

Do no-loan policies change the matriculation patterns of low-income students?

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Abstract

Using the universe of Pell Grant recipients, we empirically examine whether there is discernable variation in the matriculation patterns of low-income students around changes in institutional financial-aid policies that targets low-income students with need-based aid. We find no enrollment response attributable to these programs. Yet, we find that institutions that introduce these programs subsequently enroll financially needier and geographically more distant students. These findings may imply that “improved” access may actually displace some needy students in favor others.

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JEL classification: I23, I21, J24

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1 Introduction

In the preceding decade, over thirty of the top post-secondary institutions in the United States have formally introduced financial-aid programs that explicitly target students from low-income families. Within what might constitute a general move toward relaxing budgetary constraints on low-income students with targeted aid, there are a variety of methods by which this has administered. Four typical allocation mechanisms are “no loan” policies that eliminate loans for low-income students (e.g., Princeton, Michigan, Tennessee), “loan cap” policies that institute a low cap on student loans for low-income students (e.g., Brown), “no parental contribution” policies that eliminate the parental contribution but retain the student contribution or the standard self-help level (e.g., Yale, Stanford), and “Pell Grant match” policies that match the student’s Federal Pell Grant (e.g., Minnesota).¹ At public flagship institutions, these aid programs have typically been of the type that replaces loans with grants through a “no loan” policy.

In order to consider the efficacy of such initiatives, we exploit data acquired on the universe of dependent Pell Grant recipients in academic years 1999 through 2007. While there are important efficacy issues that will remain outside of our immediate focus, as a general approach we will think of efficacy being evidenced in three ways.

First, we consider efficacy as evidenced in institutional enrollments. Establishing the nature of any link between these programs and enrollment is, of course, of fundamental interest if not fundamental importance. This is particularly true as the extant literature often implies that this population of students is quite inelastic with respect to aid (e.g., Curs, Singell, and Waddell (2007)). Where conditions allow for a valid test – which we will discuss with some detail below – we find no systematic enrollment responses that can be explained by the institutions’ policy innovations.

Second, we consider efficacy as evidenced in the *ex poste* incomes of matriculating-

¹Some variation still exists within such groups. For example, Princeton has eliminated loans from the aid packages of all students.

students' families. Without family-income data, we will analyze the expected family contributions (*EFC*) of matriculating low-income students. *EFC* is determined formulaically within the Federal Pell Grant Program and is a function of student attributes (e.g., increasing in family income, home equity, number of dependent children in the student's family) and institutions' costs of attendance. While we find no relative increase in the number of low-income students enrolling, we establish that the relative wealth of those who matriculate to institutions with their introduction of targeted-aid programs varies systematically with the policy innovations. In short, from among matriculating Pell students, there is evidence that these aid innovations have led to a relative shift toward lower-income Pell students at aid-innovating institutions.

Third, we consider efficacy as evidenced in institutions' geographic basins of attraction. Here we are asking whether these policies change the geographic dispersion of the low-income students they successfully enroll. As other associated costs of matriculation are unchanging over the period of our analysis, we will be inclined to interpret any increase in an institution's basin of attraction as akin to them being better able to draw in low-income students. That is, with an objective of aid policies being to facilitate the matching of needy students with institutions, any increase in an institutions basin of attraction is consistent with prior constraints on the matriculation of low-income students being relaxed. We show that these "basins of attraction" do change, with enrolling low-income students tending to travel relatively farther distances to aid-innovating institutions with the policy changes. We interpret this evidence as suggestive that aid policies such as those analyzed can relax geographic barriers to the enrollment of low-income students. While evaluating the match itself remains outside our purview, we expect that such movement is in the direction of improving the potential matching of students with institutions.

In the following section we will address existing literature insofar as it provides context for our analysis. In Section 3, we then set up our empirical test given the changes to financial-aid policy that fall within the period of data availability. In the process of doing so,

we will identify our exact identification strategy and the way in which one should consider “treatment” and “control” groups for the purpose of the analysis. In defining this test, we will also address the inappropriateness of alternative specifications and set up a falsification exercise that we will perform along side our examined relationships throughout the paper.

In Section 4, we describe the data used in our analysis and provide some summary statistics that are new to the literature given our data – the entire “Pell recipient file” of the United States Department of Education. We then devote separate sections to each of the relationships we examine; we model the enrollment response to the adopted aid initiatives in Section 5, we model changes in expected family contributions around adopted aid initiatives in Section 6, and in Section 7 we examine the proximity of students to institutions. We then share some concluding remarks and discuss several important questions that remain unanswered.

2 Literature

Despite significant resources being spent on need-based financial aid in the United States, the gap between low- and high-income students’ matriculation rates has not only persisted but has widened in the last three decades (Ellwood and Kane (2000)). Thus, understanding matriculation patterns is not unimportant, especially given the suggestion from existent literature that enticing an otherwise non-college bound, low-income student to matriculate to college with federal aid is not easily accomplished.² Knowing this, we continue to see large transfers toward need-blind admissions and “all need met” policies.

²Enrollment effects in general populations of students have been weak (e.g., Hansen (1983); Kane (1994); Kane (1995); Heller (2004); McPherson and Schapiro (1998); Kane (2001)). There is evidence of Pell influencing more-narrowly-defined groups of institutions or students. For example, Kane (1995), although not separating needy from non-needy students, suggests that Pell increases overall enrollment at public two-year colleges. Seftor and Turner (2002) report increased access for non-traditional students, using variation in the Pell-eligibility formula in the late 1980s that increased the generosity of the program for financially independent students.

In part, income-targeted need-based programs have grown out of recognition on the part of policy pundits and university administrators that the rising real cost of college and student debt level have potentially threatened the access of low-income students to college. For example, Mishel, Bernstein, and Allegretto (2007) find that the real wage premium rose by 27 percent between 1993 and 2005, whereas *Trends in College Pricing 2005* indicate that real tuition and fees rose by 63 (43) percent at private (public) universities.³ Snyder, Tan, and Hoffman (2006) show that, commiserate with the rising real cost of college, students are: more likely to require student aid to attend college; are covering a smaller portion of their college costs with grants; and, taking out nearly twice the level of debt in real terms than in the previous decade (i.e., over \$20,000 in 2004). Because federal need-based aid programs have not kept pace with the growing costs of college (e.g., McPherson and Schapiro (1998)), a growing number of institutions and some states have attempted to bridge the funding gap in an attempt to ensure the accessibility of college to needy students.

