The Consequences of Mismatch between Students and Colleges

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1. Introduction

Academic mismatch is everywhere! You can find it in newspapers, in the trade press, in blogs and in scholarly articles and books. A student mismatches by attending a college of much higher, or much lower, quality and suggested by her ability. The literature refers to the first case as overmatching (think over-achieving) and the second case as undermatching (think under-achieving). Until the last five years or so, the literature focused almost exclusively on overmatch, particularly on overmatch induced by racial and ethnic preference policies at selective colleges. More recently, undermatch has moved into the spotlight as a result of the widely-read studies by Bowen, Chingos and McPherson (2009) and Roderick et al. (2008). Our work encompasses both types of mismatch.

Despite all the attention to the prevalence and causes of mismatch, we have little evidence on the causal effects of mismatch. Mismatch matters because it may affect academic outcomes and thereby labor market and other life outcomes later on. For example, an overmatched student might flounder and drop out or perhaps they might rise to the challenge and do better than they otherwise would have done. More deeply, the effects of mismatch (if any) inform our understanding of the human capital production function. On the policy side, learning about the consequences of mismatch informs the design and operation of state university systems with diversified college quality portfolios.

Despite the current ubiquity of the mismatch conversation, we lack credible estimates of the effects of mismatch constructed using a national representative sample of undergraduates. As its primary substantive contribution, this paper applies a "selection on observed variables" identification strategy to the data from the U.S. National Longitudinal Survey of Youth 1997

Cohort to provide such estimates. These data cover the most recent cohort for which sufficient time has elapsed since the completion of high school to allow for a serious analysis of the effects of mismatch not only on post-secondary outcomes but on initial labor market outcomes as well. Though always somewhat heroic, the NLSY-97 contains a vast enough array of relevant conditioning variables to make our selection-on-observed variables assumption at least moderately compelling. We hope to conduct a formal sensitivity analysis examining the robustness of our findings to remaining selection on unobserved variables in a future version of the paper.

As our second major contribution to the mismatch literature, we examine a wide variety of outcome measures other than simply degree completion. With a couple of important recent exceptions discussed in greater detail below, the existing mismatch literature focuses primarily on degree completion as the outcome of interest. Bowen and Bok's (1998) finding of no apparent impact of on degree completion for the overmatched students in the "College and Beyond" data suggested to us that these students might find other ways to deal with better-prepared colleagues and a high pressure environment. For example, they might follow the increasingly common path of increased time-to-degree, as highlighted in Bound, Lovenheim and Turner (2010). Or they might follow scholarship athletes at some colleges in taking easy courses and completing easy majors, as suggested in journalistic exposés such as Steeg et al. (2008) and Ann Arbor News (2008). Or they might transfer to another school that represents a better match. This version of the paper examines degree completion and transfer outcomes; future versions will also examine time-to-degree and major choice within broadly defined categories, as well as earnings.

Our third major contribution to the literature arises from our incorporation of students who start at two-year colleges (with aspirations to a four-year degree) in our analysis. Dillon and

Smith (2013) document the large amount of undermatch among students who start their post-secondary careers at a two-year college. Leaving these students out, as the literature commonly does, misses a potentially important aspect of the mismatch story, especially as Reynolds (2012) and others emphasize the long odds of eventual bachelor's degree attainment associated with this path. This aspect of the analysis is not yet realized in the current version of the paper.

Conceptually and empirically, we frame our analysis of mismatch as an analysis of college quality interacted with student ability. Thus, as a natural byproduct of our analysis, and as our fourth major contribution, we replicate and extend the earlier analyses of the college quality main effect in Black, Daniel and Smith (2005), but using the NLSY-97 cohort. This allows us to compare estimates of the impact of college quality for the NLSY-79 and NLSY-97 cohorts obtained from the same econometric setup and with sets of conditioning variables as close as the data allows. This aspect of the analysis is not yet realized in the current version of the paper.

Less important, but still worth noting, we contribute to the literature via both our measures of student ability and college quality which, as we discuss in detail in Section 3, embody less measurement error than those used in other papers. Also, in future versions, we plan to perform wider set of subgroup analyses both in terms of student characteristics and in terms of college institutional characteristics.

To preview our results, we find substantial amounts of both overmatch and undertmatch in the NLSY-97 cohort. Dillon and Smith (2013) argue that this mismatch results largely from the choices of students rather than the choices of college admissions offices. Our examination of the effects of ability, college quality and their interaction on college completion reveals strong main effects of college quality and ability, which comports with almost all of the existing literature. Most strongly, college quality raises completion probabilities for weak students, strong students, and those in between. In contrast, we find little evidence of a casual effect of mismatch. Our analysis of transfer behavior reveals patterns partially, but not fully, consistent with mismatch. We look forward with great curiosity to examining other outcomes in our ongoing work.

We structure the remainder of the paper as follows: Section 2 reviews the literature on the college quality and mismatch. Section 3 describes the many wonders of our data, with particular attention to the construction of our student ability and college quality measures, which together yield our mismatch measure, and to the outcomes we consider. Section 4 lays out our conditioning variables and justifies our identification strategy. Section 5 presents our econometric framework. Section 6 displays and interprets our findings to date. Finally, Section 7 summarizes our work so far.

2. Literature

We frame the literature on college mismatch as a subset of the literature that examines the effect of college quality on academic, labor market and other outcomes. In particular, the mismatch literature allows the effect of college quality to vary according to the ability of the student. Our brief survey here organizes the literature by identification strategy and focuses in detail on the most recent studies and the ones that, in our view, illustrate the key issues involved. Though the literature has become international in recent years, with studies for Canada such as Milla (2012) and Betts et al. (2013), for the United Kingdom, such as Hussain, McNally and Telhaj (2009) and Chevalier and Conlon (2003) as in developing countries, such as Bordón and Braga (2013) for Chile, we limit ourselves to the US literature. We also restrict ourselves to studies of mismatch at the undergraduate level, putting to the side the tendentious literature on law school quality (see Sander and Taylor (2012) and the references therein) and business school quality. Black, Daniel and Smith (2005) provide links to the earlier college quality literature. We are unaware of a good recent survey paper though one is surely warranted.

Recent studies relying on a selection on observed variables identification strategy to look at the college quality main effect include Black and Smith (2004), Black, Daniel and Smith (2005), and Black and Smith (2006). Bowen and Bok (1998) and Bowen, Chingos and McPherson (2009) examine college quality and mismatch. Turner (2002) Black, Daniel and Smith (2005), Dale and Krueger (2011) and Hershbein (2013) provide evidence on the persistence of college quality effects estimated under selection on observed variables.

Another set of papers relies on instrumental variables strategies, sometimes embedded in structural models. These papers include Light and Strayer (2000), Arcidiacono (2005), and Long (2008). Future versions of the paper will give our thoughts on the plausibility of the instruments employed on this paper and on the interpretation of the obtained estimates.

Hoekstra (2009) looks at the effect of attending a flagship university using a regression discontinuity design. This study finds large positive effects on labor market outcomes, but the setup complicates their interpretation. First, as is well known, the impacts strictly apply only to students at the margin of admission. In this sense, they inform about mismatch, as they implicitly compare the weakest students admitted to the flagship with the strongest students not admitted to the flagship. A positive finding in that sense represents strong evidence against mismatch. However, the context provides not a sharp discontinuity but rather a fuzzy one, meaning that the effect properly applies only to the "compliers" at the discontinuity, those students whose enrollment in the flagship depends on crossing the admissions threshold. Potential statistical discrimination issues further complicate the interpretation. If employers, at least initially, rely primarily on college attended as a proxy for ability, then the short run impact at the discontinuity will overstate both the longer-run impact and the impact for enrollees away from the discontinuity.

Dale and Krueger (2002, 2011) adopt a pair of provocative and original identification strategies that tries to get around the problem that students and their parents, as well as the college admissions officers who read their applications, have information that the researcher does not by making use of the partial revelation of that information in students' application choices and the resulting college acceptance decisions. One strategy, which they call their self-revelation model, conditions on the average SAT score of colleges to which the student applied as well as the number of colleges to which they apply. The second strategy compares students accepted into (roughly) the same sets of colleges who make different choices regarding where to attend. We have concerns about both strategies that we will describe in greater detail in future versions of the paper.

Most recently, Arcidiacono, Aucejo and Hotz (2013) use the variation induced by California's Proposition 209, which attempted to ban racial preferences in university admissions, to study mismatch effects on minority graduation in STEM fields, while Arcidiacono, Aucejo and Spenner (2012) study the same phenomenon using administrative data from Duke University.

In future versions of the paper we will engage the literature in greater depth. At this point our takeway from the literature: First, college quality improves both academic and labor market outcomes, though this relationship may have important non-linearities at the upper end. Second, mismatch may affect some outcomes, such as major choice but any negative effects on college completion appear small, if indeed they exist at all.

3. Data

3.1. NLSY

We use the National Longitudinal Survey of Youth 1997 Cohort (NLSY97) data, which samples the population of Americans born between 1980 and 1984. The first interview was in 1997 with follow-up interviews each year since. The majority of the sample graduated high school and made their college choice between 1999 and 2002. 87 percent of the un-weighted sample graduated high school or got a GED. Of these high school graduates, 38 percent started at a four-year college after high school. We focus in this version of the paper on students who start at a 4-year college. Appendix Table 1 lists our sample restrictions and the associated sample losses. The NLSY97 sample includes both a representative cross-section and an over-sample of black and Hispanic youths. We combine these samples in our analyses. We use probability of inclusion (in the overall NLSY97 sample) weights, constructed by the NLSY, to combine the two samples, and also to control for differing sampling and response rates in different regions of the U.S. and by age, gender, race-ethnicity groups.

One of the main strengths of the NLSY97 data lies in the rich set of individual and family covariates it provides. Using the restricted access geocode data provides additional information on the identities of colleges attended and allows the use of contextual information based on the respondent's residential location. Appendix Table 2 defines the variables we use in our analysis. Many of the variables we use have modest amounts of item non-response. Rather than do listwise deletion of observations when an independent variable is missing, which would cumulatively result in massive sample loss, we recode missing values to zero and include an indicator variable for missing values in our multivariate analyses.

We mostly use standard variables and variable definitions that do not require additional discussion here. Exceptions are the constructed ability, college quality and mismatch variables considered in detail in the next section, and the NLSY97 measures of family income and wealth. The NLSY measures these variables at a single point in time, namely the 1997 interview. As a result, they get measured at different ages for different respondents. In addition, they include only income and wealth for the household in which the respondent resides. Thus, they will miss parental income and wealth entirely for older respondents with their own households as well as the income and wealth of the non-custodial spouse in the case of parental divorce. Even without these issues, our ideal measures would include the stock of wealth available at the time of the college choice as well as expected future income and wealth. The available measures fall well short of this ideal, which has implications for how we interpret the estimates from these variables in our multivariate analysis.

