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by

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A Regression-Discontinuity Evaluation of the Effect of Financial Aid Offers on College Enrollment

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In evaluating the effect of financial aid offers on college enrollment decisions the former cannot be considered exogenous with respect to the latter. This paper shows how idiosyncratic features of an east coast college's financial aid offer process can be exploited to solve this endogeneity problem and to obtain credible effect estimates. More precisely, a key component in the college's aid offer decision is a simple formula used to rank students into a few groups on the basis of several continuous measures of academic ability. As a result, its aid assignment rule has features (discontinuities) which characterize the assignment rule of a powerful, but largely overlooked quasi-experimental design: the Regression-Discontinuity design originally introduced by Campbell and Stanley (1963). It is shown that credible estimates of the financial aid effect can be obtained without having to rely, as in usual selection bias correction methods, on arbitrary exclusion restrictions, and functional form and distributional assumptions on errors. Moreover, it is shown that, without making the connection to the design explicit, several recent studies such as Angrist and Krueger (1991), Imbens and van der Klaauw (1995) and Angrist and Lavy (1996) rely heavily on the Regression-Discontinuity design in identifying treatment effects. More generally, this study provides an illustration of how more detailed knowledge of the selection mechanism can aid in obtaining reliable program effect estimates.

Keywords: Regression-Discontinuity design, program evaluation, selection bias, instrumental variables, financial aid, college enrollment.

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I. Introduction

To influence the size, quality and composition of an incoming freshman class a college has control over two principal instruments: the decision to offer admission to a prospective student who applies for admission and the decision how much financial aid to offer to that student. While the decision to deny admission is typically used to restrict entry to only those with adequate ability and promise, the financial aid decision is used to encourage a subset of those admitted, especially those with the greatest academic ability, to enroll. The specific admissions criteria and financial aid allocation mechanism chosen will reflect the college's various goals, such as maintaining or increasing the college's total enrollment, attracting higher quality students and maintaining or improving ethnic diversity, while keeping the total costs of achieving these goals within an acceptable financial aid budget[2].

In determining the total amount of fincancial aid to offer as well as the size of individual financial aid offers, the anticipated effect of an offer on a student's decision to enroll in the college plays a crucial role[3]. While a college's past records of financial aid offers and student enrollment decisions provide a valuable source of data and experience in this regard, the actual evaluation of the effect of financial aid offers on student enrollment decisions remains a complicated matter.

A student's decision whether or not to enroll in a particular college, is influenced by a number of different factors, many of which are unobserved by college administrators. The most important piece of information, that is typically missing, is information on a student's alternative options. These options may include admissions and financial aid offers from other colleges and the option to join the labor force directly after high school. While student application and financial aid request forms often provide some information concerning other

[2]For examples of theoretical and empirical analyses of financial aid offer decisions, see Barnes and Neufeld (1980), Miller (1981), Venti (1983) and Ehrenberg and Sherman (1984). Venti, using national data from the NLS-72, found merit and affirmative action-based aid to constitute a larger part of discretionary college aid than need-based aid.

[3]Depending on the magnitude of the aid effect and on a college's objectives, it may, for example, be optimal for a college to offer more aid to a weaker student if the probability of getting a better student is very low and aid allocation is constrained by a fixed budget.

colleges and universities the student has applied to, it is not known whether these colleges will admit the student and what their financial aid offers will be[4]. In addition, typically little or nothing is known about any employment options each applicant may have. This lack of information does not only pertain to new applicants, but also to applicants in previous years. Most colleges do not collect information about the destinations of applicants who chose not to enroll. In particular, it is not known whether they decided to go to college at all, and if so, in which other colleges these students chose to enroll.

This lack of information about alternative options makes it very difficult to distinguish the effect of a college's own offers of financial aid from the (unobserved) offers of other colleges as well as possible alternative opportunities in the labor market. In general we could expect each college's aid offer to depend (at least in part) on the same student characteristics, such as available measures of academic ability, ethnicity and family income, typically reported on federal and college aid application forms. Missing information about alternative opportunities is therefore likely to cause an omitted variable bias in estimating the effect of financial aid on enrollment.

The evaluation problem is further complicated by the fact that financial aid decisions can rarely be described completely in terms of measured student characteristics. To some extent the financial aid decision is a subjective one, depending on an admission officer's assessment of the student's complete 'package', of which certain aspects, such as statements of purpose, extracurricular activities, recommendation letters, are typically not kept in a computer data base. It is possible that, even if the student's outside opportunities were known, some of these unobserved aspects may be correlated with the remaining error term in an enrollment equation. For example, in choosing between two different colleges, students showing great athletic talent may have a preference for the college with the highest reputation in sports, even if this talent was equally rewarded in the financial aid offers from the two colleges. The resulting dissimilarity in characteristics, observed by the financial aid officer but not by the

[4]It is in fact not legal for colleges to share this information.

econometrician, between individuals receiving different amounts of financial aid leads to a second omitted variable or selection bias problem.

Because of both omitted variable problems, when evaluating the effect of financial aid on enrollment the former can not be considered exogenous with respect to the enrollment decision. For example, when comparing the enrollment rates of two groups of applicants who differ in the amount of financial aid they were offered but are equal in all measured characteristics, it is quite possible that the enrollment rate of the group who received more was actually lower. This would be the case, if those who received more, had unmeasured (but observed by college financial aid officers) characteristics, such as special awards, recommendation letters and extracurricular activities, which made them likely to have received similar or possibly better aid offers from other, perhaps more attractive colleges.

In this paper I analyze the effect of financial aid offers on the enrollment decisions of a large sample of individuals admitted to an east coast college, refered to as College X, during the period from 1989 to 1993. This data base has the usual shortcomings discussed above, in that it lacks important information on each student's choice alternatives as well as some information (such as statements of purpose, reference letters and characteristics of the financial aid officer) that influenced the financial aid decision, but that was not included in the data base. I will show, however, that it is possible to exploit idiosyncratic features of the financial aid offer decision process to obtain credible estimates of the effect of fincancial aid offers on enrollment. More precisely, a key component of the aid allocation decision is a simple formula used to rank students into a few groups on the basis of several continuous measures of academic ability. As a result, the assignment rule has features that make it similar to that of a powerful, but relatively ignored[5] quasi-experimental design: the Regression-Discontinuity design originally introduced by Campbell and Stanley (1963). It is shown that credible estimates of the financial aid effect can be obtained without having to rely, as in usual selection bias correction approaches (e.g. Heckman, 1979), on (often) arbitrary exclusion

[5] Given the lack of attention it has received in applied economic studies, the design was only briefly mentioned in Meyer (1995)'s excellent survey of the different evaluation approaches used by economists.

restrictions and functional form and distributional assumptions on errors[6].

This study provides an illustration of how knowledge of (aspects of) the selection mechanism can aid in obtaining reliable program effect estimates. Moreover, it is argued that similar features, which characterize the so-called (fuzzy) Regression-Discontinuity design, should be relatively easy to incorporate and are already likely to be found in the assignment or selection rules of many other non-experimental program designs. In fact, it will be shown that, without making explicit the connection to the design, recent studies by Angrist and Krueger (1991), Imbens and van der Klaauw (1995), Black (1996) and Angrist and Lavy (1996), all rely heavily on the Regression-Discontinuity design in identifying treatment effects.

In the next section I discuss the standard program evaluation problem and describe the Regression-Discontinuity design. Section 3 discusses the data set and the particular features of the financial aid allocation process which will be exploited to obtain estimates of the effect of financial aid on enrollment. Section 4 provides a simple model of student enrollment decisions. Estimates and results from a sensitivity analysis are presented in section 5 and section 6 provides a conclusion.

II. The Standard Evaluation Problem and Regression-Discontinuity Design

Consider first the standard problem of evaluating the effect of treatment or participation in a program on a particular outcome measure. For each individual i, let Y_i represent the outcome measure and T_i the treatment indicator, equal to one if treatment was received and zero otherwise, e.g., where Y_i is the acceptance decision and T_i is the decision to award financial aid. Let $Y_i(1)$ be the outcome given treatment and $Y_i(0)$ the outcome without treatment. Only one of these is observed, i.e. what we observe is $Y_i = T_i Y_i(1) + (1 - T_i) Y_i(0)$. Further, assume that Y_i is related to a set of observed characteristics, represented by the

[6] For a survey of earlier studies of the effect of financial aid on college enrollment, see Leslie and Brinkman (1988). Most of the existing studies have ignored the likely endogeneity of financial aid offers in their analyses, while others have relied on conventional parametric selection bias correction procedures.

vector X_i, such that $E[Y_i(0)|X_i] = X_i'\beta$ and $E[Y_i(1)|X_i] = X_i'\beta + \alpha$, where α represents the treatment effect which is assumed to be constant across individuals. In terms of a regression equation we have

$$Y_i = X_i'\beta + \alpha T_i + u_i \tag{1}$$

where the error term u is assumed to be uncorrelated with X. Random assignment to treatment conditional on X would imply T to be independent of u given X so that T can be considered exogenous in the regression of Y on T and X[7]. OLS estimation of this regression will provide an unbiased estimate of the program effect. Even though desirable, true random assignment is in practice rare as randomization is often considered unethical and difficult to adhere to at each stage of program implementation (for a discussion of such implementation problems leading to evaluation biases, see Heckman and Smith, 1995).

When assignment or selection into the program or treatment is nonrandom or nonignorable, selection bias in the estimation of α can arise because of a dependence between T and u. In this case $E[u|T, X] \neq 0$ and the endogeneity of T will generally lead to an inconsistent OLS estimate of the program effect α. This dependence between T and u arises when the treatment status of an individual is related to some characteristic, not included in X, that itself is related to the program outcome. Depending on whether such a characteristic is observable or unobservable, the cause of this endogeneity has been referred to as 'selection on observables' or 'selection on unobservables' (Heckman and Robb, 1985). This resulting dependence between T and u is then incorrectly attributed to the causal effect of the program treatment on the outcome.

To solve the selection bias problem, additional knowledge or assumptions about the selection or assignment rule is required. Commonly, assumptions are made which take the form of exclusion restrictions (variables which are assumed to influence treatment selection but not outcomes), and/or functional form and distributional assumptions about error

[7]Similarly, random assignment unconditional on X would imply that $E[u_i|T_i = 0, X_i] = E[u_i|T_i = 1, X_i]$ (but not necessarily $E[u|T, X] = 0$) in which case T can also be considered exogenous in the regression.

that point are placed in the treatment group ($T_i = 1$) (or vice versa). Thus, assignment occurs through a known and measured *deterministic* decision rule. The variable S itself may well be directly related to the outcome Y. That would automatically make T be related to Y as well, even if the treatment has no causal effect on Y. This is in sharp contrast with pure randomization. While randomization guarantees that treatment and control groups will be as similar as possible in characteristics other than the treatment itself, the RD design does the opposite. The design also violates the 'strong ignorability condition' of Rosenbaum and Rubin (1983), which, in addition to requiring $Y(0)$ and $Y(1)$ to be independent of T conditional on S, requires $0 < Pr(T = 1|S) < 1$ for all S while here $Pr(T = 1|S) \in \{0, 1\}$[10].

