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Educational Debt Burden and Career Choice: Evidence from a Financial Aid Experiment at NYU Law School[†]

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This paper examines the influence of psychological responses to debt on career choices from an experiment in which alternative financial aid packages were assigned by lottery to a set of law school admits. The packages had equivalent monetary value, but one required the student to take on a loan that would be paid for by the school if he worked in public interest law, while the other covered tuition as long as the student worked in public interest law. If he did not, the student would be required to reimburse the school. Tuition assistance recipients have a 36 to 45 percent higher public interest placement rate and, when lottery results were announced before enrollment, were twice as likely to enroll. (*JEL* I21, I22, J44, D14)

Financing higher education in the United States increasingly requires a large amount of debt, a trend that has the potential to alter schooling and career choices. Educational loans mitigate the degree to which tuition hikes constrain investment, but even perfect access to credit may be insufficient to prevent market distortions resulting from debt burden. In particular, previous studies provided empirical evidence of debt aversion in many settings, which suggests that individuals experience disutility from debt beyond the interest expense of borrowing (Richard Thaler 1992).[1] It is unclear, however, whether these patterns can be explained entirely by rational responses of risk averse individuals to uncertainty over future income, or if choices about debt reflect psychological responses to borrowing. If individuals are not rational in terms of choices about debt, the need to finance higher education with loans may discourage optimal human capital investment and divert individuals toward careers with high monetary rather than social returns, even in the absence of borrowing constraints.

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[†] To comment on this article in the online discussion forum visit the articles page at: http://www.aeaweb.org/articles.php?doi=10.1257/app.1.1.1

[1] For instance, payoff rates of mortgages and student loans are irrationally rapid. See George Loewenstein and Thaler (1989), Thaler (1992) and Claire Callender (2006) for a discussion.

This paper examines the influence of educational debt burden on career choices in the context of a unique field experiment conducted at New York University's (NYU) School of Law in which two distinct financial aid packages were randomly allocated to incoming students. Both packages were forms of income-contingent financial aid, in which the tuition cost of law school is dependent on whether a student works in the low-paying public interest sector after graduating. The experimental manipulation was subtle. The first package, a standard loan repayment assistance program, consisted of tuition loans taken out by the student upon entering school, which would be repaid by NYU after graduation if the student chose to work in a low-paying job. The second package consisted of tuition waivers issued by NYU, which had to be repaid by the student after graduation only if she chose to work in a high-paying job.

The key feature of the experiment is that the two aid packages were designed to be equivalent in monetary value but differ in terms of the horizon over which a student considered herself in debt during and after school and in the framing of educational debt. Since standard economic theory predicts no differences between career decisions of people in the two treatment groups, the experiment provides a unique opportunity to isolate the influence of psychological factors on high stakes decisions pertaining to human capital investment and career.

Given that educational debt has the potential to influence career choices when entering school and when choosing a job after graduation, a central advantage of the experimental design is that, for half of the study classes, lottery assignment was announced early enough to influence matriculation decisions, while in the other half lottery assignment was announced after most enrollment had occurred. This feature makes it possible to identify separately the influence of a randomly assigned financial aid package on the decision to attend law school and on the decision to pursue a public interest career.

The experimental results indicate that career choices are indeed sensitive to receiving income-contingent tuition subsidies in place of loan repayment of an equivalent financial value. Law school graduates who received tuition waivers had a significantly higher rate of first job placement in public interest law. Furthermore, among students in two classes for which lottery outcomes were announced prior to application and enrollment deadlines, the availability of tuition subsidies increased the likelihood that law school admits enrolled, and appears to have increased the likelihood that prospective students apply to NYU. Not only do the findings offer rare experimental evidence of psychological influences on high-stakes decisions, but these findings have potentially important policy implications for educational finance in the United States and abroad given increasing use of income-contingent loans for higher education.

I. The NYU Innovative Financial Aid Study

At the country's premier law schools, students are graduating with average educational debt between $90,000 and $100,000, and the figure is rising. As seen in Table 1, between 1991 and 2001, law school tuition at private and public law schools nearly doubled, while wages in private sector and public interest law jobs steadily diverged (Equal Justice Works, NAPIL, and the Partnership for Public Service

Table 1—Median Law School Tuition

		1991		2001
Public school (resident)		$3,225		$7,738
Public school (nonresident)		$8,006		$17,538
Private school		$12,999		$22,870
	Class of 1998	Class of 1999	Class of 2000	Class of 2001
Annual mean starting salaries				
Public interest law	$34,494	$36,006	$36,523	$39,922
Private sector	$95,783	$100,872	$124,355	$123,517

Note: Nominal salaries reported.

Source: National Association for Public Interest Law (2002). Statistics from American Bar Association Section of *Legal Education and Admissions to the Bar.*

2002).[2] There is growing concern that educational debt of the current magnitude dissuades even the most dedicated graduates from taking public interest jobs. A recent survey of 1,622 law school graduates found that 66 percent did not consider a public interest job because of law school debt (Equal Justice Works, NAPIL, and the Partnership for Public Service 2002). In response, over the last few decades, many schools have initiated income-contingent loan repayment assistance programs (LRAP) designed to encourage public interest careers.[3] Loan repayment assistance defrays or, in some instances, fully covers the educational debt payments of graduates who work in qualifying public service jobs.[4]

NYU Law School's Weiss LRAP was among the first in the country, and a 1993 enhancement of funding made it one of the most generous. At NYU, for all graduates who choose careers in the public sector or other low paying fields of law, the majority of law school loans are forgiven annually for up to ten years after graduation. To be eligible for loan repayment, graduates must work full time in a position that involves law and earn less than an annual income threshold, defined for the class of 2004 as $57,651. In 1994, NYU Law School announced a $10 million research initiative, the Innovative Financial Aid Study (IFAS), which further expanded the amount of income-contingent aid available to graduates.[5] The IFAS was deemed innovative for two reasons. First, the program introduced income-contingent tuition subsidies, called public service scholarships (PSS), to a subset of students in the classes of 1998, 1999, 2000, and 2001. The PSS provided a grant for two-thirds tuition that converted to a loan in the event a recipient did not pursue a public interest law career.[6] The second innovation of the IFAS was the randomized allocation of

[2] Here, as throughout the paper, "public interest law" includes all government law jobs as well as nongovernmental nonprofit law. The salary gap widens over the ten year payback period because of the steeper wage profiles of private sector law jobs (Equal Justice Works, NAPL, and the Partnership for Public Service 2002).