3.2. Ability

Our primary measure of student ability is the Armed Forces Vocational Aptitude Battery (ASVAB). The ASVAB is designed for applicants to the U.S. military and was taken by most of the NLSY97 respondents as part of a norming exercise. NLSY respondents took the ASVAB during the first wave of the survey in 1997 and those who took the test were paid $75 for their time. 78% of the sample, and 84% of respondents who started at a 4-year college, completed all portions of the test.

The ASVAB has twelve components, covering both the sorts of skills measured by the SAT such as algebra, geometry, vocabulary, and reading comprehension and other skills such as electronics knowledge and spatial reasoning. The ASVAB is a computer adaptive test, meaning that the difficulty of the questions asked in the latter part of each section of the test depends on how well respondents do on the initial questions in the section. The score for each section reported by the NLSY depends on both the number of questions answered correctly and the difficulty of those questions as estimated from an earlier sample of test takers. The ASVAB offers a somewhat richer measure of ability than the SAT or ACT score, and should be less influenced by variation in preparation because there was nothing riding on this test for the NLSY participants.[1] The ASVAB score also has the useful feature that colleges do not observe it. We can therefore capture some of the college mismatch generated by colleges having incomplete information.

When survey participants took the ASVAB, they ranged in age from 12 to 18, younger than most of the larger population taking the test. We adjust the scores for age at testing and then take the first principal component of the 12 section scores as our primary measure of ability, which we call ASVAB1. We calculate each respondent's percentile within the sample distribution of college-bound NLSY97 respondents, weighted by their probability of inclusion in the survey.

As shown in Appendix Table 3, the first principal component explains 60% of the total variance in test scores across the 12 sections. The first component places the highest weight on subjects like those on the SAT (or ACT): arithmetic, word knowledge, and paragraph comprehension. Not surprisingly giving the loadings, the correlation between ASVAB1 and the respondent's SAT or ACT score equals 0.81.

[1] The ASVAB test is not a straightforward measure of "innate" ability because it includes the influences and training that the student has had up to the point she takes the test. See Neal and Johnson (1996) for a more thorough discussion of what the ASVAB test is measuring. We do not mind if the ASVAB also measures intrinsic motivation, as argued by Segal (2012). More broadly, we use the term "ability" quite agnostically to mean the set of skills, innate or otherwise, that students possess around the time of the college choice.

The second component, which we call ASVAB2, explains a further 11% of the variance. It places the most weight on the two timed sections of the test: numerical operations and coding speed. Cawley, Heckman, and Vytacil (2001) find that the first two principal components of the ASVAB score both predict later earnings in the NLSY 1979 sample. To construct our measure of mismatch, for which we need a single measure of ability, we use only ASVAB1. However, we will explore conditioning on ASVAB2 in future versions of the paper.

While we prefer our ASVAB-based ability measure to the SAT or ACT scores commonly relied on in the literature, it remains an imperfect measure of ability. Although the ASVAB tests a richer variety of skills than most standardized tests it still does not capture all the abilities that make for a strong college student. Even if it did attempt to measure all relevant abilities, the score from a single ASVAB test would be an imperfect measure of ability because some students will perform above or below their usual level on the day of the test.

3.3. College quality

We construct a multifaceted index of college quality by combining measures related to selectivity and college resources. In particular, we combine data from the U.S. Department of Education's Integrated Post-Secondary Education Data System (IPEDS) and U.S. News and World Report, both from 2008.[2] The components of our college quality index are mean SAT score (or mean ACT score converted to the SAT scale) of entering students, percent of applicants rejected, the average salary of all faculty engaged in instruction, and the faculty-student ratio.

[2] US News and IPEDS collect many of the same statistics and for the same college in the same year the numbers are often identical. US News has average SAT or ACT scores for the students at a number of schools that do not report test scores to IPEDS. However, US News focuses on selective schools and excludes 2-year colleges altogether. Combining data from the two sources gives us the most complete sample of colleges. We use US News data to fill in average SAT and ACT scores and faculty/student ratios when these statistics are missing from IPEDS. Rejection rates and faculty salaries come only from IPEDS.

Our faculty-student ratio includes only undergraduate students and faculty who do not teach exclusively in graduate or professional schools within universities. Most of the NLSY97 respondents started college between 1999 and 2002, somewhat earlier than our college quality measures. 2008 is the earliest year for which we could obtain US News data and the first year that IPEDS reported faculty-student ratios focused only on undergraduates. The other components of our college quality measure are quite stable between 2000 and 2008, so we feel the improved data available in 2008 outweigh the measurement error from observing college quality in a later year.

Following Black and Smith (2004), we use the first principal component across these four measures of quality as our quality index. Like Black and Smith (2006), we view our index as providing an estimate of latent college quality, which we view as continuous and one-dimensional. Within this framework, combining multiple proxies for college quality into a single index measures latent quality with less error than using a single proxy (such as the average SAT score of the entering class) or the categorical quality ratings (e.g. from Barron's) used in much of the literature. Our index corresponds well to a priori notions of relative quality. For example, taking one corner of one state at random, the University of Michigan lies at the 93rd percentile, Michigan State at the 74th, Wayne State at the 36th, and Eastern Michigan at the 28th. Appendix Table 4 presents the loadings. At the same time, our measure does not capture differences in the quality that different students experience within the same university due to, for example, quality difference across fields of study or participation in honors programs.

This 4-factor quality index is a good measure of the quality of at least somewhat selective 4-year colleges. However, some 4-year colleges and many 2-year colleges do not report the average SAT or ACT scores for their entering classes, often because they do not require these tests as part of their applications. Our baseline measure of college quality, which we only construct for colleges with all four quality measures, disproportionally misses less selective schools. To address this problem, we also construct an alternative 6-factor measure of college quality that includes an indicator for colleges that do not report SAT or ACT scores (setting the average SAT scores to zero for those schools). This alternative index also includes an indicator for admitting all applicants; that is, for having a rejection rate equal to zero. This 6-factor college quality measure is our baseline measure for our analysis combining 2-year and 4-year college starters. We designed this measure to better capture college quality across both 2-year and 4-year colleges, but it also allows us to include students starting at 4-year schools that do not report SAT scores. Failure to report SAT scores and open admission policies both have negative weights in our college quality factor analysis, so these new schools are mostly in the lower part of the quality distribution. In this version, we do not make use of the 6-factor index.

Table 2 present descriptive statistics for 4-year college starters by college quality quartile using the 4-factor index.

3.4 Measuring mismatch

We employ three alternative measures of mismatch. Our primary measure of mismatch combines the student ability and college quality measures just described. We calculate the college's quality percentile across all four-year institutions in the United States included in the IPEDS, weighted by student body size.[3] Because we weight the quality percentile by student body size, a college in the n^{th} percentile is the college that a student in the n^{th} percentile of the ability distribution would attend if there were perfect assortative matching of students and colleges. We consider students mismatched when they deviate substantially from this type of matching. When

[3] Our measure of student body size is full-time equivalent undergraduates.

considering both 2-year and 4-year college starters we calculate student ability percentiles across all 2- and 4-year starters in the NLSY97 sample and calculate weighted college quality percentiles using all 2-year and 4-year colleges in IPEDS and the 6-factor college quality measure.

In practice, substantial gaps between a student's ability percentile and her college's quality percentile are quite common. Table 1A gives the joint distribution of student ability and college quality, including only 4-year college starters. Students concentrate along the diagonal, which indicates a good match, but there are also many mismatched students. The three upper right cells, corresponding to high ability students at low quality colleges, account for 12.5% of the sample, while the three lower left cells, corresponding to low ability students at high quality colleges, account for 12.9%. A comparison of Table 1A to Table 4 of Black and Smith (2004) reveals (perhaps surprisingly given the recent policy focus on mismatch) no dramatic changes in the joint distribution between the NLSY79 and NLSY97 cohorts.

In much of the following analysis we categorize students as overmatched, well-matched, or undermatched for their college based on the difference between their ability percentile and their college quality percentile. Figure 1 reveals an approximately normal distribution for this difference. We consider students to be undermatched or overmatched, as appropriate, if their percentile difference exceeds 20. These cutoffs assign about a quarter of the sample to each mismatch category. Using binary indicators for mismatch simplifies the analysis and presentation, but loses some information relative to directly studying the differences in the ability and college quality measures. In future versions, we will examine the sensitivity of our results to changes in the cutoff used to define the binary mismatch indicators.

We construct our second mismatch measure in the same way as the first, but using student SAT score as the measure of ability and the average SAT score of the entering class as the measure of college quality. This measure links us somewhat to the wider literature, which tends to focus on these specific variables (or on discretized versions of them). Table 1B presents the joint distribution using the SAT-based variables. This table reveals less extensive mismatch, as measured by the fraction in the six corner cells, presumably because colleges observe the student's SAT score directly but only observe proxies for ASVAB1. This measure of mismatch does not appear in the current version.

Our third mismatch measure compares the student's SAT score to the inter-quartile range of SAT scores at the student's college. This measure captures, in a crude but important way, the notion that being a bit different from the average means something different at a college with a very heterogenous (in terms of ability) student body than it means at a college with a very homogenous student body. To our knowledge, we are the first in this literature to consider variance in student ability in defining mismatch. This measure of mismatch does not appear in the current version.

Other important studies in the literature, such as Roderick et al. (2008), Bowen et al. (2009), and Smith et al. (2012) create their measures of mismatch by making tables with student test score bins on one axis and college quality bins on the other. For each student test score bin, they then determine the highest quality bin with a high probability of admission. Students in the highest bin get labeled well-matched, with undermatch then defined by the distance (measured in bins) between the bin of the college the student actually enrolled in and the well-matched bin. Relative to these measures, our primary measure employs better (in the sense of less measurement error) measures of both college quality and ability. Our first two measures also have the feature that it is possible for everyone to be well-matched without violating institutional enrollment constraints. This is not the case with the other measures in the literature; for every student to be well-matched by those measures would require a vast expansion in the enrollment capacity of more selective schools. We think this is an unattractive feature. House (2013) surveys the literature on mismatch measures in (much) greater detail, and demonstrates by applying multiple measures to a common data set that the amount of mismatch varies widely depending on the particular measure adopted.

Table 3 presents descriptive statistics separately for undermatched, well-matched and overmatched students.

3.5. Outcomes

As noted in the introduction, we view our analysis of outcomes other than just completion of a BA or BS as an important component of our contribution. In the current version we examine four outcomes: graduation within five years, dropout, transfer to a higher quality college, and transfer to a lower quality college.