To obtain an estimate of the treatment effect, notice that consideration of the sample of individuals within a very small interval around the cutoff point would be very similar to a randomized experiment at the cutoff point (i.e. a tie-breaking experiment)[11]. That is, individuals just below the cutoff score should on average be almost identical to individuals just above the cutoff point[12]. Comparing the average outcomes of the two groups should therefore provide a good estimate of the treatment effect. Of course, in the case of varying treatment effects, our estimate would only be valid for those individuals at the cutoff point. Increasing the interval around the cutoff point would increase the bias of the effect estimate, especially if the assignment variable was itself related to the outcome variable in absence of a program effect. If an assumption is made about the relationship between the outcome and the continuous measure, on the other hand, we can extrapolate from above and below the cutoff point to what a tie-breaking random-assignment experiment would have shown. By using all observations and by relying more on the functional form of the regression it will also be possible to estimate interaction effects between T and S, which is not possible if we only look at people at the cutoff point. This double extrapolation combined with the exploitation of the randomized experiment around the cutoff point is the main idea of

[10]In the terminology of Heckman et al. (1995), there is no region of common support.

[11]To my knowledge, this aspect or interpretation of the design has not been discussed before.

[12]In particular, they will have almost identical values of S and, because T is independent of X conditional on S, their average values of X will also be very similar.

Regression-Discontinuity analysis.

We can analyze the estimability of the treatment effect in the RD design more formally by noting that this design is a special case of selection on observables (that is, where the endogeneity of T is caused by an observed determinant correlated with u), where the assignment rule is deterministic. A dependence between the assignment variable S_i and u_i would generally lead to biased estimates of the equation:

$$Y_i = \beta_0 + \alpha T_i + u_i \tag{2}$$

where

$$\begin{aligned} T_i &= 1 \quad \text{if } S_i \geq \bar{S} \\ T_i &= 0 \quad \text{otherwise} \end{aligned}$$

and $\bar{S}$ is the cutoff value of S. An unbiased estimate of the treatment effect can be obtained by specifying the functional form of the conditional mean or 'control function' $E[u_i|T_i, S_i]$ in the outcome equation

$$E[Y_i|T_i, S_i] = \beta_0 + \alpha T_i + E[u_i|T_i, S_i] \tag{3}$$

(see Heckman and Robb, 1985). In this case of selection on observables $E[u_i|T_i, S_i] = E[u_i|S_i]$ because $T_i = T(S_i) = 1\{S_i \geq \bar{S}\}$. Since S is the only systematic determinant of treatment status T, S will capture any correlation between T and u. As a result, by entering the correct specification of the control function alongside T, the equation can be estimated to yield a consistent estimate of the program effect, as it will free T from the contamination which leads to selection bias.

In the Regression-Discontinuity case where the assignment rule is a deterministic function of S, when adopting a nonparametric specification of the control function $K(S_i) = E[u_i|S_i]$ it will not be possible to identify α in $E[Y_i|S_i, T_i] = \beta_0 + \alpha T_i + K(S_i)$. From the available data on Y_i and S_i for the sample with $T_i = 0$ (the nonparticipants), we can obtain an estimate of $E[Y|S, T = 0] = K(S)$ for all values $S < \bar{S}$, and from the sample with $T_i = 1$ (the participants) we obtain an estimate of $E[Y|S, T = 1] = K(S) + \alpha$ for all values $S \geq \bar{S}$. If we make the additional assumption that $K(S)$ is **continuous at the cutoff point**, however, then α will be identified and can be estimated by the difference between $\lim_{S \uparrow \bar{S}} E[Y|S, T = 1]$ and $\lim_{S \downarrow \bar{S}} E[Y|S, T = 0]$.

Golberger (1972) and Cain (1975) assumed a constant treatment effect and adopted a control function which was linear in S. Figure 1 illustrates the corresponding case of a positive program effect where the program was assigned to those who scored at or above the cutoff point $\bar{S}$. The distance between the two regression lines at the cutoff point (which equals the difference in the intercepts of the two lines) provides an unbiased estimate of the treatment effect if the control function is correctly specified.

More generally, we may wish to allow for varying treatment effects

$$Y_i = \beta_0 + \alpha(S)T_i + u_i \tag{4}$$

in which case $E[Y_i|T_i, S_i] = \beta_0 + \alpha(S)T_i + E[u_i|S_i]$. By adopting a parametric specification of the control function we can estimate the treatment effect function $\alpha(S)$[13]. If we leave the control function unspecified, on the other hand, and only assume it to be continuous at the cutoff point, then we will only be able to identify the treatment effect at the cutoff point. Given an estimate of $E[Y|S, T = 1] = K(S) + \alpha(S)$ for all values $S < \bar{S}$ and of $E[Y|S, T = 1] = K(S)$ for all values greater than $\bar{S}$, the difference $\lim_{S \downarrow \bar{S}} E[Y|S, T = 1] - \lim_{S \uparrow \bar{S}} E[Y|S, T = 0]$ will be an estimate of $\alpha(\bar{S})$. Note that for values greater than $\bar{S}$ it will not be possible to separately identify $K(S)$ and $\alpha(S)$ nonparametrically.

There are three commonly cited weaknesses of the RD design (see Trochim, 1984). First of all, the design requires strict assignment with respect to the cutoff point, which is rare in practice. Of the few programs which fit the 'sharp' RD design, almost all have been compensatory education programs (see the survey by Trochim, 1984)[14]. In practice, social allocation decisions are rarely made on the basis of quantitative scores alone.

[13] Identification clearly becomes more fragile in this case and requires the parametric form to be reasonably smooth so that all parameters of the control function can be estimated of the cases with values of S less than $\bar{S}$.

[14] A notable exception is the study by Berk and Rauma (1985) who exploited the fact that a crime control program inadvertently conformed to a RD design.

A second weakness of the design, as with all regression-based analyses, is that it relies heavily on functional form assumptions about the statistical relation between the outcome and assignment variable. This criticism is however not completely justified. While it is true that an incorrectly specified parametric form for the control function may lead to biased estimates, we saw earlier that the treatment effect (at least for those at the cutoff point) is identified under the much weaker assumption that the control function is continuous in S at that point. To reduce the potential for misspecification, one could specify a very flexible form for $K(S) = E[u_i|S_i]$, such as a high order polynomial, or use local or nonparametric regression around the cutoff point $\bar{S}$. As long as the conditional expectation is reasonably smooth and continuous in S, identification will be guaranteed because of the discontinuity in the function T of S. Of course, it is true that in principle, any program effect can be captured by a highly nonlinear curve. However, in most cases a smooth curve (or in the extreme the two discontinuous regression segments themselves) that would eliminate the treatment effect would be so extremely convoluted and far-fetched that it would be almost impossible to imagine how in reality the two variables could be related to one another in the fashion represented[15],[16]. Especially if both segments are found to be very smooth, it will be difficult to believe that the discontinuity is due to a nonlinear effect of S rather than to an effect of T.

To address the potential for misspecification of the regression function (ie. the control function), Trochim (1984) proposed a curve-fitting strategy where one uses the lowest order polynomial curve not statistically significantly demonstrated to be too simple. This approach will lead to a parsimonious bias toward underfitting, while minimizing the loss of efficiency

[15]Note that if data were available from a setting in which no program was present, one would actually be able to discover the functional form of the relationship between the outcome and score.

[16]A related problem which emphasises the need to allow for a flexible (nonlinear) specification in S in the regression is that even with no actual nonlinear effect of S on Y curvilinearity could be caused by so called 'floor and ceiling' effects. As pointed out by Campbell (1984) if the assignment variable is an imprecise measure of a variable that actually affects the outcome, one will generally observe a flattening of the slope of the regression of the outcome on the assignment variable at both extremes. This in turn, combined with the assumption of a linear dependence of Y on S, may lead to a discontinuity at the cutoff point (a nonzero treatment effect) even if the true effect is zero. Thus even if no actual nonlinear effect exist, it will be appropriate to allow for a curvilinear fit.

caused by overfitting (that is, by using too high an order of the polynomial function).

A third, less important, weakness of the design is that even if the functional forms are properly specified, treatment effect estimates will be less efficient than in the case of a randomized experiment, that is, it will require a larger study population to accomplish the same statistical sensitivity to a treatment effect (Goldberger, 1972). Of course, one way to improve the precision of the effect estimate is to add additional regressors.

These disadvantages not withstanding, it is important to recognize the attractiveness of this quasi-experimental design. The main advantage of the RD design is that it is often easier to justify than randomization. In particular, as pointed out by Trochim (1984), the design is conceptually compatible with the political and social goal of allocating scarce resources to those individuals that need or deserve them most. Randomization typically implies that persons who might otherwise be eligible for a program be denied it. The RD design and its ability to generate credible effect estimates exemplifies the fact that the crucial difference for avoiding bias is not whether the assignments are random or nonrandom, but whether the investigator has knowledge of, and can model, the selection process.

The fuzzy RD design

The assumption of sharp assignment is in practice difficult to maintain. In case the program is known or expected to be beneficial, program administrators may be more sensitive to pressure by individuals wanting to join the program, and more willing to change their assignment if that individual's score was just below the cutoff. Alternatively, in addition to the position of the individual's score relative to the cutoff value, assignment may be based on additional variables observed by the administrator, but unobserved by the evaluator. In this case, even in the event when there is no actual program effect, we may find a regression discontinuity indicating a nonzero program effect. This would occur, for example, if in the area around the cutoff point individuals with lower scores went into the treatment group and those with higher scorers into the control group. If scores are positively correlated with the outcome of interest, this would lead to a regression discontinuity indicating a negative program effect. The case of misassignment relative to the cutoff value, with some scores of S near the cutoff appearing in both treatment and control group, has been referred to as the 'fuzzy RD design' (Campbell, 1969). Compared to the sharp design, selection here is on both observables and unobservables. Instead of having the step function $Pr(T = 1|S) = 0$ for $S < \bar{S}$ and $Pr(T = 1|S) = 1$ for $S \geq \bar{S}$ in the sharp RD design, the selection probability will now be a smoother function P(S), such as the two shown in Figure 2.

While in general misassignment will lead to a selection bias, there is one case discussed by Cain (1975, page 309) where this does not occur. In his 'mixed model' assignment is based on the selection variable and an independent assignment error e, which is independent of S and u:

$$\begin{aligned} T_i &= 1 \quad \text{if } S_i - \bar{S} + e_i \geq 0 \\ T_i &= 0 \quad \text{otherwise} \end{aligned} \tag{5}$$

The selection process is now fully represented by a random assignment that is conditional upon a known value for S. In this case of randomization combined with nonrandom selection, the earlier defined regression model, with a correctly specified control function of S included as independent variable, will as before produce an unbiased treatment effect estimate, but with more precision than in the previous case of rigid S-based assignment. Moreover, we need not rely on continuity of the control function near the cutoff point.

