Estimating the Perceived Returns to College*

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Abstract

College financial aid increases college attendance for recipients who believe their returns to college are near zero and increases government costs for all recipients. Using data from the National Longitudinal Survey of Youth 1979, I estimate perceived private returns to college by leveraging exogenous variation in tuition within a model that allows for systematic misperceptions about both costs of college and returns to college. The estimated distribution of perceived returns has low variance relative to estimates of pecuniary lifetime returns, implying large responses to unconditional financial aid offers. I show that the cost-minimizing financial aid policy for an aggregate attendance target makes aid offers conditional on observables, extending more aid to individuals who are less likely to attend college. Relative to unconditional aid increases, policies designed to reduce costs also reduce racial and parent-educational inequality, and policies designed to reduce inequality also reduce costs.

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1 Introduction

Young people may make socially suboptimal choices about their college education. First, they may not know their private costs of or returns to college (Manski, 1993; Cunha, Heckman, and Navarro, 2005; Wiswall and Zafar, 2015), and may therefore fail to make privately optimal decisions regarding their education. Furthermore, college education can produce positive externalities on local productivity (Iranzo and Peri, 2009), firm profits in imperfectly competitive labor markets (Becker, 1962), and government revenue through a combination of income taxes and (partially internalized) pecuniary returns. It follows that policies that alter beliefs about college prices (such as financial aid) can increase aggregate welfare if they reduce the distance between perceived private returns and actual social returns. This paper estimates perceived private returns to college conditional on observed characteristics in order to inform the magnitude of college financial aid that would be sufficient to induce students to attend college, conditional on their observed characteristics.

This paper develops a broadly applicable workhorse method for estimating parameters sufficient to describe cost-minimizing financial aid policies that reach target college attendance rates for the entire population or for subpopulations of interest, such as underrepresented minorities, while allowing for systematic misperceptions on both pecuniary costs of college and returns to college. The method described makes use of data on observed individual level college attendance decisions, local college tuition costs, and realized tuition costs for individuals who attend college. It does not require elicitation of beliefs about college costs or returns, estimation of true returns, nor does it require agents to have perfect information or rational expectations about true returns or costs. It can thus be used across locations, times, and policy regimes with minimal data demands to estimate predicted responses to counterfactual financial aid policies conditional on available covariates that may vary across settings.

The main substantive result of the current paper is an estimated distribution of perceived returns to college, using data from the NLSY79, that is centered near zero with low variance relative to existing estimates of ex post lifetime pecuniary returns to college. It follows that there is a large mass of individuals with perceived returns near zero, which in turn implies large predicted responses to financial aid changes. Specifically, I present counterfactual analysis that shows a predicted increase in college attendance of 5.1 percentage points from a \$1,000 (year 2000 dollars) universal increase in annual financial aid, consistent with the 3–6 percentage point predictions commonly implied by linear extrapolations of results in the financial aid program evaluation literature surveyed by Deming and Dynarski (2010). The estimates in the current paper are thus consistent with those in the program evaluation literature while facilitating exploration of counterfactual policies that provide heterogeneous financial aid to students conditional on observed characteristics.

The identification of agents' beliefs about returns to college in this paper relies on revealed preference assumptions and the assumption that agents with different values of local tuition at age 17 have known (to the researcher) differences in perceived returns to college. The scale of the perceived returns distribution is identified from the mass of individuals shifted into and out of college attendance by known-to-agents, exogenous shifts in college costs driven by local tuition at age 17. The location of the perceived returns distribution is identified from college attendance rates. The predictive contributions of other observed covariates to perceived returns are identified by comparing their effects on college attendance to the impact of tuition. Perceived returns estimates in this paper are identified from observed choices, and therefore do not distinguish beliefs from preferences (Manski, 1993, 2004).[1] However, they are robust to arbitrary and idiosyncratic agent misperceptions of true returns, costs, and determinants of returns other than tuition variation driven by local tuition at age 17.

In general, inferring agents' beliefs from data requires an assumed coincidence of inference between researchers and agents regarding data available to both, for instance, that elicited beliefs are accurate. In this setting, the assumption that researchers and agents agree on the effect of local tuition on perceived costs allows us to infer perceived returns to college without relying on data on elicited beliefs. Relative to the data used in the present paper, data containing elicited beliefs in particular settings may be difficult to acquire or even nonexistent, to say nothing of the substantial methodological challenges in ensuring its accuracy as described by Charness, Gneezy, and Rasocha (2021); Schotter and Trevino (2014); Trautmann and van de Kuilen (2015); Danz, Vesterlund, and Wilson (2022); Schlag, Tremewan, and Van der Weele (2015), and others. Existing research on education that relies on elicited beliefs includes Jensen (2010); Arcidiacono, Hotz, and Kang (2012); Wiswall and Zafar (2015) and Kapor, Neilson,

[1]The results of this paper can be explained with either systematic misperceptions of returns, or with large average psychic costs for college (centering the perceived returns distribution near zero) that are positively correlated with pecuniary returns (reducing the perceived returns distribution's variance). The psychic cost explanation requires that young people with particularly high pecuniary returns to college particularly dislike it. Implications for effects of counterfactual financial aid policies on attendance are unaffected by alternative interpretations of results, but there are substantial welfare implications.

and Zimmerman (2020), with the caveat that much of this work relies on weaker assumptions such as elicited beliefs being valid proxies for actual beliefs rather than assuming a known, deterministic relationship between elicited beliefs and actual beliefs.[2]

In settings where reliable elicitation of agents' beliefs is unavailable, such as that of the present paper, researchers make other assumptions on mutual coincidences of beliefs between agents and researchers. For instance, Cunha, Heckman, and Navarro (2005) and related work use panel data to estimate true pecuniary returns and assume that agents have rational expectations over their pecuniary returns, effectively imposing that agents and researchers arrive at similar estimates of agents' returns to college.[3] Under such assumptions, the scale of perceived returns is inherited from the scale of actual returns and discrepancies between returns and decisions can be attributed to psychic costs or expectational errors. Dickstein and Morales (2018) assume that differences in expected export profits between firms are a known function of their expected revenues, where the known function is an equilibrium relationship from a Melitz (2003) model.

The primary advantage of the identifying assumptions in this paper relative to the alternatives described above is that I assume that agents and researchers share knowledge of an accounting identity relating perceived returns and costs (costs reduce returns at the rate of one dollar per dollar), rather than assuming they arrive at similar estimates of structural parameters, such as returns to college or demand elasticities. The validity of identifying assumptions regarding beliefs across alternative methods is application-specific. However, given the difficulty that researchers have at arriving at similar estimates as one another even when using the same data (Huntington-Klein, Arenas, Beam, Bertoni, Bloem, Burli, Chen, Grieco, Ekpe, Pugatch, et al., 2021), avoiding assumptions of common estimates between agents and researchers of structural parameters is a likely benefit in many applications, including the present one.

To validate my estimates of perceived returns to college, I estimate the effect of the Social Security Student Benefit (SSSB) on perceived returns to college. Prior to its elimination in 1982, the SSSB gave eligible students an average of approximately \$6,700 per year (year 2000 dollars), or \$26,800 over four years. Given the prominence of the policy and the fact that the policy was in effect before being abruptly terminated, it is likely that it serves as a valid known and exogenous shifter of perceived returns for previously eligible individuals. It follows that

[2]For instance, Wiswall and Zafar (2015) estimate heterogeneous responses to an information intervention related to earnings for college majors for individuals with different elicited beliefs. Their broad conclusions hold for monotonic transformations of the difference between true earnings and beliefs about earnings.

[3]Several related papers use similar methods, including Cunha, Heckman, and Navarro (2006); Cunha and Heckman (2007) and Cunha and Heckman (2008).

the effect of its termination on perceived returns should be equal to the value of financial aid expected by previously eligible students. A difference in differences design similar to that used by Dynarski (2003) embedded within my structural model yields an estimated effect of the policy on perceived returns to college of \$21,300, which is statistically indistinguishable from the \$26,800 average four-year aid granted to treated students.

Finally, I use the estimated distribution of perceived returns to consider counterfactual financial aid policies. First, I consider the effect of a \$1,000 (year 2000 dollars) annual increase in financial aid on college attendance, finding an increase of 5.1 percentage points. This effect is consistent with predictions from the program evaluation literature surveyed by Deming and Dynarski (2010). I also consider the effect of a policy that seeks to induce 75% of the population to attend college at minimum cost to the policy maker by making financial aid offers conditional on observables. I find that such a policy, which in my sample constitutes an increase in college attendance of about 20 percentage points, would cost an average of \$3,009 per student per year of attendance, which is less than 80% of the cost of an unconditional policy that achieves the same attendance rate.

In general, policies that minimize costs subject to expected attendance constraints focus financial aid offers on students who are unlikely to attend college, because allocating financial aid to students who are likely to attend college in the absence of the policy increases costs without increasing attendance. Insofar as students unlikely to attend are often poor or otherwise disadvantaged, it follows that such policies decrease educational inequality between advantaged and disadvantaged groups. I also consider policies that explicitly seek to reduce educational inequality by targeting underrepresented minorities and (potential) first-generation college students (about 70% of the NLSY79 sample). I find that such policies allocate aid qualitatively similarly to the cost-minimizing policy, with higher cost-effectiveness (measured as attendance increases per cost) than unconditional aid offers of comparable magnitude.

The paper proceeds as follows. Section 2 introduces a model of agents' college attendance decisions as determined by their beliefs about college costs and returns. Section 3 describes the econometric strategy and the assumptions required for identification of perceived returns to college. Section 4 discusses the data used in estimation of the model. Section 5 provides the results and discusses their implications. Section 6 investigates effects of an actual policy (the Social Security Student Benefit) and some counterfactual policies. Section 7 concludes.

2 Model

I present the college attendance decision as a two-sector generalized Roy (1951) model. I denote agent i's schooling choice with S_i, where $S_i = 1$ denotes the choice of the college sector and $S_i = 0$ denotes the choice of the non-college sector. I denote i's potential ex post earnings in time t conditional on choosing the college sector as $Y_{1,i,t}$, and their potential ex post earnings in time t conditional on choosing the high school sector as $Y_{0,i,t}$. I denote per period tuition costs net of financial aid for college as $Tuition_{i,t}$, where college loans are implicitly modelled as tuition costs paid in later time periods. Finally, I let $C_{i,t}$ denote ex post net psychic costs associated with the college sector in year t, which expresses in monetary terms the costs associated either with college itself or with outcomes of the college decision, such as job amenities.

I define ex post net present value income in the college sector as

$$Y_{1,i} = \sum_{t=0}^{\infty} \frac{Y_{1,i,t}}{1 + r_{1,i,t}}, \tag{1}$$

net present value income in the high school sector as

$$Y_{0,i} = \sum_{t=0}^{\infty} \frac{Y_{0,i,t}}{1 + r_{0,i,t}}, \tag{2}$$

net present value psychic costs, expressed in monetary units, as

$$C_i = \sum_{t=0}^{\infty} \frac{C_{i,t}}{1 + r_{1,i,t}}, \tag{3}$$

and net present value pecuniary (tuition) costs as

$$Tuition_i = \sum_{t=0}^{\infty} \frac{Tuition_{i,t}}{1 + r_{1,i,t}}, \tag{4}$$

in terms of i's idiosyncratic time series of schooling-specific interest rates $r_{S,i,t}$, which are also unobserved by the researcher. I take the time series indefinitely into the future noting that each potential outcome (and presumably agents' beliefs about them) will go to zero for sufficiently large t due to agents' finite lifespans. This allows for differences in longevity as a result of the schooling choice. Of the model variables, only S_i is assumed to be observed by the econometrician for all i, with $Tuition_i$ observed only for those who attend college.[4]

[4] $Y_{1,i,t}$ or $Y_{0,i,t}$ may be observed for some time periods, depending on i's observed schooling choice. However,

I allow for agents to be uncertain and systematically incorrect about potential outcomes.[5] I denote an agent's distribution of beliefs about $(Y_{1,i}, Y_{0,i}, C_i, Tuition_i)$ at time $t = 0$ as $G_{i;Y_{1,i},Y_{0,i},C_i,Tuition_i}(y_{1,i}, y_{0,i}, c_i, tuition_i)$, henceforth abbreviated as $G_i(\omega_i)$. I assume that agents maximize income independently of how they consume it, as in the case of perfect credit markets, such that the net present value potential outcomes are sufficient to describe agents' decisions.[6] It follows that agents make their college attendance decision according to

$$S_i = \begin{cases} 1 \text{ if } \int_{-\infty}^{\infty} u_i(y_{1,i} - c_i - tuition_i)\, \mathrm{d}G_i(\omega_i) \geq \int_{-\infty}^{\infty} u_i(y_{0,i})\, \mathrm{d}G_i(\omega_i) \\ 0 \text{ otherwise,} \end{cases} \tag{5}$$

wherein $u_i(\cdot)$ gives i's utility function as a function of values of potential outcomes.[7] This expression compares utility across schooling choices while accommodating treatment effects of schooling on earnings, longevity, and interest rates.

