness outcomes. Coefficients are presented in Table 16. For life satisfaction, both group differences were non-significant in the unweighted regression (CRH vs. OC: $p = .124$; FIGARC vs. OC: $p = .322$) but were significant when weighted. Students living in conventional residence halls, $t(50.53) = -2.06$, $p = .022$, and students living in FIGARCs, $t(65.71) = -2.02$, $p = .023$, were more satisfied with life than were students living off-campus. Students

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[14] Although they were not technically preregistered, as the focus of preregistration was on the intervention, my dissertation proposal is part of the preregistration which is frozen in time and contains my hypotheses for these analyses.

living in conventional residence halls did not differ from students living off-campus in terms of self-esteem when samples were unweighted ($p = .205$), but students in conventional residence halls had *lower* self-esteem when weighted, $t(61.21) = 1.91$, $p = .030$. This was the only other difference among these comparisons that came out in the opposite direction than that predicted. Going with a two-tailed test, $p = .060$, the difference still may be worth attending to. There was no difference on self-esteem, weighted ($p = .295$) or unweighted ($p = .292$), between students living in FIGARCs and students living off-campus. There were no differences whatsoever on sadness, ps $\geq .141$. For stress, students living in conventional residence halls were significantly less stressed than were students living off-campus in the unweighted regression, $t(58.95) = 1.92$, $p = .030$, but the difference was non-significant when weighted ($p = .446$). When samples were unweighted, students living in FIGARCs were marginally less stressed than were students living off-campus ($p = .080$), and significantly less stressed when weighted, $t(69.73) = 2.15$, $p = .018$.

Table 16
Coefficients for Residential Group Comparisons on Psychological Wellness Constructs

	Life Satisfaction		Self-Esteem		Sadness		Stress	
	no PS	PS	no PS	PS	no PS	PS	no PS	PS
CRH^C FIGARC^T	-0.12	-0.02	-0.22	-0.41	-0.03	-0.08	0.00	-0.04
CRH^C OC^T	-0.43	-0.81*	0.36	0.84†	-0.25	-0.18	0.35*	-0.03
FIGARC^C OC^T	-0.19	-0.77*	-0.25	0.24	0.05	-0.28	0.27†	0.38*

Note: Coefficients are unstandardized. PS = regressions used sample weights produced by propensity score matching, no PS = regressions did not use sample weights, CRH = students living conventional residence halls, FIGARC = students living in residential freshman interest groups or academic residential communities, OC = students living off-campus. Superscripts ^{C} and ^{T} indicate conditions specified as control and treatment, respectively, in propensity-score matching. Tests are one-tailed, except for CRH vs. OC on self-esteem, which was kept two-tailed on account of the difference being in the opposite direction of that predicted. †$p < .10$, *$p < .05$, ‡$p < .01$

Physical wellness outcomes. Coefficients are presented in Table 17. Across physical wellness outcomes, only one marginally significant difference emerged between students living in FIGARCs students living off-campus. Students living in FIGARCs had a marginally lower

frequency of illness than did students living off-campus when samples were unweighted, $t(61.37)$

$= 1.47$, $p = .073$, but not when weighted, $p = .110$.

Table 17
Coefficients for Residential Group Comparisons on Physical Wellness Constructs

	General Health		Illness		Body Mass Index	
	no PS	PS	no PS	PS	no PS	PS
CRH^C FIGARC^T	-0.08	-0.20	-0.07	-0.08†	0.71	1.08
CRH^C OC^T	0.09	0.09	0.02	-0.01	-0.67	-0.70
FIGARC^C OC^T	-0.26	-0.21	0.11	0.09†	0.25	-0.90

Note: Coefficients are unstandardized. Tests are one-tailed. PS = regressions used sample weights produced by propensity score matching, no PS = regressions did not use sample weights, CRH = students living conventional residence halls, FIGARC = students living in residential freshman interest groups or academic residential communities, OC = students living off-campus. Superscripts ^{C} and ^{T} indicate conditions specified as control and treatment, respectively, in propensity-score matching. Tests are one-tailed, except for CRH vs. FIGARC on body mass index, which was kept two-tailed on account of the difference being in the opposite direction of that predicted.
†$p < .10$, *$p < .05$, ‡$p < .01$

Success outcomes. Coefficients are presented in Tables 18 and 19. There were no

differences on achievement and persistence outcomes (ps $\geq .232$), except for one marginally

significant difference on enrollment for summer term *when controlling for cumulative first-year*

GPA. Students living off-campus were slightly more likely to be enrolled than were students

living in conventional residence halls when samples were unweighted, $z(92) = 1.33$, $p = .093$, and

this was just outside of marginal significance when weighted, $p = .102$. For academic enjoyment,

students living in FIGARCs enjoyed academic pursuits significantly more than did students living

off-campus when samples were unweighted, $t(65.35) = -1.81$, $p = .037$, but that difference was

just outside of marginal significance when weighted ($p = .102$). There was no significant

difference, weighted ($p = .103$) or unweighted ($p = .195$), between students living in conventional

residence halls and students living off-campus on academic enjoyment. For extracurricular

activity, students living in conventional residence halls were significantly more engaged than were students living off-campus when samples were unweighted, $t(72.04) = -2.87$, $p = .003$, but the difference was non-significant when weighted ($p = .115$). Students living in FIGARCs were also more engaged in extracurricular activities than were students living off-campus, $t(65.59) = -3.30$, $p = .001$, and that difference remained significant when weighted, $t(55.38) = -2.83$, $p = .003$. For resource seeking, students living in FIGARCs had a marginally greater tendency to seek resources using the original measurement items than did students living off-campus when samples were weighted, $t(60.82) = -1.34$, $p = .092$, and the p-value was just outside of marginal significance when unweighted, $p = .106$. The difference between students living in conventional residence halls and those living off-campus was not significant when samples were weighted ($p = .118$) or unweighted ($p = .158$). Better results obtained with the expanded measurement. Students living in conventional residence halls had a marginally greater tendency to seek resources than did students living off-campus when samples were weighted, $t(70.90) = -1.45$, $p = .076$, and the difference was significant when weighted, $t(69.37) = -1.71$, $p = .046$. And students living in FIGARCs also had a marginally greater tendency to seek resources than did students living off-campus when samples were weighted, $t(63.05) = -1.47$, $p = .073$, and when they were unweighted, $t(64.38) = -1.45$, $p = .076$.

Social-belonging. Coefficients are presented in Table 20. Students living in conventional residence halls had a greater sense of social-belonging than did students living off-campus in the unweighted regression, $t(74.23) = -2.00$, $p = .049$, two-tailed, and that difference remained significant when weighted, $t(69.73) = -2.30$, $p = .025$, two-tailed. Similarly, students living in FIGARCs had a greater sense of social-belonging than did students living off-campus in the unweighted regression, $t(65.35) = -3.35$, $p = .001$, two-tailed, and that difference remained significant when weighted, $t(65.35) = -3.95$, $p < .001$, two-tailed.

Table 18
Coefficients for Residential Group Comparisons on Success Constructs (Engagement)

	Academic Enjoyment		Extracurricular Activity		Resource Seeking (Original)		Resource Seeking (Expanded)	
	no PS	PS	no PS	PS	no PS	PS	no PS	PS
CRH^C FIGARC^T	0.20	0.07	0.20	0.23	-0.12	-0.10	0.06	0.08
CRH^C OC^T	-0.29	-0.41	-0.93‡	-0.43	-0.09	-0.10	-0.15†	-0.18*
FIGARC^C OC^T	-0.70*	-0.50	-1.05‡	-0.95‡	-0.11	-0.12†	-0.04†	-0.07†

Note: Coefficients are unstandardized. Tests are one-tailed. PS = regressions used sample weights produced by propensity score matching, no PS = regressions did not use sample weights, CRH = students living conventional residence halls, FIGARC = students living in residential freshman interest groups or academic residential communities, OC = students living off-campus. Superscripts ^{C} and ^{T} indicate conditions specified as control and treatment, respectively, in propensity-score matching. Tests are one-tailed, except for CRH vs. FIGARC on resource seeking (original), which was kept two-tailed on account of the difference being in the opposite direction of that predicted. †$p < .10$, *$p < .05$, ‡$p < .01$

Table 19
Coefficients for Residential Group Comparisons on Success Constructs (Achievement and Persistence)

	Achievement		Persistence							
			2nd Year		2nd Year CGPA		Summer		Summer CGPA	
	no PS	PS	no PS	PS	no PS	PS	no PS	PS	no PS	PS
CRH^C FIGARC^T	0.04	-0.04	0.15	0.41	0.03	0.21	0.49	0.24	0.43	0.34
CRH^C OC^T	0.01	-0.09	-0.23	-0.47	0.15	-0.08	0.97	0.93	1.11†	1.05
FIGARC^C OC^T	-0.14	-0.11	-0.26	-0.29	-0.09	-0.12	0.32	0.17	0.45	0.29

Note: Coefficients are unstandardized and those for Persistence are in log units. 2nd Year = enrolled for fall of 2nd year, Summer = enrolled for summer, CGPA = controlling for cumulative 1st-year GPA, PS = regressions used sample weights produced by propensity score matching, no PS = regressions did not use sample weights, CRH = students living conventional residence halls, FIGARC = students living in residential freshman interest groups or academic residential communities, OC = students living off-campus. Superscripts ^{C} and ^{T} indicate conditions specified as control and treatment, respectively, in propensity-score matching. Tests are one-tailed. †$p < .10$, *$p < .05$, ‡$p < .01$

Table 20
Coefficients for Residential Group Comparisons on Social-Belonging

	Social-Belonging	
	no PS	PS
CRH^C FIGARC^T	0.02	-0.22
CRH^C OC^T	-0.64*	-0.74*
FIGARC^C OC^T	-1.24‡	-1.43‡

Note: Coefficients are unstandardized. PS = regressions used sample weights produced by propensity score matching, no PS = regressions did not use sample weights, CRH = students living conventional residence halls, FIGARC = students living in residential freshman interest groups or academic residential communities, OC = students living off-campus. Superscripts ^{C} and ^{T} indicate conditions specified as control and treatment, respectively, in propensity-score matching. Tests are two-tailed. †$p < .10$, *$p < .05$, ‡$p < .01$

CHAPTER IV

DISCUSSION

Metanarrative

In the following sections, I start by summarizing and interpreting my testing of Stephens and colleagues' (2014) difference-education intervention. Given the circumstances made evident in the Results section, this portion is rather protracted. It begins very simply with a reiteration of major findings insofar as end-of-year outcomes and some other significant findings that deserve a reiterated dismissal, and segues into describing structural model results, including the troubling ones, pertaining to proposed mechanisms. Because these results are rather different than anything theoretically predicted, I then reiterate model predictions and reframe results in contrast. Subsequently, I summarize and interpret the findings regarding end-of-year outcome differences between propensity-score matched residential groups. This exposition is much simpler and more straightforward, but its content is not free of oddities. In the final sections, I discuss various limitations to inferences and potential routes of future investigation, which are mostly practical and applied, and offer some final conclusions.

Summary and Interpretation: Testing the Difference-Education Intervention

End-of-year outcomes. I found very few differences between the intervention condition and control conditions at the end of students' transitional year. The only difference between the difference-education and standard-control conditions was that students in the difference-education condition had significantly lower body mass index (BMI), on average, *after* controlling for baseline BMI, but not before baseline BMI was entered as a predictor. Entering baseline control reduced the magnitude of the coefficient, but it also reduced the standard error. This result may be notable, given that the conditions were descriptively reversed at baseline ($M_{DE} = 24.91$, $SD_{DE} = 4.32$; $M_{SC} = 23.79$, $SD_{SC} = 5.30$) and significantly different; however, exercise was not entered as a control covariate.

Compared to students in the program-passive-control condition, students in the difference-education condition were, on average, less sad and stressed at the end of the year, and these differences remained after controlling for baseline sadness and stress. Compared to students in the nonprogram-passive-control condition, students in the difference-education condition found academic pursuits more enjoyable and were less engaged in extracurricular activities. The difference on academic enjoyment remained after controlling for baseline, but the difference on extracurricular activity did not. Though not the focus of the present research, the lack of end-of-year differences between the difference-education condition and the nonprogram-passive-control condition may be of note. At the beginning of the year, students in the difference-education condition were significantly less satisfied with life and less healthy, had significantly higher BMI, got marginally less enjoyment out of academic pursuits, and were marginally less engaged in extracurricular activities. At the end of the year, academic enjoyment had reversed and the rest were absent.

Finally, there were two condition by generation status interactions: those which affected achievement and persistence to the second year. In terms of model-predicted achievement, first-generation and continuing-generation students in the standard-control and nonprogram-passive-control conditions as well as first-generation students in the difference-education and program-passive-control conditions had similar GPAs, on average, which were all higher than those of continuing generation students in the difference-education and program-passive-control conditions, which were similar to each other. In terms of raw means, the same pattern mostly obtained, but consistent with practical expectation, continuing-generation students who were not in the bridge program had the highest GPA, on average – about a quarter of a point higher than their first-generation counterparts (refer back to Figure 1). On persistence, model-predicted probabilities (with and without controlling for cumulative first-year GPA) mostly agreed with each other (refer back to Figure 2). The greatest persistence was among first-generation students in the difference-education condition, continuing-generation students in the standard-control

condition, and first-generation students in the program-passive-control condition. Persisting at somewhat lower levels were continuing-generation students in the difference-education, program-passive-control, and nonprogram-passive-control conditions. Of potential concern were first-generation students in the standard-control and nonprogram-passive-control conditions. However, looking simply at the raw numbers of students enrolled and not enrolled for fall of the second year (refer back to Figure 3), first-generation students in the nonprogram-passive-control condition appear to be the only concerning group: 24% of them were not enrolled, compared to 14% or less for all other cells.

Models of mechanisms. I had three aims for modeling mechanisms: (a) test Stephens and colleagues' (2014) model, (b) test whether the difference-education intervention has the same psychological consequence as Walton and G. L. Cohen (2011) theorized for their social-belonging intervention, and (c) synthesize the mechanism models proposed by Stephens et al. (2014) and Walton and G. L. Cohen (2011) by adding resource seeking as a behavioral consequence of the uncoupling of the belonging-adversity contingency. In attempting to replicate Stephens and colleagues' (2014) model, I found that first-generation students in the standard-control condition sought fewer resources (original measurement) than did continuing-generation students during winter quarter and seeking fewer resources resulted in lower cumulative first-year GPA; the paths through fall and spring resource seeking were nonsignificant. There were no significant indirect paths in the difference-education condition. These results do not support the mechanism model proposed by Stephens et al. (2014), that difference-education leads to greater resource seeking for first-generation students, which leads to higher achievement. It also does not support a reduced, main-effect model (i.e., no condition by generation interaction on account of implementing the intervention within the bridge program, in which the students are arguably all at a similar disadvantage).

In attempting to extend the model to include theoretical propositions of Walton and G. L. Cohen (2011), I found that first-generation students felt less stressed during winter quarter than

88

did continuing generation students in each of the bridge-program conditions and feeling less stressed led to a higher sense of social belonging during winter quarter. The effect in the difference-education condition was less than half the size of those in the standard-control and program-passive-control conditions. Although the effect size differences are consistent with Walton and G. L. Cohen's (2011) mechanism model, the direction of the effects is the opposite of what they should be. That is, the effect of generation status on social-belonging through stress should have been more prominent in the standard-control condition than in the difference-education condition, because the intervention treatment should have reduced the belonging-adversity contingency, but first-generation students should have felt *more* stressed and consequently had a *lower* sense of social-belonging. Moreover, the indirect effects through fall and spring stress were nonsignificant, and the ones through winter stress did not translate into cumulative first-year GPA differences when that was added to the model as an ultimate outcome. Because of this fact and several others (e.g., small sample size), I did not attempt to synthesize the models so that resource seeking, stress, social-belonging, and GPA were all in the same model.

There were also some troubling differences between the two-group and three-group models for both replication and extension. For replication, the glimmer of hope in the two-group model was the generation → fall resource seeking → GPA specific indirect effect in the standard-control condition. It had the largest coefficient and was fairly close to marginal significance. However, in the three group model, the marginally significant specific indirect path in the standard-control condition was generation → *winter* resource seeking → GPA. It is not clear why the indirect effect would have shifted like that, but it could indicate that earlier resource seeking transmits the effect to later resource seeking and adding the program-passive-control condition altered the covariance structure enough to alter how the effect was transmitted. For example, the indirect path could look like generation → fall resource seeking → winter resource seeking → GPA, but the bidirectional paths connecting fall, winter, and spring resource seeking block that

89

transmission, making its estimation volatile and sensitive to perturbations of the covariance structure. Alternatively, empirical underidentification (Kenny, 1979) could be making estimates unstable. Or, the fact that there are no detectable group mean differences on resource seeking could be producing instability.

For extension, the indirect effects from generation status to social-belonging through stress in the difference-education and standard-control conditions were of opposite magnitudes in the two-group and three-group models. In the two-group model, the coefficient for the difference-education condition was over 2.5 times that for the standard-control condition, whereas in the three-group model, the coefficient for the standard-control condition was 2.5 times those for the difference-education and program-passive-control conditions. Potential explanations of inconsistencies described above apply here as well. Whatever the case, none of these shifty results align with theoretical predictions.

What do the theoretical models predict? Theoretically, what should happen according to Stephens and colleagues' (2014) model is that first-generation students exposed to the difference-education treatment should be motivated to seek and take advantage of resources, including their professors and various student services. Seeking and taking advantage of those resources should result in greater achievement (i.e., higher GPA). This should boost resource seeking and achievement of first-generation students in the difference-education condition up to a level on par with continuing-generation students generally. It should also produce an indirect effect where first-generation students in the difference-education condition seek more resources than do first-generation students in the standard-control condition, and increased resource seeking should result in higher achievement.

If we put this in the context of implementing the intervention in a bridge program, then it may be more reasonable to expect only a main effect of condition. The bridge program is itself a treatment, albeit one that Stephens et al. (2014) argue is insufficient for closing achievement gaps between more and less advantaged students. It is also wholly comprised of students with low-

90

income backgrounds. Thus, perhaps students exposed to the difference-education treatment should generally be motivated to seek and take advantage of resources and doing so should result in greater achievement in comparison to students exposed to the standard-control treatment (i.e., just an indirect effect, not a conditional indirect effect). Presumably, there should also be no difference between standard-control and program-passive-control because standard-control is intended to simulate the bridge program (i.e., Stephens et al. modeled the design of the standard-control condition on typical practices of bridge programs).

Among students not in the bridge program, we should expect the typical generation status difference on achievement (i.e., continuing-generation students have higher GPAs, on average, than do first-generation students). Together, this predicts a particular picture that I should have observed in both mean-level resource seeking and GPA. Specifically, means for standard-control, program-passive-control, and first-generation students not in the bridge program should have been roughly similar to each other and lower than means for the difference-education condition and continuing-generation students not in the bridge program, which should be similar to each other. This is clearly not what happened in the present research, and I did not attempt to reduce the conditional indirect effects model to an indirect effects model because there was actually an interactive effect on achievement, just not of the form the theory predicts.

Theoretically, what should happen according to Walton and G. L. Cohen's (2011) model, assuming the difference-education intervention operates like the social-belonging intervention, is that sense of social-belonging should not be contingent on experiencing adversity for first-generation students exposed to the difference-education treatment. Disrupting this contingency should result in greater achievement. This should first produce an indirect effect from generation status to social-belonging through stress in the standard-control condition, but not in the difference-education condition. The indirect effect in the standard-control condition should continue to transmit to achievement, specifically lower achievement for first-generation students, while the effect would have already been eliminated in the difference-education condition on

91

account of disrupting the belonging-adversity contingency. This is also clearly not what happened in the present research. Putting this in the context of the bridge program, I might have reduced the model from conditional indirect effects to indirect effects, but given that there may actually be conditional indirect effects at play, that venture seemed like it would produce more problematic results than promising ones.

   What *did* happen? Focusing first on the treated groups, in particular, the pattern of mean GPAs is the reverse of that predicted by theory not contextualized within the bridge program. *Continuing-generation* students in the *difference-education* condition, rather than first-generation students in the standard-control condition, had a lower mean GPA than did the other three groups. Additionally, first-generation students in the standard-control condition sought fewer resources than did their continuing-generation counterparts during winter quarter, and seeking fewer resources resulted in lower GPA; the indirect effects in the difference-education condition were all virtually zero. This is what the coefficients say anyway, but it is a strange way to frame it in the context of both the theory behind the intervention and in terms of what was observed regarding mean-level GPA. Perhaps a better way is to paraphrase Stephens et al. (2014): *continuing-generation* students in the *standard-control* condition more fully took advantage of resources during winter quarter and this ultimately improved their academic performance.

   Moreover, first-generation students in both the difference-education condition and the standard-control condition felt less stressed during winter quarter than did continuing-generation students and feeling less stressed led to a higher sense of social belonging during winter quarter. However, the coefficient for that effect in the difference-education condition was less than half the size of the coefficient in the standard-control condition. This is again what the coefficients say and it is a strange framing, given the situation. Reframing it, continuing-generation students felt more stressed in winter quarter, which resulted in a lower sense of social-belonging, and this effect was stronger in the standard-control condition than in the difference-education condition. The belonging-adversity contingency may have been reduced by the difference-education

condition, but if it was reduced, such a reduction did not occur among the group of people the theory predicts it should, and relieving that burden did not disinhibit academic performance.

Zooming out a level, the GPA difference between first-generation and continuing-generation students in the difference-education condition is on par with the difference between those groups in the program-passive-control condition, suggesting that difference-education did not affect achievement. There were also no indirect effects of generation on GPA through resource seeking for the program-passive-control condition. There was an indirect effect of generation on winter social-belonging through winter stress of equal magnitude as that observed in the standard-control condition, which again is more than twice the size of that in the difference-education condition, further supporting the idea that difference-education reduced the belonging-adversity contingency (though for the opposite group of students it should have). Further, students in the program-passive-control condition were generally the most stressed, on average, at the end of their first year, likely because of the continuing-generation students in particular (that interaction coefficient was bordering on two-tailed marginal significance, $p = .123$, and it was negative, meaning that continuing-generation students, who were coded as 0, were more stressed than first-generation students).

Zooming to the full picture, continuing-generation students who were not in the bridge program had the highest raw mean GPA, although that was attenuated in regression to being among the highest mean GPAs. This is the only result from testing the intervention that makes straightforward sense on its own. These students are the people who tend to have the highest GPAs. The central purpose of bridge programs and social-psychological interventions is to bring other groups' mean GPAs in line with that of the advantaged demographic group. In line with this group's GPA, in terms of model-predicted means, are first-generation students across the board and continuing-generation students in the standard-control condition. In addition, by the end of the year, students in the difference-education condition did not differ from students outside of the bridge program on four of the five constructs on which they differed at matriculation and the fifth

had reversed. That is, the differences on life satisfaction, general health, body mass index, and (when controlling for baseline) extracurricular activity were notably absent, and students in the difference-education condition enjoyed academic pursuits more than did students outside of the bridge program. It is not clear, however, whether this is attributable to the bridge program or the difference-education treatment.

A few tentative conclusions might be drawn from these findings. First, the bridge program does a better job of supporting first-generation students than it does continuing-generation students. Second, the difference-education treatment did not boost achievement. Third, the standard-control-treatment may have actually helped continuing-generation students in exactly the same way the difference-education condition was supposed to help first-generation students: by encouraging resource seeking, thereby boosting achievement. Fourth, perhaps we do not know what we thought we knew about these sorts of interventions, where what we think we know is based on research that did not conduct efficacy tests within programs that are thought to be insufficient for closing achievement gaps.

Summary and Interpretation: Investigating Residential Groups

Considering results from only regressions weighted to match residential groups (i.e., using propensity-score matching), I found only one predicted difference on end-of-year outcomes between students living in residential freshman interest groups or academic residential communities (FIGARCs) and students living in conventional residence halls: the former had a marginally lower frequency of illness than did the latter. I also observed two unexpected differences between these groups. Students living in FIGARCs had marginally higher body mass index and a marginally lesser tendency to seek resources. The latter of these differences obtained only when using Stephens and colleagues' (2014) three-item measurement, not the expanded 20-item measurement. I caution against putting much emphasis on these results, generally; however, the resource seeking difference may deserve a bit more scrutiny, which I address below. For the most part, not much makes students living in conventional residence halls different from those

94

living in FIGARCs upon matriculation or at the end of the transitional year. The lack of differences at the end of the transitional year is not consistent with the literature on living-learning programs (Inkelas & Weisman, 2003; Inkelas, et al., 2007; Jaffee et al., 2008; Pike, 1999; Pike, Schroeder, & Berry, 1997; Soldner et al., 2012; Stassen, 2003) on which I based my hypotheses.

In the present research, any kind of living on campus appears to foster some aspects of student wellness and success, as I found several differences when students living off-campus were involved in the comparisons, some of which are more trustworthy than others. Compared to students living off-campus, students living in FIGARCs were significantly more satisfied with life, were significantly less stressed, were significantly more engaged in extracurricular activities, had a marginally greater tendency to seek resources (on both the original and expanded measurements), and had a significantly higher sense of social-belonging. Compared to students living off-campus, students living in conventional residence halls were significantly more satisfied with life, had unexpectedly lower self-esteem (significant when one-tailed, marginal when two-tailed), were significantly more engaged in extracurricular activities, had significantly greater tendency to seek resources (on the expanded measurement only), and had a significantly higher sense of social-belonging.

There were three inconsistencies between the two sets of differences: self-esteem, stress, and resource seeking (within the original measurement and between the original and expanded measurements). The self-esteem difference was in the opposite direction of that predicted – the prediction was that students living in conventional residence halls would have higher self-esteem. Substantively, it is not clear why students living in conventional residence halls would have had lower self-esteem compared to students living off-campus at the end of the transitional year, especially considering that the former were more satisfied with life, felt a greater sense of social-belonging, and were not sadder or more stressed. Logically, however, it makes no sense that students living in conventional residence halls did not differ from students living in FIGARCs,

who did not differ on self-esteem from students living off-campus, but students living in conventional residence halls differed from students living off-campus. The sizes and directions of the coefficients do not line up either (refer back to Table 16). Descriptively, students living in FIGARCs had lower self-esteem than did students living in conventional residence halls (b = -0.41) and students living off-campus had higher self-esteem than did students living in conventional residence halls (b = 0.84). Pausing there, this implies that students living in conventional residence halls should be somewhere between the other two groups on self-esteem (FIGARC < CRH < OC), which further implies that the difference between students living off-campus and students living in FIGARCs should be *larger* than the difference between them and students living in conventional residence halls. However, that coefficient is 3.5 times *smaller* (b = 0.24). Relatedly, students living in FIGARCs were less stressed than were students living off-campus, as predicted, but were not any more or less stressed than were students living in conventional residence halls. Yet students living in conventional residence halls and students living off-campus did not differ on stress either. This also makes no sense, logically. Referring back to Table 16 again, the coefficients are virtually zero in comparing students living in conventional residence halls to students living in FIGARCs (b = -0.04) and to students living off-campus (b = -0.03), which implies that students living in FIGARCs should not differ from those living off-campus. Yet, somehow they do (b = 0.38).

These strange results could be a function of matching and analyzing groups separately or could be due to poor precision of estimation and may be worth exploring to those ends. I think perhaps the likeliest explanation may hinge on matching pairs of groups separately *and* matching with replacement. For the time being, these results are simply not trustworthy. The original measurement of resource seeking suffered from the same issues afflicting analyses involving self-esteem and stress. The coefficients for the three comparisons (Table 18) were all of approximately the same magnitude in the same direction (bs were -0.10, -0.10, and -0.12), which for the same reasons described above, makes no sense. However, comparisons on the expanded

measurement did make sense and they were in the predicted direction. Students living off-campus had a lesser tendency to seek resources than did both groups of students living on campus, which did not differ from each other. Why exactly the different measurements do different things for the same group comparisons may be a matter worth exploring. In the present set of analyses, the expanded measurement seems more trustworthy than does the original measurement for making inferences.

The most trustworthy results in these analyses appear to be those where there was overlap between comparisons of students living off-campus to students living in conventional residence halls and to students living in FIGARCs. Supporting the idea that living on campus fosters wellness and success, I found four differences between students living on campus and those living off-campus. Two of those differences were that students living on campus engaged in more extracurricular activities and sought more resources than did students living off-campus. These may well be predicated simply on opportunity. The activities and resources are right there, on campus, where students live. Ready access to such activities and resources is part of what makes the experience of living on campus different than the experience of living off-campus. Thus, living on campus may be influencing what students do (i.e., promoting engagement in extracurricular activities and taking advantage of campus resources).

More remarkable perhaps, insofar as not being simply a function of opportunity, is that students living on campus were more satisfied with life and had a higher sense of social-belonging compared to students living off-campus. Belonging was the strongest effect of all, and it also had the one of the richest imputation models (tied with self-esteem) and one of the better propensity-score models. The imputation and propensity-score models for life satisfaction were also quite good, though propensity-score matching was not quite as successful as it was for social-belonging. Although matching went reasonably well, imputation models for extracurricular activity and resource seeking were not as rich, which should reduce confidence in the inferences drawn from the differences observed. In particular, the estimates of the differences may not be

97

quite as trustworthy. However, given that matching of groups was reasonably successful for comparisons on all four outcomes, it may also be reasonable to claim causality. That is, although it is always possible that other confounding variables exist and were omitted from models because they were not measured, several very relevant variables were considered, selected, entered, and balanced, which increases the plausibility that the link between exposure to groups and the variables that determine that exposure has at least been reduced if not broken, making the treatment and control units interchangeable.

The strongest case for that claim is clearly for social-belonging; matching was excellent, as was imputation. Unfortunately, it is still not clear where the action of the effect is. Is there something about living on campus that *increases* students' sense of social-belonging, or is there something about living off-campus that *decreases* it? For example, does the intentional community-building of Residence Life programming increase sense of social-belonging among students living on campus, or do particular challenges to finding social fit off-campus decrease sense of social-belonging among students living off-campus? What is clear, is that the experiences of living on campus and off-campus are different. It is not just that the people who choose to live on campus vs. off-campus are different; they *are* different in several ways and these characteristics were well-balanced by propensity-score matching. More than people being different, the *experiences* are different and the experiences generate robust differences in students' of sense of social-belonging.

Limitations

First and foremost among the limitations of this study, the sample size was smaller than planned, even initially (158 students participated in workshops, 94 of whom were considered participants of the study, compared to 168 in Stephens et al.), and by the end of the year, it was even smaller in terms of survey participation. Consequently, null results in the present research are not diagnostic of anything, despite information recovery through multiple imputation. Many may well be false negatives, and this is more of a problem for analyses comparing intervention

and control groups than it is for those comparing residential groups. Of the non-null results, some are more trustworthy than others. Results regarding achievement are among the most trustworthy, because first-year GPA data were mostly complete. If students consented to releasing educational records, there were data, and if there were missing data otherwise, that meant the students dropped out before they could accumulate any points (i.e., they attempted no GPA hours for which they could accumulate GPA points). There were only two such cases among students who were considered participants. However, in testing the difference-education intervention, very few significant and marginally significant results were observed from a relatively large number of tests. So, these positive results, including the condition by generation interaction on GPA, are entirely within the bounds of statistically significant effects that may have occurred by chance. Therefore, I cannot conclude whether the difference-education intervention failed to replicate or truly did something theoretically unexpected.