This is a case of selection of observables with a nondeterministic assignment rule. The difference between $E[Y|T = 0, S < \bar{S}]$ and $E[Y|T = 1, S < \bar{S}]$ and between $E[Y|T = 0, S \geq \bar{S}]$ and $E[Y|T = 1, S \geq \bar{S}]$ can then be used to help identify the treatment effect. That is, α would be nonparametrically identified, as conditional on S, the variation in T will be uncorrelated with u (and $Y_i(0)$ and $Y_i(1)$ will be independent of T_i given S_i). Similarly, in the varying treatment effect case, we will now be able to nonparametrically identify the varying treatment effect over the region of common support for $T = 0$ and $T = 1$.

In general, however, as discussed by Barnow, Cain and Goldberger (1980), if the selection process is not perfectly known and the unknown component e were not an independent assignment error, then estimation of

$$Y_i = \beta_0 + \alpha T_i + u_i$$

will lead to biased estimates even if we included a control function $E[u_i|S_i]$ to address the correlation between S and T. The bias will depend on the covariance of T and u conditional on S and may be positive or negative. To solve the selection bias problem we could adopt any of the existing approaches for the case of selection on unobservables. Each approach uses or requires additional information about the selection process to obtain an unbiased estimate of the treatment effect. As I mentioned earlier, almost all approaches rely on the availability of an exogenous regressor in Z which is excluded from the outcome equation, ie. an instrument. In the fuzzy RD design generally no such instrument exists. However, it may still be feasible to exploit the discontinuity and/or highly nonlinear nature of the assignment rule near the cutoff point. In addition to continuity of the control function near the cutoff point, to identify a constant or varying treatment effect we may need to rely on additional smoothness of the control functions $E[u|T=0,S]$ and $E[u|T=1,S]$.

In the fuzzy assignment case we can specify the selection or assignment equation in terms of a general function of S and $I(S > \bar{S})$ and an error term e_i:

$$\begin{aligned} T_i &= 1 \quad \text{if } g\Big(S_i,\ I(S_i > \bar{S})\Big) + e_i > 0 \\ T_i &= 0 \quad \text{otherwise} \end{aligned} \tag{6}$$

where e_i may be correlated with u_i in the outcome equation and where I is the indicator function. By including $I(S > \bar{S})$ we allow for the possibility that the assignment function may still contain some discontinuities caused by the existence of the cutoff value for S. By specifying the functional form of g and the distribution of e we could then proceed by replacing T_i in the outcome equation (augmented with the control function) by an estimate of $Pr(T_i = 1)$ as in Maddala and Lee (1976). One possible specification for g may, for example, be the piecewise polynomial function

$$g\Big(S_i,\ I(S_i > \bar{S})\Big) = (a_0 + \cdots + a_n S_i^n) \cdot I(S_i \leq \bar{S}) \ + \ (b_0 + \cdots + b_n S_i^n) \cdot I(S > \bar{S}) \tag{7}$$

Alternatively we could adopt a nonparametric approach for estimating the propensity score $Pr(T_i = 1)$ as a function of S, such as those considered by Todd (1995). In the evaluation literature, a similar nonparametric procedure has been termed the relative assignment variable approach (Spiegelman, 1979; Trochim and Spiegelman, 1980) where the relative assignment variable is in fact equal to the propensity score. In their approach the nonlinear relationship between treatment status and the selection variable S is estimated by a nearest neighbor moving average method, which involves computing the moving average of the T values for cases ordered by S. It can be expected that the nonparametric propensity score estimates, while perhaps no longer discontinuous, will generally be a highly nonlinear function of S, going from 0 to 1, such as those in Figure 2. Therefore, similar to the sharp RD design, identification will be based on the s-shaped nonlinearity and possible discontinuity near the cutoff point.

A third, and least restrictive possibility is to adopt an IV procedure, using the discontinuity (the indicator $I(S_i > \bar{S})$ with possible interactions with S_i) as instrument for the treatment indicator.

It is important to recognize at this point, that, what in a sense is identification on functional form, is based on specific knowledge of characteristics of the selection mechanism (the existence and values of the selection variable and cutoff points)[17]. Moreover, without making the connection to this quasi-experimental design, several other studies have similarly relied on known or explained nonlinearities (or discontinuities) in the assignment rule as source of identification. For example, Angrist and Krueger (1991) use quarter of birth as instrument for years of schooling in an earnings equation. Quarter of birth is correlated with educational attainment because of a mechanical interaction between compulsory school attendance laws and age at school entry. To be able to identify the effect of education on earnings, it is clear that an assumption has to be made about the form of direct age effects on

[17]In this regard, it is also important to note that the existence of multiple program or treatment levels and cutoff points will further aid in identification. The financial aid assignment rule discussed later is an example of this.

earnings[18]. Angrist and Krueger add a quadratic function of age to the equation to control for within-year-of-birth age effects on earnings and the schooling effect is therefore identified through deviations from this quadratic trend[19]. Similarly, Imbens and van der Klaauw (1995) use nonlinear policy-induced variation in aggregate military enrollment rates across birth cohorts to evaluate the effect of military service on subsequent earnings in the Netherlands. In that study the estimated earnings equation included a third order polynomial function in age and the effect estimate was shown to be relatively insensitive to the inclusion of a fourth and fifth order polynomial.

Black (1996) exploits the regression-discontinuity design in evaluating the effect of elementary school quality on local housing prices. In this study geographic location within a school district with respect to school attendance district boundaries (which determine which school a child attends within the school district), is used to isolate the school quality effect from other neighborhood effects on housing prices. By assuming geographic location, and thereby neighborhood effects, to directly affect housing prices in a smooth way, the effect is essentially identified on the difference between those who live on different sides of the street which forms a boundary (ie. the discontinuity), and for whom presumably only the assigned elementary school differs.

The regression-discontinuity design is also exploited in a recent study by Angrist and Lavy (1996). That study is the closest to the study here in that it makes explicit use of a discontinuity in the assignment rule to identify a treatment effect, in this case the effect of class size on student performance. This discontinuity is induced by a rule, called Maimonides' rule in reference to the Talmudic scholar, used in Israel to determine school class size as a function of (beginning of the year) total school enrollment. By allowing total school enrollment to directly affect student outcomes in a smooth fashion, the class size effect is identified on the discontinuities in the actual student outcome-school enrollment relation

[18] If the effect of age was modelled as a step function of age in quarters identification of the education effect would be impossible.

[19] Variation between states in school attendance laws (ie., the cutoff date) was also used to identify the schooling effect.

caused by the class size assignment rule[20].

Finally, I would like to emphasize the advantages and the wide applicability of the fuzzy RD design in program evaluation. Unlike randomization it allows the program to be primarily geared to the subpopulation of focus and may be easier to implement and, unlike the sharp RD design, it allows for somewhat greater flexibility in the assignment of treatment (note that this design also represents the degraded version of the sharp RD design in which the assignment strategy was incorrectly implemented). For example, eligibility criteria for welfare programs may be based on need (income, assets and household composition) and eligibility rules for training programs may limit entry only to those for whom the program is likely to be most beneficial (e.g. on the basis of educational background). As will be discussed later, many existing welfare programs, such as the Food Stamps and AFDC programs, have eligibility requirements that fit the RD design. In the next section I will show that the assignment rule used by College X in determining individual financial aid offers conforms to that of a fuzzy RD design.

III. The data set

Annual information about all students who apply and are admitted to College X is stored in a large computer data base. This information is obtained from three different sources: each student's original application package, the student's financial aid application forms and a record kept by the college of the total financial aid package offered to the student and the student's subsequent enrollment decision.

The application package for college admission provides information on a wide range of student characteristics, such as age, gender, race, place of residence, citizenship, as well as information about each student's high school record. The latter information is available in the form of transcripts reporting individual course grades, the student's high school grade point

[20] According to this rule no class size can be greater than 40. Thus, for example, if total enrollment equals 79, one class of 39 students and one other of 38 are formed, while a total enrollment of 81 would lead to three classes of 27 students each.

average (GPA), SAT scores, school or class rank as well as letters of recommendation and a statement of purpose. For those eligible for federal or state aid, the financial aid application form contains information on the income of the student's parents, as well as their expected financial contribution. The data base also includes for all students information about College X's offered aid package, consisting of the amounts of different types of financial aid that make up the total aid package of a student - loan or grant, federal aid or College X sponsored aid. In our empirical analysis we will focus on the effect of local, college sponsored aid.

The information on each year's pool of admitted applicants is stored in two different files. One file includes only those applicants (referred to as FILERS) who did submit the FAFSA (the Free Application for Federal Financial Aid) form to apply for federal aid jointly with a NAD (Need Analysis Document) form to apply for aid from College X. The submission of these forms makes the applicant eligible to receive federal and state funded financial aid for college, which can be provided in the form of a federal or state grant and/or in the form of a loan, where the latter typically is the largest. On the basis of their academic ability and parental income, an estimate of the total amount and types of state and federal financial aid the student will receive is calculated using a set of given formulas, which is then forwarded to College X. On the basis of this estimate the college then determines the amount of discretionary aid to offer to the student. Finally, all forms of financial aid are included as part of the total aid package offered to the applicant. The NAD form contains personal information not included in the student's application form for college admission, such as reported parental income and the expected parental contribution to schooling costs.

The other data file includes all those applicants who did not formally apply (did not qualify) for federal financial aid (NONFILERS). This group includes individuals who do not qualify for federal aid because of high parental icome as well as foreign citizens who are not permanent residents of the US and who are therefore ineligible to any federal aid. Unlike the data file on filers, the file on non-filers does not include information on parental income and their expected contribution. These applicants who are not eligible for federal and state aid, are still considered for discretionary aid. As with the filers, the nonfilers are sent a complete aid package, together with a notice of admission. Because of differences in the financial aid allocation rule and in the available information, and because the student enrollment decision is likely to differ for both groups, in our empirical analysis both groups will be studied separately[21].

The actual decision rule the college adopts in determining each student's financial aid offer is fairly complex, involves both objective and subjective evaluations, and is therefore difficult to characterize by a simple formula. While many relevant student characteristics, such as different ability measures, minority status and (for some) parental income are included in the data base, others, such as the student's statement of purpose, extracurricular activities, transcripts and recommendation letters, are not.

An important feature of the financial aid decision process of most universities, however, is the existence of simple rules, the specifics of which are generally unknown to student applicants, which are designed to make the allocation of aid more objective, regulated and easier to implement. One of these features, adopted by College X, is the use of a simple formula which converts a student's SAT scores and high school GPA into an **index** which is then used to rank students into a small number of categories. More precisely, during the period studied here, the particular index formula used was

$$S = \phi_0 \text{ x (first three digits of total SAT score)} + \phi_1 \text{ x GPA}$$

where S represents the calculated index. Applicants were then divided into four groups on the basis of the interval the calculated index fell into. These intervals were determined by three cutoff points on the S scale. Let the three cutoff points in ascending order be denoted by $\bar{S}_1$, $\bar{S}_2$ and $\bar{S}_3$ respectively, then the highest rank or category would consist of students with index scores above $\bar{S}_3$.

