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A National Evaluation of the "I Have a Dream" Program

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1. Abstract

The "I Have a Dream" program (IHAD) provides support to an adopted class of students (typically a 3rd to 5th grade class) or to an adopted age cohort from a housing project. Support includes tutoring and mentoring during the elementary school, middle school and high-school years as well as partial tuition reimbursement for students who eventually enroll in higher education. Program goals are to (1) enhance the school experiences for participants, (2) reduce the rate at which children engage in negative social behaviors (e.g., delinquency) and experience undesirable consequences (e.g., drug use and pregnancy), (3) retain children in school, and (4) encourage participation in higher education.

During 2003, the National Institute of Justice and the Bureau of Justice Assistance commissioned Abt Associates Inc. to determine whether or not the IHAD program could be evaluated. The Abt study team deemed the IHAD program evaluable, but issued a cautionary note that earlier evaluators had confronted resistance from IHAD sponsors who saw evaluation as unnecessary and potentially burdensome. We (the Abt study team) recommended further study to determine whether the requisite cooperation would be forthcoming. Conditional on cooperation, we also recommended further investigation into the availability and accessibility of evaluation data. Based on the fact that we have secured cooperation and have determined that data are available and accessible, we therefore recommend and propose to conduct a full-scale evaluation of the IHAD program.

The proposed evaluation would occur in five locations: Boulder, Denver, Los Angeles, New York City, and Portland (Oregon). We selected these locations because sponsors have run multiple IHAD programs in each location, and there are economies from negotiating with just a few geographically clustered sponsors (who are receptive to evaluations) and a few school districts and housing authorities (who have agreed to provide access to necessary data) in order to reach many IHAD programs. We propose to evaluate a total of 27 IHAD program, with an average of 67 program participants per program.

The evaluation will employ a non-equivalent comparison group design. Although the details differ across the included IHAD programs, the comparison groups are typically elementary school classes from the year before and the year after the IHAD group was founded in the same elementary school, or children in the prior age cohort from the same housing projects. Although this design is not based on random assignment, this is an extremely strong quasi-experimental design because the selected classes/age cohorts have no special attributes that are not shared by the comparison groups.

Because the IHAD groups typically comprise about 67 children, the power for detecting a program effects is modest for individual programs, but the power of detecting a program effect is substantial when that effect is averaged across programs. We will employ meta-analytic techniques to combine the effects from multiple programs, thereby deriving an average effect that is descriptive of IHAD programs.

2. Problem Statement

This problem statement begins with an overview of the "I Have a Dream" program. It then reviews extant evaluations of IHAD program evaluations and other program evaluations that have important elements in common with the IHAD program.

2.1. Overview of the "I Have a Dream Program"

There is considerable variation across IHAD programs because sponsors play the central role in determining how programs are funded and operate. Some sponsors are individuals or couples, others are corporations, and still others are foundations. Although there is variation across programs, all IHAD programs have six principal components (in italics, below). The following is adapted from program documentation, with editorial comments based on our field investigations.

(1) The *sponsor* provides program funds, oversight and direction. By design, the sponsor is expected to provide mentoring to program participants, known as Dreamers. In practice some sponsors take an active role; some pay for surrogates; and others have little direct contact with Dreamers. Consistent with National Institute of Justice direction, we propose to evaluate programs that adhere to best IHAD practices, so we worked with the National IHAD Foundation to purposefully select programs that demonstrate the desired application of the IHAD model.

(2) A *project coordinator* is a full-time paid staff member who coordinates program activities and mentors program participants. Most evaluators consider the project coordinator to be the key contributor to an IHAD program, although some programs also get substantial mentoring input from sponsors and volunteers.

(3) *Dreamers* are the primary recipients of program services. Although IHAD attempts to serve Dreamer families, or at least engage them in supportive activities, most evaluations indicate that family members (parents, guardians, siblings) are not active IHAD participants. As described by the National Foundation, the program provides "…a diverse array of educational, mentoring, counseling, employment, cultural, community and recreational activities that must keep evolving to keep pace with the changing needs of Dreamers as they mature."

(4) The *community* is expected to provide "support, meeting space, recreational space, resources, volunteers and expertise…making it possible to offer an extraordinary range of services without incurring prohibitive costs." Extant evaluations do not stress community contribution, with the exception of schools and housing projects that often provide space for the project coordinator and program activities. According to some evaluations, the broader community has shunned the project, seeing it as already receiving extensive resources from its benefactors.

(5) The *resources* are the financial commitment by the sponsor. Sponsor commitments vary from program to program with some programs seeming to have endless funding and others having definite upper limits. Minimum commitments are extensive. A sponsor is expected to make an immediate $300,000 commitment and must make an incremental commitment of an additional $150,000 over time. In practice programs typically cost more than $450,000.

(6) The *program* guarantees at least partial funding of education at a community college, state university or accredited technical school. Some programs are more generous than others in subsidizing post secondary education.

Prior to 1993 all IHAD projects were school-based. Thereafter, some IHAD projects were public housing-based. As described by IHAD documentation:

> A good school partner is supportive of the Program's mission and is cooperative in allowing "I Have a Dream" to work in the school and with school personnel. Typically, the Project Coordinator works out of the school, and Dreamers may participate in "I Have a Dream" activities there both during and after school. The elementary school often provides the Project's primary access to the Dreamers and becomes a key player in the Project's work.

When it describes the public housing-based project model, program documentation says that:

> The public housing-based model includes the traditional partners—Sponsors, Project Coordinator, school, a community-based organization, Dreamers, and families—and adds as primary partners the local housing authority and a local college or university partners.... The local housing authority helps identify eligible residents and may provide assistance such as rent-free office space, a cash grant, and program guidance.... The college partner provides

volunteers, program expertise, facilities, staff support, and access to special programs for Dreamers and parents.

2.2. Evaluations of IHAD and Mentoring Programs

This section discusses what evaluators have learned about IHAD programs. Because IHAD has a strong mentoring component, we provide a separate assessment of evaluation research with respect to mentoring. And because IHAD has components like those of other large, national programs, we also review what is known about the success of those programs.

2.2.1. What is Known about IHAD Programs?

Davis and Hyatt (1998) formed a treatment group of 5th grade Dreamers from the '95 class in King Elementary School (Portland). There were two comparison groups, both drawn from 5th graders in the same school, one from the class of '96 and the other from the class of '94. Using archival data, the researcher concluded that IHAD had positively affected school achievement, behavior in the community, educational achievement, and that the program was cost beneficial. However, there are limitations to this evaluation. Only 99 Dreamers began the study. By the end of the 11th grade, that number had shrunk to 82. The two comparison groups began at about 100 each. By the end of the 11th grade, their numbers had shrunk to about 71 each. These small sample sizes raise the issue of statistical power, while the high dropout rates raise questions of validity.

Aaron and Barnow (1994) examined eight IHAD programs, all involving the 1992 high school class, and all implemented in elementary schools in New York City. This evaluation is intriguing because of the diversity that it found across the eight programs. Moreover, the researchers reported on an outcome evaluation (using a matched sample) done by the New York City Board of Education. Results in terms of benefits to "I Had a Dream" were modest in terms of reading, mathematical skills, absenteeism, and grade advancement.

Kahne and Bailey (1997) framed the IHAD program as a process of developing social capital. For the two IHAD programs, high-school graduation rates were 76% and 69%; for the two comparison groups, rates were 37% and 34%. These are impressive differences, but this success was not indicative of other IHAD programs around Chicago for which Kahne and Bailey had outcome data. Success seemed to stem from three sources. First, the programs built a "… strong, trusting, and sustained relationship with youth…" and "…social networks became valuable only in the context of strong trusting relationships." Second, the program was able to attract additional staffing from the AmeriCorps program and some from a special internship program for Princeton students. Third, these two programs may have been successful because they removed many children from public schools and paid tuition for them in parochial schools. This complicates drawing conclusions about which factor was more important—out-of-school support or the parochial education.

Shoemaker and Sims (1997) evaluated the effectiveness of an IHAD program using 4th and 7th grade computerized standardized reading and math test scores. The evaluators concluded: "Comparing the Dreamers' overall test performances to that of a quota sampling group does not yield dramatic evidence that IHAD is improving their students' academic achievement" (page 80).

This short review does not exhaust the IHAD evaluation literature, but we have attempted to include the most informative studies, and others are noted in the bibliography. These evaluations imply that programs that adhere to the IHAD model have improved the lives of students, their school performances, and the relationships with their communities. However, these studies have been based on small samples, and the evaluators have faced several challenges to validity. Therefore, additional rigorous evaluation is required.

2.2.2. What is Known about Mentoring Programs?

Although its Guarantee differentiates IHAD, the IHAD program is at its heart a mentoring program. In that respect, we might ask what is known about the effectiveness of mentoring programs? We emphasize, however, that unlike many mentoring programs, IHAD makes a long-term commitment to its Dreamers, so the impact of IHAD-based mentoring might differ materially from that of other mentoring programs.

Programmatic mentoring—the practice of matching "at-risk" or "disadvantaged" youth with an adult who is expected to develop a caring, supportive relationship with that youth—has grown dramatically over the past 20 years. Over 2.5 million American youth are involved in school or community-based volunteer mentoring programs each year (Carson, 2000). Several researchers have reported efficacious outcomes for mentoring programs that screen, match and support relationships of adequate intensity to affect the lives of youths (Harter, 1993; Slicker and Palmer, 1993; LoSciuto et al., 1996; Brown, 1996; DuBois, Felner, Brand and George, 1999; Thompson and Kelly-Vance, 2001; Grossman and Rhodes, 2002), although salutary findings are not universal and effects are sometimes small and ephemeral (Sipe, 1996). Understanding what parts of mentoring are material and lasting is an important theme of current of research.

There are a number of notable school-based and community-based mentoring programs. Among these, OJJDP's *The Juvenile Monitoring Program* (JUMP) is designed to improve academic performance and school attendance, and reduce juvenile delinquency and gang involvement through one-to-one mentoring of 10–14 year olds.[1] The ongoing outcome evaluation is based on a one-group pre/post design that contrasts self-reported changes in aggressive behavior/delinquency, substance abuse, mental health, family relations, peer relations, and education status. Contrasts are made within

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[1] See http://ojjdp.ncjrs.org/jump. A 1998 report to Congress describes the program and its (at that time) ongoing evaluation. See: http://www.ncjrs.org/pdffiles1/952872.pdf. A 2000 bulletin provides an update. See: http://www.ncjrs.org/pdffiles1/952872.pdf. The evaluators web site provides a 2003 report: http://www.itiincorporated.com/AnnRpt.

subjects (pre- versus post-intervention scores) and across subjects. Interim results indicate lowered risk in aggressive behavior/delinquency, peer relationships, and mental health among participants over time. Absent a comparison group, however, it is difficult to attribute these changes solely to the mentoring program.

The Safe and Drug Free School Mentoring program is the largest school-based mentoring program. Operated by the Department of Education, Office of Safe and Drug Free Schools, this program includes over 300 school, community and faith-based institutions. Individual programs have completed local evaluations; and the ED has recently contracted with Abt Associates to conduct a national evaluation based on an experimental design beginning in April 2005.

The most widely cited study of program impacts is the experimental study of Big Brothers Big Sisters conducted by Public/Private Ventures (P/PV) in the early to mid-1990s (Tierney and Grossman, 1995; Grossman and Tierney, 1998). Youth in the randomly assigned treatment group were less likely to have initiated drug and alcohol use; had better school attendance and grades and felt more competent in school; were less likely to hit someone; and had better relationships with their peers and parents compared to youth in the comparison group. Although gains degraded over time, problems increased at a slower rate for the mentored group than for the comparison group.

Other evaluations (Sipe, 2000) that examined the effect of mentoring in conjunction with other, primarily academic, services found that youth improved in a number of academic outcomes. For example, students with mentors tended to have better grades, were more likely to participate in college preparatory activities, and were more likely to enroll in college and remain in college for a longer period than students not receiving the program's services (see Sipe, 2000 for a summary).

DuBois, et al. (2002) conducted a meta-analysis of 55 mentoring program evaluations, concluding that mentoring programs were effective. Benefits arose for both stand-alone mentoring programs and programs in which mentoring was embedded among other program services. Further, benefits extended to youth regardless of age, gender, ethnicity, and family structure. DuBois and his colleagues also concluded that mentoring holds the greatest potential benefits for youth who might be considered "at risk", particularly if at-risk is defined in socio-economic terms.

Although evidence is mixed for mentoring in general, evaluation research has shown that mentoring can improve the lives, school performance, family relationships and community relations for youth who receive quality and sustained mentoring services. This literature lends credence to the assertion that IHAD improves the lives of youth and their families. Our proposed evaluation is structured to test whether or not that assertion is true.

2.2.3. What is Known about Similar Programs?

IHAD programs are similar to early intervention programs funded by the Higher Education Act of 1965 and other Federal initiatives. Like IHAD, these programs are designed to provide aid and support services to students who are disproportionately at risk for inadequate educational attainment to assist them in the transition from elementary to secondary and higher education (Fenske, Geranios, Keller, and Moore, 1997). "At risk" describes students—oftentimes economically disadvantaged or racial and ethnic minorities, who would be the first generation to attend college—who have a disproportionately low probability of attending college.