Table 4 presents summary statistics for these three outcomes and a few others for our sample. Among our 2,101 4-year college starters, 28 percent graduate in four years (or less), 46% graduate in five years and 61% graduate in six years or more. Thus, our data contain substantial variation in time-to-degree. Of the remainder, the majority left school without a four-year degree, though small fractions remain in school at a 2-year or 4-year institution or left the data before the final year. As Bound, Lovenheim and Turner (2010) point out, time-to-degree has increased over time and now represents in important implicit source of variation in both the direct cost and the opportunity cost of obtaining a degree.

Table 5 breaks down the numbers by college quality and student ability. The data clearly show both that, unconditionally, completion probabilities increase in college quality. Relatively less important than the college quality gradient, but still clear in the data, conditional on college quality degree completion probabilities generally increase in ability. Our multivariate analysis below shows that these patterns hold conditionally as well.

Table 5 also includes information on transfers. We define a transfer up as a move that increases college quality by at least 10 percentile points on our scale; a transfer down has a similar definition but in the other direction. Consistent with the somewhat earlier cohort studied by Goldrick-Rab (2006), we find a great deal of transfer behavior among our students. As with time-to-degree this represents a change from earlier cohorts; for example, Light and Strayer (2000) note that in the earlier NLSY-79 cohort, they do not find enough transfers to bother with in their analysis. Transfer down (which includes transfer to any 2-year college) occurs substantially more often than transfer up and occurs surprisingly often even among students in the lower half of the quality distribution. Transfer up declines strongly with initial quality and shows hints of mismatch, with high ability students more likely to transfer up.

As foreshadowed in the introduction, future versions of this paper will also present impacts on some or all of the following: four-year graduation and six-year graduation (in place of the single five-year graduate measure considered at present), employment during college, college major category, college grades, enrollment in graduate programs, completion of a graduate program, debt, drug and alcohol use, and earnings.

4. Identification

We adopt a “selection on observed variables” strategy in this paper. More formally, we assume that we have a sufficiently rich conditioning set that the remaining variation in college quality that serves to identify our effects is uncorrelated with the error term in the outcome equation. To accomplish this, we need two things. First, we need the observed covariates included in our model to capture either directly or as proxies all the factors that affect *both* (not either but both) college quality choice and the outcomes we study. We consider that case shortly. Second, in order to avoid identification via functional form, we need there to exist variables not included in our model that vary college quality choices in ways unrelated to the unobserved component of the outcomes. Put differently, we need instrumental variables to exist, even though we do not observe them, as they produce the variation in college quality we implicitly (want to) use in our estimation.

The estimates in the current version condition on the following variables: sex, race / ethnicity indicators, number of other children in the household, indicators for parental wealth quartiles, indicators for parental education categories, indicators for census region, indicator for rural residence, log median income at the census tract level, percent of adults with a 4-year college degree at the census tract level, log of average 2-year in-state tuition, log of average 4-year in-state tuition, and indicators for having a matched public 4-year college (in the same state) and matched private 4-year college within 50 miles. All of these variables affect either undermatch or overmatch or both in our earlier study of the determinants of mismatch, Dillon and Smith (2013). In most cases, we also have clear predictions regarding how we expect them to affect the outcomes we study. For example, parental education affects college completion independently of its effect on college quality choice if more educated parents can provide better advice on how to succeed in college and/or if parental education proxies for the otherwise unobserved component of pre-college academic preparation (i.e. it is correlated with the measurement error in student ability). The two indicators for a well-matched college nearby likely represent the least obvious inclusions for most readers. Indeed, in the spirit of various papers using distance to college as an instrument, such as Card (1995) and Kane and Rouse (1995), we might think of these as instruments rather than as conditioning variables. We include them as conditioning variables because we worry that they correlate with living at home, which likely has its own treatment effect on the outcomes we study, particularly completion. At the same time, we do not condition on living at home because it represents an intermediate outcome..

Note that we do not condition here on SAT score, ASVAB2, or high school grades, which we view as alternative proxies for student ability, broadly conceived. This increases the interpretability of the estimates at the cost of making the selection on observed variables assumption less plausible at the margin. In future versions, we will explicitly discuss the results obtained when including these additional variables (short answer, they do not change much). We will also consider constructing an ability super-index that includes the ASVAB tests and the SAT components and the high school GPA.

Should we expect our conditioning set to suffice for selection on observed variables to hold, at least approximately? We can make this case in two ways. First, we can think about what we know from existing theory and empirical evidence regarding what we should condition on, and then ask whether our conditioning set contains those things, or at least compelling proxies for those things. We clearly want to condition on family resources, both intellectual and financial. More money makes many things about college easier, including longer time-to-degree, more frequent visits home, not having to work during school and so on. More money may also affect the college quality choice, particularly for students without a good match nearby and/or a good college nearby. Parental education will correlate with their knowledge of the college choice process and of how to succeed at college in both the institutional and academic senses. More educated parents may also have a stronger taste for education, and so may push their children harder to finish. As in Becker and Lewis (1973), parents face a quality-quantity tradeoff. As such, number of other children may reflect both resources and preferences. We include direct measures parental resources, education and number of children. We also expect that our census tract income and education variables will both help with measurement error in the parental resource variables and proxy for primary and secondary school quality as well as peer pressure and expectations. We also clearly want to condition on student ability, and we do so flexibly. As noted below, we plan to condition on additional student characteristics in future work.

The second way to think about our covariate sets asks whether the marginal covariates make any difference to the estimates. In the framework of Heckman and Navarro (2004), we might imagine that there exist multiple unobserved factors that we need to condition on in order to solve the problem of non-random selection into colleges of different qualities. We can then think of our conditioning variables as proxies for those factors. In a world with just one unobserved factor, as we increase the number of proxies in our conditioning set, the amount of selection bias in our estimates should decrease to zero. The same holds with two unobserved factors so long as we keep adding proxies for both. Turning this around, if we observe that the estimates stabilize as we increase the richness of the conditioning set, this provides evidence that we are doing a good job of proxying for the unobserved factors, unless there exists an additional unobserved factor uncorrelated with our covariates. We note that Black, Daniel and Smith (2005), using the earlier 1979 NLSY cohort data and a covariate set similar to ours find such stabilization of the college quality main effect they estimate. As described below, we plan a similar exercise in future versions of this paper.

As noted above, in addition to having the correct covariates in our model, we need the correct kind of variation (and, ideally, lots of it) conditional on those covariates. In our view, the literature suggests plenty of exogenous variation conditional in college quality choices conditional on our observed covariates. In this context, what normally represents a sad feature of this literature, namely the consistent finding that many students, parents, and high school guidance counselors have little or no idea about how to choose a college, provides aid and comfort for our identification strategy. For example, the literature on "one-offs" (the occasional strong student at a weak high schools), such as the important recent contribution by Hoxby and Turner (2012), shows the difference a small amount of reliable information can make in college choice (and college mismatch) for many students. Similarly, the literature provides many examples of small behavioral economics tricks having non-trivial effects on college choices. Here we have in mind findings that in Pallais (2012) that you can change college choices by changing the number of colleges to which students can send their ACT scores for free or the finding that having H & R Block help with the federal financial aid form can have real effects on college-going, as shown by Bettinger et al. (2009). Finally, many students explicitly choose among colleges, at least at the margin, for reasons unlikely to be strongly related to outcomes, such as because of the local amenities or because their best friend from high school is going there and so on.

Future versions of the paper will incorporate an even richer set of covariates as well as selected interaction terms using the current conditioning variables. Our list of additional conditioning variables to explore includes variables related to parental income, high school grades and course-taking, health and appearance (such as body mass index), religiosity variables, home environment variables, and various measures of non-cognitive skills. As noted above, we plan an analysis in which we examine the sequence of estimates that results from adding groups of additional variables to our existing specification, as in Black, Daniel and Smith (2005, Table 4). Following Heckman and Navarro (2004), should the estimates stabilize in the course of this process, we will view this as suggestive evidence that we have done a good job of satisfying the selection on observed variables assumption in our context, subject always to the proviso that some other unobserved factor, completely unrelated to our observed covariates, may yet lurk in the shadows of the error term and generate bias in our estimates.

What, if anything, can we say about the nature of any remaining bias due to selection on unobserved variables? Putting aside mismatch for the moment and thinking just about the college quality main effect, two worries usually arise. First, we might expect students, their parents, and college admissions officers have access to information on student quality that we, the researchers, do not. To the extent that those unobserved bits affect admissions in the expected way, with better unobserved bits leading to admission to higher quality colleges conditional on the observed bits, and worse unobserved bits the reverse, then we would expect an upward bias in the estimated effect of college quality because it proxies in part for unobserved student quality. Second, we might worry about measurement error in college quality, as in Black and Smith (2006). Though our use of a quality index based on multiple proxies should help with this issue relative to the common strategy of using only the average test scores of the entering class, some measurement error likely remains, and we would expect it to push the estimated coefficient toward zero. Of course, we have no basis for arguing that these two biases cancel out in practice.

Now think about the interaction of college quality and student ability. If we overstate the effect of a high quality college for all students, then overmatched students will look better than they should relative to other students of the same ability. Similarly, undermatched students will look relatively worse than they should. Thus, upward bias in the estimated effect of college quality should lead us to understate the effects of overmatch and to overestimate the effects of undermatch. Measurement error in ability, in contrast, should attenuate our estimates of both overmatch and undermatch.

We are considering doing an analysis along the lines of Altonji, Elder and Taber (2005) or Ichino, Mealli and Nannicini (2008) to quantitatively examine the sensitivity of our estimates to any remaining selection on unobserved variables in a future version of the paper.

5. Econometric framework

To determine whether the data provide evidence of negative (or positive?) effects of mismatch, we want to look flexibly at the relationship between ability and quality and the outcomes of interest, conditional on other variables. We can think of several alternative econometric frameworks that would allow us to do so. This section describes the one we adopt for this version of the paper; at the end of the section we briefly note some alternatives we plan to explore.

In the spirit of starting simple, in this paper we estimate linear models of outcomes on conditioning variables and on indicators for combinations of college quality quartile and ability quartile. In order to avoid the notoriously tricky "dummy variable trap" we include indicators for only 15 of the 16 possible combinations, with ability quartile one and college quality quartile one as the omitted category. We also include a number of conditioning variables as described in the preceding section. Dressing up our approach in econometric finery, we can view it as a partially linear model in which we non-parametrically estimate the impacts of ability and quality while conditioning parametrically on the other variables. The indicator variables for combinations of ability and quality become non-parametric once we promise to increase the number of categories (but not too quickly!) on those happy occasions when our sample size increases. Note that we cannot proceed with the even simpler approach of a linear model with ability, quality and mismatch entered parametrically because of perfect collinearity.