Conditional indirect effects models that had GPA as the outcome are not as trustworthy, given the missing data on intervening variables and the fact that no additional auxiliary variables were used to aid in information recovery. However, these models may be more trustworthy than the belonging-adversity contingency models in which GPA was not entered. For survey-measured, end-of-year outcomes, analyses based on some imputation models are more trustworthy than those based on other imputation models, and the same applies to propensity-score models. Generally, the psychological wellness variables and social-belonging had the best imputation models and some of them were among the best propensity-score models, though there were certainly some red flags regarding residential group comparisons on self-esteem and stress.

Additional limitations pertain specifically to propensity-score matching of residential groups. Although propensity-score matching of residential groups increases the plausibility that the observed differences between residential groups are causal, this must be tempered by the possibility that there are variables not used in propensity-score matching that may still be confounded with residential group differences. Also, as noted in the Method, a close-to-ideal

99

method is direct or exact matching, because it guarantees very minimal distances between propensity scores of subjects matched one-to-one, which improves the plausibility of the idea that the subjects are interchangeable, which is a necessity for causal inference. Nearest neighbor matching was used, which matches units with the *most similar* propensity scores, meaning there is variability in distances between propensity scores. Matching was also conducted with replacement, meaning it was many-to-one not one-to-one. That is, a given control unit could be matched with several treatment units. This, combined with the fact that groups were matched separately for the three comparisons made on each outcome, may be the best explanation for the nonsensical results regarding self-esteem, stress, and resource seeking (original measurement). To the extent that a given control unit provided a good match to several treatment units and was thereby used multiple times and that control unit had an aberrant score on the outcome variable, the more that unit is used the more could have an undue influence on the result of comparing the groups on the outcome. When the processing is done separately for each comparison, it could produce an impossible set of results in terms of repeated comparisons. An additional limitation to the methods I used was I did not conduct a typical test like the average treatment effect or the average treatment effect on the treated (Ho et al., 2007; Oakes & Johnson, 2006). Rather, I used sample weights produced by the matching process in regressions after multiple imputation. The methods used are, therefore, not ideal, but they are better than simple, uncontrolled analyses wherein confounders abound.

As for the intervention itself, original procedures were followed as closely as possible, but it was not executed perfectly. Group size of attendees varied considerably, to the point of canceling two sessions on account of lack of attendance. One of the panelists was absent for one of the days of workshops; that day also had the lowest attendance. In addition, the putative manipulation check did not "work." There were no condition differences on the two categories coded from attendees' video testimonials. A substantial portion of attendees did mention something along the lines of "feeling normal/not alone" (49% in difference-education, 39% in

standard-control). However, very few attendees mentioned something to the effect of "backgrounds influence experiences" (11% in difference-education, 12% in standard-control), which could be indicative of an implementation defect or could be indicative of an issue with coding-scheme interpretation and application. Judging by panelists' responses to questions (Appendix F), the core framework appears intact. If that assessment is accurate, then an implementation defect would be in the details tangential to the core framework, and if the intervention is that sensitive to implementation details, it is not a terribly useful or scalable solution, especially not for implementation by nonspecialists. Scalability is also questionable in that the endeavor was costly, in terms of time and money, and was a fairly difficult event to coordinate, especially with limited resources and competing heavy demands for students' time and attention during the first week of classes. Apart from making the workshop mandatory or building it into orientation activities, it may not be feasible at scale, and even at smaller scales, smaller administrative units or under-resourced programs seeking to implement something of this nature may consider less costly alternatives.

Future Research

The limitations themselves deserve some future exploration. In particular, the nonsensical results from residential group comparisons on self-esteem, stress, and the original measurement of resource seeking are perplexing. It is not immediately clear why they turned out as they did, but they call for explanation, though one explanation does seem to stand out (i.e., that matching groups separately *with replacement* may have odd effects on results). Similarly, the frustrating inconsistencies in structural modeling results, in addition to nonpositive definite matrices of some growth models, may warrant further tinkering with model specifications, though this may very well be a fool's errand given the sample size.

Beyond the limitations, these data are very useful and interesting. There is much in them yet to be explored. Most of the variables described in the Psychological Mechanisms section of the Method were used only instrumentally in the present research. Their utility awaits

101

exploration. I centered this research on social-belonging, in particular, for theoretical reasons relevant to testing the intervention. However, academic-belonging (Lewis & Hodges, 2015) may provide a fruitful path for exploration. Students in all of the bridge program conditions had lower anticipated academic-belonging at baseline compared to nonbridge-program students, whereas no baseline differences whatsoever were observed on social-belonging. Perhaps treatments shift academic-belonging in ways that improve achievement and persistence. Moreover, residential groups differed on anticipated academic-belonging at baseline in the stair-step pattern I expected but did not find for the end-of-year outcomes examined (i.e., off-campus < conventional residence hall < FIGARC), whereas students living on campus generally had a greater anticipated sense of social-belonging than did students living off-campus. It may be interesting to see whether the stair-step pattern holds for end-of-year outcomes or whether academic-belonging might help explain other differences. To that point, the field is wide open for exploring where the action is in the differences observed between residential groups. This is of particular practical import, because living-learning programs like freshman interest groups and academic residential communities are supposed to generate more positive outcomes than are more traditional, conventional residence hall communities (Inkelas & Weisman, 2003; Inkelas, et al., 2007; Jaffee et al., 2008; Pike, 1999; Pike, Schroeder, & Berry, 1997; Soldner et al., 2012; Stassen, 2003). Why they do not appear to be doing so at the University of Oregon deserves exploration and further testing, as they are costly programs to maintain.

As for the experimental portion, it is unclear whether it would be useful to repeat the intervention for the next cohort at the University of Oregon, given that The College Transition Collaborative (n.d.) – the nonprofit organization mentioned in the introduction, which works with college administrations in implementing such interventions – will be implementing mindset interventions, likely including the social-belonging intervention (Walton & G. L. Cohen, 2011), on a large scale at the University of Oregon during the upcoming academic year. In any event, replicating Stephens and colleagues' (2014) difference-education intervention and perhaps

102

comparing it to other interventions (e.g., those of the mindset variety), specifically in bridge programs generally seems warranted, because it and the mindset interventions, for that matter, may not do what we think they should do within that context. We should attempt to know what they do and do not do in that context.

The intervention portion of the present research was an independent replication (and extension) of a study published in a high-impact journal, and it was a strong test of an application of social-psychological theory in the field. In an attempt to know what the application did in the context for which it was ostensibly designed, I took Stephens and colleagues' (2014) difference-education intervention and implemented it within a bridge program. Independent replication such as this is fundamentally and generally important for the accumulation of knowledge (Campbell & Stanley, 1963; Easley, Madden, & Dunn, 2000; Epstein, 1980; Johnson, 2002; Klein et al., 2014; Lee, Kuo, Whitmore, & Sklar, 2000; Nosek, Spies, & Motyl, 2012; Open Science Collaboration, 2012, 2015; Robinson & Levin, 1997; Thompson, 1996), and it is *essential* for applications in the field, where there are real-world consequences. Critically testing such applications and the theories on which they are constructed is also essential, because recommending an application to practitioners is not usually followed by practitioners experimentally testing it to see if it works for them in their context. For example, upon hearing of the merits of the difference-education intervention, a bridge program administrator would most likely take that one condition, the difference-education condition, and insert it into their program wholesale. This is not an unreasonable thing to do, given that it is reportedly the efficacious part of the application and they are unlikely to be social-scientists trained in methods needed to test it.

However, without direct evidence that it is what their program lacks, it would be an ill-advised thing to do, because it may not have the effect they were told it should have. For example, as appears to have happened in the present research, the difference-education treatment may have a psychological effect (e.g., reduced belonging-adversity contingency), but not a behavioral effect (e.g., increased resource seeking), at least not as measured, and fail to result in

higher achievement (e.g., increased GPA). Or, as appears to have also happened in the present research, the control treatment may have a behavioral effect resulting in higher achievement and no psychological effect, at least not as measured. Or, the treatments may affect the opposite group of people they theoretically should have affected (e.g., continuing-generation students). Or further, the treatments may not affect anything at all. This is an expensive endeavor with an uncertain return. Such expenditure of time, energy, and money in a vacuum of unknown might as well be wasted time, energy, and money, which is ethically dubious (Button et al., 2013).

Practical Advice and Recommendations

Advice to administrators. Given the results of the present research, I would suggest to the researchers from the College Transition Collaborative and the University of Oregon administrators collaborating with them that they pay close attention to effects of their interventions on students in the bridge program and students living off-campus. The interventions may not do what they are supposed to do in the context of the bridge program. In contrast, students who choose to live off-campus may have a particular need for such treatments, perhaps especially their social-belonging intervention (Walton & G. L. Cohen, 2011).

Enrollment Management may benefit from considering some of the wave 0 survey-measured variables that had non-negligible zero-order relationships with achievement and persistence end-of-year outcomes for potential controls of pre-enrollment/baseline characteristics or for research in their own right. In particular, study time, which is a single item ("In an average week, about how much time, rounded to the nearest half-hour, did you spend studying outside of class in high school?") had the largest baseline association with achievement (i.e., cumulative first-year GPA), though it was still small ($r = .26$). Need to belong had the largest baseline association with persistence to the second year (i.e., enrollment in fall of the second year), though it was also small ($r = .23$). It was measured using a ten-item scale (e.g., "I need to feel that there are people I can turn to in times of need", Appendix B), which could feasibly be reduced to a

104

short form based on factor loadings to be added to the freshman survey to curtail increasing the burden on participants.

Preexisting differences between students in the bridge program and those not in the bridge program and between students living in conventional residence halls and those living in FIGARCs (Appendix J, see also Appendix G) may be of use to administrators of the bridge program and of University Housing, respectively, in considering future programming and assessments thereof. The differences pertinent to the bridge program are broken up by conditions within the bridge program. They are still interpretable in that state, but could be aggregated for a clearer reference. The differences pertinent to Housing may over-represent students in the bridge program. However, because students in the bridge program tended to live off-campus in higher proportions, the results may not change much with resampling to more accurately represent bridge program participation. Additionally, the end-of-year outcome analyses are unlikely to be affected by that sampling fact, given that I used bridge program participation, among other attributes that make the groups different, as a matching variable where appropriate. For both sets of administrators, it may be reasonable for the particular purpose at hand to re-include data from students who were not considered participants in the present research because they completed only the wave 0 survey. This may be similarly useful for evaluation of baseline predictors pertinent to Enrollment Management described above.

During the time I conducted this research and presently, I have worked as a full-time employee in Student Life, particularly in Assessment and Research thereof. My role exists because there is increasing demand to demonstrate the value of student services, to show evidence that the programs implemented contribute positively to student wellness and success. The present research, in addition to partially fulfilling the requirements of my doctoral program, has value for my professional work in Student Life. It also contains an important lesson: well-powered experiments are the way to infer causation and thereby provide evidence of efficacy. I am ultimately unable to infer much at all from the experiment I conducted, because the sample

size was small. So, power matters. And although propensity-score matching of residential groups was reasonably successful – a strategy I used to increase the plausibility that the observed differences are causal – that success must always be tempered by the possibility that there are still omitted variables confounding group differences. So, random assignment matters.

I have noticed that many of my colleagues in Student Life appear to have an aversion to experimentation. To be sure, there are various practical reasons why, in some cases, exposure to "treatments" or implementation of "conditions" are not randomly assignable (e.g., policy change, legal compliance, groups are necessarily self-selected like the residential ones studied here, etc.). However, random assignment is not itself unethical. Having a control group is not tantamount to withholding treatment; in fact, only in a very few cases is it that, all of which are a subset of cases confined to the context of delivering medical and mental health services. Doing things that may reasonably result in harm to a person or persons is unethical, for fairly self-evident reasons. So, we should avoid doing things that could be harmful. More subtly, however, doing costly things of unknown efficacy with insufficient means to test efficacy is also unethical, because it is wasteful of resources (for discussion of how wasting resources is unethical, see Button et al., 2013). Promoting programs of unknown efficacy is also unethical, because the claims are baseless; they are, in effect, misinformation.

Universities implement programs all the time, sometimes even mandating participation. Not unreasonably, we become saturated with programs, because we care about supporting students. Almost none of the programs might reasonably result in harm. Almost all of them are promoted as efficacious with little to no supporting evidence. Part of my job is ostensibly to muster such evidence. There appear to be no qualms about collecting data; we may, in fact, have too much data, another potential waste of resources. Having data per se is not the problem. Having the right kinds of data is a massive problem. We are data rich and information poor, and we are in this state largely due to issues of design. I would be much more effective at my job (i.e., I could muster much more evidence of efficacy) if my hands were not perpetually tied by

106

nonexperimental, and usually cross-sectional, designs. If we truly want to know whether our programs positively contribute to student wellness and success, we need experimental and longitudinal designs coupled with thoughtful measurements.

In most cases, experimental work would also not be terribly difficult to do. Fundamentally, this is literally all that would be involved: Instead of implementing one program wholesale, implement more than one program and randomly assign the delivery of them. That is all an experiment really is. In context, it is actually easier than this. For the most part, new implementations are really alterations of or updates to existing programs. Here, generation of a second, third, or n^{th} program, which could be consuming, is not even necessary. Simply randomly assign the delivery of the business-as-usual version (control) and the altered/updated version (intervention). Alterations and updates are typically multifaceted. So, to really get at the action of the effect, which is a matter of increasing efficiency, break the alteration/update down to its component elements and deliver multiple alterations/updates to determine where the action is. This answers the question of which part or parts of the program are efficacious and which are not.

Relatedly, there are complementary principles for structuring conditions so that more precise causal inferences can be drawn, but that is why more empirically and theoretically trained researchers such as me should be employed by administrative units of universities. The bottom line is that the scientific enterprise did not make the strides that it has by avoiding random assignment, and we are not going to make many strides in improving student services if we continue to avoid it.

Advice to researchers. If you are considering doing research that has stakes for a bridge program, a living-learning program, or one of the myriad programs that comprise the university microcosm, talk to the administrator(s) of the program(s) early and often, learn about what they do from conversations with them, and if your project is longitudinal, maintain good relationships along the way. This seems like common-sense advice, and it is, but I think it warrants emphasis, because it is very important. A less obvious set of stakeholders to include are units of Enrollment

Management (e.g., Registrar, Admissions, Financial Aid). If your research involves access to students prior to matriculation and/or access to student records data, talk to these administrators early in your planning.

At least at the University of Oregon, Enrollment Management guards access to students before they are students (i.e., prior to matriculation). They are rightfully protective of sharing contact information for students who are still *prospective* at that point, because at least in part, excessive solicitation may be sufficiently intrusive to sway students' decisions toward attending another university. If your project involves contacting students at this point in time, your project has a better chance of succeeding if it can also serve the interests of Enrollment Management, and they will be the judges of whether or not it does. Regardless of the point at which you plan on contacting students, if you plan on using student records in your research, you should talk to Enrollment Management administrators early and often. They are the keepers of and the experts on those data; they can advise what kinds of data are and are not available to you, as well as what particular variables represent (e.g., generation status is not as straightforward or as widely standardized as one might like to think). Their input could potentially change (and improve) the research plan.

Plan ahead for purchasing and distribution of any compensation for participation. The current study was unlike the typical human subjects pool arrangement between introductory psychology courses and psychology laboratories, in which students are required to participate in *n* hours of research studies. Subject pool set-ups often facilitate participation by providing a web portal designed for students to enroll in studies and for researchers to provide them credit for participation. In contrast, non-subject pool students who participate in non-class related research are not beholden to participate; it is up to the researchers to convince prospective participants to do them a favor. Some students are quite unflustered by delays in remuneration produced by complications encountered by the researchers. However, others are not as forgiving, even if delays are purely unintentional. If at all possible, automating the process of remuneration is

108

recommended. It is not always possible, but if gift codes can be automatically distributed at the end of an online survey, for example, it will not only make life easier, but it will also make participants happier.

Relatedly, make sure that the intended research plan is well-funded. I am extremely grateful for the funding that I did have and to the many generous people who contributed to that funding, but it was frankly not enough for the research I was trying to do. Stephens and colleagues (2014) had an excellent retention rate between intervention and end-of-year survey (87.5%), probably partly because they were able to give each participant $70 and partly because their study's burden on participants was at least 40% lighter than that of mine. I did what I could to stitch together a decent-sized purse. The end result ($14,736.76) was nothing to scoff at. It nonetheless did not match the project's ambition and that is something I now regret.

Aside from financial cost, the coordination and implementation of the intervention, in particular, was costly in terms of time and effort. The process involved recruiting and selecting student panelists, coordinating panelists' and moderators' schedules, working with panelists to prepare their responses to discussion questions, scheduling workshop sessions for and communicating with prospective participants, reserving appropriate space to hold the workshops – and in my case, this involved finding someone to sponsor it, because space isn't free – preparing materials for a day's workshop sessions, supervising and helping facilitate workshops, uploading video testimonials from camcorders after a day's workshops, coding video testimonials, and entering data from the paper-and-pencil post-intervention questionnaires. I would not do this again and do not recommend that anyone else do it without a small army of research assistants, and I would especially not attempt to scale up without such an army.

The need to follow up on results from testing the intervention. To the extent that a main effects (of condition) model is plausible, the group sizes in the present research should be sufficient to have detected effects. Stephens and colleagues' findings – that there were main effects of condition on most variables except resource seeking and GPA, where condition by

109

generation status interactions emerged – suggest that such a model is at least partially plausible. Contextualizing the theory underpinning the intervention within the bridge program further suggests that there may be no condition by generation interactions at all. However, the interactive effect on GPA observed in the present research suggests otherwise. To reiterate a previous point, the cell sizes after parsing condition by generation status were small, and so null results are not diagnostic of anything. There is also nothing among the analyses reported in the present research, and possibly even nothing in the data, that can argue against a conclusion that the treatments (difference-education or standard-control) had no effects whatsoever. That is, the very few significant and marginally significant results observed from a relatively large number of tests are entirely within the bounds of chance. Therefore, I cannot conclude whether the difference-education intervention failed to replicate or did something theoretically unexpected. Nevertheless, the GPA results raise important interrelated questions for practices of the bridge program and reproducibility of the intervention effects.

First, is the difference on GPA between first-generation students and continuing generation students in the bridge program real? This can be answered using student records from the entire 2015-16 bridge program cohort. If there is not a population-level difference, then the gap observed between them in the program-passive-control condition is likely due to sampling error. And if mean GPA is also around 3.0, then a question arises for the intervention: did the difference-education treatment *attenuate* continuing-generation students' achievement? Replication is the only way to test this. If there *is* a population-level difference, then questions arise for both the intervention and the bridge program. For the intervention, did the standard-control treatment *boost* continuing-generation students' achievement? Again, replication is the only way to test this. For the bridge program, did the same difference emerge in previous cohorts? If not, what differed, if anything, in programming for the 2015-16 cohort? Further, is there a similar difference in previous cohorts for their 2nd, 3rd, n^{th} years? If not, or if it diminishes to a negligible gap over time, perhaps the first-year difference does not matter in practical terms. If

110

there are appreciable differences in later years, is there anything in the data from the present

research, student records, or future research that could explain the gap?

Baseline differences between on-campus and off-campus students matter. For me,

one of the most fascinating parts of preprocessing and exploratory data analysis in the present

research was observing how much and in what ways students who chose to live on campus

differed from those who chose to live off-campus. The popular wisdom on that matter, that

students who live on campus vs. off-campus are different, is not wrong, though I doubt popular

wisdom could accurately identify all of the ways in which they do and do not differ. Of course,

my data do not identify *all* of the ways in which they do and do not differ, but they do identify

several ways. In particular, students living off-campus were, at baseline, less satisfied with life,

sadder, more stressed, less healthy, lonelier, and less self-assured. They also felt like they would

not fit in as well (both socially and academically), had less social support, and felt greater

stereotype threat. In addition, they were of lower socioeconomic status and were more likely to be

in the bridge program.

Understanding how those differences shape students' experiences transitioning to college

and/or how experiences transitioning to college might reshape some of those differences would

be interesting to pursue in its own right, but could also be useful for informing decisions about

how to best support students' transition. This is especially important as the University of Oregon

prepares to implement, in the 2017-18 academic year, a requirement that first-time, full-time

students live on campus their first year. An important question is, then: will the live-on

requirement help, hurt, or do nothing for these students' wellness and success? Unfortunately, this

is an example of something for which random assignment is impossible, but assessment of it is

nonetheless crucial.

The upcoming academic year (2016-17) provides an opportunity to replicate the baseline

differences observed in the present research between students living on campus and those living

off-campus, and to extend the research to assess the live-on requirement in the following years. I

111

recommend continuing to collect baseline data from large samples of incoming students starting with the 2016-17 cohort; following up to collect end-of-year outcome data from each cohort in the spring quarter of their first year and of subsequent years; and following up with the 2015-16 cohort, represented in the present research, at the end of their second year and of subsequent years.

In the 2016-17 baseline data, I propose developing latent classes of students, using variables on which on-campus and off-campus students in the present research differed at baseline. To the extent that these latent classes align with known classes (i.e., residential groups), they will provide a basis for grouping students in the 2017-18 cohort, who will not be able to choose whether they live on campus or off-campus, notwithstanding some exceptions. This is an inferential leap, but it may be a valid one if evidence shows that latent classes align with known classes in the 2016-17 cohort. If trajectories of known classes before implementation and latent classes after implementation differ, that would also be very useful information. At best, this recommended research will allow assessing the live-on requirement in terms of students who *would have chosen* to live on campus vs. off-campus, and at worst, it will allow assessment of whether students who come to the university worse-off have a trajectory that is worse still, one that improves, or one that stays the same. Whatever the case, this is a beneficial, informative, and actionable endeavor.

Conclusions

The difference-education intervention did not do what it was theorized to do by attempting to fit to the data Stephens and colleagues' (2014) framework, a more charitable version of that framework contextualized in the bridge program, or the related framework of Walton and G. L. Cohen (2011). This may be largely attributable to the fact that the intervention was implemented within the bridge program, but that is the point. If the purpose and utility of the intervention rests on the premise that bridge programs, which provide academic and financial support, are insufficient for closing achievement gaps, and that the students need psychological

112

support too, then an appropriate test of such an intervention is implementing it within a bridge program. If such a test yields theoretically unexpected results, the unexpected results need additional evidence including but not limited to replication, and the theory may need revision.

The results of residential group comparisons, which used propensity-score matching to attempt to reduce the biasing influence of confounders, were more positive. This is the first study of which I am aware to use propensity-score matching for investigating residential groups at a local level (i.e., what little published research that exists uses multi-institutional data: Padgett, Salisbury, An, & Pascarella, 2013; Pascarella, Salisbury, Blaich, 2013; Shudde, 2011). Given that dearth, there are no agreed upon standards for what variables should feed into propensity-score models in the college residence context. The positive results of the present research show that it is a very promising assessment tool, and suggest that there is much to be explored and tested. To be sure, there were some disappointing results as well. I am quite sure that practitioners would like to see that their residential programing results in greater achievement and persistence, and that living-learning programs do something over and above more traditional residential programs. However, it is nonetheless encouraging that living on campus seems to foster a greater sense of social-belonging, and may also facilitate life satisfaction, extracurricular activity, and resource seeking, because those results suggest that the full "college experience" is best realized when students live on campus during their transitional year.

APPENDIX A

REPLICATION, ALTERATION, AND EXTENSION

The basic core of the procedure used by Stephens et al. (2014) to deliver the intervention

messages was directly replicated. Eight upper-year students acted as panelists in a set of panel-

discussion workshops by sharing their real-life stories in response to the question prompts

developed by Stephens et al. for the difference-education condition and the standard-control

condition. One moderator introduced the panel and workshop activities. Another moderator posed

questions to the panelists. Panelists responded with prepared stories. After panel-discussion,

attendees (i.e., incoming students) were asked to create a video "testimonial" articulating the

message of the panel discussion and were given the same instructions and prompts for the

exercise as were used by Stephens et al. After the video testimonial exercise, attendees were

asked to respond with paper and pencil to follow-up items similar to those used by Stephens et al.

The following differences in procedure and materials applied:

- Group size of attendees ranged from 5 to 23 (Stephens et al. reported 20 to 25; my preregistration on Open Science Framework [https://osf.io/n4t75/?view_only=28d11f3bbf9449e491eb536d948e2206] described 15 to 40).
- Consent to participate required participation in the video testimonial exercise. This was a soft requirement, as not all students participated in the exercise.
- To improve ease of understanding and congruence between items and response scales the following alterations to Stephens and colleagues' follow-up questionnaire were made:
- Items assessing the usefulness of the information presented in the student panel and participants' liking of panelists were changed from interrogatives to declarative statements and attached to a 7-point agree/disagree scale with the following anchors: *strongly disagree*, *moderately disagree*, *slightly disagree*, *neutral*, *slightly agree*, *moderately agree*, and *strongly agree*.
- The two items assessing participants' experience with the video testimonials remained the same, but they were attached to the following 5-point scale, the lowest category of which differs slightly between the two items: *none* (*not*) *at all*, *a little bit*, *a moderate amount*, *quite a bit*, *and a great deal*.

Stephens and colleagues' study was extended in several important ways, all of which were

meant to improve upon the methodology of original study and rigorously test the claims made by

the study's authors. Most of the extensions pertained to issues of measurement and analysis, but

the first one listed pertained to the intervention in particular.

- Stephens et al. claim that the financial and academic advising support provided to disadvantaged groups of students by typical "bridge" programs is insufficient for reducing achievement gaps between them and more advantaged groups of students, arguing that disadvantaged students also need psychological support. Indeed, this is the justification for doing the kind of psychological intervention that these researchers did. An appropriate test of this claim, then, would be to implement it within such a program. Hence, the present research implemented the intervention within a bridge program.
- Stephens and colleagues' measured their proposed mediator, tendency to seek college resources (resource seeking; e.g., visiting the writing center), at the end of the growth period under examination (i.e., the end of the transitional first-year at university), along with the focal outcome, cumulative first-year GPA. This design feature meant asking students to recall the number of times they sought resources over the entire academic year. The present research used a multi-wave longitudinal design, measuring resource seeking on a shorter, one-month period of recall for each wave rather than the original recall of the entire preceding year.
- In measuring resource seeking, Stephens et al. included only three resources about which students were asked to recall. The time frames of recall were also different for different items. One item asked: "Since you started at [university name], in a typical month, how many times have you emailed a professor to ask a question?" The other two items asked: "Since you started at [university name], how many times have you met with a professor outside of class?" and "Please estimate how many times you have gone to the writing center since you started at [university name]." (p. 10 Supplemental Material). Additionally, the items were measured on a count scale starting at 0 and truncated at 5. The present research used an expanded, 20-item measurement of resource seeking, including the original three items adapted to the local context, using non-truncated frequency estimates for responses to items (i.e., students were asked to report a numeric value rather than respond to a truncated scale).
- Stephens et al. reported a mediated moderation model where resource seeking mediated the interaction between generation status and treatment condition affecting GPA. However, their model appears to have been specified as GPA regressed on resource seeking and the treatment by generation interaction term and resource seeking regressed on the interaction term. That is, it appears that treatment condition and generation status were not included in the model, only their interaction term was. To correct this potential model misspecification, both treatment condition and generation status were entered into the model along with their interaction. Additionally, my preregistration specified estimation of a conditional indirect effects growth model testing whether the indirect effect of generation status on growth/change in GPA through growth in resource seeking is conditioned on treatment (intervention vs. control), specifically focusing on moderation of the first path of the indirect effect (i.e., generation $\rightarrow$ resource seeking). The model actually estimated was a path model, which is more similar to what Stephens et al. did.
- Building on the conditional indirect effects growth model described above, additional models were estimated in an effort to synthesize the theoretical claims about mechanism made by Stephens et al. with those made by Walton and G. L. Cohen (2011).
- Stephens et al. included measurements of a host of "psychosocial constructs" (e.g., social-belonging) in their end-of-year survey to assess "quality of students' college transition." Many were the same or similar constructs measured by Walton and G. L.

Cohen (2011). The present research employed the same tactic, measuring the same or similar constructs in very similar ways, in the same spirit, with the same goals in mind, but measurements did ultimately differ somewhat.

- Stephens et al. used MANCOVA to analyze the "psychosocial constructs" above, but outcomes of interest should differ between conditions in quite predictable ways and I am not aware of a good reason to think that covariation among outcomes needed to be included in a model. As such, univariate multiple regressions were used to test hypotheses rather than MANCOVA.

MEASUREMENT ITEMS

Post-Intervention Questionnaire

The original items used by Stephens et al. (2014) were interrogative (e.g., "Did you learn from this experience?"). Items 1-8 were changed to declarative statements and attached to the following anchors (1-7): *strongly disagree*, *moderately disagree*, *slightly disagree*, *neutral*, *slightly agree*, *moderately agree*, *strongly agree*. Items 9 and 10 remained the same, but were attached to the following anchors: *none at all* (*not at all* for 10), *a little bit*, *a moderate amount*, *quite a bit*, *a great deal*.

1. I think the information provided by the panelists will be useful to me.

2. I enjoyed hearing other [bridge program] students' stories.

3. I learned from this experience.

4. I feel better prepared for my transition to the UO.

5. I would recommend the program to my peers.

6. I would recommend the program to future incoming students.

7. I liked the students in the panel.

8. I feel like I could relate to the panelists.

9. How much effort did you put into your video testimonial?

10. How much did you enjoy creating the video testimonial?

Infrequency Subscale of the Attentive Response Scale

Anchors (0-4): *not at all true*, *a little true*, *somewhat true*, *mostly true*, *very true*

1. I don't like getting speeding tickets.

2. It feels good to be appreciated.

3. I'd rather be hated than loved.

4. I enjoy the music of Marlene Sandersfield.

5. My favorite subject is agronomy.

6. I don't like being ridiculed or humiliated.

Block 1: Psychological Wellness

Life Satisfaction Scale

Anchors (1-7): *strongly disagree*, *moderately disagree*, *slightly disagree*, *neutral*, *slightly agree*,

moderately agree, *strongly agree*

1. In most ways, my life is close to my ideal.

2. The conditions of my life are excellent.

3. I am satisfied with my life.

4. So far, I have gotten the important things I want in life.

5. If I could live my life over, I would change almost nothing.

Single Items Self-Esteem Scale

Anchors (1-7): *strongly disagree*, *moderately disagree*, *slightly disagree*, *neutral*, *slightly agree*,

moderately agree, *strongly agree*

1. I have high self-esteem.

Sadness Subscale of the Positive and Negative Affect Schedule

Anchors (1-5): *very slightly or not at all*, *a little*, *moderately*, *quite a bit*, *extremely*

Prompt: Indicate to what extent you have felt this way during the past month.

1. sad

2. blue

3. downhearted

4. alone

5. lonely

Perceived Stress Scale Short-Form

(R) indicates reverse-scored item. Anchors (1-5): *never*, *almost never*, *sometimes*, *fairly often*,

very often

Stem: In the last month, how often have you felt…

1. …that you were unable to control the important things in your life?

