Educational Debt Burden and Career Choice:

Evidence from a Financial Aid Experiment at NYU Law School

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Abstract: This paper explores how the timing of career-contingent financial aid influences its effectiveness in encouraging law students to enter public interest work, and hence the isolated effect of educational debt timing on career choice. I analyze quasi-experimental data from NYU Law School's *Innovative Financial Aid Study*, in which career-contingent financial aid packages with equivalent net values but varying debt structures were randomly assigned to applicants. My results indicate that debt timing matters: law school graduates who receive tuition waivers rather than ex-post loan assistance have a 37% higher rate of first job placement in public interest law and a 94% higher rate of clerkships. Furthermore, recipients of tuition waivers are more likely to enroll in law school conditional on being admitted. Using propensity score methods to correct for sample selection bias at the matriculation stage, I find that differences in first job placement according to debt timing persist after controlling for differential enrollment rates, implying an independent post-enrollment influence of debt timing on career decisions. I present a behavioral model that accounts for both of these stylized facts, in which the time-inconsistent influence of debt on career decisions is rationalized as a commitment device when agents are both debt averse and loss averse.

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1 Introduction

As public and private sector wages continue to diverge and educational debt levels rise, a growing number of professional schools are offering career-contingent financial aid packages aimed at increasing the incentive for graduates to pursue low-paying public interest work. The majority of career-contingent aid is awarded in the form of loan repayment assistance, although a few schools also offer tuition subsidies to entering students interested in non-profit careers. Despite the fact that career-contingent aid programs by and large favor retrospective debt relief over ex-ante tuition waivers, the importance of the timing of aid – and hence, the timing of educational debt – on career choice has never been evaluated. According to standard economic models, if the net present values are equal, current and future debt should have identical influence on career choices. However, as will be shown in this paper, if there is debt aversion, or disutility associated with debt beyond borrowing costs, the payout schedule of educational loans could influence career decisions.[1] To address this issue I compare the impact of career-contingent tuition assistance and loan repayment assistance on the job sector placement of law school graduates utilizing data from a randomized allocation of financial aid packages conducted over four years at New York University's School of Law. Comparing students' responses to financial aid packages of comparable monetary value but distinct payout schedules provides a unique opportunity to isolate the non-financial cost of debt with a high-stakes experiment and thereby study the degree to which psychological debt burden influences career choices.

Given the current social interest in encouraging public interest employment and the growing amount of funding allocated for this purpose, not only is this a relevant behavioral question, but one with important policy implications. In particular, is loan repayment the most efficient manner for a school interested in influencing career outcomes to allocate funds? Depending on the nature of debt aversion, tuition subsidies may be more effective in encouraging students to take low-paying jobs with high social value than are loan repayment programs which fail to alleviate debt burden. This policy implication is potentially relevant in many educational settings in which career-contingent financial aid is designed to steer people

[1] There is substantial empirical evidence of debt aversion in many settings. For instance, payoff rates of mortgages and student loans are "irrationally" rapid. See Loewenstein and Thaler (1989) and Thaler (1992) for a discussion.

towards public interest work.[2] For instance, British public universities are considering a universal program of income-contingent educational loans (Barr, et. al. , 1998).

The first section of the paper describes the NYU financial aid experiment in detail. I then present experimental results which indicate that graduates indeed respond differently to up-front tuition subsidies and retrospective debt relief programs when making career choices. In particular, law school graduates who receive tuition waivers while in school have a significantly higher rate of first job placement in public interest law as well as a substantially higher rate of clerkships. Furthermore, debt timing appears to influence the matriculation decisions of law school applicants, such that recipients of tuition waivers are more likely to enroll conditional on being admitted.

The second half of the paper disentangles the role of anticipated educational debt on enrollment decisions from the effect of debt while in school on first job choice using propensity score methods to correct for sample selection bias at the matriculation stage. Ultimately, differences in public sector placement according to debt timing persist after controlling for differential matriculation rates according to observable characteristics, suggesting an independent post-enrollment influence of debt aversion on career decisions. The effect of financial aid package on clerkships, however, appears to be largely explained by high-ability applicants selecting into the pool of tuition subsidy recipients during enrollment.

2 Project Background

2.1 Career-contingent Financial Aid

At the country's premier law schools, students are graduating with average educational debt between $70,000 and $80,000, and the figure is rising. The primary source of growing indebtedness is the rapid rise in law school tuition: As Table 1 illustrates, between 1987 and 1997, law school tuition at both private and public law schools nearly doubled (EJW et al., 2002).[3]

[2] See Appendix E-F for a survey of career-contingent financial aid programs available in 2000.
[3] Kornhauser and Revesz (1995) find similar patterns of tuition growth between 1980 and 1990.

At the same time, wages in private sector and public interest jobs have steadily diverged.[4] Table 2 reports the difference in private and public sector average starting salaries for graduates from New York University's School of Law in the classes of 1998-2001.[5] In light of the relative stagnace of public interest salaries, there is growing concern that educational debts of the current magnitude dissuade even the most dedicated lawyer from taking a public interest job. Indeed, a recent survey of 1,622 law school graduates found that 66% did not consider a public interest or government job on account of law school debt (EJW et al., 2002). In response, many law schools have initiated career-contingent financial aid policies designed to increase incentives for public interest work by reducing educational debt burden. At present, loan repayment assistance programs (LRAP), which are largely funded and administered by law schools, are by far the most common form of career-contingent financial aid.[6] Loan repayment assistance defrays or, in some instances, fully covers the educational debt payments of graduates once they enter qualifying public service jobs. While in 1986 there were only five law school LRAPs nationwide, today there are 47 law school and four state LRAPs.[7]

In contrast, career-contingent public service scholarships (PSS) are far less common. PSS are broadly defined as tuition grants offered to entering law students who express interest in public service careers with conditional repayment clauses that apply if students take private sector jobs after graduation. While LRAP is generally available to anyone who pursues qualifying work, PSS are almost universally offered as merit-based awards to a select few. Of the 13 PSS programs nationwide, twelve offer under ten scholarships per year and all are highly competitive (NAPIL, 2002). Through the new aid packages offered under the Innovative Financial Aid Study, NYU Law School was the first law school to offer both types of aid to interested students regardless of merit or need.

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[4] Here, as throughout the paper, "public interest law" includes all government law jobs as well as non-governmental non-profit law.
[5] The growth in the starting salary gap is equally large at the national level. The disparity is even greater taking into account the steeper wage profiles of private sector law jobs over the ten-year payback period (EJW et al., 2002).
[6] There are also a handful of LRAP programs sponsored by state governments and employers (see Appendix E).
[7] LRAP programs vary greatly in the amount of debt assistance offered and the eligibility requirements. See NAPIL (2002) for a comprehensive description of all programs.

2.2 The New York University Innovative Financial Aid Study

NYU Law School's Mel and Barbara Weiss Loan Repayment Assistance Program (LRAP) was among the first LRAPs in the country. A 1993 enhancement of funding made it also one of the most generous loan assistance programs anywhere. At NYU, for all graduates who choose careers in the public sector or other low paying fields of law, the majority of educational loans incurred during law school are forgiven through LRAP.[8] As Table 3 illustrates, it is currently the second largest loan repayment program in the country in terms of total funds dispersed.

Additionally, in 1997, NYU Law School announced a $10 million research initiative, the Innovative Financial Aid Study (IFAS), which further expanded the amount of available career-contingent aid. The IFAS was deemed innovative for two primary reasons. First, as mentioned in Section 2.1, in addition to LRAP, the program introduced two forms of career-contingent tuition subsidies to students in the NYU Law classes of 1998, 1999, 2000, and 2001 interested in public service careers. The NYU public service scholarship (PSS) provided a grant of two-thirds tuition that converted to a loan in the event that a recipient did not pursue a career in public interest law.[9] In addition, under IFAS, NYU expanded its Root-Tilden-Kern (RTK) Scholarship program, which also provided two-thirds tuition as well as an array of public service seminars, discussion groups, and other activities at the law school to a select group of merit-eligible applicants.[10]

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[8] LRAP provides quarterly prospective funding to alumni for up to ten years following graduation, providing all other conditions are met. Full time employees who work 35 hours or more each week, and who work in a position that “involves law” as determined by NYU, are eligible for the program. For the class of 2004 the program defines "low-paying" as income of less than $57,651annually. Qualifying income is gross income minus adjustments made for annual debt service on educational debts, dependents, medical expenses, other LRAP benefits, and spouse's income.