As we examine only binary outcomes, we estimate linear probit models. Formally, we estimate the conditional probability function as:

$$\Pr(Y=1 \mid A, Q, X) = \Phi\left(\beta_0 + \sum_{m=2}^{16} \beta_m D_m + \beta_X X_i + \varepsilon_i\right). \tag{1}$$

In (1), Y denotes the binary outcome of interest, A denotes student ability, Q denotes college quality, X denotes a vector of other covariates and $m = a(q-1)$ where $a \in \{1,2,3,4\}$ denote quartiles of student ability and $q \in \{1,2,3,4\}$ denote quartiles of college quality. Due to the non-linearity of the probit, we present average derivatives (or finite differences in the case of binary covariates) of the conditional probability rather than coefficient estimates as we regard these as more interpretable.

In future work, we will examine an alternative version of (1) that uses a flexible polynomial in ability and quality in place of the indicators. That approach has the benefit of capturing any within-quartile mismatch missed by the current approach. It comes at the cost of requiring a careful specification search in order to avoid the oft-observed instability of estimates based on higher order polynomials away from the center of the data. We may also examine a multi-treatment weighting estimator like that in Cattaneo (2010), treating the 16 combinations of ability quartile and quality quartile as separate treatments.

6. Results

6.1 College quality

Our replication and updating of Black, Daniel and Smith's (2005) study of the main effect of college quality must await the next version of the paper.

6.2 Mismatch

Tables 6 and 7 present our estimates of equation (1), with Table 6 covering graduation and dropout and Table 7 covering transfers up and down. The values in the tables represent average derivatives (sometimes called mean marginal effects) or finite differences (in the case of discrete covariates) and so have the interpretation of the expected change in the probability the dependent variable equals one given the other conditioning variables. For example, the value of -0.123 for "male" in the graduation column in Table 6 means that males students have a 12.3 percentage point lower probability of graduation than females conditional on ability, college quality, and the other variables in the model. Figures 2 to 5 present the average derivatives on the indicators for combinations of student ability and college quality in graphical form. These figures complement the tables by making the patterns in the average derivatives easier to see at the cost of leaving out the standard errors and the point estimate.

We begin with our findings for degree attainment and, among those estimates, with the effects of college quality and student ability. We find two important patterns in the data. First, holding ability (and the other variables in the model) constant, increasing the quality of the initial college in which a student enrolls leads to a higher probability of obtaining a degree within five years. For example, for student in the third quartile of ability ("ability 3" in the table), the effects start at 0.161 for college quality quartile one. This represents the estimated increase in the probability of degree completion relative to a student in the first ability quartile and the first quality quartile as those students constitute the omitted category. Moving up to college quality quartile two, the effect for ability quartile three rises to 0.180, and from there it rises to 0.210 and 0.299 in college quality quartiles three and four, respectively. A similar monotone increasing pattern holds for every ability quartile, though the individual changes in the average derivative from one quality quartile to the next do not always statistically differ from one another. Thus, the college quality main effect found in (almost all of) the literature persists in our data, and does so for students of all ability levels.

Our second important pattern emerges when we hold look at how the effect of ability varies for each value of college quality. For example, for college quality quartile three moving along the ability levels from one to four reveals effects equal to 0.106, 0.164, 0.210 and 0.268. For the other quality quartiles, we find a similar pattern of monotone increases, with but three modest exceptions when going from ability quartile three to ability quartile four. In the case of quality quartile one, this likely just represents sampling variation as the number of very high ability students at very low quality schools is quite low. In the case of quality quartile one, the difference in estimated average derivatives is substantively quite minor, just 0.299 versus 0.281. The more puzzling case arises in ability quartile two, where the effects for ability quartiles three and four equal 0.180 and 0.143. The overall picture, though, consists of generally monotone increasing effects of ability for every college quality quartile. In the main, more able students have a higher probability of completing a degree than low ability students.

What do these patterns mean for mismatch? Mismatch would predict non-monotone patterns for the middle quartiles of both ability and quality. For example, for the third ability quartile, it would predict increasing degree completion probabilities up through the third quartile of college quality as undermatch decreased followed by a decrease in the fourth quartile due to overmatch. For the lowest quartile of ability, mismatch predicts decreasing graduation probabilities with college quality due to increasing overmatch, and for the lowest quartile of quality, it predicts decreasing probabilities of completion with ability due to increasing overmatch. Similar, but reverse predictions, hold for the top quartiles of ability and college quality. Other than the small number of relatively minor departures from monotonicity described in the preceding paragraph, the patterns predicted by the mismatch hypothesis simply do not appear in our data.

We can quantify the lack of evidence for mismatch in our college completion results in two ways. First, because our model nests a model with only indicators for college quality quartiles and indicators for ability quartiles, we can test the implicit restrictions in going from one to the other. Put differently, we can test the restrictions on our model required to reduce it to a model with only main effects in ability and college quality. The p-value (multiplied by 100) from this test appears in the last row of Table 6, and the corresponding chi-squared statistic appears in the penultimate row. We obtain a p-value of 0.52, indicating that the restrictions required for the main-effects-only model cause very little trouble for the data.

Second, we can look to Table 8, which compares the observed degree completion rate with the degree completion rate implied by our model for the counterfactual world of perfect matching. We obtain this value by predicting degree completion for every observation but with their college quality quartile recoded to match their ability quartile. Based on our model, we find that degree completion *falls* very slightly if we eliminate mismatch, moving from 51.6% to 51.3%. The fall occurs because the positive effect of moving higher ability students at low quality colleges to their matched quality level just falls short of the negative effect of moving lower ability students away from high completion rate high quality colleges to their matched quality level. Because it omits any effects of within-quartile mismatch, this exercise does not quite capture the entirety of mismatch, but we might expect mismatch effects to increase non-linearly in the extent of the ability-quality difference, in which case we capture most of what we want to capture in this estimate.

Although our dropout outcome measure does not exactly equal one minus our degree completion measure, it comes close enough that the dropout results tell essentially the same story as the completion results, so we do not describe them in detail here.

We now turn to our analysis of transfer behavior; for this outcome we do find some bits of evidence consistent with mismatch. The first column of estimates in Table 7 corresponds to model (1) with transfer up as the dependent variable, while the second column of estimates corresponds to transfer down. For this table, we consider only transfers that result in a change in college quality of ten percentile points in one direction or the other. We have repeated the analysis using a zero cutoff (i.e. even moving by one percentile point up or down the quality scale counts) and we obtain qualitatively similar findings. In future versions of the paper, we will also consider a 20-point cutoff and will present estimates for a lateral transfer (i.e. within 10 percentile points of the initial college on either side) dependent variable.

Consider first our results for transferring to a higher quality college, and start once again by looking at how the estimates vary by ability for each quartile of college quality. In the lowest college quality quartile, we find some evidence consistent with mismatch, in that students in the lowest ability quartile are substantively and statistically less likely to transfer to a higher quality college than students in the other three ability quartiles. The pattern is non-linear though, rather than strictly increasing in ability as mismatch would predict. In the second college quality quartile, we again see non-linear effects, which again coincide to some extent with what the mismatch hypothesis would predict in that students in the highest ability quartile have a much larger probability of transferring up than students in the other ability quartiles. For the third college quality quartile, we again find a non-linear pattern somewhat consistent with mismatch in the sense that the highest ability students have a lower probability of transferring up than the rest. In the top quartile of college quality, we find a (partly or perhaps wholly) mechanical effect of much lower probabilities of transfer up relative to the omitted group. Inconsistent with the mismatch hypothesis, this effect looks about the same for the first, third and fourth ability quartiles (and the second quartile has an empty cell problem).

Looking now from the perspective of a particular ability quartile and varying the college quality quartile, we again find mixed results somewhat but not completely consistent with mismatch. At the lowest ability level, we find a much higher probability of moving up when starting in the lower half of the quality distribution than in the upper half. Mismatch would say that low ability students should never move up, so our pattern fails to match (pun intended) the prediction in that sense. At the second ability quartile, we lack evidence for the highest quality quartile, but the probability decreases monotonically up to that point. For ability quartile three, mismatch would predict larger probabilities of transfer up when starting in quality quartiles one and two and a zero probability when starting in the highest quality quartile. What we find is that the probability monotonically decreases. It also monotonically decreases for students in the highest ability quartile, which in their case comports with the mismatch prediction. Taken together, the results for transferring to a better college provide a bit of support for mismatch around the edges and a lot of support for a strong (and partly mechanical) negative main effect in college quality. Students who start at stronger colleges have a lower probability of transferring up, regardless of their ability level.

Overall, for the transfer up model as for the completion model, we cannot reject the null of only ability and quality main effects. In this case, as shown at the bottom of Table 8, the p-value equals 0.73. In Table 8, we find a non-trivial change in the estimated probability of upward transfer overall, from 7.0% in the data to 4.3% with mismatch removed. Based on our point estimates, mismatch, particularly initial undermatch, leads to upward transfers later on, transfers that presumably come at some cost to the student and (often) the taxpayer in terms of more time spent in school and less time spent in the labor force.

Finally, consider transfer down to a lower quality college. Our results in this case display some surprising patterns. As always, we start by looking at variation in the estimated effects by student ability conditional on college quality. Mechanically, not many students at the lowest quality quartile transfer down. At the second quality quartile, students in the lower half of the ability distribution have higher probabilities of downward transfer. Mismatch would predict this for the lowest quartile but also for the top half of the distribution. In the third quality quartile, everyone transfers down a lot relative to other groups, even students in the upper ability quartile, though a bit less so in their case. We do not have a clear explanation for this pattern. Perhaps big state schools predominate here, and perhaps those schools do a poor job of helping students who struggle. At the top quality colleges, students in the lowest ability quartile transfer down a lot, but not other students. This non-linear pattern partly comports with the mismatch hypothesis.

Now we consider the results from the perspective of varying quality within student ability quartiles. In the lowest ability group, the relative probability of downward transfer increases in college quality from the first quality quartile to the third, but then falls again from the third to the fourth, going from 0.177 to 0.146. Mismatch would predict the upward trend, but not the drop at the top. Students in ability quartile two have the largest probabilities of downward transfer at quartiles two and three of college quality. Again, this partially but not fully comports with mismatch. Students in ability quartile three have their highest downward transfer probability in college quality quartile three, exactly the opposite of what mismatch would predict, and they are less likely to transfer down from the top quality quartile than any other, again not what mismatch would predict. Finally, students in the top quartile are relatively most likely to transfer down in the third quality quartile.

Overall, we once again fail to reject the null of only main effects in student ability and college quality for the transfer down outcome, in this case with a p-value of 0.75. In Table 8, we see a modest *uptick* in downward transfers in a matched world relative to the observed data, with the predicted value of 28.8% exceeding the observed value by 1.9%. Our analysis of downward transfers raises some interesting questions, but the data once again provide at best mixed evidence consistent with mismatch.