Yet, few studies have examined whether these targeted need-based aid programs actually improve access. Most recent work in the area has exploited natural experiments in federal, state, and institutional aid programs to focus on whether an exogenous increase in financial aid has influence on the likelihood that students apply, enroll, and graduate from college (e.g., Angrist (1993); Dynarski (2000); Bound and Turner (2002); Long (2004); Cornwell, Mustard, and Sridhar (2006); Abraham and Clark (2006)). This literature speaks to the potential efficacy of the targeted-aid programs we focus on here. However, while there is generally consistent evidence that improved generosity of financial aid improves some college outcomes, effects are often relatively small in magnitude (e.g., Long (2004); Dynarski (2004); Bettinger (2004); Singell, Waddell, and Curs (2006)). Our analysis builds on this literature, examining how institutional “no loan” programs might assist the low-income population along several dimensions of access.

Two recent studies have examined the efficacy of targeted need-based aid programs, both

³See *Trends in College Pricing 2005*, The College Board (Table A1).

using institution-specific data for two highly selective schools. In general, they suggest that credit constraints play an important role in the college behavior of low-income students.

First, Avery et al. (2006) uses administrative and Census data to evaluate the first year of Harvard University’s Financial Aid Initiative (HFAI), which increased aid to low-income students. Using estimated family incomes for “plausible U.S. applicants,” they find that HFAI attracted a larger and slightly poorer pool of applicants and that, once admitted, enrolled at rates similar to the prior year. This suggests that HFAI was effective at recruiting low-income students due to some otherwise-untapped supply of qualified students who would not have applied to Harvard in the absence of such aid. However, it is unclear the extent to which Harvard’s program merely expands their pool of high-quality, low-income students at the expense of other selective institutions.

Second, Rothstein and Rouse (2007) uses data for a selective university that adopted a no-loan policy under which the loan component of financial aid awards was replaced with grants. Using this natural experiment to assess the causal effect of student debt on employment outcomes, they find that that debt affects students’ academic decisions during college and can be associated with graduates choosing higher-salary jobs and not choosing poorly paid “public-interest” jobs. Furthermore, suggestive evidence is provided that credit constraints and debt aversion interfere with a student’s ability to optimize over the life cycle. For example, debt is found to reduce students’ donations to the institution in the years after they graduate and increases the likelihood that graduates default on pledges made in their senior years.

3 Identification

Table 1 identifies the set of institutions at which income-targeted initiatives were implemented within our sample period of 1999 through 2007. While there are thirty assistance programs of this type introduced over this period, our identification strategy has us exploit

only a subset: those programs initiated at public institutions that limited potential beneficiaries to their own state residents. That this would define our strategy is revealed in considering the actual experiment underlying any relevant efficacy test. In particular, note that treatment is not at the institution level but, rather, at the student level. By extension, where an institution does not limit the set of potential beneficiaries by some exogenous mechanism (e.g., state of residence) there is no well-defined group of comparable students against which one can measure the response to treatment. That is, where no low-income students are excluded from treatment the “control group” is empty.⁴ Private institutions have not limited the scope of their initiatives and will therefore not contribute to our test.⁵

In testing the efficacy of targeted-aid programs, then, our strategy exploits the existence of state borders and the public-university practice of offering income-targeted aid only to residents within their respective states. We then measure the response of treated residents with respect to their own flagship against non-treated non-residents with respect to their flagship institutions.

To illustrate our strategy more specifically, consider a flagship public institution in state i that newly introduces a program of financial assistance to low-income residents in state i who matriculate to the state’s flagship. Low-income residents of this state may or may not respond to the policy (which is what we aim to determine). However, as residents of state $j \neq i$ (i.e., nonresidents from i ’s perspective) are excluded from the institution’s offer, we expect no direct response from j ’s residents.⁶ In fact, residents of state j face the

⁴As state flagships “treat” all resident students in the state, there is likewise no within-state experiment to be run. In general, one could imagine a regression-discontinuity approach to the research question, using students around the margin of receiving financial support, or, around the margin of admission based on non-financial consideration. See van der Klaauw (2002) for an example of a regression-discontinuity approach to examining the relationship between financial aid and enrollment.

⁵There are four public flagship institutions that also introduce targeted aid but do not limit the potential recipients exogenously (i.e., North Carolina (adopted in 2003), Virginia (2004), Michigan State (2006), Maryland (2007)). We drop these institutions and their associated students. For completeness, we also note that it is not discontinuities in costs of attendance by residency status that enables this test.

⁶Indirectly, they may appear to have responded if, for example, in-state enrollments rise

same relative margins in their matriculation decisions before and after the introduction of targeted aid in state i . Moreover, in the absence of the flagship institution in state j initiating a similar program, we would expect no behavioral response whatsoever from j 's residents, toward their own flagship institution in particular. It is in this way that we adopt low-income resident of all states $j \neq i$ as a control group against which to measure the response of i 's low-income students to the policy change at i 's flagship. We will therefore draw claims of efficacy only to the extent that i -students' patterns of matriculation change with the policy change at i 's flagship institution *relative to* the observed patterns of j -students' matriculation to j 's flagship. The experiments we perform are therefore comparisons of the matriculation patterns of low-income students when their own public flagship institution initiates a change in policy to the matriculation patterns of low-income students in states where no such initiative is adopted.