[3] While in 1986 there were only five law school LRAPs nationwide, today there are 47 law school and four state LRAPs. There are also a handful of LRAP programs sponsored by state governments and employers.

[4] LRAP programs vary greatly in the amount of debt assistance offered and in eligibility requirements. See NAPIL (2002) for a comprehensive description of all programs.

[5] The IFAS was funded by an anonymous donor with the explicit objective of investigating tuition subsidies as a means of encouraging higher public interest law placement among NYU Law graduates.

[6] Specifically, a legally binding contract stipulated that any PSS recipient who takes a nonqualifying job during the first ten years of his career had to repay the amortized portion of his tuition scholarship corresponding to the portion of time spent in the private sector according to a repayment schedule matching federal loan terms.

all PSS by lottery across the pool of students who chose to enlist in the study during any year of law school. In total, 141 lottery winners were selected from the pool of 270 applicants, which consisted of 64 three-year (PSS3), 57 two-year (PSS2), and 20 one-year (PSS1) scholarships.[7] Lottery winners received PSS grants for all remaining years of law school, while lottery losers had to take out interest-free loans at the beginning of each year, but were eligible for loan repayment from NYU after graduation to cover tuition debt accrued during school.

As part of the IFAS evaluation, data were collected from six sources: law school applications; financial aid applications; law school academic records; first-year entry surveys on work experience, personal debt, career goals, and job preferences; third-year exit surveys identical to the entry survey but including school and summer activities; and work experience surveys mailed biennially for six years after graduation. Among study subjects, application data and job sector outcomes are available for all participants, while financial aid application data are available from 80 percent of participants and complete entry and exit surveys from 84 percent and 80 percent of participants (with 69 percent filling both). There are no statistically significant differences in reporting rates across treatment groups.

II. Conceptual Framework: Income-Contingent Loan Repayment versus Tuition Subsidies

The most important feature to note in comparing the financial aid options allocated through the IFAS is that the two packages were designed to be equivalent in net present value. Because all study subjects who did not receive tuition waivers had access to interest-free loans covering tuition through the school financial aid office, and because recipients of tuition subsidies were also eligible for loan repayment for the portion of expenses financed by loans, there was no difference in the monetary values of the two packages. Tuition subsidies were essentially loan forgiveness in reverse.

To illustrate, consider the cost of tuition and the monetary value of the two financial aid instruments given an annual tuition expense of $15,000, portrayed in the path diagram in Figure 1. While in school, lottery losers must borrow $45,000 to cover tuition transfers to NYU, while lottery winners incur only $15,000 of debt. After law school, NYU repays all tuition debt for individuals in either group who work in qualifying public interest jobs, which means that over ten years lottery losers receive $45,000 and lottery winners receive $15,000 in indirect transfers from NYU to the government. In the case of private sector employment, lottery winners immediately owe an additional $30,000 to NYU, bringing their total debt to $45,000, exactly what lottery losers continue to owe to the government after graduation. As the expected cost of law school is equal for winners and losers under both employment scenarios, economic theory predicts a Von Neumann-Morgenstern utility-maximizing individual to be indifferent between winning and losing the financial aid lottery. Similarly,

[7] Two students who failed to graduate in three years are excluded from these figures and proceeding analysis. In the case of failure to complete law school, the full amount of tuition debt is owed by the student in both study arms.

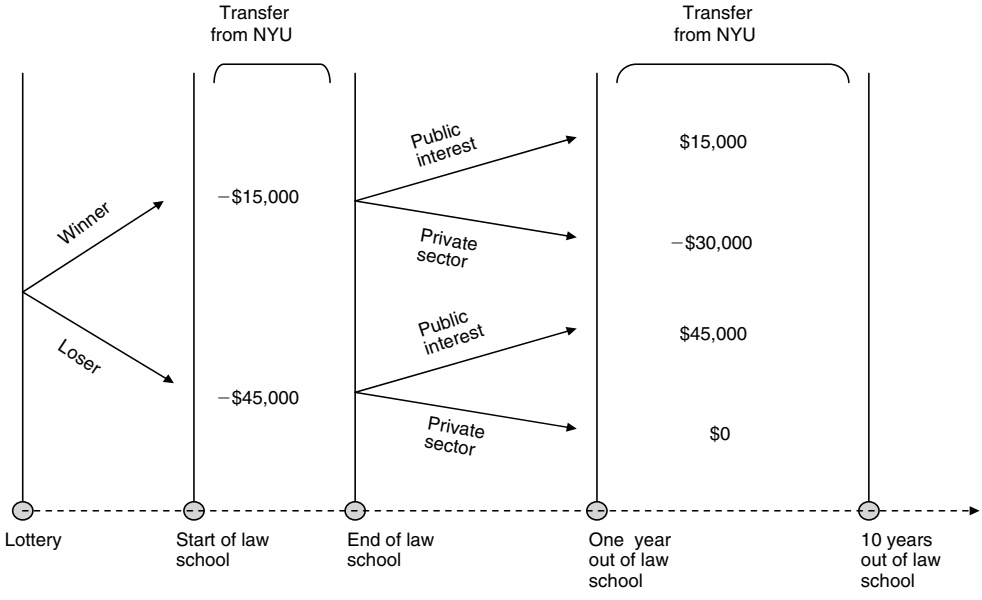

FIGURE 1. IFAS PATH DIAGRAM

because both packages offer the same reward for public interest work, the same individual should respond identically to the two forms of aid when making career choices.

However, two differences across the programs in the duration and framing of debt have the potential to generate differences in students' relative valuations of the programs and corresponding differences in their career decisions according to lottery outcome.

A. *Debt Burden*

First, under income-contingent loan repayment, students are formally in debt for an additional three years during school and ten years after graduation, regardless of their eventual career choice. The latter is due to the fact that, rather than signing over loans to NYU immediately upon graduating, loan repayment entails the school paying off loans over ten years during which time the declining balance remains in the student's name. Although a key intention of the study was to equate the borrowing costs of the two packages, such that longer duration of indebtedness imposed no additional financial risk or interest expense on lottery losers, it may be the case that students either anticipate indirect financial costs of debt or are burdened by the psychological or social cost of owing money.