Future versions of the paper will include a brief discussion of the estimates on the conditioning variables in the models presented in Tables 6 and 7.

6.3 Other college characteristics

Our analysis including both 2-year and 4-year starters, as well as our analysis using only students who start at a public university (where selection on observed variables is more plausible due to extensive reliance on simple admissions formulae) and our analysis interacting the college quality variable with the heterogeneity of student ability (e.g. the inter-quartile range of student SAT scores) at the college, must await the next version of the paper.

6.4 Subgroups

Our analysis of subgroups defined by sex, race, family income, parental education, whether or not the students attends a public college, and variance of student ability at the chosen college must await the next version of the paper.

6.5 Sensitivity to conditioning variables

Our analysis of the sensitivity of the estimates to changes in the conditioning set must await the next version of the paper.

7. Sensitivity analysis

Our analysis of the sensitivity of the results to any remaining selection on unobserved variables must await the next version of the paper.

8. Summary and conclusions

This paper examines the effects of college quality and student ability on academic and (in future versions) labor market outcomes. We use the rich data from the NLSY-97 and adopt a "selection on observed variables" identification strategy. In the current version, we examine only a limited set of outcomes: degree completion, college dropout, and transfer to a higher quality college or a lower quality college. We find strong evidence that college quality increases the probability of degree completion, and similarly strong evidence that student ability increases it as well. We do not find any evidence of the interactive effects of college quality and student ability predicted by the mismatch hypothesis on the outcomes we examine here.

We end this draft with two caveats: First, our results consider only mismatch at the undergraduate level. Our results may not generalize to other contexts, such as law schools, that provide students with fewer dimensions on which to respond to an environment that proves too challenging, or not challenging enough. In law school, for example, the student cannot change majors, or easily reduce their course load. For this reason, mismatch, particularly overmatch, might have very different overall effects in that context than in ours.

Second, this paper considers what we might call academic mismatch. Other types of mismatch between students and their undergraduate institutions represent an important omission from most of the literature (all of it inside economics and much of it outside as well). Perhaps the most obvious concerns mismatch in terms of social class or socio-economic status, or what an economist might prefer to call (at the cost of losing some nuance in interpretation) family resources. The recent Armstrong and Hamilton (2012) *Paying for the Party* book and their related academic work emphasize this form of mismatch, as does Tom Wolfe (2004) in his novel of college life entitled *I Am Charlotte Simmons*. Because mismatch on social class will likely correlate with academic mismatch, it represents a potentially confounding treatment in our context.

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Consequences of Mismatch Tables
August 24, 2013

Table 1: **Joint distribution of college quality and ability—NLSY97, four-year starters**

	College Quality Quartiles				
Ability Quartiles	1st Quartile (lowest)	2nd Quartile	3rd Quartile	4th Quartile (highest)	Total
1st Quartile (lowest)	9.1 (45.4) [34.4]	5.7 (28.7) [22.9]	3.2 (16.0) [12.5]	2.0 (9.9) [8.5]	(100.0) (N=531)
2nd Quartile	8.0 (30.9) [30.3]	7.1 (27.5) [28.3]	6.5 (25.3) [25.5]	4.2 (16.4) [18.3]	(100.0) (N=548)
3rd Quartile	5.2 (19.9) [19.8]	7.6 (28.9) [30.3]	7.5 (28.4) [29.1]	6.0 (22.8) [25.9]	(100.0) (N=509)
4th Quartile (highest)	4.1 (14.6) [15.5]	4.6 (16.4) [18.4]	8.4 (30.1) [33.0]	10.9 (38.9) [47.3]	(100.0) (N=513)
Total	[100.0] [N=585]	[100.0] [N=547]	[100.0] [N=506]	[100.0] [N=463]	100.0 N=2,101

Each cell contains the overall percentage, (the row percentage), and [the column percentage]. College quality is measured by the 4-factor index. Ability is measured by the first principal component of the ASVAB scores. Percentages are weighted as described in the text. Observation counts are unweighted.

Table 2: **Average characteristics of students by college choice, four-year starters**

	College Attendees	College quality quartile 1, lowest	2	3	4, highest
N	2,101	585	547	506	463
Male	45%	42%	43%	46%	48%
White	77%	75%	75%	82%	77%
Black	11%	17%	13%	8%	5%
Hispanic	6%	7%	7%	4%	7%
Other (not white)	6%	1%	5%	6%	11%
Household members age 18 or under	2.2	2.3	2.2	2.1	2.2
ASVAB 1 percentile	53%	41%	48%	58%	66%
ASVAB 2 percentile	51%	47%	50%	50%	58%
High school GPA percentile	54%	45%	51%	58%	63%
SAT percentile	53%	36%	47%	59%	71%
Household wealth in 1997	$184,385	$127,251	$166,649	$205,868	$249,675
Wealth quartile 1 (lowest)	10%	13%	8%	8%	9%
Wealth quartile 2	18%	25%	20%	13%	12%
Wealth quartile 3	28%	29%	28%	30%	22%
Wealth quartile 4 (highest)	45%	33%	44%	49%	58%
No parent completed high school	2%	4%	3%	2%	1%
At least one parent grad. high sch.	17%	22%	22%	14%	8%
At least one parent has some college	26%	31%	26%	21%	24%
At least one parent completed college	55%	44%	49%	63%	67%
Northeast region	21%	13%	16%	22%	36%
South region	30%	35%	25%	31%	30%
Midwest region	31%	35%	36%	36%	18%
West region	17%	17%	23%	11%	16%
Rural	18%	29%	14%	18%	9%
Median income in census tract	$37,110	$33,091	$36,619	$37,929	$41,318
% Adults w/college deg. in tract	21%	18%	20%	22%	24%
Avg. 2-year in-state tuition	$1,633	$1,660	$1,515	$1,706	$1,652
Avg. 4-year in-state tuition	$3,146	$3,010	$3,001	$3,253	$3,339
Matched public 4-year in 50 mi	60%	55%	61%	56%	66%
Matched private 4-year in 50 mi	70%	57%	70%	72%	83%

Notes: This table describes the characteristics of students at each college quality quartile. For example, the third row shows the percent of students attending each college type who are male. All results are weighted as described in the text. Ability percentiles are among 4-year college starters, with the ASVAB measures adjusted by age when taking the test. In-state tuition is measured in the year each student graduated from high school, deflated to 1997 dollars.

Table 3: **Average characteristics of students by match quality, four-year starters**

	College Attendees	Very Overmatched	Well-matched	Very Undermatched
N	2,101	510	1,007	584
Male	45%	37%	44%	51%
White	77%	66%	75%	88%
Black	11%	18%	12%	5%
Hispanic	6%	8%	6%	4%
Other (not white)	6%	9%	6%	2%
Household members age 18 or under	2.2	2.2	2.2	2.2
ASVAB 1 percentile	53%	32%	52%	70%
ASVAB 2 percentile	51%	56%	53%	44%
High school GPA percentile	54%	48%	54%	59%
SAT percentile	53%	42%	52%	63%
Household wealth in 1997	$184,385	$172,727	$194,083	$177,264
Wealth quartile 1 (lowest)	10%	15%	9%	8%
Wealth quartile 2	18%	20%	17%	18%
Wealth quartile 3	28%	26%	28%	28%
Wealth quartile 4 (highest)	45%	40%	47%	47%
No parent completed high school	2%	4%	2%	1%
At least one parent grad. high sch.	17%	16%	16%	18%
At least one parent has some college	26%	27%	25%	26%
At least one parent completed college	55%	53%	57%	55%
Northeast region	21%	31%	21%	13%
South region	30%	33%	33%	24%
Midwest region	31%	20%	29%	44%
West region	17%	15%	17%	18%
Rural	18%	13%	17%	23%
Median income in census tract	$37,110	$39,223	$37,627	$34,801
% Adults w/college deg. in tract	21%	23%	22%	19%
Avg. 2-year in-state tuition	$1,633	$1,623	$1,615	$1,671
Avg. 4-year in-state tuition	$3,146	$3,208	$3,170	$3,065
Matched public 4-year in 50 mi	60%	68%	69%	38%
Matched private 4-year in 50 mi	70%	83%	73%	57%

Notes: This table describes the characteristics of all college attendees (in the first column) and of students in each mismatch category. For example, the third row shows the percent of all students and of students in each match category who are male. All results are weighted as described in the text. Ability percentiles are among 4-year college starters, with the ASVAB measures adjusted by age when taking the test. In-state tuition is measured in the year each student graduated from high school, deflated to 1997 dollars.

Table 4: Summary of outcomes, four-year starters

	Respondents	Percent
4-year College starters	2,101	
Graduate in 4 years or less	596	28%
Graduate in 5 years	384	18%
Graduate in 6 or more years	321	15%
Leave school without a BA	610	29%
Still in 4-year college as of last interview	64	3%
Still in 2-year college as of last interview	35	2%
Not in most recent survey wave	91	4%
Of 4-year college starters		
Transfer to a higher quality college	92	4%
Transfer to a lower quality college	514	24%
Transfer to a similar or unknown college	239	11%
Never transfer	1,165	55%

Categories are mutually exclusive. Respondents who left college while still participating in the survey are counted as graduated or left without BA. Respondents who were in school as of their last interview but have not participated in the most recent waves of the survey are counted as out of survey.

Table 5: Summary of outcomes by ability and college quality quartile

	Count	Graduate within 5 years	Drop out	Transfer to a higher quality college	Transfer to a lower quality college
Quality 1, ability 1	237	27%	48%	8%	26%
Quality 1, ability 2	173	33%	44%	15%	26%
Quality 1, ability 3	101	46%	38%	10%	18%
Quality 1, ability 4	74	38%	39%	16%	22%
Quality 2, ability 1	163	39%	38%	8%	35%
Quality 2, ability 2	153	42%	35%	6%	32%
Quality 2, ability 3	147	53%	24%	7%	23%
Quality 2, ability 4	84	49%	28%	12%	24%
Quality 3, ability 1	83	40%	43%	3%	41%
Quality 3, ability 2	133	53%	24%	6%	32%
Quality 3, ability 3	140	61%	21%	3%	33%
Quality 3, ability 4	150	65%	20%	8%	29%
Quality 4, ability 1	48	57%	27%	1%	34%
Quality 4, ability 2	89	73%	12%	0%	22%
Quality 4, ability 3	121	72%	16%	1%	18%
Quality 4, ability 4	205	71%	20%	1%	23%

Percentages are weighted as described in the text. Observation counts are unweighted. Transferring to a lower quality college includes transferring to a two-year college.

