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A Re-Analysis of the Impacts of the California GAIN Program

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ABSTRACT

As part of recent reforms of the welfare programs in the U.S., many states and localities have refocused their Welfare-to-Work programs from an emphasis on human capital acquisition (i.e., providing basic education and vocational training) to an emphasis on "work-first," (i.e., moving welfare recipients into unsubsidized employment as quickly as possible.) This change in emphasis has been motivated, in part, by results from the experimental evaluation, conducted by the Manpower Demonstration Research Corporation (MDRC), of California's Greater Avenues to Independence (GAIN) programs during the early 1990s. Their evaluation found that, compared to programs in other counties that emphasized skill accumulation, the work-first program in Riverside County had larger effects on employment, earnings, and welfare receipt. In addition, the Riverside program was cheaper per recipient than the other programs.

This paper reexamines the GAIN programs from two complementary perspectives. First, we extend the earlier analysis through nine years post-randomization, which is the longest follow-up of any randomized training program, and find that the stronger impacts for Riverside County's work first program tend to shrink, whereas the weaker impacts for the human capital programs in Alameda and Los Angeles Counties tend to remain constant or even grow over time. Second, we develop and implement methods to allow the comparison of programs implemented by random assignment in different places despite striking differences in the composition of the participant populations. On a substantive level, our reexamination of the GAIN experiment lead us to conclude that although the work first programs were more successful than the human capital accumulation programs in the early years, this relative advantage disappears in later years. On a methodological level, our results suggest that—at least in this welfare context—these methods are a promising approach both for the estimation of program effects from non-experimental data and for extrapolating program results from one location to a different location with a different population mix.

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1. Introduction

The passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) in 1996 provided the most radical reform of the U.S. cash assistance, welfare system in the last 60 years. In particular, the legislation directs states to reorient their welfare programs toward encouraging earnings, and not cash assistance, as the means for disadvantaged parents to provide for their children. To encourage this "work rather than welfare" objective, families under PRWORA are limited to 5 years of federally-funded cash-aid, states are obliged to require adult family members to engage in some type of work after two years of aid, and the full funding of the Temporary Assistance for Needy Families (TANF) block grant is subject to the states' meeting stringent work participation requirements for adults in assistance units. This combination of time limits and participation requirements have placed increasing pressure on states to devise strategies and programs that get low-income households out of welfare and into jobs.

The focus on work in this most recent effort to reform the U.S. welfare system is not new. Beginning in the 1960s, the federal government—through the Work Incentives (WIN) program and its successor program, the Job Opportunity and Basic Skills (JOBS) program begun in 1988—has supported employment and training programs in an effort to increase the employment rates and skill levels of welfare recipients. What has changed in the most recent welfare legislation is the emphasis on getting recipients into jobs quickly rather than allowing for the more deliberate acquisition of basic and vocational skills in state "welfare-to-work" programs. To achieve the federal government's objective, states have increasingly relied on "work-first," "quick-job-entry," or "labor-force-attachment" (LFA) strategies that aim to quickly move welfare recipients into unsubsidized employment through the provision of job search training and assistance. Such programs are in contrast to programs that emphasize "human-capital development" (HCD), through more expensive and longer duration basic skills and vocational training programs.[1]

Although the movement of states, as well as the federal government, away from basic skills and vocational training programs and towards programs that encourage quick entry into jobs has been motivated by several factors, one important impetus has come from the findings of several recent experimental evaluations of various state welfare-to-work programs. One of the earliest and most influential of these studies is the evaluation of California's Greater Avenues to Independence (GAIN) program conducted by the Manpower Demonstration Research Corporation (MDRC) in the late 1980s and early 1990s. In this evaluation, welfare recipients in six California counties were randomly assigned to either a "treatment" group that was to receive services in a county based and designed "welfare-to-work" program, or to a "control" group to which these services were denied. Counties were given considerable discretion in the types of recipients they selected, as well as in the way they designed their programs. Thus, in effect, the MDRC study was an evaluation of six separate programs, each with its own distinct population and each with a within-site random-assignment design.

The largest effects on participants were found for Riverside County's GAIN program. Among female heads on AFDC at the time of their enrollment in GAIN, Riverside's program boosted annual employment rates and earnings by 39% and 63%, respectively, over the first three years after randomization compared to those of non-participants and reduced annual AFDC/TANF participation rates by 8%.[2] In contrast to the programs run in the other analysis

[1] See Friedlander, Greenberg, and Robins (1995) and LaLonde (1997) for more on government-sponsored training programs in the U.S.

[2] See estimates for the AFDC-FG (female-headed) in Table 5 below.

counties that emphasized human capital acquisition (usually involving longer periods of basic education and training), Riverside emphasized a tightly focused job search program (known as "Job Club") and maintained a consistent message "that employment is central and should be sought expeditiously and that opportunities to obtain low-paying jobs should not be turned down."[3]

The "work-first" approach of Riverside, which received national (and international) acclaim for its success,[4] has become the standard-bearer and model for welfare-to-work programs not only for California but also for the rest of the nation.[5] The emphasis in the work-first approach on placing people into jobs quickly, even if at low initial wages, reflects a view that the workplace is where welfare recipients can best acquire their work habits and skills. However, there are several reasons why the findings from MDRC's evaluation of Riverside's GAIN program do not necessarily imply that the work-first strategy is more effective than the human-capital strategy for increasing the self-sufficiency and reducing the welfare dependence of recipients.

The first issue concerns the timeframe over which the effects of these types of programs are typically measured. As noted above, the MDRC has published estimates of GAIN impacts only for the first three years after random assignment to treatment. As a general matter, extrapolating from short run estimates of the impacts of social programs to what will happen in the longer run can be misleading, as Couch (1992) and Friedlander and Burtless (1995) have noted. More importantly for the case at hand, reliance on short-run estimates of program effects will

[3] Hogan (1995).