If students perceive the state of debt to be costly for any reason, they will find it less attractive to enroll in law school and less attractive to enter public interest work when offered loan repayment rather than tuition subsidies, generating potential differences in both matriculation and public interest placement according to financial

aid package. Furthermore, the marginal influence on career choices of tuition subsidies relative to loan repayment will depend on the individual-specific costs of debt and the perceived likelihood of taking a public sector job.[8]

In considering potential sources of debt burden, it is important to keep in mind that career responses to differences in the duration of debt in this setting must reflect anticipation of either indirect financial costs or psycho-social costs of indebtedness rather than the cost to risk-averse individuals of smoothing consumption with debt when faced with uncertainty over future income streams (a classic source of debt aversion). Although the latter may well influence all students' decisions over public versus private law, income uncertainty is irrelevant when comparing income-contingent loan repayment to income-contingent tuition subsidies since both offer equal opportunity for debt forgiveness in case of negative shocks to private sector salaries.

Two potential, indirect financial costs of debt, either real or anticipated, are the time costs of debt service and limited access to noneducation loans during and after law school.[9] In this setting, however, both of these costs are unlikely to vary by lottery outcome. First, a similar amount of annual paperwork is required of all participants regardless of career choice. Second, treatment assignment (conditional on current job) is unlikely to influence access to consumer, business, or housing credit after graduation since credit limits are based on annual debt service rather than outstanding debt, which should be identical for winners and losers in a given job.[10] Annual debt service is identical for all private sector workers, and all public sector workers can legitimately claim equal annual tuition debt payments in mortgage applications.[11] During school, unpaid deferred loans would be classified by credit scorers as inactive installment loans. As such, they should have no effect on credit scores used to determine moderate amounts of consumer credit (and no effect on credit scores more than one year before payments begin). The only exception is access to large installment loans during law school. For larger loans that are repaid over several years, future payments on deferred loans that resume within a year

[8] These predictions are straightforward to derive in a simple dynamic utility-maximization problem in which an individual's lifetime utility is an additive function of discounted lifetime consumption, the discounted lifetime disutility of holding debt in each period (debt burden), and the discounted lifetime utility of the premium she places on public interest work when employed in that sector. The model and predictions are detailed in Field (2006).

[9] There is no reason to anticipate uncertainty about access to education loans for lottery losers, since applicants to NYU were notified of federal loan application results at the same time as admissions decisions.

[10] Mortgage lenders traditionally follow what is known as the 28/36 rule. No more than 28 percent of monthly gross income should be dedicated to a mortgage payment, property taxes, and insurance, and total debt payments should equal no more than 36 percent of gross income.

[11] Note that LRAP recipients generally would be required to obtain a statement from NYU verifying the conditions of loan repayment (and would be advised to do so by any competant broker). While it is true that banks can foresee that an LRAP recipient will become responsible for debt service if she moves into a nonqualifying job, whereas they cannot necessarily see the same thing for lottery winners (who would become equally indebted to NYU), no commercial bank in the United States would take into account a detail as subtle as an individual's job prospects conditional on education, salary, and the current job (i.e., collect sufficient information to calculate the likelihood of an applicant losing or quitting his current job and the expected salary of his next best offer) when calculating credit limits and interest rates. Even if they did, it is not clear that credit limits would differ between two otherwise identical indivduals working in public interest law, one receiving LRAP and one with no observable tuition debt. Unemployment would make both borrowers unable to repay, and the two borrowers would still have the same expected debt service in the event of a pay cut since the LRAP borrower is still eligible for income-contingent aid. The two borrowers differ only in the observable debt payments they would face if they move to the private sector, in which case both would be well above the credit limit for which they are eliegible as public sector workers.

typically would be taken into account as a future expense, increasing debt to income ratios and thereby reducing loan eligibility. Because this future expense is observable for losers but not winners, winners are likely to have higher credit limits for large purchases, such as a home, while they are in school.

While this could potentially alter the real value of tuition subsidies relative to LRAP, two empirical facts suggest that limited access to large loans during school is rarely a binding constraint for NYU law students and therefore unlikely to be an important factor influencing students' valuations of the two financial aid packages. First, there is no observable difference between lottery winners and losers in noneducational debt levels at graduation (Table 7). Second, there is no instance of a change in noneducational debt from the start of to the end of law school for more than $6,000 among students in our sample.[12]

For these reasons, the most relevant source of potential debt burden in this experiment is arguably the psychic or social cost of debt. In particular, social norms regarding indebtedness may generate anxiety or impose social costs on borrowers. This may be lower among recipients of tuition subsidies since their expected debt is easier to hide from others and potentially less salient to themselves. For instance, the negative reactions of parents or spouses, who may share responsibility for debt incurred later on, may be a strong deterrent to entry into debt.

B. *Framing Effects*

A second possible reason for the sensitivity of career choices to income-contingent tuition subsidies versus loan repayment is the difference between the two packages in the way choices over debt are framed at career entry and exit. In particular, under tuition subsidies, lottery winners decide whether to remain out of debt versus enter into debt at the end of law school, whereas under loan repayment losers are forced to sign a loan contract at the start of law school and then decide whether to remain in debt versus having existing debt forgiven when leaving law school. Although participants should anticipate the same amount of debt associated with each career path regardless of lottery outcome, this difference in how the two programs are framed gives rise to a number of possible psychological phenomena that have the potential to influence decisions.

First, at the end of law school, status quo debt levels differ between the two treatment groups because students must enter into debt at the start of law school under loan repayment, which could generate a shift in reference points over debt at the point of making career choices as in the model of Amos Tversky and Daniel Kahneman (1991).[13] If individuals are loss averse with respect to debt, and reference points over debt are determined by existing levels of debt, entering a debt contract will have a bigger effect on behavior than exiting a contract of the same magnitude.[14] Since lottery losers are already in debt at graduation, they may be more willing to forego

[12] Author's calculations using exit survey data.

[13] This framework generally implies a role of mental accounting in order for individuals to treat debt at entry and exit as separate gains and losses, as discussed in Thaler (1985, 1992).

[14] Although winners have less debt at graduation, loss aversion has the same implications for behavior as long as there is diminishing sensitivity to losses, as is traditionally assumed in prospect theory. See Botond Köszegi

debt relief than lottery winners are willing to incur the same amount of debt by taking a private sector job. A great deal of work in behavioral economics deals with the influence of status quo bias both theoretically and empirically, and previous studies have documented endowment effects and the importance of default options in high-stakes settings (William Samuelson and Richard Zeckhauser 1988; Kahneman, Jack L. Knetsch, and Thaler 1991; James Choi et al. 2004).