[9] Specifically, a legally binding contract stipulated that any PSS recipient that takes a non qualifying job during the first ten years of his or her career has to repay a prorated fraction of the scholarship according to a repayment schedule matching federal loan terms. Graduates that leave public interest work prior to the required ten years must pay back the amorticized portion of their tuition scholarship corresponding to the portion of time spent in the private sector.

[10] As opposed to debt conversion specified in a legally binding contract, RTK graduates have a “moral obligation” to repay their grants through charitable donations to the school if they earn a salary greater than the prevailing public interest salary during the first ten years after graduation. In this sense, RTK scholarships differ most markedly from the PSS in that they involve no legally binding obligation on the part of the recipient to repay the loan in the event that they leave or do not enter the public sector. Almost all RTK activities are open to the public.

The key innovation of the IFAS, however, was the randomized allocation of all PSS and a subset of RTK grants. Each year of the study, PSS were randomly assigned by lottery across the entire pool of (admitted) applicants and RTK grants assigned by lottery to all merit-eligible applicants.[11] All lottery winners received scholarships of two-thirds tuition for three years of law school, while lottery losers received no tuition subsidy but were eligible to apply for two- and one-year PSS scholarships during their second and third years of law school. More importantly, all participants including lottery losers who entered public interest work were also eligible for LRAP for any portion of tuition loans not covered by subsidy. In total, 158 lottery winners were selected from the pool of 320 applicants, consisting in 102 three-year (PSS0 and RTK), 43 two-year (PSS1), and 13 one-year (PSS2) scholarships.[12]

As part of the IFAS study, data were collected on all members of the four participating law school classes from six separate university sources: law school applications; financial aid applications; law school academic records from the registrar's office; first-year entry surveys on work experience, personal debt, career goals and job preferences; third-year exit surveys with data identical to the entry survey but also including school and summer activity information; and work experience surveys mailed biennially to alumni for ten years following graduation.

3 Conceptual Framework: LRAP versus PSS

The key characteristic of the IFAS lotteries and the most important feature to note in comparing the two loan options is the fact that the two packages were designed to be equivalent in net present value. To illustrate, the expense sheet in Figure 1, available from the law school's Office of Financial Aid, gives an idea of the relative debt burden faced by PSS recipients versus students eligible for LRAP only. While the first row of Figure 1 reveals a significant difference in annual student debt, due to the availability of loan forgiveness through LRAP and the career-contingent nature of PSS, *there is no difference in the monetary values of the two financial aid packages*, conditional on the availability of federal loans free of interest during school. Indeed,

[11] Only applicants meeting a merit-based criteria entered the RTK lottery. In addition, roughly 15% of applicants in the highest merit category were automatically awarded RTK scholarships and excluded from my experimental analysis.

[12] Students who quit or failed to graduate in three years are excluded from these figures and the proceeding analysis.

all applicants in the study received interest-free loans covering tuition through the school financial aid office. Because PSS job eligibility requirements are identical to those of LRAP, and because PSS recipients are also eligible for LRAP for the portion of expenses financed by loans, the PSS is essentially loan forgiveness in reverse. To illustrate, assuming an annual tuition expense of $30,000, the cost of tuition and therefore the monetary value to law school entrants of the two financial aid programs is presented in Figure 2.

In equations (1a) and (1b) of Figure 2, because two-thirds is covered by the PSS, lottery winners are only responsible for $30,000 tuition while in school, which is repaid after they graduate if they take an LRAP-qualifying job. If they take a private sector job, they must repay the $60,000 loan after graduation. Equations (2a) and (2b) illustrate that lottery losers are responsible for all $90,000 tuition in period 1, all of which is repaid in period 2 if they work in public interest law. As expressions (a) and (b) are equivalent for lottery winners and losers, economic theory predicts a Von Neumann-Morgenstern utility-maximizing individual to be indifferent between winning and losing the financial aid lottery. Hence, because both packages offer the same reward for public sector work, an individual should respond identically to the two forms of aid when choosing whether to take a public interest law job. Nonetheless, two distinct aspects of debt timing have the potential to generate differences in students' valuations of the programs and corresponding differences in the likelihood of choosing public interest work in response to lottery outcome.

3.1 Risk value of debt

If students perceive earlier debt to be costlier, they may require more financial compensation under an LRAP program to enter low-paying public interest work and therefore be less likely to choose a public sector job than are PSS recipients. The only difference in the real financial value of the two aid packages is the potential risk associated with the non-binding nature of the LRAP agreement. Unlike under a contractual agreement such as the PSS entails, neither the existence of the LRAP program, nor the formula used by the program in a given year is guaranteed to remain constant by the time law school applicants enter the job market. Thus, any uncertainty regarding continuation of the program, change in benefit amount, change in eligibility requirements, or change in tax treatment of loan payments could cause risk-averse

students with debt to refocus their career towards the financially secure private sector. In spite of this potential uncertainty, given that NYU's LRAP is one of the oldest and most established loan repayment programs in the country and NYU Law School promotes itself as a school committed to public interest law, program discontinuation or cutbacks should be evaluated as highly unlikely by incoming students.[13]

Even if changes in the program are deemed unlikely, there are other potential financial costs of holding debt for three years. For instance, debt could limit access to credit for students considering large non-educational loan needs such as purchasing a house which may arise during law school.[14] Finally, though it is reasonable to assume that law school entrants have substantial access to both private and public loans free of interest while in school, it is possible that applicants perceive themselves to be credit constrained. For example, since applicants may not receive federal loan application results as quickly as admissions decisions, anticipation of credit constraints could also influence applicants' preferences over the two types of aid.

3.2 Psychological debt aversion

In addition to financial considerations, behavioral responses to debt could play a role in influencing graduates' career choices. While the expected post-graduate debt payments of PSS recipients versus LRAP qualifiers are equal conditional on the probability of public interest employment, students' debt levels while in school and upon graduating differ substantially: At the end of law school, the balance sheets of non-PSS-holders register up to three years of actual debt to the federal government, while PSS holders face only the *risk of future debt* to the university. As a consequence, one possible reason that students' employment responses to LRAP and PSS could differ is that individuals are not standard expected-utility-maximizers in the sense that they evaluate going into debt and debt forgiveness asymmetrically. This possible behavioral explanation is a variation of the loss aversion model of Kahnammen and Tversky, in which

[13] In fact, according to program coordinators, the school's commitment to providing loan assistance was particularly emphasized to students in IFAS classes.

[14] However, according to exit survey information on debt, none of the students in the four study classes purchases a house during law school.

individuals associate higher disutility with a loss than the utility associated with an equivalent gain.[15]

Figure 3 illustrates a behavioral model based on this possibility in which an individual making career decisions is loss averse and her reference point is always her current level of debt. The first characteristic of agents in this model is that they are debt averse. As long as going into debt is interpreted as a loss, a loss aversion framework can be applied to educational debt. While in the one-stage problem of Part A, which ignores debt experienced in the first stage, LRAP and PSS are evaluated identically, the existence of debt aversion requires modeling the financial aid lotteries as a two-stage decision problem as in Part B. In a dynamic decision model, initial events or choices of the individual influence her reference points in the future and therefore her choices in the future. Also, as a dynamic decision problem, the possibility of loss aversion has important implications for individuals' response to each type of financial aid since loss aversion has the potential to generate a preference reversal in the second stage under one financial aid package only. Thus, if debt averse individuals are also characterized by loss averse preferences, they may choose distinct careers depending on the timing of debt.

This potential dynamic inconsistency can be seen by considering the job choices of a loss averse individual at Stage 2 of the decision trees in part (b). Even if in (a) an individual would choose to go into public interest work to avoid the $90,000 loss, in the two-stage problem of (b), loss averse individuals may choose private sector over public interest work under LRAP only. Since, at the point when graduates are faced with the decision of which job sector to enter they face a penalty in the case of PSS only, loss averse individuals will be more likely to enter the public sector under PSS than under LRAP. In other words, they choose $0 and a private sector job over $90,000 and a public interest job but not (-$90,000) and private sector work over $0 and public interest work. Whereas under LRAP, the loss associated with taking a private sector job has already been suffered so is not taken into account in the second stage, the PSS aid package avoids time-inconsistent career decisions by postponing the penalty aspect of career-contingent financial aid.

[15] Kahneman and Tversky (1984) define loss aversion as " the disutility of giving up an object being greater than the utility associated with acquiring it."

In turn, ex-ante knowledge of this behavioral effect will cause scholarship applicants to assign a higher probability to entering the public sector conditional on winning the PSS. As a result, students interested in committing themselves to public interest work will favor postponing debt in order to discourage themselves from entering the financially tempting private sector. In this sense, tuition subsidies may serve as a commitment device to a debt averse student cognizant of his nature. This idea has intuitive appeal in explaining why some people considering professional schools express the seemingly irrational fear of getting "sucked into the private sector."