Table 6: Effect of mismatch on college outcomes, four-year starters

	Graduate within 5 years	Drop out
Quality 1, ability 2	**0.039 (0.014)**	-0.005 (0.012)
Quality 1, ability 3	**0.161 (0.017)**	**-0.059 (0.014)**
Quality 1, ability 4	**0.071 (0.018)**	-0.026 (0.016)
Quality 2, ability 1	**0.091 (0.015)**	**-0.055 (0.013)**
Quality 2, ability 2	**0.095 (0.015)**	**-0.055 (0.012)**
Quality 2, ability 3	**0.180 (0.016)**	**-0.133 (0.014)**
Quality 2, ability 4	**0.143 (0.017)**	**-0.109 (0.015)**
Quality 3, ability 1	**0.106 (0.019)**	-0.016 (0.016)
Quality 3, ability 2	**0.164 (0.016)**	**-0.132 (0.014)**
Quality 3, ability 3	**0.210 (0.017)**	**-0.139 (0.014)**
Quality 3, ability 4	**0.268 (0.020)**	**-0.160 (0.015)**
Quality 4, ability 1	**0.176 (0.022)**	**-0.110 (0.018)**
Quality 4, ability 2	**0.283 (0.023)**	**-0.202 (0.018)**
Quality 4, ability 3	**0.299 (0.022)**	**-0.185 (0.017)**
Quality 4, ability 4	**0.281 (0.020)**	**-0.142 (0.014)**
Male	**-0.123 (0.008)**	**0.083 (0.008)**
Black	**-0.079 (0.009)**	**0.043 (0.009)**
Hispanic	**-0.089 (0.011)**	**0.081 (0.011)**
Other (not white)	**0.063 (0.014)**	**-0.097 (0.014)**
Household members age 18 or under	-0.000 (0.003)	0.006 (0.003)
Wealth quartile 2	0.023 (0.013)	**-0.083 (0.012)**
Wealth quartile 3	**0.087 (0.013)**	**-0.149 (0.014)**
Wealth quartile 4 (highest)	**0.161 (0.013)**	**-0.177 (0.014)**
No parent completed high school	0.010 (0.019)	**-0.116 (0.016)**
At least one parent has some college	**0.021 (0.010)**	-0.008 (0.008)
At least one parent completed college	**0.067 (0.010)**	**-0.084 (0.010)**
Northeast region	**0.037 (0.009)**	**-0.019 (0.009)**
South region	0.017 (0.009)	**-0.044 (0.010)**
West region	0.015 (0.012)	**-0.051 (0.012)**
Rural	**-0.027 (0.009)**	**0.054 (0.009)**
Log median income in census tract	**0.104 (0.025)**	-0.040 (0.023)
% Adults w/college deg. in tract	**-0.250 (0.059)**	**0.157 (0.056)**
Log avg. 2-year in-state tuition	**-0.070 (0.009)**	**0.033 (0.009)**
Log avg. 4-year in-state tuition	**0.183 (0.020)**	**-0.137 (0.021)**
Matched public 4-year in 50 mi	**-0.020 (0.007)**	**0.018 (0.007)**
Matched private 4-year in 50 mi	**0.020 (0.008)**	**0.022 (0.009)**
N	2,023	1,946
Pseudo R2	0.107	0.087
Log likelihood	-1,413	-1,223
Test: interactions nest separate dummies (Chi stat)	8.17	10.68
P-value from test of no interactions	52%	30%

Mean marginal effects (a.k.a. average derivatives) reported. Estimates statistically different from zero at the five percent level appear in bold. Estimates are weighted as described in the text.

Table 7: Effect of mismatch of transfers, four-year starters

	Transfer higher	Transfer lower
Quality 1, ability 2	**0.061 (0.011)**	0.006 (0.013)
Quality 1, ability 3	**0.030 (0.010)**	**-0.068 (0.014)**
Quality 1, ability 4	**0.076 (0.016)**	-0.018 (0.017)
Quality 2, ability 1	-0.012 (0.007)	**0.080 (0.016)**
Quality 2, ability 2	**-0.017 (0.007)**	**0.059 (0.015)**
Quality 2, ability 3	-0.005 (0.007)	-0.017 (0.014)
Quality 2, ability 4	**0.042 (0.012)**	0.004 (0.017)
Quality 3, ability 1	**-0.047 (0.009)**	**0.177 (0.021)**
Quality 3, ability 2	**-0.029 (0.007)**	**0.102 (0.018)**
Quality 3, ability 3	**-0.050 (0.008)**	**0.113 (0.017)**
Quality 3, ability 4	0.008 (0.008)	**0.062 (0.016)**
Quality 4, ability 1	**-0.065 (0.010)**	**0.146 (0.027)**
Quality 4, ability 2	†	0.016 (0.018)
Quality 4, ability 3	**-0.066 (0.010)**	**-0.047 (0.014)**
Quality 4, ability 4	**-0.065 (0.010)**	0.021 (0.014)
Male	**-0.016 (0.004)**	**-0.034 (0.006)**
Black	**0.019 (0.006)**	**0.029 (0.009)**
Hispanic	0.007 (0.006)	**0.051 (0.011)**
Other (not white)	**-0.017 (0.008)**	**-0.074 (0.013)**
Household members age 18 or under	**0.005 (0.002)**	-0.010 (0.003)
Wealth quartile 2	**0.018 (0.008)**	**0.052 (0.013)**
Wealth quartile 3	0.009 (0.007)	0.010 (0.012)
Wealth quartile 4 (highest)	-0.005 (0.007)	0.001 (0.012)
No parent completed high school	**0.076 (0.017)**	**-0.030 (0.017)**
At least one parent has some college	**0.037 (0.008)**	**-0.059 (0.009)**
At least one parent completed college	**0.039 (0.007)**	**-0.067 (0.009)**
Northeast region	**0.059 (0.011)**	**-0.062 (0.008)**
South region	**0.011 (0.005)**	-0.008 (0.008)
West region	**-0.016 (0.006)**	0.013 (0.011)
Rural	0.006 (0.005)	-0.007 (0.009)
Log median income in census tract	**0.025 (0.008)**	**0.108 (0.015)**
% Adults w/college deg. in tract	**0.125 (0.038)**	**-0.235 (0.043)**
Log avg. 2-year in-state tuition	-0.002 (0.005)	**-0.046 (0.010)**
Log avg. 4-year in-state tuition	**-0.045 (0.012)**	**-0.047 (0.017)**
Matched public 4-year in 50 mi	-0.005 (0.003)	-0.004 (0.006)
Matched private 4-year in 50 mi	**0.023 (0.007)**	-0.004 (0.008)
N	1,861	1,945
Pseudo R2	0.107	0.036
Log likelihood	-467	-1,235
Test: interactions nest separate dummies (Chi stat)	5.25	5.93
P-value from test of no interactions	73%	75%

Mean marginal effects (a.k.a. average derivatives) reported. Estimates statistically different from zero at the five percent level appear in bold. Estimates are weighted as described in the text.
† predicts failure perfectly, so excluded from this regressions and relevant observations dropped.

Table 8: Counterfactual outcomes with no mismatch

	Actual outcome	Predicted outcome with no mismatch
Graduate within 5 years	51.6%	51.3%
Drop out	29.4%	30.1%
Transfer to a higher quality college	7.0%	4.3%
Transfer to a lower quality college	26.9%	28.8%

This table presents the share of respondents who achieve each outcome in the data and the predicted share of respondents achieving each outcome in the counterfactual case of no mismatch (all students attend a college in the quality quartile that matches their ability quartiles). Predictions are made using the coefficients in tables 6 and 7.

Appendix Table 1: Sample

Total Observations	8,984
Graduated HS	7,143
Did not graduate HS but got GED	701
Started at a 4-year college*	2,868
Starting college qualities	
Of quality quartile 1	*705*
Of quality quartile 2	*630*
Of quality quartile 3	*603*
Of quality quartile 4	*541*
Missing quality	*389*
Has quality, but missing ability	*378*
Analysis sample	2,101

* The 4-year starters include 29 respondents who got a GED and 13 respondents with no recorded high school graduation date or GED.
College quality is for the first college attended. Of the 389 respondents who started at a 4-year school for whom we do not have a quality index, 26 are missing quality because we could not identify the college and 363 are missing quality because the school was not in IPEDS or did not have enough information to construct the quality measure.

Appendix Table 2: Description of independent variables

Variable	Description
Male	Indicator variable that respondent is male
Black	Indicator variable that respondent lists black as a racial category
Hispanic	Indicator variable that respondent lists Hispanic as an ethnic category and doesn't list black as a racial category
Other (not white)	Indicator variable that respondent doesn't list black or white as racial categories or Hispanic as an ethnic category
Household members 18 and under	Number of children age 18 and under living at the respondent's address in 1997 (including the respondent)
ASVAB percentile	Percentile over 4-year college starters in the NLSY97 (or all college starters for some specifications) of the first (ASVAB1) and second (ASVAB2) principal components of the 12 sections of the ASVAB test, taken by NLSY97 respondents in 1997 or 1998.
High School GPA	Collected from the respondent's high school transcript and standardized to a 4-point scale weighted by Carnegie credits. GPA percentile is calculated within our [weighted] sample of college-goers in the same way as the ASVAB percentile.
SAT score	Combined score on the math and verbal section of the SAT (max score 1600) or the composite score on the ACT converted to the SAT scale using the table provided by the ACT, collected from the respondent's high school transcript. SAT percentile is calculated within our [weighted] sample of college-goers in the same way as the ASVAB percentile.
Household wealth	Total 1997 household net worth for the household where the respondent lived in 1997. This number is taken from the parent survey where available and from the youth survey when the parent response is missing (98.6% from parent survey). We use total wealth across everyone living in the same household as the respondent (whether or not respondent is independent from parents in 1997). 1997 wealth quartiles are calculated within the (weighted) sample.
Parents' education	We consider the highest educational attainment of either of the respondent's resident parents (or of the only parent in single parent households) as reported in the fall before the respondent finished high school (or earlier if that year is unavailable). We include at most one resident mother and father figure per respondent using the following prioritization: biological, adopted, step, or foster.
Region of the U.S.	Region where the respondent lived in the fall before he or she finished high school.
Rural	Indicates that the respondent did not live within a Metropolitan Statistical Area (MSA) in the fall before she finished high school.
Log median income in census tract	Log median income (from the 1990 census) in the census tract where the respondent lived during his last year of high school.
% in census tract with BA	The share of the adult (over 25) population that has a 4-year college degree (from 1990 census) in the census tract where the respondent lived during his last year of high school.