– Table 1 approximately here –

Before we continue, note that the practice of distinguishing residents from nonresidents in the allocation of financial assistance provides an opportune falsification exercise. Quite simply, if there is a significant response from residents of j to the introduction of income-targeted aid at the flagship institution in state i we would be forced to conclude either that there were unobservable time-varying attributes driving matriculation patterns more generally at institution i (and not just of the type for which we expect to see response only from in-state students) or that the experiment was altogether invalid. We therefore perform falsification tests through a comparison of the matriculation patterns of *nonresident* low-income students at flagships that initiate relevant policies to the matriculation patterns of *nonresident* low-income students at other flagships that do not. As nonresident students sufficiently to crowd out nonresident students. However, to the extent that nonresidents (i.e., students who do not reside in i) are crowded out by any increase in resident matriculants to i , the bias will be in favor of finding no effect. For example, as students in i respond to i -specific policy and crowd out students from j , we would expect j 's flagship to gain resident students *relative to* i 's flagship institution, which attenuates our findings.

are not being treated by the policy, there should be no systematically occurring relative difference in the realized matriculation patterns at treated flagship institutions from this group of students.⁷

4 Data

Our primary data source is the Pell “recipient file” held by the offices of the U.S. Department of Education, obtained through our request under the Freedom of Information Act (FOIA). While the dataset includes all Pell recipients over the academic years 1999 through 2007 (at roughly six million observations per year), we use only those students who are recorded as first-time, dependent recipients in their first year at one of the 70 official state flagship institutions. Independent students are dropped from the analysis as they are recognized by the literature as having different attributes and patterns of behavior (e.g., Seftor and Turner (2002)). We also drop student-level observations where we are unable to match them with an institution within the IPEDS dataset using the mapping of Pell-provided institution code to IPEDS institution code. We are confident, however, that this problem is primarily one that exists outside of the well-established institutions we analyze here. Moreover, as this type of data issue would keep entire institutions from appearing in our final sample, that we have the entire set of state flagships is consistent with having no missing students at these

⁷In terms of potential bias in the falsification exercise, note that to the extent residents of i (i.e., part of j 's nonresident population) might forgo an out-of-state alternative when they respond to i 's policy (e.g., they forgo the flagship in state $j \neq i$), the bias will be in favor of finding relative differences in matriculation patterns between treatment and control. For example, as students in i respond to i -specific policy, j 's flagship may well lose nonresident students *relative to* treated flagship institutions, which would yield a positive coefficient on the treatment variable in the nonresident falsification model. We will therefore interpret finding no significant relative difference as secondary evidence of the efficacy of the targeted-aid programs.

institutions.^{8,9}

To receive federal aid, a student must first complete a Free Application for Federal Student Aid (FAFSA) form, which provides aid administrators with the information needed to determine the size of an applicants Pell Grant. Related research has relied on indirect measures for the number of low-income students, such as minority enrollments or other student background measures that are correlated with income (e.g., Kane (1994); Dynarski (2004)).¹⁰ To the contrary, our analysis exploits unique student-level Pell data to directly examine the effects of changes in aid policy on low-income students. Furthermore, the program size ensures that to a large extent our data constitute the population of poor students attending U.S. higher-educational institutions.

5 Enrollment response

In order to accommodate the policy implementation across multiple time periods, we set up the following difference-in-difference model with a full set of time-period indicators and a policy indicator defined to be unity for institutions and time periods that are subject to the policy.

As a general framework, then, we are interested in the estimate of β in the following time-series/cross-section model:

$$\ln(LIenroll_{it}) = \mathbf{x}_{it}\delta + T_{it}\beta + \alpha_i + \gamma_t + \epsilon_{it}, \quad (1)$$

⁸We note that the student counts provided in the “recipient files” of the Department of Education are a perfect match with the administrative records of our own institution, the University of Oregon.

⁹As time-invariant institutional attributes are absorbed into the error structure, we have not integrated the detailed institution-level information from the Post-Secondary Education Data System (IPEDS). However, such a merge was performed and no significant patterns of behavior are found. Given student-location coordinates, our data were also supplemented with US Census data (e.g., standard zip-level aggregates on income, population, and educational measures) with no clear discernible patterns.

¹⁰See Curs, Singell, and Waddell (2007) for a more comprehensive review of the related literature and a summary of the history and chronology of the Pell program.

where $LIenroll_{it}$ captures the low-income enrollment at institution i in academic year t (which we measure as the number of Pell recipients), and T_{it} is the treatment variable, defined to be unity for institutions and time periods (i, t) that are subject to the treatment. The model includes a full set of institution effects, α_i , and a full set of time effects, γ_t . As institution fixed effects will not account for other time-varying factors that influence $LIenroll_{it}$, \mathbf{x}_{it} includes the log-cost of attendance (e.g., tuition, fees, etc.) and the state-level log-population of first-year, low-income students enrolled in four-year institutions within each state. Institution-specific errors are captured in ϵ_{it} . An identifying assumption of the above model is that low-income students in treatment and control groups are on similar trends in low-income enrollment before the introduction of targeted aid. We find no evidence of differential trending.¹¹

In Column (1) of Table 2 we report the estimated coefficients of Equation (1), which operates as an empirical test of the efficacy of targeted-aid programs as measured by the total enrollment of resident low-income students. With respect to the treatment variable, the point estimate suggests that targeting low-income students has not helped this population in terms of enrollment – the point estimate is negative. Given the degree of noise in the estimated coefficient, however, we conclude that while targeted-aid policies cannot be credited with attracting needy in-state students to state flagship institutions, there is insufficient evidence to claim that the policies have been detrimental. Prior work has found that enrollment increases due to the District of Columbia Tuition Assistance Grant Program (that allows D.C. residents to attend public colleges and universities throughout the country at considerably subsidy) were concentrated at less-selective institutions with no decrease found at more-selective schools (i.e., Abraham and Clark (2006), Kane (2007)).