Since 1991, the U.S. Department of Education (ED) has funded a National evaluation of *Upward Bound* programs, which provide instruction in literature, composition, mathematics, and science after school, on Saturdays, and during summers at college campuses. (Myers, Olsen, Seftor et al., 2004) found small increases in the number of math credits earned (but not in other academic areas), total

high school credits, Advanced Placement (AP) courses, graduation, or grade point average (GPA). Increases in four-year college enrollment were not statistically significant.

Career Beginnings provides mentoring, academic support (competency-based curriculum and workshops), and workforce training and placement to over 1,500 11th and 12th grade students annually at 24 sites throughout the U.S. and Canada. Programs target at-risk students with low to moderate family income, and average academic achievement (middle 60% of their class). Cave and Quint (1990) evaluated 11th graders randomly assigned to program and control groups in seven sites, finding that participants had fewer unexecuted absences, and were more likely to attend college.

Other programs have yet to be evaluated. Since 1967, *Talent Search* programs have provided counseling and information regarding college admissions requirements, scholarships, and financial aid programs to disadvantaged students in grades 6 though 12. A national evaluation of *Talent Search* is now underway (see Maxfield, Cahalan, Silva et al., 2000 and Cahalan, Silva, Humphrey et al., 2004). *Gaining Early Awareness and Readiness for Undergraduate Programs* (GEAR UP) provides five-year discretionary grants to supplement programs that provide college scholarships and postsecondary education preparation services to cohorts of low-income students from seventh grade through high school (see Westat, 2003 and Westat, 2004).

Levine and NiDiffer (1996) have alerted the public to the handicaps faced by at-risk children who face possible college careers. Several national programs have attempted to help at-risk children overcome these impediments, and while some programs have shown promising (if limited) results, from an evaluation perspective, this is largely uncharted territory. Policy makers and researchers seek to known whether a sustained intervention targeted on promoting post-secondary education can substantially help youth for whom education post high school would otherwise be unobtainable. An IHAD evaluation is relevant to this concern.

2.3. A Research Design

This research design section specifies the research hypotheses and explains how we would test them. It identifies the IHAD programs that we have recruited to participate in this study. Additionally, it identifies the data that we will collect from those programs, and explains how we will analyze those data. Throughout this section, we assume that the Dreamer group is formed in the 3rd grade of an elementary school. In fact, Dreamers are often formed in other years, and housing-based programs select Dreamers from age cohorts rather than grade cohorts. Nevertheless, this assumption simplifies the presentation without avoiding analytic issues that would otherwise arise.

2.3.1. Research Questions

The planned study will evaluate whether or not the IHAD experience results in decreased dropout rates, increased post-secondary education rates, and generally improved school experiences and outcomes for IHAD participants. Specifically we seek to test three generic hypotheses:

- H1: Does IHAD cause children to remain in school longer than they would otherwise?

- H2: Does IHAD cause children to have better school and post-graduate experiences than they would otherwise?

- H3: Does IHAD cause children to attend post-secondary school more frequently than they would otherwise?

Establishing a counterfactual for the first and third hypotheses is relatively straightforward once we have identified a suitable comparison group of children who did not participate in IHAD. Given a suitable comparison group, the percentage of children who remain in school as of a specified date is a measure of school retention (H1) or post-secondary school continuation (H3). A complication arises because school recordkeeping does not enable us to distinguish between children who withdraw (drop-out) from school and those who transfer to other school districts, an issue that we discuss

further later in this section. Establishing a counterfactual for the second generic hypothesis is more complicated. The problem is that children with relatively negative school experiences may withdraw from school in the absence of IHAD, but they may stay in school in the presence of an IHAD program. Consequently, comparisons that are made after children reach an age at which they can legally leave school may be biased against the IHAD program. This issue will also be discussed further in this section.

2.3.2. The Design

Although this discussion assumes an evaluation in a single school, we would actually conduct the evaluation across several IHAD programs. Thus, this evaluation will produce multiple estimates of the effectiveness of IHAD programs, and we will combine them using meta-analytic procedures.

It is difficult to see how we could set-up a randomized design given that all children in the 3rd grade are IHAD participants. Furthermore, we are envisioning a retrospective evaluation so that we can take advantage of ongoing IHAD programs. Dreamers could not be selected randomly on a retrospective basis. Therefore, within any single school, random assignment designs are impractical.

Fortunately, a strong quasi-experimental design is provided by the experiences of children who entered the 3rd grade in the same elementary school both one year before and one year after the Dreamers. Hereafter we will call the comparison group members NonDreamers, with no pejorative connotation intended. We expect Dreamers and nonDreamers to be statistically comparable by virtue of the way that the treated and comparision groups were selected. We will furthermore introduce control variables (by way of covariance models, propensity scores or instrumental variables) when testing hypotheses. The rest of this section discusses these and other issues that arise in a quasi-experimental setting.

Intent to Treat

We will be testing the generic hypotheses H1, H2 and H3 by comparing the outcomes for children who participate in IHAD with the outcomes for children who do not participate in IHAD. This observation raises the question of what we mean by *participate*? For evaluation purposes, a Dreamer is a child eligible for IHAD in the 3rd grade, regardless of whether he or she chose to be active in IHAD activities. A NonDreamer is a child who was in the 3rd grade one year before or one year after the formation of the IHAD program, even if he or she managed to participate in IHAD activities. We will thus compare the outcomes for all Dreamers with the outcomes for all NonDreamers, regardless of their treatment status).

Evaluators sometimes use the terms *refusal* and *crossover* when referring to situations where a subject refuses to participate in an intervention (refusal) or declines to remain a comparison group member (crossover). If a child is selected to be a Dreamer but declines to actively participate in the IHAD program, he or she is a refusal. If a child is selected to be a NonDreamer but manages to participate in IHAD, he or she is a crossover. The intent-to-treat model retains refusals as Dreamers, because to do otherwise would invite selection bias resulting from the fact that those who refuse may differ materially from participants. The model retains crossovers as NonDreamers for the same reason. The test of a null hypothesis asks whether or not the Dreamers (including refusals) do better than the NonDreamers (including crossovers).

The test may seem strange to reviewers. After all, by including refusals as Dreamers, an evaluator would seem to be diluting the effect of an IHAD program. Also, by including crossovers as NonDreamers, an evaluator would seem to be artificially enhancing the performance of the comparison group, presuming that IHAD does contribute something to the performance of crossover NonDreamers. Comparing the outcomes for Dreamers (including refusals) with the outcomes for NonDreamers (including crossovers) would thus seem to understate the value of IHAD participation.

In fact, techniques have evolved for estimating the size of an undiluted treatment effect in the

presence of refusals and crossovers. See Bloom (1984) and Angrist (2004). We will provide a

regression-based estimate, the use of which is justified in Appendix A.

Testing the H1 and H3 Hypotheses

Although there are some complications, which are discussed in Appendix A, testing the generic

hypotheses H1 and H3 is straightforward using the intent-to-treat framework. Generic hypotheses H1

and H3 are of the same form, asking whether Dreamers are more likely than NonDreamers to have

status S at time T years post 3rd grade. Status might be the condition "remained in school," or

"graduated from high school," or "entered college." Other statuses are possible, but the key is that

the status is known for all Dreamers and NonDreamers.

A minor complication arises because the Dreamer group is formed in year T1, when Dreamers are in

the 3rd grade. One group of NonDreamers is formed in year T1-1, when they are in 3rd grade. The

other group of NonDreamers is formed in year T1+1, when they are in the 3rd grade. Thus, the

evaluation will compare the status of Dreamers at time T2 with the status of NonDreamers from the

first group at time T2-1 and of NonDreamers from the second group at time T2+1. In the event of a

time trend, comparing Dreamers with NonDreamers who are in classes one year before or one year

after the Dreamer class could provide biased estimates of the treatment effect. If the trend were

linear, averaging the differences between the lagged and advanced comparison groups would provide

an unbiased estimate of the treatment effect. This is one reason for selecting comparison groups from

3rd grade classes that were formed both one year before and one year after the Dreamer's 3rd grade

class.

A second, more serious complication arises because children transfer among schools, and this has implications for testing hypotheses about status changes. A regression-based latent variable approach to overcome this problem is described in an appendix.

Testing Hypothesis H2

Testing generic hypothesis H2 does not raise any problems not already discussed provided we test the hypothesis for children younger than 16. After that age, children can legally withdraw from school.[2] This creates a problem because children with relatively negative school experiences may be most prone to leave school. If IHAD manages to retain some of those otherwise dropouts, a comparison based on the remaining Dreamers and NonDreamers would understate the effectiveness of IHAD.

Manski (1995) recommends placing bounds on treatment effects when selection bias is an issue. Although putting a lower bound on treatment effectiveness is often impractical, meaningful lower limits exist for the effectiveness of the IHAD program. Again Appendix A provides details.

2.3.3. Data Collection from Each Program

The above discussion implies data requirements, and we had to confirm that those data would be available. After we identified a set of 27 potential Dreamer classes, the local IHAD program staff referred us to housing authority and school district contacts who normally review external information requests in their positions (e.g., research and evaluation directors). We discussed three issues[3]:

- Availability of roster-like information to build Dreamer and comparison student samples;

- Availability of student-level information on academic and other measures; and,

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[2] In fact, the legal age for withdrawal from school varies from state-to-state. We use age 16 as a simplifying placeholder. The actual analysis would substitute the correct age for each state.

[3] We also discussed ongoing research and evaluation activity, and agreed to avoid redundancy and incorporate extant procedures and data when possible.

- Level of effort necessary to extract data from these records, and other access issues.

Given an understanding of these issues, we are able to propose pragmatic procedures for collecting data, which are discussed next.

Selecting the Sample

The evaluation design requires that we identify Dreamers and NonDreamers who were in the third grade of an elementary school in three consecutive years such that the Dreamers were in the 3rd grade during the middle year. If we can acquire 3rd grade rosters for three years surrounding the year when the Dreamers were formed, then the evaluation can proceed as explained earlier.

For classes recruited from public housing, rosters would be based on housing authority records; otherwise, all records would be accessed from IHAD programs and school districts.[4] We did not formally request access to data, which would require review of a final approved protocol. Instead, we reviewed data access options with experienced program, housing, and school district staff, who regularly work with these records. Based on their experiences, they were able to confirm that roster retrieval and data extract was feasible, but could involve a triangulation of information from several sources ranging from paper to electronic records and staff consultation. One caveat is that recordkeeping generally improved over time, so recent records will have more specificity than older ones, and recordkeeping among schools within a single district will vary.

Personal Identifiers

This evaluation requires tracking school records of Dreamers and NonDreamers, which requires access to personal identifiers. We recognize the need to protect confidential and sensitive

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[4] Most schools types (including charter and other alternative schools) are included in public school records. In NYC, we are negotiating access to private school records maintained by the Archdiocese because a significant number of Dreamers transfer to private schools with IHAD program assistance.

information—such as special education status, free/reduced meal program eligibility, and delinquent behavior.[5] However, a robust impact evaluation requires information for all students, and therefore, we would request a waiver of written authorization to release records. Considering the consent and other issues involved in extraction of sensitive information from a variety of records, we propose to partner with in-house housing authority and school researchers to recruit staff that have approved access to school records. Under our direction, IHAD program staff will prepare an initial file containing information on Dreamers including personal identifiers. They will transfer that file to housing authority or school district staff,[6] who will add roster records for non-Dreamer students (i.e., comparison group) to complete the sample. Next, school district staff will append data on academic information, and prepare a final analysis file that is stripped of personal identifiers. Since we cannot view personal identifiers, contracts will stipulate that standard quality control is exercised to ensure data integrity.

We will recruit and train data extraction staff on a standard instrument for laptop data collection that will allow for data to be recorded in varying levels of precision (e.g., occurrence of behavior versus severity of behavior). Typically, the most reliable information will be available for those who are most closely monitored; we expect that the outcome of post-secondary education attendance will be available for all Dreamers, but not for all comparison students.[7]

Having contacted school record keepers in the IHAD jurisdictions, we have confirmed that schools will allow school employees to collect data under contract to Abt Associates. Abt staff will never

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[5] The Family Rights and Privacy Act (FRPA) requires written authorization to release school records, and public housing tenancy is protected by similar legal and policy provisions.

[6] Note Dreamer families sign waivers at program admission that allow IHAD programs to share information with schools (e.g., to monitor academic performance).

[7] The outcomes evaluation would be based on data items that are available for both Dreamers and NonDreamers within each of the 27 IHAD programs. Nevertheless, data that are available exclusively for Dreamers provides a useful description of IHAD activities and the Dreamers who participate in those activities.

have access to identifiers or schools files. The timing of data collection is unimportant because we will use administrative records to assemble retrospective files. For example, teachers or school administrators could be employed during the summer, when data extraction would not conflict with teaching or administrative duties.