An analogous framing effect relates to perceived default options of choices over public interest work at the end of law school. Living with the label "public service scholarship winner" throughout law school may lead individuals to reflect upon this career possibility more readily or treat it as the default option, which could also encourage them to enter the public interest sector. Since there is no binding career default in the context of this experiment, status quo bias here must reflect either a perceived default option or responses to others' expectations that are influenced by the label. For instance, winning a PSS might increase friends' and family's awareness of a student's intentions to enter public interest law, and desire to meet others' expectations could deter her from changing her mind.

Although default options have no potential to vary by lottery outcome prior to school entrance, and thereby directly influence entry decisions, if individuals are sophisticated about the influence of reference points, admitted students concerned about losing sight of their preferences during school may favor tuition subsidies as a form of insurance against changing preferences or "commitment to type." As a result, they would be more likely to enter law school if they receive a subsidy, and, conditional on entry, would therefore update their prior on the likelihood of entering the public sector, which could influence career choices during school. Alternatively, at the point of deciding whether to enter law school, the label may serve to remind lottery winners of this option and, in turn, lead them to view law school more favorably as an entryway into this career.

Another possible psychological response to differences in frames stems from differences between the two packages in the timing of perceived entry into debt relative to career choices, which could influence choices in this context if there is heightened aversion to debt at the point of entering into a debt contract. Simply being confronted with a contract could increase awareness of the debt all subjects will face in the private sector and raise the momentary perceived cost of debt. In this case, the fact that lottery losers must enter into debt at the onset of school would potentially dissuade them from enrolling. Analogously, since tuition subsidies involve entering into debt at school exit, heightened awareness of debt associated with private sector employment would have a similar effect on the career decision of lottery winners, disproportionately encouraging them to enter the public sector.

Through any of these channels, differences by experimental group in how debt is framed could simultaneously discourage losers from entering law school and winners from entering the private sector. Since both implications are identical to those of psychic debt burden, our experimental results cannot distinguish between any of these potential psychological pathways.

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and Matthew Rabin (2005) for a formal treatment of loss aversion in dynamic settings with endogenous reference points.

C. *Other Potential Differences in the Value of the Programs*

Differential responses to the two forms of aid can be attributed to either the debt aversion or psychological stories presented above only in the absence of differences in the real value of the two packages. Here, it is important to underscore the fact that lottery outcome had no direct influence on students' peer interactions or activities during school. Neither loan repayment nor tuition subsidies involved any extra curricular activities, organized meetings, or special coursework. Both were simply private transactions with the NYU financial aid office. Furthermore, because neither program was merit based, we should expect no difference in job opportunities between the two programs. The possibility that PSS increased students' ability to signal interest in public service work is explored empirically in section 5.3 of the paper.

The only difference in the financial value of the two aid packages is the potential risk associated with the nonbinding nature of the LRAP agreement. In particular, neither the existence of LRAP nor its benefits formula was guaranteed to remain constant for ten years after law school applicants entered the job market. Thus, uncertainty regarding program continuation or changes in benefit amount, eligibility requirements, or tax treatment of loan payments could cause applicants to favor the tuition subsidy and students with debt to refocus their career towards the private sector.

Uncertainty about future support is unlikely in this setting for two reasons. First, 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. So, discontinuation or reduction in LRAP benefits should be deemed highly unlikely by incoming students. Historically, benefit amounts from NYU's LRAP have increased monotonically (NAPIL 2000). Second, the school's commitment to providing loan assistance was emphasized particularly to students in IFAS classes, who were made aware of the fact that they were part of a ten-year experimental study. In particular, a document distributed to admitted students explained that *"NYU is setting aside specific funds to finance this program. Given current projections, it is confident that these funds will be sufficient to meet the needs of all its graduates who enter the Law School in 1995, 1996, 1997, and 1998* (New York University School of Law 1995)."

Even if the real value of the packages is equal, it is always possible that individuals make systematic errors in equating income-contingent debt and repayment streams across the packages. Unfortunately, such a possibility cannot be completely ruled out without survey data on perceptions, which were not collected from IFAS participants. However, results from a laboratory experiment I conducted indicate that psychological responses to income-contingent loans versus tuition subsidies are unlikely to reflect calculation errors.[15] In particular, 120 students in an intermediate microeconomics class were asked to make hypothetical career choices under each of the two types of aid given varying levels of interest in public-sector work. Beforehand, half of the students received a detailed explanation of the difference

[15] Thanks to David Card for the idea and opportunity to implement this laboratory experiment in his undergraduate class. An overview of results is presented in Field (2005).

Table 2—Lottery Weights

Year	Lottery	Probability of winning (1)	Number from control (2)	Number from treatment (3)	Weight assigned to control only (4)
1998	PSS3	0.250	48	16	1
	PSS2	0.467	8	7	2.625
	PSS1	0.571	3	4	4
1999	PSS3	0.289	27	11	1.222
	PSS2	0.400	9	6	2
2000	PSS3	0.346	34	18	1.588
	PSS2	0.286	5	2	1.2
2001	PSS3	0.380	31	19	1.839
	PSS2	0.455	12	10	2.5

Notes: Column 1 reports the fraction of lottery applicants who won in each year, directly calculated from columns 2 and 3. Column 4 reports the weight assigned to the control group in order to balance the composition of treatment and control group members across years in the aggregate sample. PSS3 refers to the three-year public service scholarship for which students apply prior to the first year of law school. PSS2 refers to the two-year public service scholarship. PSS1 refers to the one-year public service scholarship.

between the programs in which they were told explicitly that repayment streams were equivalent under the two forms of aid. Not only did hypothetical career decisions differ systematically according to lottery outcome in the same manner as the real world experimental results, but they were identical across experimental arms, indicating that choices were not driven by miscalculation.

III. Construction of Control Group

Among the pool of 270 IFAS study participants in the classes of 1998–2001, 141 tuition subsidies were assigned randomly and 129 participants received the default option of LR. The distribution of applicant winners and losers by lottery type is shown in the Appendix. Sample weights were constructed to account for differences in the probabilities of winning according to class year and type of lottery to which the student first applied—PSS3, PSS2, or PSS1. For instance, late applicants had a higher probability of winning the lottery, and therefore the unweighted treatment group is composed of a higher percentage of PSS2 and PSS1 applicants. To account for these composition effects, sample weights were assigned to control subjects to equate the distribution of lottery types across treatment and controls. Table 2 gives the precise sampling weights for each applicant type.