We report the estimates from the falsification exercise in Column (2) of Table 2, where we

¹¹Differential pre-treatment trends introduce bias into the estimated treatment coefficient. For example, if resident, low-income enrollment was rising faster at treated flagship institutions then this methodology would falsely attribute any continuation of this trend to the targeted-aid programs themselves. Modeling $\ln(LIenroll_{it})$ as cubic in t , we find no significant differences by treatment and control group in pre-treatment trends.

expect no significant predictive power in the treatment variable. While this specification reveals significant patterns with respect to costs of attendance and the number of Pell students in the population, as expected, the treatment variable remains insignificant in predicting the low-income enrollment of nonresident students. This is expected given that no such student is treated by the initiated policies.¹²

– Table 2 approximately here –

While not the focus of our investigation, Table 2 also highlights the importance of residency status in considering the elasticity of enrollment to costs of attendance. In fact, we find no enrollment response with (time-series) variation in costs among resident, low-income students, where non-resident enrollment increases significantly as costs increase, with a 10-percent increase in tuition yielding a 5-student increase in low-income enrollment at the mean enrollment level in the sample.¹³ The positive elasticity suggests that it may be difficult to separate price effects from the correlation of price and quality (either real or perceived, since time-invariant institutional heterogeneity is absorbed in the error structure of the model)

¹⁴ An alternative explanation that is possible where institutions enjoy excess demand from out-of-state students and are in search of higher revenues they may admit more nonresident students while raising nonresident tuition. The state-level population of low-income students attending four-year institutions has the expected sign, and magnitudes that seem quite plausible given the selectivity of the institutions within our sample (i.e., elasticities of roughly 1).

¹²Cast differently, one might be inclined to model the (untreated) nonresident enrollment as a kind of difference in difference in difference specification. If the two samples are pooled, with the resident low-income enrollment response to treatment also measured against the “response” on nonresident low-income enrollments, there is the suggestion that enrollment increases with treatment.

¹³Mean nonresident low-income enrollment at flagship institutions in 1999 is 132 students.

¹⁴We will return to this point when considering the distance models, where we learn more about the underlying patterns by considering the price effect on across the student-proximity distribution. (In short, the price effect (on distance quartiles) reverses sign (i.e., positive to negative) as one considers students farther from the institution.).

6 Family income

As in the enrollment model above, our empirical objective is to capture any changes in expected family contributions (*EFC*) that systematically relate to the timing of “no-loan” policy implementation, using untreated students as untreated “control” observations. With *EFC*, however, the implementation of such analysis is somewhat nontrivial. For example, *EFC* is determined formulaically within the Federal Pell Grant Program and is a function of student attributes (e.g., increasing in family income, home equity, number of dependent children in the student’s family) and institutions’ costs of attendance. In practice, however, no family is expected to contribute negatively and we only observe the student-specific *EFC* after the application of a non-negativity.

In Column (1) of Table 3 we report a conditional logit model of the form

$$Prob(EFC_{sit} > 0) = \mathbf{x}_{sit}\delta + T_{sit}\beta + \alpha_i + \gamma_t + \epsilon_{sit}, \quad (2)$$

where EFC_{sit} captures the expected family contribution of resident low-income student s at institution i in academic year t . In (2), T_{sit} is the treatment variable, defined to be unity for students and time periods that are subject to the treatment. Again, as in the enrollment model, we include a full set of institution indicators in α_i , a full set of time effects in γ_t , the log-cost of attendance (e.g., tuition, fees, etc.) and the log-population of first-year low-income students enrolled in four-year institutions in \mathbf{x}_{sit} . Student-level errors are captured in ϵ_{sit} . As was the case in Eq. (1), the identifying assumption of Eq. (2) requires that low-income students in treatment and control states trend similarly prior to the introduction of treatment. Finding no evidence of differential trending in the data, we can confirm the validity of this assumption.¹⁵

With a rather large dataset, where each group has many observations and there are

¹⁵Modeling $Prob(EFC_{sit} > 0)$ as cubic in t , we find no significant differences by treatment and control group in pre-treatment trends.

multiple positive outcomes per group, conditional logit can be a very difficult estimation to perform. For example, the `-clogit-` command in Stata uses an exact calculation to handle multiple positive outcomes. Thus, if there are 5,000 observations per group and 1,000 positive outcomes within a group, `-clogit-` must calculate how many ways 1,000 subjects can be drawn from a pool of 5,000. $\prod_{k=4,001}^{5,000} k$ exceeds that which can be recorded, even in double precision.¹⁶ As such, in the first column of results we will exploit the equivalence of the conditional logistic regression likelihood function and the Cox proportional hazard likelihood function.¹⁷ We also provide several alternative specifications as a transparent attempt to document that our results are not peculiar to this exercise.

The dependent variable is set to unity for all positive *EFC* and is otherwise zero, yielding coefficients on the treatment variable that are intended to capture the marginal change in the propensity for a low-income student at a given institution to have at least some financial assistance expected from the student’s family. The results of the baseline model in Column (1) indicate that there is a significant decrease in the probability that students will matriculate with the expectation of family assistance associated with the introduction of targeted aid programs of the type being considered. That is to say, low-income students matriculating to treated institutions are roughly 3.7-percent less likely to report positive *EFC*.¹⁸ Columns (2) through (4) of Table 3 provide these three alternative specifications of the underlying pattern.

In columns (2) and (3) we employ a linear probability model and a probit specification, respectively. As specified, there is nothing in the linear probability model bounding the

¹⁶Our models fail to converge using Stata on a Linux box with 16GB of RAM.

¹⁷Namely, `-stcox-` can be used to estimate `-clogit-` models and vice versa. While there is no reason to do so in general, `-stcox-` offers alternative methods by which one can handle the multiple positive outcomes within group. The default method it exploits is the Breslow approximation to the exact calculation, by which we are able to handle multiple positive outcomes in our data. A nice demonstration of equivalency is available at stayconsistent.wordpress.com.

¹⁸In the language of the Cox model, the hazard of failing (i.e., of *EFC* > 0 shifts down by 3.7 percent.

projected probabilities to the unit interval. Yet, institution fixed effects can be estimated without yielding bias. While not yielding the unbiased estimation of parameters of the Chamberlain procedure, a Probit specification with institution indicator variables does yield an estimate of the key parameter of interest that is not different from that of the linear probability model of Column (2). That the treatment coefficient estimate does not change across columns (2) and (3) suggests that neither the unboundedness of the OLS specification (where we have no incidental parameters problem) nor the potential bias from the incidental parameter problem in the probit specification (where we face an unbounded prediction problem) are cause for concern. Moreover, neither the sign nor economic significance – treated students being roughly 2.6-percent less likely to have levels of family wealth that yield positive *EFC* – raise concern that our preferred specification of Column (1) is to be questioned.