To simplify expression (5), I define the mean of agent i's beliefs about a random variable x_i as $\widetilde{x}_i$, which I may refer to as i's "perceived x", and write

$$S_i = \begin{cases} 1 \text{ if } u_i(\widetilde{Y}_{1,i} - \widetilde{C}_i - \widetilde{Tuition}_i - \epsilon_{1,i}) \geq u_i(\widetilde{Y}_{i,0} - \epsilon_{0,i}) \\ 0 \text{ otherwise,} \end{cases} \tag{6}$$

where $\epsilon_{S,i}$ is i's scalar risk premium associated with education choice S, such that the left and right sides of expressions (5) and (6) are each equivalent, respectively. Next, I assume that $u_i(\cdot)$

it is possible that outcomes in some or all periods will not be observed, and their observation is not necessary for the methods in this paper.

[5]This flexibility requires some unconventional notation regarding agents' beliefs. The standard practice of describing an agent's expectation about an arbitrary object X as $\mathbb{E}[X|\mathcal{I}_i]$, where $\mathcal{I}_i$ denotes the agent's information set, implicitly assumes rational expectations via the law of iterated expectations. To avoid this assumption, I describe agents' beliefs using integrals over their belief distributions in lieu of using the expectations operator.

[6]The method used in this paper will combine perceived returns and perceived credit constraints if agents believe they are credit constrained. If observed or unobserved characteristics affect perceived returns and perceived credit constraints according to a common ratio, the univariate binary choice model in this paper will estimate the combination of perceived returns and perceived credit constraints consistently. If different observed or unobserved traits affect perceived returns and perceived credit constraints in different proportions to one another, then the model here is misspecified. Estimation of perceived returns and credit constraints jointly within a similar framework to that of the present paper is left to future work.

[7]Not only might young people vary in their utility functions, but the decision making unit (e.g. young people and their parents) may differ across individuals due to differences in in-kind side payments, bequests, other within household transfers, and cultural norms regarding parental involvement in educational decisions. Allowing for utility heterogeneity in preferences for education allows for predictions of counterfactual financial aid policies without taking a stance on how students and their parents distribute financial aid within the household.

is globally monotonic such that there exists a unique scalar $\widetilde{\pi}_i$ that satisfies

$$u_i(\widetilde{Y}_{1,i} - \widetilde{C}_i - \widetilde{Tuition}_i - \epsilon_{1,i} - \widetilde{\pi}_i) = u_i(\widetilde{Y}_{i,0} - \epsilon_{0,i}), \tag{7}$$

wherein $-\widetilde{\pi}_i$ is i's compensating variation associated with attending college rather than not doing so.

I define $\widetilde{\pi}_i$ as i's perceived return to college. The perceived return, thus defined, is particularly policy-relevant because it is the amount of money agent i would need to have taken from them if they attended college to make them indifferent between attending college and not attending. It is thus sufficient for determining effects of changes in college costs, such as financial aid policy changes. The above equation can be rearranged to express perceived returns as

$$\widetilde{\pi}_i = \widetilde{Y}_{1,i} - \widetilde{Y}_{i,0} - \widetilde{C}_i - \widetilde{Tuition}_i + \epsilon_{0,i} - \epsilon_{1,i}. \tag{8}$$

It follows that perceived returns are a sufficient statistic for the education decision, with

$$S_i = \begin{cases} 1 \text{ if } \widetilde{\pi}_i \geq 0, \\ 0 \text{ otherwise.} \end{cases} \tag{9}$$

It also follows from (8) that perceived tuition affects perceived returns at a known (to agents and the econometrician) marginal rate of -1.[8] This allows us to write the projection of perceived returns on observed characteristics, X_i, and perceived tuition as

$$\widetilde{\pi}_i = X_i\beta - \widetilde{Tuition}_i + \epsilon_i, \tag{10}$$

where X_i contains observed characteristics of i, perceived tuition has a known causal effect on perceived returns (the coefficient is constrained to negative one), and ϵ_i contains i's idiosyncratic perceived returns, including their net risk-premia in comparing college to non-college, insofar as these are not explained by X_i. There is no assumption that X_i is known to agents, but rather that β captures the associations between observed characteristics and potential outcomes such as income, psychic costs, or uncertainty about these.[9]

[8]The perceived return as expressed here also has known relationships with i's expected earnings, psychic costs, and risk premium associated with college attendance. In principle, any of these known relationships can be leveraged to estimate perceived returns, but the estimation will require known and exogenous (to agents) variation in the chosen component of perceived returns, which is most readily available for tuition.

[9]For an example of a variable that may be in X_i without being known to agents, consider the average wage

If we observed $\widetilde{Tuition}_i$, we could make use of its known effect on perceived returns to estimate perceived returns in the context of a binary choice model, using (9) and (10). While we do not observe $\widetilde{Tuition}_i$, we can assume that

$$\widetilde{Tuition}_i = Tuition_i + X_i\alpha + \nu_i \tag{11}$$

without loss of generality, where the agent's expected tuition is decomposed into their true potential tuition and their expectation error, $X_i\alpha + \nu_i$. The expectation error is determined by observables, X_i, and an unobserved idiosyncratic component ν_i, where α gives the marginal effect of observables on misperceptions. If expected tuition moves with potential tuition at a rate other than 1, it by definition constitutes a misperception and is captured in this expression by correlation between $Tuition_i$, X_i, and ν_i.[10]

It follows from the expression for expected tuition in (11) that equation (10) can be rewritten as

$$\widetilde{\pi}_i = X_i\theta - Tuition_i + \epsilon_i - \nu_i, \tag{12}$$

where the contributions of observables to tuition misperceptions and to other determinants of perceived returns are combined in $\theta = \beta - \alpha$, and the idiosyncratic misperception, ν_i, now joins the other idiosyncratic components of perceived returns in ϵ_i to form a composite error. This expression is in terms of observed characteristics and potential tuition, making it amenable to estimation, and it frames misperceptions on tuition as a source of omitted variable bias, which will motivate the empirical strategy described below.

3 Empirical Strategy

I estimate perceived returns to college using binary choice methods that leverage the observable expression for perceived returns in (12) and the sufficiency of perceived returns for determining observed college attendance. Briefly, the assumed marginal effect of tuition on perceived returns frees up the scale parameter in the binary choice model, allowing it to be estimated. This

of the county of residence. Regardless of agents' ability to accurately report this quantity, it nonetheless serves to explain differences in the health of the local economy between agents, and likely predicts perceived returns to college.

[10] The methods in this paper allow for arbitrary correlation between $Tuition_i$ and ν_i. Negative correlation would occur, for example, if agents form rational expectations about their tuition using a strict subset of its determinants, causing them to understate their tuition's deviation from the population average. Positive correlation would occur, for example, if agents overreact to tuition predictors for behavioral reasons, such as a high (low) achieving student erroneously expecting more (less) financial aid than they will actually get.

assumption also places economic content on the parameters of the binary choice model by fixing their units to those of tuition (i.e. dollars). Because the model's assumed marginal effect of tuition on perceived returns is causal, the method requires instruments that drive tuition but are independent of idiosyncratic tuition misperceptions, ν_i, and other idiosyncratic determinants of perceived returns, ϵ_i. Furthermore, because tuition is only observed for individuals who attend college, I impute tuition while accounting for selection into college following Heckman (1979).

For clarity, I will describe the estimation procedure in stages, starting with the simple case in which tuition is observed for all individuals in the sample, and tuition is exogenous and known to agents, such that $Cov(Tuition, \epsilon - \nu) = 0$. Next, I will describe the case in which tuition is observed for all individuals, but tuition is endogenous and/or unknown to agents. Finally, I will describe the estimator that I use for the empirical application, where the method accounts for the researcher's inability to observe tuition for individuals who do not attend college along with tuition being endogenous and misperceived by agents.

3.1 Exogenous Tuition Known to Agents and Researcher

If tuition were exogenous and known to agents (such that $\epsilon_i - \nu_i$ were independent of $Tuition_i$ given X_i) and observed to the researcher for all individuals, we could estimate perceived returns from equation (12) with minimal difficulty.[11] For instance, we could estimate perceived returns by assuming

$$\epsilon_i - \nu_i | X_i, Tuition_i \sim \mathcal{N}(0, \sigma^2), \tag{13}$$

and estimating perceived returns with a slight modification to a probit. The only difference from a standard probit is restricting the coefficient on tuition to -1 rather than imposing the standard restriction $\sigma = 1$. Under the independence assumption on $\epsilon_i - \nu_i$, the normality assumption above, and the assumptions in Section 2, the estimated probit coefficients give the contributions, in dollars, of each observed covariate in X_i to perceived returns to college.

In more detail, defining $(\theta^*, \gamma^*) = (\frac{\theta}{\sigma}, \frac{1}{\sigma})$ allows us to write the log-likelihood for the probit

[11] A stricter definition of tuition being known to agents is that $\widetilde{Tuition}_i = Tuition_i$ for all i. The weaker condition of misperceptions being independent of tuition conditional on X_i is sufficient for the current method to obtain consistent estimates of perceived returns.

as

$$
\begin{aligned}
&\mathcal{L}\Big(\theta^*, \gamma^* | X_i, Tuition_i\Big) = \\
&\sum_i S_i \log\Big[\Phi\Big(X_i\theta^* - Tuition_i\gamma^*\Big)\Big] \\
&+(1-S_i)\log\Big[1-\Phi\Big(X_i\theta^* - Tuition_i\gamma^*\Big)\Big],
\end{aligned} \tag{14}
$$

wherein $\Phi(\cdot)$ denotes the standard normal cumulative density function. We can estimate perceived returns by choosing $(\hat{\theta}, \hat{\sigma})$ to maximize this log likelihood, yielding an estimated distribution of perceived returns given by

$$
\hat{\tilde{\pi}}_i | X_i, Tuition_i \sim \mathcal{N}\Big(X_i\hat{\theta} - Tuition_i, \hat{\sigma}^2\Big). \tag{15}
$$

Note that while the residual component of perceived returns is assumed to follow a normal distribution, the population distribution of perceived returns conditions on X_i and will inherit the distribution of X as weighted by $\hat{\theta}$. If the amount of variation in college attendance explained by observables is large, the shape of the perceived returns distribution will be determined less by the normality assumption on $\epsilon_i - \nu_i$, and more by the distribution of $X\hat{\theta}$.

3.2 Endogenous Tuition Unknown to Agents, Observed by the Researcher

It is likely that potential tuition is unknown to agents, both because of the many factors at play in net tuition, such as scholarship amounts, and because tuition may change year to year in ways that students do not forecast. Furthermore, existing work has elicited students' beliefs about tuition and found substantial errors across a wide range of demographic groups (Grodsky and Jones, 2007). It is also likely that potential tuition is correlated with perceived returns. For instance, unobservably high-achieving students may have high perceived returns and attend expensive colleges, or may have high perceived returns and get large amounts of financial aid.

With misperceptions or endogeneity with respect to potential tuition, the cross-sectional relationship between tuition and college attendance will not be driven exclusively by the effect of perceived tuition on perceived returns. Because identification of perceived returns rests on this causal relationship, estimated perceived returns using the method in the preceding section will likely be biased. I show here that because both tuition misperceptions and other unobserved determinants of perceived returns enter the model as omitted variables, the problems they present can be addressed with instrumental variables.

I assume the instrumental variables, Z_i, are strongly correlated with tuition and are independent of tuition misperceptions (they are known) and other unobserved determinants of perceived returns (they are exogenous).[12] Under these assumptions, perceived returns to college are generated by the system of equations

$$\begin{aligned} \widetilde{\pi}_i &= X_i\theta - Tuition_i + \epsilon_i - \nu_i \\ Tuition_i &= Z_i\delta + u_i, \end{aligned} \tag{16}$$

where $X_i \subset Z_i$ and the instruments have marginal effects of δ on potential tuition and u_i contains the problematic variation in tuition that is potentially correlated with ν_i and ϵ_i. There are multiple methods for consistently estimating this system, with the commonality of relying only on variation in Z, not u, to identify effects of tuition on perceived returns.