Constructing unbiased experimental groups was complicated by the fact that losers could reapply for a tuition subsidy in their second and third year of law school. To address this complication, only an individual's lottery outcome the first time she applied was taken into account (whether in the first, second, or third year of school). The treatment group then consists of all first-time applicant winners ($N = 93$), and the control group is made up of all those not awarded a scholarship the first time they apply ($N = 177$). Characterizing lottery participants by their initial outcome amounts to an intent-to-treat (ITT) analysis. The ITT estimates provide a lower bound on the treatment effect. Due to the possibility of reapplying, which 51 percent of lottery

TABLE 3—ENROLLMENT RATES BY LOTTERY OUTCOME
(*PSS3 Participants*)

Class	*N* (1)	Winners (2)	Losers (3)	$\lvert p_{\Delta} \rvert$ (4)
1998	168	38.46	37.92	0.95
1999	112	42.31	31.52	0.31
2000	187	43.48	23.33	0.01
2001	141	35.56	35.53	0.99
Total		39.64	32.29	0.09

Notes: Columns 2 and 3 report the fraction of PSS3 lottery applicants who enrolled at NYU Law School. In 1999 and 2000, lottery assignment was announced to participants prior to enrolling, while in 1998 and 2001, lottery assignment was announced after the bulk of enrollment decisions had been made.

Source: NYU Law School Admissions Office.

losers did, the control group includes 48 lottery losers who received scholarships at a later stage. Since these "persistent" individuals are not a random sample of original losers, excluding them from the control group would generate biased estimates of program effect.

IV. Experimental Results

A. *Matriculation*

The empirical analysis begins by exploring the effect of lottery outcome on enrollment. The matriculation analysis is confined necessarily to the two classes of participants—1999 and 2000—for which both the opportunity to enroll in the lottery and the lottery results were announced early enough to influence school application and entry decisions. In these two years only, the IFAS study was announced prior to application deadlines and the lottery results announced well before acceptance deadlines. In contrast, for the class of 2001, lottery outcomes were announced after the enrollment deadline, and for the class of 1998, winners were announced shortly before the enrollment deadline, after the majority of school entrance decisions had been made. In 2001, the decision was made to announce lottery winners after matriculation as a means of reducing take-up of subsidies for financial reasons. In 1998, organizational constraints prevented early announcement of lottery results. Another factor reducing the influence of lottery outcome on enrollment for the class of 1998 is the fact that tuition subsidies did not influence the decision to apply to NYU in the first year of the program since the first public announcement about the program appeared after law school applications were due. In general, individuals on the extensive margin of applying should all be "conditional matriculators" in the sense of being willing to attend only if the tuition subsidy becomes available, so announcing the lottery after the application stage should unambiguously reduce selection.[16]

[16] In a fully rational model, individuals who would only apply to NYU given the possibility of a tuition subsidy would only attend if that subsidy became available, although the possibility of winning a PSS1 or PSS2 makes the decision slightly more complicated. While it is feasible that winning the lottery could have also encouraged an

TABLE 4—SAMPLE CHARACTERISTICS OF PSS3 APPLICANTS

	Classes of 1998 and 2001 only (no selection)				Classes of 1999 and 2000 only (potential selection at enrollment)			
	N	Control (1)	Treatment (2)	$\lvert t_{\Delta}\rvert$ (3)	*N*	Control (4)	Treatment (5)	$\lvert t_{\Delta}\rvert$ (6)
Application data								
Female	79/35	0.61	0.63	0.16	61/29	0.57	0.55	0.18
Age	79/35	31.55	31.17	0.43	61/29	31.31	31.72	0.49
Minority (African American or Hispanic)	79/35	0.11	0.06	0.89	61/29	0.08	0.00	2.28
Foreign citizen	79/35	0.02	0.00	1.00	61/29	0.00	0.03	1.01
LSAT	79/35	167.76	167.94	0.19	61/29	168.20	169.17	0.93
Undergraduate GPA	79/35	3.64	3.63	0.06	61/29	3.62	3.53	0.72
Rank undergraduate institution	79/35	4.11	4.03	0.53	61/29	4.09	4.07	0.13
Undergraduate school public	79/35	0.27	0.29	0.16	61/29	0.34	0.41	0.70
Financial aid data								
Parents' net worth	65/30	212,593	179,581	0.47	52/25	220,110	133,432	1.38
Parents' net income	65/30	65,939	84,083	1.31	52/25	44,790	52,992	0.42
Entrance survey data								
Nonlaw school educational debt	73/33	10,323	8,452	0.53	58/26	6442	6,330	0.10
Married	73/33	0.18	0.09	1.22	58/26	0.14	0.19	0.49
Years of PI experience	73/33	1.02	0.94	0.22	58/26	1.08	1.59	0.81
Preferences at start of law school								
Importance of salary	73/33	1.12	0.70	1.41	58/26	1.09	1.01	0.19
Importance of benefits	73/33	1.35	1.24	0.30	58/26	1.38	0.99	0.99
Importance of social contribution	73/33	4.29	4.00	1.19	58/26	3.52	4.10	1.31
Importance of practical experience	73/33	1.17	1.07	0.27	58/26	2.13	0.80	3.86
Importance of reasonable hours	73/33	1.49	1.01	1.57	58/26	0.92	1.59	1.54
Plans at start of law school								
Planned time in private law firm	73/33	1.21	1.00	0.38	58/26	1.21	0.46	1.83
Planned time as a corporate lawyer	73/33	0.11	0.23	0.47	58/26	0.35	0.00	2.11
Planned time in nonprofit law	73/33	5.20	6.31	1.49	58/26	5.85	6.12	0.29
Plans to be a clerk	73/33	0.63	0.76	0.76	58/26	0.86	0.93	0.46

Notes: Mean values are reported from variables in three sources: application data, entrance survey data, and exit survey data. The sample is restricted to participants in the PSS3 lottery. Application data are available from everyone in the study. Missing observations in other data sources are due to failure to participate in the entrance or exit survey or failure to apply for financial aid through NYU. Rank of undergraduate institution comes from the 2000 *US News and World Report* ranking of all undergraduate institutions in the United States, where one is the highest ranking school. Educational debt and parents' net worth and net income are reported in nominal values. Years of public interest experience is the number of years a respondent worked in any public interest setting prior to law school. Preferences at the start of law school come from an entrance survey in which respondents were asked to rank 15 job characteristics in order of importance. Because some students rank as few as 10 characteristics, only the top characteristics were assigned values from 10 to 1 (10 being the most important), and any characteristic that was unranked or outside of the top-10 was assigned a value of zero. Planned time in public interest work comes from respondents' reports of desired amount of time spent in 20 different job settings over the first 10 years out of law school. Since some students reported more than 10 years of job experience, total amount of time reported for all public interest jobs was divided by total amount of time reported in all settings. Plan to do a clerkship is a binary variable indicating any plans irrespective of amount of clerk time planned.