For the reasons outlined above, both loss averse and risk-related debt aversion have the potential to influence the career-related decision problems of financial aid recipients, generating a difference in public interest placement rates according to financial aid timing. Though not a separate causal factor, if the financial aid packages are *perceived* to be different on account of either factor, the expected matriculation rates among admitted applicants will differ according to debt timing. In this manner, a difference in job outcomes related to financial aid package will generally be associated with a corresponding difference in enrollment propensities.

4 Construction of Control Group

Participants in the experimental component of the IFAS included a total of 102 3-year, 43 2-year and 13 1-year scholarships assigned by lottery to the pool of 320 matriculating applicants from the classes of 1998, 1999, 2000 and 2001. Appendix A presents the distribution of applicant winners and losers. Constructing unbiased experimental groups was complicated by the fact that losers could reapply for a PSS scholarship in their second (PSS1) and third (PSS2) years of law school. To address this complication, two steps were taken in assigning lottery participants to control and treatment groups. First, only an individual's lottery outcome the *first* time he applies for a PSS was taken into account. The treatment group then consists of all first-time applicant winners. The analogous control group comprises all those not awarded a scholarship the first time they apply. However, due to the possibility of reapplying, this group includes 46 lottery losers that receive scholarships at a later stage, which contaminates identification of the

treatment effect.[16] To eliminate this bias, those 46 original losers who eventually win the lottery were excluded from the control group, generating a "net control group" that includes only the lottery applicants that never won a scholarship.

However, eliminating eventual winners from the initial pool of losers introduces a potentially strong bias due to the fact that re-applicants are dropped from the control group only. If multiple applicants have different characteristics than one-time applicants, dropping a large number of these types from the control but not the treatment group will alter the equal distribution of characteristics across experimental groups achieved by random assignment. If the propensity to reapply is correlated with any individual characteristics influencing career choice (such as level of interest in public service), mean differences in job outcomes between control and treatment groups will be biased measures of program impact on career outcomes.

To eliminate this bias, sample weights were constructed to account for missing observations. Specifically, applicants who applied multiple times and repeatedly lost the lottery were over-weighted to reflect the total number of re-applicants including those who won and were dropped. Essentially, it is assumed that every applicant has an individual "type" – one-time, two-time or three-time propensity to apply. While types are unobservable among lottery winners, random assignment in second- and third-year lotteries ensures that winning re-applicants are characterized by the same type distribution as repeat losers conditional on the number of applications. In this manner, losers' reapplication rates can be used to determine the correct distribution of types among the discarded winners.[17] The weighting formulas are described in detail in the notes to Appendix A.

Second, the sample weights had to be adjusted to account for differences in the probabilities of winning according to the type of lottery to which the student first applied – PSS0, both PSS0 and RTK, PSS1 or PSS2. Because of the smaller number of PSS1 and PSS2

[16] In this case, it would be impossible to separate the effects of losing the PSS in an early year from winning at a later point, so that estimates of scholarship effect would be biased downwards This is analogous to the standard problem of control group members seeking outside treatment. See Robins (1998) for a discussion.
[17] Hence, it was assumed that, since two-thirds of losers reapplied once and one-fourth reapplied twice, so would have the same fractions of winners.

first-time applicants and the smaller pool of eligible RTK applicants, these participants had a higher probability of ending up in the treatment group, and therefore the treatment group is composed of a higher percentage of PSS1, PSS2 and RTK applicants relative to PSS0 applicants. Thus, sample weights of control subjects in the last three categories were adjusted to equate the distribution of lottery types across treatment and control groups. Table 4 gives the precise sampling weights for the five types of lottery.

Finally, in constructing the comparison groups to be used in the analysis, an important concern over non-random assignment arises from the fact that many applicants for first-year scholarships did not attend NYU.[18] An intent-to-treat analysis, in which all applicants are included in the study regardless of participation, would yield unbiased comparison groups. However, in practice, since data was only collected for NYU attendees it was necessary to exclude from the analysis all lottery applicants that failed to matriculate.[19] If matriculation rates are correlated with lottery outcome as well as other individual characteristics, the enrolled lottery winners and losers will not reflect a random assignment of individuals to experimental groups. For instance, if very dedicated students' acceptance decisions depend heavily on scholarship money, then matriculating lottery winners will be on average more dedicated to public interest work. On the other hand, if the correlation between matriculation rates and lottery outcome is uniformly distributed across applicant types, the random assignment assumption remains valid. Since the only difference between lottery winners and losers in the value of law school is that control group members face (interest-free) tuition debt while in school as opposed to after graduation, sample selection would only occur if applicants are characterized by time-inconsistent debt aversion. In that case, we would expect a higher matriculation rate among first year lottery winners, producing a higher average level of debt aversion within the treatment group. To the extent that debt aversion is either itself a determinant of career choice or correlated with other individual characteristics that influence job outcomes, the estimate of program impact will be biased.

[18] Unfortunately, there is currently no data on enrollment according to lottery outcome available from the NYU Law School admissions office. However, anecdotal evidence from one IFAS administrator reveals surprise at "how few members of the control group actually enrolled" (Kornhauser, 2001).

[19] For a discussion of non-random non-compliance with missing data, see Tsitsi et al. (1999).

While complicating the analysis of job sector outcomes, matriculation patterns according to lottery outcome constitute an important program outcome in themselves. The following section on experimental results begins by exploring the effect of lottery outcome on enrollment decisions.

5 Experimental Outcomes

5.1 Matriculation Rates

Table 5 presents summary statistics of the experimental data, providing a rough check of random assignment among matriculating applicants.[20] Any statistically significant differences in mean characteristics among matriculating winners and losers can be assumed for incentive reasons to imply a greater propensity to enroll among lottery winners. The statistically significant difference between mean *Law School Admissions Test* (LSAT) scores, with lottery winners averaging 1.2 points higher than lottery losers, suggests that applicants in fact have differential enrollment rates according to lottery outcome. In addition, the $90,000 difference in mean family worth is large and statistically significant at the 10% level. Thus, it appears that postponing debt encourages applicants to enroll in law school. Since lottery losers should be aware that career-contingent financial assistance with the same option value as the tuition waiver is available to them at NYU regardless of lottery outcome, a higher matriculation rate among lottery winners provides important evidence of time-inconsistent debt aversion.

Because sample selection is presumed to occur only among first-year scholarship applicants who are deciding whether and where to attend law school, I continue by comparing the following pre-law-school characteristics among the sub-sample of first-year lottery applicants only: sex, race, LSAT scores, undergraduate GPA, rank of undergraduate school, parental net worth, net parental income, undergraduate debt, and other outstanding debt. As revealed in Table 6, among the first-year matriculating applicants the same pattern of mean differences in pre-law

[20] Here, as in Tables 8-9, partial weights are applied only to equate the application lotteries in the treatment and control groups and no control group members are excluded as in the final weighting scheme. Comparison of partially weighted and fully weighted sample means of the treatment and control groups verifies that the final weighting scheme, in which 45 eventual winners are dropped and the control group re-weighted, does not alter the distribution of demographic traits.

school characteristics appears, with LSAT score being the only difference that is close to significant.

However, further dissimilarity among first-year participants suggestive of type differences according to lottery outcomes is evident from a comparison of correlations between pre-lottery observables, presented in Table 7. From these data, it appears that sample selection at the enrollment stage is a result of tuition waivers leading higher quality students to matriculate at NYU. This is evident by the fact that not only are LSAT scores lower in the control group, but both LSAT and undergraduate GPA, and LSAT and college rank have reverse correlations across experimental groups. In the control group, as in the population of non-participants, these two performance measures are inversely related to LSAT scores, reflecting the fact that they are substitute criteria in admissions decisions. However, apparently after lottery results encourage a group of high LSAT applicants to matriculate, both negative relationships disappear in the treatment group.[21] Thus, it appears that not only are high LSAT students more likely to attend a different school if they are not offered tuition waivers, but among them, the ones with high undergraduate school ranks and GPAs are the most likely to do so. This is consistent with a story in which students with first-tier outside options are wiling to trade off some amount of school reputation for the absence of tuition debt during school.

The effect of this matriculation pattern on career paths, explored in the following section, will depend on the relative propensities of these two types to enter public interest law. However, an immediate policy implication of the enrollment findings is that schools interested in attracting higher ability students will benefit from offering tuition subsidies in addition to loan repayment.