Perceived returns can be estimated in this setting by maximum likelihood, assuming that X_i, u_i, and $\epsilon_i - \nu_i$ are i.i.d. with u_i and $\epsilon_i - \nu_i$ jointly normally distributed with mean zero and covariance matrix

$$\Omega = \begin{bmatrix} \sigma_u^2 & \rho\sigma_u\sigma \\ \rho\sigma_u\sigma & \sigma^2 \end{bmatrix}. \tag{17}$$

The second stage can then be rewritten as

$$\widetilde{\pi}_i = X_i\theta - Z_i\delta + \eta_i, \tag{18}$$

where $\eta_i = \epsilon_i - \nu_i - u_i$ and $\eta_i|X_i, Z_i$ is normally distributed with mean 0 and variance $\sigma_\eta^2 = Var(\epsilon_i - \nu_i - u_i) = \sigma^2 + \sigma_u^2 - 2\rho\sigma_u\sigma$. Defining $(\theta_\eta^*, \gamma_\eta^*) = (\frac{\theta}{\sigma_\eta}, \frac{1}{\sigma_\eta})$, the log-likelihood is

$$\begin{aligned} &\mathcal{L}\Big(\theta_\eta^*, \gamma_\eta^*, \delta, \sigma_u | Z_i, Tuition_i\Big) = \\ &\sum_i S_i \log\left[\Phi\Big(X_i\theta_\eta^* - (Z_i\delta)\gamma_\eta^*\Big)\right] + (1 - S_i)\log\left[1 - \Phi\Big(X_i\theta_\eta^* - (Z_i\delta)\gamma_\eta^*\Big)\right] \\ &+ \log\left(\phi(Tuition_i - Z_i\delta, \sigma_u^2)\right), \end{aligned} \tag{19}$$

where $\phi(\cdot,\cdot)$ denotes the normal density with mean given by the first argument and variance

[12] The weaker but less intuitive condition that $Cov(Z, \epsilon - \nu) = 0$ is also sufficient. This weaker condition allows for the possibility of bias from tuition misperceptions and bias from other unobserved determinants of perceived returns cancelling each other out.

given by the second. Defining $\widehat{Tuition}_i = Z_i\hat{\delta}$, perceived returns to college are then given by

$$\hat{\widetilde{\pi}}_i | Z_i \sim \mathcal{N}\Big(X_i\hat{\theta} - \widehat{Tuition}_i, \hat{\sigma}_\eta^2\Big). \tag{20}$$

This method of estimating perceived returns moves some explanatory variation in u_i into the second stage residual, rather than conditioning on it (as in a control function approach) to obtain more precise estimates of perceived returns. The estimator described here is reasonable in this context because policymakers do not know agents' potential tuition prior to their college attendance decisions and will thus be unable to condition on this information when designing policies.

3.3 Endogenous, Unknown, and Selectively Observed Tuition

In addition to the concerns regarding endogeneity and misperceptions on potential tuition described in the preceding section, the practical problem persists that agents' potential net tuition levels are unobserved unless they attend college. This problem arises because we do not know ex ante which college individuals would attend or what their financial aid would be if they attended. To address this, I augment the estimation procedure in the preceding section with imputation of tuition that accounts for systematic differences in tuition for individuals who do and do not attend college, following insights from Heckman (1979). Because of its robustness to tuition misperceptions, endogeneity, and selective-observed tuition, I use the method of the current section to estimate perceived returns to college.

The data-generating process in this setting is still

$$\begin{gathered}\widetilde{\pi}_i = X_i\theta - Tuition_i + \epsilon_i - \nu_i \\ Tuition_i = Z_i\delta + u_i,\end{gathered} \tag{21}$$

where the substitution $\widetilde{Tuition}_i = Tuition_i + \nu_i$ has already been made, with the complication that potential tuition, $Tuition_i$, relates to observed tuition, $Tuition_i^*$, according to

$$Tuition_i^* = \begin{cases} Tuition_i & \text{if } \widetilde{\pi}_i \geq 0, \\ . & \text{otherwise.} \end{cases} \tag{22}$$

To impute tuition, note that while we cannot estimate (21) without observing $Tuition_i$, we can estimate the reduced form of perceived returns,

$$\widetilde{\pi}_i = Z_{1i}\lambda + \eta_i, \tag{23}$$

where $\eta_i = \epsilon_i - \nu_i - u_i$ as in Section 3.2 and $Z_{1i} = Z_i \cup X_i$ affects perceived returns at rate λ. I assume that Z_i contains at least one determinant of potential tuition that excluded from X_i, and X_i contains at least one determinant of perceived returns that is excluded from Z_i.[13] Assuming as in Section 3.2 that

$$\eta_i | Z_{1i} \sim \mathcal{N}(0, \sigma_\eta^2) \tag{24}$$

and defining $(\lambda_\eta^*, \rho_\eta^*) = (\frac{\lambda}{\sigma_\eta}, \frac{\rho}{\sigma_\eta})$ allows estimation of the effect of instruments on potential tuition for all individuals by maximizing the familiar "Heckman correction" log-likelihood

$$\begin{gathered}\mathcal{L}\Big(\lambda_\eta^*, \rho_\eta^*, \delta, \sigma_u | Z_{1i}\Big) = \\ \sum_i S_i \log\left(\Phi\Big(\frac{Z_{1i}\lambda_\eta^* + (Tuition_i^* - Z_i\delta)\rho_\eta^*/\sigma_u}{\sqrt{1-\rho_\eta^{*2}}}\Big)\right) + S_i \log\big(\phi(Tuition_i^* - Z_i\delta, \sigma_u^2)\big) \\ +(1-S_i)\log\big(1-\Phi(Z_{1i}\lambda_\eta^*)\big),\end{gathered} \tag{25}$$

where potential tuition is estimated by

$$\widehat{Tuition}_i = Z_i\hat{\delta}. \tag{26}$$

An intuitive two-step procedure estimates perceived returns by maximizing the log-likelihood in (19) with $\widehat{Tuition}_i$ from (26) substituted in for $Tuition_i$. It is convenient for computation of standard errors to estimate perceived returns jointly with the imputation of tuition. The log-likelihood for jointly estimating the model is

$$\begin{gathered}\mathcal{L}\Big(\lambda_\eta^*, \rho_\eta^*, \delta, \sigma_u, \theta_\eta^*, \gamma_\eta^* | Z_{1i}\Big) = \\ \sum_i S_i \log\left(\Phi\Big(\frac{Z_{1i}\lambda_\eta^* + (Tuition_i^* - Z_i\delta)\rho_\eta^*/\sigma_u}{\sqrt{1-\rho_\eta^{*2}}}\Big)\right) + S_i \log\big(\phi(Tuition_i^* - Z_i\delta, \sigma_u^2)\big) \\ +(1-S_i)\log\big(1-\Phi(Z_{1i}\lambda_\eta^*)\big) \\ +S_i \log\Big(\Phi\big(X_i\theta_\eta^* - (Z_i\delta)\gamma_\eta^*\big)\Big) + (1-S_i)\log\Big(1-\Phi\big(X_i\theta_\eta^* - (Z_i\delta)\gamma_\eta^*\big)\Big),\end{gathered} \tag{27}$$

[13] The exclusion of an instrument for tuition from the perceived returns equation is needed to estimate the effect of tuition on perceived returns. The exclusion of a variable in the perceived returns equation from the tuition equation avoids reliance on functional form for imputing tuition, as described by Heckman (1979).

where parameters are defined above. This likelihood is essentially an instrumental variables probit with an unconventional scale normalization in the second stage and a Heckman (1979) correction in the first stage. Perceived returns are then given by

$$\hat{\tilde{\pi}}_i | Z_{1i} \sim \mathcal{N}\Big(X_i\hat{\theta} - \widehat{Tuition}_i, \hat{\sigma}_\eta^2\Big), \tag{28}$$

as in Section 3.2. The estimates $\hat{\delta}$, and therefore $\widehat{Tuition}_i$, are consistent for their true values under the assumptions described above.

4 Data

The primary dataset used is the Geocode file of the 1979 National Longitudinal Survey of Youth (NLSY79) (Bureau of Labor Statistics, 2023). The NLSY79 is a longitudinal, nationally representative survey of 12,686 youths who were 14 to 22 years old when they were first surveyed in 1979. Respondents were interviewed annually from 1979 to 1994 and have been interviewed biannually since then. This data source is attractive because it provides a wide variety of proxies for individuals' academic abilities, as well as family characteristics that are predictive of college attendance. Additionally, this dataset contains data on college-age individuals both before and after the termination of the Social Security Student Benefit, facilitating a validation exercise that I describe in Section 6, in which I compare the estimated effect of this policy on perceived returns (using the methods of this paper) to the likely effect implied by institutional details.

I make no ex ante sample restrictions, such as limiting the sample to males, as has been done in some existing research (Cunha, Heckman, and Navarro, 2005; Carneiro, Heckman, and Vytlacil, 2011; Ashworth, Hotz, Maurel, and Ransom, 2021; Mogstad, Torgovitsky, and Walters, 2021). I drop 5,596 individuals for missing or logically inconsistent values for variables of interest. Further details on data construction are provided in Appendix A.

Estimating perceived returns to college using the method described in Section 3.3 requires observation of college attendance, S_i, tuition for individuals who attend college, $Tuition_i^*$, determinants of potential tuition, Z_i, and determinants of perceived returns, X_i, where at least one variable in Z_i is excluded from X_i and vice versa. The NLSY79 provides information on the college(s) that individuals attended by year, allowing measurement of college attendance. It also provides information on financial aid received during college by year, which allows me to determine $Tuition_i^*$ by combining reported financial aid by year with information on college-specific tuition by year from the Higher Education General Information Survey (HEGIS) (U.S. Department of Education, 2023a) or the Integrated Postsecondary Education Data System (IPEDS) (U.S. Department of Education, 2023b), depending on the year. The NLSY79 includes a bevy of personal background characteristics, described below, that are included in both X_i and Z_i. I instrument for tuition with enrollment-weighted average local tuition at public four-year colleges in the county (or state, if no college is present in the county) of residence at age 17, including this variable in Z_i but not in X_i. Finally, I allow a binary indicator for the presence of a four-year, public college in county of residence at age 14 to affect perceived returns but not tuition. Table 1 lists the variables in each equation.

Table 1: List of Variables Included and Excluded in Each System

Variable Name	Tuition Observation (Z_{1i})	Tuition Imputation (Z_i)	Perceived Returns (X_i, $\widehat{Tuition_i}$)
Nearby College at Age 14	Yes	No	Yes
Local Tuition at Age 17	Yes	Yes	No
Imputed Tuition	No	No	Yes
Female Indicator	Yes	Yes	Yes
Race Indicators	Yes	Yes	Yes
AFQT	Yes	Yes	Yes
Parents Together	Yes	Yes	Yes
Mother's Education Years	Yes	Yes	Yes
Number of Siblings	Yes	Yes	Yes
Permanent State Unemployment at Age 17	Yes	Yes	Yes
Permanent County Wage at Age 17	Yes	Yes	Yes
State Unemployment at Age 17	Yes	Yes	Yes
County Wage at Age 17	Yes	Yes	Yes
Urban Residence at Age 14	Yes	Yes	Yes
Cohort Indicators	Yes	Yes	Yes

Notes: I assume the presence of college at age 14 affects attendance without affecting potential tuition. I also assume that local tuition at age 17 affects tuition without otherwise affecting perceived returns. All other variables are allowed to affect both potential tuition and perceived returns.

I define someone as a college attendee only if they report attending any college at age 23 or younger. Human capital theory suggests that educational investments in early life may be more valuable than later ones (Ben-Porath, 1967), which suggests that early adulthood college attendance may be a relatively more attractive target for financial aid interventions than later life college attendance. I include individuals who do not graduate high school as non-college attenders.[14]

The model above describes a college attendance decision, but is silent on whether $Tuition_i^*$ is one-year or four-year tuition. I use four-year tuition on the assumption that agents attend

[14]Financial aid policy changes could affect high school graduation by increasing the perceived option value of a high school degree, via an increase in perceived returns to college. It follows that excluding high school dropouts from analyses could miss a policy-relevant population.

college primarily because they plan, at the time of initial attendance, to incur four years of costs. Accordingly, I expect the local tuition at age 17 tuition instrumental variable to affect college attendance not only via affecting first-year tuition, but via subsequent year tuition, which would produce an exclusion violation if tuition were coded as first-year tuition.[15]

To calculate net tuition for individuals who attended college, I assign college-specific in-state or out-of-state sticker prices to individuals based on the geographic location of the colleges they attended and their state of residence in the year prior to attending. I then subtract student-reported financial aid from the NLSY79 data from the sticker price to define net tuition for each year, defining four-year tuition as the average per-year amount multiplied by four, which produces an imputation of potential four-year tuition for college dropouts. Not all colleges respond to the HEGIS and IPEDS surveys, so I impute missing values for in-state tuition, out-of-state tuition, and enrollment for each college using its expected value for each conditional on the mean value among similar colleges in a given year (with respect to public and four-year statuses) and its average ratio of these values compared to the mean value among similar colleges in other years. Imputation is relatively rare for public four-year colleges (which are used in the local tuition instrument), as these generally have good data coverage across years. Because estimates of perceived returns inherit their scale from tuition, I convert dollar values into year 2000 dollars for comparison to studies in the financial aid program literature, which commonly report effects in year 2000 dollars for cross-study comparisons, such as those in Deming and Dynarski (2010).