Enrollment data from the NYU Law School admissions office indicate that lottery outcome influenced matriculation as well as application decisions. As seen in Table 3, enrollment rates by lottery outcome and class reveal substantially lower propensity to enroll among lottery losers relative to winners in the classes of 1999

unconditional applicant to attend NYU, the data from 1998 do not reflect this, implying that conditional applicants are the *only* conditional matriculators. By extension, large differences in matriculation rates by lottery outcome in 1999 and 2000 suggest that knowledge of IFAS encouraged applications to NYU.

and 2000. In 1999, lottery winners were 30 percent more likely to attend NYU, while in 2000 lottery winners enrolled at NYU at twice the rate of lottery losers. The numbers suggest that 18.5 percent of lottery winners in the classes of 1999 and 2000 would not have attended NYU had they lost the lottery. In contrast, matriculation rates are identical across experimental groups in the classes of 1998 and 2001, indicating that enrollment was not influenced by lottery outcome as expected.

If the subsidies are leading individuals to switch from another law school to NYU, this is a relatively minor effect except from the school's perspective. In contrast, if subsidies encourage individuals to attend *any* law school, it is a small intervention with a large consequence on human capital accumulation. Unfortunately, it is not possible to distinguish between these stories with the available data. However, while we cannot safely conclude that switching from income-contingent loan repayment to income-contingent tuition subsidies increases the supply of lawyers, it is unambiguous that tuition subsidies increased the supply of NYU law graduates.

Differences in observable characteristics according to lottery outcomes among those who matriculated provide information on the type of individuals on the extensive margin of attendance. Because sample selection is presumed to occur only among incoming students, Table 4 compares characteristics before law school among first-year lottery applicants only. The first three columns restrict the sample to incoming students in the classes of 1999 and 2000. Among this subsample, any statistically significant differences in mean characteristics among matriculating winners and losers can be assumed, for incentive reasons, to reflect a greater propensity to enroll among winners. The second three columns compare winners and losers in the incoming classes of 1998 and 2000, as a basic check on random assignment, and confirm the validity of the comparison groups.

One notable difference between lottery winners and losers in the selected sample is the amount of time students plan to spend working in private sector law or for a private corporation. In the entry survey, lottery losers report planning to spend nearly twice the amount of career time in a private law firm relative to lottery winners. Unless this difference reflects immediate changes in plans after learning about lottery outcome, the pattern is consistent with the prediction that winning the lottery should have a disproportionate influence on matriculation decisions of students who plan to work in public service. Other than career plans, the only detectable differences in observables are a lower fraction of minority students and lower average value placed on "practical experience" relative to other job characteristics such as salary, benefits, and social contribution. This is hard to link to student type. One interpretation consistent with higher selection into NYU among the treated is that students encouraged to enroll because of the lottery have less ambitious career trajectories, and therefore greater fear of debt as well as less desire to accumulate career experiences valuable on the job market. Although it is striking that no minority students in the treatment arms in 1999 and 2000 enrolled at NYU, the comparison could easily reflect random year-to-year variation in minority applications given the extremely small number of minorities in any class.

The matriculation result alone provides our first piece of evidence of nonstandard responses to debt. An immediate policy implication of the enrollment findings is that

Table 5—Job Placement Two Years after Graduation

	Control (1)	Treatment (2)	Δ (3)	$\lvert t_{\Delta} \rvert$ (4)
Subsample without selection; Late applicants and PSS3 applicants in classes of 1998 and 2001				
In public interest law after two years	47.37	68.57	21.20	2.14
Took clerkship after graduation	24.67	24.99	0.32	0.05
N	116	64		
Subsample with selection; PSS3 applicants in classes of 1999 and 2000 only				
In public interest law after two years	38.34	58.62	20.28	1.81
Took clerkship after graduation	20.22	41.38	21.16	1.99
N	61	29		
Late study enlisters in all classes (PSS2 or PSS1)				
In public interest law after two years	51.35	55.17	3.82	0.30
Took clerkship after graduation	21.62	31.03	9.41	0.86
N	37	29		

Notes: Table shows the weighted fraction of students in the control (LRAP) and treatment (PSS) groups who are working in public interest law two years after graduating and the fraction who take one- to two-year clerkships immediately after graduation (not mutually exclusive). Late study enlisters are students who signed up for the PSS lottery for the first time in either the first or second year of law school.

schools interested in increasing yield, particularly among students with ex-ante relatively high public interest commitment, will benefit from offering tuition subsidies in addition to loan repayment.

B. *Job Placement*

The empirical analysis of job sector outcomes explores mean differences in career outcomes between the control and treatment groups. The main sample of interest is the nonselected sample composed of the classes of 1998 and 2001. By eliminating the two classes of participants affected by differential matriculation, the experimental analysis among this subsample isolates the average treatment effect of tuition waivers on job choice. However, in order to make inferences about the nature of selection, throughout the analysis I present separate estimates from the nonselected and selected samples (1999 and 2000). The results are also broken down by timing of initial lottery application since those who chose to participate late are likely to be systematically different than those who enrolled from the onset of law school in terms of the degree to which they value subsidies over loan repayment.

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. Qualifying public interest law jobs are classified according to the LRAP criteria. Of those who qualify, 8 percent are working for the public defender's office, 42 percent are working in legal aid organizations, 14 percent are working in district attorney's offices, 8 percent are in other government agencies, and 28 percent work for other nonprofit agencies (academic positions do not qualify as legal services). Job placement is complicated by the fact that graduates have the option of accepting a one- to two-year position as a law clerk immediately after graduation. Hence, to approximate long-range career paths, I look at job placement two years after law school, by which

time all but two clerkship positions have ended. Information on the pattern of post-clerkship employment is available for all but three study participants from follow-up surveys mailed to graduates two years out of school and by directly contacting students who failed to return the full surveys. The three observations without follow-up data are assumed, for incentive reasons, not to be working in qualifying jobs. Overall, 55.6 percent of clerks transition to public interest jobs by the time of the follow-up survey, and the rate is almost identical by lottery outcome. In addition to those who took clerkships, this outcome takes into account any job changes made within two years of graduating. Almost one-third of students switch jobs within two years of graduating, and 8 percent of them move between private and public interest jobs.

Table 5 reports job sector placement two years after graduation. Among first-year participants in the classes of 1998 and 2001, members of the treatment group are over one-third (36 percent) more likely to enter public interest law two years after graduating, and the difference is statistically significant at the 5 percent level. 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. These results indicate that, despite the equivalent net present value of these two programs, income-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 soon after law school.