The empirical analysis of job sector outcomes proceeds as follows. The first round of estimates explores mean differences in final career outcomes between the control and treatment groups. It is important to keep in mind that, on account of possible sample selection at the matriculation stage, mean differences between experimental subjects capture both the effect of

[21] In a regression of treatment on GPA, LSAT and their interaction, the interaction term is statistically significant at the 1% level, while in a regression of treatment on rank, LSAT and their interaction, the interaction term is statistically significant at the 10% level. Meanwhile, in analogous regression estimates for all remaining covariates the interaction term is insignificant.

participation *and* the effect of winning the lottery on the decision to enroll.[22] In this sense, while unsuccessful applicants may not provide an ideal control group for measuring the effect of educational debt during school on post-graduate career choice, comparing the matriculating treatment and control group members *does* provide an unbiased estimate of the "total program effect" – that is, the effect of debt burden on career decisions together with the effect of prospective debt burden on the decision to enroll. From a policy perspective, both channels of impact of offering up-front tuition waivers are of interest.[23]

In the second round of estimates, I attempt to correct for sample selectivity in order to estimate the average treatment effect of tuition waivers. In particular, I adjust for observable confounding variables arising from potentially endogenous matriculation rates by comparing control group members to a matched sub-sample of treatment group members with propensity score matching techniques. The resulting matched outcome, along with a descriptive investigation of control and treatment differences, provides inference on the size of the selection effect.[24]

5.2 Mean Differences in Job Placement

In comparing the impact of the two forms of loan assistance, the fundamental outcome of interest is the likelihood of pursuing a career in public interest law. To approximate long-range career paths, I look at both the first job placement of graduates as well as subjective statements of career plans from the exit survey. Unfortunately, the first job placement measure is complicated by the fact that graduates also have a third option of accepting an intermediate position as a law clerk. Mindful of this shortcoming, I begin by looking at the allocation of

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[22] Furthermore, since some degree of "slippage" occurs among treatment members – i.e. some lottery winners fail to renew their three-year tuition waivers in years two or three of law school – the treatment effect accurately measures the impact of the *availability* of tuition waivers rather than the effect of take-up. Regardless of slippage, all treatment group members received tuition waivers for at least one of the three years.

[23] Aside from using the matriculating lottery losers as a control group, a different option would have been to select a matched sample from the pool of non-applicants, as in Rouse's (1998) quasi-experimental evaluation of the Milwaukee School Choice Program. In my case, this approach is inappropriate because lottery participation is a decision variable undeniably highly correlated with job outcomes. Non-applicants comparable to applicants but who did not apply for exogenous reasons would be difficult to convincingly identify in the data.

[24] With the exception of the propensity score estimates, the sampling weights described in the previous section are applied throughout the statistical analysis.

experimental subjects across all three sectors.[25] As shown in Table 8, there is a significant difference between the experimental groups in the distribution of first job placements.

In pair-wise comparisons, treatment group subjects are 16.1 percentage points (39%) less likely to take a non-qualifying job and 19.7 percentage points (94%) more likely to take a one-to-two year clerkship after leaving law school. The difference across treatment and control groups in terms of the likelihood of directly entering the public sector is small and insignificant. Nonetheless, these results indicate that, despite the equivalent net present value of these two programs, career-contingent tuition subsidies are associated with a lower rate at which law students with a self-reported interest in public sector work abandon this pursuit immediately after law school.

The relationship between financial aid timing and the primary outcome of interest, the long-term (post-clerkship) proportion of public interest lawyers, depends on the rate at which clerks enter public interest work. Information on the pattern of post-clerkship employment is currently available for the classes of 1998, 1999 and 2000 from follow-up surveys mailed to graduates two years out of school. Overall, 58% of the 70 clerks in these classes transition to public interest jobs and two are in second clerkships at the time of the follow-up survey.[26] While the sample is small, the rate of post-clerkship public interest employment differs substantially by lottery outcome: 70% of lottery winners and only 41% of lottery losers take public interest jobs. Table 9 incorporates these data and reports the updated job sector distribution for the classes of 1998-2000 two years after graduation.[27] Here, the same patterns as in Table 8 are observed. Members of the treatment group are over one-third (37%) more likely to enter public interest law after two years.

Classifying the two students in second clerkships as public sector lawyers yields a 39% placement differential across experimental subjects in the classes of 1998-2000, shown in Table

[25] The ten experimental subjects that fail to report post-graduate employment are assumed for incentive reasons to not be employed in qualifying public interest jobs.
[26] Five observations without follow-up data are again assumed to not be working in qualifying public interest jobs.
[27] Analogous weights are constructed for the participants in the classes of 1998-2000 only, described in Appendix B.

10a.[28] Regression controlled means accounting for year of graduation, lottery type or demographic characteristics consistently produce an even larger treatment effect, ranging from 19-20 percentage points.

Allowing the program effect to differ according to whether or not treated individuals participate in the RTK program reveals that, while RTK applicants have a higher average rate of public interest employment, the estimated program effect of winning the lottery is roughly equivalent for both types of treatment. This is somewhat surprising given that RTK involves only a "moral obligation" to repay in the event of private sector employment.

Unfortunately, the small number of PSS1 and PSS2 applicants does not permit a dose response analysis of tuition subsidies. However, looking separately at PSS0 and RTK applicants reveals that the difference in public interest law placement is indeed concentrated among applicants to three-year tuition lotteries among whom the debt difference is the largest. In Table 10b, we observe a 20.8 percentage point differential in the rate of public interest law between three-year lottery winners and losers. On the other hand, the rate of clerkships among these applicants is slightly below the sample average (17.7%). Meanwhile, the public interest law differential among PSS1 and PSS2 individuals is 10.2 percentage points and the clerkship differential is 15.5 percentage points, although both differences are insignificant due to the small number of late applicants (60).

As an approximation of final job outcomes for all four classes of study subjects, I use data on clerks in the classes of 1998-2000 to assign predicted job sectors to clerks in the class of 2001 based on a vector of coefficients from a probit estimation of public interest employment on the following individual characteristics: age, sex, minority, marital status, public interest commitment reported in the exit survey – including what fraction of the next decade a student plans to spend in a private law firm and in non-profit law, the importance of social contribution, and the importance of salary –, and first-year and second-year summer public interest

[28] Alternatively, assuming they enter the private sector gives virtually the same treatment effect as in Table 9 – 35% with a t-statistic of 2.00. Thus we can safely assume that the real effect lies between 35 and 36%. However, anecdotal evidence supports the prediction that students taking more than one clerkship disproportionately take public sector jobs.

employment dummies.[29] Based on the predicted placement data for the class of 2001, clerks in the treatment group are nearly twice as likely to enter public interest work. Incorporating predicted values for clerks in the class of 2001 to generate an expected distribution of sample-wide first job placement produces a similar estimate of program effect on public interest work for all four classes, shown in Table 11a. Table 11b shows the predicted distribution for early applicants only.

As an alternative outcome measure not complicated by clerkships, I also look at exit survey data on long-term career plans among three-year lottery participants. Specifically, students are asked about the job settings in which they plan to spend the next ten years of their careers. Table 12 reports the percentage of the next 10 years students plan to spend in private and public interest law, net of time out of the labor force. This data shows a pattern strikingly similar to the distribution of first job placement.

Treatment group subjects report planning to spend 25.5 percentage points more of the next ten years in non-profit law, even higher than the 18.6 percentage point predicted difference in first job placement. It is also evident that this difference does not simply reflects the fact that control group subjects also plan to spend a significant less amount of time in a clerkship, which they will substitute for private sector work. As reported in Table 13, the mean differences in career settings net of planned fraction of next ten years in a clerkship is even larger.

From Tables 12 and 13, it is evident that stated career plans of all participants are consistent with observed first job choice. Just as treatment group members are less likely to take an immediate private sector job and more likely to take a clerkship, so are they more likely to report a greater planned percentage of the next ten years in these settings. This is hardly surprising since most graduates have already made first job arrangements at the time of the exit survey. Nonetheless, the exit survey findings suggest that patterns of first job choice do not simply reflect a difference in the *order* of job setting, but more likely a long-term difference in career experience. In other words, there is strong evidence from these data that the timing of

[29] Choice of covariates based on the observed probability of public interest employment post-clerkship, see Appendix C.

financial aid affects not only the likelihood that a student takes an immediate job in public interest law, but also the likelihood that he or she ever does. This implies that the long-term rate of public interest employment will depend even more on financial aid timing than the initial estimates suggest.

The exit survey also provides data on desired job characteristics. Interestingly, while both observed job choices and exit survey career plans differ substantially according to financial aid package, the pattern of preferences in job characteristics at the end of law school is remarkably similar across experimental groups. Only one of 15 job characteristics that students were asked to rank – the importance of practical experience – was significantly different at the end of law school. The fact that job preferences, including the relative importance of such factors as salary and contribution to society, are virtually equivalent between control and treatment groups, yet both stated plans and observed choices differ substantially suggests that systematic type differences in work preferences are not driving the experimental results.