Data from School Records

We propose data extraction from school records to examine outcomes that include academic performance (e.g., test scores), high school degree status (e.g., graduation), and post-secondary education (e.g., college attendance). Appendix B provides a simple logic model that illustrates the kinds of information we seek, including student background and behaviors and aggregate school/housing characteristics. This model represents the information appropriate for a retrospective impact evaluation; it is a subset of the information that would be collected for a complete process and impact evaluation of a successful mentoring program, which would require self-report information on psychosocial and other measures from subject families.

Section 2.3.4 identifies the Dreamer classes that would participate in this study. Some of those Dreamer classes would not have graduated by the time that our data collection has terminated, so for them, we would only have school records current as of the end of this evaluation. Most children will have graduated, however, so retrospective data collection will not unduly hinder our ability to infer IHAD's effectiveness at promoting graduation. Also, as noted in that section, we have confirms that retrospective school records are available for these Dreamer and NonDreamer classes.

While measuring academic performance is straightforward, measuring "delinquency" may be problematic. If this information is kept in the school records, we will have access. On the other hand, if delinquency is part of the juvenile justice system record keeping, we will not have access, for two reasons. The first is that we will not have access to personal identifiers, so we cannot match our study

subjects with juvenile justice records. Possibly we could employ teacher coders as intermediaries, but the second problem is that juvenile justice records are difficult to access. Negotiating for that access would be beyond the resources of this study.

We will also record other variables that describe the child. Birth date is important so that we can identify when a child can legally withdraw from school. Variables such as gender should be available and are likely to be useful covariates. Address could be a useful way to associate a child with a socioeconomic environment. Of course, children within the same school (and certainly those within the same housing project) may be homogenous with respect to those socioeconomic variables, so there would be no strong reason to collect those data. These variables come from school or IHAD records.

Data from Surveys

We are principally interested in the use of administrative records, which can be assembled retrospectively at costs that are modest when compared with interviewing. The reason is that the effects of IHAD will take years to unfold, so we are interested in longitudinal data that can be collected retrospectively for a large number of IHAD projects. A prospective study would require much more time both because the study would be limited to newly formed IHAD groups and because we would have to observe those groups for many years. Such a design is impractical given the needs for more timely evaluation results.

Furthermore, the process of acquiring parental agreement to interview children would pose two problems. One is that the cost of contacting parent and interviewing their children at multiple times would be expensive. The second is that we anticipate that most Dreamer parents would agree to the request (as their children are already part of the IHAD program), while many NonDreamer parents would implicitly or explicitly deny the request. Consequently we decided against a survey.

This decision has consequences for the evaluation, of course. We will not be able to measure directly childhood development for Dreamers and NonDreamers, except as such development is reflected in administrative records. We will not be able to answer questions that require self-report responses, and these include interesting questions about student attitudes and perceptions.

2.3.4. Participating IHAD Programs

In preparation for this proposal, we have discussed this project with the National Foundation, with local foundations that sponsor IHAD programs, with project coordinators and individuals who are knowledgeable about school records and with gatekeepers for those records. The fundamental questions that had to be answered were: (1) Will you cooperate with an evaluation of the IHAD program?, and (2) Can you and will you provide access to data necessary to evaluate the IHAD program? Without positive responses to these questions, we could not propose a credible evaluation.

With the assistance of National IHAD, we narrowed the pool of over 180 IHAD projects developed in 27 States since 1981 to several potential sites. This search eliminated sites with single IHAD programs, programs deemed inconsistent with the standard IHAD model, and programs that no longer maintained Dreamer records. IHAD program staff at each site provided historical information on all classes for which they had records (see Appendix C). These include year and grades adopted, class size, and origin (school or public housing). Not all projects adopt entire grades in schools or public housing, so we reviewed additional IHAD program eligibility criteria, such as eligibility for free/reduced meal programs (e.g., Boulder's Class 2).

The final sample listed in Table 1 comprises 27 Dreamer classes in five sites—Boulder, Denver, Los Angeles, New York City, and Portland—that:

- Had replicable eligibility criteria (records could be used to construct a comparison group),

- Maintained Dreamer records with personal identifiers,

- Retained Dreamers between adoption and contract and signing,[8]

- Reached at least the 8th grade, and

- Recruited grade spans that did not exceed three grades.

Table 1

IHAD Sites Selected for Evaluation

SITE	CLASS	YEAR	GRADE	SIZE	ORIGIN
Boulder	Class of 1998	1990	5th-7th	36	Housing
	Class of 2000	1992	5th	79	School
	Class of 2008	1996	3rd-6th	32	Housing
Denver	Class of 1995	1989	6th	60	School
	Class of 1998	1990	5th	45	School
	Class of 2003	1994	4th	60	School
	Class of 2006	1997	4th	46	School
	Class of 2008	1997	4th	32	School
	Rocky Mountain	1999	2nd-4th	35	Housing
	Class of 2008	1998	4th	50	School
Los Angeles	Projects 1-5	1987	6th	128	School
	Project 7	1987	6th	49	School
	Project 8	1989	6th	57	School
	Project 9	1991	6th	65	School
	Project 10	1991	6th	51	School
	Project 11	1992	6th	60	School
	Project 13	1996	4th	55	School
New York City	Manhattan	1993	2nd-3rd	77	Housing
	Queens	1994	3rd	80	Housing
	Bronx	?	3rd	57	Housing
Portland	Class 1	1990	4th	108	School
	Class 2	1991	4th	109	School
	Class 3	1992	4th	94	School
	Class 4	1995	4th	70	School
	Class 5	1995	4th	77	School
	Class 6	1999	3rd	89	School
	Class 7	2000	3rd	114	School

In most cases, the comparison group will comprise students in the grade preceding and following the

adopted grade(s) of Dreamers, from the same school or public housing. The exception to the last

criterion is Boulder's Class 3. We will attempt to compare 3rd–6th grade Dreamers recruited from one housing project to 3rd–6th grade non-Dreamers from a comparable housing project, who attend the same schools.

These 27 classes were adopted between 1987 and 2000, and most (21) were recruited from schools. In all but one class (Boulder's Class 2), entire grades were adopted from 3rd to 7th grades. Classes recruited from public housing typically span multiple grades to increase cohort size. Classes ranged from 32 to 114 Dreamers (or 128 combined in Los Angeles' Classes 1–5 recruited from a single school), or about 67 Dreamers on average.

We have received assurances of cooperation/data access from the National Foundation and from each of the IHAD programs. Letters of support are included in Appendix D.

2.3.5. Data Analysis

Data analysis is complicated by the problems with testing the generic hypotheses H1, H2 and H3 as was explained above. Details are provided in appendices. The main points are summarized here.

Hypotheses H1 and H3 are statements about whether or not an event has occurred as of a given time, so some form of survival analysis seems recommended. The survival curve is unlikely to follow any familiar parametric pattern because of institutional rules about when a child can legally leave school, so we anticipate using partially parametric procedures, such as proportional hazard models. A principal analytic problem is that children can transfer from a school district, at which point the child would effectively leave the study.[9] Children who transfer from the school district raise some

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[8] IHAD programs attempt to keep Dreamers engaged despite relocation, but Dreamer families at one site were so transient that many adopted students relocated before contracts stipulating to Dreamer status and tuition assistance were signed.

[9] We have no problems with a child transferring to a different school in a single school district because we will still be able to follow that child. We have no practical way of following children who leave a school

analytical problems, the solutions for which are discussed in Appendix A. Practically, however, we treat transfers as right-hand censoring, so they raise no serious concerns for the analysis.

Hypotheses H2 are statements about conditions that hold at time T2 for Dreamers and T2-1 and T2+1 for NonDreamers. The principal problem here is with selection bias, that is, students who perform relatively poorly in school may be motivated to remain in school if they are IHAD participants and leave school if they are not IHAD participants. Therefore, comparing Dreamers at time T2 with NonDreamers at time T2-1 and T2+1 will be subject to bias.

Econometricians often deal with selection bias by employing instrumental variables, but it is difficult to identify useful instruments for this application. Another partial approach to dealing with this problem is to introduce control variables into a regression. We will, in fact, introduce control variables, but we doubt that the introduction of control variables will be altogether satisfactory. Therefore we propose to follow Manski's suggestion and place bounds on the size of the treatment effect. This approach is explained in Appendix A.

We will evaluate each IHAD program on a case-by-case basis. Given 27 IHAD programs, there will be 27 measures of treatment effectiveness. Because an IHAD program has about 67 participants, and because the size of the control group will be about 134, the power will be fairly low for any single IHAD program. However, we will use meta-analytic procedures to estimate the average "effect" from IHAD across these 27 programs.

The latter inference need not be complicated. We can treat each of the 27 estimates as being independent and asymptotically normally distributed. The sampling variance for the average will also be distributed as approximately normal with the sampling variance being a weighted average of the

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districts because we will not necessarily know the school to which they transfer, nor will we know for sure

sampling variances from the N IHAD sites. This gives us a test statistic for judging IHAD effectiveness.

2.3.6. Report Preparation

We will prepare yearly interim reports and a draft final report three months before this grant ends. The draft final report will include an executive summary. The executive summary will be suitable for use by the National Foundation and local foundations to describe the success of the IHAD program. Following a review by NIJ, we will prepare a final report that is responsive to NIJ comments. We will prepare two professional journal articles. The first will be suitable for publication in a criminal justice journal. The second will be suitable for publication in an education journal.

3. Work Plan

Task 1: Planning the Evaluation

Following notification of award, we will prepare a final work plan for NIJ's review. After discussing this plan with the NIJ monitor, we will prepare a final work plan and commence work following approval.

We propose assembling an advisory team of representatives from the National Foundation and each of the local foundations that participate in this evaluation. This advisory team will help us keep focused on the issues that concern IHAD. We will meet with this advisory team yearly at the annual meeting held by the National Foundation.

Also during this planning period, we will cement agreements with each of the study sites for access to data. As noted, we have secured the cooperation of stakeholders, but we do not have formal approval

that they have transferred.

from the school districts, each of which had internal review boards that review and approve requests to conduct studies in their schools. We will secure that approval during this planning period.

Task 2: Developing the Protocol

During Task 2, we will finalize a data collection protocol and seek formal approval for implementing our research design in each site. We will develop a basic data collection protocol and implement it on a laptop computer. We will develop a training manual and procedure for using that data entry program.

Given the formal data collection protocol, we will seek formal approval from school districts to implement our research design. We expect that the protocol will have to be tailored to accommodate difference across places and over time. Once that tailoring is completed, we will seek formal approval from our Institutional Review Board and from the cognizant authorities in each of the sites.

We will recruit and train data extraction staff and set a schedule for completing data collection. Data coders will sign consultant agreements that stipulate the expectations of their employment. We will train those data collectors through on-site visits and by Internet training procedures.

Task 3: Data Extraction

When the research protocol has been approved and we have hired and trained data extraction staff, we will commence data collection. Data collection can be staggered because all data will come from administrative records. Thus, as a site approves data collection, we will implement data collection procedures in that site. One advantage of staggering the data collection is that the data extraction staff will be able to share laptop computers. That is, when one data extractor finishes his or her assignment and delivers data expunged of identifiers, files stored on the laptop computer will be erased, and the computer will be transferred to the next data extractor.

Data would be transferred via the Internet, if practical, and otherwise by Federal Express delivery of electronic files. Data would be stored at Abt Associates using procedures that protect confidentiality. Those procedures are described in the attached Privacy Certification.

Task 4: Analysis

Data would be analyzed during Task 4. The analysis plans were described earlier. Details are provided in appendices.

Task 5: Reporting

We will prepare a draft report with sufficient time for NIJ to review that report and provide comments. We will respond to those comments and provide a final report. The report will be designed so that we can easily extract both a technical paper suitable for publication in a scientific journal and a policy-oriented paper suitable for publication in an applied education journal and appropriate for use by the National I Have a Dream Foundation and by local foundations and other sponsors.

4. Staffing and Schedule

One challenge posed by this proposed evaluation is that the IHAD program spans three substantive areas: justice, education and housing. It includes justice by virtue of the fact that the National "I Have a Dream" program has received Congressional earmarks through the Bureau of Justice Assistance, so the Department of Justice has an interest and responsibility to evaluate the program. Furthermore, the Department has an independent interest in promoting mentoring and other support services that can turn juveniles away from destructive behaviors and toward constructive behaviors. It includes education by the fact that IHAD programs are school and community based; it includes

housing because many IHAD programs recruit their participants from public housing. Abt Associates can provide researchers with substantive knowledge in these interconnected areas as well as experts with the requisite tools of evaluation research.

Abt Associates has played an important role in the dramatic transformation of American criminal justice policy and practice that has occurred during the last 30 years. We provide clients with policy-oriented research and evaluation, performance measurement, training and technical assistance, and translation and synthesis of research. Much of that work has been done for the National Institute of Justice. Principal scientist and Abt fellow William Rhodes (Ph.D., economics, University of Minnesota, 1974) will lead this evaluation. Rhodes directed the research leading to this proposal, and he is currently working on other juvenile mentoring/development projects: an NIJ-sponsored evaluation of Youth Crime Watch programs and Abt's evaluation of ED's Safe and Drug Free School Mentoring program. He will be joined by Tom Rich and Audrey Martinez, both of whom are juvenile justice researchers. Rich currently directs our evaluation of Youth Crime Watch programs and is the principal developer of our widely distribute School Cop software that schools use to track and analyze incidents occurring on their campuses. Martinez—a Mexican-American research assistant, who is currently enrolled in the Harvard International Education Policy masters program—has worked with Rhodes and Rich on both the IHAD evaluation design and the Youth Crime Watch evaluation, she has worked with education researchers conducting Abt's evaluation of the Even Start Literacy program.