Students of different rank are eligible to different amounts of aid. Within a rank, a base amount is assigned which is subsequently adjusted on the basis of the student's minority

[21] In differentiating between these two groups of applicants, I assume that all those eligible for federal aid actually applied for it.

status, family income as well as more subjective and detailed evaluations of the student's complete application package. These adjustments are therefore both merit and need based. It is likely that these adjustments will to some extent depend on the value of the individual's index in relation to the nearest cutoff point. That is, if an individual's index score is just below a cutoff point then it is likely that, if the financial aid officer is impressed with the student's total package, the adjustment will make the amount of financial aid offered more similar to that offered to someone who scored just above the cutoff. Nevertheless the overall aid offer is still likely to be less that that of an applicant who scored just above the cutoff point.

Note that, because students do not know this aspect of the allocation rule and are unlikely to be able to learn about the rule from observed offers[22], we need not worry about a possible effect of the rule on a student's S score which would make S endogenous (students may retake the SAT test if they knew their score was just below a cutoff value)[23].

The financial aid offer rule therefore fits that of the fuzzy RD design, with multiple cutoff points and multiple treatment (financial aid offer) levels. Denoting the total amount of discretionary college aid by F, the financial aid allocation process just described can be characterized as follows:

$$F_i = f\Big(S_i\ , I(S_i > \bar{S}_1), I(S_i > \bar{S}_2), I(S_i > \bar{S}_3)\Big) + e_i \tag{8}$$

where the unobserved component e_i captures all other relevant characteristics of the student (and possibly of the financial aid officer) which influenced the financial aid decision.

[22]Both the weights ϕ_0 and ϕ_1, as well as the three cutoff points did vary during the period studied.

[23]In general, knowledge of the assignment rule may lead to selection biases that may complicate the RD evaluation problem. For example, in the Angrist and Krueger (1991) study, if some parents take the effect of birth month on the child's first school enrollment year into account in timing the birth of their child (although perhaps unlikely), this could lead to a selection problem where children born to such parents can no longer be assumed to be on average the same as those born to other parents. Similarly, knowledge of the class size rule may affect the school choice or application decision of parents, which could complicate the analysis in Angrist and Lavy (1996).

IV. Student enrollment decisions

As outlined in Manski and Wise (1983, chapter 2), the complete admission process can be treated as a series of decision stages, with each individual decision taken by a different agent. The first stage represents the student's college application decision. The second the institution's admission decision. The third is the institution's financial aid offer decision, conditional on an offer of admission, and the fourth is the student's enrollment decision. While the previous section discussed stage three of this process, in this section I provide a simple model of stage four.

The student's decision problem can be characterized, as having to make an optimal choice from a discrete set of school and non-school characteristics. In our case where we model the choice to enroll at College X, we can define a student's options, given his prior college application decisions and each college's subsequent admission and financial aid decisions, as (1) enroll at College X (and accept its financial aid offer) and (2) enroll at another college[24]. The enrollment decision can be thought of as involving a comparison between the student's utility associated with each choice alternative. The utility a student receives from each decision will depend on the total costs and total benefits associated with each choice. The costs include tuition and living expenses minus financial aid, while the benefits include the consumption value of studying either at College X or at another college as well as the student's expected future earnings and job prospects after graduation from college. With missing data on tuition cost, living expenses and post-graduation earnings for different colleges, I will specify the difference in the utility associated with each choice alternative simply as a function of the difference in financial aid offered and an unobserved component capturing all other factors. Let F represent the amount of discretionary aid offered by College X and F^* the financial aid offer made by the most preferred college other than College X. Then, for an individual i the difference in utility associated with the choice to enroll or not

[24] I ignore the additional option to join the labor market or military. Few of those admitted at College X tend to choose this option.

can be specified as

$$\Delta U_i = \delta(F_i - F_i^*) - v_i \tag{9}$$

where the unobserved random component v_i measures all other individual differences in the utility associated with alternative choice options.

The decision to enroll ($E = 1$) or not ($E = 0$) therefore depends on the amount of discretionary aid offered by College X, as well as the financial aid offers made by the most preferred alternative college. The probability that the student will enroll at College X is then given by:

$$\begin{aligned} Pr(E_i = 1) &= Pr(\Delta U_i > 0) = Pr(\delta(F_i - F_i^*) - v_i > 0) \\ Pr(E_i = 0) &= 1 - Pr(E_i = 1) \end{aligned} \tag{10}$$

Unfortunately it is not possible to directly estimate the enrollment equation (10) above, because F_i^* is unobserved. Generally one would expect financial aid offers from other colleges, and thus that of the student's most attractive alternative college option to depend on the same observed student ability measures. Accordingly, I characterize the amount of financial aid offered by the student's most prefered alternative as

$$F_i^* = h(GPA_i, SAT_i) + w_i \tag{11}$$

where w_i captures all other relevant characteristics of the student. It is important to note that there is no reason to also include the binary rank indicators (the $I(S_i > \bar{S}_k)$ terms) as determinants of F^*, as these indicators are idiosyncratic components of College X's aid allocation process and, conditional on GPA and SAT, should have no relevance in the offer decisions of other colleges[25].

After substituting the above equation into the expression for ΔU, we obtain

$$\Delta U_i = \delta F_i - \delta h(GPA_i, SAT_i) - u_i \tag{12}$$

[25]Note that h represents the average (over individuals and schools) of the aid offer functions of all prefered alternatives. While other colleges may similarly use some discontinuous aid allocation rule, we can therefore assume h to be reasonably smooth. The specification of h will be discussed below.

where $u_i = v_i + \delta w_i$. Note that college aid F_i will be correlated with the composite error u_i, when e_i and w_i (the errors in the two aid equations) are correlated and/or if e_i and v_i (the error in the college aid equation and in the enrollment equation) are correlated.

Some of the specifications that I will estimate will also include a set of other individual characteristics, such as parental income and minority status, in both the financial aid equation (8) and in the expression for ΔU to capture potential differences in preferences for studying at college X.

Estimation Method

The evaluation problem as described by our model

$$Pr(E_i = 1) = Pr[\ \delta F_i - \delta h(GPA_i, SAT_i) - u_i > 0\ ] \tag{13}$$

$$F_i = f\big(S_i, I(S_i > \bar{S}_1), I(S_i > \bar{S}_2), I(S_i > \bar{S}_3)\big) + e_i \tag{14}$$

conforms to that of the fuzzy RD design. The outcome measure (enrollment) is here discrete and treatment status corresponds to the amount of financial aid offered and rather than being binary is now continuous. But most importantly, assignment is based at least in part on the basis of the relative magnitude of an observed continuous selection or assignment index with respect to a few cutoff values.

As was the case for the fuzzy design with an independent assignment error, if the error in the financial aid equation e_i is uncorrelated with the error in the outcome (enrollment) equation u_i, unbiased estimates can be obtained of the financial aid effect δ by specifying and including a control function. Given that the selection variable S (the calculated index) in this case essentially represents a weighted linear combination of two other variables, GPA and SAT, both of which are already included in the enrollment equation derived from our model, there is no need to include a control function because if h is correctly specified then u_i will be uncorrelated with S_i by definition. To reduce the potential for misspecification of h, I follow the curve-fitting strategy proposed by Trochim (1984) where we use the lowest order polynomial curve in GPA and SAT not statistically rejected.

However, our simple model implies that the assumption of uncorrelated errors is unacceptable as the stochastic component in the financial aid equation, e_i (a function of characteristics of the applicant observed by the aid officer), is likely to be correlated with the stochastic component of the aid offer of the best alternative, w_i, and also may be correlated with the error in the enrollment equation v_i, both of which are components of the composite error u_i in the enrollment equation. Therefore, estimation of the enrollment equation would lead to a biased effect estimate. Similar to the approach discussed earlier in section II, the selection bias problem can be solved by specifying the functional form of f, estimating the financial aid equation, and replacing F_i in the outcome equation by its predicted value $\hat{F}_i$. I will follow two different approaches to obtain predictions for F. First, I specify f to be a piecewise polynomial function of degree n in S as follows:

$$\begin{aligned} F_i &= I(S_i \le \bar{S}_1) \cdot \sum_{j=0}^{n} \gamma_{1j} S_i^j + I(S_i > \bar{S}_1) \cdot \sum_{j=0}^{n} \gamma_{2j} S_i^j + I(S_i > \bar{S}_2) \cdot \sum_{j=0}^{n} \gamma_{3j} S_i^j \\ &+ I(S_i > \bar{S}_3) \cdot \sum_{j=0}^{n} \gamma_{4j} S_i^j + e_i \end{aligned} \tag{15}$$

Second, I will estimate the financial aid equation as a nonparametric function of S, $g(S)$, using spline smoothing methods[26]. This estimated function $g(S)$ minimizes the sum

$$\sum_{i=1}^{n} (F_i - g(S_i))^2 + \lambda \int (g''(S))^2 dS \tag{16}$$

over the class of all twice differentiable functions over the observed domain of S. λ represents a smoothing parameter which determines the weight given to the roughness penalty $\int (k''(S))^2 dS$. The estimated curve $k(S)$ has the property that it is a cubic polynomial between two successive S-values, which at each observation for S_i is continuous, with continuous first and second derivatives (see, Hardle, 1990, pg 56-57).

V. Estimation Results

Table 1 provides the enrollment rate and average financial aid offer for both filers and non-filers for the academic year 1991-92[27]. In the table SAT represents the sum of the verbal

[26] In both approaches I ignore a non-negativity constraint on F.

[27] Observations with missing values for the variables used in the analysis were deleted.

and mathematical SAT test scores and GPA represents the student's high school Grade Point Average, on a 4-point scale. RANKi is an indicator of the interval which corresponds to the individual's index score S, with RANK4 representing the interval with the lowest scores and RANK1 the interval with the highest scores. The table shows that while financial aid increases from RANK4 to RANK1, for both filers and nonfilers the enrollment rate varies in a nonmonotonic fashion with RANK.

The table also compares those who enrolled with those who did not in 1991. Filers who enrolled were offered more financial aid, on average, then those who did not. We also find that the percentage of students with rank less than 4 is a little lower amongst those who enrolled. Similarly, those who enrolled have on average somewhat lower SAT scores than those who did not enrol. In contrast, there is little difference between the two groups in the average GPA. These patterns are also found when controlling for rank. Within each RANK, there is little difference in average GPA, but the average SAT scores are lower for those who enrol. It can be expected that those with higher scores, but equal rank, are less likely to enroll at College X because, while they are offered similar amounts of aid from College X, they will on average receive higher financial aid offers from other colleges.