Given the nature of the Pell Program’s formulaic determination of need, *EFC* has a continuous component (which is increasing in family wealth, *ceteris paribus*) that has information of interest in considering the question of efficacy. However, the true expected family contribution is some latent variable, EFC^* , such that $EFC_{sit} = \max(0, EFC_{sit}^*)$. In Column (4), we report the estimated coefficients of a Tobit-equivalent to the specification in Eq. (2). Here again we see evidence that targeted aid has drawn students of relatively lower means, with the estimated coefficient on the treatment variable implying a 25-percent decrease in the underlying family contribution (i.e., EFC^*). We interpret this large increase as partly reflecting the mass at zero *EFC*.

The associated falsification exercises are reported in columns (1) through (4) of Table 4, which shows no such pattern in the data. That is, in each case a comparable specification on nonresident low-income students shows no systematic relationship around the introduction of these aid programs. Holistically, these four specifications and their accompanying falsification tests confirm that targeted aid programs of the nature adopted by flagship public universities lead to more-needy student attending these institutions. Given that the number of needy students does not increase at flagship institutions, which may be the result of explicit or

implicit capacity constraints, these findings may be seen as somewhat provocative as they can imply that “improved” access may actually displace some (relatively wealthy) needy students in favor of others.

– Table 3 approximately here –

– Table 4 approximately here –

7 Basins of attraction

Avery et al. (2006) and Rothstein and Rouse (2007) suggest that the types of programs under our analysis should permit a better matching of students with institutions. Here, we consider the geographic basins of attraction around each university in our sample of flagships – the area from which the institutions draw low-income students. In particular, we anticipate that to the extent credit constraints on low-income students are systematically relaxed with the introductions of additional aid opportunities, improved matching may well be exhibited in the distances from which students travel to attend the state’s flagship institution.

7.1 Student proximity

As before, for our analysis of basins of attraction, we set up the following difference-in-difference model with a full set of time-period indicators and a policy indicator defined to be unity for institutions and time periods that are subject to the policy.

As a general framework, we are interested in the estimate of β in the following model:

$$\ln(\text{Distance}_{sit}) = \mathbf{x}_{sit}\delta + \mathbb{T}_{sit}\beta + \alpha_i + \gamma_t + \epsilon_{sit}, \quad (3)$$

where Distance_{sit} captures the distance (km) between (resident, low-income) student s ’s home addresses and institution i in academic year t , and \mathbb{T}_{it} is the treatment variable, defined to be unity for students and time periods that are subject to the treatment by institution i .

As in the enrollment model, the distance models include a full set of institution effects, α_i , a full set of time effects, γ_t , and in \mathbf{x}_{sit} the log-cost of attendance (e.g., tuition, fees, etc.) and the state-level log-population of first-year, low-income students enrolled in four-year institutions. Student-level errors are captured in ϵ_{sit} .¹⁹

– Table 5 approximately here –

In Column (1) of Table 5 we report the estimated coefficients of Equation (3) which operates as an empirical test of the efficacy of targeted-aid programs measured in terms of the average proximity of resident low-income students (i.e., the distance between their home addresses and the institution). In short, the data reveal a significant increase in the relative ability of policy-innovating institutions to attract low-income students, with the point estimate suggesting the average resident low-income student travels 4.6-percent farther when treated. Recalling that there are no significant enrollment responses to treatment, these findings again seem to suggest that “improved” access may be displacing some needy students in favor of others. In Column (2), we report the estimates from the falsification exercise. As was the case in the earlier falsification attempts, we find no significant predictive power in the treatment variable. This is expected, of course, given that no such student is treated by the initiated policies.

7.2 Distance quantiles by targeted-aid regime

Figure 1 produces the kernel density estimates of low-income student proximity to their associated institutions in 2000. As can be seen in the figure, the distribution of campus proximity is skewed right.²⁰ In some sense, there is a smallness to the low-income-student

¹⁹As in earlier specifications, the identifying assumption here is again that low-income students in treatment and control states trend similarly prior to the introduction of treatment. Finding no evidence of differential trending in the data, we can confirm the validity of this assumption.

²⁰This pattern is expected and occurs in all sample years. In 2000, the median proximity was 66 km while the mean proximity was 118 km.

market at state flagships, with 25 percent of (first-year, dependent) Pell students at flagship institutions in our sample reporting their official residence (prior to matriculation) within 13.7 km of campus. In this way, it seems a valuable exercise to establish the robustness of the above proximity result to different points along the distribution, for these programs, by their nature, may well experience differential effects on students across the distance range. Thus, as a complement to the analysis of Equation 3, we set up a quantile-regression model with a particular interest in the robustness of the treatment effect to considering different ranges of the proximity distribution.

In Table 6 we largely establish that the proximity effect is realized over a large range of the distribution of proximity. In Figure 2 we report the coefficient estimate and confidence intervals corresponding to the treatment variable derived from equivalent specifications over the entire range of proximity quantiles. Overall, the clear implication is that there are significant and general increases in the basins of attraction where institutions have introduced income-targeted aid and not where aid regime was unchanged. The associated falsification exercises are reported separately in Table 7, which again reveal no predictive power in the treatment variable but for the interval around the 75th percentile.²¹

While these relative increases are not present in the matriculation patterns of (untreated) nonresident students, Table 7 does reveal an interesting pattern across the proximity distribution in the underlying behavior of low-income students around changes in *COA*. Note that the price effect (on distance quartiles) reverses sign over higher distance quantiles. Given that the dependent variable is not enrollment, we are somewhat cautious but nonetheless inclined to interpret this in light of the earlier nonresident enrollment result – *COA* being positively related to low-income nonresident enrollment levels after absorbing time-invariant institution effects into the error structure. Clearly, *COA* positively influences the 10th-quantile distance yet positively influences distance at or above the median. As the confounding of

²¹While proportional measures of treatment response suggest a monotonically decreasing effect throughout the range of distances, we note that the absolute distances implied by the estimates is inverted-U shaped.

price and quality effects are often suggested as a reason for the positive correlation of price and enrollment, this can be viewed as a challenge to the often-implied prior those closer to the institution are better informed about the true quality. One interpretation of this quantile-dependent *COA* pattern is that the more geographically distant is a student, the greater are the student's individual costs to attending and the greater their incentive to determine quality independent of price. (Price then has the effect of decreasing market size.) We thus allow that institution controls may be insufficient to capture time-varying student attributes.