Abt Associates' leadership in education research and evaluation spans more than 30 years. We work at all levels of education—from primary to post-secondary. Our focus includes kindergarten through 12th-grade systemic improvement, adult and vocational education, school-to-work transition programs, and programs aimed at improving education opportunities for disadvantaged adults and youths. We have evaluated and designed special programs for school governance and finance

reforms, math and science curricula, and adult basic education. Since 1985 we have had a growing role in U.S. post-secondary education in the United States, including assessing the ways students and their families finance post-secondary education and possible constraints to access and graduation, and evaluating the effectiveness of programs designed to increase the enrollment of minority students in colleges and universities. Senior associate Larry Bernstein (Ph.D., University of Pittsburgh, Educational Research Methodology) will join our evaluation team. He has a lengthy resume in education research including having served as an advisor on the design of the IHAD evaluation plan and being the project director for our Safe and Drug Free School Mentoring program evaluation.

Over four decades Abt Associates has worked to shape housing and community revitalization practices and policy at the U.S. federal, state, and local government levels. We use our research and technical assistance expertise to promote healthy, sustainable communities, enhancing the quality of life for families and individuals. The world of Public Housing has changed dramatically in recent years with approaches and methods from the private sector now setting the standard for how agencies think about and manage their stock. Housing agencies face an array of options as they seek ways to revitalize and reposition their properties, use their resources more flexibly and strategically to meet local housing needs, and operate more efficiently in response to changes in funding levels and allocation methods. Abt Associates works with PHAs to manage this wave of change. We assist local housing agencies in every area, from strategic planning and asset management to training staff on the basics of rent calculations, program policies, and procedures. This experience provides a firm grounding for working with public housing projects as they interact with the IHAD program.

5.   Bibliography

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Appendix A
Technical Research Design

The "I Have a Dream" program (IHAD) is a privately sponsored educational intervention intended to improve the school experiences and performances of underprivileged children, and to promote and support their post-secondary school education. The National Institute of Justice has contracted with Abt Associates to design an evaluation for the IHAD program.

We (the Abt study team) have provided NIJ with a proposal for evaluating the IHAD program across 27 projects in 5 places. The planned study will evaluate whether or not IHAD has decreased dropout rates, increased post-secondary education rates, and generally improved school experiences and outcomes for IHAD participants. Specifically we seek to test three generic hypotheses:

- H1: Does IHAD cause children to remain in school longer than they would otherwise?
- H2: Does IHAD cause children to have better school and post-graduate experiences than they would otherwise?
- H3: Does IHAD cause children to attend post-secondary school more frequently than they would otherwise?

Establishing a counterfactual for the first and third hypotheses is relatively straightforward once we have identified a suitable comparison group of children who did not participate in IHAD. Given a suitable comparison group, the percentage of children who remain in school as of a specified date is a measure of school retention (H1) or post-secondary school continuation (H3). A complication arises because school recordkeeping causes children who withdraw (drop-out) from school and those who transfer to other school districts to be observationally equivalent. Establishing a counterfactual for the second generic hypothesis is more complicated. The problem is that children with relatively negative school experiences may withdraw from school in the absence of IHAD, but they may stay in school in the presence of an IHAD program. Consequently, comparisons that are made after children reach an age at which they can legally leave school may be biased against the IHAD program.

The proposal mentions these problems and summarizes solutions. Given the thirty page limitation on proposal length, however, the proposal could not provide a full discuss of these important issues. This technical appendix provides that detail.

1.0 The Evaluation Design

For purposes of discussion, we assume that the IHAD group was formed in an elementary school during the 3rd grade. Operationally the sponsor selects a 3rd grade class in an elementary school. The sponsor hires a project coordinator who assures that Dreamers receive mentoring and other support services. After leaving the 5th grade the Dreamers

disburse to different middle schools and eventually to different high schools.[1] They remain Dreamers and continue to receive mentoring and support services regardless of where they go to school, provided that they remain within the jurisdiction of the Board of Education (BOE). Transfers out of the BOE jurisdiction are lost to both the IHAD program and to this evaluation, a fact that will have consequences for the evaluation design.[2]

Although this discussion assumes an evaluation in a single school, we would actually conduct the evaluation across several IHAD programs. Thus this evaluation will produce multiple estimates of the effectiveness of IHAD programs. Later we will discuss how those estimates will be combined. Meanwhile we will focus on a general design for deriving estimates of IHAD effectiveness for a single school. Essentially this single-school design will be repeated with some variation across multiple IHAD programs.

It is difficult to see how we could set-up a randomized design given that all children in the 3rd grade are IHAD participants. Furthermore, we are envisioning a retrospective evaluation so that we can take advantage of ongoing IHAD programs. Dreamers could not be selected randomly on a retrospective basis. Therefore, within any single school, random assignment designs are impractical.

Fortunately, a strong quasi-experimental design is provided by the experiences of children who entered the 3rd grade in the same elementary school both one year before and one year after the Dreamers.[3] Hereafter we will call the comparison group members NonDreamers, with no pejorative connotation intended.

2.0 Intent to Treat

We will be testing the generic hypotheses H1, H2 and H3 by comparing the outcomes for children who participate in IHAD with the outcomes for children who do not participate in IHAD. This observation raises the question of what we mean by *participate*? For evaluation purposes, a Dreamer is a child eligible for IHAD in the 3rd grade, regardless of the whether he or she chose to be active in IHAD activities. A NonDreamer is a child who was in the 3rd grade one year before or one year after the formation of the IHAD

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[1] For convenience we assume that elementary school lasts from grade 1 through 5, that middle school lasts from 6 through 8, and that high school lasts from 9 through 12.

[2] Practicality dictates this choice. We use New York City as an example. The New York City Department of Education is organized into 10 regions and 32 school districts. Through negotiations with the New York City Department of Education, we expect to access records of children who remain in New York City schools. Tracking the continued education experiences of children outside of New York City would require negotiations with multiple other school districts, presuming that we know the destination of the transfer. We may not know the destination of the transfer, and anyway, negotiating for records from multiple school districts would be prohibitively expensive.

[3] Davis and Arras with Hyatt (1998) used a similar design to evaluate the IHAD program in the Portland Public Schools. Their Dreamers were drawn from the 1998 graduating class. Their NonDreamers were drawn from the 1995 and 1997 classes. Strusinski (1997) compared 3rd grade IHAD classes adopted in 1993 with I cannot follow Linda's notes. An ongoing study by researchers at Yale is comparing the 5th grade IHAD class adopted in 1993 with the 4th grade class in 1993 and the 5th grade class in 1992.

program, even if he or she managed to participate in IHAD activities. We will compare the outcomes for Dreamers with the outcomes for NonDreamers.

Evaluators sometimes use the terms *refusal* and *crossover* when referring to situations where a subject refuses to participate in an intervention (refusal) or declines to remain a comparison group member (crossover). If a child is selected to be a Dreamer but declines to actively participate in the IHAD program, he or she is a refusal. If a child is selected to be a NonDreamer but manages to participate in IHAD, he or she is a crossover.[4] The intent-to-treat model retains refusals as Dreamers, because to do otherwise would invite selection bias resulting from the fact that those who refuse may differ materially from those who comply with the IHAD program. The model retains crossovers as NonDreamers for the same reason. The test of a null hypothesis asks whether or not the Dreamers (including refusals) do better than the NonDreamers (including crossovers).

The test may seem strange to reviewers who are not trained in evaluation science. After all, by including refusals as Dreamers, an evaluator would seem to be diluting the effect of an IHAD program. Also, by including crossovers as NonDreamers, an evaluator would seem to be artificially enhancing the performance of the comparison group, presuming that IHAD does contributed something to the performance of crossover NonDreamers. Comparing the outcomes for Dreamers (including refusals) with the outcomes for NonDreamers (including crossovers) would seem to understate the value of IHAD participation.

In fact, techniques have evolved for estimating the size of an undiluted treatment effect in the presence of refusals and crossovers. See Bloom (1984) and Angrist (2004). Section 3.1 provides the basic estimator based on Bloom's approach. Section 3.2 provides a regression-based alternative that has some desirable features and provides the basic estimator proposed for this study. Subsequent sections will modify this basic estimator to account for other special problems.

2.1 Intent to Treat: The Bloom Estimator

We begin with a simple illustration of the case with refusals but no crossovers. Let:

$E(\bar{Y}_1)$ This is the expected value of the average outcome for children who would participate in IHAD if given the opportunity, conditional on not actually participating in IHAD.

$E(\bar{Y}_2)$ This is the expected value of the average outcome for children who would not participate in IHAD. If given the opportunity, these children would be refusals.

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[4] Crossovers can arise in at least two ways. A Dreamer's siblings may be admitted to at least some Dreamer activities, and if those siblings had originally been selected to be NonDreamers, they would become crossovers. It also seems plausible that a child who was in the 4th grade when the Dreamers were formed could be integrated into the Dreamer group if he or she failed to advance to the 5th grade. We are uncertain of the frequency of this occurrences, but we admit them as possibilities.

    Δ This is the size of the treatment effect, which we assume to be equal for all children who participate materially in the IHAD program. Assuming a constant treatment effect is convenient for the algebra, but unnecessary. Treatment effectiveness may vary over participants, in which case we are estimating the average treatment effect.[5]

    P This is the proportion of third grade students who would participate in IHAD if given the opportunity. Thus the probability of being a refusal is 1-P.

Using this notation, we express the expected value of the average outcome for NonDreamers as:

[1] $\qquad E\left(\overline{Y}_N\right) = E\left(\overline{Y}_1\right)P + E\left(\overline{Y}_2\right)(1-P)$

and we can express the expected value of the mean outcome for Dreamers as:

[2] $\qquad E\left(\overline{Y}_D\right) = E\left(\overline{Y}_1 + \Delta\right)P + E\left(\overline{Y}_2\right)(1-P) = E\left(\overline{Y}_1\right)P + E\left(\overline{Y}_2\right)(1-P) + \Delta P$

Substituting [1] into [2] and solving the resulting equation for Δ gives:

[3] $\qquad \Delta = \dfrac{E\left(\overline{Y}_D\right) - E\left(\overline{Y}_N\right)}{P}$

Finally, substituting the observed values for the outcomes in place of the expectations gives an estimate for Δ:

[4] $\qquad \hat{\Delta} = \dfrac{\overline{Y}_D - \overline{Y}_N}{P}$

The null hypothesis is that $\Delta = 0$. A difference of means test based on the numerator is a suitable small sample test of the null hypothesis that Dreamers do no better than NonDreamers. Note that this test is based on the "diluted" treatment effect, and the larger the participation rate, the greater the statistical power. Division by P "corrects" the estimate for the fact that only a proportion P of Dreamers actively participated in the IHAD program. The asymptotic variance[6] for $\hat{\Delta}$ is:

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[5] To be more precise, we are estimating the local average treatment effect. Under special conditions, this is the same as the average treatment effect on the treated. Under still other special conditions, this is the average treatment effect for the population of Dreamers. We will ignore these distinctions in this discussion.

[6] We are indebted to Stephen Kennedy, Chief Scientist at Abt Associates, for his explanation of this variance term. The first order expansion for the ratio of two stochastic variables, x/y, is (for E(y) not equal to zero):

x/y = E(x)/E(y) + [1/E(y)][x - E(x)] - {E(x)/[E(y)]2} [y - E(y)] + terms of Order (1/n)

Var(x/y) = Var(x)/[E(y)]2 - 2 {E(x)/[E(y)]2}Cov(x,y) + {[E(x)]2/[E(y)]4}Var(y)

$$[5] \qquad VAR(\hat{\Delta}) = \left(\frac{1}{P}\right)^2 VAR(\overline{Y}_D - \overline{Y}_N)$$

The asymptotic variance in expression [5] can be used for confidence intervals and for the meta analysis proposed as a device to combine effects across IHAD programs.

If there are crossovers, and if Q is the proportion of NonDreamers who actually participate in IHAD, then the estimator becomes:

$$[6] \qquad \hat{\Delta} = \frac{\overline{Y}_D - \overline{Y}_N}{P - Q}$$

There may be one prudent exception to the general rule for the intent-to-treat model. Although Dreamers are created from a class of Dreamers in the 3rd grade, most IHAD programs will incorporate as Dreamers those children who transfer into that same class cohort during the 4th or 5th grade. We would treat those children as Dreamers as well. One reason for doing this is that out-of-district transfers are material for schools in impoverished neighborhoods. Including 4th and 5th grade transfers as Dreamers will increase sample size and improve statistical power.