Similar, but somewhat smaller differences in average SAT and GPA scores are found for nonfilers. Different from filers, however, nonfilers who enrolled received on average **lower** offers of local financial aid than nonfilers who did not enroll, which suggests a negative effect of the amount of financial aid on enrollment.

Figure 3 presents a scatter diagram of financial aid offers against the calculated index S for the sample of filers. Also shown in the graph is an estimated spline smooth. The spline smooth clearly reveals the sharp increases in average financial aid at the three cutoff points (represented by vertical lines). With less smoothing (lower values of the spline smoothing parameter λ) the sharpness of the increases became even more pronounced, but this also made the rest of the curve less smooth. These increases are also clearly revealed by the data themselves, where the values at which bunching occurs change at each cutoff point. The graph for nonfilers (Figure 4) shows very similar features.

As discussed earlier, two approaches were adopted to estimate the financial aid equation. First, I adopted several piecewise polynomial function specifications. Estimates of the linear and cubic piecewise polynomial specifications are shown in Table 2. Second, a nonparametric spline smoothing method was adopted, resulting in the spline smooth estimates shown in Figures 3 and 4. Figures 5 and 6 show the spline smooth estimates together with the estimated piecewise linear and cubic regressions[28]. The sharp increases around the cutoff values are clearly visible in all estimated functions.

Before presenting the estimated enrollment equation, it will be interesting to see whether the increments seen in financial aid offers around the cutoff values can also be found in the enrollment percentage. Figure 7 shows a spline smooth of the percentage enrolled as a function of the index score S for filers. While the slope of the curve changes only little around the second cutoff point, it changes considerably around the other two (with a smaller value of the smoothing parameter λ these changes again became somewhat more pronounced). A similar pattern of slope changes near the cutoff points is found for nonfilers as shown in Figure 8.

The discontinuities which characterize the RD design can be directly exploited to obtain preliminary estimates of the program effect at each cutoff value $\bar{S}_j$ by comparing the enrollment rate of those with index scores just the below cutoff, denoted by $\bar{E}_{<\bar{S}_j}$, and above the cutoff, $\bar{E}_{>\bar{S}_j}$. More precisely, a program effect estimate can be obtained by dividing $\bar{E}_{>\bar{S}_j} - \bar{E}_{<\bar{S}_j}$ by the corresponding difference in the average aid amounts between those just below and above the cutoff point $\bar{F}_{>\bar{S}_j} - \bar{F}_{<\bar{S}_j}$, for each value of $j = 1, 2, 3$. Comparing those within 3 points below and above each cutoff results in effect estimates 0.010, 0.040 and 0.067 at cutoff points $\bar{S}_1$, $\bar{S}_2$ and $\bar{S}_3$, respectively. Similarly, for nonfilers the estimates are 0.519, 0.036 and -0.030[29]. While the estimates for filers suggest that the effect estimate may

[28]The piecewise quadratic regression was not shown to improve visibility, but was found to be very close to the piecewise cubic regression estimate.

[29]Corresponding estimates based on the much smaller samples of individuals within 2 points of the cutoff point, were 0.052, 0.075, 0.107 for filers and 0.076, 0.060 and -0.043 for nonfilers.

increase with S, for nonfilers the estimate appears to be descreasing in S.

To obtain effect estimates based on the complete sample of student applicants, the enrollment equation was estimated both as a Linear Probability model and as a Probit model. Defining $Pr(E_i = 1) = Pr[\ \delta F_i - \delta h(GPA_i, SAT_i) - u_i > 0\ ] = W[\ \delta F_i - \delta h(GPA_i, SAT_i)\ ]$ where W is the distribution function of u_i, the Linear Probability model corresponds to the case where W is linear, in which case $E[E_i|F_i, GPA_i, SAT_i] = Pr(E_i = 1) = \delta F_i - \delta h(GPA_i, SAT_i)$ and the equation can be estimated by OLS. The second case corresponds to the case where W is the normal distribution function, in which case the enrollment equation is estimated by maximum likelihood. The estimated elasticities and overall qualitative results were found to be very similar for both specifications. In what follows I therefore only present estimates corresponding to the linear probability model specification of the enrollment equation[30].

Two-stage least squares estimates of the enrollment equation are presented in Table 3[31]. The predicted aid amount $\hat{F}$ was calculated using the estimated piecewise cubic specification (the most flexible parametric specification considered) of the financial aid equation. The high R^2 values and the significance of the discontinuous increments of the estimated financial aid equation in Table 3 imply that we need not worry about estimation problems commonly encountered when only weak instruments are available.

As discussed earlier, a forward, stepwise procedure was used of specifying a piecewise linear, quadratic, cubic and, if necessary, higher degree polynomial function in GPA and SAT. For filers a quadratic enrollment function in GPA and SAT was found to be sufficient and the effect estimate for filers was found to be 0.052 which corresponds to an estimated enrollment elasticity with respect to financial aid (in thousands) evaluated at the mean of 0.87. Thus a 10% increase in financial aid is predicted to lead to a 8.7% increase in the probability an individual will enroll. For nonfilers the stepwise specification procedure

[30]Estimates corresponding the Probit specification are available on request from the author.

[31]The Linear Probability model has the drawback that it can generate predicted probability values outside the unit interval. However, in the case of filers (nonfilers) this was found to occur only in 3 (1) of the 2225 (1150) cases.

resulted in a linear enrollment equation specification and an effect estimate of 0.020 which corresponds to an elasticity of 0.14. Given that those who did not qualify for federal financial aid are likely to be less financially constrained, it is perhaps not suprising that filers were found to be more sensitive to the offered aid amount. The 2SLS standard errors presented in the table for all parametric specifications take into account the two-stage nature of the estimation process.

To explore the sensitivity to the functional forms of both equations, Table 4 provides estimates of the aid effect δ for several alternative specifications. The table shows that the estimates are relatively insensitive to the specification of the financial aid equation. The estimates found for the parametric specifications are further very similar to those for the spline smooth estimates[32]. On the other hand, the estimates are more sensitive to the specification of the aid equation. For filers, the quadratic specification was found to be required, while for nonfilers a linear specification was sufficient.

The table also presents estimates obtained when both the enrollment and financial aid equation were augmented with a host of additional observed individual characteristics, including ethnicity and parental income[33]. Overall the estimates fell slightly for filers and remained the same for nonfilers. The overall implication of the table is that the effect estimates are fairly robust to different specifications.

It is interesting to compare these estimates with those obtained when the actual value is used instead of the predicted financial aid amount. When relating enrollment to a constant and the actual aid amount, for filers an effect estimate of 0.030 (0.003) was found and for nonfilers the effect estimate was -0.011 (0.006). Table 4 shows estimates obtained for other specifications of the enrollment equation. The estimates show that ignoring the endogeneity of the financial aid offer leads to only slightly lower effect estimates for filers, but much

[32]The standard errors presented for the nonparametric aid equation specification have not been adjusted for the fact that the financial aid amount used in estimating the enrollment equation was predicted.

[33]For filers I included 15 variables, including age, gender, 2 measures of ethnicity, citizenship, 6 indicators of the state of residence, a quadratic in parental income and a quadratic in transferable federal and state aid. For nonfilers all variables except the parental income and federal/state aid variables were included. Parameter estimates of these regressions are not shown for confidentiality reasons.

smaller effect estimates for nonfilers. In addition, when using actual aid amounts, the effect estimate for filers appears much more sensitive to the inclusion of other explanatory variables.

Table 5 presents similar effect estimates for the years 1989 to 1992 separately and all years combined. To make the estimates comparable all financial aid amounts were expressed in 1991 dollars. In the estimations using the pooled data sets, the enrollment equation included separate (for each year) intercepts and slope terms for each variable except F, the amount of financial aid offered. The effect estimates are relatively stable over time, and as in the case of 1991, are much greater for filers than for nonfilers. The implied elasticities of enrollment with respect to aid evaluated at the means are 0.70 for filers and 0.10 for nonfilers. Table 6 presents a sensitivity analysis similar to that in Table 4, but now for the pooled data set, which similarly shows the effect estimates to be robust to alternative specifications.

VI. Conclusion

In this paper I provided an illustration of the applicability of a powerful, but often ignored, program design: the (fuzzy) Regression-Discontinuity design. In this design the selection or assignment rule for determining who participates in a program or who receives treatment, contains discontinuities and/or nonlinearities in one or more observed continuous variables. These variables may themselves affect the outcome variable of interest, even in absence of the program. I show how this design, and in particular these discontinuities and nonlinearities can be exploited to obtain credible program effect estimates, without the need to impose arbitrary exclusion restrictions and functional form assumptions about error distributions to solve the selection bias problem.

The allocation rule used by an east coast college in determining individual financial aid offers was shown to resemble the selection or assignment rule in the fuzzy RD design. Estimates of the effect of financial aid on college enrollment were obtained which were found to be robust to alternative specifications of the enrollment and aid offer equation. Discretionary college aid was found to have a strong impact on college enrollment. In the analysis the effect of financial aid on enrollment was assumed to be constant. However, it is reasonably straightforward to allow for nonlinear effects and to include interaction terms with other explanatory variables, in which case the resulting aid effect estimates could be used to help design more effective financial aid allocation rules.

An important implication of this study is that more detailed knowledge of the selection mechanism can often aid in obtaining reliable program effect estimates. The features of the assignment rule which characterize the fuzzy Regression-Discontinuity design, should be relatively easy to incorporate and in fact are already likely to be found in the assignment or selection rules of many other non-experimental program designs. Some recent evaluation studies, such as those by Angrist and Krueger (1991), Blank (1996) and Angrist and Lavy (1996) were shown to have relied on the design in obtaining effect estimates. Moreover, it is likely that features of existing eligibility requirements can potentially be exploited to evaluate the impact of different welfare programs, such as the Food Stamps and AFDC programs. Eligibility conditions to Food Stamps, for example, includes a maximum wealth or assets test, while eligibility to AFDC requires the presence of child below 18 years of age. While the former generates a discontinuous relation between Food Stamps receipt and assets, the latter implies a discontinuous relation between AFDC receipt and the age of the child (if only one such child is present)[34]. The study and possible exploitation of such program characteristics has been relatively ignored and represents an important area for future evaluation research in economics.

[34]Given that this AFDC eligibility requirement is fairly well known, the presence of an age-eligible child may become endogenous, complicating the evaluation problem.