2. …confident about your ability to handle your personal problems? (R)

3. …that things were going your way? (R)

4. …difficulties were piling up so high that you could not overcome them?

Block 2: Physical Wellness

General Health Portion of Medical Outcomes Survey Short-Form

(R) indicates reverse-scored item. Anchors (1-5): *definitely false*, *mostly false*, *don't know*, *mostly*

true, *definitely true*

1. I seem to get sick a little easier than other people. (R)

2. I am as healthy as anybody I know.

3. I expect my health to get worse. (R)

4. My health is excellent.

Frequency of Illness

Numeric Response

Stem: In the past month, how many times did you… (Please enter a number.)

1. …go to the doctor for mental health reasons.

2. …go to the doctor for <u>physical health</u> reasons.

3. …get sick (e.g., caught a cold or flu) but did not go to the doctor.

<u>Body Mass Index</u>

Numeric Response

1. What is your height in feet and inches (e.g., 5 feet, 6 inches)? (Please enter a number.)

2. What is your weight in pounds? (Please enter a number.)

<u>Exercise</u>

Response Options: 0, 1, 2, 3, 4, 5, 6, 7

Stem: On how many of the last 7 days did you…

1. …do moderate intensity cardio or aerobic exercise (e.g., walking briskly) for at least 30 minutes?

2. …do vigorous intensity cardio or aerobic exercise (e.g., running, swimming) for at least 20 minutes?

3. …do 8-10 strength training exercises (e.g., weightlifting, push-ups, crunches) for 8-12 repetitions each?

Block 3: Success (Engagement) and Other Followup Questions

<u>Academic Enjoyment</u>

Anchors (1-7): *strongly disagree*, *moderately disagree*, *slightly disagree*, *neutral*, *slightly agree*, *moderately agree*, *strongly agree*

Wave 0

1. I took courses in high school that allowed me to study what truly interests me.

2. I found topics that I was excited and passionate about in my studies during high school.

3. I enjoyed learning new things and getting excited about ideas in my classes during high school.

4. I enjoyed talking about course material with my friends outside of class during high school.

Waves 1-3

1. I am taking courses this quarter that allow me to study what truly interests me.

2. I have found topics that I am excited and passionate about in my studies this term.

3. I am enjoying learning new things and get excited about ideas in my classes this quarter.

4. I am enjoying talking about course material with my friends outside of class this term.

Extracurricular Activity

Response Options: yes, no

Wave 0

Stem: In high school, did you…

1. …join a student group or club (e.g., academic, cultural, political, religious, etc.)?

2. …run for student office?

3. …do any volunteer work on- or off-campus?

4. …join an off-campus religious group?

5. …join a sports team?

6. …participate in intramural sports?

Waves 1-3

Stem: This term, have you…

1. …joined a fraternity or sorority?

2. …joined an ASUO student group (e.g., academic, cultural, political, religious, etc.)? If "yes,"

which group or groups?

3. …run for student office?

4. …done any volunteer work on- or off-campus?

5. …joined an off-campus religious group?

6. …joined a club sports team?

7. …participated in intramural sports?

8. …participated in events specific to your residence hall?

9. …participated in other events on campus?

If students responded "yes" to any of these, they were presented with the following:

"Thinking about all of the things on the previous page that you indicated doing, how much time in an average week, rounded to the nearest half hour, did you spend on these activities all together?" Responses were made using a dropdown menu of options from 0 to 20+ in half-hour increments.

If students responded "yes" to joining a student group, they were presented with the following:

"On the previous page, you indicated that you joined a student group or club. What kind of group(s) or club(s)? (Please select all that apply.)" Response options were: Academic, Cultural, Environmental, Faith-Based/Religious, International, LGBTQA, Media, Multicultural, Performing, Political, Professional, Recreational, Service Learning, Student Government, and Other.

Wave 0

Did you have a job during high school?

Waves 1-3

Are you employed?

If students responded "yes" in any wave, they were presented with the following: How many hours a week did you work? (Please enter a number.)" and "What motivated you to get the job? (Select all that apply.)" The latter had the following response options: money to pay living expenses; extra money, not for living expenses; experience to add to your résumé; for fun; and other (please specify).

If students responded "yes" in waves 1-3, they were also presented with the following: "Do you work on-campus for the UO or somewhere off-campus?" Response options were: on-campus, off-campus, and both.

Resource Seeking

The first three items comprise the original measurement used by Stephens et al. (2014).

Numeric Response

Stem: In the past month, how many times have you... Please enter a number. Enter 0 if you have not done the thing described.

1. …emailed a professor to ask a question?

2. …met with a professor outside of class?

3. …gone to the Writing Lab at the Teaching and Learning Center?

4. …asked a question or offered a comment in class?

5. …attended an instructor's regular office hours?

6. …gone to a tutoring session through the Teaching and Learning Center for help in a class?

7. …attended a workshop through Student Support Services (e.g., "Stress Reduction Sampler" or "Math Anxiety")?

8. …met with a study group outside of class?

9. …attended a review session before a test?

10. …asked for a meeting with an academic advisor?

11. …talked with a Faculty Fellow at a residence hall event, during their office hours, or otherwise?

12. …sought personal support from an instructor or administrative staff?

13. …sought support from the Veteran Center?

14. …sought support from the Women's Center?

15. …sought support from the Office of the Dean of Students?

16. …sought support from the Center for Multicultural Academic Excellence?

17. …gone to the Mills International Center?

18. …gone the Health Center?

19. …gone to the University Counseling and Testing Center?

20. …gone to the Career Center?

Study Time

Responses were made using a dropdown menu of options from 0 to 20+ in half-hour increments.

Wave 0

1. In an average week, about how much time, rounded to the nearest half-hour, did you spend studying outside of class in high school?

Waves 1-3

1. In an average week, about how much time, rounded to the nearest half-hour, do you spend studying outside of class?

Hometown

Wave 0 only

1. Where is your hometown? (Please type the town/city you consider home. Indicate which state if inside the US and which country if outside the US.)

2. state (if inside the U.S.) (dropdown menu of US states and territories and District of Columbia)

3. country (if outside of the U.S.) (dropdown menu of countries, which apparently did not include Taiwan as a student from Taiwan informed me in their text response to "Where is your hometown?")

<u>Residence</u>

Wave 0

1. Will you live on-campus or off-campus? (Response options: on-campus, off-campus)

2. Why did you choose to live where you plan to live? (Text Response)

Waves 1-3

1. Do you live on-campus or off-campus? (Response options: on-campus, off-campus)

2. Why did you choose to live where you live? (Text Response)

Block 4: Psychological Mechanisms

<u>Social-Belonging</u>

(R) indicates reverse-scored item. Anchors (1-7): *strongly disagree*, *moderately disagree*, *slightly disagree*, *neutral*, *slightly agree*, *moderately agree*, *strongly agree*

Wave 0

1. I feel confident that I will belong at college.

2. I sometimes feel that people at college will not accept me. (R)

3. I worry that I will be an outsider at college. (R)

Waves 1-3

1. I feel confident that I belong at the UO.

2. I sometimes feel that people at the UO do not accept me. (R)

3. I worry that I am an outsider at the UO. (R)

<u>Academic-Belonging</u>

(R) indicates reverse-scored item. Anchors (1-7): *strongly disagree*, *moderately disagree*, *slightly disagree*, *neutral*, *slightly agree*, *moderately agree*, *strongly agree*

Wave 0

1. I sometimes feel like other students going into college have skills that I don't have. (R)

2. I'm not sure that I'm cut out for college. (R)

3. I feel similar to the kinds of people who have what it takes to succeed in college.

4. I'm not certain I'll fit in intellectually at college. (R)

Waves 1-3

I sometimes feel like other students at the UO have skills that I don't have. (R)

I'm not sure that I'm cut out for the UO. (R)

I feel similar to the kinds of people who have what it takes to succeed at the UO.

I'm not certain I fit in intellectually at the UO. (R)

Need to Belong Scale

(R) indicates reverse-scored item. Anchors (1-7): *strongly disagree*, *moderately disagree*, *slightly disagree*, *neutral*, *slightly agree*, *moderately agree*, *strongly agree*

Wave 0 only

1. If other people don't seem to accept me, I don't let it bother me. (R)

2. I try hard not to do things that will make other people avoid or reject me.

3. I seldom worry about whether other people care about me. (R)

4. I need to feel that there are people I can turn to in times of need.

5. I want other people to accept me.

6. I do not like being alone.

7. Being apart from my friends for long periods of time does not bother me. (R)

8. I have a strong need to belong.

9. It bothers me a great deal when I am not included in other people's plans.

10. My feelings are easily hurt when I feel that others do not accept me.

Three-Item Loneliness Scale

Anchors (1-4): *never*, *rarely*, *sometimes*, *often*

1. How often do you feel that you lack companionship?

2. How often do you feel left out?

3. How often do you feel isolated from others?

Appraisal Subscale of the Interpersonal Support Evaluation List Short-Form

(R) indicates reverse-scored item. Anchors (1-4): *definitely false*, *probably false*, *probably true*,

definitely true

1. I feel that there is no one I can share my most private worries and fears with. (R)

2. There is someone I can turn to for advice about handling problems with my family.

3. When I need suggestions on how to deal with a personal problem, I know someone I can turn

to.

4. If a family crisis arose, it would be difficult to find someone who could give me good advice

about how to handle it. (R)

Belonging Subscale of the Interpersonal Support Evaluation List Short-Form

(R) indicates reverse-scored item. Anchors (1-4): *definitely false*, *probably false*, *probably true*,

definitely true

1. If I wanted to go on a trip for a day (for example, to the country or mountains), I would have a

hard time finding someone to go with me. (R)

2. If I decide one afternoon that I would like to go to a movie that evening, I could easily find

someone to go with me.

3. I don't often get invited to do things with others. (R)

4. If I wanted to have lunch with someone, I could easily find someone to join me.

Tangible Subscale of the Interpersonal Support Evaluation List

(R) indicates reverse-scored item. Anchors (1-4): *definitely false*, *probably false*, *probably true*, *definitely true*

Items from the short-form:

1. If I was stranded 10 miles from home, there is someone I could call who could come and get me.

2. If I needed some help in moving to a new house or apartment, I would have a hard time finding someone to help me. (R)

Items from the college student long form:

1. I know someone at school or in town who would bring my meals to my room or apartment if I were sick.

2. I don't know anyone who would give me some old furniture if I moved into my own apartment. (R)

Self-Efficacy

Anchors (1-7): *strongly disagree*, *moderately disagree*, *slightly disagree*, *neutral*, *slightly agree*, *moderately agree*, *strongly agree*

1. I am confident that I can earn a 'B' or better grade in all my courses this year.

2. I am confident that I can complete my undergraduate degree.

3. I am confident that I can employment after I graduate.

Self-Assurance Subscale of the Positive and Negative Affect Schedule

Anchors (1-5): *very slightly or not at all*, *a little*, *moderately*, *quite a bit*, *extremely*

Prompt: Indicate to what extent you have felt this way during the past month.

1. proud

2. strong

3. confident

4. bold

5. daring

6. fearless

Stereotype Threat

Anchors (1-7): *strongly disagree*, *moderately disagree*, *slightly disagree*, *neutral*, *slightly agree*, *moderately agree*, *strongly agree*

1. At the UO, I worry that people will draw conclusions about people like me based on my performances.

2. At the UO, I worry that people will draw conclusions about people like me, based on the performances of other people with similar identities.

3. At the UO, I worry that people will draw conclusions about me, based on what they think about people with backgrounds like mine.

4. At the UO, I worry that people will draw conclusions about me, based on the performances of other people like me.

Block 5: Religiosity and Socioeconomic Status

Religiosity

Anchors (1-7): *never*, *less than once a month*, *once a month*, *several times a month*, *weekly*, *several times a week*, *daily*

Note: *several times a week* was misrepresented as *several times a month* in wave 0 and part of wave 1.

How frequently do you take part in religious services?

How frequently do you pray and/or meditate?

Family Income

Wave 0 only

Response options (1-8): between $0 & $14,999; between $15,000 & $24,999; between $25,000 & $34,999; between $35,000 & $49,999; between $50,000 & $74,999; between $75,000 & $99,999; between $100,000 & $199,999; more than $200,000

Although you do not need to be exact, what is your best guess as to your family's income in an average year?

Parental Education

Wave 0 only

Response options: did not finish high school; high school diploma, GED; some college experience, but did not finish; 2-year technical /Associate's degree; 4-year college/university degree; graduate degree (Masters, Doctorate, Law); don't know/not applicable

1. What is the highest level of education attained by your mother?

2. What is the highest level of education attained by your father?

Subjective Social Status Scale

Wave 0 only: family

Imagine that this ladder represents how society is set up. At the top of the ladder are the people who are the best off – they have the most money, the highest amount of schooling, and the jobs that bring the most respect. At the bottom are the people who are the worst off – they have the least money, little or no education, no job or jobs that no one wants or respects.
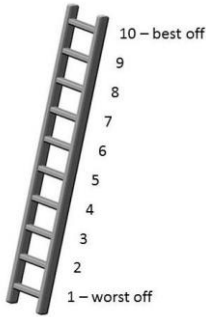
Now think about your family. Please tell us where you think your family would be on this ladder. Select the number on the scale below that corresponds to the rung that best represents where your family would be on this ladder.

Wave 0: self

Instructions: Now assume that the ladder is a way of picturing the students at your high school. At the **top** of the ladder are the students at your high school with the most respect, the highest grades, and the highest standing. At the **bottom** are the students who no one respects, no one wants to hang around with, and have the worst grades.
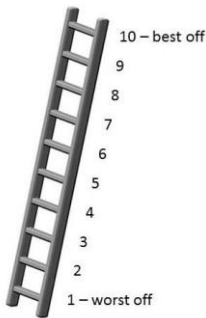
Where would you place yourself on this ladder? Select the number on the scale below that corresponds to the rung that best represents where you would be on this ladder.

Waves 1-3: Self

Instructions: Assume that this ladder is a way of picturing the first-year students at the UO. At the **top** of the ladder are the students with the most respect, the highest grades, and the highest standing. At the **bottom** are the students who no one respects, no one wants to hang around with, and have the worst grades.

131

10 – best off
9
8
7
6
5
4
3
2
1 – worst off
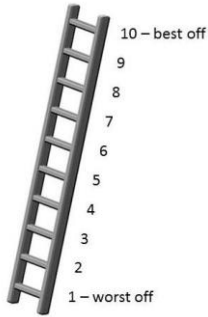
Where would you place yourself on this ladder? Select the number on the scale below that corresponds to the rung that best represents where you would be on this ladder.

Block 6: Affective Experience

Positive Affective Experiences

Wave 0

1. Please list four events that occurred this summer that made you feel **positively** about your summer experience and rate how positive and important each event was. (4 text boxes provided for each experience labeled experience 1, experience 2, experience 3, experience 4)

Waves 1-3

1. Please list four events that occurred this term that made you feel **positively** about your experience here at the UO and rate how positive and important each event was.

2. How positive were these experiences?

Students responded to the labels above using the following anchors: *neutral*, *slightly positive*, *moderately positive*, *very positive*

3. How important were these experiences?

Students responded to the labels above using the following anchors: *not at all important*, *slightly important*, *moderately important*, *very important*

Negative Affective Experiences

Wave 0

1. Please list four events that occurred this summer that made you feel **negatively** about your summer experience and rate how negative and important each event was. (4 text boxes provided for each experience labeled experience 1, experience 2, experience 3, experience 4)

Waves 1-3

Please list four events that occurred this term that made you feel **negatively** about your experience here at the UO and rate how negative and important each event was.

2. How positive were these experiences?

Students responded to the labels above using the following anchors: *neutral*, *slightly negative*, *moderately negative*, *very negative*

3. How important were these experiences?

Students responded to the labels above using the following anchors: *not at all important*, *slightly important*, *moderately important*, *very important*

APPENDIX C

BODY MASS INDEX DATA INTERPRETATIONS

Note: Each bullet pertains to entries of an individual student.

- Entries of 5 and 73 and 5 and 74 for feet and inches in waves 0 and 1 and then 5 and 9 in wave 2 and 5 and 8.9 in wave 3. Because 9 inches is .75 feet, Interpreted the wave 0 and wave 1 entries as 5.73 and 5.74 feet and converted 73 and 74 to 9, even though they are technically 8.76 and 8.88.
- Entries 5.18 for feet and 62.2 for inches in wave 1, which are both about 5'2" in feet and inches, which is what was entered for waves 0, 2, and 3. I converted 5.18 and 62.2 to 5 and 2.
- Entry of 1606 for pounds in wave 1, 140 in wave 0, 145 in wave 2, and 150 in wave 3. Treated BMI as missing for wave 1.
- Entries of 5.5 and 65 for feet and inches in wave 0, 5'5 and 65 in wave 1, and 5'6 and 66 in waves 2 and 3. 66 inches is exactly 5'5". Converted inches to 5 for all waves.
- Entries of 5.52 and 66 for feet and inches in wave 0, data were missing for wave 1, and 5 and 6 in waves 2 and 3. 66 inches is 5'5". Converted feet and inches to 5 and 5 for wave 0.
- Entries of 5'3 and 63 for feet and inches in wave 0, 5 and 3 in wave 1, 5'3 and 63 in wave 2, and data were missing in wave 3. Converted feet and inches to 5 and 3 for waves 0 and 2.
- Entries of 5'4" and 64" for feet and inches in wave 0, 5 and 4 in wave 1, 5 and 5 in wave 2, and data were missing for wave 3. Converted feet and inches to 5 and 4 for wave 0. However, there was no entry for pounds in wave 0, but there were in waves 1 and 2, between which there was an increase of 10lbs. Treated BMI as missing for wave 0.
- Entries of 5'7 and 67 for feet and inches in wave 0, and 5 and 7 in waves 1, 2, and 3. Converted feet and inches to 5 and 7 for wave 0.
- Entries of 5'8 and 68 for feet and inches in wave 0, and 5 and 8 in waves 1, 2, and 3. Converted feet and inches to 5 and 8 for wave 0.
- Entries of 5 and 4 for feet and inches in waves 0 and 2, 5'4 and 0 in wave 1, and data were missing for wave 3. Converted feet and inches to 5 and 4 for wave 1.
- Entries of 5 and 8 feet and inches (height) in wave 0, 7 and 8 in wave 2, and data were missing for waves 1 and 3. Converted 7 feet to 5 for wave 2.

INTERNAL CONSISTENCES OF SCALE ITEMS AND BETWEEN-WAVE

CORRELATIONS OF COMPOSITES FOR AUXILIARY CONSTRUCTS

Construct	Wave	α	SD	n Items	n Students	Between-Wave rs		
Academic-Belonging	0	.75	.10	4	335	wave 0	wave 1	wave 2
	1	.71	.12	4	305	.63		
	2	.74	.14	4	273	.57	.67	
	3	.73	.13	4	227	.55	.69	.72
Loneliness	0	.84	.02	3	333	wave 0	wave 1	wave 2
	1	.86	.04	3	307	.61		
	2	.88	.03	3	273	.65	.63	
	3	.87	.04	3	226	.59	.58	.69
Social Support: Appraisal	0	.74	.07	4	335	wave 0	wave 1	wave 2
	1	.79	.09	4	306	.48		
	2	.77	.10	4	273	.48	.58	
	3	.70	.14	4	227	.46	.53	.67
Social Support: Belonging	0	.84	.08	4	333	wave 0	wave 1	wave 2
	1	.83	.11	4	307	.63		
	2	.76	.13	4	273	.55	.68	
	3	.72	.11	4	227	.52	.56	.67
Social Support: Tangible	0	.65	.02	4	336	wave 0	wave 1	wave 2
	1	.68	.09	4	306	.61		
	2	.71	.07	4	272	.56	.67	
	3	.71	.12	4	227	.51	.57	.60
Self-Assurance	0	.88	.08	6	334	wave 0	wave 1	wave 2
	1	.90	.09	6	306	.63		
	2	.91	.07	6	273	.52	.58	
	3	.92	.07	6	226	.53	.58	.64
Self-Efficacy	0	.71	.09	3	335	wave 0	wave 1	wave 2
	1	.67	.13	3	306	.50		
	2	.68	.07	3	272	.46	.53	
	3	.60	.11	3	225	.54	.56	.69
Stereotype Threat	0	.88	.10	4	335	wave 0	wave 1	wave 2
	1	.89	.08	4	306	.58		
	2	.88	.11	4	273	.56	.65	
	3	.90	.09	4	228	.49	.53	.65
Need to Belong	0	.78	.14	10	333			
	0	.78	.14	10	333			
Religiosity	0	.76		2	329	wave 0	wave 1	wave 2
	1	.73		2	302	.88		
	2	.77		2	270	.84	.87	
	3	.82		2	223	.84	.87	.89

WORKSHOP FACILIATION SCRIPT

The script followed that of Stephens et al. (2014) with the exception of added or deleted content

[noted in brackets].

Welcome Introduction

Welcome everyone and welcome to the UO. We appreciate your participation in the College Transition Project, and hope that today's experience will be valuable for your transition to college. In this session, you will get to hear the stories and experiences of your peers. They were once first-years too, and look forward to sharing their perspective with you. There will be six questions addressed to the student panel today. Each of the speakers has prepared some thoughts and remarks to share with you. Our panel moderator, [student name], is going to ask the questions. First, the speakers will go around the room and introduce themselves. Then, they will answer a series of questions about their experiences at the UO. Finally, we will ask you to complete a short activity and then a brief survey about what you learned. Now it's time for the speakers to introduce themselves. They will start by saying their name, year, major, and where they are from.

Panelist Introductions

<Panelists introduce themselves.>

Interlude

Difference-Education Condition:

The speakers are excited to have you here and to share their stories with you. Students come from very different backgrounds before arriving at the UO. These differences make the UO an amazing place to be.

Standard Condition:

The speakers are excited to have you here and to share their stories with you. Students' interests span a wide range of topics and areas of study. These differences make the UO an amazing place to be.

Panel Discussion

<Moderator poses question prompts and panelists respond in turn.>

Video Testimonial

Now it's time for a short video activity. [content deleted: "Keep in mind that it's your choice to participate in this next activity, but it's highly encouraged."] We want to hear from you and learn about your perspective. [content added: After that, we'd like you to complete a short two-page survey about the workshop.]

<Distribute video prompt sheets and camcorders.>

While distributing…

<For groups of 20 or smaller> [content added: We're passing out video prompt sheets that provide instructions for the activity and prompts to help you think about what you want to say with space to jot down notes about what you're thinking. We're also passing out video camcorders to each of you along with instructions on how to operate them. When you're finished with the video activity, please come to the front to return your camcorder and video prompt sheet. Then, we'll give you the short follow-up survey, which will be the last activity for the workshop.]

<For groups larger than 20> [content added: We're passing out video prompt sheets that provide instructions for the activity and prompts to help you think about what you want to say with space to jot down notes about what you're thinking. We're also passing out video camcorders to each of you along with instructions on how to operate them. We have only 20 camcorders and there are more than 20 of you, so pair up with a neighbor and share. One of you can hold the camcorder for the other and then switch or you can do it all yourself and pass it along, whatever you like. Just make sure you each make your own video. When you're finished with the video activity, please come to the front to return your camcorder and video prompt sheet. If you're sharing a camcorder with a neighbor, one of you bring both of your video prompt sheets along with the camera. Then, we'll give you the short follow-up survey, which will be the last activity for the workshop.]>

Follow-up Survey

<Distribute follow-up surveys.>

APPENDIX F

PANELISTS' RESPONSES TO DISCUSSION QUESTIONS

Content has been edited to redact information about real people, places, and things.

Difference-Education Condition

Q1: People come to college for many different reasons. What did coming to college mean to you?

First-Generation

P1: Coming to college meant fulfilling many dreams that my family and I have always dreamed of. It meant that I was going to experience something new and exciting that my family had never gone through. As a first generation college student in my family, I knew coming to college would open a lot of new opportunities for me. I knew that college was going to give me many experiences I had not had before, but one of the most exciting things to me was the fact that I had the freedom to choose the classes that I was interested in. It makes you engage in learning so much more!

P2: College was my ticket out of a very traditional family who did not fit into the American way of life. Being an American citizen I was eager to be thrown into a crowd of people with different lifestyles and mindsets. It was an opportunity for me to learn new things as I challenged myself academically. College to me meant that I would have the opportunity to earn a degree and have the chance to live comfortably with a career rather than a dead end job while experiencing diversity in many situations first handedly.

P3: It was very important for me to go to college. My parents immigrated here and did not go to college and I see them working hard to provide for my family. I chose to go to college so I do not end up working a job that I did not enjoy. I want a career in something that I find interesting and enjoy. Therefore, going to college means the world to me because it allows me to get one step further into achieving my dreams.

P4: For me, coming to college meant starting the next chapter of my life. While I was growing up, there was never a question of if I was going college, but it also brought about a lot of uncertainty. Coming to college meant trying to figure my life out, and just building a solid foundation for my future career.

P5: To me coming to college was coming to my dream college. I have dreamt of coming to Eugene since I was 8 years old. I started running and participating in the [proper noun] track meet since I was little and I didn't think anything of it until I came and competed at the Historic Hayward Field. I fell in love with the atmosphere and the people and I knew that this was the college that I was going to come and pursue my career at. Once I was here I realized what it really meant to live on your own. My parents weren't as supportive as most parents were since I was the first in my family to go to college but I learned a lot from that. I became very independent and I loved it, I learned that relying on yourself was very important and that being responsible was just as important.

<u>Continuing-Generation</u>

P6: Coming to college meant something new. It's a once in a lifetime experience and it is a place where people can shine. There is something about being on your own that makes you feel like you are invincible before reality sets in. I realized that I was given a big responsibility in taking control of my life and finally being in the driver's seat.

P7: For me, coming to college meant I would be facing a lot of new experiences and challenges. I would have a whole new group of friends, live on my own for the first time, and basically start a whole new life. I didn't know anyone when I got to college, so I had to make all new friends. I started working at two new jobs, joined a sorority, and got involved with clubs and events on campus. I was also living on my own for the first time and having all of these new experiences. Ultimately, coming to college meant my life was going to be completely changed (in a good way!)

P8: To me, college was starting my life. College, in my head, was a place for me to have the time of my life and work hard to change the world in my own way and make a name for myself. It was expected of me to go to college by my family but I was really excited to create a path for myself and set myself up for a great future.

Q2: Students can have a wide variety of experiences when they transition to college and come from many different backgrounds. Thinking back, what was the transition to the UO like for you?

<u>First-Generation</u>

P1: As a first generation college student, it was both exciting and scary. I did not know what to expect, who to reach out to, how I was going to make friends and so on. However, I was also happy and excited because I knew that college was going to open many doors for myself and my family. The first month was all about getting to know people and getting adjusted into the college life. One of my best resources, who until this day I consider a good friend, was my dorm RA. She helped me transition so well from high school into college. It was a scary experience but totally worth it!

P2: Exciting is how I would like to describe my transition to the UO. I spent a lot of time daydreaming about how my life would be in the next four years at the university. As a senior in high school, I was eager to move on to the next step in my life and see what opportunities would open up for me in the future. I was excited to be the first in my family to go to college, meet new people and experience new things. Being a first generation college student was stressful, however it has helped shape me to become the person that I am today. I felt as if I was going into the whole college process blind. Seeking out the best college, application process, finding scholarships and filling out FAFSA were all processes that no one in my family were familiar with. This pushed me to take on the responsibility to make sure everything needed to be done was done right. I stepped out of my comfort zone and asked advisers and teachers for help and spent sleepless nights researching. Eventually I got the hang of it and now I realize that stressful situations can bring out the best of you in the end, and I am very much enjoying my life here at the university. Living in the dorms easily exposed me to new people. My hall mates come from states that I never thought I would ever get a feel for, but meeting people from those states has given me a little taste. I have learned to understand different people's points of view and developed a broader mind. In addition I have met many adults who are caring and supportive which aids me academically and keeps me on the right track. The thought of experiencing college

life was very exciting for me. Back at home I felt stuck. Everybody always did the same thing and didn't really strive to become better, so I was excited to be a part of the UO community where professors, faculty and students all strive to the best to their abilities. I have had a lot of fun being here at UO, but I have also grown and developed in many ways.

P3: The transition to the UO was rough for me at first because I have never been away from home by myself before. I did not know anyone and I felt very alone since I was so closed off. My roommate and I got along, but we did not become good friends. Only when I started opening up to making friends and experience everything that this university offered did I fall in love with this place and everything about it. From then on, I adapted and found my place being here. So originally, it felt like it was a rough transition, but in honesty, it was a great transition.

P4: For me, the transition to the University of Oregon was a smooth but frightening one. I've lived in Springfield, Oregon my entire life, and I've known that the UO would be the college of my choice for a while. I was confident in the school, but I was uncertain about how the difficulty of college compared to the difficulty of high school. I also made the decision to live in the dorms, which was an experience to remember, but took some planning. Moving into the dorms, figuring out how financial aid worked, and just learning the requirements for my major were difficult tasks, but they were made profoundly simple by the help of advisers all across the UO campus.

P5: Personally my transition to UO was like a reality check. As a student who didn't have parents who went to college it was a scary experience having to do things on my own without their guidance. I realized real quick that college was way different than high school but I did adjust. I believe that my experience became much better after meeting with my [bridge program] advisor [name] because he made me feel at ease and less anxious about all the changes that were going on. I felt as if there was someone here at the University who cared about my success.

Continuing-Generation

R6: The transition to the University of Oregon was a major challenge for me. Never before I had really been on my own. Though my parents taught me how to be independent, my parents were always there if I needed them. This time, however, I was on my own and starting anew. It was exciting but scary. I didn't know what to expect being on my own. Having to manage my own time, my finances, my education, everything now was entirely in my hands. That was the scary part, having that much responsibility. However, it was exciting as this was a new chapter in my life. During the week of welcome, I went and did almost everything I could. I knew that if I wanted to start off college strong, I needed to get involved. Back in high school, I was shy. However, college was a time for change and I knew that in that first year, I was going to discover more about myself than I ever did in my entire lifetime.

R7: My transition to the University of Oregon was a moderate adjustment, it took some getting used to the dorms and to having a roommate. Classes in college were also much different than in high school, so that was also a transition. I had to get used to having such big classes and having midterms and finals count for so much of my grade. Overall I was excited for college and for all of the new experiences! The transition was not hard, just an adjustment.

R8: Transitioning to life at the University of Oregon was one of the most exciting, yet challenging experiences that I've ever endured. Coming to college, there was a lot of fear built up in my mind: this was the first time I had ever been away from home, I had to leave my mom at home all alone, I was unsure how I was going to be able to afford living at school, and I was not sure of how I would fit into the community here at the UO. However, getting involved quickly

and attending all of the kick-off events, sports games, and participating in free activities really helped me settle in fast. I made friends with people who shared common interests with me coming from similar or even way different backgrounds than my own. Having to be in charge of myself has been a great learning experience and the UO was a great place to do it at. There are plenty of resources and people to help you along the way, and I credit a lot of my successful transition to the UO to taking advantage of those opportunities.

Q3: Now we'd like you to share some specific challenges about coming to college. Can you provide an example of an obstacle that you faced when you came to the UO and how you resolved it?