There is a danger to allowing this exception, of course. If transfers were motivated by the ability to participate in the IHAD program, then allowing these exceptions would introduce selection bias. There is a way to test for selection bias. This test would require that we include 4th and 5th grade transfers into the NonDreamer group as well. Then we could compare the original Dreamers with the original NonDreamers and we could also compare the transfer Dreamers with the transfer NonDreamers. If selection bias exists, we would expect the results from the second comparison to differ from the results from the first comparison. If they do not, we would not consider selection bias to be a large problem. If there were a problem, we would conclude that the augmented sample should not be included in the study.

2.2 Intent to Treat: The Regression Estimator

As a computational device, we could estimate [4] using a regression. (We show this at 3.3) To explain, define:

Y_{ijk} This is the observed outcome for the ith student in the jth category (j=D for Dreamers and j=N for NonDreamers) at the kth time. The index k indicates when we take a measurement of the outcome variable with respect to when the study

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The correlation and variance of the denominator drop out of the asymptotic variance whenever the expected value of the numerator is zero. This is because the terms in the denominator-numerator covariance and denominator variance are multiplied by the expected value of the numerator.

subject was in 3rd grade. For example, if k=7, then the measurement is taken seven years after 3rd grade when most study subjects will be in the 10th grade.

D_j A dummy variable coded one if a Dreamer (j=1) and zero otherwise.

We can estimate Δ by using the regression:

[7] $Y_{ijk} = \mu_N + \Delta P D_j + e_{ij}$

There are two parameters: μ_N is the average outcome for NonDreamers; as before, Δ is the treatment effect. Note the variable PD_j that is the product of the proportion of Dreamers who participate actively in IHAD (P) and the dummy variable denoting that the child was a Dreamer (D_j). Beyond being a computational device, [7] can be readily extended to include covariates in the model. Instead of estimating [7], we could add a vector of control variables and estimate:

[8] $Y_{ijk} = \mu_N + \Delta P D_j + X_{ij}\beta + e_{ij}$

There is another advantage of turning to the regression model. Suppose that the treatment effect were random such that the treatment effect for the ith student were $\Delta_i = \overline{\Delta} + u_i$ and u_i is a mean zero random effect. Substituting Δ_i for Δ in the regression, and noting that the random effect u_i gets captured in the error term, shows that the regression will estimate the average treatment effect.[7]

2.3   Equivalency of the Regression Estimator and the Bloom Estimator

Consider the regression:

[9] $Y = \beta_0 + \beta_1 P \bullet D + e = X\beta + e$

where:

Y a 1xN column of outcome variables for the n NonDreamers and d Dreamers, such that N=n+d.

P a scalar, the proportion of Dreamers who participate in the IHAD program.

D a 1xN column vector comprised of 0s for NonDreamers and 1s for Dreamers.

The covariance matrix is:

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[7] This presumes independence between e and u. We will not discuss that problem here. Nor will we thoroughly discuss what *average* actually means, because the answer depends on the processes that affect participation decisions. We reserve this discussion for later.

[10] $\quad X'X = \begin{bmatrix} n+d & Pd \\ Pd & P^2d \end{bmatrix}$

The inverse is:

[11] $\quad [X'X]^{-1} = \begin{bmatrix} \dfrac{1}{n} & -\dfrac{1}{Pn} \\ -\dfrac{1}{Pn} & \dfrac{n+d}{P^2dn} \end{bmatrix}$

The vector $X'Y$ is:

[12] $\quad X'Y = \begin{bmatrix} n\bar{Y}_n + d\bar{Y}_d \\ Pd\bar{Y}_d \end{bmatrix}$

The ordinary least squares solution for the parameters is:

[13] $\quad \begin{bmatrix} \beta_0 \\ \beta_1 \end{bmatrix} = [X'X]^{-1} X'Y = \begin{bmatrix} \bar{Y}_N \\ -\dfrac{\bar{Y}_N - \bar{Y}_D}{P} \end{bmatrix}$

The solution for β_1 is the same as was given by equation [4] in the main text. The parameter covariance matrix is:

[14] $\quad \begin{bmatrix} \sigma^2_{\beta_0} \\ \sigma^2_{\beta_1} \end{bmatrix} = [X'X]^{-1} \sigma^2_e$

Substituting from [11] and solving for the second term in the resulting vector gives:

[15] $\quad \sigma^2_{\beta_1} = \sigma^2_e \left[\dfrac{n+d}{P^2nd} \right] = \dfrac{1}{P^2} \left[\dfrac{\sigma^2_e}{d} + \dfrac{\sigma^2_e}{n} \right]$

It remains to show that:

$$VAR(\bar{Y}_D - \bar{Y}_N) = VAR(\bar{Y}_D) + VAR(\bar{Y}_N) = \dfrac{\sigma^2_e}{d} + \dfrac{\sigma^2_e}{n}$$

To see this, note that:

[16] $\quad \sigma^2_e = \dfrac{VAR(Y_D)(d-1) + VAR(Y_N)(n-1)}{d+n-2}$

7

3.0   Testing Hypotheses H1 and H2

Although there are some complications, which are discussed here, testing the generic hypotheses H1 and H3 is straightforward using the intent-to-treat framework. Generic hypotheses H1 and H3 are of the same form, asking whether Dreamers are more likely than NonDreamers to have status S at time T years post 3rd grade. Status might be the condition "remained in school," or "graduated from high school," or "entered college." Other statuses are possible, but the key is that the status is known for all Dreamers and NonDreamers.

There are a few complications. A minor complication arises because the Dreamer group is formed in year T1, when Dreamers are in the 3rd grade. One group of NonDreamers is formed in year T1-1, when they are in 3rd grade. The other group of NonDreamers is formed in year T1+1, when they are in the 3rd grade. Thus the evaluation will compare the status of Dreamers at time T2 with the status of NonDreamers from the first group at time T2-1 and of NonDreamers from the second group at time T2+1.

Possibly there is a time trend; so comparing Dreamers with NonDreamers who are in classes one year before or one year after the Dreamer class could provide biased estimates of the treatment effect. If the trend were linear, averaging the differences between the lagged and advanced comparison groups would provide an unbiased estimate of the treatment effect. This is one reason for selecting comparison groups from 3rd grade classes that were formed one year before and one year after the Dreamer's 3rd grade class.

A second, more serious complication arises because children transfer among schools, and this has implications for testing hypotheses about status changes. The rest of the section discusses estimation given out-of-district transfers. As before, we begin with a simple estimator that illustrates the approach, and then we provide a regression-based counterpart.

3.1   The Problem of Drop-Outs

Table A1 (adapted from Davis and Hyatt, 1998) shows the number of students from three cohorts who were enrolled in the Portland Public Schools through the 12th grade. The Dreamers began in the 5th grade, and they are compared with 5th graders from the same school from two earlier cohorts. The Dreamers had not graduated at the time that data collection ended, and the sample size is small, but nevertheless informative. By the 9th grade between 11 and 17 percent of children had transferred from the Portland schools. By the 10th grade and thereafter, we presume that children began to withdraw from school. The bottom part of the table treats the 9th grade as a base and reports the percentage of those enrolled in the 9th grade who remained in school. The table makes two points: First, in the Portland Schools, an appreciable proportion of children transfer from the school district. The proportion would have been higher if the IHAD group had formed in the 3rd grade rather than the 5th grade, and of course, some students in grades

10 through 12 were transfers. Given that about 4% of students per year transfer from the Portland Schools, we project that about 39% of the original Dreamers and NonDreamers would transfer by the 12th grade. The second point is that an appreciable proportion of these children drop from school. Projecting school transfers at 4% per year, about 90 percent of the NonDreamers graduate. These projections are not reported in the table.

	Grade							
	5th	6th	7th	8th	9th	10th	11th	12th
Class of '95	101	94%	92%	91%	89%	74%	69%	58%
Class of '97	99	99%	92%	87%	83%	80%	73%	64%
Dreamers '98	110	100%	90%	88%	83%	78%	75%	NA
Class of '95						83%	78%	66%
Class of '97						96%	88%	77%
Dreamers '98						95%	90%	NA

3.2 Testing Hypotheses H1 and H2: A Simple Estimator

To explain the simple estimator, we assume there are neither refusals nor crossovers, so we can ignore the problems they cause. Putting that issue aside, we consider testing the null hypothesis that Dreamers and NonDreamers are equally likely to remain in school as of time t, where time t is measured relative to when the Dreamers and NonDreamers were in 3rd grade. The test statistics S can be written:

[17] $$S = \frac{N_D - T_{Dt} - D_{Dt}}{N_D} - \frac{N_N - T_{Nt} - D_{Nt}}{N_N}$$

where:

N   This is the size of the 3rd grade class for Dreamers (N_D) and NonDreamers (N_N) at the time that these groups were formed.

T   This is the number of transfers as of time t for the Dreamers (T_{Dt}) and NonDreamers (T_{Nt}). We only consider transfers outside the school district to be transfers. A child who transfers within the school district is not considered to be a transfer for purposes of this evaluation because we can continue to track his or her records.

D   This is the number of drop-outs as of time t for Dreamers (D_{Dt}) and NonDreamers (D_{Nt}).

We can identify children who transfer within the school district, and they cause no problems. However, we cannot distinguish between children who transfer out of the school district and those who discontinue their educations, because school records do not

differentiate. Both are simply "lost to follow-up." Thus, while we can observe the sum
of T and D, we cannot observe T and D separately. This has consequences for the test
statistic.

Provided the probability of transferring out of the school district is independent of
participating in IHAD, the test statistic is still a valid statistical test of the null hypothesis
that IHAD does not change the probability that a child will withdraw from school.
However, it has excessive noise due to our inability to identify transfers. Furthermore,
we would like to estimate the treatment effect with respect to children who did not
transfer out of the district, not with respect to the original base, which includes children
who did transfer. With some manipulation, we can do better.

While school records do not distinguish between out-of-district transfers and withdrawals
from school, we can presume that any child who disappears from the sample before the
age of sixteen[8] has transferred out of the district because he or she could not have
withdrawn legally before that age. Let:

t_{D16} This is the number of Dreamers who have transferred from school before age 16.

t_{N16} This is the number of NonDreamers who have transferred before age 16.

Using this information, we can rewrite the test statistics as:

$$[18] \quad S = \frac{(N_D - t_{D16}) - (T_{Dt} - t_{D16}) - D_{Dt}}{N_D - t_{D16}} - \frac{(N_N - t_{N16}) - (T_{Nt} - t_{N16}) - D_{Nt}}{N_N - t_{N16}}$$

This revision of the test statistics has two advantages. The first is that the test statistic
based on [18] will be more efficient than the test statistics based on [17], because [18]
removes some of the noise resulting from failure to distinguish between transfers and
withdrawals. The second advantage is that the estimate of treatment effectiveness based
on [18] might be deemed a better measure of IHAD effectiveness than is the measure
based on [17]. To explain, consider a simple illustration.

In this illustration, suppose there were 100 original Dreamers and 100 original
NonDreamers. Of those 100 Dreamers, 10 transferred out of the District, so they are lost
to the Dreamer program and to the evaluation; of the remaining Dreamers, 10 withdraw
from school. Of the 100 NonDreamers, 10 transferred out of the District, and 20
withdraw from school. Using the first test statistics [17], we would conclude that the
improvement rendered by IHAD was:

$$\frac{100 - 10 - 10}{100} - \frac{100 - 10 - 20}{100} = -\frac{10}{100}$$

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[8] In Massachusetts, a child can leave school with parental permission at the age of sixteen; he or she can
leave school without parental permission at the age of seventeen. The age varies across states, and we
would substitute the state-specific legal age in the actual evaluation. We are indebted to Susan Atwater-
Rhodes, Vice-Principal at the Acton-Boxboro High School, for discussing this and other issues with us.

IHAD has reduced withdrawals by 10 children or 10 percent of the base. Suppose, however, that we knew that 5 Dreamers and 5 NonDreamers had transferred before age 16. The new test statistic based on [18] is:

$$\frac{95 - 5 - 10}{95} - \frac{95 - 5 - 20}{95} = \frac{10}{95}$$

Clearly 10/95 is a better and more justifiable success rate than 10/100.

We may be able to do better than this. Suppose that the data justified the conclusion that the transfer rate is constant before the age of sixteen. We might feel justified in inferring that the transfer rate would be constant for children who are sixteen, seventeen and eighteen and use that information to estimate the number of transfers for children in those older age ranges. We could modify the test statistic in a fashion that is analogous to what was done for transfers under the age of sixteen. Provided the predictions for transfers by older children do not have too higher of a sampling variance, this should improve the efficiency of the test statistic. Furthermore, it would provide a more meaningful measure of the effectiveness of the IHAD program. To see this, presume that the predictions are perfectly accurate, so that the test statistic becomes:

$$\frac{90 - 0 - 10}{90} - \frac{90 - 0 - 20}{90} = \frac{10}{90}$$

All three test statistics imply that IHAD reduces the number of school withdrawals by 10 children. However, the third test statistics says that IHAD reduces the number of school withdrawals by 10 per 90 Dreamers who remain in the school district, and this rate seems to be a better measure of program success.

The test statistic is complicated when we substitute predictions of transfers for observations of transfers. A regression-based estimator provides one way to deal with this issue, and we turn to the regression-based estimator next.