I estimate the effect of local tuition at age 17 on potential tuition using a Heckman correction in which the presence of a public four-year college in the county of residence at age 14 is allowed to affect college attendance, but is assumed to have no effect on potential tuition. If the presence of a nearby college is exogenous conditional on other covariates and only affects college attendance by inducing marginal college attendees to attend college, then this exclusion restriction will be valid. If families of students with preferences for expensive (e.g. high quality) colleges systematically choose to live near public four-year institutions, this exclusion restriction will be violated. The same issue arises when using the presence of a nearby college as an instrument for college attendance to estimate returns to college, so I follow the returns to college literature in addressing it by including student ability and local economic health control

[15] If the effect of local tuition at age 17 on tuition is constant across years, the exclusion violation using 1-year observed tuition is a technicality (the effect would be 1/4 of the effect on four-year tuition). This is not guaranteed to hold, so I use four-year costs.

variables.[16]

A separate issue is that even if conditionally exogenous, the presence of a nearby college at age 14 could directly affect tuition by shifting individuals between different versions of college, as described by Harris (2022). For example, suppose the college decision is ordered as non-college, cheap college, then expensive college. If the presence of a nearby college causes individuals on the margin of non-college and cheap college to attend cheap college and causes individuals on the margin of cheap college and expensive college to attend cheap college, the method described here will produce upward bias in the estimated correlation between preference for college attendance and potential tuition. This is because it attributes the entire difference in tuition between locations with and without nearby colleges to the potential tuition of individuals on the margin between non-college and college, who are in truth only driving part of the effect. I assume that individuals on the margin between college and non-college are much more responsive to the presence of a nearby college than individuals on the margin between cheap college and expensive college, who likely have stronger preferences about the college they attend.

I instrument for tuition using enrollment-weighted average local tuition at public four-year colleges in the county or state of residence at age 17, using state only for individuals who do not reside in a county with such a college. The tuition amounts and student enrollment for each college, by location, is obtained from IPEDS (for 1980, 1984, and subsequent years) or HEGIS (for other years). Local tuition at age 17 has been used as an instrument for college attendance in existing work (Kane and Rouse, 1995; Cameron and Heckman, 1998, 2001; Carneiro, Heckman, and Vytlacil, 2011), with the argument that it is uncorrelated with unobserved determinants of ex post pecuniary returns to college. I argue that it is likely that the same unobserved factors that determine ex post pecuniary returns also determine ex ante perceived returns and that local tuition affects attendance via pecuniary costs, rendering the instrument similarly valid in the setting of this paper. The validity of this instrument is threatened by unobserved factors correlated with local tuition and perceived returns to college, such as parental characteristics or the health of the local economy.

I address the potential for correlation between local tuition at age 17 and unobserved determinants of perceived returns by controlling for a variety of personal background characteristics and indicators of local economic prosperity. A major concern is the possibility that high quality

[16] For instance, Carneiro and Heckman (2002) and Cameron and Taber (2004) find that AFQT exam scores are correlated with the presence of a nearby college at age 14, so I control for AFQT.

colleges are expensive and are also located in generally attractive locations, which may draw economically successful parents whose children have high returns to college. I address this potential for correlation between local tuition and the quality of the local geography by controlling for average wages in the county of residence at age 17 (obtained from the Bureau of Economic Analysis), the unemployment rate in the state of residence at age 17 (obtained from the Bureau of Labor Statistics), "permanent" versions of the preceding two variables constructed as their averages from 1976–2000, and an indicator for residence in an urban location at age 14. Because parent characteristics in a geographic location are likely to be incompletely captured by these labor market indicators, I also include controls for maternal educational attainment, family size, and an indicator for living with both biological parents from birth to age 18. To address the potential for persistent differences in academic ability between students living in different geographic locations that are not captured by the above controls, I also control for student characteristics. Namely, I control for gender, race, cohort fixed effects, and Armed Forces Qualification Test (AFQT) scores. Hansen, Heckman, and Mullen (2004) show that years of schooling at the time of testing affects AFQT scores, so rather than using raw scores, I use the residual of each test score after controlling for years of education.

Table 2 shows mean values of the variables described above for college attendees and non-college attendees. College attendees are more likely to live near a college at age 14 and have lower local tuition at age 17, suggesting that these variables are meaningful for determining college attendance. College attendees predictably have higher AFQT test scores, are more likely to live with both biological parents, have more-educated parents, and have fewer siblings. College attendees also more likely to female and less likely to be Hispanic, with Black people attending college at similar rates to the sample average.

5 Results

In this section I discuss estimates of perceived returns to college from the estimation procedure described in Section 3.3. My preferred specification estimates perceived returns to college attendance under the assumption that individuals attend college with an expectation of paying four years of tuition, which is implemented by coding individuals as college attendees if they attend at least one year of college and by defining $Tuition_i^*$ in the model as a college attendee's four year college tuition net of financial aid. The parameter estimates of this model are shown

Table 2: Means of Included Variables

	Overall (1)	Non College Attendees (2)	College Attendees (3)
College Attendee	0.443	0.000	1.000
Nearby College at Age 14	0.564	0.535	0.600
Local Tuition at Age 17	1.438	1.447	1.427
4 Year Net Tuition	4.797	.	4.797
Female	0.511	0.490	0.538
Black	0.259	0.265	0.251
Hispanic	0.155	0.172	0.134
AFQT	-0.000	-0.226	0.285
Parents Together	0.604	0.546	0.678
Mother Education Years	10.827	9.894	12.001
Number of Siblings	3.799	4.287	3.185
Permanent County Wage at Age 17	23.491	23.187	23.874
Permanent Unemployment at Age 17	6.482	6.487	6.475
Average County Wage at Age 17	19.473	19.275	19.722
State Unemployment at Age 17	6.955	6.983	6.920
Urban Residence at Age 14	0.784	0.762	0.811
Age 1979	17.251	17.224	17.285
Sample size	7090	3952	3138

Notes: Means are of all NLSY79 samples. All dollar values are adjusted to year 2000 values using a 3% interest rate. Education-corrected-AFQT scores are transformed to have within-sample unit variance and zero mean. Parents Together indicates that both biological parents were in the home for all years from birth to age 18.

in Table 3, with the implied distribution of perceived returns shown graphically in Figure 1.

It is possible to estimate perceived returns to college completion using the same method. This is straightforward to implement by using a college completion binary indicator in place of the college attendance indicator in the model, while defining $Tuition_i^*$ as observed total net tuition for students who graduate rather than four-year tuition for those who attend. Completion may be more normatively important than attendance because of returns to attendance largely being driven by the option value from completion. However, in the dataset used in this paper, local tuition at age 17 is an extremely weak predictor of college completion, while being a strong predictor of college attendance. It may be that financial incentives are inferior policies from a cost-effectiveness standpoint relative to other policies that improve college experiences upon attendance. Regardless, the weak statistical relationship between local tuition and completion in the present sample leaves immense uncertainty in perceived returns parameter estimates that precludes policy-relevant inference regarding likely college completion rates under counterfactual tuition regimes. I show results on estimated perceived returns to college completion in Appendix B.

Table 3: Perceived Returns Estimates

	Tuition Observation (1)		Tuition Imputation (2)		Perceived Returns (3)	
Nearby College at Age 14	0.118	(0.049)			3.329	(2.111)
Local Tuition at Age 17	-0.111	(0.045)	3.009	(0.716)		
Female	0.167	(0.039)	2.380	(0.575)	6.678	(2.372)
Black	0.430	(0.057)	-4.038	(0.918)	6.816	(5.480)
Hispanic	0.549	(0.075)	0.296	(1.000)	14.083	(7.257)
AFQT	0.347	(0.024)	1.798	(0.488)	10.391	(4.107)
Parents Together	0.366	(0.044)	3.752	(0.775)	12.864	(4.431)
Mother Education Years	0.181	(0.010)	1.266	(0.211)	5.763	(2.140)
Number of Siblings	-0.054	(0.010)	-0.528	(0.164)	-1.876	(0.732)
Permanent Unemployment at Age 17	0.046	(0.030)	0.084	(0.421)	1.283	(1.241)
Permanent County Wage at Age 17	0.044	(0.012)	1.057	(0.185)	2.046	(0.584)
State Unemployment at Age 17	-0.015	(0.019)	0.019	(0.279)	-0.317	(0.689)
Average County Wage at Age 17	-0.053	(0.014)	-0.806	(0.214)	-2.020	(0.735)
Urban Residence at Age 14	0.099	(0.054)	0.488	(0.813)	2.898	(2.090)
Age 15 in 1979	-0.000	(0.127)	-0.053	(2.223)	-0.072	(4.230)
Age 16 in 1979	0.024	(0.132)	0.921	(2.360)	1.668	(4.420)
Age 17 in 1979	-0.029	(0.153)	-1.869	(2.622)	-2.150	(5.110)
Age 18 in 1979	-0.146	(0.157)	0.477	(2.619)	-2.752	(5.491)
Age 19 in 1979	-0.180	(0.155)	-2.918	(2.755)	-6.968	(5.600)
Age 20 in 1979	-0.245	(0.155)	-3.818	(2.764)	-9.157	(5.822)
Constant	-2.515	(0.241)	-29.072	(4.270)	-91.740	(31.788)
Additional Parameters						
atanh(ρ)	0.372	(0.125)				
ln(σ_u)	2.663	(0.038)				
σ_η	25.174	(11.824)				
F-stat for Local Tuition in (2)	80.569					
Log Likelihood	-7.158e+09					
Sample Size	7,090					

Notes: Estimates are from maximizing the likelihood in (27) with 1988 sample weights, where columns (1) and (2) are equivalent to maximum likelihood estimates of a Heckman (1979) model of the effect of local tuition on (selectively observed) tuition, and equations (2) and (3) are equivalent to estimates of an instrumental variables probit for the effect of tuition on perceived returns, using local tuition as an instrument and restricting the effect of tuition on perceived returns to -1. Parameters in column (3) are estimated marginal effects of each variable on perceived returns to college in thousands of dollars. Robust standard errors in parentheses.

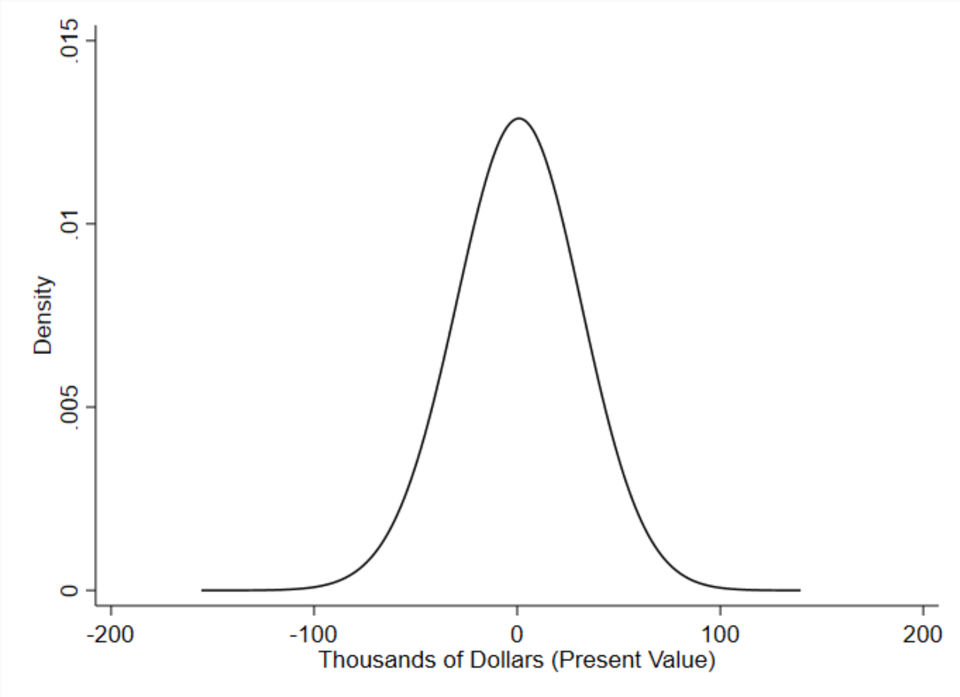

Figure 1: Perceived Returns to College

Notes: Perceived returns to college implied by model estimates in columns (2) and (3) of Table 3. The distribution is a mixture of N normals, weighted by 1988 sample weights, with means given by $X_i\hat{\theta} - \widehat{Tuition}_i$ and variances given by $\hat{\sigma}^2_\eta$.

Columns (1) and (2) of Table 3 show point estimates that are identical to the first stage and second stage of a Heckman (1979) model, estimated by maximum likelihood. Importantly, nearby college at age 17 significantly predicts college attendance (observation of tuition), which is important for imputation of tuition. Local tuition at age 17 is also a strong predictor of attendance as well as strongly predicting tuition, with sensible signs and magnitudes; a \$1 increase in local tuition causes an estimated \$3.01 increase in four-year tuition. The positive effect of local tuition on four-year tuition is consistent with agents who face costly local colleges tending to pay more due to attending those colleges, while the effect being less than \$4 suggests that they also sometimes seek out cheaper alternatives in other locations. Column (3) shows point estimates of effects of variables on perceived returns, in thousands of (year 2000) dollars. These point estimates are identical to those of a probit with college attendance as the outcome, including the variables listed along with $\widehat{Tuition}_i$, with all coefficients multiplied by the scale parameter σ_η (such that the coefficient on $\widehat{Tuition}_i$ is -1).