– Table 6 approximately here –

– Table 7 approximately here –

8 Conclusion

We use unique data on all (first-time, first-year, dependent) Pell students between 1999 and 2007 that constitutes essentially the population of poor students entering higher education over this period to examine the access effects of the introduction of targeted, need-based aid programs available to in-state students at several flagship public institutions. In particular, we use a series of difference-in-difference regressions to examine whether institutions that adopted targeted need-based aid programs for in-state students experienced any systematic increase in the number of Pell students, the income of matriculating Pell students or their proximity to campus.

Our analysis strongly suggests that target-aid programs influence the behavior of needy students, but in a subtle way. Specifically, the enrollment models generate negative but insignificant point estimates of enrollment gains from adopting a targeted need-based aid program. At the same time, models of effective family contribution and proximity (as measured by the distance between the institution and their official residence prior to matriculation) indicate that institutions that introduce these programs subsequently enroll more-

financially-needy and more-geographically-distant students. Quantile regressions also indicate that these aid programs generate a non-linear (i.e., an inverted ‘u’) effect on proximity with maximum effect well below the mean distance. Thus, targeted need-based aid programs appear not to change the overall number of needy students, but instead change the profile and composition of students towards students who historically may have had less access to these institutions.

As a falsification exercise, we replicate these analyses using nonresident Pell students who are ineligible for the targeted need-based aid. We find no systematic responses from nonresident, low-income students, which supports the assumption that there are no unobservable time-varying attributes driving matriculation patterns more generally.

These findings are important as they suggest that institutions can influence access by using need-based aid but that Pell students are responsive to financial incentives, to the contradiction of much of the prior literature (e.g., Manski and Wise (1983)). On the other hand, these findings are also provocative insofar as they suggest that the number of needy students does not increase with no-loan policies at flagship institutions. Capacity constraints may well imply that “improved” access in a matching sense may actually displace some needy students in favor of others. Collectively, our findings are consistent with Rothstein and Rouse (2007) insofar as increased basins of attraction are suggestive that need-based aid programs reduce credit constraints. Our findings are also consistent with the Avery et al. (2006) hypothesis that these programs permit students to better match within the hierarchy of institutions. While it is possible that improved access and consequent displacement could work towards a need-blind process where needy students are appropriately matched within the hierarchy of institutions, further work must be done to understand the full general-equilibrium enrollment and access effects of such programs.

Table 1: **Institutions introducing income-targeted financial aid, 1999-2007**

Institutions, with year of adoption in parentheses.

Institutions adopting exogenously restricted aid initiatives	Minnesota (2005)
	Tennessee (2005)
	Michigan (2006)
	Florida (2006)
	Indiana (2007)
	Washington (2007)
	Illinois (2007)

Other institutions adopting income-targeted financial aid	Brown (1999)
	Princeton (2001)
	North Carolina (2003)
	Virginia (2004)
	Rice (2005)
	Pennsylvania (2005)
	Stanford (2006)
	Yale (2005)
	Swarthmore (2006)
	MIT (2006)
	Michigan State (2006)
	Amherst (2007)
	Columbia (2007)
	Davidson (2007)
Maryland (2007)	

Table 2: **Institution-level enrollment of low-income students**

The dependent variable is equal to the log of low-income enrollment (i.e., which we measure as the number of Pell recipients) at institution i in academic year t . All specifications include a full set of institution effects and a full set of time effects over annual institution-level observations.

	Resident enrollment	Nonresident enrollment (falsification)
	(1)	(2)
Treatment period	-0.060 [0.052]	-0.136 [0.096]
ln(Cost of attendance)	-0.040 [0.080]	0.522*** [0.15]
ln(Resident Pell population)	1.066*** [0.092]	0.184 [0.17]
ln(Nonresident Pell population)	2.617 [3.36]	-6.035 [6.25]
Constant	-36.18 [43.7]	75.80 [81.1]
Institution controls	Yes	Yes
Year controls	Yes	Yes
Observations/Institutions	630/70	630/70
R^2	0.32	0.06

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in brackets.

Table 3: Patterns of expected family contributions around the adoption of “no-loan” policies

In columns (1) through (3), the dependent variable is equal to one if the student’s expected family contribution (*EF**C*) is positive (i.e., a family contribution is expected to be made) by student *s* at institution *i* in academic year *t*. Estimated logit^{*a*} coefficients are reported in Column (1), estimated coefficients are reported in Column (2) and estimated probit marginal effects are reported in Column (3). In Column (4), the dependent variable is equal to student *s*’s *EF**C* at institution *i* in academic year *t*, continuous for positive values and left-censored at zero. All specifications include a full set of institution and time indicators.

	FE Logit	Linear probability	Probit	Tobit
	(1)	(2)	(3)	(4)
Treatment Period	-0.038*** [0.014]	-0.026*** [0.005]	-0.026*** [0.00544]	-0.253*** [0.0577]
ln(Cost of attendance)	-0.024*** [0.009]	-0.016*** [0.004]	-0.017*** [0.004]	-0.157*** [0.039]
ln(Resident Pell population)	-0.100*** [0.032]	-0.061*** [0.012]	-0.057*** [0.012]	-0.857*** [0.131]
ln(Nonresident Pell population)	2.214*** [0.971]	1.305*** [0.351]	1.244*** [0.353]	14.00*** [3.887]
Constant		-15.57*** [4.628]		-167.6*** [50.79]
Institution fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations / Institutions	409,565/70	409,565/70	409,565/70	409,565/70

Standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. ^{*a*} As discussed in the text, we exploit the equivalency of the Cox proportional hazard likelihood and the conditional logit likelihood in achieving convergence.