5.3 Selection on Observables Model

As discussed in Section 5.1, one factor plausibly driving the discrepancy in final outcomes is a difference in the matriculation rates of law school applicants according to lottery outcome. This section attempts to gauge whether or not sample selection is responsible for the previous set of job placement results. We have already observed evidence of sample selection in a comparison of pre-law school observables in Tables 5 and 6. In order to isolate selection bias at the matriculation stage, the earlier findings suggest the need to account for differences in nonlinear relationships between pre-treatment observable characteristics. With this in mind, I construct an alternative set of comparison groups using propensity score matching techniques to identify a set of treatment group members best matched to control group members based on non-linear relationships between variables associated with the likelihood of program participation.[30] Assuming that differential matriculation rates reflect lottery winners being disproportionately encouraged to attend, the control types are necessarily a subset of treatment types. Essentially,

[30] For an overview of matching techniques, see Heckman, LaLonde and Smith (2000).

this procedure attempts to identify that subset of treated individuals most comparable to the subset of original control group members who remain after matriculation.

To identify such participants, for each lottery loser I associate a match outcome by way of minimum distance estimation according to the following set of pre-treatment characteristics: graduation year, RTK applicant, LSAT score, undergraduate debt, undergraduate GPA, undergraduate school rank, whether undergraduate school is public, sex, age, minority, prior years in public interest employment, marital status, family net worth, family net income, and the interaction between LSAT and GPA, and LSAT and college rank. In particular, a standard probit over the entire sample of pre-enrollment lottery applicants (PSS0 and RTK) estimates a maximum likelihood probability that a student was a member of the control group. Predicted match scores were then calculated from the vector of coefficients on these variables and assigned to all members of the treatment and control groups. Matching outcomes were determined according to kernel-weighted average outcomes of all treated individuals in the region of common support, and all results were compared for robustness with stratified and nearest-neighbor matching.

The general technique is an inverse application of traditional propensity score methods used to estimate the average treatment effect in a population. In my method, the difference in average job placement between the control group members and the matched subset of treatment group members instead estimates the hypothetical effect of treatment on the subgroup of individuals who would have matriculated even in the absence of the program. Hence, the difference between this estimate and the estimate of gross program effect from the previous section, which also includes an effect on the probability of participating and consequent changes in average sample characteristics, is reasonably interpreted as the "gross selection effect" – that is, the change in the distribution of average student characteristics plus the effect of treatment on these marginal participants. While I am unable to fully identify the average treatment effect, decomposing the gross program effect into these two components allows me to rule out the possibility of zero average treatment effect as long as a positive program effect is observed among the subset of the treated that would have enrolled regardless of lottery outcome.

In the case of public interest placement, that is indeed the case. Table 14 presents the estimated average program effect using kernel matching methods on the identified subset of "unconditional" participants.[31,32] Note that the control group means are identical to the previous estimate in Table 10b since the composition of the control group does not change. In contrast to the result for public interest law, individuals in the new treatment group are substantially less likely to take a clerkship relative to the original set of treatment individuals.

The fact that a treatment effect on public interest employment is observed indicates that the gross program effect cannot be attributed to selection on observables at enrollment. This suggests that, unless there is a significant role of selection on unobservables occurring at matriculation, even in a scenario in which applicants are informed of the lottery outcome post-matriculation, the influence of educational debt while in school would still generate a difference in the rate of public interest placement. In fact, it appears that students encouraged to matriculate on account of tuition waivers have slightly lower rates of public interest placement, controlling for other factors: the measured effect of treatment for this subset of lottery winners is 23.8 rather than 20.8 percentage points. Thus, sample selection out of treatment may actually bias downward the impact of financial aid timing on career choice. A reasonable explanation consistent with this observation is that treatment individuals whose enrollment decision is influenced by debt timing not only have higher undergraduate debt and higher LSAT scores, but also have lower average commitment to public interest work, controlling for these characteristics. Since non-matriculaters are those most easily dissuaded from attending a law school with a high commitment to public interest work by a difference in financial aid timing, they are also likely to be those most easily dissuaded from entering public interest law.

In the case of clerkships, the propensity score estimates are half as large and insignificant.[33] This suggests that, were matriculation rates unaffected by lottery outcomes, debt timing would be associated with less of a difference in the rate of clerkships than we observe in the presence of selection. In light of this result, differential enrollment according to observable

[31] Both stratification and random draw nearest neighbor matching produced a similar pattern of results.
[32] OLS regression estimates using the same set of covariates are presented in Appendix D.
[33] Stratified and nearest neighbor matching estimates of ATE are 3-5 percentage points and insignificant.

performance measures such as LSAT scores and college rank is a likely explanation for the extreme difference in clerkships between the original control and treatment groups, as clerkships are largely merit-based appointments. In other words, students with the best outside options lie disproportionately in the margin of influence for career-contingent aid at the enrollment stage.

In sum, propensity score estimates of comparable groups of enrolled students indicate that, while sample selection on observables into treatment explains much of the difference in rates of clerkships, matriculation patterns do not account for the public interest placement differential between the treatment and control groups. Thus, unless there is a significant degree of selection on unobservables occurring at the enrollment stage, it appears that, in addition to influencing enrollment decisions, financial aid timing has an independent effect on the career decisions of students who matriculate.

5.4 Job Market Signaling

Before concluding that the results of this analysis reflect a response to debt among students, it is important to eliminate the possibility of job market signals altering the relative employment prospects of treatment and control group members. Despite the fact that PSS were distributed by randomized lottery so provide no information on winners and losers, it is conceivable that public interest employers perceive career-contingent scholarships as valuable job market signals of quality and commitment to public interest work. Since lottery losers are presumably unable to indicate to employers that they applied for a PSS, winning the lottery could conceivably alter job opportunities in the public sector. If present, this asymmetry between winners and losers should be reflected in higher average wage offers conditional on ability for scholarship-holders, thereby disproportionately encouraging lottery-winners to enter public interest law.

A useful way to test for the signalling effect of scholarships would be to look at differences according to experimental group in callback rates and salary offers of public interest employers independent of job acceptance. Unfortunately, these data are not yet available from the IFAS. A much cruder indicator of any significant demand advantage of PSS subjects is found

by looking at relative wage differences in public interest versus private firms between control and treatment groups, controlling for observable measures of ability and commitment level. Assuming that all relevant characteristics that are observable to employers are contained in the data set, then a significant positive coefficient on the treatment dummy in a regression of starting salary on employee characteristics would indicate a premium on scholarship participation.

From the regression results in Table 16, it appears that treatment status is unrelated to mean sector-specific salary when controlling for indicators of ability and commitment that are plausibly used by employers to evaluate candidates. While the desirability of a given job is not fully captured by starting salary, especially given the level of compression of starting salaries in all three sectors, a few factors explain most of the variation in private sector salaries – including class rank, GPA, age, and the importance of social contribution – and treatment status is not among them. This suggests that financial aid package is an unimportant signal of commitment or ability to employers. Clearly, if there are important unobservable factors influencing employment opportunities, these estimates will also suffer from selection problems so should be interpreted with caution.

6 Conclusions

This study has provided evidence that the timing of educational debt influences career choices. Under a career-contingent financial aid program that offers tuition waivers rather than an equivalent amount of loan repayment assistance, rates of first job placement in public interest law are roughly one third higher. Very little of this appears to be explained by differential matriculation rates according to loan package. Thus, the positive effect of the tuition subsidies on graduates' rate of public interest employment operates not through attracting students more committed to public interest work, but by altering the role of debt in students' post-enrollment career decisions. Recipients of forward looking career-contingent financial aid are also nearly twice as likely to take a competitive clerkship after law school, though much of this appears to be determined at the enrollment stage.

The fact that career-contingent tuition subsidies are associated with higher rates of public interest law than are financially equivalent backward-looking loan repayment schemes provides strong evidence of time-inconsistent debt aversion.[34] As discussed in Section 3.2, there are two possible reasons debt aversion could lead students to make career choices that depend on the timing of educational debt: Either debt suffered early is perceived to be costlier, or else borrowers' career decisions are influenced by framing effects which arise from differences in financial aid timing. For instance, borrowers' preferences may be characterized by loss averse debt aversion, which has the potential to generate a reversal in career preferences when debt is absorbed in the first stage. There is significant reason to be doubtful that early debt is perceived to be costlier due to the non-binding nature of LRAP. Given that NYU Law School is widely known for offering the most comprehensive public service infrastructure of any law school in the nation and actively markets itself as a public interest law school, discontinuation or reduction in LRAP benefits should be deemed highly unlikely. For these reasons, time-inconsistent debt aversion such as that which would occur if individuals were characterized by loss averse preferences, is arguably a more plausible explanation.