Log average 4-year or 2-year in-state tuition.	Average in-state tuition, by year, for public four-year and two-year schools is from the State of Washington Higher Education Coordinating Board. "In-state" tuition for District of Columbia residents is calculated as max(national average in-state tuition, national average out-of-state tuition - $10,000) in accordance with DC Tuition Assistance Grant Program. For each respondent, in-state tuition is the in-state tuition in the fall before he finished high school in the state where he lived that fall. All tuition is CPI-deflated to 1997 dollars.
Well-matched public or private college nearby	Well-matched is defined as having a college of the relevant category whose weighted quality percentile is within 20 percentage points of the student's ASVAB ability percentile (as detailed in the text). Distance is calculated from the zipcode of the respondent's residence in the fall before he finished high school. In the 352 cases where the zipcode that fall was missing, the zipcode from the last available year prior to graduation is used.

Note: Many variables are measured in relation to the time a respondent finished high school. This is the reported high school graduation date for high school graduates or the last month a respondent reported being enrolled in high school for respondents who did not graduate.

Appendix Table 3: Sample construction for outcome regressions

<table>
<tr><td></td><td colspan="3">Value of dependent variable</td></tr>
<tr><td>Outcome:</td><td>Graduate in 5 years</td><td>Drop out</td><td>Transfer up or down</td></tr>
<tr><td>Graduate in 4 years or less</td><td>1</td><td>0</td><td rowspan="6">1 if transferred to a college in a quality percentile more than 10 percentage points above (below) starting college. Transfers down include transfers to 2-year colleges.</td></tr>
<tr><td>Graduate in 5 years</td><td>1</td><td>0</td></tr>
<tr><td>Graduate in 6 or more years</td><td>0</td><td>0</td></tr>
<tr><td>Leave school without a BA</td><td>0</td><td>1</td></tr>
<tr><td>Still in 4-year college as of last interview</td><td>0 if in college for >5 years, missing otherwise</td><td>missing</td></tr>
<tr><td>Still in 2-year college as of last interview</td><td>0</td><td>0</td></tr>
<tr><td>Not in most recent survey wave</td><td>0 if in college for >5 years before leaving sample, missing otherwise</td><td>missing</td><td>missing</td></tr>
</table>

Appendix Table 4: Principal components of the 12 test sections of the ASVAB

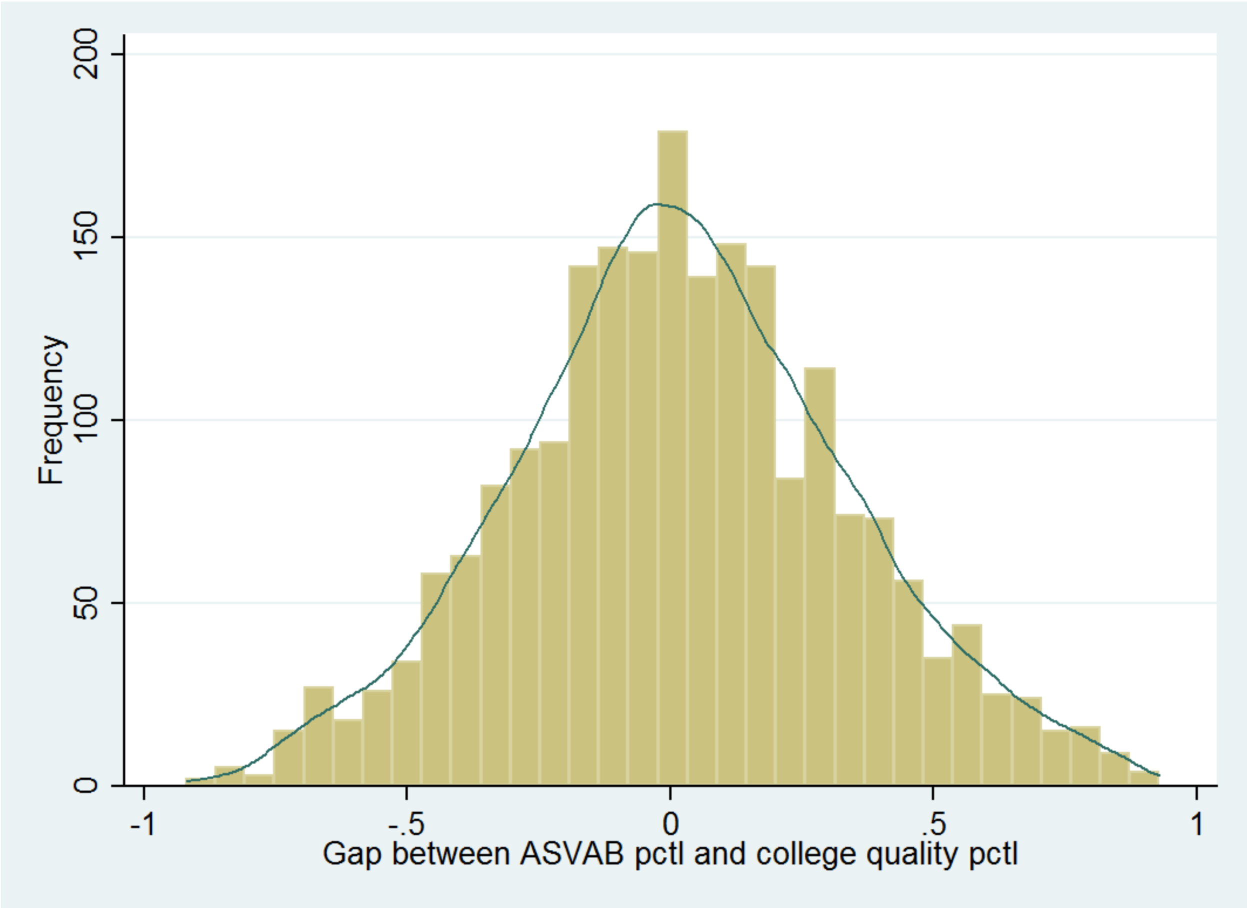
	1st Component	2nd Component	Unexplained variance
Eigenvalue	7.18	1.36	
Total variance explained	59.8%	11.3%	
Eigenvectors:			
General Science	0.326	-0.114	21.9%
Arithmetic Reasoning	0.325	0.117	22.2%
Word Knowledge	0.322	-0.038	25.4%
Paragraph Comprehension	0.320	0.114	24.8%
Mathematics Knowledge	0.318	0.239	19.7%
Mechanical Comprehension	0.310	-0.162	27.4%
Electronics Information	0.304	-0.228	26.8%
Assembling Objects	0.273	0.107	45.1%
Shop Information	0.245	-0.462	27.9%
Numerical Operations	0.240	0.444	31.8%
Auto Information	0.225	-0.456	35.6%
Coding Speed	0.223	0.441	37.8%

Note: scores on each test component are adjusted for the age of the respondent when they took the test by regressing the score on age dummies and using the residuals for the principal components analysis. The first two principal components combined explain 71.1% of the total variance of the 12 test section scores.

Appendix Table 5: Principal components of the college quality indices

	1st Component	Unexplained variance
Eigenvalue	2.09	
Total variance explained	52.2%	
Eigenvectors:		
Mean SAT	0.588	27.8%
Rejection rate	0.479	52.1%
Faculty/Student ratio	0.359	73.1%
Average faculty salaries	0.544	38.2%

Figure 1: **Distribution of estimated college mismatch, four-year starters**



Mismatch defined as student ability percentile minus college quality percentile. Histogram includes estimated kernel density distribution.