First-Generation

P1: An obstacle that I faced when I came to college was finding financial support. The way I dealt with this was to get a work study job where it was flexible enough to work with my schedule. When I found out that the work-study offered college scholarships for students, I immediately applied. This year compared to my freshman year has been a piece of cake. It's all about knowing what options and resources you have available to you.

P2: Financial support was one of my biggest challenges that determined whether or not the UO was an option for me. Having divorced parents who both have incomes that lie between low income and middle income families made it a struggle because they made too much to get need-based grants but made too little to help me pay for college. With that it was my mission junior and senior year of high school to find scholarships and grants to help aid the financial struggle. After applying for so many scholarships, I was awarded the local [proper noun] Scholarship, the [proper noun] Scholarship, and the [proper noun] Grant. I continued to seek help to understand my Award Letter that was sent in by UO. I learned the difference between subsidized and unsubsidized loans from a guest college adviser that I met with once a week towards the end of my high school year. It was challenging because my parents weren't familiar with the process as well, so I had to clarify everything to understand myself and to explain to my parents. On top of this, I worked part time to help pay out of my own pocket as well. I was also [admitted into the bridge program], so once I got here I made frequent visits to the advisers to make sure that I was meeting the requirements to be eligible.

P3: I believe that my hardest challenge about coming to the UO was finding my place. There are so many different ways that one can fit in here, so I found it challenging finding my place here. When I first came, I did not know who to become friends with or confide in. I felt alone and very homesick. However, I realized that if I wanted to stay here and enjoy my time here, I had to at least try. I put myself out there more. I started chatting with people who lived in my residence hall and got to know them. Eventually, I became friends with them and my other classmates, as well, and started feeling comfortable and enjoying my time here.

P4: The biggest challenge, when it came to coming to college, was how I was going to be able to afford it. I did not want money to be an obstacle for me, but my parents are not the best with money and there was no way they'd be able to provide any financial assistance. To solve this problem I worked hard in high school and applied for every scholarship I heard about. In the end, I became a [bridge program] student, which was an amazing gift. With the help of [bridge program], the [proper noun] scholarship, and other scholarships, I am able to make my dream of obtaining a higher education a reality.

P5: A challenge that I faced when coming to UO was learning how to deal with my financial needs. I remember receiving an email stating that my UO account balance was available and at first I was confused so I went to [website] to check it out and I remember having this knot in my throat after seeing that my balance was over a thousand dollars. I didn't know what to do so I called my mom in a panic telling her that I owe all this money. She calmed me down a bit and then told me to go talk to financial aid or an advisor and that's exactly what I did. I talked to my now [bridge program] advisor [name], who helped me out and found that there was a charge on my account that should have been covered. I was very relieved and grateful that [name] was able to help me and assured me that everything would be okay.

Continuing-Generation

P6: One obstacle about coming to the University of Oregon was getting to know people. I was never a social person, or really the first one to start a conversation. I was always shy and quiet. I didn't know if I was going to get along with any of my residential hallmates or my roommate. My roommate was my best friend, and there have been stories that even best friends can begin to hate each other when you are roommates with one another. It was definitely outside of my comfort zone. However, I knew that if I would continue being shy and not get out there and make friends, I wouldn't benefit from it. I decided to step out of my comfort zone and started to make friends. That first week the hall was together, I organized a hall movie night, and we watched the popular film, "The Princess Bride." I knew from that night, how we all talked, and enjoyed the film, that I was going to get along with those people very well. They, in the end, became my best friends at the University of Oregon.

P7: For a while when I first came to college I really missed living at home with my Mom and being able to talk to her whenever I needed to. It was hard for me to not feel like I always had someone there for me. I dealt with this by calling her throughout the day whenever I felt like I needed to talk to her. This really helped me feel closer to her and also helped make my transition to college easier.

P8: When I came to the University of Oregon, not everything was a perfectly smooth transition, not everything was easy. A big problem that I faced when leaving home was being able to afford the expenses of attending school here. I was now off on my own and it was up to me to figure out how I was going to be capable of paying my bills each term so I could stay here at the UO. I applied for many jobs and work-studies through the University of Oregon [resource] Center in hopes that I could find a job that I could work well in and would help to pay for my schooling. However, after a lot of hard work and looking, I found a job off campus in Eugene that I enjoyed and used my paychecks to go towards my student bills. Another effective resource that helped me along the way of figuring out how I would pay for my expenses of living was located just in Oregon Hall. I could make visits into the Financial Aid Office or simply one of the cashiers and their advice and instructions really helped to guide me through the difficulties that I faced this first year when trying to work out my payments.

Q4: Did your decision to attend the UO affect your relationships with your friends and family at home? If yes, how?

First-Generation

P1: My decision of attending the UO did affect my family and friends. I do not necessarily see it in a negative way. It affected them in the sense that I am constantly missed since they were people I interacted with on a daily basis; however, at the same time they always tell me how

much they appreciate me and how proud they are of me succeeding with life. It makes me feel really good whenever I have the opportunity to go back home because it makes me feel extra appreciative and sometimes I even get extra presents.

P2: My decision to attend the UO affected my relationship with both my family and friends. My family didn't understand why I wanted to go out of the way to spend the extra money for an education when it would have cost less going to a community college. I had to spend much time explaining to my family that I was offered scholarships and grants that made the cost just a little more than community college and that the university offered a lot more opportunities. Even after giving my explanation my decision still didn't sit well with my family, because the majority of my family are legal immigrants who have lived a very frugal lifestyle to survive in America. Although it is not often that I go against my family, I took the risk to go against their thoughts and opinions to break out of the lifestyle that they lived by getting an education here at the UO where there are so many opportunities and so much support that the university offers. The majority of my friends are back at home attending community college, and because I decided to work a little harder and attend a university they feel that I am superior to them. It saddens me that my friends feel this way about me because they were the people I cherished the most after my family. Many times I encouraged my friends to strive to go to a university but they just didn't see the point in it. Every time I am home and when the topic of school comes up I get cut short of my words and opinions, jokes are made about how much smarter I am than they are. I chose to come to the university to better myself and to become my own person. All my life I felt like I conformed to my family and friends, so when there was an opportunity for me to get away and explore at the UO I didn't hesitate to make it work.

P3: My decision to come the UO did not really affect my relationships with my friends and family at home. However, it was difficult for my parents to accept me leaving home for college at first since I was the first child to leave and go off on my own. They eventually accepted and got used to it and my relationship with them is as strong as ever. My friends were very happy that I got accepted and decided to go to the UO. They are very supportive of me and encourage me to do my best here.

P4: Attending the UO did not affect my relationships with my friends or family at home. Although I seldom see friends from high school anymore, I'm the type of person that likes to keep in touch with old friends. I'm also so close to my home that I visit family and friends all the time. I also still make an effort to attend church, which is how I'm able to stay in touch with several of my friends and family. College is a busy time in everyone's life, and it can put a strain on any relationship, but I've worked hard to keep in touch with the people I've grown close to.

P5: I believe that the hardest part about deciding to come to UO was the fact that I wouldn't be as close in distance to my family and friends. I know that my family and friends are very proud of me for pursuing my dreams and also for achieving a scholarship to be able to attend this university. So, although we aren't as close in distance I know that I have their support and I believe I made the right choice by coming to UO.

Continuing-Generation

P6: Yes, of course. Every decision that is made has an impact on those around me. My father wanted me to go to a private school, or an Ivy League school, or [university name] or a school that was part of the [state university] system. This put the relationship with my father in a bit of a strain. He felt that the University of Oregon was a "party school" and only a school where it attracted mainly athletes. However, I told him on many occasions to give it a chance, which he did, and he in the end, he liked the school, much to my surprise.

P7: My decision to attend the UO definitely affected my relationships with my friends and family back home. All of my friends from home went to different colleges, and not getting to see them every day changed our relationships. We learned to call and text each other to stay in touch. It affected my relationship with my family too, because I was no longer able to see and talk to them every day. I got used to calling home and texting my family instead.

P8: When I began my college search, I already knew that I wanted to attend the UO. So, when I packed my bags in the Fall and headed off to college, no one was surprised that I was going down to Eugene; everyone knew from the beginning that the University of Oregon was going to be my new home. Although I had to leave my family, my boyfriend and many friends behind, this change allowed me to really grow on my own. I did lose connection with many people, and as that was hard, other relationships grew stronger. I also had new friendships at the UO that were the best source of comfort if I ever felt homesick. But, I rarely ever did feel homesick since I kept myself busy in Eugene and I only faced a 2 hour bus ride to get back home. My family lives just outside of [city], so attending the University of Oregon actually affected my relationships with family and close friends positively. I had the luxury of having my family and friends close by that they were able to come down and see my dorm, meet my friends, and spend sunny Eugene weekends with me.

Q5: What advice do you have for students with backgrounds similar to your own?

First-Generation

P1: As a first generation college student, my word of advice is to not be afraid to ask questions or for help! When I came here I was so afraid of asking any questions to anybody at all. I was so afraid of being judged; however, thanks to my RA, I lost that fear! She helped me out with resources when I had issues, I would ask professors or friends for help on certain things or I would simply go to my advisor for some help. All you have to remind yourself is, there is no such thing as a dumb question. If you have a question, there are probably 3 or 4 others who are wondering the same thing. Don't be afraid to ask! Your advisors, friends, RAs, professors, and GTFs are great resources!

P2: Ask questions and seek help when you need it. Every single human being around you can teach you something, you just don't know what it is until you start up a conversation and find out what their expertise is.

P3: My advice to them is to enjoy college, and life in general, despite the hardship and struggles that we have to go through. We are working hard to achieve our dreams, but we must not forget to live and enjoy life while we can.

P4: For students like me, I would encourage you to not let money be an obstacle. There are so many sources of financial aid and scholarships out there, but you must be willing to look. Living in the dorms, although expensive and not always pleasant, was a great decision. Studying abroad, although it can be expensive too, will also be one of the best decisions I've made. So I just want to advise all of you to seek out assistance when it comes to paying for school.

P5: For the students who are also first of their family to attend college it is a scary and an intimidating experience. My advice to those students is to come to college with an open mind and seek help from teachers, advisors and also check out the help centers that are available on campus. One experience that I had was, I was having trouble in one of my math courses and at first I would be nervous to go in and talk to my teacher during office hours. It's perfectly normal

to feel that way but just know that those people are there to help you do well and succeed in that class. I got the help that I needed and I also looked into other opportunities to help me in math such as the math, writing, and tutoring labs at the [resource] Center.

Continuing-Generation

P6: Persevere. College is a whole different experience. There are times that you may fail, and times where it just gets' tough. It may be completely out of your control and it may get overwhelming. My goal is to never give up, no matter what. I've learned a lot that you may get knocked down. Life will hit you hard, and there are times where you will be hit much harder than the others. But, every good fight needs a few good hits. The point being that you should never give up and just go for it. My goal in college is to get through it without any regrets, and I want to finish, saying that through the ups, the downs, the good times and the bad, I had a wonderful time.

P7: I advise students to make friends with people who live in their dorm, because it is so easy to meet people and find friends that way. I would suggest going to many of the events the school puts on throughout the year because they are a lot of fun and are good for meeting new people. I would also recommend going in to financial aid frequently so you have a good grasp on how financial aid and [bridge program] work, what loans to take out, and what you may be eligible for. Understanding my financial aid fully has been extremely beneficial for me during my college career.

P8: Be open to everything! Join clubs, meet people, make friends, get to know your professors, talk to your advisers. It's all about who you know, so getting to know your resources and communities around campus really pays off. Take every opportunity to get involved and you'll be surprised where it takes you.

Q6: What experiences that you had prior to starting at the UO prepared you to excel in ways that you wouldn't have anticipated at the time?

First-Generation

P1: Ever since I was little, my mom would talk about how she wanted us to get a great education. So, I have always had in mind since a young age that I was going to college. What prepared me for college the most was taking the [proper noun] program's classes. These classes give you a sense of how much work load you can expect in a college level class and how you should prepare and study for quizzes and exams. I was also part of a community group where we interacted with all sorts of people. I feel like this was also important because when you come to college you meet all sorts of people from different parts of the world!

P2: During my junior and senior year in high school I joined an organization called [proper noun]. [proper noun] provided the opportunities to learn about business with education in hand. Not only did I join the club, but I took the optional course that was offered at my high school as well. There I learned communications skills, how to create professional documents (resume, cover letters, etc.), and basic business practice and terminology. There were also opportunities to compete with our individual business projects on a regional, state and national level. Most of the projects required a presentation. Before [proper noun] I tried to avoid public speaking as much as I could, and I wasn't confident that my presentations would go well. I would cry in fear that I would let down my teammates and adviser who was very passionate about the program. However with a lot of practice, dedication and support and helpful criticism from my chapter adviser I was

able to place 1st and 2nd in my events and qualified for the national level. I went out and got to experience competing on the national level in [city], [state] and [city], [state] back to back each summer. Now presenting is something that I volunteer to do. It is an act that brings me much energy and joy knowing that it was once a fear that I have now overcome.

P3: The answer to this question would have to be that working hard in high school has prepared me to excel at the UO. I knew that I had to work hard to make it to the UO and get scholarships and all that stuff. However, little did I know that I was building my work ethic for college. My parents had not allowed me to go out or hang out with friends like most teenagers have the privilege of doing. Instead, I was at home studying and reading. My focus was all on school and making it to a good university. I was quite envious of the kids hanging out and having fun. However, I am glad that my parents had done this because it has allowed me to build a work regime that is up to par with attending a university. My first year at the university was not as challenging as I thought it would be because I knew how to focus on school and how to manage my time efficiently.

P4: One of the most memorable experiences in my life was having the opportunity in the seventh grade to be a [proper noun] Student Ambassador, traveling to [country] and [country] for nineteen days. At the time, I viewed this trip a simple opportunity to travel. I now realize that this life-changing experience has made me into an understanding young adult with a desire to meet people, experience new things, and be involved in all aspects of this college community. This travel experience helped me to realize my dreams for traveling, and has also helped push me towards studying abroad next spring. My desire to travel cannot be fulfilled without having a successful career, so this opportunity has indirectly fueled my desire to excel in school.

P5: In high school I was the varsity girls' soccer captain which provided me with leadership skills and it also helped me break out of my shell. Having the opportunity to be a leader and make decisions prepared me for the reality of college. Once you're in college there aren't going to be people who remind you about every little thing to do. You must take it upon yourself to have responsibility for assignments, tests and studying. Another thing that being a team captain taught me was independence. Once I came to college and got used to a routine I felt more confident and sure that I could do things on my own but also keeping in mind that if I needed help there were people who could guide me in the right direction.

Continuing-Generation

P6: Volunteering helped me, or at least, forced me to work with others to achieve a common goal. In college, I met people from all walks of life and from around the world. Some of these people I barely knew, and yet I was supposed to work with them in the classroom environment. Complete strangers that would eventually become good friends, and some, best friends. I grew close to some of the folks that I have "endured" hard classes with, only to realize that these classes only last 10 weeks, and I probably won't see them again. Another experience that prepared me was dance. I loved to dance, and that made me comfortable around large crowds of people. I found a hobby that I liked and my life at the University of Oregon wasn't all just school. It was about dancing and learning things other than what I had to learn in the classroom. With the resources that pointed me in the right direction, I formed my own crew and had one of the best rooms on campus to dance in, reserved for my crew and I to practice in.

P7: I went to a private high school that had a rigorous curriculum and played multiple sports. This prepared me to attend the UO because I was already busy in high school so I was used to being busy in college. I had no idea how busy I would be when I first arrived at school. I think being so

146

involved in high school really set me up for success because I was excited to get involved in college.

P8: My parents went through a tough divorce a few years back and although I did not realize it at the time, all of the hardships that I underwent prepared me to be a successful adult here at the UO. I was given responsibilities of myself and siblings, handling financial matters, and even being able to do simple tasks like my laundry or having good house-keeping practices. Being able to be independent, handle my own, and know where to go if I had any problems made me feel like I was thoroughly prepared to be an adult. Something I did just by my own nature is asking for help from others while I feel as if others around me were holding themselves back from being successful because they were reluctant to ask for help. Also, keeping a clean living space and respect in my dorm prevented any possible problems with my roommate.

Standard-Control Condition

Q1: Trace your path for finding your major.

First-Generation

P1: It all started out from actually sitting down and observing the news, not just watching them. I became very interested in everything and I always had questions on how the stuff worked. In high school I was part of a local community service group. One time our group got an opportunity to go to the Oregon Capital and talk on live television about teenage health in Oregon. It felt so great to know that I was letting others know about what was happening in the community and it gave me a taste of what it would feel like to inform the world of something they might not know yet.

P2: I haven't declared a major yet, but I'm thinking about declaring General Social Science with the concentration of Society, Crime and Law. I guess you could say that I just stumbled upon it. A lot of the general education classes and other classes that seemed interesting to me just happened to fall into that major.

P3: In high school, television and documentaries got me interested in forensic science. I became fascinated with human osteology and how bones can tell so much information about what had happened to an individual. So, I decided to major in anthropology with the hopes of becoming a forensic anthropologist. However, over the course of my first year at college, I found anthropology to be more than what I expected it to be and I wanted to focus more on science, so I declared biology as my second major. What I meant by anthropology being more than what I expected was that the field included so many other aspects to it, such as cultural and archaeological aspects, and while I do find some of that interesting, I wanted to focus on science, such as doing lab work, rather than social science, which led me to declare biology as my second major.

P4: The path for finding my major was not so simple. I'm currently double majoring in Geology and Economics. I applied to the UO with the intention of being a biochemistry major and looking towards medical school, which I soon decided I would not be able to handle all the things doctors must deal with. I knew I wanted a science major, but I wanted one that encompassed a lot of different sciences and wasn't too focused, which is exactly how Geology is. I chose geology without really looking into it much, but after taking several classes I haven't looked back. The economics major is a recent addition, which came when I realized I had a lot of free time, and enjoyed the economics classes I have taken so far.

P5: The path to finding my major went like this; I came to college wanting to be a Physical therapist. I soon found out that science wasn't my strong suit, I tried Chemistry and it was a real challenge for me. I passed the course but I didn't want to continue struggling and barely passing my classes. I decided to look into becoming a nutritionist so I took a biology course and once again I found that Bio wasn't for me either. I started to feel a little discouraged but I talked to [name], my [bridge program] advisor and we talked about my options and what classes did interest me. I took my first Psychology class in the winter and I really liked it. I was interested in what we were learning about the mind and brain and it really grew on me. I decided to continue and take my second Psychology class in the spring and it furthered my interest even more. About half way through spring term I decided to declare my major in Psychology and it was the best decision I made for my college career.

Continuing-Generation

P6: My major can be traced all the way back to when I first had my tonsils removed. I was scared. My parents, coming from a Vietnamese tradition, did not give me any comfort and for a nine-year-old going into surgery for the first time, you can't help but be frightened. I even lied to the doctors about my condition saying that I was fine, but it was all swept away when Dr. Richardson, the head surgeon for my operation came by to my room to say hi. He sat down next to me as Sponge-Bob was playing and he was saying how he always used to like this show, but being a doctor took a lot of time away from him. He personally wheeled me into the operating room and heated up a blanket and made sure I was comfortable. He made the promise to me that when it was all over and I woke up, he'd be there when I woke up. He held my hand as I went under. I remember waking up, and the first thing I see was Dr. Richardson, and he hands me a popsicle for me to suck on to ease the pain, but also a teddy bear that he personally bought from the gift shop in the lobby of the hospital. At that moment, I wanted to become a doctor. I admired that ability to spread love and compassion to those that are in need of help, and that you may not know that person, but you can still help them out.

P7: I originally came in to my freshman year as a Pre-Business major, but by spring term I knew it was probably not the right major for me. I am not very good with math, and I realized I might not be able to handle many of the math classes required for getting into the business school. Also, the business classes I had already taken had not interested me. I took my first journalism class in the winter term of my freshman year, and by fall term of my sophomore year I knew I wanted to be a Public Relations major, and switched to the journalism school. I am much happier and more interested in my journalism classes, so I know that it is the right major for me.

P8: At first, I was in the Journalism school, wanting to be an advertisement major. But after taking different classes and getting to see what I was more interested in, I declared myself as a Pre-Business Major and now I am working towards a double major degree in Finance and Advertising!

Q2: What were some of the experiences that led you to your major and what were some challenges?

First-Generation

P1: In high school, I got to be part of a workshop of a young Latina who was a news reporter. She explained her path and what she had to do to get to where she was now. It was very inspiring and I told myself that if she could do it, so could I. Also, since I love interacting with many different

kinds of people and learning about them, I decided to bundle my journalism major up with ethnic studies as my minor. I feel very satisfied with my decisions so far.

P2: <Undeclared. Did not respond.>

P3: I have always been interested in science. In high school, I was lucky enough to discover anthropology. I have always been good in the subject of biology and originally, I did consider majoring in biology. However, seeing as I was just entering college, I wanted to just focus on one subject, so I chose anthropology. As I was taking courses in anthropology at UO, I found that the field was not as science based as I would like. I did take into consideration that anthropology is a social science, so there are social and cultural aspects to it. I found myself wishing for more science. I did not enjoy taking these anthropology courses as much as I would like, but I do enjoy the biological anthropology courses I have taken. Therefore, I decided to stick with anthropology and declared biology as my second major as my solution to wanting more science in my courses.

P4: A class in high school actually led me to become a geology major. It was called [proper noun], and was all about the geology and environment of Oregon, which I found fascinating. I also had to think about what I wanted my future career to involve, and the type of work environment I wanted to have, so thinking about those aspects led me to geology. For me, the major challenge of the majors I have is not knowing the exact direction they'll take me in regard to a career, but I have been gradually narrowing down exactly what I want to do after college.

P5: Some challenges that I faced while deciding my major was figuring out what classes and courses that I liked and could see myself doing for the rest of my life. I mentioned that the science classes weren't my strong suit but it took trial and error to figure that out. Once I started taking Psychology classes I found what I was interested in and I stuck to it.

Continuing-Generation

P6: The biggest determining factor for my major was the fact that I loved Biology, and it was sealed after I took an introductory course in Mendelian Genetics back at Community college. Genetics was something that I found that I was drawn to, and I couldn't help but want more. Furthermore, my declaration of Pre-Med came when I was job-shadowing a cardiovascular surgeon and an anesthesiologist. Being in the operating room was cool, and at that point, I knew what I wanted to do with my life. Helping others by becoming a doctor.

P7: After I took my first journalism class, I started thinking about if I should switch my major. The class ([course number and name]) kept my interest and I was excited to go to class every day. The biggest challenge I faced was making the decision to change my major. Coming in to college I had my heart set on being a business major, so it was scary for me to change my major and ultimately, possibly my career path. But in the end I knew the business major was not right for me, and the journalism major is a much better fit.

P8: On my college application, I actually had no idea of exactly what I wanted to do. I knew that I was good at and enjoyed writing, and I wanted to connect the business world to everyday life. After taking a bunch of different classes, I got a better feel of what I wanted to be doing. My introductory business, economics and marketing courses opened my eyes to Finance and Sports Marketing. I met with my adviser and changing my major was so easy and I felt very happy and confident that this was the best path for my life.

149

Q3: What has been your favorite class and least favorite so far and why?

<u>First-Generation</u>

P1: My favorite class so far has been [specific topic]! I had heard so many good things about this class so I decided to give it a shot. I took it my sophomore year and I fell in love with it. It opened my eyes and made me view the world from many perspectives and understand issues I didn't understand or know about before. I was always excited to read for this class, which is not necessarily something I enjoy doing very much. I fell in love with this class so much that I decided to minor in [field]. My least favorite class was probably my anthropology class on the [specific topic]. The reason for that is that my friends and I had a hard time understanding the material and when it came down to taking tests, we wouldn't do as great as we thought we would; however, I did pass the class by studying for a couple days and asking for help from my GTF.

P2: My favorite was [course number and name] with [instructor name]. The instructor of the class truly has a passion for career/major advising and it made class more enjoyable. The diversity of the class helped me realize that I wasn't alone with my struggles and fears of finding the perfect major or career. [course number and name] was my least favorite class because I felt so lost in the class all the time. The content that I was learning was very interesting but I did not have time to enjoy it since the class was moving at such a fast pace. I ended up barely passing the class and it was my fault that I didn't make the time to visit the professor during office hours to try and learn the content to do better in the exams.

P3: My favorite class that I have taken so far was philosophy of [specific topic]. Besides science, I have always liked learning about the [specific topic] because it is important to us in our everyday lives and knowing about it is important. I took this law class as a last minute class because I did not have another class to take, so I signed up for it. It ended up being my favorite class. The [material] and discussions we had were good because they stimulated my interest which I feel is important and necessary. As a college student, I believe that stimulating student's interest in the topic at hand is very necessary because we are paying for our education and we should be interested in learning what we're paying for. My least favorite class had to be the third class of the [course] sequence. I was not interested enough in the topic and I did not like my professor very much. My professor did not stimulate my interest in the topic and I felt like I was wasting my time in that class since I felt like I did not learn anything.

P4: One of the best classes I've taken so far was intro to [specific topic]. It might have also been one of the most difficult classes I've taken, but I think I liked it so much because it was challenging. It was also extremely interesting, and the class size was small, so everyone was involved. I haven't taken a class yet where I've hated it, but there have been a few classes I've taken that I absolutely dreaded going to, I'd say my least favorite class was [course number and name] just because it was such a large class, which I didn't anticipate at all, and the professor seemed disinterested with the subject.

P5: My favorite class so far would have to be my [course number and name] class, because my professor was excellent. He was funny and witty and he kept the class interesting. He would include material that was relevant to the students which helped understand scenarios in a different point of view. When material is explained in a way that helps the student learn I believe that it makes the more eager to learn. My least favorite class that I've taken would be my [specific topics] class for my freshman interest group. I'm not a very [adjective relating to topic] person so I think it made the class seem challenging for me. Although I will say that it was interesting to learn how [topics] are connected.

Continuing-Generation

P6: That is a little difficult to say. It may not be the class, but the Professor that makes the class unenjoyable. Any class however, can be interesting if you find something that may intrigue me. I never thought I'd enjoy Psychology, or even some Organic Chemistry. However, the classes that I didn't enjoy are the classes where the instructor did not do a good job (in my eyes) or where I didn't learn anything, or if the exams were unreasonable or if the textbook wasn't helpful and the instructor wasn't either. The class that I DID enjoy the most, well, I have a lot, actually. [course number and name] with [instructor name], both quarters of [course] lab, Second Term [course] and [course] for sure makes my top 5.

P7: My favorite class was [course number and name]. I really enjoyed the class and the professor, and I felt like I learned a lot. I learned a lot not just about economics but also how the world works, and applying that knowledge within some of my other classes made me start to feel like I knew a lot about the economy, which was a topic I never expected to really grasp. My least favorite class was [course number and name]. I took it my first term freshman year, and did not realize how hard it would be. I did not focus in the class as much as I should've, and I really struggled to understand the concepts. After that class I realized I needed to apply myself and study harder.

P8: [course number and name] was my favorite class because of the awesome class atmosphere and my professor's energy. I was always excited to go to my business class to really participate in a class that made me feel like I was really learning and taking steps in life. But my least favorite class was [course] because I was totally uninterested. I didn't care about what we were learning and that made it super easy to fall behind, especially in reading assignments because trying to read about something that I thought was boring was so unappealing.

Q4: What do you do to be successful in your classes? For example, how do you plan your courses and what are some strategies for being successful in those courses?

First-Generation

P1: When I plan my courses I try to register for classes that I am interested in but also fulfill my pre-requirements. They may be requirements, but you have a large variety of class topics to choose from! When I have trouble figuring out exactly what to register for, I always go in or schedule an appointment with my advisor. I highly suggest meeting with your advisors and discussing what classes to take. They are of great help! They will be one of the best resources when it comes down to stuff like this. Thanks to my advisor I was able to register for a couple classes where you could "double dip," meaning you would get two requirements out of the way with just one class! If I hadn't gone to my advisor, I would have never known that. They are some very helpful and friendly people.

P2: Pick classes that seem somewhat interesting to you. That way you're more eager to learn and it makes studying a lot easier. I plan my courses out by the times they have. I like to study at night and sleep in. However if I have to I'll take early morning classes (8AM) that way I have the rest of the day to study or work. I try not to have too many classes with friends because friends in classes can become a huge distraction. One or two classes per term is good enough, that way you can help each other study. Make school work your number one priority. That's how I did well. Make time for fun after the study guide is filled out.

151

P3: I try to be one step ahead when it comes to school. When I choose my classes, I do extensive research regarding which professor is teaching the class, what time the class is held, the possible workload I would have, and how much time I would have for that class. This is vital because if I cannot put the effort into that class because of other things I have to do, then I would rather take another class that would allow me to be both successful and less stressed.

P4: When planning my courses, I usually try to set it up where most of my courses are at the beginning of the week and are scheduled fairly close together. After doing this I usually have large parts of some days open where I can get most of my homework done. To be successful in classes I just attend every class, do all the required work, and review my notes and all the materials before major tests. I also make sleep a priority, because I cannot function without sleep. I also have to manage time wisely and put school work before social activities.

P5: One of the most important things when planning your schedule is figuring out what kind of student you are. For example, do you prefer morning or afternoon classes or what time will give more time to finish all your homework and study time? I found that I would prefer morning classes to get my day started early and that way I could have most of my afternoon and evening free to go to work and do homework. Another thing I do to stay organized is write in my planner. If I know when a test or midterm is scheduled I write it in my planner so that I can plan ahead just in case I have conflicts with other classes. Taking notes with different colored pens was very helpful and a good way to stay organized.

Continuing-Generation

P6: Study, and study more. Now people say that studying isn't always the best idea, and it's true. Overloading yourself with information is not the best way to go. The new culture shock is going to office hours. Office hours are a must if you want to do well in a class. Now, that doesn't mean go to every office hour, but it seems to me the more office hours I go to, the better I understand the material walking out of it. Also, try to not overload yourself with so many hard-hitting classes. If you think you are ready for a large load, you need to want it badly. Your friends won't be seeing much of you if you take a large number of credits that require a bunch of work. It's just the nature of the beast. But, on this mountain that we are climbing that is college, it doesn't hurt to take some courses that you may be interested in. Just trying out a course, a course that you may enjoy, or a PE class that may get your mind off of things. Martial Arts, Judo, Ju-Jitsu and Combatives played a big role in keeping me sane freshman year.

P7: Generally, I plan my courses so that I can fulfill my major and general education requirements. I try to take as many courses as I can that fulfill both with just one course. Being a triple major, I don't ever have room in my schedule to take any classes that aren't required. In order to be successful, I pay attention in class and read the syllabus about what is expected of me. I never skip class, and I always do the readings for that class. That way I understand what is going on better in class and am more prepared for tests and quizzes, I don't have to cram last minute for them.

P8: I really try not to overload my schedule or even my days, allow your schedule to give you enough time to do your homework and actually get sleep. I also liked to plan my classes close together in time so that I wouldn't have to go back and forth and fall back asleep between classes.

Q5: How do you study for midterms and final exams? What are some challenges that you encountered?