I estimate $\text{atanh}(\rho) = 1/2 \ln(1+\rho)/(1-\rho)$, $\ln(\sigma_u)$, and σ_η in addition to coefficients on variables. The estimated value of $\text{atanh}(\rho)$ implies that $\hat{\rho} = -0.495$, suggesting negative correlation between unobserved determinants of college attendance propensity and unobserved determinants of net tuition. This is identified by attributing differences in observed net tuition between individuals who live near a college at age 14 and those who don't to the former group including college attendees with lower intrinsic interest in college, who attend college if and only if they live near one. The estimated value of $\ln(\sigma_u)$ implies that $\hat{\sigma}_u = 14.33$, which is the standard deviation of the residual in the tuition equation. Finally, $\hat{\sigma}_\eta$ gives the scale of perceived returns. It is identified by comparing college attendance rates of otherwise similar individuals who face different tuition costs as driven by local tuition at age 17, where the mass of individuals shifted into college attendance by a given change in known tuition is determined by the variance of the perceived returns distribution.

The perceived returns coefficients in column (3) of Table 3 give marginal effects of variables in thousands of dollars, and are sufficient to compare perceived returns between groups. For instance, the coefficient on the Black race indicator implies that Black individuals perceive returns to college that are \$6,816 higher than those of otherwise similar white individuals (the omitted group), with a stronger similar pattern among Hispanic individuals. Minorities may perceive particularly high value from education if they anticipate statistical discrimination in labor markets, as described by Lang and Manove (2011). As seen in Table 2, women are somewhat more likely to attend college than to men. Table 3 shows that their increased propensity is not fully explained by other covariates such as test scores, with a positive and significant coefficient on the Female indicator variable.

With the exception of minority race indicators, the variables generally associated with positive academic and labor market outcomes tend to positively predict perceived returns. For instance, individuals with a standard deviation higher AFQT scores have over \$10,000 higher perceived returns to college. Other than test scores, variables associated with high socioeconomic status, such as parental education, also predict high perceived returns. For instance, the results suggest that an individual with a college-educated mother (16 years of schooling) would have perceived returns to college that are over \$23,000 higher than an individual with a high-school educated mother (12 years of schooling), all else equal.

If my estimates of perceived returns to college are biased, it is likely that they overestimate the variance of the distribution, and therefore will predict inaccurately small reactions to financial aid policy changes. First, if local tuition at age 17 is positively correlated with the unobserved component of perceived returns, for instance because the local labor market controls are insufficient to completely proxy for positive local amenities, it will positively bias estimates of the scale parameter, σ_η. In this case, the comparison between students with high and low pecuniary costs for college is also a comparison between students with high and low unobserved preferences for college, muting apparent responsiveness to cost shifters. The model would rationalize this with high variance in perceived returns.

Second, if prospective college goers are not completely aware of variation in potential tuition insofar as it is driven by local tuition, this will cause attenuation bias in the estimated effect of perceived tuition on attendance. The attenuation bias arises here for the same reason it arises in effects of variables affected by classical measurement error – some of the variation in the measured variable has no effect on behavior. It is possible that students instead overreact to differences in prices, with students with high local tuition believing their individual tuition will be very high and students with low local tuition believing their individual tuition will be very low, which would cause bias in the opposite direction.[17] This sort of behavior strikes me as unlikely, as I expect individuals to underreact to this sort of information rather than overreact. As above, in such a case the model would rationalize low responsiveness to tuition differences with high variance in perceived returns to college.

An intuitive test of the validity of the estimates is to attempt to predict effects of financial aid policies that have been implemented. If the local tuition instrument is invalid, as described above, we would expect to predict inaccurately small responses to well-publicized financial aid policy shocks. For the same reasons, if we attempt to estimate the magnitudes of financial aid awarded by such policies (by including policy change indicators in the model), we should expect to overestimate award amounts relative to documented policy details. I investigate the robustness of the model estimates in the next section.

6 Robustness and Policy Counterfactuals

The estimates of perceived returns to college are useful for predicting effects of counterfactual policies that shift perceived returns by predictable amounts, such as price changes. In this section, I explore predicted effects of various financial aid policies on college attendance and costs. I begin by estimating the effect of the elimination of the Social Security Student Benefit (SSSB) on perceived returns to college and comparing the estimated effect to the average amount of the benefit made available to the eligible population, in order to assess the validity of identi-

[17]Systematic misperceptions regarding tuition that are independent of local tuition will not cause bias, as effects are estimated by comparing across groups.

fication assumptions described in preceding sections. Afterward, I derive the cost-minimizing, conditional-on-observables financial aid policy for an arbitrary college attendance rate target. Finally, I compare college attendance and government costs between the cost-minimizing financial aid policy, the baseline, and some simple alternatives.

6.1 Social Security Student Benefit

The Social Security Student Benefit was a policy that paid substantial sums of money to children of deceased, disabled, or retired Social Security beneficiaries who were between the ages of 18 and 22 and who attended college full time. This policy was eliminated in 1981, and individuals not enrolled in college by May 1982 were ineligible for the benefit, while those who began prior to May 1982 had benefits reduced. The average policy benefit in 1980 was $6,700 (year 2000 dollars). The elimination of this policy was found to have substantial negative effects on college attendance among previously eligible populations by Dynarski (2003).

To estimate the effect of the SSSB on perceived returns to college, I would ideally use an indicator variable for SSSB eligibility. Unfortunately, eligibility is a function of multiple parent characteristics (such as earnings histories) that are not present in the NLSY79. I follow Dynarski (2003) and proxy for program eligibility with an indicator for having a deceased father and a high school senior year prior to 1982, which may miss some eligible individuals without deceased fathers. I code student's high school senior year as their actual year of graduation (most students) or the year they would have completed high school, had they continued their education, for students who did not graduate. I drop 829 individuals who have high school graduation years outside of 1977 and 1983, as these are the years in which students in my sample would have had on-time graduation.[18] Including a (likely) SSSB receipt indicator in the model described in Section 3 will provide an estimate of the extent to which the SSSB predicts perceived returns to college, but will not necessarily identify the effect of the policy because receipt of the SSSB may be correlated with unobserved determinants of perceived returns to college.

To estimate the effect of the policy, I use a difference-in-differences research design, which compares differences in college attendance between individuals with and without deceased fathers who graduated high school before and after 1981. I implement this by including two-way

[18]Results are robust to including all high school senior years, I omit extra years because senior years outside of 1977-1983 have very few individuals, leading to imprecision in the event study shown in Figure 2.

fixed effects for both having a deceased father and for high school senior year in all equations along with the other variables mentioned in Table 1 when maximizing the likelihood in (27), while including the SSSB indicator only in the perceived returns equation. The restriction of the SSSB indicator to the perceived returns equation forces the model to ignore any effect of the SSSB on tuition paid (for instance, due to recipients choosing more expensive colleges) and to only infer the magnitude of the perceived financial benefit by comparing the effect of the SSSB on the college attendance extensive margin to that of local-tuition-induced-tuition. Because the SSSB policy change happened nationwide in a single moment in time, two-way fixed effects are sufficient to identify the treatment effect of the policy under a parallel trends assumption. Following Dynarski (2003), I include interactions between control variables and indicators for having a deceased father as well as having a high school senior year prior to 1982 to allow for group-specific trends in college attendance over time, such as the secular decrease in Black college attendance that occurred at the same time.

The parallel trends assumption holds if the perceived returns to college of individuals with and without deceased fathers would have differed only by a constant in the absence of the policy change. This assumption cannot be tested for the time period in which the policy shock occurs, but we can test it in previous periods with an event study. To perform this test, I interact the deceased father indicator with each year and include these indicator variables in the perceived returns to college equation when estimating the likelihood in (27). One such year will be colinear with the constant, so I omit the year immediately prior to the termination of the policy, 1981, such that all year-specific effects are relative to that year. This gives year-specific effects of the SSSB elimination, which should be zero in the years where the SSSB was not eliminated if the parallel trends assumption holds in those years. The event study plot investigating the validity of the parallel trends assumption prior to the policy termination is shown in Figure 2. I fail to reject the null that the trends are parallel with a p-value of 0.775.

The estimated effect of the SSSB on perceived returns to college, as shown in Table 4, is \$21,312. This result is statistically significant at the 90% level. More importantly, I test the hypothesis that the SSSB increased perceived returns by \$26,800, the average policy reward in 1980, and I fail to reject this with a p-value of 0.638. The ability of the estimation procedure to return a plausible value for the effect of a known cost shifter suggests that the identification assumptions described above are valid, and model estimates can be credibly used to forecast effects of other policy changes.

Table 4: Estimated Effect of Social Security Student Benefit on Perceived Returns

	Tuition Observation		Tuition Imputation		Perceived Returns	
	(1)		(2)		(3)	
SSSB					21.312	(11.660)
Nearby College at Age 14	0.132	(0.052)			3.192	(1.953)
Local Tuition at Age 17	-0.128	(0.047)	2.880	(0.723)		
Female	0.046	(0.092)	1.736	(1.573)	2.723	(2.681)
Black	0.708	(0.120)	-3.719	(2.073)	11.697	(7.915)
Hispanic	0.584	(0.144)	0.407	(2.036)	12.880	(7.012)
AFQT	0.569	(0.052)	1.947	(1.010)	14.269	(5.819)
Parents Together	0.333	(0.094)	6.785	(2.083)	12.625	(3.925)
Mother Education Years	0.139	(0.021)	1.248	(0.334)	4.100	(1.389)
Number of Siblings	-0.076	(0.022)	-0.663	(0.518)	-2.208	(0.994)
Permanent Unemployment at Age 17	-0.071	(0.063)	-0.237	(0.920)	-1.657	(1.738)
Permanent County Wage at Age 17	0.005	(0.022)	1.014	(0.399)	1.069	(0.642)
State Unemployment at Age 17	0.075	(0.030)	0.147	(0.412)	1.760	(0.893)
Average County Wage at Age 17	-0.009	(0.025)	-0.485	(0.416)	-0.661	(0.702)
Urban Residence at Age 14	0.144	(0.114)	-1.579	(2.067)	1.616	(3.840)
Before x Female	0.109	(0.103)	0.874	(1.693)	3.299	(3.204)
Before x Black	-0.252	(0.134)	-0.098	(2.078)	-5.762	(4.617)
Before x Hispanic	0.044	(0.165)	-0.620	(2.215)	0.427	(4.549)
Before x AFQT	-0.280	(0.059)	-0.253	(0.934)	-6.467	(3.277)
Before x Parents Together	0.003	(0.105)	-3.397	(2.135)	-1.622	(3.456)
Before x Mother Education Years	0.037	(0.023)	-0.031	(0.341)	0.900	(0.780)
Before x Number of Siblings	0.028	(0.023)	0.147	(0.528)	0.632	(0.779)
Before x Permanent Unemployment at Age 17	0.086	(0.068)	0.122	(0.988)	2.047	(2.051)
Before x Permanent County Wage at Age 17	-0.000	(0.025)	-0.041	(0.433)	0.008	(0.729)
Before x State Unemployment at Age 17	-0.091	(0.035)	0.148	(0.481)	-1.935	(1.283)
Before x Average County Wage at Age 17	0.009	(0.029)	-0.292	(0.469)	-0.111	(0.822)
Before x Urban Residence at Age 14	-0.025	(0.129)	2.696	(2.179)	1.981	(3.841)
Deceased Father	-0.711	(0.863)	5.134	(11.053)	-29.078	(27.428)
DF x Female	0.312	(0.185)	3.859	(2.564)	11.531	(6.303)
DF x Black	-0.089	(0.232)	1.081	(2.935)	3.034	(6.415)
DF x Hispanic	0.914	(0.389)	4.261	(4.545)	28.772	(15.145)
DF x AFQT	-0.071	(0.114)	-0.187	(1.511)	0.267	(2.992)
DF x Mother Education Years	0.076	(0.044)	-0.088	(0.612)	1.825	(1.426)
DF x Number of Siblings	-0.013	(0.038)	-0.439	(0.721)	-0.558	(1.183)
DF x Permanent Unemployment at Age 17	0.069	(0.106)	0.736	(1.088)	-0.429	(2.880)
DF x Permanent County Wage at Age 17	-0.070	(0.036)	-0.418	(0.549)	-3.168	(1.652)
DF x State Unemployment at Age 17	-0.011	(0.057)	-0.846	(0.611)	0.312	(1.639)
DF x Average County Wage at Age 17	0.093	(0.042)	0.472	(0.597)	4.182	(2.098)
DF x Urban Residence at Age 14	-0.619	(0.249)	-0.579	(3.195)	-13.731	(8.936)
Senior Year 1977	0.121	(0.096)	-2.438	(1.290)	0.523	(2.971)
Senior Year 1978	0.077	(0.085)	-2.770	(1.341)	-1.075	(2.556)
Senior Year 1979	-0.012	(0.079)	0.406	(1.109)	0.172	(2.219)
Senior Year 1980	0.043	(0.071)	-0.273	(1.079)	0.664	(2.046)
Senior Year 1982	0.389	(0.471)	-6.355	(6.660)	5.016	(14.039)
Senior Year 1983	-0.009	(0.474)	-5.686	(6.759)	-2.915	(13.183)
Constant	-2.421	(0.252)	-27.459	(4.109)	-79.920	(26.645)
Additional Parameters						
atanh(ρ)	0.313	(0.131)				
ln(σ_u)	2.650	(0.040)				
σ_η	21.641	(9.880)				
F-stat for Local Tuition in (2)	60.210					
Log Likelihood	-6.648e+09					
Sample Size	6,261					

Notes: Estimates obtained by maximizing the likelihood in (27) with 1988 sample weights. The coefficient on SSSB is the Difference-in-Differences estimate of the effect of Social Security Student Benefit on perceived returns. Before is an indicator for having a high school senior year prior to 1982. DF is shorthand for deceased father. Robust standard errors in parentheses.