Table 4: Falsification tests for patterns of expected family contributions around the adoption of “no-loan” policies

In columns (1) through (3), the dependent variable is equal to one if the student’s expected family contribution (*EFC*) is positive (i.e., a family contribution is expected to be made) by student *s* at institution *i* in academic year *t*. Estimated logit^a coefficients are reported in Column (1), estimated coefficients are reported in Column (2) and estimated probit marginal effects are reported in Column (3). In Column (4), the dependent variable is equal to student *s*’s *EFC* at institution *i* in academic year *t*, continuous for positive values and left-censored at zero. All specifications include a full set of institution and time indicators.

	FE Logit (1)	Linear probability (2)	Probit (3)	Tobit (4)
Treatment Period	-0.003 [0.020]	-0.003 [0.012]	-0.004 [0.012]	-0.070 [0.119]
ln(Cost of attendance)	0.096*** [0.018]	0.065*** [0.007]	0.063*** [0.007]	0.717*** [0.070]
ln(Resident Pell population)	-0.090 [0.068]	-0.060** [0.026]	-0.057** [0.026]	-0.822*** [0.266]
ln(Nonresident Pell population)	-0.654 [3.068]	-0.524 [1.141]	-0.628 [1.145]	-9.852 [11.88]
Constant		7.330 [14.94]		131.4 [154.3]
Institution fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations / Institutions	84,514/70	84,514/70	84,514/70	84,514/70

Standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1. ^a As discussed in the text, we exploit the equivalency of the Cox proportional hazard likelihood and the conditional logit likelihood in achieving convergence.

Table 5: **Proximity responses to targeted financial aid at flagship public institutions**

The dependent variable is equal to the log of the distance (km) between student s 's home address and institution i of enrolling low-income students in academic year t . All specifications include a full set of institution effects and a full set of time effects, over annual institution-level observations.

	Resident proximity	Nonresident proximity (falsification)
	(1)	(2)
Treatment period	0.046*** [0.016]	0.033 [0.028]
ln(Cost of attendance)	-0.004 [0.010]	-0.142*** [0.017]
ln(Resident Pell population)	0.594*** [0.035]	-0.038 [0.064]
ln(Nonresident Pell population)	5.098*** [1.04]	-4.035 [2.82]
Constant	-66.94*** [13.6]	59.74 [36.7]
Institution controls	Yes	Yes
Year controls	Yes	Yes
Observations/Institutions	409,564/70	84,514/70
Overall R^2	.08	.01
Distance (km)	116.1	967.6

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in brackets.

Table 6: Resident proximity-responsive to targeted financial aid at proximity quantiles

The dependent variable is equal to the log of the X^{th} distance quantile between home address and institution i of enrolling low-income students in academic year t , where X is given by the column heading. All specifications include a full set of institution effects and a full set of time effects, over annual institution-level observations.

	10th (1)	25th (2)	Median (3)	75th (4)	90th (5)
Treatment period	0.253*** [0.022]	0.144*** [0.018]	0.058*** [0.010]	0.009** [0.0039]	-0.007** [0.0032]
ln(Cost of attendance)	-0.207*** [0.017]	-0.082*** [0.013]	0.006 [0.0069]	0.004 [0.003]	-0.001 [0.002]
ln(Resident Pell population)	0.496*** [0.049]	0.387*** [0.041]	0.378*** [0.023]	0.183*** [0.0088]	0.078*** [0.0071]
ln(Nonresident Pell population)	8.905*** [1.44]	3.432*** [1.22]	2.295*** [0.69]	1.606*** [0.26]	0.740*** [0.21]
Constant	-115.7*** [18.8]	-43.90*** [15.9]	-28.59*** [9.04]	-17.66*** [3.40]	-5.080* [2.71]
Institution controls	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	Yes
Observations	409,564	409,564	409,564	409,564	409,564
Pseudo R^2	0.12	0.17	0.20	0.23	0.21
Distance (km)	3.6	14.9	64.5	167.5	292.5

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in brackets.

Table 7: Nonresident proximity-responsive to targeted financial aid at proximity quantiles

The dependent variable is equal to the log of the X^{th} distance quantile between home address and institution i of enrolling-low-income students in academic year t , where X is given by the column heading. All specifications include a full set of institution effects and a full set of time effects, over annual institution-level observations.

	10th (1)	25th (2)	Median (3)	75th (4)	90th (5)
Treatment period	0.013 [0.018]	0.020 [0.015]	0.007 [0.018]	0.062*** [0.017]	0.017 [0.023]
ln(Cost of attendance)	0.026** [0.011]	-0.006 [0.009]	-0.049*** [0.010]	-0.045*** [0.010]	-0.087*** [0.013]
ln(Resident Pell population)	0.019 [0.041]	-0.015 [0.034]	-0.013 [0.040]	-0.048 [0.037]	0.012 [0.049]
ln(Nonresident Pell population)	-0.330 [1.81]	-2.130 [1.54]	-3.766** [1.78]	-2.855* [1.67]	0.858 [2.12]
Constant	8,497 [23.6]	33.18* [20.0]	54.97** [23.1]	44.32** [21.6]	-3.099 [27.5]
Institution controls	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	Yes
Observations	84,514	84,514	84,514	84,514	84,514
Pseudo R^2	0.29	0.26	0.22	0.21	0.18
Distance (km)	66.5	184.8	453.5	1,171.9	2,973.1

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in brackets.

Figure 1: **The proximity of resident, low-income students**

This figure reproduces kernel density estimates of the (km) distance between each state's flagship institution(s) and the reported residences of first-year Pell students attending their own-state flagship institutions in 2000 as residents.