<u>First-Generation</u>

P1: I used to study by re-reading slides or notes from my classes for my upcoming exams and quizzes. That was a big mistake. In my head, I thought I was "studying" but nothing was sticking when it came down to taking the actual test. I went to my advisor and talked to her about the situation and she suggested for setting up an appointment on test taking skills. She set the appointment for me and it was one of the best decisions ever. The person I talked to asked me what I did to study and then gave me various tips on how to take multiple choice tests, written tests, timed tests and so on. I practiced all the tips she gave me and I got an A on my final for the class, I was so happy! Creating study guides and making note cards can be very helpful when done correctly.

P2: Constantly test yourself on the content that you're learning in class. Make yourself do a weekly test on the material that was being taught in class. I realized that the more I tested myself throughout the term; I learned more so I wasn't as stressed when it came to exams and tests. Some challenges that I encountered were the consistent pattern of comprehension exams. This is where learning the ins and outs of concepts instead of memorizing them helps out because a question on the exam may not make any sense to you if you are so caught up in memorizing the content rather than learning all the different ways that the concepts apply.

P3: I like to write and rewrite my notes over and over until I feel like I have gotten it down. That's the best way I learn. I also go over lecture notes and see what I don't understand and from there. I go back into my hand written notes and refer to my books for clarification. The challenging thing for me when it comes to studying for midterms and finals is time management. I always find myself studying last minute and I cram for my exams.

P4: When studying for midterms and final exams I usually try to start at least a few days before, but usually try studying a week before. I organize all my notes and course materials. I then go online to look at any notes or course materials posted online. For some classes I start by rereading my notes, but for others I start by reading over the textbook, but it really just depends on the class and the professors teaching style. One of the biggest challenges is when the professor won't tell you what will be on the test or when so much has been covered in the course that you don't know what to focus on. To try and overcome that challenge, I usually begin by asking the professor a lot of questions.

P5: If I have a class where the teacher posts lecture slides I make sure to save the slides to my iPad and then I review them and rewrite important information. Another thing that is helpful is going over practice material and tests. In my statistics class it was nice to feel prepared and reviewed right before the tests.

<u>Continuing-Generation</u>

P6: Most professors will have exam dates listed in their syllabus. Study for these as soon as possible. It is something that is said constantly, but if you paid 120 dollars for a book, you might as well use it! Read the book ahead, even if it's just a few pages and ask in advance. The earlier you can prepare for the exam the better. Cramming is never a good option. Another is to find a study group. Lone Wolves are a rare-breed, studying on their own. Most of us do better studying in groups. Try a bit of both. I found that in study groups there are a variety of people there with

different strengths and weaknesses. If you can explain the material to them, you show that you know the material well enough to teach it. I'd also get to know people in your classes as soon as possible and get their numbers. You never know, life may throw a curve ball and you may need to miss class, but also these could be your study groups. You may not know them or talk to them ever again, but when push comes to shove, you need to learn how to work with those in your class to get through it. You are all sharing a common bond, going through that class together. There's some camaraderie in that.

P7: I study for midterms and final exams by looking at the study guide (if there is one), rereading the chapters in the book that will be covered, and doing any problem sets or quizzes that I have over again. I will also look through all the notes I have and all of the slides that are posted online. I typically spend a few nights in the library studying, because I typically focus better than when I'm just studying in my apartment. Going to the library puts my brain in the mindset that I am there to work, so I don't get distracted as easily. The biggest challenge I encountered was when I first got to college I underestimated how difficult my classes would be, and my grades were not great because of it. I didn't study nearly as much as I should've because I didn't realize I needed to. Once I realized I needed to change my study habits and focus more, my grades improved significantly.

P8: Go to the library or a study area!! Being in a quiet place that is designated for academics or studying will really motivate you to study harder. If I stayed home, I would become lazy! The library has really great studying rooms where you can get individual or group assignments done much faster and better.

Q6: What are some options that you are considering as a future career path? How did you come to recognize those options? What are the advantages and/or disadvantages of the different paths you are considering?

First-Generation

P1: The career path that I want to pursue is to become a bilingual news reporter and anchor. To be more specific, my dream career would be to work for Univision, which is a Spanish channel for Latinos around the United States. I love my Latin roots and I am not ashamed to embrace them. Having a career where I can speak my native language, do what I love, travel the world, inform others of what is going on seems pretty great to me. I cannot think of a better career for me than that. I fell in love with the news reporting industry ever since I got to be on live television at the Oregon state capital when I was in high school. I knew then that news reporters and anchors play a big role in informing the world of current events. I want to be able to do that for my American and Latin communities. I know that a disadvantage to this career path is that there is a lot of competition but all I tell myself is that I have to work hard for what I really want and I will get it sooner or later.

P2: One option that I have been considering is doing social work to help children/young adults or becoming an academic/career advisor. I have come to realize this because I have always had the passion to work with people and I excel with hands on work. I would be face to face with people and helping them make decisions to help themselves. Between my last year in high school and my first year in college I have spent a large amount of time with advisors discussing the best options for me. They never made me do what I didn't want to do they would just lay out the different options and discuss the pros and cons. Working in this field would be an advantage because I would get to be altruistic and that is my passion. A downfall of that field would be how stressful the work would be. I would imagine it would be difficult getting attached to people with

whom I would be working with and not being able to walk away from the situation without guilt when there isn't something I could do to help.

P3: My goal for now is to work for the Centers for Disease Control. They offer careers in both anthropology and biology, which provides me with the best of both worlds. What got me interested in this was the fact that I wanted to work in a lab and do more of the "behind the scenes" work when it comes to trying to help people. Another option I am considering is working for a college in a lab as a researcher. This is an option I am considering because research labs at universities do conduct research that pertains to the public and I would like to be part of that if it can be beneficial. There are many advantages to both paths, such as a paid job while doing something that I enjoy. I think the main disadvantage is actually obtaining these careers because it is a competitive field and many people may be more qualified than I am. However, that does not mean that I am going to not try my hardest to make this happen.

P4: Geology and Economics are such broad majors that the career paths I can take are almost endless. I'm considering getting my masters though in order to specialize more, and possibly joining the Peace Corps. At this point I'm either going to look more into the mining field and the buying and selling of minerals, or something that has to do with hazard mitigation and the protection of groundwater. I came to recognize these options through classes taken here, and my own personal research with some help from the [resource] center. The major disadvantage of these paths is the extra schooling, but there are so many more advantages.

P5: I am considering sports psychology where I can help athletes and that way I can be more hands on with my patients. An advantage of my major is that there is always a need in every town or state. A disadvantage is that currently I do not know of any schools that have a sports psychology program but I will need to get further information hopefully after talking to an advisor.

Continuing-Generation

P6: Right now, my goal is to go to medical school and become a Cardiovascular Surgeon. However, other career paths that may lie within my interests include a career at [corporation], a dance instructor here at the University of Oregon, or a Psychologist, with the latter only being considered after I've taken a few psychology courses. At [corporation], I can continue, but I can only go so far before things reach their limit.

P7: When I graduate I am hoping to work at a public relations firm, potentially abroad somewhere in Europe. If I don't do that, then I hope to work in the public relations department of a corporation, or potentially for a non-profit. I took my first real public relations class in the spring of my sophomore year, which is when I realized public relations is my passion and how much I would enjoy working at a PR firm. I have been speaking German since I was in 8th grade and continue to take classes in college, so I am hoping to potentially work in Germany once I graduate, because I think it would be a great way to further my German speaking as well as see a new part of the world. The greatest advantage to working at a PR firm is that I know I would be doing something I love. The biggest disadvantage is that it is highly competitive to get a job at a PR firm.

P8: Right now, my goal/dream job is to be on the marketing team for an NBA team. I met a senior girl last year who just graduated from UO and went on to be on the marketing board for the [NBA team name]. Then I realized how obtainable that job is and how stoked I get even thinking about it. I have connections at [corporation] as well and would love to be an executive.

155

VARIABLE SELECTION FOR PROPENSITY-SCORE MODELS

Variable Label Codes

Type	Label	Variable
Student Records	GENDER	Gender
	advantaged	Race/Ethnicity
	PnP	Bridge Program Participation
	HS_GPA	High School GPA
	SAT_CONV_ACT	Best SAT/ACT Score
	apcred_binary	AP Credit
	Pell	Received Pell
	Loan	Received Loan
	Work.Study	Received Work Study
	Scholarship	Received Scholarship
Psychological Wellness	LS_0	Life Satisfaction
	SEs_0	Self-Esteem
	Sa_0	Sadness
	St_0	Stress
Physical Wellness	GH_0	General Health
	logI_0	Illness
	BMI_0	Body Mass Index
	rcE1_0	Moderate Aerobic Exercise
	rcE2_0	Vigorous Aerobic Exercise
	rcE3_0	Strength Training Exercise
Success (Engagement)	AE_0	Academic Enjoyment
	EA_0	Extracurricular Activity
	rcST_0	Study Time
Psychological Mechanisms	SB_0	Social-Belonging
	AB_0	Academic-Belonging
	NtB_0	Need to Belong
	L_0	Loneliness
	SSa_0	Social Support: Appraisal
	SSb_0	Social Support: Belonging
	SSt_0	Social Support: Tangible
	SA_0	Self-Assurance
	SEf_0	Self-Efficacy
	STh_0	Stereotype Threat
Socioeconomic Status	FGcombo	Generation Status
	FI_0	Family Income
	SSf_0	Subjective Social Status (Family)
	SSs_0	Subjective Social Status (Self)
Religiosity	Rel_0	Religiosity

Life Satisfaction

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
SAT_CONV_ACT	Pell	SAT_CONV_ACT
AB_0	PnP	Pell
Pell	apcred_binary	PnP
Loan	LS_0	apcred_binary
advantaged	Sa_0	LS_0
PnP	St_0	Sa_0
apcred_binary	GH_0	St_0
LS_0	SB_0	GH_0
SEs_0	AB_0	rcE1_0
Sa_0	L_0	rcST_0
St_0	SSa_0	SB_0
GH_0	SSb_0	AB_0
rcE1_0	SA_0	L_0
rcE2_0	STh_0	SSa_0
rcE3_0	FI_0	SSb_0
EA_0	SSf_0	SA_0
rcST_0	SAT_CONV_ACT	FI_0
SB_0	Loan	SSf_0
L_0	advantaged	Loan
SSa_0	SEs_0	SEs_0
SSb_0	rcE1_0	rcE2_0
SSt_0	rcE2_0	rcE3_0
SA_0	rcE3_0	EA_0
SEf_0	EA_0	SSt_0
STh_0	rcST_0	SEf_0
FI_0	SSt_0	STh_0
SSf_0	SEf_0	SSs_0
SSs_0	SSs_0	Rel_0
Rel_0	Rel_0	advantaged
HS_GPA	Work.Study	HS_GPA
FGcombo	Scholarship	FGcombo
Scholarship	GENDER	GENDER
Work.Study	logI_0	BMI_0
GENDER	HS_GPA	Work.Study
logI_0	FGcombo	Scholarship
BMI_0	BMI_0	logI_0
AE_0	AE_0	AE_0
NtB_0	NtB_0	NtB_0

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts ^{C} and ^{T} indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

Self-Esteem

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
AB_0	GENDER	GENDER
Loan	PnP	PnP
GENDER	LS_0	LS_0
PnP	Sa_0	Sa_0
LS_0	St_0	St_0
SEs_0	GH_0	GH_0
Sa_0	SB_0	rcE1_0
St_0	AB_0	SB_0
GH_0	L_0	AB_0
rcE1_0	SSa_0	L_0
rcE2_0	SSb_0	SSa_0
rcE3_0	SA_0	SSb_0
EA_0	STh_0	SA_0
NtB_0	Loan	Loan
SB_0	SEs_0	SEs_0
L_0	rcE1_0	rcE2_0
SSa_0	rcE2_0	rcE3_0
SSb_0	rcE3_0	EA_0
SSt_0	EA_0	NtB_0
SA_0	NtB_0	SSt_0
SEf_0	SSt_0	SEf_0
STh_0	SEf_0	STh_0
SSs_0	SSs_0	SSs_0
Rel_0	Rel_0	Rel_0
HS_GPA	Pell	HS_GPA
SAT_CONV_ACT	Work.Study	SAT_CONV_ACT
FGcombo	Scholarship	FGcombo
Scholarship	apcred_binary	Pell
Pell	logI_0	apcred_binary
Work.Study	FI_0	BMI_0
advantaged	SSf_0	rcST_0
apcred_binary	HS_GPA	FI_0
logI_0	SAT_CONV_ACT	SSf_0
BMI_0	FGcombo	Work.Study
AE_0	advantaged	Scholarship
rcST_0	BMI_0	advantaged
FI_0	AE_0	logI_0
SSf_0	rcST_0	AE_0

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts ^{C} and ^{T} indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

Sadness

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
AB_0	apcred_binary	apcred_binary
apcred_binary	LS_0	LS_0
LS_0	Sa_0	Sa_0
SEs_0	St_0	St_0
Sa_0	GH_0	GH_0
St_0	SB_0	SB_0
GH_0	AB_0	AB_0
rcE2_0	L_0	L_0
rcE3_0	SSa_0	SSa_0
SB_0	SSb_0	SSb_0
L_0	SA_0	SA_0
SSa_0	STh_0	SEs_0
SSb_0	SEs_0	rcE2_0
SSt_0	rcE2_0	rcE3_0
SA_0	rcE3_0	SSt_0
SEf_0	SSt_0	SEf_0
STh_0	SEf_0	STh_0
SSs_0	SSs_0	SSs_0
Rel_0	Rel_0	Rel_0
HS_GPA	Pell	HS_GPA
SAT_CONV_ACT	Work.Study	SAT_CONV_ACT
FGcombo	Scholarship	FGcombo
Scholarship	GENDER	Pell
Pell	PnP	GENDER
Loan	logI_0	PnP
Work.Study	FI_0	BMI_0
GENDER	SSf_0	rcE1_0
advantaged	HS_GPA	rcST_0
PnP	SAT_CONV_ACT	FI_0
logI_0	FGcombo	SSf_0
BMI_0	Loan	Loan
rcE1_0	advantaged	Work.Study
AE_0	BMI_0	Scholarship
EA_0	rcE1_0	advantaged
rcST_0	AE_0	logI_0
NtB_0	EA_0	AE_0
FI_0	rcST_0	EA_0
SSf_0	NtB_0	NtB_0

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts ^{C} and ^{T} indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

Stress

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
AB_0	apcred_binary	apcred_binary
apcred_binary	LS_0	LS_0
LS_0	Sa_0	Sa_0
SEs_0	St_0	St_0
Sa_0	GH_0	GH_0
St_0	SB_0	BMI_0
GH_0	AB_0	rcST_0
BMI_0	L_0	SB_0
rcE2_0	SSa_0	AB_0
rcE3_0	SA_0	L_0
rcST_0	STh_0	SSa_0
SB_0	FI_0	SA_0
L_0	SSf_0	FI_0
SSa_0	SEs_0	SSf_0
SSt_0	BMI_0	SEs_0
SA_0	rcE2_0	rcE2_0
SEf_0	rcE3_0	rcE3_0
STh_0	rcST_0	SSt_0
FI_0	SSt_0	SEf_0
SSf_0	SEf_0	STh_0
Rel_0	Rel_0	Rel_0
HS_GPA	Pell	HS_GPA
SAT_CONV_ACT	Work.Study	SAT_CONV_ACT
FGcombo	Scholarship	FGcombo
Scholarship	GENDER	Pell
Pell	PnP	GENDER
Loan	logI_0	PnP
Work.Study	SSb_0	rcE1_0
GENDER	HS_GPA	SSb_0
advantaged	SAT_CONV_ACT	Loan
PnP	FGcombo	Work.Study
logI_0	Loan	Scholarship
rcE1_0	advantaged	advantaged
AE_0	rcE1_0	logI_0
EA_0	AE_0	AE_0
NtB_0	EA_0	EA_0
SSb_0	NtB_0	NtB_0
SSs_0	SSs_0	SSs_0

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts ^{C} and ^{T} indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

General Health

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
Loan	LS_0	LS_0
LS_0	Sa_0	Sa_0
SEs_0	St_0	St_0
Sa_0	GH_0	GH_0
St_0	logI_0	BMI_0
GH_0	SB_0	SB_0
logI_0	L_0	L_0
BMI_0	SSa_0	SSa_0
rcE2_0	SSb_0	SSb_0
rcE3_0	SA_0	SA_0
NtB_0	STh_0	Loan
SB_0	Loan	SEs_0
L_0	SEs_0	logI_0
SSa_0	BMI_0	rcE2_0
SSb_0	rcE2_0	rcE3_0
SSt_0	rcE3_0	NtB_0
SA_0	NtB_0	SSt_0
SEf_0	SSt_0	SEf_0
STh_0	SEf_0	STh_0
SSs_0	SSs_0	SSs_0
HS_GPA	Pell	HS_GPA
SAT_CONV_ACT	Work.Study	SAT_CONV_ACT
FGcombo	Scholarship	FGcombo
Scholarship	GENDER	Pell
AB_0	PnP	GENDER
Pell	apcred_binary	PnP
Work.Study	AB_0	apcred_binary
GENDER	FI_0	rcE1_0
advantaged	SSf_0	rcST_0
PnP	HS_GPA	AB_0
apcred_binary	SAT_CONV_ACT	FI_0
rcE1_0	FGcombo	SSf_0
AE_0	advantaged	Work.Study
EA_0	rcE1_0	Scholarship
rcST_0	AE_0	advantaged
FI_0	EA_0	AE_0
SSf_0	rcST_0	EA_0
Rel_0	Rel_0	Rel_0

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts ^{C} and ^{T} indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

Illness

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
HS_GPA	GH_0	HS_GPA
SEs_0	logI_0	GH_0
GH_0	SSb_0	SSb_0
logI_0	HS_GPA	SEs_0
rcE2_0	SEs_0	logI_0
SSb_0	rcE2_0	rcE2_0
SSs_0	SSs_0	SSs_0
SAT_CONV_ACT	Pell	SAT_CONV_ACT
FGcombo	Work.Study	FGcombo
Scholarship	Scholarship	Pell
AB_0	GENDER	GENDER
Pell	PnP	PnP
Loan	apcred_binary	apcred_binary
Work.Study	LS_0	LS_0
GENDER	Sa_0	Sa_0
advantaged	St_0	St_0
PnP	SB_0	BMI_0
apcred_binary	AB_0	rcE1_0
LS_0	L_0	rcST_0
Sa_0	SSa_0	SB_0
St_0	SA_0	AB_0
BMI_0	STh_0	L_0
rcE1_0	FI_0	SSa_0
rcE3_0	SSf_0	SA_0
AE_0	SAT_CONV_ACT	FI_0
EA_0	FGcombo	SSf_0
rcST_0	Loan	Loan
NtB_0	advantaged	Work.Study
SB_0	BMI_0	Scholarship
L_0	rcE1_0	advantaged
SSa_0	rcE3_0	rcE3_0
SSt_0	AE_0	AE_0
SA_0	EA_0	EA_0
SEf_0	rcST_0	NtB_0
STh_0	NtB_0	SSt_0
FI_0	SSt_0	SEf_0
SSf_0	SEf_0	STh_0
Rel_0	Rel_0	Rel_0

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts ^{C} and ^{T} indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

Body Mass Index

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
HS_GPA	PnP	HS_GPA
SAT_CONV_ACT	GH_0	SAT_CONV_ACT
Loan	logI_0	PnP
PnP	SB_0	GH_0
SEs_0	L_0	BMI_0
GH_0	SSb_0	rcE1_0
logI_0	SA_0	SB_0
BMI_0	HS_GPA	L_0
rcE1_0	SAT_CONV_ACT	SSb_0
rcE2_0	Loan	SA_0
rcE3_0	SEs_0	Loan
EA_0	BMI_0	SEs_0
SB_0	rcE1_0	logI_0
L_0	rcE2_0	rcE2_0
SSb_0	rcE3_0	rcE3_0
SSt_0	EA_0	EA_0
SA_0	SSt_0	SSt_0
SSs_0	SSs_0	SSs_0
FGcombo	Pell	FGcombo
Scholarship	Work.Study	Pell
AB_0	Scholarship	GENDER
Pell	GENDER	apcred_binary
Work.Study	apcred_binary	LS_0
GENDER	LS_0	Sa_0
advantaged	Sa_0	St_0
apcred_binary	St_0	rcST_0
LS_0	AB_0	AB_0
Sa_0	SSa_0	SSa_0
St_0	STh_0	FI_0
AE_0	FI_0	SSf_0
rcST_0	SSf_0	Work.Study
NtB_0	FGcombo	Scholarship
SSa_0	advantaged	advantaged
SEf_0	AE_0	AE_0
STh_0	rcST_0	NtB_0
FI_0	NtB_0	SEf_0
SSf_0	SEf_0	STh_0
Rel_0	Rel_0	Rel_0

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts ^{C} and ^{T} indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

Academic Enjoyment

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
HS_GPA	LS_0	HS_GPA
SAT_CONV_ACT	GH_0	SAT_CONV_ACT
AB_0	AB_0	LS_0
LS_0	SSa_0	GH_0
GH_0	HS_GPA	AB_0
AE_0	SAT_CONV_ACT	SSa_0
EA_0	AE_0	AE_0
SSa_0	EA_0	EA_0
SSt_0	SSt_0	SSt_0
SEf_0	SEf_0	SEf_0
SSs_0	SSs_0	SSs_0
FGcombo	Pell	FGcombo
Scholarship	Work.Study	Pell
Pell	Scholarship	GENDER
Loan	GENDER	PnP
Work.Study	PnP	apcred_binary
GENDER	apcred_binary	Sa_0
advantaged	Sa_0	St_0
PnP	St_0	BMI_0
apcred_binary	logI_0	rcE1_0
SEs_0	SB_0	rcST_0
Sa_0	L_0	SB_0
St_0	SSb_0	L_0
logI_0	SA_0	SSb_0
BMI_0	STh_0	SA_0
rcE1_0	FI_0	FI_0
rcE2_0	SSf_0	SSf_0
rcE3_0	FGcombo	Loan
rcST_0	Loan	Work.Study
NtB_0	advantaged	Scholarship
SB_0	SEs_0	advantaged
L_0	BMI_0	SEs_0
SSb_0	rcE1_0	logI_0
SA_0	rcE2_0	rcE2_0
STh_0	rcE3_0	rcE3_0
FI_0	rcST_0	NtB_0
SSf_0	NtB_0	STh_0
Rel_0	Rel_0	Rel_0

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts ^{C} and ^{T} indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

Extracurricular Activity

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
Scholarship	Pell	Pell
Pell	Work.Study	PnP
Work.Study	Scholarship	LS_0
PnP	PnP	St_0
LS_0	LS_0	L_0
St_0	St_0	SSb_0
logI_0	logI_0	SA_0
rcE2_0	L_0	FI_0
rcE3_0	SSb_0	Work.Study
EA_0	SA_0	Scholarship
L_0	FI_0	logI_0
SSb_0	rcE2_0	rcE2_0
SA_0	rcE3_0	rcE3_0
FI_0	EA_0	EA_0
Rel_0	Rel_0	Rel_0
HS_GPA	GENDER	HS_GPA
SAT_CONV_ACT	apcred_binary	SAT_CONV_ACT
FGcombo	Sa_0	FGcombo
AB_0	GH_0	GENDER
Loan	SB_0	apcred_binary
GENDER	AB_0	Sa_0
advantaged	SSa_0	GH_0
apcred_binary	STh_0	BMI_0
SEs_0	SSf_0	rcE1_0
Sa_0	HS_GPA	rcST_0
GH_0	SAT_CONV_ACT	SB_0
BMI_0	FGcombo	AB_0
rcE1_0	Loan	SSa_0
AE_0	advantaged	SSf_0
rcST_0	SEs_0	Loan
NtB_0	BMI_0	advantaged
SB_0	rcE1_0	SEs_0
SSa_0	AE_0	AE_0
SSt_0	rcST_0	NtB_0
SEf_0	NtB_0	SSt_0
STh_0	SSt_0	SEf_0
SSf_0	SEf_0	STh_0
SSs_0	SSs_0	SSs_0

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts [C] and [T] indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

Resource Seeking (Original)

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
HS_GPA	GENDER	HS_GPA
GENDER	Sa_0	GENDER
SEs_0	STh_0	Sa_0
Sa_0	FI_0	FI_0
SSt_0	HS_GPA	SEs_0
STh_0	SEs_0	SSt_0
FI_0	SSt_0	STh_0
SAT_CONV_ACT	Pell	SAT_CONV_ACT
FGcombo	Work.Study	FGcombo
Scholarship	Scholarship	Pell
AB_0	PnP	PnP
Pell	apcred_binary	apcred_binary
Loan	LS_0	LS_0
Work.Study	St_0	St_0
advantaged	GH_0	GH_0
PnP	logI_0	BMI_0
apcred_binary	SB_0	rcE1_0
LS_0	AB_0	rcST_0
St_0	L_0	SB_0
GH_0	SSa_0	AB_0
logI_0	SSb_0	L_0
BMI_0	SA_0	SSa_0
rcE1_0	SSf_0	SSb_0
rcE2_0	SAT_CONV_ACT	SA_0
rcE3_0	FGcombo	SSf_0
AE_0	Loan	Loan
EA_0	advantaged	Work.Study
rcST_0	BMI_0	Scholarship
NtB_0	rcE1_0	advantaged
SB_0	rcE2_0	logI_0
L_0	rcE3_0	rcE2_0
SSa_0	AE_0	rcE3_0
SSb_0	EA_0	AE_0
SA_0	rcST_0	EA_0
SEf_0	NtB_0	NtB_0
SSf_0	SEf_0	SEf_0
SSs_0	SSs_0	SSs_0
Rel_0	Rel_0	Rel_0

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts ^{C} and ^{T} indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

Resource Seeking (Expanded)

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
FGcombo	Work.Study	FGcombo
GENDER	GENDER	GENDER
Work.Study	Sa_0	Sa_0
Sa_0	St_0	St_0
St_0	GH_0	GH_0
GH_0	L_0	rcST_0
EA_0	FGcombo	L_0
rcST_0	EA_0	Work.Study
L_0	rcST_0	EA_0
SSs_0	SSs_0	SSs_0
HS_GPA	Pell	HS_GPA
SAT_CONV_ACT	Scholarship	SAT_CONV_ACT
Scholarship	PnP	Pell
AB_0	apcred_binary	PnP
Pell	LS_0	apcred_binary
Loan	logI_0	LS_0
advantaged	SB_0	BMI_0
PnP	AB_0	rcE1_0
apcred_binary	SSa_0	SB_0
LS_0	SSb_0	AB_0
SEs_0	SA_0	SSa_0
logI_0	STh_0	SSb_0
BMI_0	FI_0	SA_0
rcE1_0	SSf_0	FI_0
rcE2_0	HS_GPA	SSf_0
rcE3_0	SAT_CONV_ACT	Loan
AE_0	Loan	Scholarship
NtB_0	advantaged	advantaged
SB_0	SEs_0	SEs_0
SSa_0	BMI_0	logI_0
SSb_0	rcE1_0	rcE2_0
SSt_0	rcE2_0	rcE3_0
SA_0	rcE3_0	AE_0
SEf_0	AE_0	NtB_0
STh_0	NtB_0	SSt_0
FI_0	SSt_0	SEf_0
SSf_0	SEf_0	STh_0
Rel_0	Rel_0	Rel_0

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts ^{C} and ^{T} indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

Achievement

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
HS_GPA	Pell	HS_GPA
SAT_CONV_ACT	Scholarship	SAT_CONV_ACT
Scholarship	GENDER	Pell
Pell	apcred_binary	GENDER
Loan	LS_0	apcred_binary
GENDER	SB_0	LS_0
apcred_binary	L_0	rcST_0
LS_0	SA_0	SB_0
rcE2_0	FI_0	L_0
AE_0	SSb_0	SSb_0
rcST_0	HS_GPA	SA_0
SB_0	SAT_CONV_ACT	FI_0
L_0	Loan	Loan
SSb_0	rcE2_0	Scholarship
SA_0	AE_0	rcE2_0
FI_0	rcST_0	AE_0
SSs_0	SSs_0	SSs_0
FGcombo	Work.Study	FGcombo
AB_0	PnP	PnP
Work.Study	Sa_0	Sa_0
advantaged	St_0	St_0
PnP	GH_0	GH_0
SEs_0	logI_0	BMI_0
Sa_0	AB_0	rcE1_0
St_0	SSa_0	AB_0
GH_0	STh_0	SSa_0
logI_0	SSf_0	SSf_0
BMI_0	FGcombo	Work.Study
rcE1_0	advantaged	advantaged
rcE3_0	SEs_0	SEs_0
EA_0	BMI_0	logI_0
NtB_0	rcE1_0	rcE3_0
SSa_0	rcE3_0	EA_0
SSt_0	EA_0	NtB_0
SEf_0	NtB_0	SSt_0
STh_0	SSt_0	SEf_0
SSf_0	SEf_0	STh_0
Rel_0	Rel_0	Rel_0

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts ^{C} and ^{T} indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

Persistence (2nd Year)

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
Scholarship	Scholarship	PnP
AB_0	PnP	Sa_0
advantaged	Sa_0	AB_0
PnP	AB_0	L_0
Sa_0	L_0	Scholarship
EA_0	NtB_0	advantaged
NtB_0	advantaged	EA_0
L_0	EA_0	NtB_0
HS_GPA	Pell	HS_GPA
SAT_CONV_ACT	Work.Study	SAT_CONV_ACT
FGcombo	GENDER	FGcombo
Pell	apcred_binary	Pell
Loan	LS_0	GENDER
Work.Study	St_0	apcred_binary
GENDER	GH_0	LS_0
apcred_binary	logI_0	St_0
LS_0	SB_0	GH_0
SEs_0	SSa_0	BMI_0
St_0	SSb_0	rcE1_0
GH_0	SA_0	rcST_0
logI_0	STh_0	SB_0
BMI_0	FI_0	SSa_0
rcE1_0	SSf_0	SSb_0
rcE2_0	HS_GPA	SA_0
rcE3_0	SAT_CONV_ACT	FI_0
AE_0	FGcombo	SSf_0
rcST_0	Loan	Loan
SB_0	SEs_0	Work.Study
SSa_0	BMI_0	SEs_0
SSb_0	rcE1_0	logI_0
SSt_0	rcE2_0	rcE2_0
SA_0	rcE3_0	rcE3_0
SEf_0	AE_0	AE_0
STh_0	rcST_0	SSt_0
FI_0	SSt_0	SEf_0
SSf_0	SEf_0	STh_0
SSs_0	SSs_0	SSs_0
Rel_0	Rel_0	Rel_0

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts ^C and ^T indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

Persistence (Summer)

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
FGcombo	apcred_binary	apcred_binary
Scholarship	Pell	FGcombo
advantaged	PnP	Pell
AE_0	Scholarship	PnP
apcred_binary	advantaged	advantaged
Pell	AE_0	AE_0
PnP	FGcombo	Scholarship
SEs_0	SEs_0	SEs_0
AB_0	AB_0	AB_0
HS_GPA	FI_0	BMI_0
SAT_CONV_ACT	GENDER	FI_0
BMI_0	GH_0	GENDER
EA_0	L_0	GH_0
FI_0	logI_0	HS_GPA
GENDER	LS_0	L_0
GH_0	Sa_0	LS_0
L_0	SA_0	rcE1_0
Loan	SB_0	rcST_0
logI_0	SSa_0	Sa_0
LS_0	SSb_0	SA_0
NtB_0	SSf_0	SAT_CONV_ACT
rcE1_0	St_0	SB_0
rcE2_0	STh_0	SSa_0
rcE3_0	Work.Study	SSb_0
rcST_0	BMI_0	SSf_0
Rel_0	EA_0	St_0
Sa_0	HS_GPA	EA_0
SA_0	Loan	Loan
SB_0	NtB_0	logI_0
SEf_0	rcE1_0	NtB_0
SSa_0	rcE2_0	rcE2_0
SSb_0	rcE3_0	rcE3_0
SSf_0	rcST_0	Rel_0
SSs_0	Rel_0	SEf_0
SSt_0	SAT_CONV_ACT	SSs_0
St_0	SEf_0	SSt_0
STh_0	SSs_0	STh_0
Work.Study	SSt_0	Work.Study

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts ^{C} and ^{T} indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

Social-Belonging

CRH^C FIGARC^T	CRH^C OC^T	FIGARC^C OC^T
FGcombo	LS_0	FGcombo
AB_0	Sa_0	LS_0
LS_0	St_0	Sa_0
SEs_0	GH_0	St_0
Sa_0	SB_0	GH_0
St_0	AB_0	rcE1_0
GH_0	L_0	SB_0
rcE1_0	SSa_0	AB_0
rcE2_0	SSb_0	L_0
rcE3_0	SA_0	SSa_0
EA_0	STh_0	SSb_0
SB_0	FGcombo	SA_0
L_0	SEs_0	SEs_0
SSa_0	rcE1_0	rcE2_0
SSb_0	rcE2_0	rcE3_0
SSt_0	rcE3_0	EA_0
SA_0	EA_0	SSt_0
SEf_0	SSt_0	SEf_0
STh_0	SEf_0	STh_0
SSs_0	SSs_0	SSs_0
Rel_0	Rel_0	Rel_0
HS_GPA	Pell	HS_GPA
SAT_CONV_ACT	Work.Study	SAT_CONV_ACT
Scholarship	Scholarship	Pell
Pell	GENDER	GENDER
Loan	PnP	PnP
Work.Study	apcred_binary	apcred_binary
GENDER	logI_0	BMI_0
advantaged	FI_0	rcST_0
PnP	SSf_0	FI_0
apcred_binary	HS_GPA	SSf_0
logI_0	SAT_CONV_ACT	Loan
BMI_0	Loan	Work.Study
AE_0	advantaged	Scholarship
rcST_0	BMI_0	advantaged
NtB_0	AE_0	logI_0
FI_0	rcST_0	AE_0
SSf_0	NtB_0	NtB_0

Note: CRH = conventional residence hall, FIGARC = residential freshman interest group or academic residential community, OC = off-campus. Superscripts [C] and [T] indicate control and treatment, respectively. Purple = related to exposure and outcome (i.e., confounders; rs > .1, ds > .2, ORs > 1.4), blue = related to outcome, red = related to exposure, no color = not related to exposure or outcome. Purple and blue fed into propensity-score models.