Figure 2: Social Security Student Benefit Event Study

Notes: Event study of the estimated effects of the Social Security Student Benefit termination over time on perceived returns to college under the parallel trends assumption. The SSSB is in effect for all periods to the left of the vertical line, and is not in effect for those to the right. The final year prior to policy termination (1981) is omitted. I fail to reject the null that all pre-period effects before 1981 are equal to zero with a p-value of 0.775.

The point estimate for the SSSB is below the average four-year policy award, which could be due to substantive issues beyond sampling noise. One possibility is that estimated effect is biased. The effect of the SSSB on perceived returns is identified by comparing its estimated effect on college attendance to that of tuition as instrumented by local tuition at age 17. If the local labor market and other controls are insufficient to absorb omitted variable bias that drives positive correlation between local tuition and the perceived returns to college residual, the effect of local tuition on college attendance will be biased in the positive direction (toward zero). This would cause upward bias in the effect of the SSSB on perceived returns, as the effect of the SSSB would be large compared to local tuition, which identifies the scale of the estimates. Alternatively, as seen in column (2) of Table 4, $1 of local tuition is estimated to increase four-year tuition costs by $2.88. Constraining this effect to $4 (imposing that perceived pecuniary college costs are exactly local tuition for four years) would imply an estimated effect of the SSSB on perceived returns of $29,600. Additionally, as described above, I code SSSB eligibility using deceased father, which likely misses some eligible individuals (for instance, those with deceased mothers with sufficient earnings histories). This will attenuate the effect estimates depending on the share of eligible individuals who do not have deceased fathers.

Another possibility for the estimated effect being below the expected amount of $26,800 is that recipients actually valued the SSSB at an amount below the average four-year award amount when deciding whether to attend college. This would happen if they expected less than four years of benefits, either due to expecting to drop out of college, expecting to graduate early, or expecting an early termination of the policy. It could be that the individuals receiving the benefit prior to its elimination may have worried that their benefits would not last, though Dynarski (2003) describes limited scope for anticipation of the policy elimination. It is also possible that individuals who did not receive the SSSB believed they would receive it, for instance by relying on the benefit receipt of an older sibling to predict their own benefit receipt rather than relying on official correspondence from the government. This would reduce the effect of actual SSSB eligibility on perceived returns to college.

Despite these caveats to the interpretation of the estimated effect of the SSSB, I view the value being close to $26,800 as validating the model. In the next section, I derive the policy that reaches an aggregate attendance target at minimum cost via conditional subsidies using the estimates of perceived returns to college from Section 5.

6.2 Attendance Target with Cost-Minimization

The evidence on the external validity of the estimation procedure provided by matching the effect of the SSSB increases my confidence in using my estimates of perceived returns to predict the effects of other potential policies. Here I derive the cost-minimizing policy that reaches a given attendance target using positive financial aid offers, given the results from Section 5. I will choose $\mathbb{E}[S_i] = 75\%$ as the target level of college attendance, but any target can be chosen. The cost-minimizing schedule of student aid conditional only on observables, restricting financial aid to positive values, is shown in Figure 3, with the resulting shift in perceived returns to college shown in Figure 4.

To derive the cost-minimizing financial aid offers for all individuals, I note that the government's ideal problem is to choose positive financial aid offers a_i, to each individual indexed by i in a population of N young adults in order to induce at least a share A of them to attend

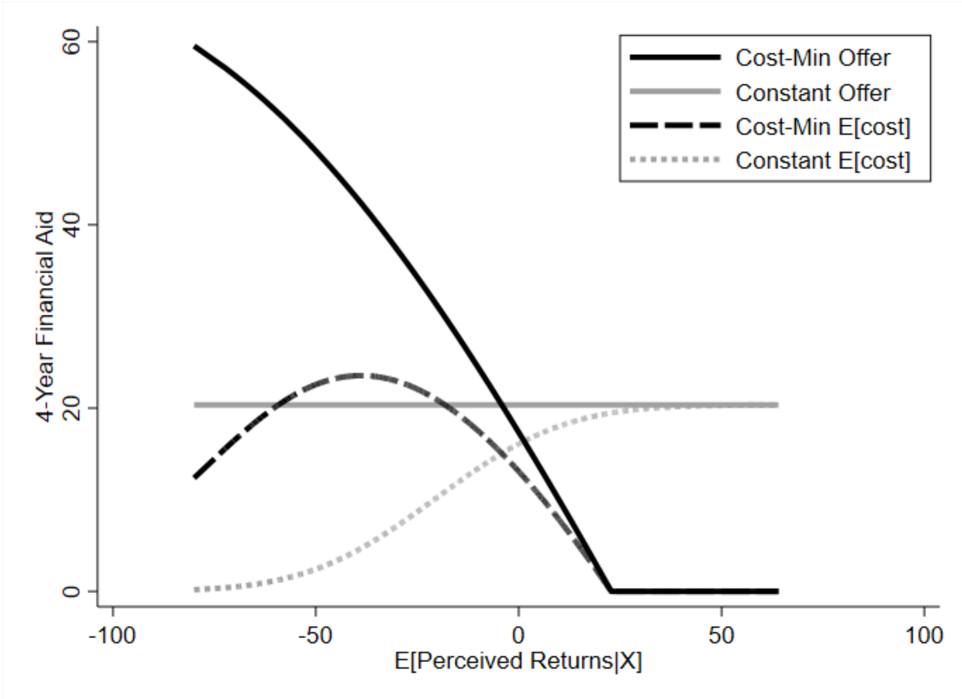

Figure 3: Offers and Expected Costs for Selected Policies

Notes: Financial aid offers over the support of $\hat{\tilde{\pi}}$ with an expected attendance target of 75%. The constant offer policy chooses a constant financial aid offer, $a_i = a$, while the cost-minimizing offer targets aid on observables. Expected costs under each policy are given by conditional expectations of attendance given observables multiplied by offer amount.

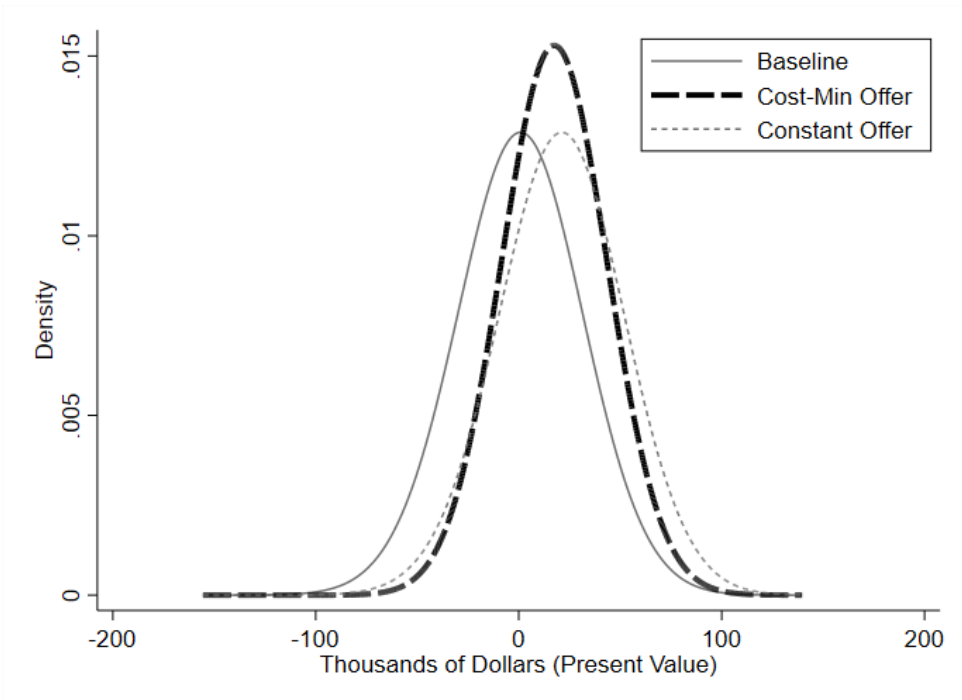

Figure 4: Perceived Returns Under Alternative Policies

Notes: Perceived returns to college at baseline alongside counterfactual policies. The cost-minimizing offer policy allocates financial aid to students conditional on observables in order to achieve 75% expected attendance at minimum cost. The constant offer policy gives a common financial aid offer to all students in order to achieve 75% expected attendance. Visual comparison of perceived returns under these policies shows that the cost-minimizing policy shifts perceived returns to college less for individuals with high perceived returns than for those with low perceived returns.

college at minimal cost. Formally, the government would like to solve

$$\begin{aligned} \min_{a_1,a_2,\ldots,a_N} \quad & \sum_{i=1}^{N} a_i S_i(a_i) \\ \text{s.t.} \quad & \sum_{i=1}^{N} \frac{S_i(a_i)}{N} \geq A \\ & a_i \geq 0 \ \forall i. \end{aligned}$$

I assume that the government does not know $S_i(a_i)$ for any individual with certainty, so it cannot solve this problem. Instead, I assume that it minimizes expected costs subject to an targeted expected college attendance rate, predicts college attendance for each individual using the estimated model from Section 5, and that it knows Z_i for all individuals. It follows that the government can solve a feasible version of the above problem by replacing all terms with their expectations conditional on Z_i, and conditioning only on Z_i to offer financial aid. Recalling from the model that

$$\mathbb{E}\left[S_i | Z_i, a_i\right] = \Phi\left(\frac{\hat{\tilde{\pi}}_i + a_i}{\hat{\sigma}_\eta}\right),$$

with $\hat{\tilde{\pi}} = X_i\hat{\theta} - \widehat{Tuition}$, the government's feasible problem is

$$\begin{aligned} \min_{a(Z_i)} \quad & \sum_{i=1}^{N} a_i \Phi\left(\frac{\hat{\tilde{\pi}}_i + a_i}{\hat{\sigma}_\eta}\right) \\ \text{s.t.} \quad & \frac{\sum_{i=1}^{N} \Phi\left(\frac{\hat{\tilde{\pi}}_i + a_i}{\hat{\sigma}_\eta}\right)}{N} \geq A \\ & a_i \geq 0 \ \forall i. \end{aligned} \tag{29}$$

The first order condition for a_i imposes a constant ratio of marginal costs to marginal benefits for individuals for whom the constraint $a_i \geq 0$ does not bind,

$$\psi = \frac{\psi^N}{N} = \frac{\Phi\left(\frac{\hat{\tilde{\pi}}_i + a_i}{\hat{\sigma}_\eta}\right)}{\phi\left(\frac{\hat{\tilde{\pi}}_i + a_i}{\hat{\sigma}_\eta}\right)} \hat{\sigma}_\eta + a_i. \tag{30}$$

Note that ψ is strictly monotonically increasing in a_i, which implies that a unique value of a_i, a_i^ψ, solves this equation for each such individual for a given value of ψ.[19] For a_i^ψ that solves equation (30), the constraint that $a_i \geq 0$ for all individuals implies that each individual will

[19] This will be true for any symmetric, log-concave distribution (such as the normal). This is a sufficient condition but not a necessary one, as this condition guarantees that the first term is monotonically increasing in a_i.

have $a_i(\psi) = \max(0, a_i^{\psi})$. Given this financial aid offer function for an arbitrary value of ψ, the constrained cost-minimizing ratio of marginal costs to marginal benefits, ψ^*, satisfies

$$\frac{\sum_{i=1}^{N} \Phi\left(\frac{\hat{\bar{\pi}}_i + a_i(\psi^*)}{\hat{\sigma}_\eta}\right)}{N} = A,$$

which defines unique financial aid for each individual as a function of the expected college attendance target, A, and i's observed characteristics, Z_i.

The cost-minimizing financial aid solution has several interesting features. First, it focuses aid on individuals with low perceived returns who, by definition, rarely attend college and empirically are from low socioeconomic status households. This happens because marginally increasing aid for an individual increases their attendance probability by $\phi((\hat{\bar{\pi}} + a_i)/\sigma_\eta)$ at cost $\Phi((\hat{\bar{\pi}} + a_i)/\sigma_\eta)$, and the latter is large for large values of $\hat{\bar{\pi}}$ while the former is not. In other words, tuition subsidies for individuals with low perceived returns allows the government to avoid offering as much aid to people who would have attended college anyway.