Table 1: Sample Means

	Variable	FILERS Total	FILERS Enrol	FILERS Not Enrol	FILERS % Enrol	NONFILERS Total	NONFILERS Enrol	NONFILERS Not Enrol	NONFILERS % Enrol
	GPA	3.40	3.34	3.43		3.26	3.19	3.28	
	SAT	1160	1133	1171		1179	1159	1182	
	F	5080	6018	4673		1012	777	1052	
	$\%F = 0$	0.12	0.07	0.14		0.73	0.77	0.72	
RANK4		0.50	0.60	0.46	36.3	0.60	0.71	0.58	17.2
	GPA	3.11	3.10	3.12		3.01	2.99	3.02	
	SAT	1093	1083	1099		1123	1119	1124	
	F	4016	5027	3439		126	50	141	
	$\%F = 0$	0.13	0.07	0.17		0.96	0.98	0.95	
RANK3		0.20	0.17	0.21	26.3	0.15	0.08	0.17	7.3
	GPA	3.52	3.55	3.51		3.47	3.47	3.47	
	SAT	1168	1147	1176		1198	1207	1197	
	F	4633	5845	4200		288	692	256	
	$\%F = 0$	0.14	0.13	0.15		0.91	0.85	0.91	
RANK2		0.19	0.13	0.22	20.9	0.15	0.16	0.14	16.1
	GPA	3.75	3.76	3.74		3.66	3.66	3.66	
	SAT	1220	1206	1224		1263	1250	1266	
	F	5944	7587	5511		2810	2783	2816	
	$\%F = 0$	0.11	0.00	0.14		0.07	0.04	0.07	
RANK1		0.11	0.10	0.12	26.6	0.10	0.05	0.11	8.0
	GPA	3.91	3.91	3.91		3.91	3.93	3.90	
	SAT	1336	1317	1343		1366	1342	1368	
	F	9085	10195	8683		4942	4494	4981	
	$\%F = 0$	0.01	0.00	0.01		0.00	0.00	0.00	
	Obs	2225	674	1551	30.3	1150	168	982	14.6

GPA and SAT represent the average high school grade point average and total SAT test score. F equals total discretionary aid offered by college X and RANKi represents the rank or category an individual is assigned on the basis of the value of the index S, with RANK1 representing the highest rank.

Table 2: Estimates Financial Aid Equations

Variable	FILERS		NONFILERS	
	pw linear	pw cubic	pw linear	pw cubic
$I(S < \bar{S}_1)$	3977.66	3961.39	52.47	44.19
	(116.30)	(133.42)	(34.84)	(39.99)
$S \cdot I(S < \bar{S}_1)$	2.57	10.94	5.65	4.57
	(5.76)	(11.71)	(1.79)	(3.59)
$S^2 \cdot I(S < \bar{S}_1)$		-0.079		0.059
		(0.307)		(0.108)
$S^3 \cdot I(S < \bar{S}_1)$		-0.0078		-0.0000
		(0.0119)		(0.0042)
$I(S > \bar{S}_1)$	5234.08	5168.41	354.25	159.44
	(238.59)	(409.22)	(107.97)	(206.02)
$S \cdot I(S > \bar{S}_1)$	-85.09	6.63	-9.19	175.55
	(28.64)	(258.38)	(13.19)	(124.00)
$S^2 \cdot I(S > \bar{S}_1)$		-19.255		-33.995
		(43.182)		(20.172)
$S^3 \cdot I(S > \bar{S}_1)$		0.9826		1.6443
		(2.0050)		(0.9302)
$I(S > \bar{S}_2)$	6123.77	5643.35	2667.16	2652.06
	(236.65)	(388.18)	(98.35)	(162.20)
$S \cdot I(S > \bar{S}_2)$	-21.37	283.41	17.03	-21.30
	(23.69)	(199.21)	(9.86)	(87.81)
$S^2 \cdot I(S > \bar{S}_2)$		-37.721		9.487
		(26.852)		(11.932)
$S^3 \cdot I(S > \bar{S}_2)$		1.2493		-0.4576
		(0.9948)		(0.4456)
$I(S > \bar{S}_3)$	9149.79	9124.45	4979.57	5033.85
	(275.83)	(485.79)	(125.29)	(226.88)
$S \cdot I(S > \bar{S}_3)$	-7.01	46.15	-3.21	-9.45
	(23.79)	(177.01)	(9.04)	(75.37)
$S^2 \cdot I(S > \bar{S}_3)$		-8.126		-0.456
		(16.611)		(6.827)
$S^3 \cdot I(S > \bar{S}_3)$		0.2604		0.0300
		(0.4254)		(0.1720)
Observations	2225	2225	1150	1150
Adj. R^2	0.802	0.802	0.883	0.883

Standard errors in parentheses.

Table 3: Estimates Enrollment Equations

(Linear Probability Model)

Variable	FILERS	NONFILERS
constant	0.030	0.748
	(1.092)	(0.197)
GPA	0.410	-0.098
	(0.335)	(0.037)
$SAT/100$	0.090	-0.255
	(1.210)	(0.110)
GPA^2	0.025	
	(0.032)	
$SAT^2/10000$	0.632	
	(0.412)	
$SAT * GPA/100$	-0.667	
	(0.219)	
$\hat{F}/1000$	0.052	0.020
	(0.013)	(0.012)
Observations	2225	1150
Adj. R^2	0.035	0.008

Estimates were obtained using the stepwise curve-fitting procedure described in section IV. Two-stage LS standard errors in parentheses.

Table 4: Sensitivity Analysis - Effect Estimates 1991

Piecewise Polynomial Specification Financial Aid Equation	Specification enrollment Equation			
	linear	quadratic	cubic	cubic-extended
	FILERS (2225 observations)			
linear	0.040	0.056	0.052	0.039
	(0.010)	(0.013)	(0.015)	(0.011)
quadratic	0.039	0.054	0.049	0.038
	(0.010)	(0.013)	(0.015)	(0.011)
cubic	0.037	0.052	0.046	0.037
	(0.010)	(0.013)	(0.015)	(0.010)
nonparametric	0.040	0.056	0.052	
	(0.011)	(0.013)	(0.016)	
actual aid	0.047	0.049	0.049	0.013
	(0.003)	(0.003)	(0.003)	(0.003)
	NONFILERS (1150 observations)			
linear	0.020	0.030	0.034	0.034
	(0.012)	(0.016)	(0.020)	(0.020)
quadratic	0.019	0.029	0.034	0.031
	(0.012)	(0.016)	(0.020)	(0.020)
cubic	0.020	0.030	0.035	0.033
	(0.012)	(0.016)	(0.020)	(0.020)
nonparametric	0.019	0.030	0.036	
	(0.012)	(0.017)	(0.021)	
actual aid	0.007	0.007	0.005	0.014
	(0.009)	(0.011)	(0.012)	(0.003)

Entries represent estimated coefficient on financial aid F measured in thousands. Standard errors in parentheses.

Table 5: Effect Estimates 1989 - 1992

	FILERS		NONFILERS	
	Estimate	Obs	Estimate	Obs
1989	0.054	2182	0.014	1147
	(0.015)		(0.012)	
1990	0.042	2131	0.035	1210
	(0.020)		(0.013)	
1991	0.052	2225	0.020	1150
	(0.013)		(0.012)	
1992	0.040	2434	0.019	1169
	(0.012)		(0.007)	
1989-1992	0.045	8972	0.020	4676
	(0.008)		(0.005)	

Entries represent estimated coefficient on financial aid F measured in thousands of 1991 dollars. Standard errors in parentheses.

Table 6: Sensitivity Analysis - Effect Estimates 1989 - 1992

Piecewise Polynomial Specification Financial Aid Equation	Specification enrollment Equation			
	linear	quadratic	cubic	cubic-extended
	FILERS 1989-1992 (8972 observations)			
linear	0.036	0.045	0.049	0.039
	(0.005)	(0.007)	(0.008)	(0.006)
quadratic	0.035	0.043	0.045	0.036
	(0.005)	(0.006)	(0.008)	(0.006)
cubic	0.035	0.043	0.044	0.036
	(0.005)	(0.006)	(0.008)	(0.005)
nonparametric	0.038	0.046	0.049	
	(0.005)	(0.007)	(0.008)	
	NONFILERS 1989-1992 (4676 observations)			
linear	0.020	0.022	0.013	0.014
	(0.005)	(0.006)	(0.007)	(0.007)
quadratic	0.020	0.022	0.013	0.014
	(0.005)	(0.006)	(0.007)	(0.007)
cubic	0.020	0.022	0.013	0.013
	(0.005)	(0.006)	(0.007)	(0.007)
nonparametric	0.020	0.022	0.012	
	(0.005)	(0.007)	(0.008)	

Entries represent estimated coefficient on financial aid F measured in thousands. Standard errors in parentheses.