VARIABLE SELECTION FOR IMPUTATION MODELS

Target	Predictors
Life Satisfaction Wave 3	Subjective Social Status (Self) Wave 1
	Life Satisfaction Wave 0
	Life Satisfaction Wave 1
	Stress Wave 1
	Self-Esteem Wave 1
	Social-Belonging Wave 1
	Academic-Belonging Wave 1
	Self-Efficacy Wave 1
Self-Esteem Wave 3	Self-Esteem Wave 0
	Self-Esteem Wave 1
	Life Satisfaction Wave 0
	Stress Wave 1
	Social-Belonging Wave 1
	Academic-Belonging Wave 1
	Social Support: Belonging Wave 0
	Self-Assurance Wave 0
	Self-Assurance Wave 1
Sadness Wave 3	Sadness Wave 0
	Sadness Wave 1
	Social-Belonging Wave 1
	Loneliness Wave 1
	Social Support: Appraisal Wave 1
Stress Wave 3	Stress Wave 0
	Stress Wave 1
	Self-Esteem Wave 1
	Stereotype Threat Wave 1
General Health Wave 3	General Health Wave 0
	General Health Wave 1
Illness	Illness Wave 0
	Illness Wave 1
	General Health Wave 1
Body Mass Index Wave 3	Body Mass Index Wave 0
	Body Mass Index Wave 1
Academic Enjoyment Wave 3	Academic Enjoyment Wave 0
	Academic Enjoyment Wave 1
	Stress Wave 1
Extracurricular Activity Wave 3	Extracurricular Activity Wave 0
	Extracurricular Activity Wave 1
	Religiosity Wave 0

Target	Predictors
Resource Seeking (Original) Wave 3	Resource Seeking (Original) Wave 1
	Gender
Resource Seeking (Expanded) Wave 3	Resource Seeking (Expanded) Wave 1
	Extracurricular Activity Wave 1
	Study Time Wave 1
Social-Belonging Wave 3	Social-Belonging Wave 0
	Social-Belonging Wave 1
	Academic-Belonging Wave 1
	Social Support: Appraisal Wave 1
	Social Support: Belonging Wave 1
	Social Support: Tangible Wave 1
	Loneliness Wave 0
	Loneliness Wave 1
	Stereotype Threat Wave 1

Note: Predictors for analysis models were also included in imputation models. For intervention/control comparisons: condition dummies, generation status and control covariates (gender, race/ethnicity, high school GPA, and SAT/ACT score). For residential group comparisons: grouping variable and weights produced by propensity-score matching.

PROPORTIONS AND PATTERNS OF MISSINGNESS FOR IMPUATION MODELS

Intervention/Control Comparisons

<u>Life Satisfaction</u>
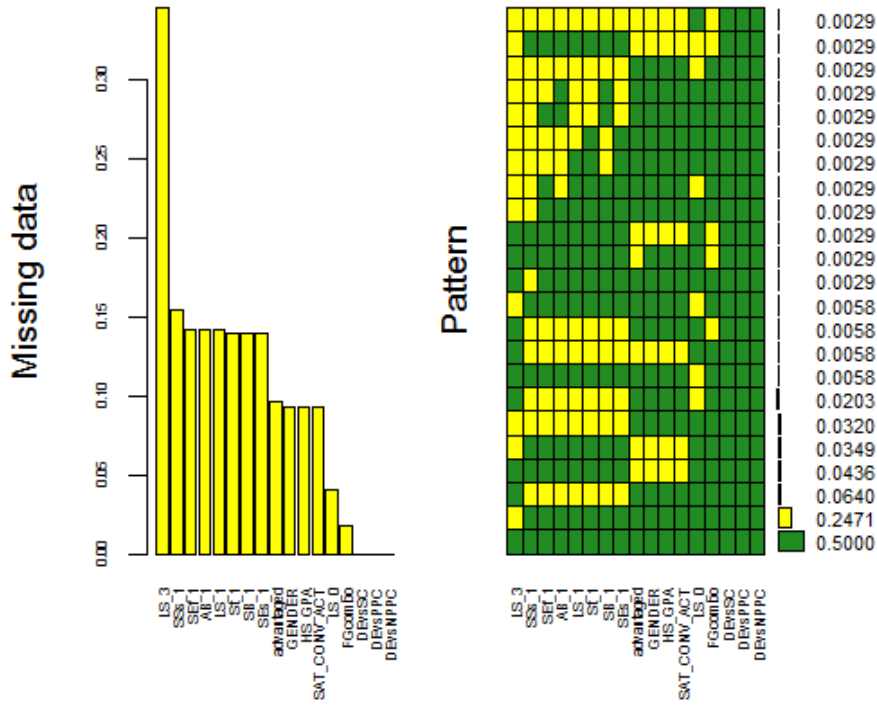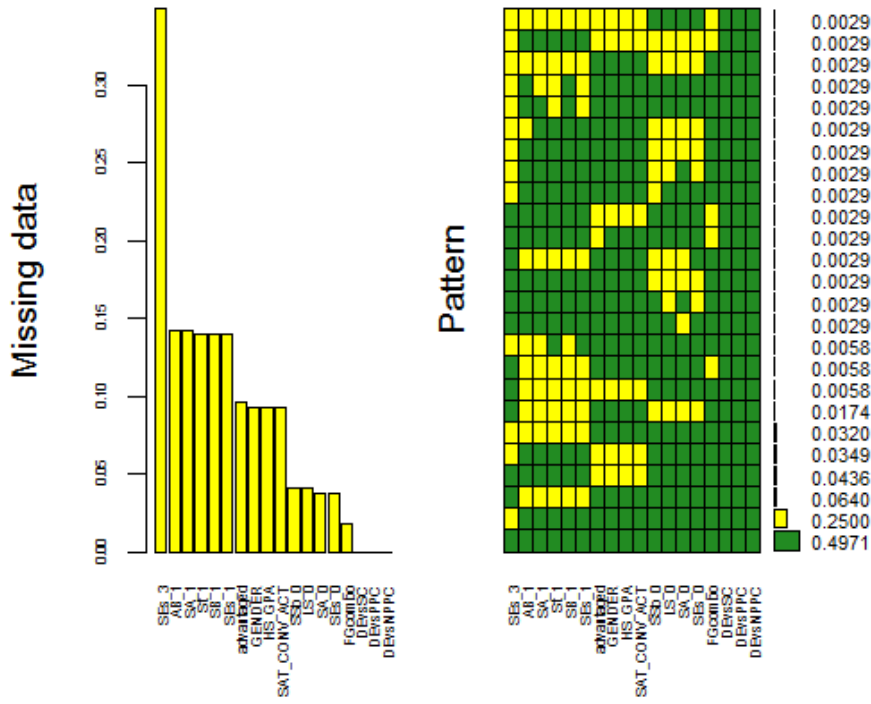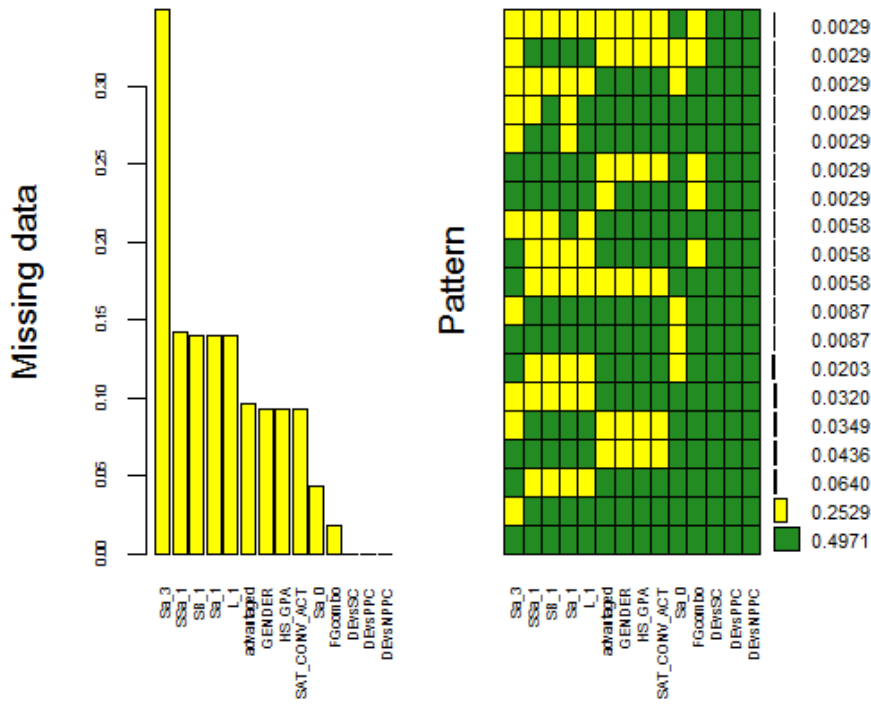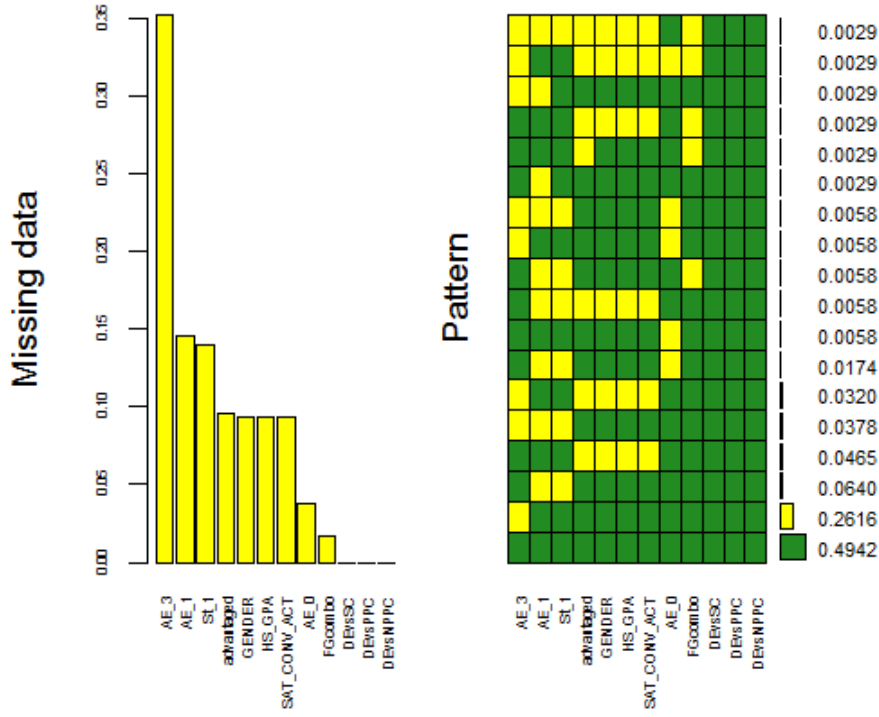
Self-Esteem

Sadness

Stress

General Health

Illness

Body Mass Index

Academic Enjoyment

Extracurricular Activity

Resource Seeking (Original)

Resource Seeking (Expanded)

Social-Belonging
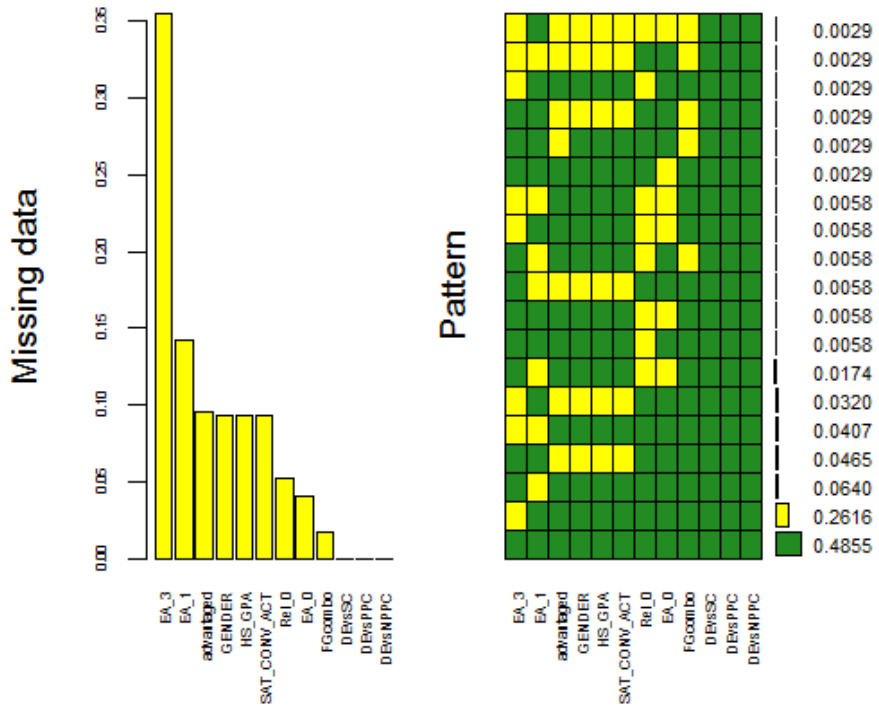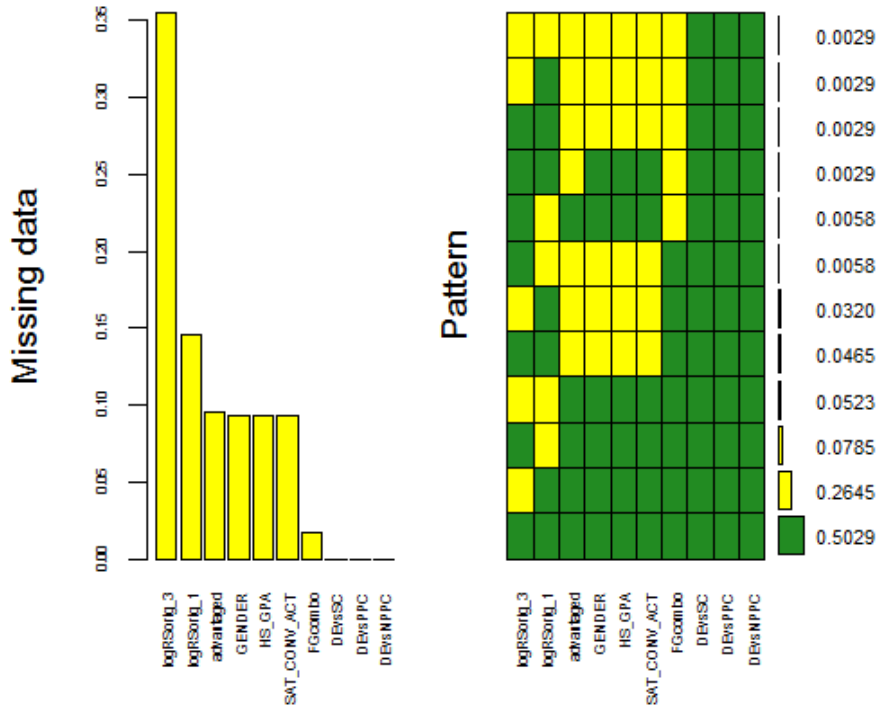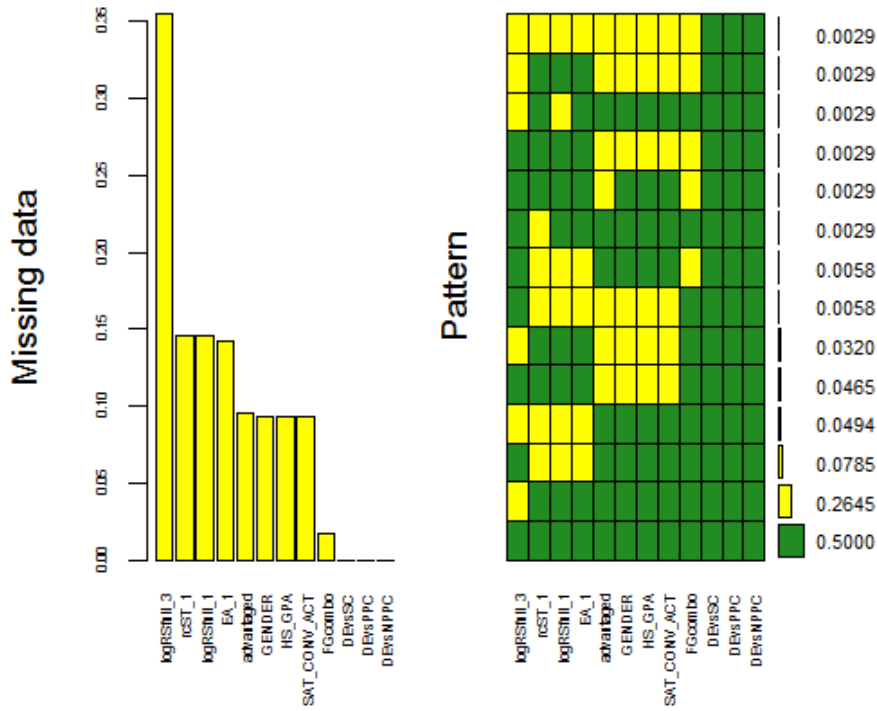
Residential Group Comparisons

Life Satisfaction

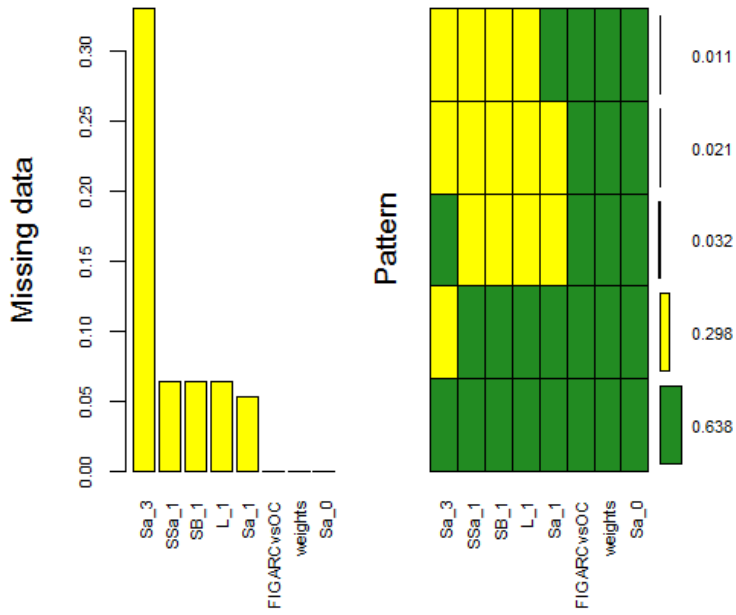*Conventional Residence Hall vs. Residential Freshman Interest Group/Academic Residential Community*
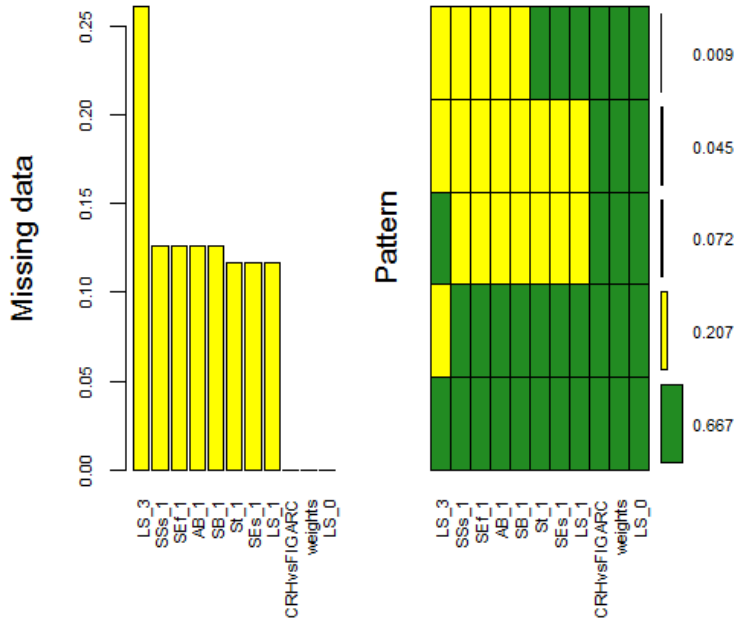
Conventional Residence Hall vs. Off-Campus
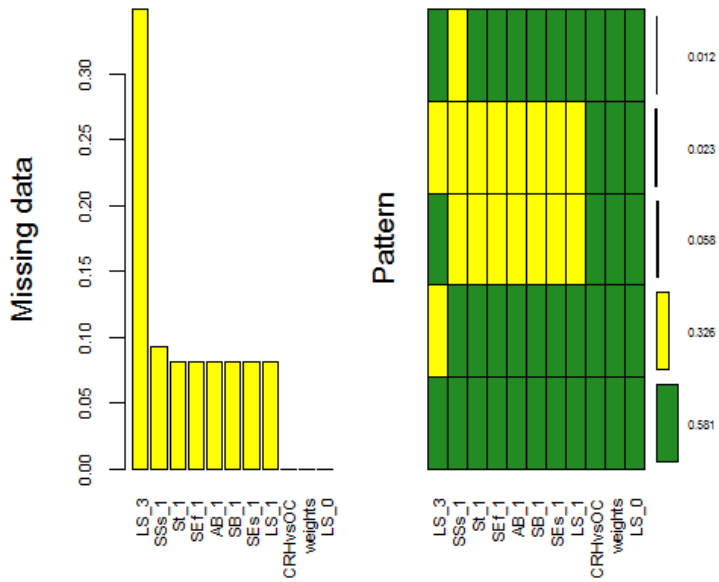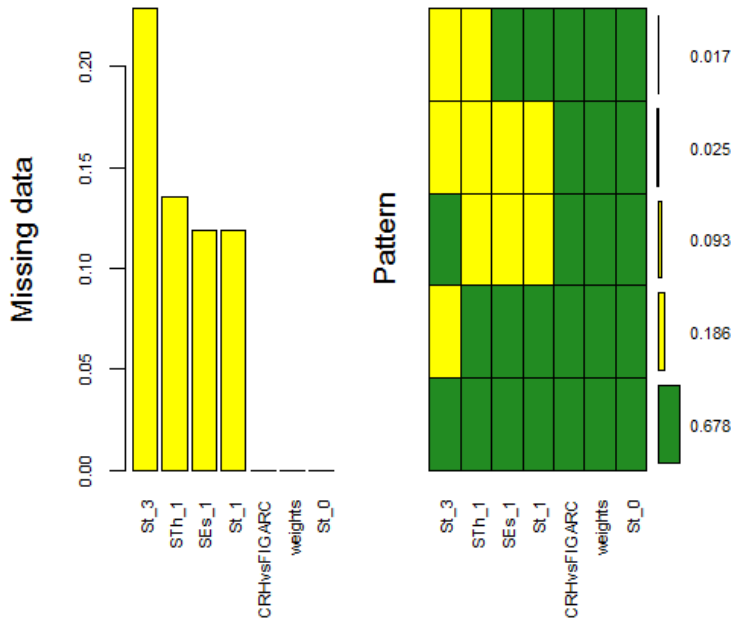
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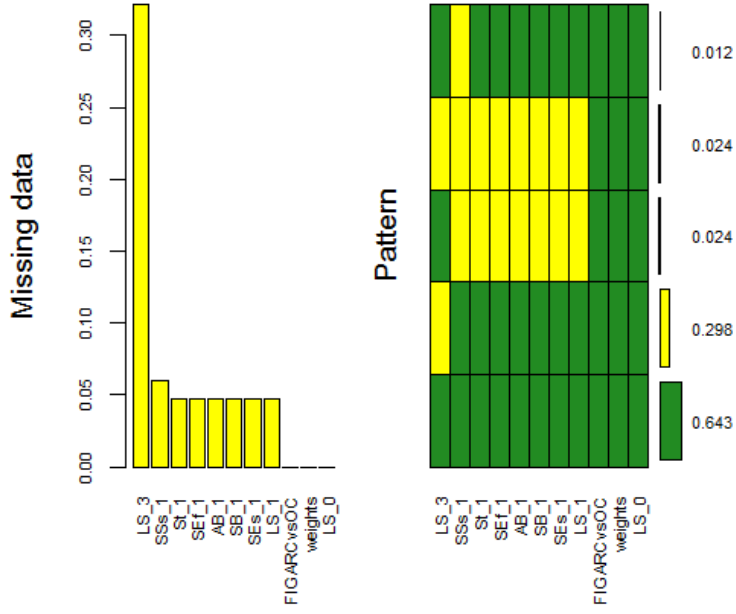
Self-Esteem