Additionally, individuals with low predicted perceived returns, $\hat{\bar{\pi}}$, that nonetheless respond to aid offers do so because they have high draws from the error term in their perceived returns equation. Carneiro, Heckman, and Vytlacil (2011) and others find that such individuals (those with high unobserved preferences for college) also have relatively high pecuniary returns. Because this policy targets low socioeconomic status individuals who are likely to have relatively high returns while minimizing costs, it can likely serve as a useful heuristic for the government if it seeks to both reduce inequality and induce selection on gains. I conclude discussion of this policy by noting that its solution can easily be modified to provide optimal idiosyncratic financial aid conditional on known actual returns to college or to provide optimal aid conditional on a binding total financial aid budget constraint for the government.

6.3 Additional Counterfactual Policies

This section compares various alternative financial aid policies to each other and to the baseline with respect to costs and college attendance. A summary of the policies' effects on attendance and costs is shown in Table 5. The first policy considered is the baseline. The second ($1k) is a policy that offers $1,000 in aid per year to all individuals. The third policy considered (Cost-Min-75%) is the expected-cost-minimizing policy from Section 6.2, which makes aid offers conditional on observables to induce 75% of individuals to attend college in expectation. The

Table 5: Comparison of Counterfactual Policies

	Baseline (1)	$1k (2)	Cost Min 75% (3)	Constant 75% (4)	URM (5)	First-Gen (6)
Panel 1: Aggregate						
Average Aid Offer	0.000	1.000	4.310	5.088	0.985	3.311
	(.)	(.)	(.)	(.)	(.)	(.)
Expected Attendance	0.507	0.559	0.750	0.750	0.556	0.682
	(0.007)	(0.025)	(0.109)	(0.097)	(0.022)	(0.075)
Expected Costs	0.000	0.559	3.009	3.816	0.674	2.251
	(.)	(0.025)	(0.710)	(0.495)	(0.106)	(0.380)
$\Delta\mathbb{E}$[Attendance]/$\Delta\mathbb{E}$[Cost]	.	9.202	8.062	6.357	7.174	7.761
	(.)	(3.894)	(1.733)	(1.727)	(1.961)	(2.013)
Panel 2: Attendance Inequality						
White-Black	8.050	7.945	2.368	6.199	-17.894	2.476
	(1.508)	(1.514)	(3.095)	(1.949)	(11.044)	(3.077)
White-Hispanic	11.400	12.085	4.703	12.748	-11.345	7.742
	(1.841)	(1.879)	(5.442)	(1.869)	(10.364)	(3.026)
College Parents - First-gen	27.463	26.732	8.676	20.047	22.990	0.611
	(0.750)	(0.831)	(9.047)	(5.030)	(2.306)	(11.473)

Notes: Policy implications for selected groups and policies. Offers and costs are per year in year 2000 dollars. Column (1) describes the baseline policy. Column (2) predicts effects of a $1,000 annual financial aid offer. Column (3) predicts effects of the cost-minimizing policy described in Section 6.2. Column (4) predicts effects of a constant financial aid offer intended to reach the same target attendance rate (75%) as the cost-minimizing policy. Column (5) predicts effects of a policy that offers the constant financial aid offer from (4) only to underrepresented minorities. Column (6) predicts effects of a policy that offers the constant financial aid offer from (4) only to first-generation college students. Robust standard errors in parentheses.

fourth candidate policy (Constant-75%) also induces 75% of individuals to attend college in expectation, but does so with a constant aid offer to all individuals. The fifth policy considered (URM) is an affirmative action-style policy that extends the homogeneous aid offer from the fourth option only to Black and Hispanic students. The sixth policy (First-Gen) is an affirmative action-style policy that extends the homogeneous aid offer from the fourth option only to first-generation college students.

The policies considered differ in their average offer magnitude, their predicted effects on aggregate attendance rates, their expected costs, and their cost-effectiveness, measured here as the ratio of expected changes in attendance to expected costs. They also differ in their effects on educational inequality. Table 5 reports estimated effects of each policy on gaps in college attendance between White and Black students, White and Hispanic students, and students with at least one college educated parent and those without. Table 6 provides a more granular break down by race and parent education of the effects of each policy on college attendance, costs, and the ratio of attendance increases to costs, relative to baseline.

One important characteristic of each candidate policy is its effect on aggregate attendance rates. Comparing, for instance, the $1k policy to the Constant-75% policy, we see that while

Table 6: Comparison of Counterfactual Policies, by Group

	Baseline (1)	$1k (2)	Cost Min 75% (3)	Constant 75% (4)	URM (5)	First-Gen (6)
Panel 1: White						
Average Aid Offer	0.000	1.000	4.083	5.088	0.000	3.115
	(.)	(.)	(.)	(.)	(.)	(.)
Expected Attendance	0.525	0.577	0.756	0.766	0.525	0.690
	(0.008)	(0.025)	(0.103)	(0.095)	(0.008)	(0.070)
Expected Costs	0.000	0.577	2.881	3.897	0.000	2.161
	(.)	(0.025)	(0.641)	(0.483)	(0.000)	(0.354)
$\Delta\mathbb{E}$[Attendance]/$\Delta\mathbb{E}$[Cost]	.	8.963	8.018	6.182	.	7.639
	(.)	(3.792)	(1.784)	(1.672)	(.)	(1.977)
Panel 2: Black						
Average Aid Offer	0.000	1.000	5.062	5.088	5.088	4.071
	(.)	(.)	(.)	(.)	(.)	(.)
Expected Attendance	0.444	0.497	0.732	0.704	0.704	0.665
	(0.013)	(0.028)	(0.130)	(0.110)	(0.110)	(0.097)
Expected Costs	0.000	0.497	3.507	3.582	3.582	2.681
	(.)	(0.028)	(0.880)	(0.559)	(0.559)	(0.495)
$\Delta\mathbb{E}$[Attendance]/$\Delta\mathbb{E}$[Cost]	.	10.607	8.207	7.244	7.244	8.235
	(.)	(4.473)	(1.658)	(1.936)	(1.936)	(2.103)
Panel 3: Hispanic						
Average Aid Offer	0.000	1.000	5.683	5.088	5.088	4.250
	(.)	(.)	(.)	(.)	(.)	(.)
Expected Attendance	0.411	0.456	0.709	0.638	0.638	0.613
	(0.016)	(0.027)	(0.154)	(0.103)	(0.103)	(0.095)
Expected Costs	0.000	0.456	3.625	3.249	3.249	2.508
	(.)	(0.027)	(1.277)	(0.525)	(0.525)	(0.481)
$\Delta\mathbb{E}$[Attendance]/$\Delta\mathbb{E}$[Cost]	.	9.838	8.219	7.002	7.002	8.040
	(.)	(4.216)	(1.354)	(2.033)	(2.033)	(2.211)
Panel 4: College Ed Parent						
Average Aid Offer	0.000	1.000	2.146	5.088	0.533	0.000
	(.)	(.)	(.)	(.)	(.)	(.)
Expected Attendance	0.686	0.733	0.806	0.880	0.705	0.686
	(0.008)	(0.022)	(0.051)	(0.065)	(0.010)	(0.008)
Expected Costs	0.000	0.733	1.605	4.480	0.472	0.000
	(.)	(0.022)	(0.226)	(0.329)	(0.032)	(0.000)
$\Delta\mathbb{E}$[Attendance]/$\Delta\mathbb{E}$[Cost]	.	6.369	7.500	4.338	4.075	.
	(.)	(2.690)	(2.092)	(1.127)	(1.080)	(.)
Panel 5: First-Gen College						
Average Aid Offer	0.000	1.000	5.471	5.088	1.228	5.088
	(.)	(.)	(.)	(.)	(.)	(.)
Expected Attendance	0.411	0.465	0.720	0.680	0.475	0.680
	(0.008)	(0.026)	(0.141)	(0.115)	(0.029)	(0.115)
Expected Costs	0.000	0.465	3.764	3.460	0.782	3.460
	(.)	(0.026)	(0.971)	(0.584)	(0.145)	(0.584)
$\Delta\mathbb{E}$[Attendance]/$\Delta\mathbb{E}$[Cost]	.	11.597	8.190	7.761	8.178	7.761
	(.)	(4.852)	(1.636)	(2.013)	(2.131)	(2.013)

Notes: Policy implications for selected groups and policies. Offers and costs are per year in year 2000 dollars. Column (1) describes the baseline policy. Column (2) predicts effects of a $1,000 annual financial aid offer. Column (3) predicts effects of the cost-minimizing policy described in Section 6.2. Column (4) predicts effects of a constant financial aid offer intended to reach the same target attendance rate (75%) as the cost-minimizing policy. Column (5) predicts effects of a policy that offers the constant financial aid offer from (4) only to underrepresented minorities. Column (6) predicts effects of a policy that offers the constant financial aid offer from (4) only to first-generation college students. Robust standard errors in parentheses.

larger aid offers naturally lead to higher attendance rates, responsiveness to aid suffers from diminishing returns to scale. The Constant-75% policy is 5.088 times larger than the \$1k policy in terms of average offer, with only 4.673 times the predicted attendance rate increase. Because larger policies increase aid to individuals who would have attended college if given smaller amounts, they also have higher expected costs per dollar offered. Continuing with the illustrative comparison between the \$1k and Constant-75% policies in Table 5, the Constant-75% policy has 6.826 times the expected costs as the \$1k policy. Because increasing aid offers causes decreasing marginal effects on attendance and increasing marginal effects on costs, the ratio is especially strongly affected, with the large policy (Cost-min-75%) inducing a 6.357 percentage point predicted attendance increase per \$1,000, compared to 9.202 for the smaller, otherwise identical policy (\$1k).

Another important characteristic of each candidate policy is its cost-effectiveness, measured here as the ratio of college attendance above baseline to costs. Two factors determine cost-effectiveness. First, allocating aid to individuals who are highly responsive to it increases cost-effectiveness, which in the context of the model involves targeting individuals for whom $\phi(\hat{\tilde{\pi}}_i/\hat{\sigma}_\eta)$ is large. Second, aid being allocated to individuals with low attendance probabilities in the absence of aid increases cost-effectiveness, which in the context of the model involves not targeting individuals for whom $\Phi(\hat{\tilde{\pi}}_i/\hat{\sigma}_\eta)$ is large. As discussed in the preceding paragraph, policies that make larger offers will tend to be less cost-effective, primarily via the second channel. As offers become larger, the probability that any individual would not have attended college with a slightly smaller offer decreases, and the policy wastes money from the standpoint of increasing college attendance. Aside from average offer magnitude, high cost-effectiveness comes from targeting aid at responsive individuals who are unlikely to attend college without the aid.

The cost-minimizing policy considered here (Cost-Min-75%) unsurprisingly has smaller average aid offers than other similar magnitude policies in terms of predicted attendance rates (Constant-75%). Additionally, the cost-minimizing policy has lower offer take-up, with expected costs equal to 69.8% of average offers, compared to 75% take-up for the unconditional offer policy. The reason for the lower take-up for the cost-minimizing policy is that it targets aid at individuals with low probabilities of college attendance conditional on observables in order to avoid allocating aid to individuals who are likely to attend college without it. The lower offers combined with the lower take-up allow the Cost-Min-75% policy to achieve the same 75%

aggregate attendance target at 78.8% of the expected cost of the Constant-75% policy.

Importantly, the cost-minimizing policy is not the only cost-effective policy. The policies targeted at underrepresented minorities and first-generation college students both have higher cost-effectiveness than the Constant-75% policy intended to achieve a 75% aggregate attendance rate. Recalling that these targeted policies offer the same amount of aid to eligible individuals as the Constant-75% policy offers to everyone, it follows that aid offers to these disadvantaged groups are particularly cost-effective. Specifically, the URM policy is 89% (7.174/8.062) as cost-effective as the Cost-Min-75% policy, while the First-Gen policy is 96% (7.761/8.062) as cost-effective as the cost-minimizing policy.

A further important characteristic of each possible policy is its effect on educational inequality. First, as shown in Panel 2 of Table 5, financial aid increases tend to reduce educational inequality, even when it is offered unconditionally. This is because groups with high attendance rates have fewer individuals who can be induced to attend college by receiving aid offers, while groups with low rates have more room to grow. For instance, the Cost-Min-75% policy reduces White-Black attendance inequality by roughly two percentage points, and parent college attendance inequality by roughly seven percentage points. Noteably, this policy actually increases White-Hispanic attendance inequality by about one percentage point. This is because perceived returns to college have higher variance for Hispanics, due largely to their substantially higher variance in material education, which is an important predictor of college attendance. The high variance in perceived returns for Hispanics results in relatively small aid elasticities.