The placement differential in the classes of 1999 and 2000 is almost identical and significant at the 10 percent level, suggesting a minimum role of selection in explaining job placement responses to debt. Furthermore, treatment students in the selected sample are 12.2 percentage points (52 percent) more likely to take a one-to-two year clerkship after leaving law school, and the difference is significant. Since there is no such difference observed among subjects in the classes of 1998 and 2001, sample selection appears to be entirely responsible for the difference in clerkships. Given the competitive nature of clerkships, the fact that the high placement differential is only found among conditional matriculators provides evidence that students who are on the margin of attending NYU when offered tuition subsidies are relatively high performers.

Looking separately at the small number of applicants (66) who enrolled in the study during their second or third year of law school reveals that the difference in public interest law placement is concentrated among applicants for three-year tuition lotteries among whom the debt difference is the largest. While we observe a 20.8 percentage point differential in the rate of public interest law between three-year lottery winners and losers, the public interest law differential among late applicants is only 6.1 percentage points and insignificant. While this may reflect the fact that late applicants only received one or two years of funding, it is also consistent with a higher valuation of subsidies relative to LRAP among those who applied early.

C. *Job Market Signaling*

The last section explores the possibility that employment prospects of treatment and control group members differed because of differences in signaling ability.

Despite the fact that tuition subsidies were distributed by lottery—and hence provide no information on differences between winners and losers in ability or interest—it is possible that public interest employers perceive income-contingent scholarships as valuable job market signals of quality or commitment to public interest work. In other words, since lottery losers are unable to indicate to employers that they applied for a PSS, winning the lottery could conceivably alter job opportunities in the public sector.

Table 6—OLS Regression of Mean Starting Salary

	Starting salary of first job	
	(1)	(2)
PSS [treatment]	6,457.80	2,085.40
	(4,359.42)	(3,176.97)
Private sector	75,451.39	75,560.89
	(6,811.90)**	(7,161.38)**
(PSS [treatment]) × Private sector	5,744.81	7,186.86
	(9,177.49)	(9,052.60)
Cumulative GPA		30,608.11
		(13,097.61)*
N	153	144

Notes: Table reports coefficients from an OLS regression of annual starting salary of the first nonclerk job after graduation on a dummy indicator of treatment assignment, an indicator of whether the job is in the private sector, and their interaction. Standard errors in parentheses. Cumulative GPA is averaged across all semesters of available data.

** denotes significance of correlation coefficient at 1 percent level.

* denotes significance at 5 percent level.

Two pieces of evidence suggest that this is unlikely to be an important concern. First, an informal survey of 61 resumes and biographies from lottery winners, which are available online, revealed only one example of a lottery winner listing the PSS on a law resume and one instance of a lottery winner listing the PSS in a campaign advertisement for local office. Second, employers in the field of law keep careful record of merit-based fellowships available to law graduates, as is evidenced by annual compendiums such as the "BCG Attorney Search Guide to America's Top 50 Law Schools," which provides law firms with current information on law schools' grading systems and awards.[17] Hence, law employers are unlikely to mistake the PSS as a sign of merit. While a PSS has potential to signal interest in public service, job candidates have numerous ways to convey this interest through law school activities and summer jobs.

A useful way to test for the signaling effect of scholarships would be to look at differences in callback rates and salary offers of public interest employers according to the experimental group. If present, this asymmetry should be reflected in higher average wage offers for scholarship holders. Unfortunately, job offer data are not available from the IFAS. A much cruder indicator of any significant job market advantage for PSS subjects is found by looking for relative wage differences between control and treatment groups in public interest versus private jobs that are consistent with a premium on scholarship participation. To account for the fact that many students take clerkships immediately after law school, positions for which salaries are largely predetermined, I regress the first post-clerk salary of late applicants and all participants in the classes of 1998 and 2001 on treatment assignment, job sector, and their interaction.

[17] See http://www.bcgsearch.com/ for a description of these guides and their history.

TABLE 7—LAW SCHOOL EXPERIENCES

	N	Control (1)	Treatment (2)	$\Delta_{(treat\text{-}cont)}$ (3)	$\lvert t_{\Delta} \rvert$ (4)
GPA year 1	105/53	3.184	3.193	−0.009	0.00
GPA year 2	108/57	3.382	3.392	−0.010	0.07
GPA year 3	107/57	3.361	3.475	−0.114	1.93
Change in importance of salary	94/51	−0.449	0.503	−0.952	2.52
Change in importance of benefits	94/51	−1.234	−0.192	−1.042	2.72
Change in importance of social contribution	94/51	−1.257	−1.612	0.355	0.62
Public interest job summer 1	116/64	0.696	0.641	0.055	0.87
Public interest job summer 2	116/64	0.437	0.406	0.031	0.37
Change in noneducational debt	94/51	2,015	1,113	902	0.45
Class of 1998 only:					
Journal	59/25	0.594	0.600	−0.006	0.05
Moot court	59/25	0.250	0.240	0.010	0.09
Clinical credits	59/25	10.32	8.120	2.200	1.28

Notes: Mean values reported from variables in three sources: registrar's office (grades, journal, moot court, and clinical credits), entrance survey data, and exit survey data. Sample restricted to late applicants and PSS3 participants in classes of 1998 and 2001 only (unselected sample). GPA in each year is averaged across the fall and spring semesters. Change in preferences is only available for students who participated in both the entrance and exit surveys. In both surveys, students were asked to rank 15 job attributes in order of importance. Since all students ranked at least 10 attributes, only rankings 1–10 were considered and attributes that were unranked or ranked greater than 10 were assigned 0 value. Hence, all attributes are given a value between 0 and 10. Summer work experiences based on reports for all students from the office of career services (OCS). Since students' summer salaries in public interest job are paid by NYU, we assume that any student not in the OCS database did not take a public interest job.

In this baseline wage regression, presented in Table 6, there is no evidence that treatment status is associated with a wage premium in the public sector. Although the point estimate is positive, it is insignificant and falls considerably when controlling for cumulative GPA (column 2). While not definitive, this suggests that financial aid package alone is not an important signal to employers. Clearly, these estimates fail to capture any nonwage job attributes that vary systematically by lottery outcome, so they should be interpreted with caution. Furthermore, they ignore the selection of students into jobs. If tuition subsidies increase public interest job prospects, average wages among those accepting PI jobs may still be equal across experimental groups because only the best lottery losers take public interest jobs. However, in this scenario we would correspondingly expect private sector salaries to be higher on average for lottery winners, which does not appear to be the case.