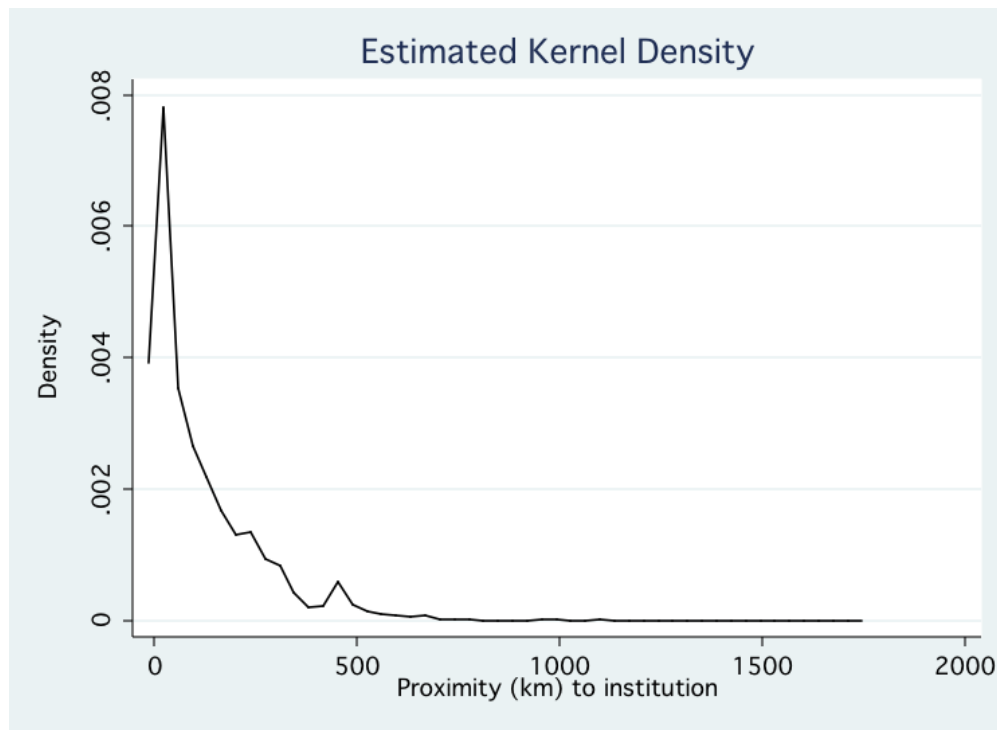
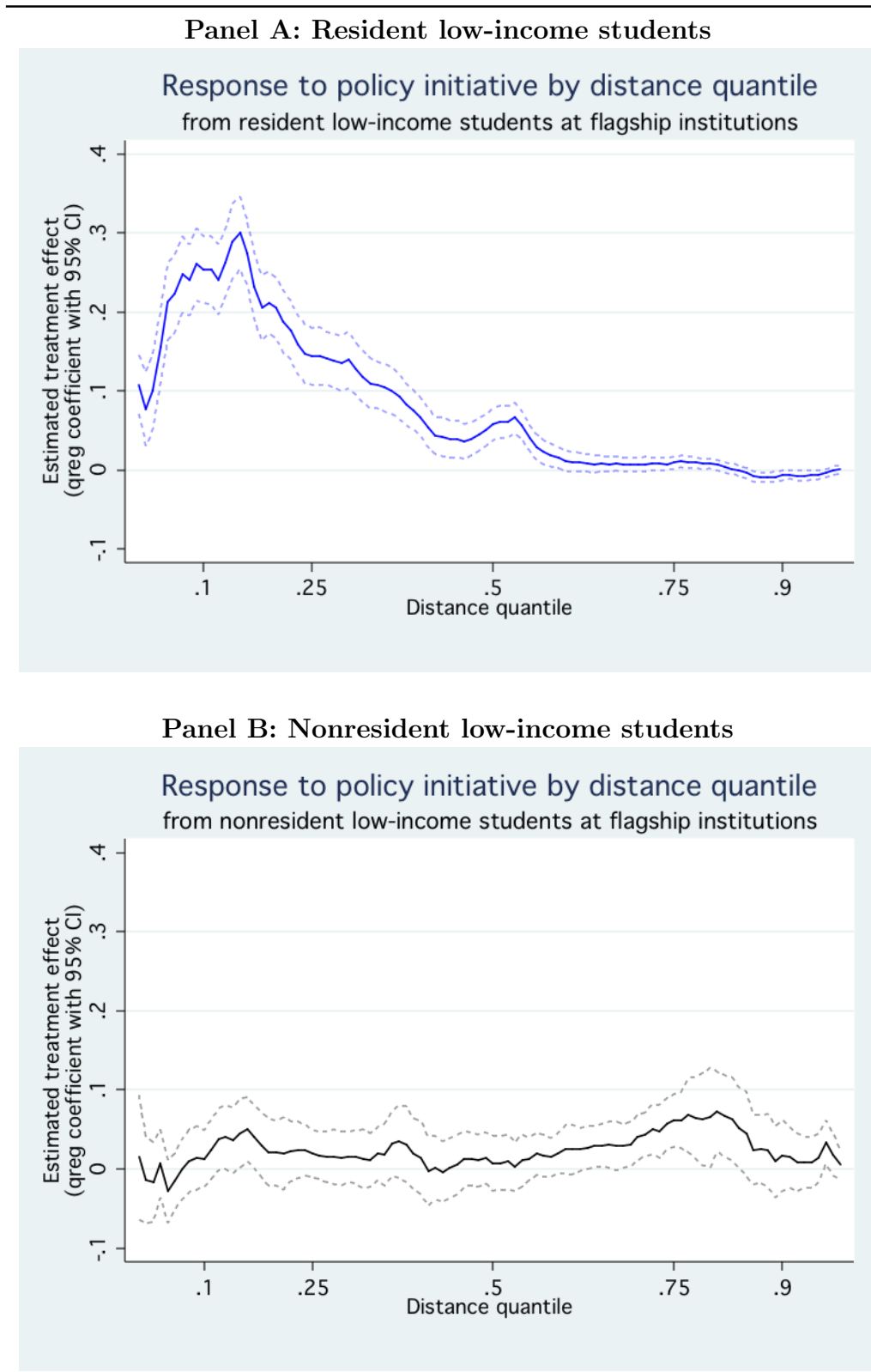


Figure 2: Estimated treatment effect at distance quantiles, by residency



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