Conventional Residence Hall vs. Residential Freshman Interest Group/Academic Residential Community
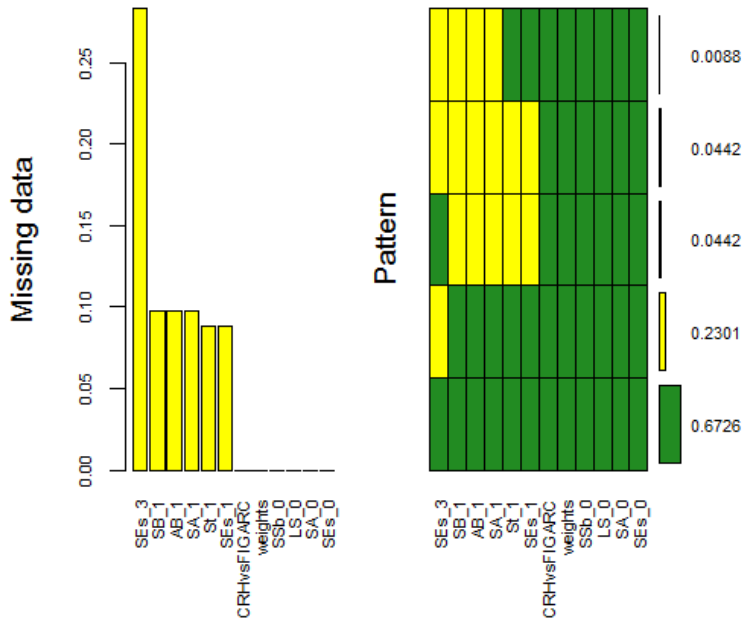
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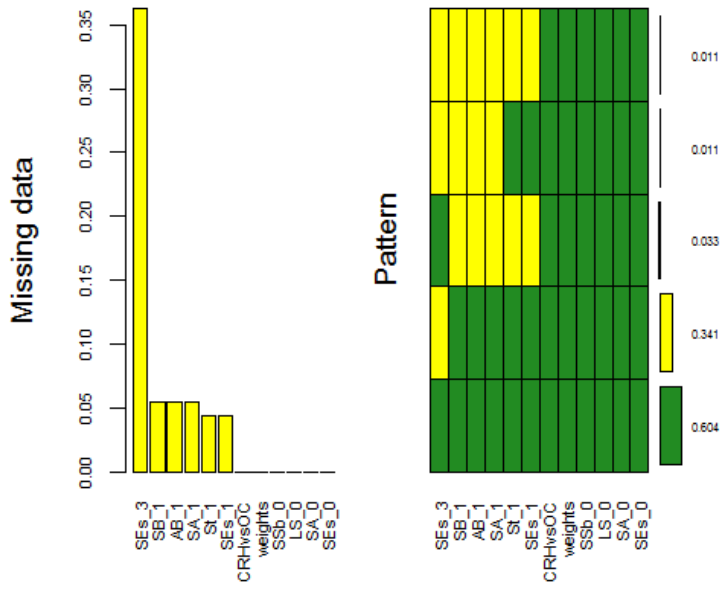
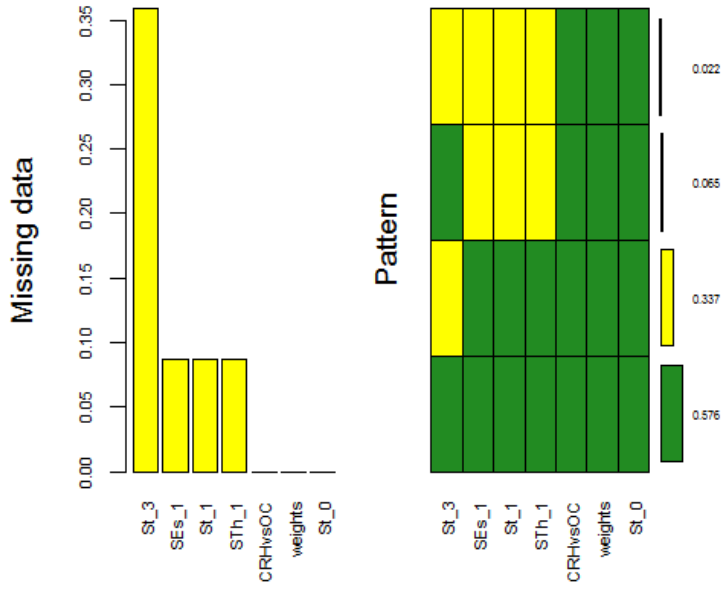*Residential Freshman Interest Group/Academic Residential Community vs. Off-Campus*
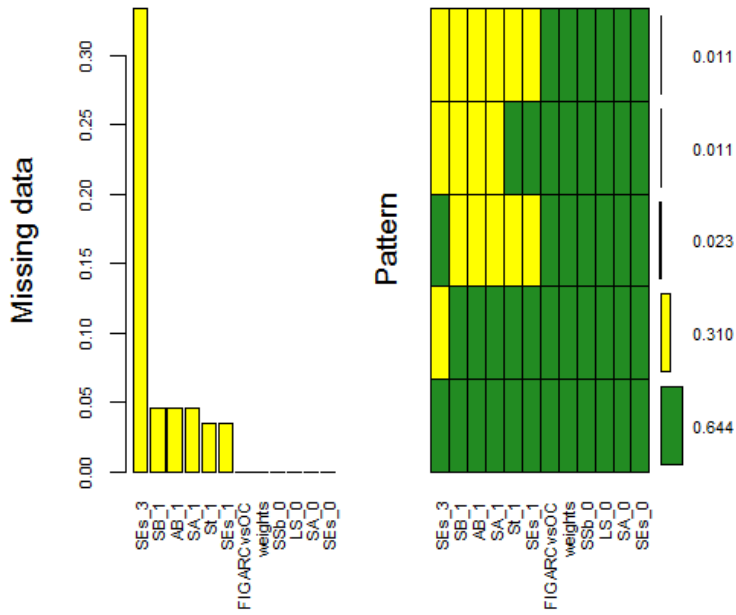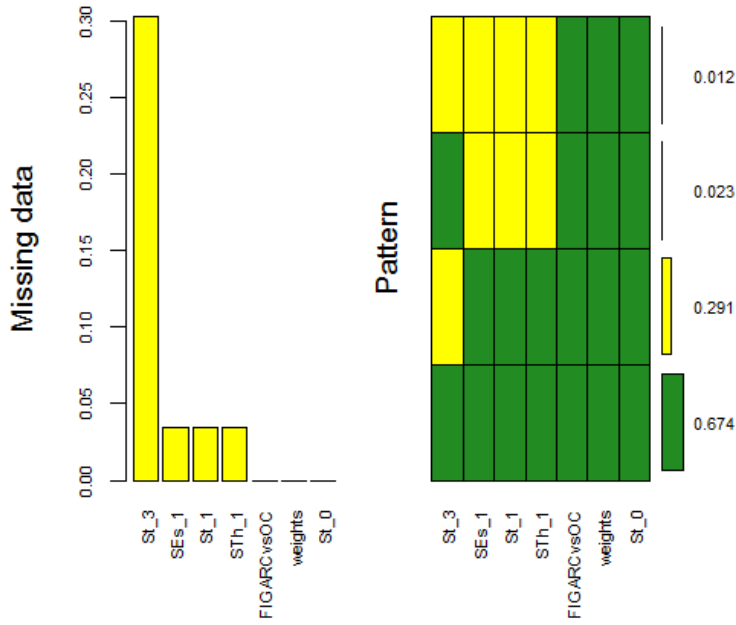
<u>Sadness</u>

Conventional Residence Hall vs. Residential Freshman Interest Group/Academic Residential Community
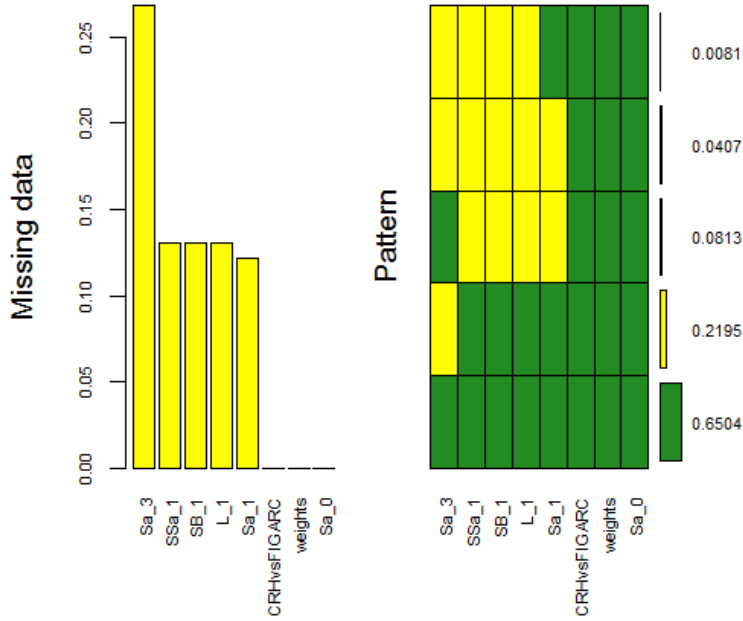
Conventional Residence Hall vs. Off-Campus
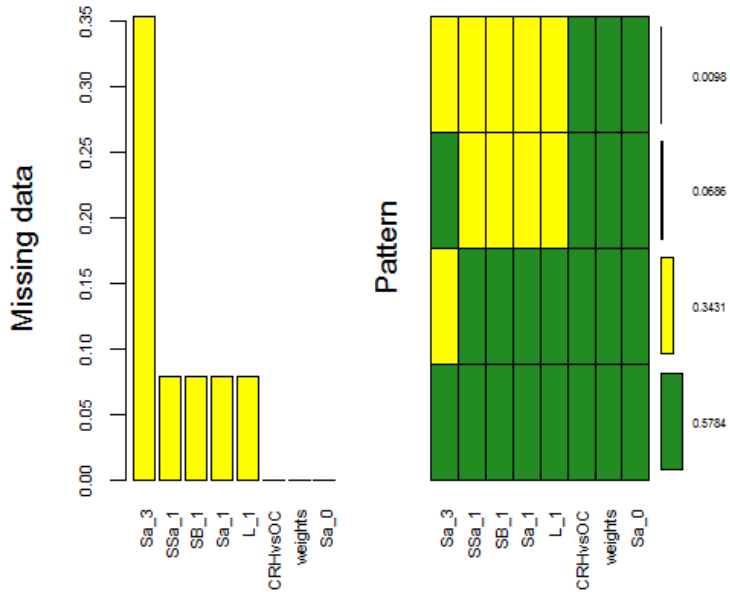
Residential Freshman Interest Group/Academic Residential Community vs. Off-Campus

Stress

Conventional Residence Hall vs. Residential Freshman Interest Group/Academic Residential Community

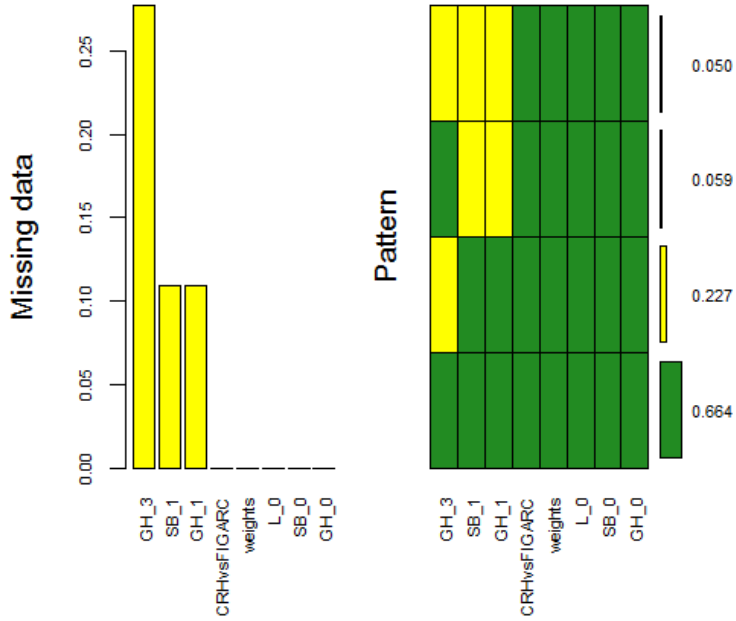*Conventional Residence Hall vs. Off-Campus*

Residential Freshman Interest Group/Academic Residential Community vs. Off-Campus

186

Conventional Residence Hall vs. Residential Freshman Interest Group/Academic Residential Community

Conventional Residence Hall vs. Off-Campus

187

Residential Freshman Interest Group/Academic Residential Community vs. Off-Campus
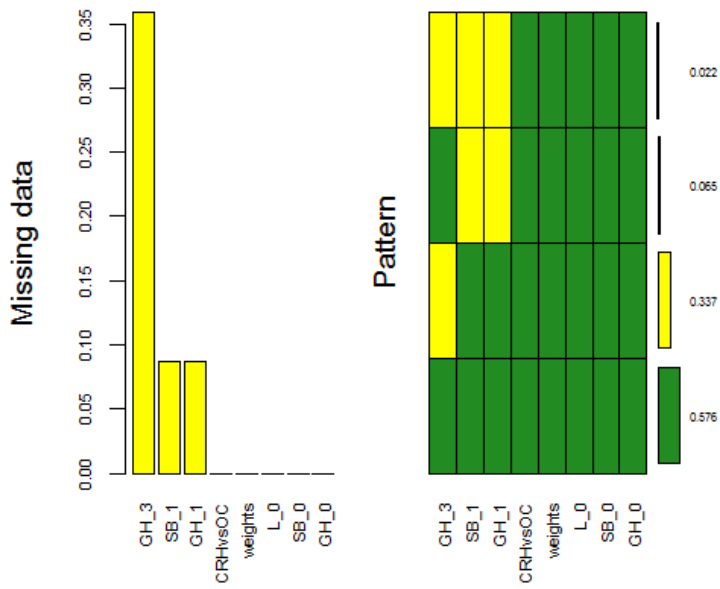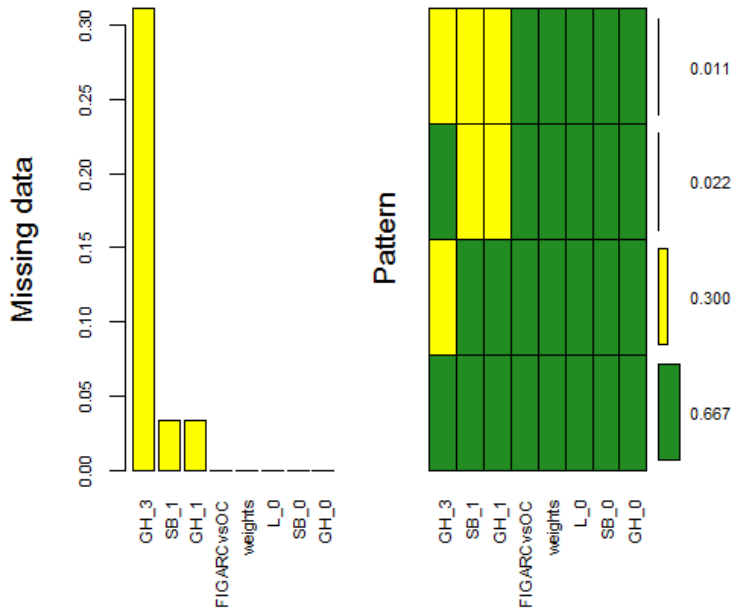

Illness

Conventional Residence Hall vs. Residential Freshman Interest Group/Academic Residential Community
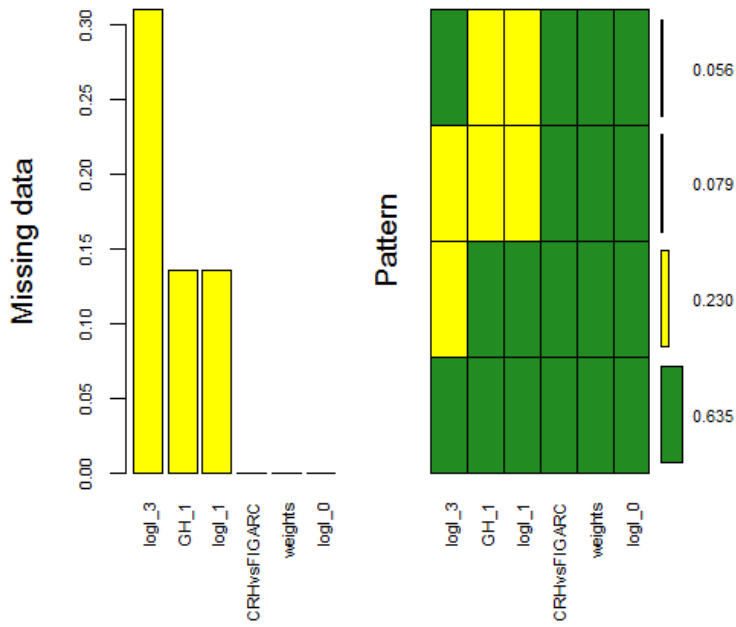
Conventional Residence Hall vs. Off-Campus

Residential Freshman Interest Group/Academic Residential Community vs. Off-Campus

Conventional Residence Hall vs. Residential Freshman Interest Group/Academic Residential Community

Conventional Residence Hall vs. Off-Campus

Residential Freshman Interest Group/Academic Residential Community vs. Off-Campus

Academic Enjoyment

Conventional Residence Hall vs. Residential Freshman Interest Group/Academic Residential Community
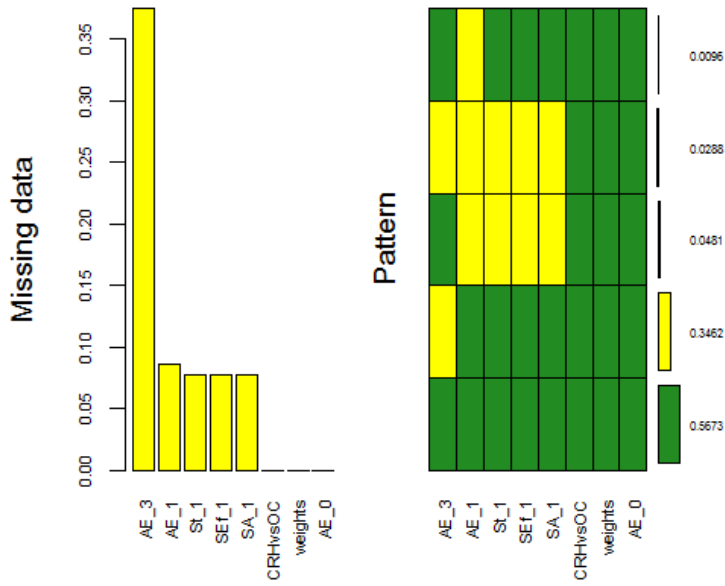
Conventional Residence Hall vs. Off-Campus

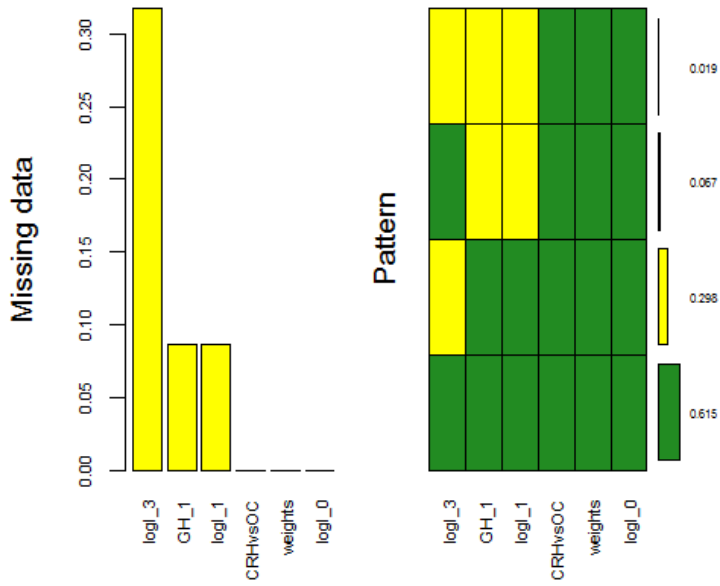*Residential Freshman Interest Group/Academic Residential Community vs. Off-Campus*
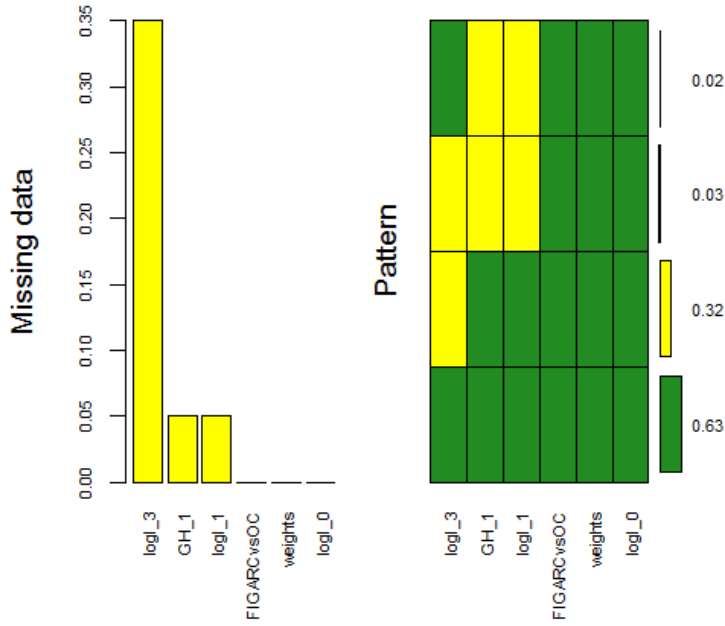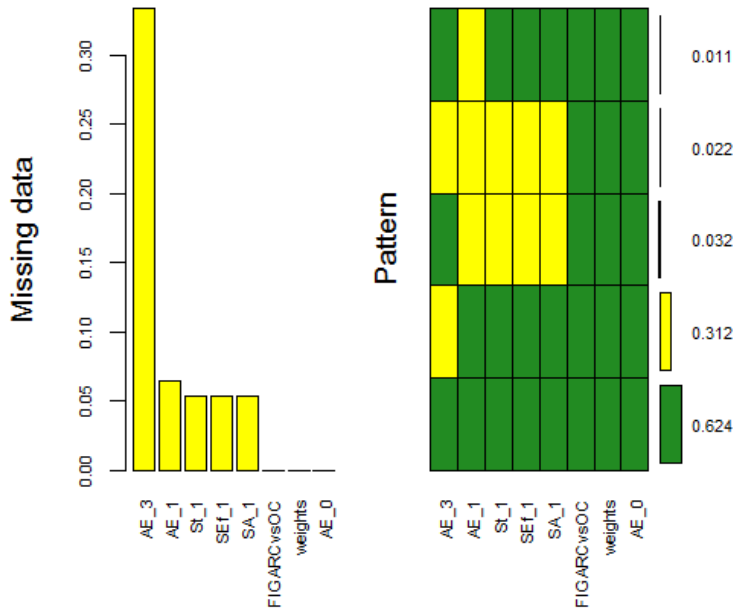
Conventional Residence Hall vs. Residential Freshman Interest Group/Academic Residential Community
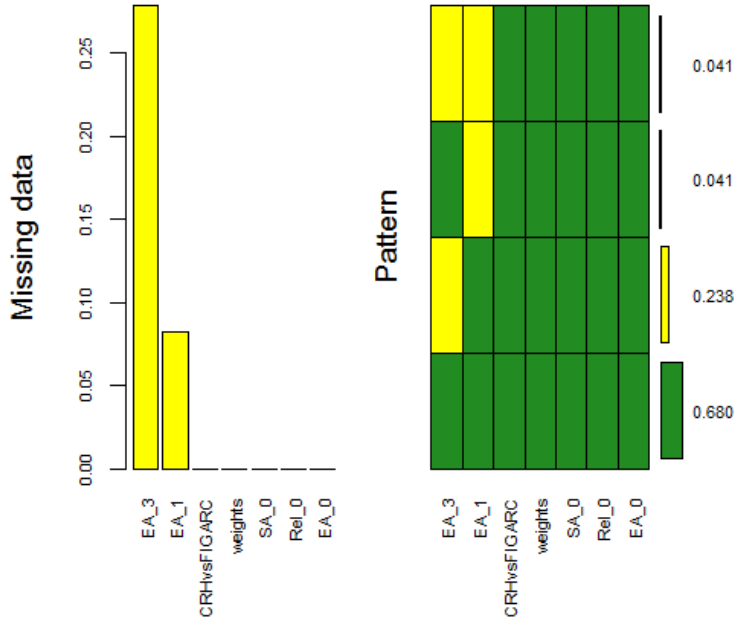
Conventional Residence Hall vs. Off-Campus
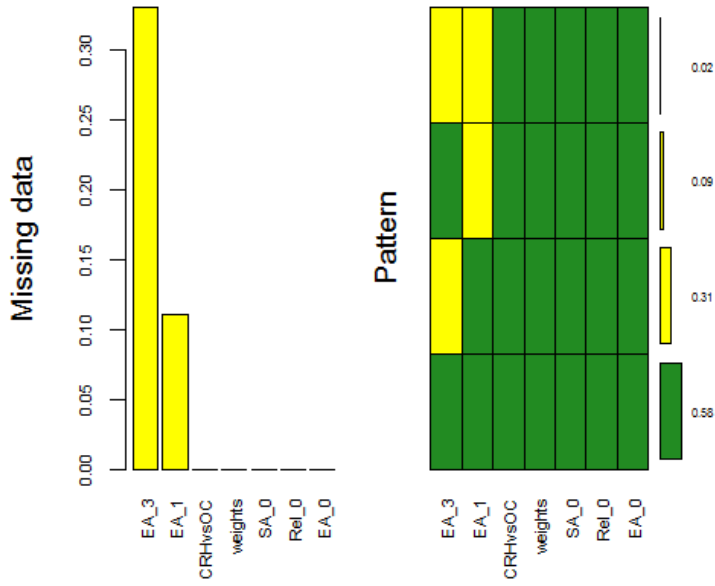
Residential Freshman Interest Group/Academic Residential Community vs. Off-Campus
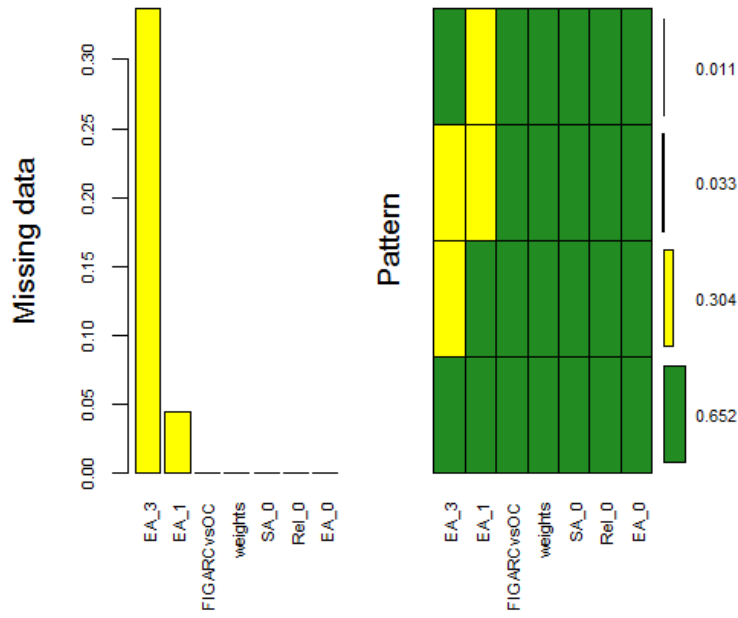
Resource Seeking (Original)

Conventional Residence Hall vs. Residential Freshman Interest Group/Academic Residential Community

Conventional Residence Hall vs. Off-Campus

Residential Freshman Interest Group/Academic Residential Community vs. Off-Campus

195
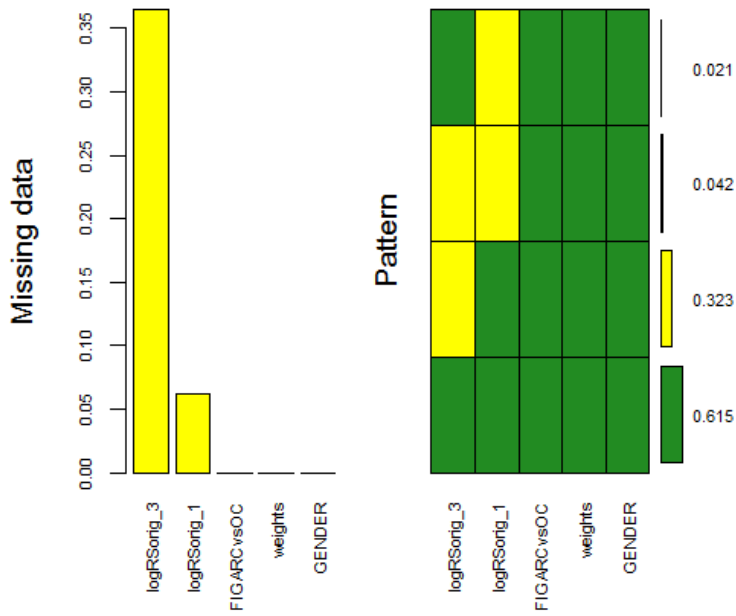
<u>Resource Seeking (Expanded)</u>

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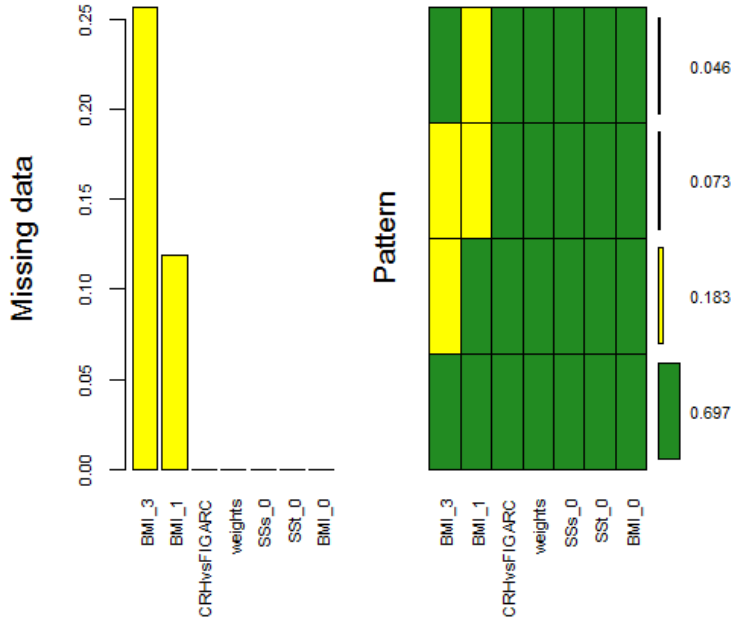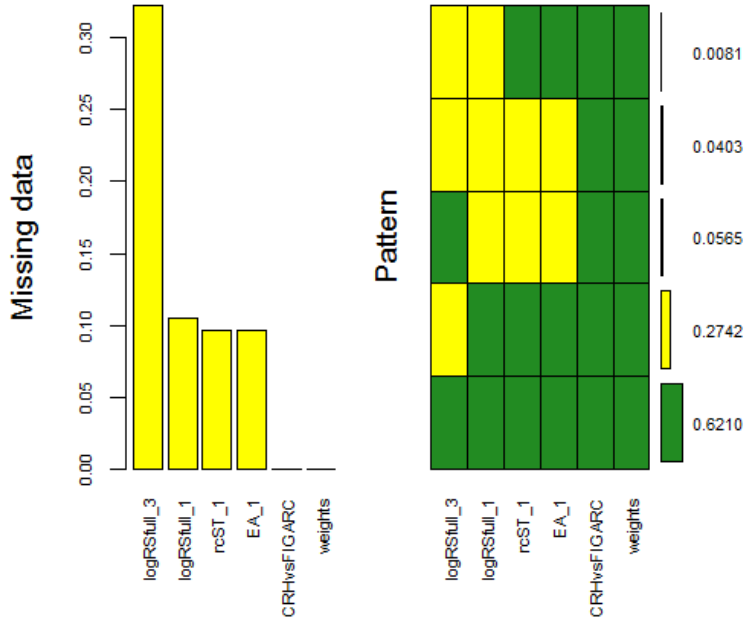