Figure 2: **Effect of ability-college quality on graduating within five years**

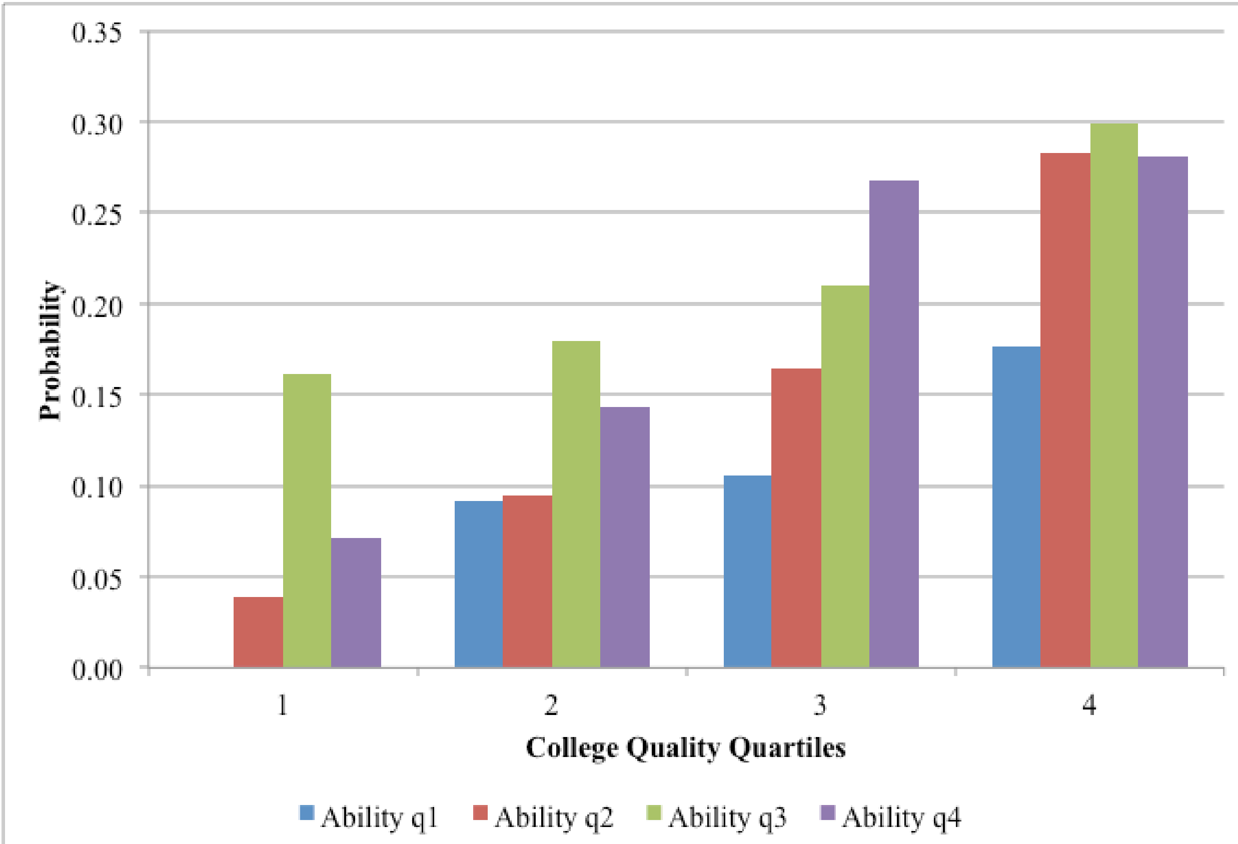
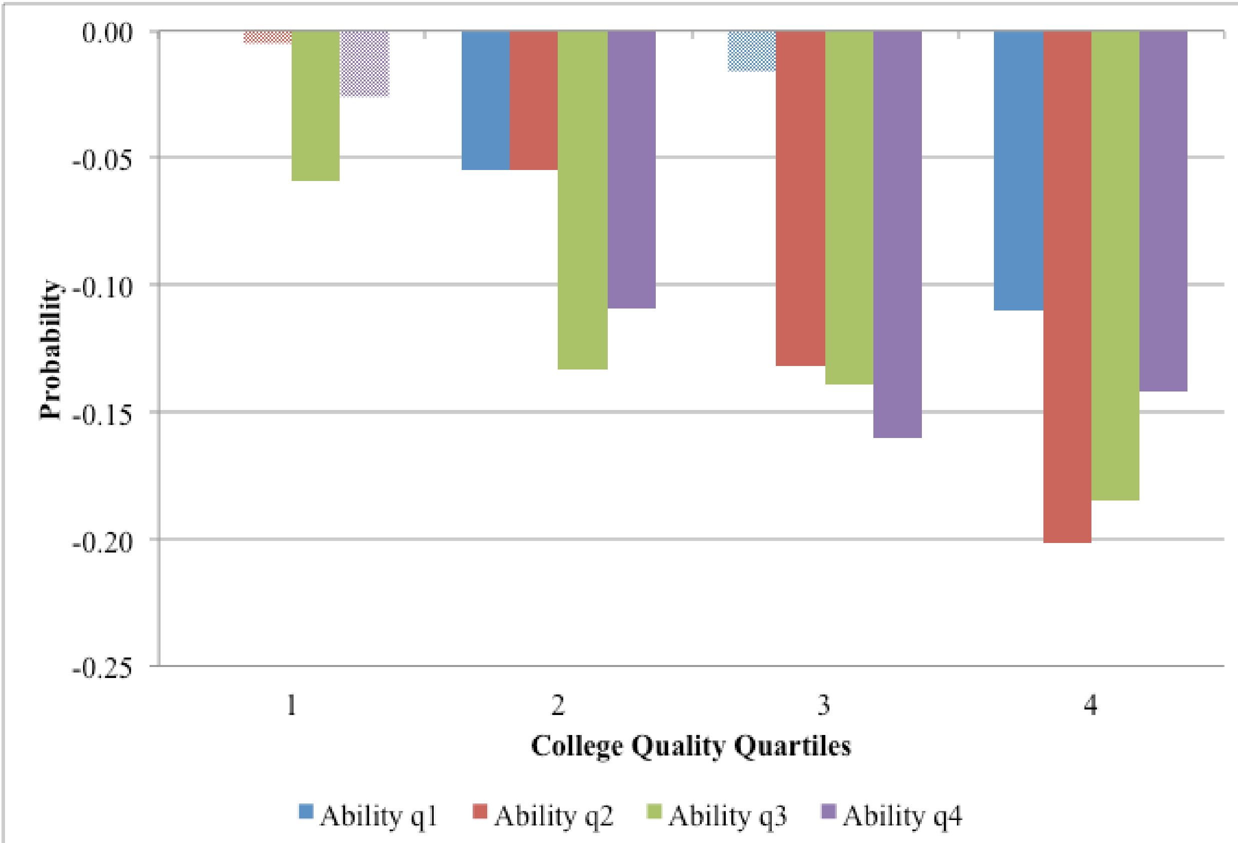
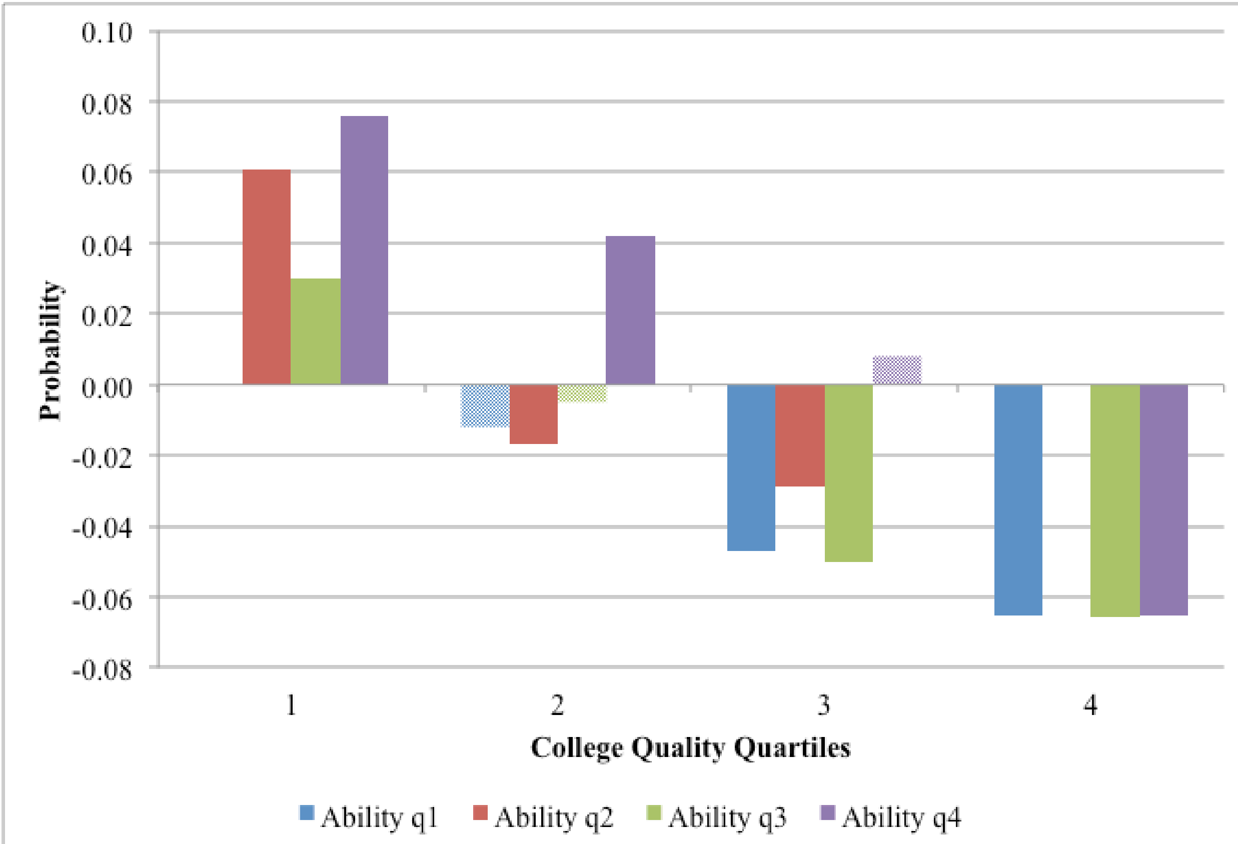


Figure 3: **Effect of ability-college quality on dropping out**



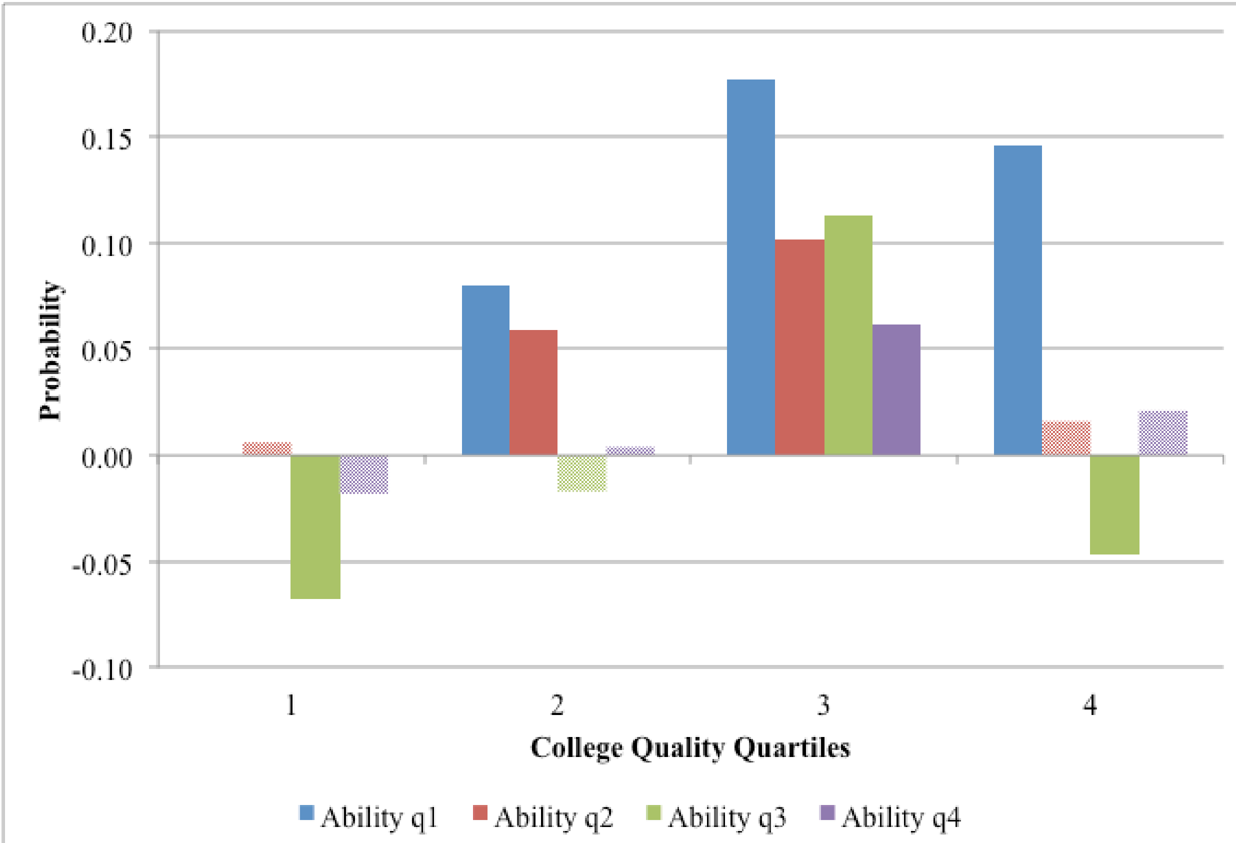
Lightly shaded columns represent coefficients that are not statistically significantly different from zero with 5% confidence.

Figure 4: **Effect of ability-college quality on transferring to a higher quality college**



Lightly shaded columns represent coefficients that are not statistically significantly different from zero with 5% confidence.

Figure 5: Effect of ability-college quality on transferring to a lower quality college



Lightly shaded columns represent coefficients that are not statistically significantly different from zero with 5% confidence.