Regardless of the mechanism, the policy implication for a school interested in increasing its supply of graduates to the public interest sector is straightforward. By distributing career contingent scholarship funds early on in students' careers rather than after they graduate, a law school's financial aid policy is likely to generate a higher rate of public interest work among graduates. Given that retrospective debt relief is currently by far the most common form of career-contingent financial aid, these results imply that up-front tuition subsidies would be a more efficient allocation of institutional funds for this purpose. Furthermore, a move to forward-looking loans also has the potential to attract a higher *quality* pool of entering students and as a result a higher rate of clerkships, as it appears that students are willing to trade off school quality for short-term debt relief. This effect alone may be of interest to school administrators. As David

[34] A corresponding piece of evidence on the existence of framing effects of debt is the fact that a significant number of scholarship winners chose *not* to renew their fellowships while in law school and take out federal loans to cover tuition costs in the remaining years. Since students face no interest payments on the loan until they graduate, failure to renew presumably reflects a preference for absorbing debt early on once it is determined by the student that he prefers private sector work. Indeed, failure to renew is a near perfect predictor of immediate employment in the private sector: 12 of the 14 lottery winners who fail to renew their scholarship take a job directly in a private law firm. It is difficult to explain such a distaste for career-contingent subsidies relative to debt in the absence of framing effects.

W. Leebron, dean of Columbia University Law School and a 1979 Harvard Law graduate, quoted in a recent New York Times article, “There is far more competition among law schools for the best applicants than there has ever been in the past … Students who think that a school will be too oppressive, unfriendly or impersonal are willing to turn it down — even if it is Harvard — in favor of a school perceived as more hospitable” (Glater, 2001).

From a social welfare perspective, a policy change has the potential to increase overall educational investment in job sectors with high social returns. While loan repayment encourages some level of this, results from the IFAS experiment suggest that forward-looking career-contingent subsidies, such as the type that are currently being considered in the British system, would be even more effective in encouraging this type of investment. Depending on the degree of external validity of this study, other policy programs also attempting to encourage public interest employment through educational loan assistance should bear in mind the potential benefit of providing tuition money up-front in place of promises of future payment. If other students mirror law school students in their attitudes towards debt, this relatively costless policy difference could have significant impact on program effectiveness in raising rates of first-job placement in the public interest sector.

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Table 1: Median Law School Tuition

	1991	2001
Public School (resident)	$3,225	$7,738
Public School (non-resident)	$8.006	$17,538
Private School	$12,999	$22,870

Table 2: Annual Mean Starting Salaries

	Class of 1998	Class of 1999	Class of 2000	Class of 2001
Public interest law	$34494	$36006	$36523	$39922
Private sector	$95783	$100872	$124355	$123517

Table 3: Law School Loan Repayment Programs

	Total LRAP Funds Disbursed (1998-99)
Yale Law School	$1,369,061
New York University Law School	$1,091,579
Harvard Law School	$1,069,081
Columbia University School of Law	$748,179
Stanford University Law School	$546,148
Georgetown University Law Center	$511,034

Source: "Financing the Future: NAPIL's 2000 Report on Law School Loan Repayment Assistance and Public Interest Scholarship Programs," NAPIL(2002)

Table 4: Lottery Weights

	Probability of winning	# Control	# Treatment	Weight assigned to controls only	Control (weighted)
PSS0 only	0.276	142	54	1	142
RTK & PSS0	0.750	16	48	7.875	126
PSS 1	0.431	31	22	1.870	58
PSS 2	0.571	3	4	3.500	10.5

Table 5: Sample characteristics

	N	control	treatment	$\|t_\Delta\|$
Female	192/128	0.62	0.64	0.29
Age	192/128	31.2	30.9	0.71
Married	192/128	0.074	0.094	0.66
Minority	192/128	0.076	0.055	0.65
Parents' net worth	148/95	244964	151515	1.78
Parents' net income	148/95	51962	60206	0.68
Home worth	148/95	49322	39012	0.60
LSAT	192/128	167.5	168.7	**2.03**
Undergraduate GPA	192/128	3.63	3.63	0.01
Rank of undergraduate institution	192/128	4.08	4.06	0.23
Undergraduate school public	192/128	0.274	0.304	0.49
Undergraduate debt	192/128	3653.5	5037.0	1.36
Other pre-law debt	192/128	4319.5	2308.0	0.94
Years of PI experience	192/128	0.98	1.24	0.84
Foreign	192/128	0.008	0.008	0.06

Table 6: Sample characteristics of PSS0 and RTK applicants only

	control	treatment	$\lvert t_{\Delta} \rvert$
Minority	0.059	0.082	0.11
Female	0.65	0.68	0.38
LSAT	167.5	168.9	**1.98**
Undergraduate GPA	3.66	3.63	0.30
Rank of undergraduate institution	4.14	4.03	1.02
Parental net worth	246436	151071	1.28
Parental net income	44892	62512	1.28
Undergraduate debt	3274	4818	1.37
Other entering debt	5130	2358	1.06

Table 7: Correlation between LSAT and pre-law school characteristics *

	treatment	control
	LSAT	LSAT
Undergraduate GPA	0.0372	-0.4198
Rank of undergraduate institution	0.0170	-0.2517
Female	-0.1727	-0.0962
Minority	-0.5498	-0.3647
Parental net worth	0.1635	0.1055
Parental income	-0.0027	-0.0528
Undergraduate debt	-0.1056	-0.1504
Other entering debt	0.0192	-0.0118

*First diff significant at 1% level, second diff significant at 10% level

Table 8: First job placement of graduates

	control	treatment	$\lvert t_{\Delta} \rvert$
Public interest law	38.0%	34.4%	0.51
Non-qualifying employment	41.1%	25.0%	2.41
Clerkship	20.9%	40.6%	3.16
N	193	128	χ^2=11.51

Table 9: First job placement of graduates after 2-years, classes of 1998-2000

	control	treatment	$\lvert t_{\Delta} \rvert$
Public interest law	48.3%	66.0%	2.17
Non-qualifying employment	51.8%	32.9%	2.08
Second clerkship	0.3%	1.1%	0.63
N	106	94	χ^2=5.91

Table 10a: Fraction in public interest law after 2-years, classes of 1998-2000

	control	treatment	Δ	$\lvert t_{\Delta} \rvert$	N
Public interest law	48.3%	67.0%	18.7	2.17	200

Table 10b: Fraction in public interest law, RTK/PSS0 participants only

	control	treatment	Δ	$\lvert t_{\Delta} \rvert$	N
Public interest law	49.3%	70.1%	20.8	2.69	163

Table 11a: Predicted final job sector for all classes (19998-2001)

	control	treatment	Δ	$\|t_\Delta\|$	N
Public interest law	47.2%	64.1%	16.9	2.38	275

Table 11b: Predicted final job sector for all classes (19998-2001), RTK/PSS0 only

	control	treatment	Δ	$\|t_\Delta\|$	N
Public interest law	48.9%	67.7%	18.8	2.30	217

Table 12: Ten-year career plans *

	control	treatment	Δ	$\|t_\Delta\|$
Percentage of years in for-profit law firm	30.2%	18.7%	-11.5%	1.99
Percentage of years in public interest law	31.2%	56.7%	25.5%	2.37
Percentage of years in clerkship	5.1%	8.8%	3.7%	1.98

* Remaining time includes non-legal employment and “unsure”.

Table 13: Ten-year career plans net of clerkship time

	control	treatment	Δ	$\lvert t_{\Delta} \rvert$
Percentage of years in for-profit law firm	31.6%	19.5%	12.1%	2.10
Percentage of years in public interest law	33.5%	60.5%	27.1%	2.45

Table 14: Kernel-based matching outcomes

	Mean of matched treated	Mean of control	Average treatment effect on unconditional applicants	Bootstrapped SE
Public interest law*	0.731	0.493	- 0.238	0.096
Clerkship	0.321	0.232	- 0.089	0.067

* Classes of 1998-2000 only.