Conventional Residence Hall vs. Off-Campus
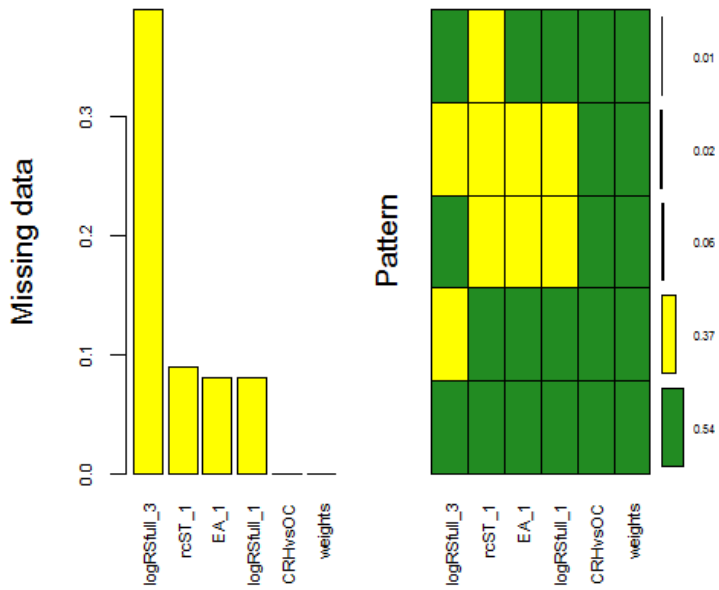
Residential Freshman Interest Group/Academic Residential Community vs. Off-Campus
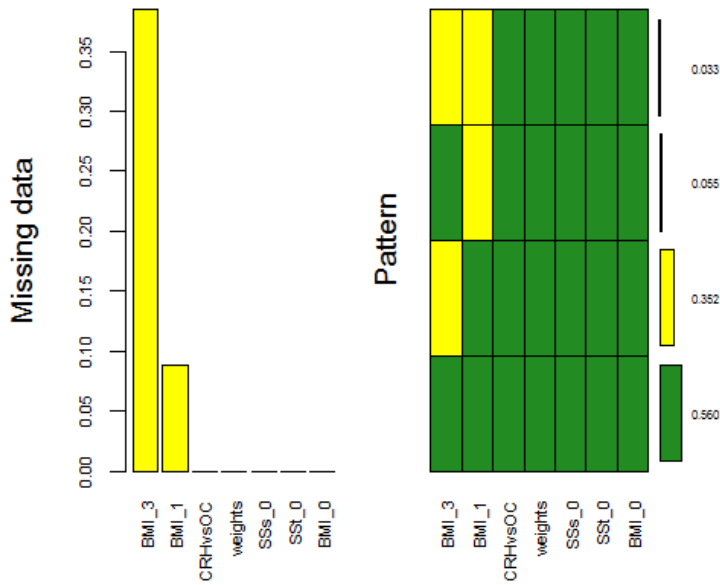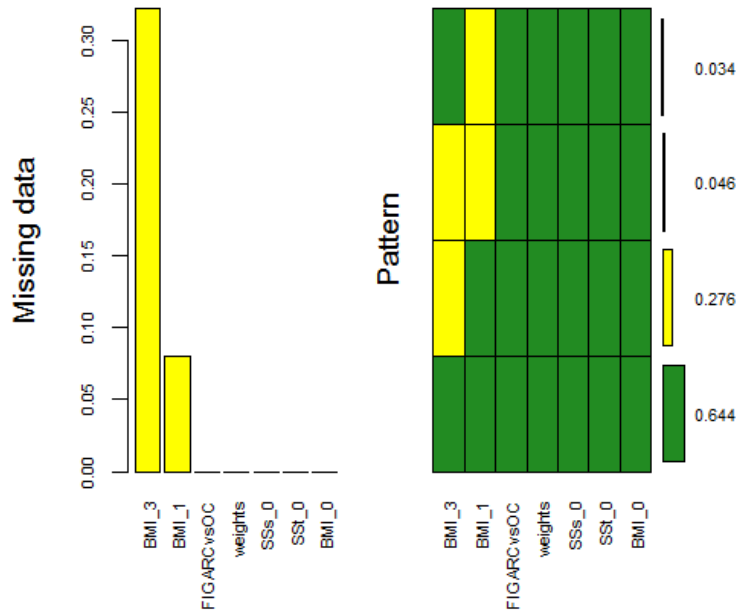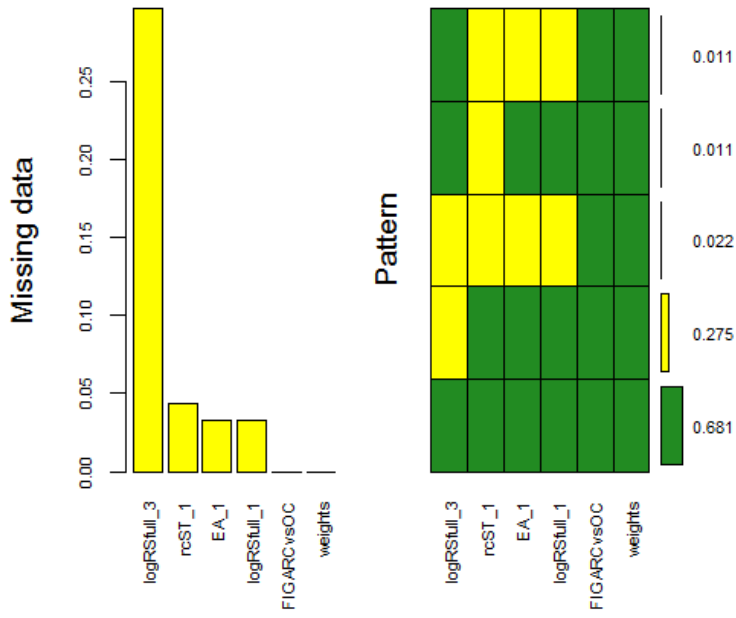

Social-Belonging

Conventional Residence Hall vs. Residential Freshman Interest Group/Academic Residential Community
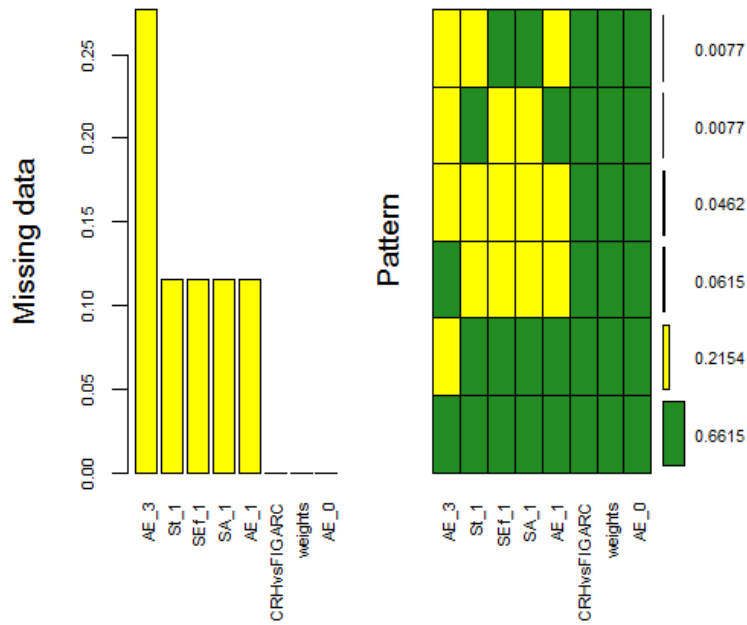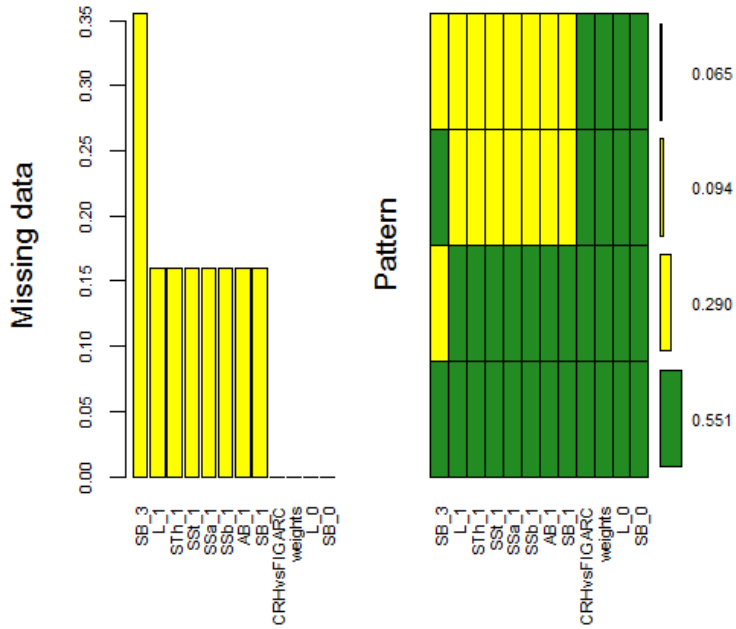
Conventional Residence Hall vs. Off-Campus
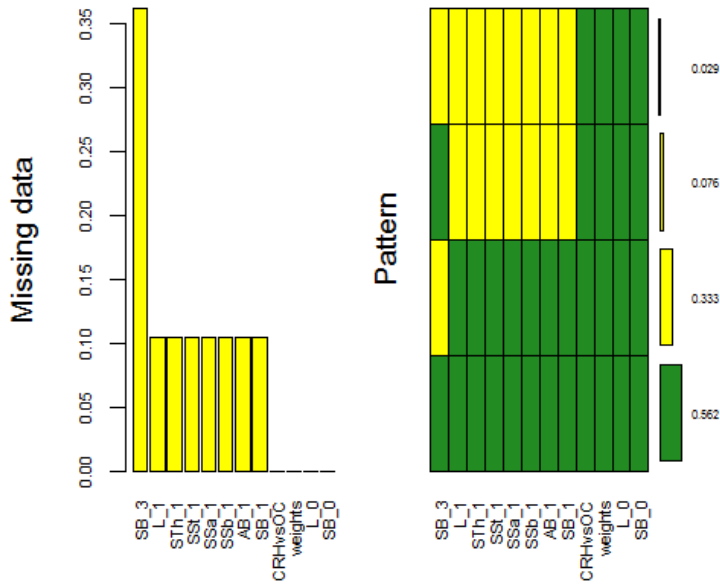
Residential Freshman Interest Group/Academic Residential Community vs. Off-Campus
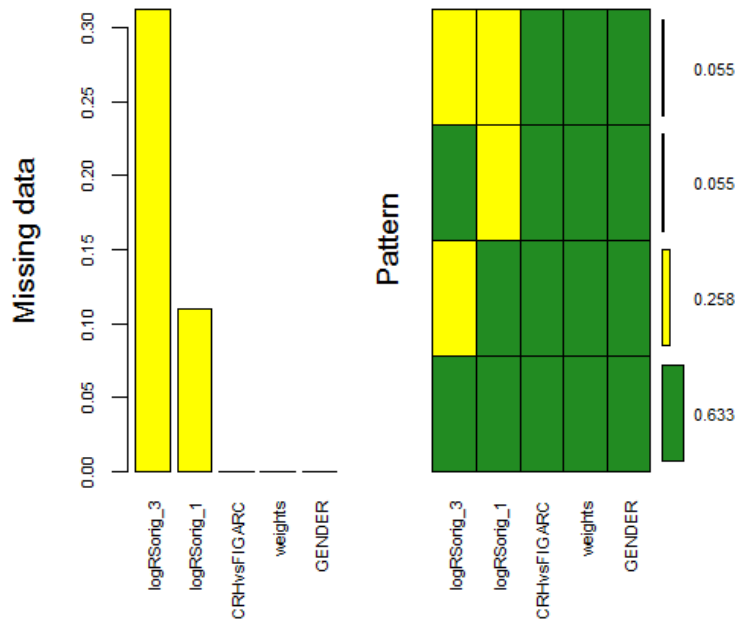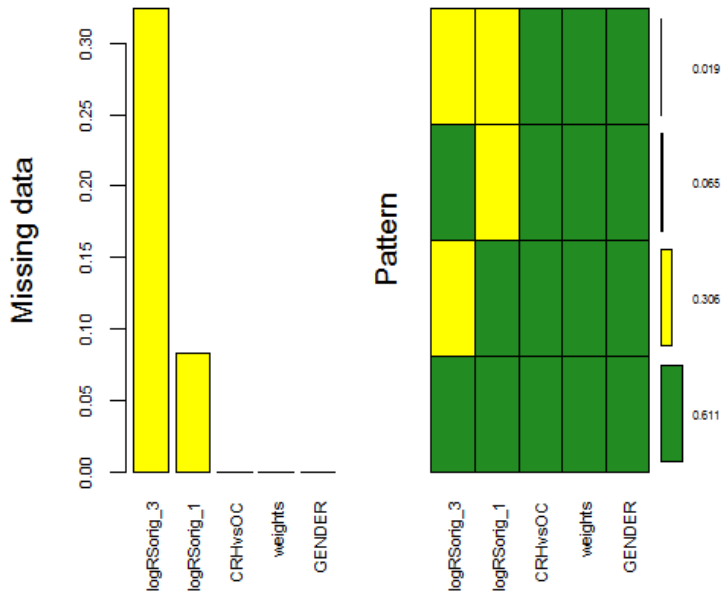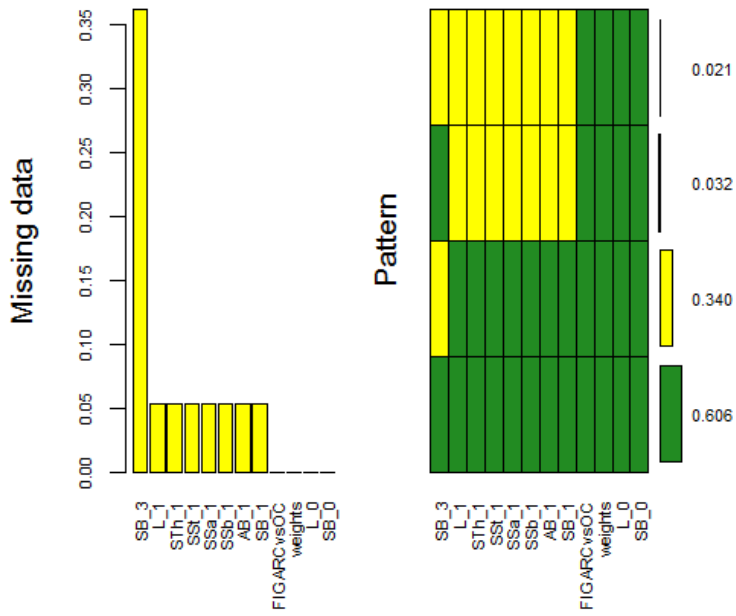
PREEXISTING DIFFERENCES

Preexisting Difference between Students in the Intervention and Control Conditions

Baseline Wave 0 Survey: Primary Constructs

	DE			SC			PPC			NPPC		
	n	*M*	*SD*	*n*	*M*	*SD*	*n*	*M*	*SD*	*n*	*M*	*SD*
LS	52	5.00	1.17	33	4.78	1.17	49	4.70	1.37	199	5.41	1.13
SEs	52	4.71	1.51	33	4.45	1.84	49	4.10	2.17	200	4.87	1.66
Sa	52	2.15	.86	33	2.41	1.03	49	2.53	1.17	198	2.26	.95
St	52	2.39	.66	33	2.51	.60	49	2.59	.75	199	2.41	.66
GH	52	3.86	.75	33	3.55	.62	49	3.68	.91	202	3.62	.76
I	52	.52	.52	33	.44	.57	49	0.62	.53	201	.55	.58
BMI	51	24.91	4.32	32	23.79	5.30	48	23.67	4.52	197	23.16	4.34
AE	52	4.96	1.30	33	5.30	1.20	49	5.26	1.23	200	5.32	1.32
EA	52	3.00	1.22	33	3.24	1.09	49	3.06	1.41	199	3.32	1.23
SB	52	4.62	1.53	33	4.40	1.37	49	4.39	1.76	201	4.57	1.44

	p-values					
	DE-SC	DE-PPC	DE-NPPC	SC-PPC	SC-NPPC	PPC-NPPC
LS	.413	.204	.024	.752	.005	<.001
SEs	.508	.080	.572	.370	.211	.006
Sa	.248	.052	.469	.560	.443	.085
St	.426	.138	.875	.598	.417	.090
GH	.077	.245	.044	.475	.667	.615
I	.481	.415	.760	.157	.277	.471
BMI	.266	.167	.013	.904	.455	.475
AE	.241	.248	.076	.890	.936	.772
EA	.382	.805	.098	.519	.735	.191
SB	.527	.460	.842	.978	.559	.466

Note: DE = difference-education, SC = standard-control, PPC = program-passive-control, NPPC = nonprogram-passive-control, LS = life satisfaction, SEs = self-esteem, Sa = sadness, St = stress, GH = general health, I = illness, BMI = body mass index, AE = academic enjoyment, EA = extracurricular activity, SB = social belonging.

Baseline Wave 0 Survey: Auxiliary Constructs

	DE			SC			PPC			NPPC		
	n	*M*	*SD*	*n*	*M*	*SD*	*n*	*M*	*SD*	*n*	*M*	*SD*
NtB	52	4.50	.86	33	4.28	.82	49	4.65	1.02	201	4.62	.94
AB	52	4.50	1.25	33	4.43	1.13	49	4.43	1.31	201	4.88	1.21
L	52	2.54	.71	33	2.71	.72	49	2.75	.87	200	2.52	.71
SSa	52	3.17	.73	33	3.23	.55	49	3.16	.74	201	3.34	.62
SSb	52	2.95	.65	32	2.76	.60	49	2.85	.88	200	2.96	.66
SSt	52	3.10	.63	33	3.03	.64	49	3.14	.69	202	3.22	.58
SA	52	3.14	.81	33	3.25	.82	49	3.11	.99	200	3.19	.85
SEf	52	5.76	1.24	33	6.00	.92	49	5.86	1.14	201	6.01	.90
STh	52	3.91	1.54	33	3.71	1.38	49	4.14	1.56	201	3.81	1.48
FI	55	3.20	1.52	34	3.18	1.80	49	3.08	1.63	195	6.06	1.52
SSSf	56	5.04	1.65	34	5.50	1.75	49	5.16	1.75	195	6.73	1.34
SSSs	52	7.29	1.63	33	7.09	1.84	49	6.90	2.04	196	7.24	1.66
R	52	2.75	1.69	33	2.59	1.74	49	2.21	1.49	195	2.54	1.74
MAE	52	2.56	2.20	33	2.64	2.26	49	3.22	2.45	199	2.99	2.17
VAE	52	1.63	1.91	33	1.12	1.83	49	1.55	1.93	199	1.64	1.77
STE	52	1.13	1.89	33	1.06	1.69	49	1.55	2.09	199	1.46	1.75
STi	52	6.18	5.10	33	6.98	5.44	49	4.85	3.96	199	6.31	5.15

p-values

	DE-SC	DE-PPC	DE-NPPC	SC-PPC	SC-NPPC	PPC-NPPC
NtB	.296	.412	.396	.079	.053	.845
AB	.816	.801	.046	.995	.054	.024
L	.323	.166	.806	.804	.170	.049
SSa	.670	.940	.094	.626	.376	.084
SSb	.210	.444	.977	.569	.133	.325
SSt	.527	.460	.842	.978	.559	.466
SA	.580	.852	.702	.477	.735	.544
SEf	.273	.588	.101	.545	.951	.353
STh	.554	.431	.678	.200	.721	.165
FI	.945	.701	<.001	.787	<.001	<.001
SSSf	.156	.664	<.001	.316	<.001	<.001
SSSs	.609	.259	.857	.621	.648	.218
R	.674	.114	.437	.326	.883	.226
MAE	.874	.133	.213	.242	.399	.509
VAE	.207	.818	.990	.296	.132	.765
STE	.855	.251	.256	.232	.247	.747
STi	.473	.182	.867	.059	.477	.067

Note: DE = difference-education, SC = standard-control, PPC = program-passive-control, NPPC = nonprogram-passive-control, NtB = need to belong, AB = academic-belonging, L = loneliness, SSa = social support: appraisal, SSb = social support: belonging, SSt = social support: tangible, SA = self-assurance, SEf = self-efficacy, STh = stereotype threat, FI = family income, SSSf = subjective social status (family), SSSs = subjective social status (self), R = religiosity, MAE =

moderate aerobic exercise, VAE = vigorous aerobic exercise, STE = strength training exercise, STi = study time.

Student Records

	DE			SC			PPC			NPPC	
	n	*%*		*n*	*%*		*n*	*%*		*n*	*%*
FG	57	54%		36	56%		49	55%		207	20%
Male	55	29%		35	26%		46	30%		185	27%
Race	55	53%		35	71%		46	61%		184	80%
AP	55	25%		35	31%		46	28%		185	51%
Loan	56	73%		36	58%		47	64%		169	41%
WS	56	38%		36	22%		47	19%		169	2%
nFIG	51	20%		35	11%		47	13%		193	26%
FIGARC	11	21%		5	14%		12	26%		63	32%
CRH	29	56%		17	47%		18	38%		103	52%
OC	12	23%		14	39%		17	36%		32	16%
	n	*M*	*SD*	*n*	*M*	*SD*	*n*	*M*	*SD*	*n*	*M*	*SD*
HS GPA	55	3.57	0.35	35	3.67	0.50	46	3.65	0.34	185	3.74	0.31
SAT	55	1062	187	35	1088	154	46	1069	179	185	1199	157

p-values

	DE-SC	DE-PPC	DE-NPPC	SC-PPC	SC-NPPC	PPC-NPPC
FG	.912	.941	<.001	.967	<.001	<.001
Male	.727	.883	.764	.641	.872	.644
Race	.080	.412	<.001	.324	.267	.008
AP	.538	.751	.001	.757	.034	.006
Loan	.140	.307	<.001	.610	.067	.007
WS	.127	.044	<.001	.731	<.001	<.001
nFIG	.318	.363	.355	.855	.073	.062
FIGARC	.158	.642	.104	.289	.002	.018
CRH	.167	.087	.472	.782	.018	.005
HS GPA	.137	.196	<.001	.777	.229	.084
SAT	.466	.828	<.001	.610	<.001	<.001

Note: DE = difference-education, SC = standard-control, PPC = program-passive-control, NPPC = nonprogram-passive-control, FG = first-generation, Male = is male, Race = is White or Asian, AP = transferred AP credit, Loan = received student loan, WS = received work-study, nFIG = participating in nonresidential academic community, FIGARC = residential freshman interest group/academic residential community, CRH = conventional residence hall, OC = off-campus, HS GPA = high school grade point average, SAT = best SAT/ACT score.

Preexisting Difference between First-Generation and Continuing-Generation Students

Baseline Wave 0 Survey: Primary Constructs

	FG			CG			
	n	*M*	*SD*	*n*	*M*	*SD*	*p*
LS	112	4.99	1.21	216	5.28	1.20	.046
SEs	113	4.44	1.86	216	4.80	1.70	.080
Sa	112	2.26	1.01	215	2.33	.97	.535
St	112	2.50	.70	216	2.41	.65	.242
GH	112	3.60	.82	218	3.69	.74	.303
I	113	0.61	.57	218	0.51	.56	.112
BMI	110	23.64	4.78	214	23.54	4.37	.848
AE	112	5.03	1.26	217	5.35	1.31	.032
EA	112	3.26	1.43	216	3.22	1.15	.777
SB	112	4.41	1.62	218	4.59	1.43	.304

Note: FG = first-generation, CG = continuing generation, LS = life satisfaction, SEs = self-esteem, Sa = sadness, St = stress, GH = general health, I = illness, BMI = body mass index, AE = academic enjoyment, EA = extracurricular activity, SB = social belonging.

Baseline Wave 0 Survey: Auxiliary Constructs

	FG			CG			
	n	*M*	*SD*	*n*	*M*	*SD*	*p*
NtB	112	4.54	.94	218	4.58	.94	.686
AB	113	4.43	1.26	217	4.87	1.20	.002
L	112	2.65	.72	217	2.54	.75	.191
SSa	113	3.27	.68	217	3.28	.64	.860
SSb	112	2.86	.71	216	2.95	.68	.284
SSt	113	3.13	.59	218	3.20	.62	.356
SA	113	3.23	.92	216	3.15	.83	.444
SEf	113	5.89	1.05	217	5.98	.98	.443
STh	113	4.01	1.56	217	3.80	1.44	.222
FI	115	3.67	1.83	215	5.49	2.00	<.001
SSSf	116	5.14	1.63	215	6.61	1.48	<.001
SSSs	112	7.14	1.86	215	7.20	1.67	.761
R	112	2.70	1.66	214	2.44	1.71	.197
MAE	113	2.89	2.30	217	2.92	2.19	.928
VAE	113	1.50	1.97	217	1.58	1.74	.718
STE	113	1.25	1.84	217	1.41	1.77	.423
STi	112	5.72	4.97	217	6.31	5.04	.309

Note: FG = first-generation, CG = continuing generation, NtB = need to belong, AB = academic-belonging, L = loneliness, SSa = social support: appraisal, SSb = social support: belonging, SSt = social support: tangible, SA = self-assurance, SEf = self-efficacy, STh = stereotype threat, FI = family income, SSSf = subjective social status (family), SSSs = subjective social status (self), R = religiosity, MAE = moderate aerobic exercise, VAE = vigorous aerobic exercise, STE = strength training exercise, STi = study time.

Student Records

	FG			CG			
	n	*%*		*n*	*%*		*p*
BP	120	65%		229	28%		<.001
Male	110	25%		207	29%		.842
Race	110	57%		207	79%		<.001
AP	110	29%		207	48%		.001
Pell	113	76%		192	34%		<.001
Loan	113	66%		192	45%		<.001
WS	113	24%		192	8%		<.001
Sch	113	89%		192	73%		.001
rHC	112	3%		209	10%		.033
nFIG	112	19%		209	22%		.494
FIGARC	67	31%		22	19%		.092
CRH	100	47%		64	57%		.766
OC	46	22%		27	24%		
	n	*M*	*SD*	*n*	*M*	*SD*	*P*
HS GPA	110	3.62	.32	207	3.73	.32	.003
SAT	110	1055	163	207	1194	163	<.001

Note: FG = first-generation, CG = continuing generation, BP = is in bridge program, Male = is male, Race = is White or Asian, AP = transferred AP credit, Pell = received Pell grant, Loan = received student loan, WS = received work-study, Sch = received scholarship, rHC = residential honors college, nFIG = participating in nonresidential academic community, FIGARC = residential freshman interest group/academic residential community, CRH = conventional residence hall, OC = off-campus, HS GPA = high school grade point average, SAT = best SAT/ACT score.

Preexisting Difference between Residential Groups

Baseline Wave 0 Survey: Primary Constructs

	OC			CRH			FIGARC		
	n	*M*	*SD*	*n*	*M*	*SD*	*n*	*M*	*SD*
LS	67	4.88	1.25	155	5.22	1.21	88	5.38	1.18
SEs	67	4.64	1.85	156	4.69	1.76	88	4.93	1.63
Sa	67	2.62	1.06	154	2.21	0.89	88	2.26	1.03
St	67	2.67	0.75	155	2.41	0.66	88	2.32	0.62
GH	69	3.49	0.92	154	3.70	0.74	89	3.71	0.66
I	69	0.61	0.62	155	0.48	0.54	88	0.56	0.58
BMI	67	23.93	4.99	154	23.44	4.41	85	23.04	4.01
AE	68	5.25	1.36	156	5.24	1.22	87	5.34	1.38
EA	68	3.13	1.34	155	3.28	1.19	87	3.29	1.18
SB	69	4.21	1.61	155	4.66	1.48	88	4.66	1.41

	p-values		
	OC-FIGARC	OC-CRH	CRH-FIGARC
LS	.013	.059	.329
SEs	.306	.863	.291
Sa	.022	.005	.737
St	.002	.009	.329
GH	.080	.059	.961
I	.584	.114	.291
BMI	.226	.458	.508
AE	.671	.959	.569
EA	.434	.395	.983
SB	.064	.041	.987

Note: OC = off-campus, CRH = conventional residence hall, FIGARG = residential freshman interest group/academic residential community, LS = life satisfaction, SEs = self-esteem, Sa = sadness, St = stress, GH = general health, I = illness, BMI = body mass index, AE = academic enjoyment, EA = extracurricular activity, SB = social belonging.

Baseline Wave 0 Survey: Auxiliary Constructs

	OC			CRH			FIGARC		
	n	*M*	*SD*	*n*	*M*	*SD*	*n*	*M*	*SD*
NtB	69	4.48	0.92	155	4.62	0.89	88	4.51	0.99
AB	68	4.40	1.28	155	4.71	1.20	89	4.98	1.24
L	68	2.88	0.78	155	2.50	0.72	88	2.50	0.70
SSa	68	3.15	0.72	155	3.34	0.63	89	3.30	0.65
SSb	68	2.71	0.82	154	3.00	0.63	88	2.94	0.67
SSt	69	3.26	0.68	155	3.15	0.60	89	3.16	0.60
SA	68	2.98	0.97	154	3.27	0.83	89	3.19	0.78
SEf	68	5.91	1.13	155	5.99	0.81	89	5.95	1.14
STh	68	4.16	1.54	155	3.69	1.46	89	3.95	1.49
FI	69	3.90	1.86	156	4.98	2.13	85	5.24	2.12
SSSf	69	5.43	1.29	157	6.20	1.69	85	6.32	1.81
SSSs	67	7.03	1.87	154	7.24	1.60	86	7.24	1.93
R	67	2.57	1.73	153	2.46	1.65	86	2.75	1.79
MAE	69	2.72	2.22	154	2.95	2.19	87	3.28	2.25
VAE	69	1.46	1.96	154	1.58	1.77	87	1.66	1.83
STE	69	1.51	1.90	154	1.39	1.83	87	1.33	1.84
STi	68	5.35	3.81	155	6.12	5.23	87	6.71	5.27

	p-values		
	OC-FIGARC	OC-CRH	CRH-FIGARC
NtB	.792	.289	.405
AB	.003	.085	.092
L	.002	<.001	.960
SSa	.164	.046	.612
SSb	.036	.004	.513
SSt	.297	.209	.914
SA	.141	.020	.442
SEf	.779	.545	.747
STh	.370	.030	.197
FI	<.001	<.001	.363
SSSf	.001	.001	.588
SSSs	.455	.414	.987
R	.511	.671	.209
MAE	.124	.487	.271
VAE	.517	.649	.773
STE	.560	.661	.821
STi	.092	.288	.376

Note: OC = off-campus, CRH = conventional residence hall, FIGARG = residential freshman interest group/academic residential community, NtB = need to belong, AB = academic-belonging, L = loneliness, SSa = social support: appraisal, SSb = social support: belonging, SSt = social support: tangible, SA = self-assurance, SEf = self-efficacy, STh = stereotype threat, FI = family income, SSSf = subjective social status (family), SSSs = subjective social status (self), R =

religiosity, MAE = moderate aerobic exercise, VAE = vigorous aerobic exercise, STE = strength training exercise, STi = study time.

Student Records

	OC			CRH			FIGARC		
	n	*%*		*n*	*%*		*n*	*%*	
BP	75	57%		167	38%		91	31%	
FG	73	37%		164	39%		89	25%	
Male	71	18%		154	33%		84	26%	
Race	70	73%		154	72%		84	68%	
AP	71	30%		154	42%		84	49%	
Pell	70	64%		148	47%		81	40%	
Loan	70	50%		148	53%		81	52%	
WS	70	17%		148	11%		81	14%	
Sch	70	86%		148	73%		81	83%	
nFIG	71	0%		167	33%		91	16%	
DE	12	16%		29	17%		11	12%	
SC	14	19%		17	10%		5	5%	
PPC	17	23%		18	11%		12	13%	
NPPC	32	43%		103	62%		63	69%	
	n	*M*	*SD*	*n*	*M*	*SD*	*n*	*M*	*SD*
HS GPA	71	3.66	0.33	154	3.63	0.32	84	3.81	0.30
SAT	71	1123	167	154	1124	167	84	1198	194

	p-values		
	OC-FIGARC	OC-CRH	CRH-FIGARC
BP	.001	.006	.227
FG	.092	.766	.023
Male	.244	.024	.269
Race	.500	.904	.495
AP	.016	.072	.328
Pell	.003	.020	.257
Loan	.820	.641	.825
WS	.544	.254	.644
Scholarship	.616	.040	.099
nFIG	.983	.982	.005
DE	.104	.472	.219
SC	.018	.005	.832
PPC	.002	.018	.170
HS GPA	.006	.517	<.001
SAT/ACT	.008	.984	.002

Note: OC = off-campus, CRH = conventional residence hall, FIGARG = residential freshman interest group/academic residential community, BP = is in bridge program, Male = is male, Race = is White or Asian, AP = transferred AP credit, Pell = received Pell grant, Loan = received student loan, WS = received work-study, Sch = received scholarship, nFIG = participating in

nonresidential academic community, DE = difference-education, SC = standard-control, PPC = program-passive-control, NPPC = nonprogram-passive-control, HS GPA = high school grade point average, SAT = best SAT/ACT score.

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