Second, financial aid policies that are targeted at underprivileged groups reduce educational inequality as well. It is straightforward that aid offers targeted at a particular disadvantaged group would reduce inequality for that group, as shown in the effects of the URM policy on White-Black and White-Hispanic attendance inequality, as well as in the effect of the First-Gen policy on parent education attendance inequality. Additionally, policies that target certain types of inequality also tend to reduce other types inequality because of correlations between difference sources of disadvantage. For instance, the policy that is targeted at first-generation college students eliminates almost 70% of the White-Black college attendance gap.

Finally, policies designed to minimize costs are quite effective at reducing inequality. While unconditional financial aid offers reduce inequality because groups with low attendance rates have more room to grow in response to policies, cost-minimizing policies intentionally target aid specifically at these populations. They do this primarily in order to avoid allocating aid to individuals who would likely attend college in the absence of the policy. Because of this, the cost-minimizing policy (policy 3) reduces educational inequality much more than the unconditional policy with the same attendance goal (policy 4). It also avoids the increase in inequality for Hispanics from the unconditional policy that is driven by their high variance in perceived returns. The high variance in Hispanic perceived returns to college is dominated by their low baseline attendance rates in the cost-minimizing aid calculation, wherein the individuals with low predicted perceived returns (e.g. those with uneducated mothers) receive substantially larger aid offers than those with higher predicted perceived returns.

A major takeaway from this policy comparison is that unconditional aid offer policies are dominated by conditional aid offer policies for policy goals of cost-minimization or inequality reduction. A second takeaway is that policies designed to reduce inequality also reduce costs, while policies designed to reduce costs also reduce inequality. Of particular interest, ad hoc inequality reduction policies perform extremely well compared to explicit, relatively complicated, cost-minimization policies. A policymaker interested in increasing college attendance at minimum cost who either lacks access to a wide breadth of predictive variables (such as those used in this paper) or who faces policy-complexity constraints would do quite well by allocating their financial aid budget toward first-generation college students or underrepresented minorities.

7 Conclusion

This paper describes a method for estimating perceived returns to college using choice data and observed cost-shifters. It avoids assuming that agents know their returns to college either individually or on average. Instead, it assumes that agents are aware of a subset of the determinants of their potential pecuniary costs. I use this method to estimate perceived returns to college in the NLSY79. The estimated model produces predictions for responses to financial aid that are consistent with those in the program evaluation literature, for instance that a $1,000 annual subsidy would increase college attendance by 5.1 percentage points. I further validate the model by estimating the effect of the SSSB on perceived returns, finding an effect of $21,312 which is statistically indistinguishable from the average four-year policy offer of $26,800. The estimates are sufficient to form heterogeneous predictions for subgroups of attendance responses to arbitrary changes in financial aid policy.

Past estimates of heterogeneous lifetime income returns to college commonly produce distributions of pecuniary lifetime returns that have much higher mean and variance than the perceived return distribution that I estimate (see Cunha and Heckman (2007) for a survey of papers that estimate heterogeneous lifetime income returns).[20] The qualitative takeaway from the discrepancy between "true" pecuniary returns and perceived returns is that individuals either tend to substantially underestimate their returns to college, or that nonpecuniary "psychic" costs (which are included in perceived returns but not pecuniary returns) are extremely high on average (to explain low average perceived returns) and are especially high for students with high pecuniary returns (to explain the low variance in perceived returns).

Minimizing costs and reducing inequality are common goals of policymakers. The empirical strategy employed in this paper facilitates execution of policies intended to achieve either goal. In the application on college attendance considered here, policies intended to reduce costs are also predicted to reduce inequality, while policies intended to reduce inequality are also predicted to reduce costs. This is not an empirical accident specific to the application chosen. The solution to the cost-minimization problem clarifies the importance of offering price subsidies to individuals who are unlikely to attend college in order to avoid increasing expenses by offering aid to individuals who would attend college without it. Policymakers interested in cost-minimization could certainly design policies intended to do so by estimating a model such as that in this paper, or they could implement ad hoc policies that prioritize financial aid offers for subpopulations with low college attendance rates. While this paper conducts a partial equilibrium analysis in which colleges do not respond to changes in financial aid policies by changing their prices (as investigated by Cellini and Goldin (2014) and others), it is plausible that well-designed cost-minimization policies would produce minimal general equilibrium price responses, as they disproportionately offer financial aid precisely to individuals who would not attend college if subject to price increases.

The model estimated in this paper can be altered to facilitate exploration of other policy goals such as inducing selection on gains (for instance regarding earnings), doing so subject to revenue neutrality concerns, or net revenue maximization. Each of these policies would make use of the parameters estimated in this paper, alongside others (such as those governing heterogeneous returns to college) that could be estimated with additional data. Furthermore,

[20] Estimates of wage returns (rather than lifetime earnings) such as from Card (2001), Carneiro, Heckman, and Vytlacil (2011), Heckman, Humphries, and Veramendi (2018), and many others, are generally consistent with these estimates of lifetime earnings when making standard assumptions about hours worked per year and years worked over the lifecycle.

the method is readily applicable to decisions made under information frictions other than college attendance, such as healthcare expenses, home purchases, or educational decisions other than college attendance, where purchase decisions and known, exogenous cost shifters are observed. As in the present application, estimates of the model in these other settings would facilitate exploration of the likely effects of well-publicized price changes on agent purchase decisions for different subpopulations.

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A Data Appendix

The sample is the entirety of the NLSY79. I do not remove over-sampled disadvantaged groups or members of the military, nor do I make any other restrictions such as considering only white men, as in some related work. I code individuals as college attendees if they report attending a college prior to age 23, or if they report a highest grade attended or completed greater than 12 by age 23. The race variables distinguish between Hispanic, Black, and other, with other combining a variety of different races including White. I define age cohorts by the age each individual is as of August 1979 to approximate school cohorts, based on the intuition that systematic differences in college attendance decisions between adjacent age cohorts are largely driven by labor market and educational circumstances in their year of (potential) high school graduation. I code a variable called "Parents Together" for individuals who live with both biological parents every year prior to age 18. The AFQT variable is the residual from a regression of AFQT on years of schooling, age at time of test taking, and a current school enrollment indicator. Mothers Education Years and Number of Siblings are simple counts, and are included as linear controls. State unemployment is obtained from the BLS, with permanent state employment calculated as the average unemployment rate from 1976 to 2000. County Wages are calculated as total yearly income by county divided by population from data from the Bureau of Economic Analysis, deflated to year 2000 dollars. Permanent county wage is the same variable averaged from years 1976–2000. Urban residence is an indicator for residing in an urban area at age 14.

The variables relating to local college are constructed using data from The Integrated Postsecondary Education Data System (IPEDS) when available (1980 and after 1984) and data from the Higher Education General Information Survey (HEGIS) otherwise, which was the precursor to IPEDS. Individuals are coded as having a nearby college at age 14 if a public four-year college is present in the same county in the year they were 14. Local tuition is obtained for individuals in a given county in a given year by taking the enrollment-weighted average tuition of all colleges in the county of residence at age 17, for individuals who live near a four-year college, and in the state of residence at age 17 for individuals who do not. Some colleges do not report tuition in all years, though this is more common for small private colleges than for public four-year colleges. For such school years, I impute likely tuition by multiplying average tuition for all schools in the year by a constant that best explains the school's tuition in years in which it is observed.

To construct actual tuition costs for individuals who attended college, I make use of retrospective interviews from the NLSY79 Geocode file regarding details about all colleges previously attended as well as public NLSY79 information on the prior year. Specifically, for each prior year in the public-use data in which college attendance is reported, I define the specific college attended as the one reported in the retrospective data as being attended in the nearest year. In some cases, individuals fail to give retrospective information about start and end dates of college attendance, for example due to memory lapses or ongoing enrollment at the time of the interview. In such cases where no start date is reported, I impute college attendance intervals as starting in the year after the most recent reported attendance at the same college. In cases where no end date is reported, I impute attendance intervals as ending in the year of survey. This procedure sometimes results in a large range of possible years of attendance for colleges, as it tends to impute long continuous spells if an individual fails to recall precise start or end dates for a college. However, the only years of attendance actually determined for a given college are those in which an individual reports college attendance in the past-year interview. For each such year, net tuition is calculated as the sticker price for the college attended minus financial aid benefits reported by the respondent in that same year. In cases where multiple colleges are potentially attended in a given year, I take the average values of tuition and financial aid for these colleges. I calculate four-year tuition as the average of net tuition in college attendance years multiplied by four.

I restrict the sample to individuals who have no missing values for all of the above variables, with one exception: tuition for individuals who do not attend college. For these individuals, I deal with missing tuition via a selection model. 764 individuals are dropped due to missing AFQT scores, which are in the data for 1981 (two years after initial interview). 1,482 of the remainder have unknown status for the Parents Together variable, which was gathered in 1988. 580 of the remainder have missing maternal education years. 11 of the remainder have missing sibling counts. 1,654 of the remainder are missing unemployment rates in their region of residence at age 17. The BLS data only contains unemployment data for 1976 and onward, so individuals who are 21 or 22 in 1979 have missing local unemployment data at age 17. 119 individuals of the remainder are dropped due to missing permanent or temporary average county wages. Finally, 24 of the remaining individuals have missing data on urban residence at age 14. Overall, 5,596 individuals are dropped due to missing data.

B Perceived Returns to College Completion

I present results on perceived returns to college completion, which I define as graduation from college at age 28 or earlier. I code individuals who attend college for any length of time, such as four years, but who do not report degree receipt as non-completers. I calculate tuition costs associated with college completion as the net tuition over all years of college attendance prior to degree receipt, as described in Appendix A. All other variables are coded as described for estimating perceived returns to college attendance.

The model estimates of perceived returns to college are in Table B.1. The standard errors on all perceived returns parameters in column 3 are substantially larger than point estimates. The key driver of this is the statistically insignificant effect of local tuition on college completion, as displayed in column 1. The uncertainty in the effect of local tuition at age 17 on completion propagates through the rest of the model, as this effect is rescaled by the effect of local tuition on actual tuition (column 2) to identify the scale parameter of perceived returns. The uncertainty on the relationship between local tuition at age 17 and college completion produces model estimates that do not make sharp predictions regarding the effects of alternative financial aid policies on college completion, though this challenge could potentially be surmounted with more data.

Table B.1: Perceived Returns Estimates

	Tuition Observation (1)		Tuition Imputation (2)		Perceived Returns (3)	
Nearby College at Age 14	0.091	(0.055)			45.528	(247.200)
Local Tuition at Age 17	-0.005	(0.051)	4.803	(1.300)		
Female	0.073	(0.046)	3.133	(0.989)	39.919	(196.783)
Black	0.181	(0.069)	-9.389	(1.835)	79.697	(475.956)
Hispanic	0.130	(0.093)	-2.025	(2.052)	61.091	(345.629)
AFQT	0.388	(0.029)	2.158	(0.957)	192.654	(1012.022)
Parents Together	0.441	(0.055)	7.586	(1.552)	225.863	(1157.688)
Mother Education Years	0.173	(0.013)	1.775	(0.318)	87.014	(453.687)
Number of Siblings	-0.065	(0.013)	-0.713	(0.341)	-32.569	(169.843)
Permanent Unemployment at Age 17	0.010	(0.035)	0.935	(0.684)	6.165	(36.506)
Permanent County Wage at Age 17	0.056	(0.014)	1.648	(0.284)	27.962	(139.842)
Average County Wage at Age 17	-0.073	(0.017)	-1.460	(0.354)	-36.360	(185.830)
State Unemployment at Age 17	-0.007	(0.022)	-0.446	(0.461)	-3.473	(22.627)
Urban Residence at Age 14	0.074	(0.062)	-0.026	(1.404)	35.933	(193.859)
Age 15 in 1979	0.000	(0.153)	0.497	(3.792)	2.900	(75.833)
Age 16 in 1979	0.034	(0.158)	1.500	(3.902)	24.913	(145.288)
Age 17 in 1979	-0.149	(0.180)	-3.611	(4.389)	-68.657	(363.965)
Age 18 in 1979	-0.116	(0.187)	-0.071	(4.369)	-47.806	(273.536)
Age 19 in 1979	-0.213	(0.184)	-5.881	(4.660)	-102.371	(520.528)
Age 20 in 1979	-0.256	(0.184)	-5.221	(4.644)	-119.055	(611.334)
Constant	-3.018	(0.296)	-46.969	(7.694)	-1542.028	(7983.753)
Additional Parameters						
atanh(ρ)	0.385	(0.140)				
ln(σ_u)	2.840	(0.049)				
σ_η	495.981	(2636.532)				
F-stat for Local Tuition in (2)	184.888					
Log Likelihood	-4.377e+09					
Sample Size	7,085					

Notes: Estimates are from maximizing the likelihood in (27) with 1988 sample weights, where columns (1) and (2) are equivalent to maximum likelihood estimates of a Heckman (1979) model of the effect of local tuition on (selectively observed) tuition, and equations (2) and (3) are equivalent to estimates of an instrumental variables probit for the effect of tuition on perceived returns, using local tuition as an instrument and restricting the effect of tuition on perceived returns to -1. Parameters in column (3) are estimated marginal effects of each variable on perceived returns to college in thousands of dollars. Robust standard errors in parentheses.