Table 15: OLS regression of mean starting salary

	Private Sector		Non-profit Sector		Clerks	
Group means (treatment/control)	*103061 / 110613*		*35199 / 35145*		*38418 / 39430*	
	Starting salary	$\|t_\Delta\|$	Starting salary	$\|t_\Delta\|$	Starting salary	$\|t_\Delta\|$
PSS/RTK (treatment)	-5409.52	0.86	1409.12	0.86	983.217	0.62
RTK winner	-4766.91	0.86	991.10	0.53	-891.411	0.72
LSAT	2103.20	2.65	-166.09	0.94	-89.0669	0.83
UG GPA	17987.8	1.72	-5782.70	1.10	2667.72	1.05
Rank of UG school	7589.24	1.88	940.520	0.74	-536.250	0.56
Class rank	33223.0	3.39	4242.45	0.86	588.059	0.20
Last GPA	30545.0	2.28	-7066.16	1.68	462.254	0.13
Minority	37822.4	3.62	-3701.76	0.99	2607.52	1.33
Female	15243.6	2.34	-3033.67	1.81	-224.889	0.20
Age	-176.637	0.33	-629.408	2.04	12.9679	0.07
Rank soc. contrib.	-505.445	0.53	72.7360	0.20	68.4256	0.33
Rank salary	731.023	0.68	-143.938	0.20	-133.298	0.33
% career plan in PI	-28469.1	2.86	-2568.27	1.01	-3725.14	1.70
Summer 1 PI job	6135.01	0.86	-3531.93	1.67	1091.23	0.61
Summer 2 PI job	2597.70	0.32	1260.94	0.69	873.149	0.79
Adj. R^2	*0.6218*		*0.3753*		*0.1914*	

Figure 1: 2000-2001 Federal Student Expense Budget

	Full Tuition + LRAP	Tuition Waiver (PSS) + LRAP
Annual Debt	$ 48,550	$ 29,183
Total Debt [Annual x 3 years]:	$ 145,650	$ 87,550
Total Amount of Available LRAP:	$ 145,650	$ 87,550

Source: Memo, NYU Law School Office of Financial Aid (2000)

Figure 2: Cost of tuition according to lottery outcome

PSS lottery winners (LRAP + PSS):

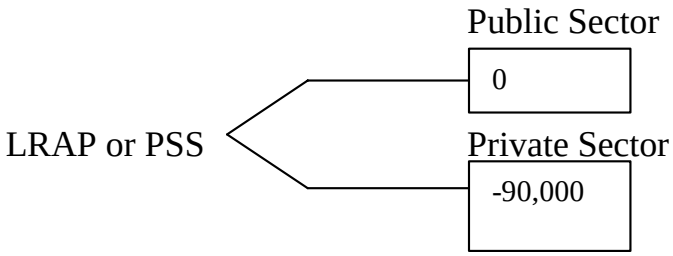
(1a) c[tuition| public interest job] = $30,000 - $30,000
(1b) c[tuition| private sector job] = $30,000 + $60,000

PSS lottery losers (LRAP only):

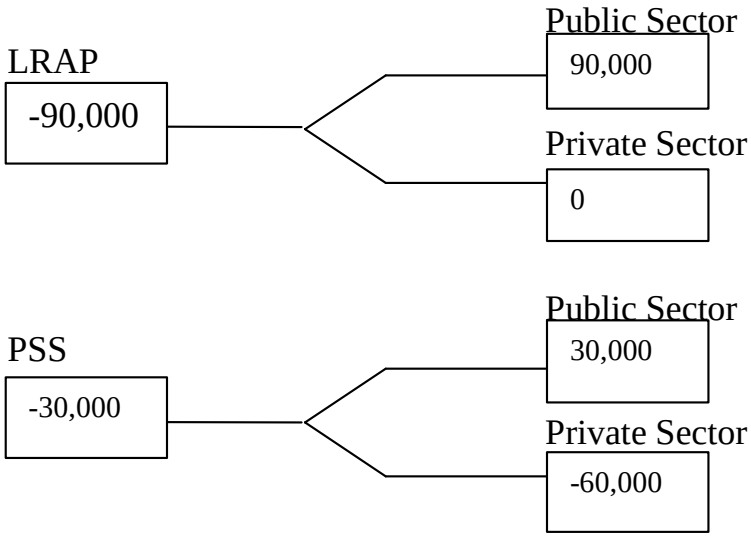
(2a) c[tuition| public interest job] = $90,000 - $90,000
(2b) c[tuition| private sector job] = $90,000

Figure 3: Job Sector Choice

A: ONE-STAGE DECISION



B: TWO-STAGE DECISION



Appendix A: Experimental design and construction of sample weights

	Root + PSS	PSS0	Root/PSS0	PSS1	PSS2	Total
Number Applications†	64	196	260	123	36	419
1st-time apps			260	53	7	
2nd-time apps				70	6	
3rd-time apps					23	
Number Winners	48	54	102	43	13	158
1st-time apps			102	22	4	
2nd-time apps				21	2	
3rd-time apps					7	
Number Losers	16	142	158	80	23	261
1st-time apps			158	31	3	
2nd-time apps				49	4	
3rd-time apps					16	
Number Reapplicants			70	29		
1st-time apps			70	6		
2nd-time apps				23		
Treatment (1st-time apps)			**102**	**22**	**4**	**128**
Control (1st-time apps)			158	31	3	192
Eventual Winners			28	2	0	
Applied twice			21	2		
Applied three times			7			
Control net of eventual winners			**130**	**29**	**3**	**162**
Applied once (unweighted)			88	25	3	
Applied twice (weighted)			26**	4*		
Applied three times (weighted)			16***			
Weight (applied twice):			1.81**	1.5*		
Weight (applied three times):			1.44***			

† Includes only matriculating applicants.

* Weight equal to total number of PSS2 first-time reapplicants (6) divided by number who lose (4): 6/4. ** Weight equal to probability of PSS1 reapplicants becoming discouraged (26/49) multiplied by number of PSS1 reapplicant winners (21) plus number of discouraged losers (26) all divided by number of discouraged losers (26): [26+21(26/49)]/26. *** Weight equal to probability of reapplying for PSS2 (23/49) multiplied by number of PSS1 reapplicant winners (21) plus number of reapplicants (23), all divided by number of losing reapplicants (16): [23+21(23/49)]/16.

Appendix 3.B: Experimental design and construction of sample weights, Classes of 1998-2000 only

	Root + PSS	PSS0	Root/PSS0	PSS1	PSS2	Total
Number Applications†	52	148	200	91	36	327
1st-time apps			200	33	7	
2nd-time apps				58	6	
3rd-time apps					23	
Number Winners	40	37	77	40	13	130
1st-time apps			77	20	4	
2nd-time apps				20	2	
3rd-time apps					7	
Number Losers	12	111	123	51	23	197
1st-time apps			123	13	3	
2nd-time apps				38	4	
3rd-time apps					16	
Number Reapplicants			58	29		
1st-time apps			58	6		
2nd-time apps				23		
Treatment (1st-time apps)			**77**	**13**	**4**	**94**
Control (1st-time apps)			123	20	3	146
Eventual Winners			25	2	0	
Applied twice			18	2		
Applied three times			7			
Control net of eventual winners			**98**	**17**	**3**	**118**
Applied once (unweighted)			65	13	3	
Applied twice (weighted)			17**	4*		
Applied three times (weighted)			16***			
Weight (applied twice):			1.94**	1.5*		
Weight (applied three times):			1.44***			

† Includes only matriculating applicants.

* Weight equal to number of PSS2 first-time reapplicants (6) divided by number who lose (4): 6/4. ** Weight equal to probability of PSS1 reapplicants becoming discouraged (14/38) multiplied by number of PSS1 reapplicant winners (20) plus number of discouraged losers (14) all divided by number of discouraged losers (14). *** Weight equal to probability of reapplying for PSS2 (23/38) multiplied by number of PSS1 reapplicant winners (20) plus number of PSS2 reapplicants (23), all divided by number of losing reapplicants (16).