Fig. 1: The Regression – Discontinuity design

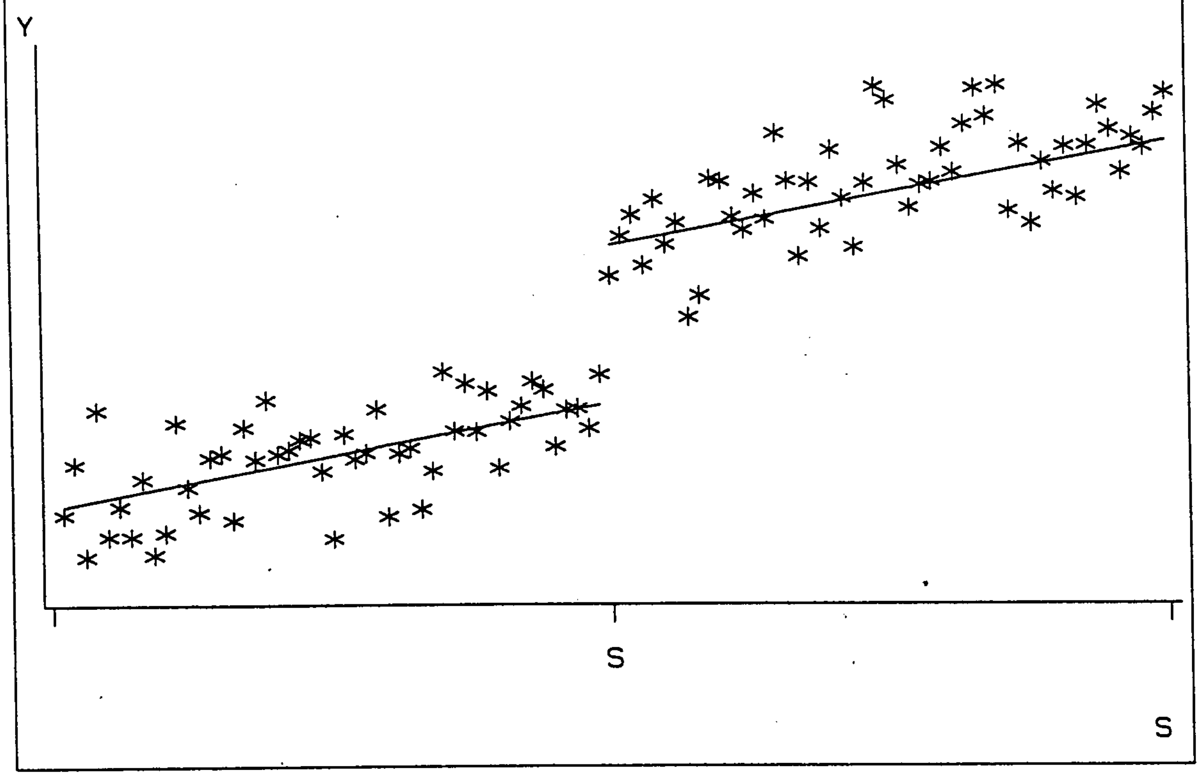


Fig. 2: Assignment in the fuzzy RD design

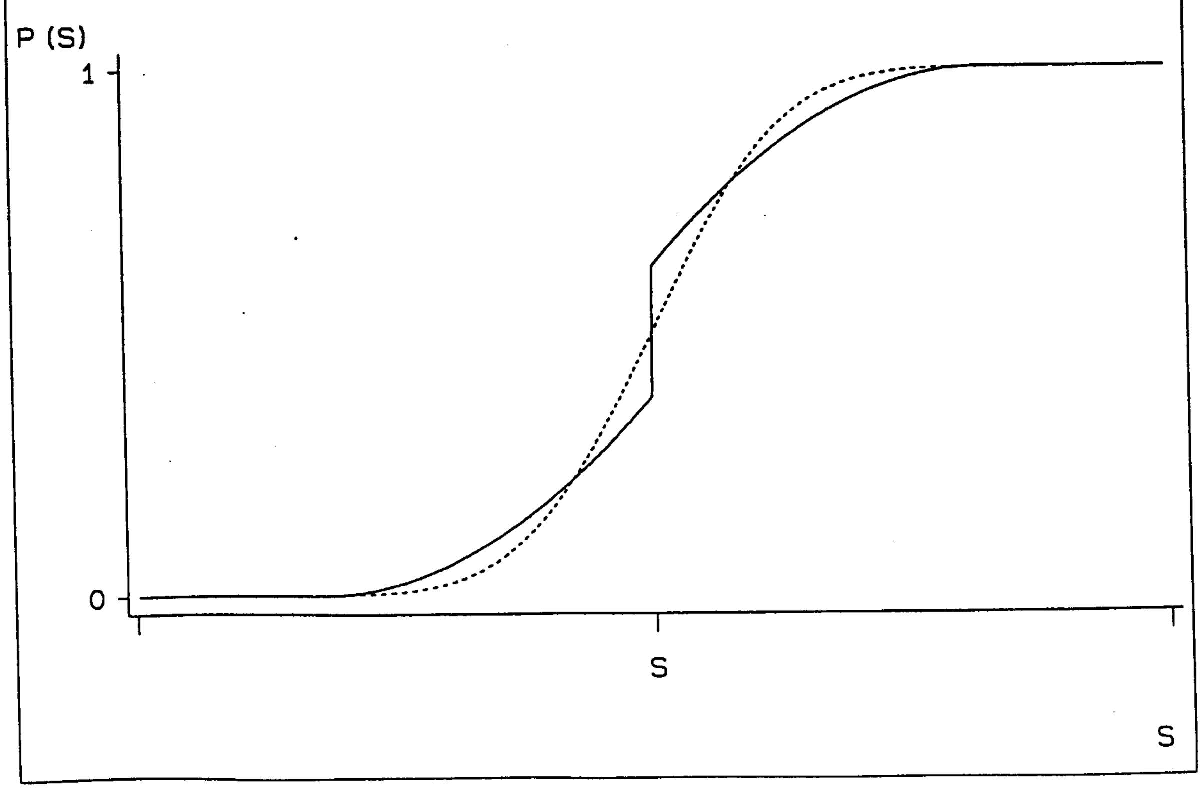


Fig. 3: Financial aid offers – Filers

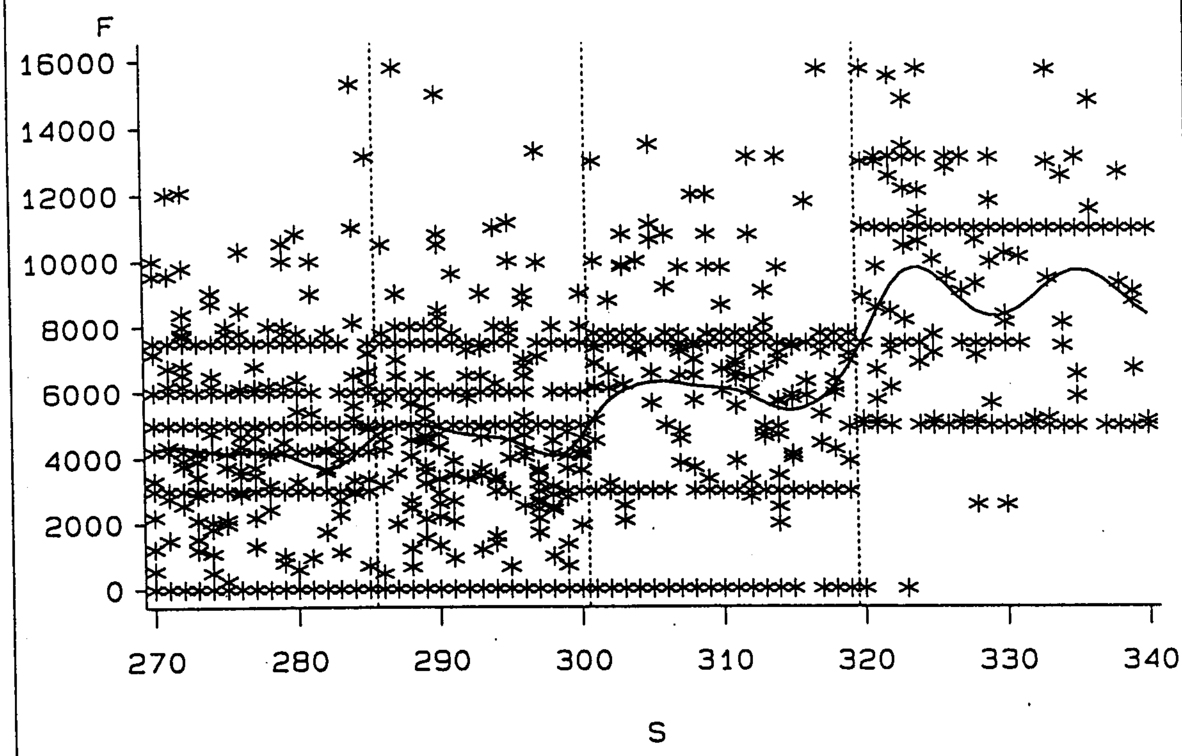


Fig. 4: Financial aid offers – Nonfilers

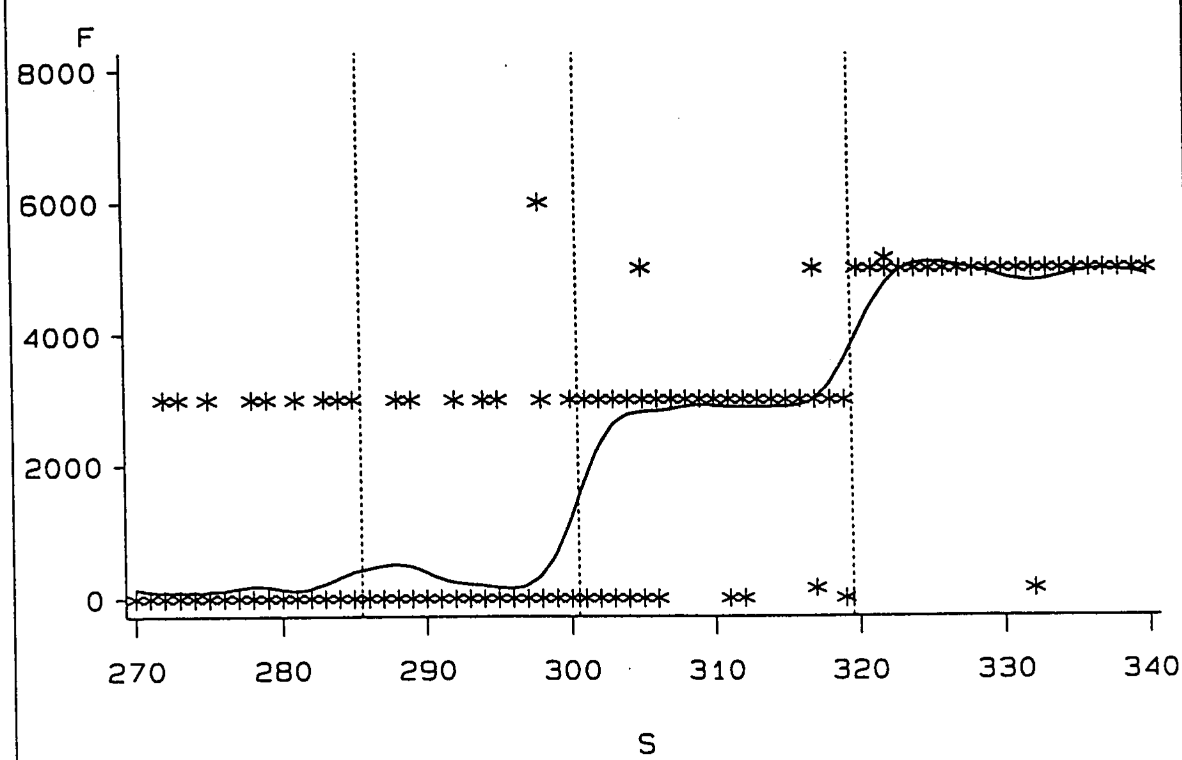


Fig. 5: Estimated financial aid functions – Filers