3.3   Testing Hypotheses H1 and H2: A Regression-Based Estimator

We would like a regression-based counterpart to equation [18] that would take the differences between transfers and dropouts into account. Here we consider a competing events survival model. In this model, there is a probability of transferring out of the school district during every year post-3rd grade. The event "transfer from the school district" can be observed prior to age sixteen, so the parameters that affect the probability of transferring are identifiable provided there is some regularity between transferring prior to age sixteen and transferring after age sixteen.[9] There is a *latent* probability of

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[9] We would consider the process of transferring to be regular if the hazard is constant over time. It would also be regular if the hazard increased or decreased linearly over time. We need to estimate the hazard after

withdrawing from school during every year post-3rd grade. The probability is latent
because a child cannot actually withdraw before age sixteen. Thus the estimated
probability of withdrawing at age sixteen is the sum of the latent probabilities of
withdrawing before age sixteen and at age sixteen.[10] The events transfer and withdraw
are competing because if one occurs first, the other cannot be observed.

Specifically, let:

Z_{1ijk} This is a latent variable for the ith student in the jth group at time k. The student

transfers from school if $Z_{1ijk} \geq 0$ and does not transfer otherwise.

Z_{2ijk} This is a second latent variable for the ith student in the jth group at time k. The

student withdraws from school if $Z_{2ijk} \geq 0$ and does not otherwise.

Specify the latent variable as linear functions[11]:

[19] $Z_{1ijk} = \alpha_0 + \alpha_1 D_j + \alpha_2 AGE_{ijk}$

[20] $Z_{2ijk} = \beta_0 + \beta_1 D_j + \beta_2 AGE_{ijk}$

We are especially interested is an estimate of β_1. A test of the null hypothesis $\beta_1 = 0$ is a
test of the effectiveness of treatment. We have added a single covariate AGE because the
probability of leaving school and the probability of transferring from the school district
may both be time sensitive.

Define k so that k=0 means *in the 3rd grade*, k=1 means *one year post-3rd grade*, k=2
means *two years post-3rd grade*, and so on. Then we define the hazard[12] of leaving
school at time t as:

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age fifteen based on data regarding transfers before age sixteen, so by "regular" we mean that the data from
before age sixteen can identify the hazard post age-fifteen.

[10] The assumption is that children who desire to quit school before age sixteen quit immediately upon
reaching the age of sixteen. This may not be true. Continuing to use Massachusetts as our illustration, a
child is required to remain in school until age seventeen if his or her parents do not authorize his earlier
withdrawal. This possibility complicated the estimation because we would expect the built-up demand for
withdrawing from school to clump over a two-year period rather than during the year when a child turns
sixteen. The solution is to treat the two-year period as a single unit rather than as two separate units. We
do not discuss the issue any more here.

[11] In practice we could add covariates to the two functions provided those covariates are exogenous. For
example, gender and test scores prior to the 3rd grade might be added as covariates. We could add errors
terms to each of the regressions. Presuming that covariates to not capture all the systematic variation
across the study subjects, the parameter estimates would be biased were the error terms omitted.
Furthermore, as represented by [11] and [12], the latent variables are independent. The addition of error
terms and a joint distribution is one way to introduce dependence among the latent variables. However, the
addition of the error term greatly increases the estimation problem. We ignore those problems in this
discussion.

[21] $H_{1ijk} = F(Z_{1ijk})$ for transfers and

[22] $H_{2ijk} = F(Z_{2ijk})$ for withdrawals where F() is a suitable function assuring that the hazard is between 0 and 1.

Then the probability of transferring from school at time t is:

[23] $P_{1ijk} = H_{1ijt} \prod_{k=1..t-1}(1 - H_{1ijk})$

The *latent* probability (hence the prime) of leaving school at time t is:

[24] $P'_{2ijk} = H_{2ijt} \prod_{k=1..t-1}(1 - H_{2ijk})$

This is a latent probability because a child cannot leave school before age sixteen. Assuming that a child was eight years old in the 3rd grade and, hence, sixteen years old at k=8, we can write the actual probability of withdrawing from school before t=8 as zero and the probability of withdrawing from school at t=8 as:

[25] $P_{2ij8} = \sum_{k=0..8} P'_{ijk}$ and $P_{2ijk} = P'_{ijk}$ for k>8.

Section 3.1 discusses the likelihood function for estimating the parameters of this model. Once the parameters are estimated, we can ask: What is the estimated treatment effect?

We can express the cumulative probability of withdrawing from school as of time t (for t>7) as:

[26] $Q_{2ijt} = \sum_{k=8..t} P_{2ijk}$

The marginal change attributable to treatment is then:

[27] $\Delta_{iDt} = \frac{\partial Q_{2ijt}}{\partial Z_{2iDt}} \alpha_1$

The exact form of the derivative will depend on assumptions made about Q. See appendix 3. The estimated treatment effect is then the average of the individual treatment effects where the average is taken over all Dreamers.

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[12] The hazard is the probability of leaving school during year t conditional on being enrolled in school at the beginning of year t. Years start at the beginning of a school year and end at the end of a school year, because children cannot be observed to transfer or to withdraw during the summer recess. A child is sixteen during a year if he or she turns sixteen during that school year.

[28] $\Delta_t = \sum \Delta_{2iDt}$

Although we have assumed there are no refusals and no crossovers, we can easily drop that assumption. If we assume there are both refusals and crossovers, then equation [20] is the average treatment effect including refusals and crossovers. The treatment effect from [20] is analogous to the numerator in equation [6]. The adjustment appearing in equation [6] could be transferred to this context.

3.1   The Likelihood

3.1   Estimating the Probability that a Child Remains in School

We wish to determine how IHAD has affected the probability of remaining in school for children who are sixteen and older. The difficulty with estimation is that we cannot generally distinguish between children who withdraw from school and those who transfer out of the school district. However, we can identify transfers prior to the child turning 16, and we exploit that information to identify the effect that participation in IHAD has on withdrawing from school.

We can distinguish among three states that exist as of t years after the child was in third grade:
- The child transferred from school district at time t and that transfer occurred before the age of sixteen. Time t refers to a school year. The transfer could have occurred anytime during that year.
- The child either transferred or withdrew from school at time t and the transfer/withdrawal occurred when the child was sixteen or older. We cannot distinguish between transfers and withdrawals.
- The child remained in school as of time t.

We associate a logic variable with each of these three conditions. If condition 1 holds, then the logic variable [S=1] equals 1 and otherwise equals zero. If condition 2 holds, then the logic variable [S=2] equals 1 and otherwise equals zero. If condition 3 hold, then the logic variable [S=3] equals 1 and otherwise equals zero.

The main text developed the probability structure for these outcomes. This appendix elaborates and builds a likelihood function based on that probability structure. From the main text:

P_{hijt} The probability that the ith child in the jth group (j=D for Dreamers and N for NonDreamers) will transfer (h=1) or withdraw from school (h=2) at time t.

We will need a join probability. Assuming that the events "transferring out of the school district" and "withdrawing from school" are independent, the probability of both occurring at time t is:

[29] $\quad P_{Bijt} = P_{1ijt} + P_{2ijt} - P_{1ijt}P_{2ijt}$

Finally, we seek the probability of being in school at time t. This is one minus the probability of have departed from school. Hence we write the probability of being in school at time t as:

[30] $\quad P_{Sijt} = 1 - \sum_{k=0..t} P_{Bijk}$

The contribution that the ith study subject makes to the likelihood is then:

[31] $\quad L_{tj} = P_{1ijt}^{[S=1]} P_{Bijt}^{[S=2]} P_{Sijt}^{[S=3]}$

If the child transferred before age 16, t is the year of the transfer. If he or she left school after the age of 15, then t is the year that he or she left school. Of he or she remains in school, then t is the last full observed year. P_{1ijj} is identified because we could always estimate the parameters of P_{1ijt} using data censoring at t=15. Substituting those estimates for P_{1ijt}, we could estimate the other parameters in the model.

Estimation becomes much more complicated if we add error terms to the above equations. The probability structure would then be conditional on those unknown errors. The solution is to adopt an error structure and integrate those unknown errors out of the equations.

5.0   Testing Hypotheses H2

Testing generic hypothesis H2 raises no special problems not already discussed above provided we test the hypothesis for children younger than 16. After that age, children can legally withdraw from school. This creates a problem because children with relatively negative school experiences may be most prone to leave school. If IHAD manages to retain at least some of those otherwise dropouts, a comparison based on the remaining Dreamers and NonDreamers would understate the effectiveness of IHAD,

5.1 Testing Hypotheses H3: A Biased Estimator

To illustrate this problem and to motivate a solution, we assume that all Dreamers participate materially in IHAD activities and that none of the NonDreamers participate, so we can ignore the problems posed by refusals and crossovers. We also assume there are no transfers, so we can ignore problems posed by the fact that out-of-district transfers and withdrawals are observationally equivalent. Here we just focus on the problem posed by school withdrawals after the age of 15.

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To explain, let:

$E(\bar{Y}_1)$ This is the expected value of the average outcome for children who would remain in school regardless of whether they did or did not participate in IHAD, conditional on not participating in IHAD.

$E(\bar{Y}_2)$ This is the expected value of the average outcome for children who would remain in school if they participate in IHAD but who would withdraw from school if they do not participate in IHAD, conditional on not participating in IHAD. Obviously this expected value is a theoretical construct.

$E(\bar{Y}_3)$ This is the expected value of the average outcome for children who would withdraw from school regardless of whether they did or did not participate in IHAD. This, too, is a theoretical construct.

Δ This is the size of the treatment effect, which we assume is the same for all children who participate in the IHAD program.

P_1 This is the proportion of children who would remain in school regardless of IHAD participation.

P_2 This is the proportion of children who would remain in school provided they participate in IHAD but who would withdraw from school if they do not participate in IHAD.

P_3 This is the proportion of children who would withdraw from school regardless of IHAD participation: $P_3 = 1 - P_1 - P_2$.

Define:

[32]
$$\Pi_N = P_1 E(\bar{Y}_1) + P_2 E(\bar{Y}_2) + P_3 E(\bar{Y}_3)$$
$$\Pi_D = P_1 E(\bar{Y}_1) + P_2 E(\bar{Y}_2) + P_3 E(\bar{Y}_3) + \Delta$$

Π_N is the expected value of the theoretical outcomes for NonDreamers, and Π_D is the expected value of the theoretical outcome for Dreamers. Neither expectation has an observable counterpart because we cannot observe the outcomes for children who withdraw from school. It seems sensible to ignore the third groups of children, who would withdraw from school under any condition, and focus on the first two groups. Setting $Q = P_1/P_2$, we rewrite [32] as:

[33]
$$\Pi_N = Q E(\bar{Y}_1) + (1-Q) E(\bar{Y}_2)$$
$$E(\bar{Y}_D) = Q E(\bar{Y}_1) + (1-Q) E(\bar{Y}_2) + \Delta$$

Π_N still lacks an observable counterpart because some NonDreamers have withdrawn from school; $E(\bar{Y}_D)$ has an observable counterpart. If we <u>could</u> observe Π_N, we could estimate the treatment effect based on the expectation:

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[34] $\Delta = E(\bar{Y}_D) - \Pi_N$

But given what we can observe, we might instead estimate the treatment effect based on the expectation:

[35] $\Delta' = E(\bar{Y}_D) - E(\bar{Y}_1)$

Equation [35] is a biased measure of the treatment effect. Substituting $E(\bar{Y}_1)$ from [33] into [35], we can see that the size of the bias is:

[25] $\Delta' - \Delta = (1 - Q)[E(\bar{Y}_2) - E(\bar{Y}_1)]$

If children who withdraw from school tend to perform at a lower level then children who remain in school, then the bias will be negative because the term in brackets will be negative.

5.2   Placing Bounds on the Treatment Effect

Manski (1995) recommends placing bounds on the treatment effect when selection bias is an issue. Although putting a lower bound on treatment effectiveness is often impractical, meaningful lower limits exist for the effectiveness of the IHAD program.[13] Formula [24] provides an estimate that is a lower limit, but that lower limit may be unduly biased against IHAD. This subsection suggests an alternative estimator of the treatment effect that has a negative but smaller bias.

As before, let:

Y_{ij} This is the outcome for the ith student in the jth category (j=D for Dreamers and j=N for NonDreamers). The outcome can be observed if the child remains in school. Otherwise it cannot be observed.

Z_{ij} This is a latent variable. A child remains in school if $Z_{ij} > 0$. He withdraws from school otherwise. Whether or not his or she withdraws from school can always be observed.[14]

D_j A dummy variable coded one if a Dreamer (j=1) and zero otherwise.

The outcome is:

[37] $Y_{ij} = \mu + \Delta D_{ij} + e_{ij}$

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[13] When discussing limits, we abstract from sampling variation by implicitly considering infinite sized samples. In this regard the limit is nonstochastic. In fact, of course, sampling variation is a relevant consideration, and must be taken into account. In that regard the limit is stochastic.

[14] The base for this calculation is children as of the age of 15 presuming that the probability of transferring from school is independent of participation in IHAD.