Appendix C: Probit Estimate of Probability of Public Interest Employment

Class of 1999	-0.1357
	(0.144)
Class of 2000	-0.1214
	(0.140)
Root applicant	-0.1614
	(0.186)
PSS1 applicant	-0.4939
	(0.314)
PSS2 applicant	-0.6636
	(0.471)
Treatment group	0.1836
	(0.195)
Treatment group * Root	0.0209
	(0.250)
Treatment group * (PSS1/PSS2)	0.4817
	(0.429)
Minority	0.1957
	(0.261)
Age	0.0240
	(0.023)
Married	-0.2208
	(0.213)
Female	0.0617
	(0.134)
Summer 1 public interest employment	0.1880
	(0.136)
Summer 2 public interest employment	-0.0077
	(0.143)
Planned % of career in private law firm	-0.0494
	(0.164)
Planned % of career in non-profit law	0.6808
	(0.221)
Importance of social contribution	0.0347
	(0.019)
Importance of salary	-0.0006
	(0.025)

Appendix D: OLS estimate of treatment effect on observables

	Public interest law	Clerk
treatment dummy	0.24	0.147
	(0.96)	(0.08)
LSAT	-0.20	0.04
	(-0.24)	(0.01)
undergrad debt	-0.04	0.08
	(0.07)	(0.06)
undergrad GPA	-0.81	0.31
	(-0.93)	(0.51)
Root applicant	0.21	0.15
	(0.12)	(0.11)
college rank	0.20	0.99
	(0.33)	(0.97)
college public school	0.26	0.11
	(0.10)	(0.11)
female	-0.04	0.06
	(-0.10)	(0.08)
minority	-0.63	-0.09
	(-0.26)	(-0.21)
age	0.05	0.01
	(0.02)	(0.02)
prior years in PI job	0.06	0.03
	(0.02)	(0.02)
married	0.14	-0.10
	(0.17)	(-0.11)
family net worth	0.22	-0.17
	(0.12)	(-0.11)
family net income	0.18	0.14
	(0.73)	(0.07)
LSAT*(undergrad GPA)	0.04	-0.02
	(0.05)	(-0.03)
LSAT*(college rank)	0.01	0.01
	(0.01)	(0.01)

Appendix E: Law Schools with Career-Contingent Financial Aid Programs

Law Schools with LRAP

1. American University College of Law
2. Benjamin N. Cardozo School of Law
3. Boston College Law School
4. Brooklyn Law School
5. Case Western Reserve University Law School
6. Columbia University School of Law
7. Cornell University Law School
8. Duke University School of Law
9. Fordham University School of Law
10. Franklin Pierce Law Center
11. George Washington University Law School
12. Georgetown University Law Center
13. Harvard Law School
14. Hofstra University School of Law
15. Loyola Law School, Los Angeles
16. Loyola University, Chicago School of Law
17. Loyola University, New Orleans Law School
18. New York University School of Law
19. Northeastern University School of Law
20. Northwestern School of Law
21. Pace University School of Law
22. Rutgers University School of Law, Newark
23. Santa Clara University School of Law
24. Stanford University Law School
25. Suffolk University Law School
26. Temple University Beasley School of Law
27. Tulane University School of Law
28. University of California Berkeley Law School
29. University of California, Davis Law School
30. University of California, Hastings Law School
31. University of Chicago Law School
32. University of Georgia School of Law
33. University of Iowa College of Law
34. University of Michigan Law School
35. University of the Pacific Law School
36. University of Pennsylvania Law School
37. University of San Diego School of Law
38. University of San Francisco School of Law
39. University of Southern California Law School
40. University of Toronto, Faculty of Law
41. University of Utah College of Law
42. University of Virginia School of Law
43. Valparaiso University School of Law

44. Vanderbilt University Law School
45. Vermont Law School
46. Yale Law School

Law School Public Interest Scholarship Programs

1. Boston College Law School
2. Drake University Law School
3. Fordham University School of Law
4. Georgetown University Law Center
5. Gonzaga University School of Law
6. Loyola Law School, Los Angeles
7. New York University School of Law
8. Northeastern University School of Law
9. Santa Clara University School of Law
10. Stanford University Law School
11. University of Denver College of Law
12. University of Iowa College of Law
13. University of Kansas School of Law
14. University of Pennsylvania Law School

Appendix F: Other Financial Aid Programs to Encourage Public Sector Work

Loan Forgiveness/Repayment Assistance

US Government: Perkins loans can be cancelled for full-time service as a teacher in a designated elementary or secondary school serving students from low-income families, special education teacher (includes teaching children with disabilities in a public or other nonprofit elementary or secondary school), qualified professional provider of early intervention services for the disabled, teacher of math, science, foreign languages, bilingual education, or other fields designated as teacher shortage areas, employee of a public or non-profit child or family service agency providing services to high-risk children and their families from low-income communities, nurse or medical technician, law enforcement or corrections officer, and staff member in the educational component of a Head Start Program, service as a Vista or Peace Corps Volunteer and service in the Armed Forces (up to 50% in areas of hostilities or imminent danger).

Army National Guard: Students who serve may be eligible for their Student Loan Repayment Program, which offers up to $10,000. (Note: the military and veterans' associations provide many scholarships and tuition assistance programs.)

Students who majored in education and teach in Mississippi are eligible for the William Winter Teacher Scholar Loan. This program forgives one year of your loan in exchange for one year of service (it forgives two years of your loan if you teach in a shortage area).

National Health Service Corps: Offers forgiveness programs to physicians who agree to practice for a set number of years in areas that lack adequate medical care (including remote and/or economically depressed regions).

California Office of Statewide Health Planning and Development: Offers a State Loan Repayment Program for resident physicians involved in primary care and community health clinics.

Maryland State Government: State and local government employees who earn less than $40,000 gross annually may be eligible for a loan assistance/repayment program to study law, nursing, physical and occupational therapy, social work and education.

Public Service Scholarships (Work-contingent Tuition Assistance)

Harvard Kennedy School: Robert G. Wilmers Program for State & Local Public Service Fellowships: Up to 10 Wilmers Public Service Fellows study at the Kennedy School each year. The fellowship program is designed “to encourage talented students to pursue public service careers, reward their commitment to helping others, and free them of the significant debt burden many incur in graduate school.” The fellowships cover the full cost of tuition (two semesters plus a summer session, if required) and fees, plus an annual stipend, and recipients must commit to working in public service for three years after completing the program.

New York Teacher's College: Under the Peace Corps Fellows Grant, former Peace Corps volunteers receive reduced tuition at Teachers College in exchange for a two-year commitment to teach mathematics, science, bilingual/bicultural education, special education, and Teaching of English to Speakers of Other Languages (TESOL) in the New York public schools where critical shortages of qualified teachers exist in these subjects.

Massachusetts Tomorrow's Teachers Scholarships: Established by the state legislature in 1999, the program offers four-year scholarships to Bay State high school students in the top 25 percent of their class who enroll in a Massachusetts college or university degree program leading to teacher certification. Scholarship winners agree to teach in Massachusetts's public schools for four years upon graduation, especially in subject areas or geographical regions and school districts where there is a documented teacher shortage.

CUNY's Teaching Opportunity Program (TOP): Beginning in 2001, the school will provide incentive scholarships and special training to highly-qualified students who commit to pursuing teaching careers, especially in critical shortage areas such as mathematics and science. Private funding, including foundation support, has been obtained to provide tuition assistance to program entrants.

Michael Murphy Loan: Students who commit to work off one-fifth per year as a State Trooper (or related law enforcement official) in Alaska are eligible for loans to study law enforcement, law, probation and parole, penology, or other related fields.

Appendix G: LRAP Guidelines*

LRAP recognizes annual debt service on law school loans approved by NYU, generally covering three years of the student expense budget less aid received and less a student contribution calculated at the time of initial application for need-based financial aid. Participants may decide to consolidate their loans under the Federal Consolidation Loan Program, or otherwise extend repayment periods. However, LRAP will only make disbursements to participants for actual payments made or monthly payments that would be required on a 10-year schedule (whichever is less) for up to ten years following graduation. At annual qualifying incomes of less than $37,651, participants pay $0 towards annual debt service on eligible loans. At annual qualifying incomes between $37,651 and $57,651, participants pay 40% of the income in excess of $37,651 towards their annual debt service on eligible loans.

For example at a total debt of $75,000:

Qualifying Incomes	Student Annual Payments	LRAP Annual Disbursements
$36,000	$0	$11,043
$38,000	$140	$10,903
$40,000	$ 940	$10,103
$42,000	$1,740	$9,303
$44,000	$2,540	$8,503
$46,000	$3,340	$7,703
$52,000	$5,740	$5,303
$55,000	$6,940	$4,103

Qualifying income is adjusted annually for inflation and career progression. If graduates seeks LRAP benefits after working in a high-paying position, NYU will assume the following: during the years in which their gross income exceeded the prevailing public service salary, they contribute, toward debt service payments and prepayments of principal forty percent of the amount by which their gross income exceeded the prevailing salary. For the purposes of LRAP, the eligible debt will be reduced by the amount of such prepayments, regardless of gross income.

Exceptions for Judicial Clerks

Alumni who work in judicial clerkships are not eligible for LRAP benefits during the year(s) of their clerkship. Graduates who complete a clerkship, and who have met all of the eligibility criteria of the program during the clerkship year(s), and who enter LRAP eligible employment immediately following the clerkship would receive LRAP benefits retroactively.

* Information reprinted from the Financial Aid Office of NYU Law School.