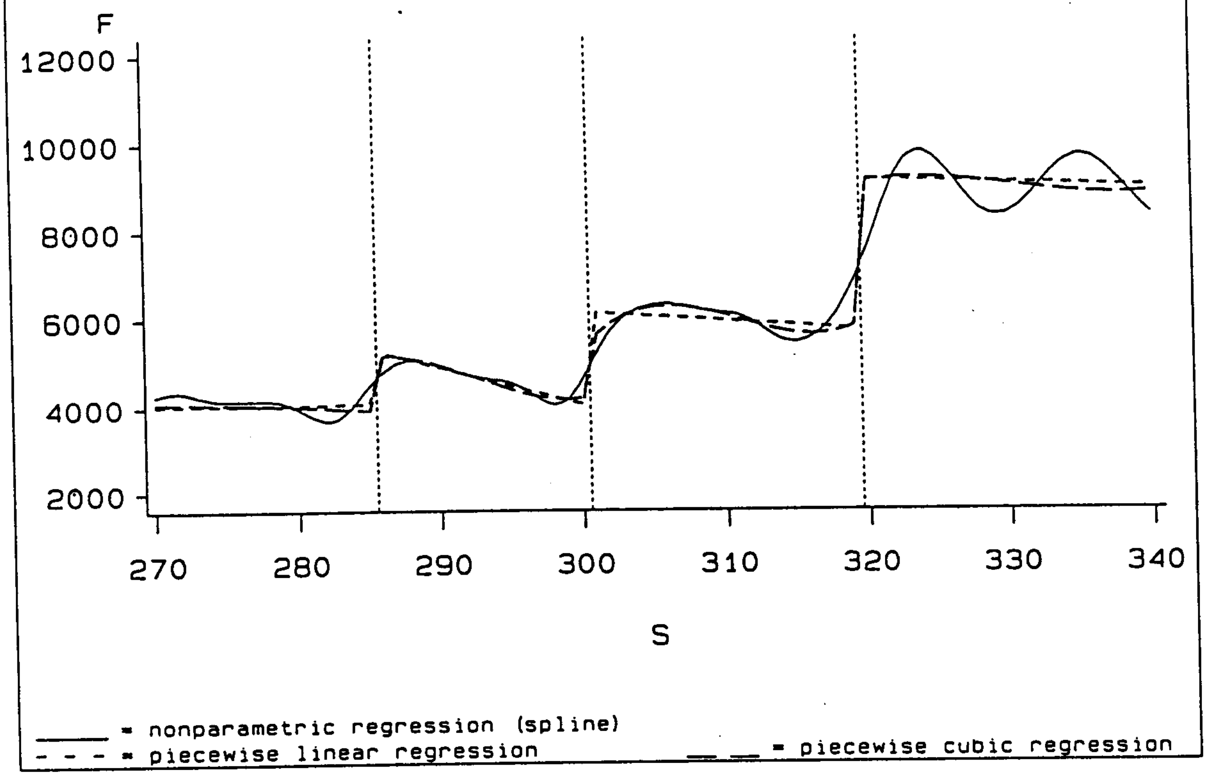


Fig. 6: Estimated financial aid functions – Nonfilers

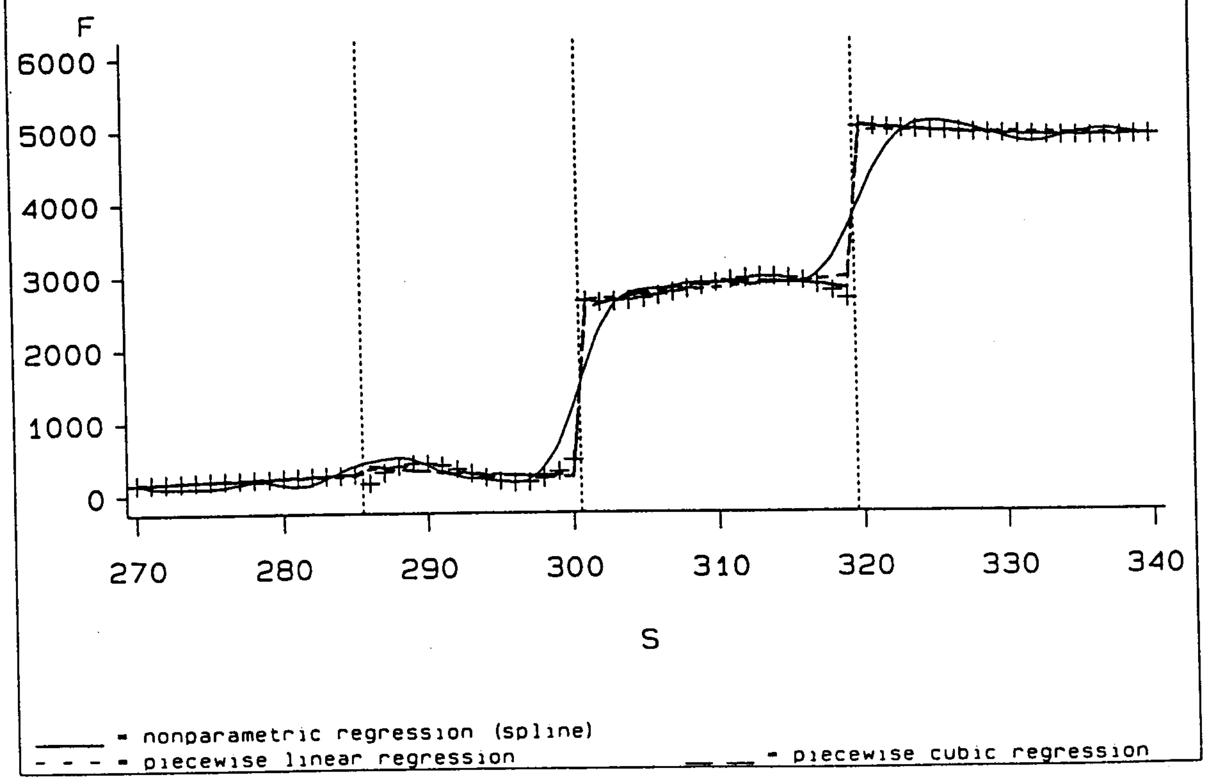


Fig. 7: Proportion enrolled – Filers

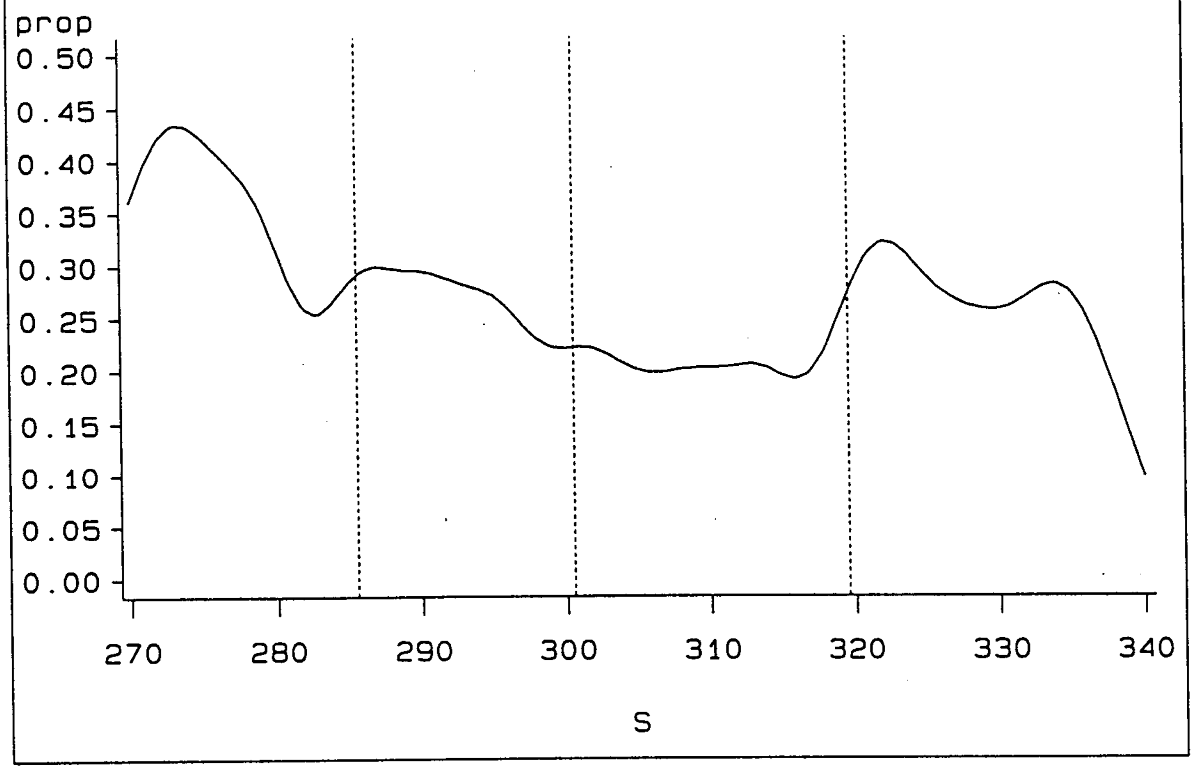
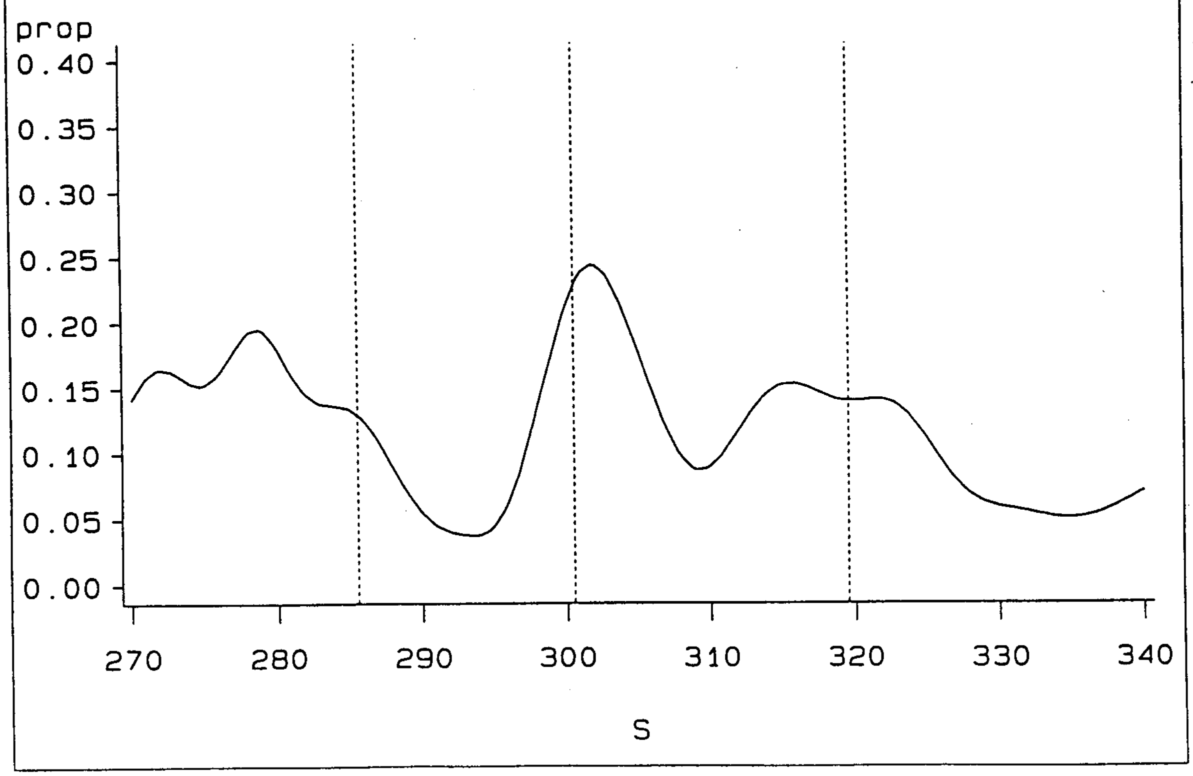


Fig. 8: Proportion enrolled – Nonfilers



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