The parameter μ is the expected value of the average outcome for NonDreamers. This average is based on NonDreamers who remain in school and those who have withdrawn. In the latter regard, the outcome is a theoretical construct. As before Δ is the treatment effect. The e is a random error reflecting the distribution of outcomes among Dreamers and NonDreamers. It has an expected value of zero.

The selection equation is:

[38]
$$Z_{ij} = \alpha_0 + \alpha_1 D_{ij} + \alpha_2 (Y_{ij} + u_{1ij}) + u_{2ij}$$
$$Z_{ij} = \alpha_0 + \alpha_1 D_{ij} + \alpha_2 Y_{ij} + u_{ij}$$

The term $(Y_{ij} + u_{1ij})$ is a child's judgment about his or her future performance. That judgment is estimated with error (u_{1ij}), and the two errors can be collapsed into a single one $(u_{ij} = \alpha_2 u_{1ij} + u_{2ij})$. A child remains in school if $Z_{ij} > 0$; he or she withdraws otherwise. We assume $\alpha_1 \geq 0$, because at worst we do not expect IHAD to provide children with an incentive to withdraw from school, and in fact we expect IHAD to encourage children to remain in school. We assume $\alpha_2 \geq 0$, because we expect that children who perform best in school have the greatest incentives to remain in school. The truth of these assumptions will be crucial for establishing limits. Fortunately, both assumptions are testable using procedures described in the previous section.

Substituting from [37] into [38], we can write the selection equation as:

[39]
$$Z_{ij} = \alpha_0 + \alpha_1 \mu + (\alpha_1 + \alpha_2 \Delta) D_{ij} + \alpha_2 e_{ij} + u_{ij}$$
$$Z_{ij} = \delta_0 + \delta_1 D_{ij} + w_{ij}$$

The sign of δ_1 will be important. As already noted, we assume that $\alpha_1 \geq 0$ and $\alpha_2 \geq 0$, so $\delta_1 \geq 0$ if IHAD improves outcomes.

Estimation requires two steps. In the first step we estimate the α parameters of the selection equation. (The previous section showed that these parameters can be identified.) This would be straightforward except that we cannot observe Y_{ij} for children who withdraw from school. Consequently we would use a proxy variable for Y_{ij}, namely, a lagged-value of Y_{ij} measured before the age of 16. For example, if class standing were the outcome measure, we would use class standing at the time that the child was 15 as the lagged-measure. Or, if performance on a standardized exam were the outcome measure, we would use earlier performance on a standardized exam. This would seem to mimic the way that children form their own expectations about the future.

Once the α parameters had been estimated, we would rank Dreamers and NonDreamers based on their estimated propensities to remain in school. Based on that analysis, we

would match Dreamers with NonDreamers so that the Dreamer with the highest probability of remaining in school would be matched with the NonDreamer with the highest probability of remaining in school, the Dreamer with the second highest probability of remaining in school would be matched with the NonDreamer with the second highest probability of remaining, and so on.[15]

Once the matches have been made, we would exclude all cases where one or both of the matched cases withdrew from school. Every remaining match would provide an estimate of Δ_i. The average over the Δ_i provides an estimate of Δ.

The estimate is biased. To investigate the nature of that bias, consider the estimate:

[40] $\Delta_i = Y_{iD} - Y_{iN}$ where i indicates the ith match.

[41] $\hat{\Delta} = \dfrac{\sum\limits_{i \in matched\ pair} \hat{\Delta}_i}{m}$ where there are m matched cases.

The expected value of the individual estimated treatment effect is:

[42] $E\left(\hat{\Delta}_i\right) = E\left(Y_{iD} \mid w_{iD} > -\left(\delta_0 + \delta_1 D_{iD}\right)\right) - E\left(Y_{iN} \mid w_{iN} > -\delta_0\right)$

By inspection, $E\left(w_{iD}\right) \le E\left(w_{iN}\right)$. Given the positive correlation between e_{ij} and w_{ij} (because $w_{iN} = \alpha_2 e_{ij} + u_{ij}$):

[43] $E\left(Y_{iD} \mid w_{iD} > -\left(\delta_0 + \delta_1 D_{iD}\right)\right) - E\left(Y_{iN} \mid w_{iN} > -\delta_0\right) =$
$\Delta + E\left(Y_{iN} \mid w_{iN} > -\left(\delta_0 + \delta_1 D_{iD}\right)\right) - E\left(Y_{iN} \mid w_{iN} > -\delta_0\right) < \Delta$

The estimate of the treatment effect is biased downward.

The bias should be smaller than the estimate based on [27]. The reason is that we expect the analysis to identify the Dreamers who have the highest probabilities of leaving school, and we also expect that those same Dreamers would be the lowest performers. Inclusion of those lower performers in [27] understates the treatment effect because their low performing counterparts among the NonDreamers are excluded from [27], thereby reducing the bias.
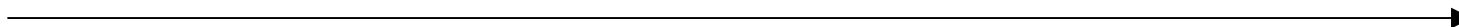
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[15] There are actually about two NonDreamers for every Dreamer. Assume there are N Dreamers. We would then cluster the NonDreamers into N NonDreamer clusters based on the probability of remaining in school. The match would be a match of Dreamers with NonDreamer clusters.

APPENDIX B: LOGIC MODEL

ASSUMPTIONS => PROCESSES => OUTCOMES

ASSUMPTIONS	PROCESSES			OUTPUTS	OUTCOMES	
	INPUTS (IHAD Intervention)	STRATEGIES/ ACTIVITIES/ METHODS	MEDIATING FACTORS		SHORT-TERM & INTERMEDIATE	LONG-TERM
• "At-Risk" Factors • Resiliency Theory • Truancy	• Mentoring • Tutoring • Sponsors • Project Coordinator • Collaboration with other organization • Setting	oMentoring: -Screening -Training -Support/ supervision -Contact frequency -Relationship duration -Relationship quality -Recruitment o Tutoring	-Improved parental relationships -Coping skills -Global self-worth or self-esteem -No truancy -Positive peer relationships	?	• Academic • Social	-High School graduation -Post-secondary/ vocational matriculation -Post-secondary graduation -Employment

————————————————————————————▶

ASSUMPTIONS	
Resiliency theory framework	Zimmerman and Bingenheimer (2002) "Natural Mentors and Adolescent Resiliency" - "Using the resiliency theory framework, natural mentors were found to have compensatory but no protective effects on problem behaviors, and both compensatory and protective effects on school attitudes. (p.221)" - "Resiliency theory provides a framework for understanding why some youths who are exposed to a risk do not exhibit the problem behavior associated with that risk." "Other factors in youths' lives may counteract the effects of a given risk factor or may protect them from the negative consequences of risks. Two models of resiliency are particularly relevant for research on natural mentors: the compensatory and protective factor models (Garmezy, Masten, & Tellegen, 1984; Zimmerman & Arunkumar, 1994; Zimmerman, Steinman, & Rowe, 1998)." "The compensatrory model of reliliency sugtgests that positive factors in an adolescent's life may counteract or neutralize the effects of risk factors." "The protective factor model suggest that some factors may modify the relationship between risks and outcomes." (p.223)
Truancy	McPartland & Nettles. (1991) p. 582-583 -"School attendance, on which RAISE demonstrated a positive impact, is a behavior that is most open to short-term improvements and that can lead to advances in other school outcomes. Good attendance may be more completely under the control of individual students and more susceptible to positive influences by adult advocates or mentors than other

	school behaviors. In contrast to report card grades and promotion rates for which teachers make the major decisions, every student can have good attendance." "any student with absenteeism problems can improve daily attendance with extra effort and effective support from family and meaningful adults…Thus, attendance rates seem to be a good student outcome on which adult mentor and advocacy programs can focus to be effective in the short run." -"Good school attendance can often be a building block to other student behaviors required for school success. Students' attendance rates are often closely tied to their school report card grades…Course failures due to poor attendance can lead to higher retention rates. Student learning as measured by standardized test scores can also be expected to suffer as a result of poor attendance, because absent students will miss instruction and engage in less drill and practice in the basic skills covered by tests. So a program of assistance by outside adults that focuses on improving student attendance may have a cumulative effect over time on other academic outcomes."
"At-Risk" Factors and Youth	Thompson & Vance (2001) "The Impact of Mentoring…" "Proponents of mentoring programs hypothesize that mentoring programs could be part of the answer to these problems (problems that affect At-risk youth)." (p.227) "At-risk" Behaviors: "those activities in which youth engage that increase the likelihood of adverse psychological, social, and health consequences (Kazdin, 1993)." (p.228) - Resnick and Burt (1996): "Youth can be defined as at risk because they engaged in risky behavior (e.g., early sexual behavior, truancy, tobacco, alcohol/drug use, running away from home/ foster home, associating with delinquent peers). Exposure to certain environments may place a child at risk, and these environments or situations included poverty, dangerous neighborhoods, and family dysfunction (e.g., abusive/neglectful caretakers, out of home placement, and single parent homes)." (p. 228)

PROCESSES

PROCESSES			SOURCE
INPUTS	STRATEGIES/ ACTIVITIES/ METHODS	MEDIATING FACTORS	
MENTOR-ING	-Screening -Training -Support/ supervision -Contact frequency -Length of relationship -Quality of relationship -Recruit-ment	-Improved parental relationships -Coping skills -Global self-worth or self-esteem -No truancy -Positive peer relationships	From "Contemporary Issues in Mentoring" - Slicker and Palmer (1993): "found that **effectively mentored** students had a lower droput rate than ineffectively mentored students.(p.14)" Thompson & Kelly-Vance (2001): - "well established infrastructures that **screen, match, and support** relationships. This infrastructure and ongoing supervision of the adult/youth relationship appear essential to the success of the friendship…" (p.238) - "These components (necessary for a successful mentoring program) include the following: *standards and procedures for screening the volunteers and youth, procedures for the creation of the relationship and ongoing supervision of the relationship, clearly defined expectations of all parties, consistent contact between the youth and the adult, ongoing training for volunteers, and program staff to operate and oversee the program.*" (p.239) Grossman and Rhodes (2002) - "youth who were in matches that lasted more than 12 months reported significant increases in their self-worth, perceived social acceptance, perceived scholastic competence, parental relationship quality, school value, and decreases in both drug and alcohol use." (p.208) Rhodes et al (2000) - "Improved perceptions of **parental relationships**, although not the sole determinant, are important **mediators of change in adolescents' academic outcomes and behaviors**." (p.1667) - "Effects of mentoring are mediated partially through improvements in **adolescents' perceptions of their parental relationships**." (p.1667) - "Whatever the underlying processes, it appears that guidance and support from an adult outside of the home can lead to improvements in the **quality of the parent-child relationship**." (p.1668) - "As predicted, improvements in perceptions of parental relationships led to improvements in the value that adolescents placed on school." (p.1668) - "This shift in values led to less truancy and improved grades." (p.1668) - "Consistent with previous research, improvements in adolescents' global self-worth were associated with improved perceptions of scholastic competence (Harter, 1993). Mentoring

did not directly affect global self-worth but was mediated instead through improved perceptions of parental relationships. It may be the case that mentors' influence on self-appraisals is more domain specific (i.e., academics) and not captured through general indices of self-worth (DuBois, Felner, Brand, & George, 1999)" (p.1668)

From Contemp Issues of Mentoring: Grossman & Johnson (Assessing the Effectiveness of Mentoring Programs):
- (ABSENTEEISM/ TRUANCY): "We also examined the differential effects of participation on youth according to their level of absenteeism before program enrollment. Those with high rates of **absenteeism are presumed to be the least motivated students**, while those with the lowest rates are presumed to be the most motivated. In BBBS (Big Brothers Big Sisters), a high rate of absenteeism was defined as skipping more than one day of school in the year before entering the program; in SAS (Sponsor-A-Scholar), a high rate of absenteeism in the ninth grade was defined as more than 9 percent."(p.31)
- (FAMILY RELATIONSHIP): Only the SAS evaluation examined the level of family support. It was measured by "parent involvement in school-related activities; level of discussion at home on important topics; and encouragement of college attendance."(p.31)

Rhodes et al. (2000)
"Indeed **mentoring relationships that last 12 months or longer** have been found to be associated with significant improvements in adolescents' self worth, whereas those with earlier terminations tend to have mild or even negative effects on these domains (Grossman & Rhodes, in press). As such future models of mentoring processes should incorporate measures of relationship duration." (p.1668)

McPartland & Nettles. (1991) p.582-583
-"**School attendance**, on which RAISE demonstrated a positive impact, is a behavior that is most open to short-term improvements and that can lead to advances in other school outcomes. Good attendance may be more completely under the control of individual students and more susceptible to positive influences by adult advocates or mentors than other school behaviors. In contrast to **report card grades and promotion rates** for which teachers make the major decisions, every student can have good attendance." "any student with absenteeism problems can improve daily attendance with extra effort and effective support from family and meaningful adults...Thus, attendance rates seem to be a good student outcome on which adult mentor and advocacy programs can focus to be effective in the short run."
-"Good school attendance can often be a building block to other student behaviors required for school success. Students' attendance rates are often closely tied to their school report card grades...Course failures due to poor attendance can lead to higher retention rates. Student learning as measured by standardized test scores can also be expected to suffer as a result of poor

attendance, because absent students will miss instruction and engage in less drill and practice in the basic skills covered by tests. So a program of assistance by outside adults that focuses on improving student attendance may have a cumulative effect over time on other academic outcomes."