V. Influence of Program Participation on Law School Experiences

If one accepts the premise that the only difference between experimental groups is the duration of indebtedness and framing of debt, then the above findings suggest that psychological responses to debt can have a large influence on high stakes decisions. However, one remaining alternative interpretation is that differences between experimental groups reflect the psychological impact of winning the lottery on interest in public service work. Although the program involved no special activities, it is possible that the Public Service Scholarship label provided by winning the lottery served to nurture an interest in public interest law. This could also

stem from the simple difference in frames of "winning" as opposed to "losing" the lottery.

A key implication of the alternative story is that tuition subsidies increase students' utility from public interest work. To explore this possibility, the last section examines academic experiences and changes in career preferences during school for evidence of an increase in ranking of characteristics such as social contribution among lottery winners. Note that diverging career preferences are insufficient to conclude that tuition subsidies strengthened preferences for public interest law. Students may make choices during law school with respect to coursework, summer jobs, and extracurricular activities purely in response to debt. However, an absence of differences indicates that winning the lottery does not directly shift students' preferences.

Table 7 presents data on grades and academic activities obtained from the registrar's office and data on career preferences before and after law school obtained from entrance and exit surveys. To facilitate interpretation of differences between experimental groups that may be driven by sample selection, the analysis is restricted to first-year applicants from the classes of 1998 and 2001 and late applicants in all classes. Two interesting facts emerge. First, although academic performance in the first and second year of school is nearly identical across experimental groups, during the third year students who received tuition subsidies achieve a significantly higher grade point average. Since the rate of clerkships is not significantly higher for these students (Table 5), the difference is likely driven by greater competition for the limited supply of prestigious and reasonably paid public interest jobs. Another possibility is that students who take jobs in law firms are more likely to have received a job offer at the end of a summer internship, in which case they have less incentive to maintain a high GPA in their final year of school.

Exit and entrance surveys provide information on changes in students' personal ranking of 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. Only two of 15 job characteristics that students were asked to rank in terms of importance—salary and benefits—were significantly different at the end of law school. Students receiving tuition subsidies increased their ranking of the importance of both forms of compensation relative to students in the control group, and the absolute difference is significantly higher at exit but not entrance. Concern over salary is likely driven by the same factor as concern over grades. A higher fraction of lottery winners plan to enter public interest law, and therefore face more pressure than losers to secure a livable wage.

The fact that other career preferences, including the relative importance of social contribution and prestige, are virtually equivalent between experimental groups both at the start and finish of law school suggests that differences in career choices do not operate through changes in preferences from participating in the program. Student academic activities, including participation in law journals and moot court, number of clinical credits, available only for the class of 1998, show further evidence of consistent preferences and experiences during school. Lottery winners and losers in the class of 1998 are equally likely to participate in law journals and moot court,

and acquire a similar number of clinical credits. More strikingly, both in the first and second year of school students in all classes are equally likely to take summer positions in public interest law despite the large wage gap between private and public internships. These patterns suggest that underlying preferences for social contribution are not influenced by lottery outcome, but instead that observed patterns of career choices reflect responses to debt.

In sum, although lottery winners in the IFAS experiment were less likely to enter the private sector, it was not because the premium they placed on social contribution rose. Rather, choosing a well-paid private sector job appears to be a tradeoff students make in response to the psychological stress associated with securing a livable wage in public interest law.

VI. Conclusions

The fact that income-contingent tuition subsidies are associated with higher rates of public interest law than are financially equivalent loan repayment schemes provides strong evidence of the influence of debt burden on job choice in a real world setting. Given that interest costs of borrowing are equalized across experimental arms and indirect costs of indebtedness are likely to be minimal in this setting, the results suggest that individuals experience significant psycho-social costs of debt or are subject to framing effects strong enough to influence high-stakes decisions about career.

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 before rather than after graduation, a law school can increase its rate of public interest placement. Although retrospective debt relief is currently the overwhelming form of income-contingent financial aid, these results imply that tuition subsidies would be a more efficient allocation of institutional funds for this purpose. This policy question is already relevant in a number of educational settings outside the United States in which college is largely financed with income-contingent loans (Bruce Chapman 2006). A number of countries have changed undergraduate financing systems to income-contingent loans over the past 15 years including Australia, New Zealand, Chile, South Africa, Ethiopia, the United Kingdom, Thailand, and Israel. A number of others are currently debating this transition (Chapman 2006, Nicholas Barr 2004, Barr and Iain Crawford 2005). Meanwhile, in the United States a growing number of professional schools are offering income-contingent loans, and such loans are likely to play an increasing role in college financing based on recent national and international trends (Evelyn Brody 1994).

From a social welfare perspective, debt reduction has the potential to increase schooling investment in job sectors with high social returns. While loan repayment encourages some level of this, results from the IFAS experiment suggest that income-contingent subsidies such as those adopted in the British system would be even more effective in increasing the supply of public interest workers. If other students mirror law school students in their attitudes toward debt, this relatively costless policy difference could have significant impact in raising rates of public interest employment.

Appendix

Table 8—Lottery Outcomes and Reapplication Rates by Lottery Type

	PSS3	PSS2	PSS1	Total
Number of applications[a]	204	131	37	372
First-time applicants	204	59	7	
Second-time applicants		72	9	
Third-time applicants			21	
Number of winners	64	57	20	141
First-time applicants	64	25	4	
Second-time applicants		32	5	
Third-time applicants			11	
Number of losers	140	74	17	231
First-time applicants	140	34	3	
Second-time applicants		40	4	
Third-time applicants			10	
Number of reapplicants[b]	72	30		
First-time applicants	72	9		
Second-time applicants		21		
Treatment (first-time applicants)[c]	64	25	4	93
Control (first-time applicants)[d]	140	34	3	177
Eventual winners	43	5		48
Applied twice	32	5		
Applied three times	11			

[a] Includes total number of matriculating applicants for all four years of study. The higher number of applications (372) than applicants (270) reflects multiple applications from single individuals. PSS3 refers to the three-year public service scholarship for which students apply prior to the first year of law school. PSS2 refers to the two-year public service scholarship, and PSS1 refers to the one-year public service scholarship.

[b] Number of lottery losers that reapplied the following year.

[c] Treatment group in experimental estimates.

[d] Control group in experimental estimates, includes 48 eventual winners who applied more than once and won on a second or third try.

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