DuBois and Neville (1997) "Youth Mentoring: Investigation of.."
- "Reports of more extensive amounts of mentor-youth contact and feelings of closeness were, in turn, each associated with ratings of greater benefits for youth." "reports of mentor-youth contact and closeness were each associated positively with perceived benefits for youth...(p.227)" "Perhaps most notably, reports of more extensive amounts of mentor-youth contact and feelings of emotional closeness in relationships were each linked to ratings of greater perceived youth benefits. This finding replicates earlier qualitative research in which Freedman (1988) found that effective mentoring relationships were characterized by continuity and consistency in mentor-youth contacts (i.e. at least once-a week) as well as strong feelings of personal closeness. Accordingly, it seems that one basic requirement for mentoring programs should be the availability of appropriate supports to insure that adult volunteers spend time with youth on a regular basis and in ways that are likely to foster close emotional bonds. Such supports might include training and on-going staff supervision, structured opportunities for mentor-youth interaction (e.g., program-sponsored events), and monitoring proceduresto insure regular patterns of contact between mentors and youths. (p.232-233)"

DuBois et al (2002) "Testing a New Model"
- "Grossman & Rhodes found, for example, that youth perceptions of **higher quality relationships** with mentors in Big Brothers Big Sisters programs were related to **greater duration of relationships**, a factor that predicted more positive gains on several measures of adjustment."(p.25)
- "Previous research has found that when youth receive **social support from family, peers,** an other natural support providers, one of the beneficial outcomes is an increase in their **self-esteem**."(p.27)
- "One important **role of mentors** may be to help youth acquire **effective coping skills**. In the process of helping youth negortiate differeing types of stressors, mentors may model and instruct youth in skills and techniques that they can apply in similar situations. Available findings indicate that social support can indeed facilitate the use of active and problem-focused coping efforts among youth. These coping tendencies are generally predictive of more positive mental health outcomes for youth."(p.27)
- "As the model indicates, we assume that a primary benefit of a significant relationships with mentors is their capacity to enhance youth **perceptions of the overall social support** available to them from extrafamilial adults... such support is posited to promote youth's **psychological and behavioral competence** (such as self-esteem and coping skills)."(p.28)

			- "Notably we found these **nominations** (of significant adults by youth) to be the first link in a chain of associations that revealed indirect positive effects of program participation on levels of emotional and behavioral problems at both assessments."(p.46) - -"for some youth, what may be most important is simply the opportunity for the relationship to progress and grow. For that to happen, however, **relationships must be sustained**."(p.47) - "Mentors feelings of confidence and tendencies toward adaptive persistence thus seem valuable in helping them to establish strong connections with youth, perhaps especially when **relationships need time** to develop."(p.48) - "Results indicate that social support provided by mentors and other significant adults tends to increase positive feelings of self-regard among youth, which can be an important mechanism leading to beneficial outcomes. Bearing out earlier research, this implies that effective mentoring is able to facilitate gains in **self-esteem that enhance academic, emotional, and behavioral adjustment**. The present research indicates that mentoring can enhance both the level and consistency of self-evaluations across multiple domains, including family, school, peers and so on. Support from mentors and other significant adults may contribute to a healthy, well-rounded foundation for overall feelings of self-worth and thus strengthen esteem-mediated pathways to positive outcomes. (p.49-50) Zimmerman and Bingenheimer (2002) "Natural Mentors and Adolescent Resiliency" - "These results suggest that, apart from promoting positive school attitudes and discouraging problem behaviors directly, natural mentors may encourage young people not to befriend peers who engage in problem behaviors or who discourage positive school attitudes. (p.238)"
TUTORING	Volunteer or in collaboration with other organizations/ schools		McPartland & Nettles. (1991) p.583 "We found strong RAISE effects only for English grades, so it is likely that direct support for academic learning by RAISE adult advocates and volunteers was also responsible, through activities such as assistance with completing homework, tutoring, in basic skill areas, or assistance in learning activities such as reading practice." "our evaluation of RAISE suggest that direct academic activities can be a successful early part of programs using outside adult advocates or mentors."
SETTING	School Housing project Community		
SPONSOR (S)	Person, foundation, community,		

	Business, Religious gp.		
PROJECT COORDINA TOR			
COLLAB. WITH ORGANIZA TION	e.g., Americorp		

OUTPUTS

OUTCOMES

OUTCOMES		INDICATOR	SOURCES
SHORT-TERM & INTERMEDIATE	ACADEMIC/ COGNITIVE	-Positive school attitude -Promotion rates -Better grades -standardize testing -School suspensions -(On time promotion.)	Thompson & Kelly-Vance (2001) (p.238): - "The results of this study indicated that having a mentor positively impacted the **academic achievement** of at-risk youth." - The treatment group performed better in **reading and math** than the control group. No differences were found between the groups in spelling." Grossman and Rhodes (2002): - "youth who were in matches that lasted more than 12 months reported significant increases in their **self-worth, perceived social acceptance, perceived scholastic competence, parental relationship quality, school value, and decreases in both drug and alcohol use**."(p.208) Grossman and Tierney (1998) - "We found improvement in **school attendance**. At the end of the study period, Little Brothers and Little Sisters were 30% less likely to skip a day of school at all. (p.415) - "On average, Little Sisters skipped 84% fewer days of school than did control girls."(p.415) - "Research shows that youths who feel more competent in school tend to be more engaged and perform better (Harter 1982). Therefore, we examined changes in Harter's scale of **perceived scholastic competence** (1985) to determine if participating in the program increased student's expectations. At the conclusion of the study period, we found that treatment youths felt more confident of their ability to complete their schoolwork than did control youths. (p.415-416) - "Using the summary measure of the parent-child relationship, we found that Little Brothers and Little Sisters **scored higher** than did control youths." (p.417) From Contemporary Issues in Mentoring: The BBS evaluation (Tierney and Grossman 1995) "Little Brothers and Little Sisters were 46 percent less likely than their control group counterparts to initiate drug use and 27 percent less likely to initiate alcohol use during the study period. They were less likely to hit someone and skipped only half as many days of school as did control youth. These youth felt more competent about their ability to do well in school and received slightly higher grades by the end of the study. And they reported more positive relationships with their friends and with their parents." (p.12) The Career Beginnings (Cave and Quint, 1990) and Sponsor-A-Scholar (Johnson, 1998) "Students participating in SAS improved their **academic performance**: they earned higher GPAs in tenth and eleventh (but not twelfth) grades than did students in the comparison group and were more likely to participate in college preparatory activities. Participants in both programs were more likely to **attend college during the** first year

			after high school graduation than were non-participants. And length of stay in college increased for Career Beginnings students."(p.13) Across Ages (LoSciuto et al., 1996) "The students who had mentors had **better attitudes toward school, toward the future and toward elders** than did youth in the other two groups. These youth also used substances less frequently and had somewhat better school attendance than did youth who did not participate in the program." "Students who were highly involved with their mentors had **better school attendance** than did youth whose mentors were less involved. (p.13) Evaluation of Project RAISE (McPartland and Nettles, 1991) "Program participants (middle school youth) had somewhat better grades and attendance than did youth not involved with the program. Although the evaluation design makes it difficult to isolate the effects of mentoring, the researchers concluded that the results suggest mentoring is an important component of the program." "The evaluation, which used a comparison group strategy, found that participants had **better attitudes toward school their family and their communities. Participants also had better school attendance** than did students in the comparison group."(p.13) Brown (1996) cites Slicker and Palmer (1993) "Those students who were effectively mentored experienced **higher academic achievement** relative to control group youth, but ineffectively mentored students showed a decline in academic achievement. What this research does not indicate, however, is that any mentoring relationship or progrogram will produce these results. (p,14) McPartland & Nettles. (1991) (p.583): - "We found strong RAISE effects only for English grades, so it is likely that direct support for academic learning by RAISE adult advocates and volunteers was also responsible, through activities such as assistance with completing homework, tutoring, in basic skill areas, or assistance in learning activities such as reading practice." "our evaluation of RAISE suggest that direct academic activities can be a successful early part of programs using outside adult advocates or mentors." Zimmerman and Bingenheimer (2002) "Natural Mentors and Adolescent Resiliency" - "...we found that having a natural mentor both offset (compensatory factor model) and modified (protective factor model) the effects of the risk factors. Youth with natural mentors had more positive school attitudes than did those without natural mentors. In addition, they were also less severely affected by the negative school attitudes or behaviors of their peers. (p.237-8)" - "Our path analysis results suggest that natural mentors may not only have direct effects on reducing problem behaviors and increasing positive school attitudes, but may also have indirect effects by helping adolescents avoid peers who provide negative

			influences.(p.238)" - "These results suggest that, apart from promoting positive school attitudes and discouraging problem behaviors directly, natural mentors may encourage young people not to befriend peers who engage in problem behaviors or who discourage positive school attitudes. (p.238)"
	SOCIAL/ BEHAVIOR-AL	-Reduced substance abuse -Improved social attitudes and relationships -Reduced delinquency/ not a juvenile offender. -Prevention of early parenting.	-Grossman and Rhodes (2002) "youth who were in matches that lasted more than 12 months reported significant increases in their **self-worth, perceived social acceptance, perceived scholastic competence, parental relationship quality, school value, and decreases in both drug and alcohol use**."(p.208) From Contemporary Issues in Mentoring: The BBS evaluation (Tierney and Grossman 1995) "They (Little Brothers and Little Sisters) were **less likely to hit someone and skipped only half as many days of school** as did control youth. These youth felt **more competent about their ability to do well in school** and received slightly higher grades by the end of the study. And they reported more positive relationships with their friends and with their parents." (p.12) Across Ages (LoSciuto et al., 1996) "The students who had mentors had better attitudes toward school, toward the future and toward elders than did youth in the other two groups. These youth also used substances less frequently and had somewhat better school attendance than did youth who did not participate in the program." (p.13) Evaluation of Project RAISE (McPartland and Nettles, 1991) "The evaluation, which used a comparison group strategy, found that participants had better attitudes toward school their family and their communities."(p.13) Grossman and Rhodes (2002) "youth who were in matches that lasted more than 12 months reported significant increases in their self-worth, perceived social acceptance, perceived scholastic competence, parental relationship quality, school value, and decreases in both drug and alcohol use."(p.208) Grossman and Tierney (1998) - "On average, the number of times Little Brothers and Little Sisters reported hitting others during the previous 12 months was 32% less than that of the control youths."(p.413) - "We also examined the number of times youths said that they lied to their parent. At the conclusion of the study period, Little Brothers and Little Sisters reported lying to their parent 37% less than did control group youths."(p.417) Zimmerman and Bingenheimer (2002) "Natural Mentors and Adolescent Resiliency" - "Respondents with natural mentors reported lower levels of problem behavior, including

			marijuana use and nonviolent delinquency, than did those without mentors. This was true even after we adjusted for demographic variables and known risk factors such as problem behavior norms and friends' problem behaviors. (p.237)" - "...we found that having a natural mentor both offset (compensatory factor model) and modified (protective factor model) the effects of the risk factors. Youth with natural mentors had more positive school attitudes than did those without natural mentors. In addition, they were also less severely affected by the negative school attitudes or behaviors of their peers. (p.237-8)" - "Our path analysis results suggest that natural mentors may not only have direct effects on reducing problem behaviors and increasing positive school attitudes, but may also have indirect effects by helping adolescents avoid peers who provide negative influences.(p.238)" - "These results suggest that, apart from promoting positive school attitudes and discouraging problem behaviors directly, natural mentors may encourage young people not to befriend peers who engage in problem behaviors or who discourage positive school attitudes. (p.238)"
LONG-TERM	ACADEMIC/ COGNITIVE	-Graduate from High School -Post-secondary / vocational school matriculation -Post-secondary graduation	DuBois et al (2002): - "Students in the Sponsor-A-Scholar Program who viewed their mentoring relationships more positively were found to perform significantly better in terms of high school grades and first-year college attendance." Grossman & Rhodes (2002) "The Test of Time"(p.25) From Contemp. Issues in Mentoring: The Career Beginnings (Cave and Quint, 1990) and Sponsor-A-Scholar (Johnson, 1998) - "Students participating in SAS improved their **academic performance**: they earned higher GPAs in tenth and eleventh (but not twelfth) grades than did students in the comparison group and were more likely to participate in college preparatory activities. Participants in both programs were more likely to **attend college during the** first year after high school graduation than were non-participants. And length of stay in college increased for Career Beginnings students."(p.13)
	SOCIAL/ BEHAVIOR-AL	-Employ-ment -Welfare	from DuBois et al (2002) - "students participating in the Adopt-A-Student Program, those who reported high levels of support from their mentors were significantly more likely to be employed at follow-up than were those who reported low levels of support." Johnson A. W. (1997) "Mentoring At-Risk Youth"(p.25)