[4] For example, the Riverside GAIN program was awarded the Harvard Kennedy School of Government's "Innovations in American Government Award" in 1996.

[5] Based on these evaluations of the GAIN programs, the Wilson administration in California pushed to have all of the state's counties adopt the Riverside work-first approach in its GAIN programs, culminating in the 1995 GAIN reforms (AB 1371).

tend to understate effectiveness of human capital development programs relative to those that emphasize early labor force entry and attachment, simply because the human-capital development programs are more time-intensive treatments and typically take longer to complete relative to work-first programs. Work-first programs typically take only a few weeks for participants to complete and such programs concentrate on getting workers into jobs as soon as possible. As such, there is a strong presumption that, relative to long-term results, short-term evaluations will favor work-first programs over human-capital development ones.[6] Estimates of program effects over a longer post-enrollment period are needed before one can accurately assess relative long-run benefits of these alternative welfare-to-work strategies.

The second reason for caution in inferring the relative effectiveness of alternative training strategies relates to the design of the GAIN evaluation. We want to compare the outcomes of two different treatments, namely, HCD and LFA. To do so, ideally, the experiment would have randomly assigned individuals in the same location to the two programs or to a control group. Instead, AFDC assistance units *within* a county were randomly assigned either to receive the services of the county's particular implementation of GAIN or to be denied these services. As MDRC made clear in its reports on this evaluation, this experimental design does not allow one to draw inferences about the *differential* impact of alternative programs—e.g., work-first versus human-capital—that vary *between* counties with the same level of rigor that apply to the within-county *gross* impacts of a county-specific program relative to no program. This is because program effects may he heterogeneous across individuals and programs in different counties may have selected different mixes of participants. Alternatively, program effects may vary with eco-

[6] A similar point is made by Mincer (1974) in his model of schooling decisions. Therein, Mincer notes that at early ages the earnings of individuals who choose additional schooling will be lower than those who choose to go to work at early ages, simply because attending school inhibits going to work, even if all alternative activities yield the same present value of lifetime earnings.

nomic conditions and these conditions may vary across the counties. Moreover, even randomization over the three types of treatments considered in the MDRC GAIN Evaluation—namely HCD, LFA and no services—would only allow one to assess the efficacy of a program within a particular site. The ability to assess the policy question of interest—whether future implementations of welfare-to-work programs should follow a work-first or human capital development strategy—will still depend on the credibility of extrapolating results from site to other.

In this paper we address both of these concerns. To address whether estimates of the GAIN impacts based on only three years of data are indicative of the longer run gains from GAIN programs, we present estimates of the impacts using nine years of post-enrollment data on the employment, earnings and welfare participation of the members of the experimental and control groups in four urban counties used in the MDRC evaluation of the California GAIN program (Alameda, Los Angeles, Riverside and San Diego counties).[7] Our estimates of longer-run impacts exploit the within-county random assignment design of the original MDRC evaluation and, therefore, maintain the credibility ascribed to such a design for the gross impacts of each of GAIN programs implemented in these counties for the populations they served.

To address the second issue—generalizing the findings from one county to another county and comparing the effects of HCD and LFA strategies—the paper also provides estimates of the *differential* impacts of the work-first strategy implemented in Riverside county relative to the more human-capital oriented programs used in the other three counties at the time of random assignment in the original MDRC evaluation. In principle, the ideal way of estimating such differential impacts within a county would be to randomly assign subjects from each of the counties to one of three arms: (a) work-first, (b) human-capital treatments, or (c) a control group that re-

[7] We omit the two rural counties included in the original MDRC evaluation, (Butte and Tulare), because these rural economies are quite different from the four urban counties.

ceives neither bundle of services. Currently, data for such an experiment does not exist, at least not one with data on long-term post-enrollment outcomes.[8] Furthermore, implementing such a design is difficult in practice, especially when a particular treatment entails sending and maintaining an overarching orientation, or "message." For example, it has been strongly suggested that an essential feature of the Riverside GAIN program was the pervasiveness of their "message" that all program participants, even those with skill deficits, should strive to get a job quickly, regardless of its compensation (e.g. Corbett, 1994/95). Evaluation designs with multiple treatments, in which one or more treatments attempt to maintain such programmatic "messages" or "orientations" would appear to be nearly impossible to implement within a single bureaucracy or agency or on a sufficient scale.

To estimate the differential impacts of alternative programs, we instead make use of statistical matching and regression adjustments based on the personal characteristics, past welfare, and earnings histories of welfare recipients in the four counties in an attempt to adjust for across-county differences in GAIN participants. While one cannot claim, *a priori*, that such adjustments eliminate these across-county differences in the participant populations, we exploit the availability of data on control groups denied all GAIN services in each of the counties to assess the quality and credibility of these adjustments. As a result of the random assignment of welfare recipients eligible for GAIN services in each county, the control groups reflect, on average, each county's participant pool in the absence of receiving the GAIN treatments available in each of the counties. Thus, we perform statistical tests of whether our adjustments eliminate across-county differences, on average, in county post-enrollment outcomes for controls.[9] If our regres-

[8] We note that MDRC is conducting such an evaluation in three counties in the U.S. in their National Evaluation of Welfare-to-Work Strategies. To date, results for two years of post-enrollment outcomes have been released. See Hamilton, et al. (1997).

[9] Dehejia (2000) discusses the related question whether data from different sites can be pooled conditional on co-

sion adjustments are sufficient, there should be no remaining differences between the outcomes of control group members, who received no GAIN services, across the counties. We note that the strategy of adjustment, including matching, and validating these methods by using data for experimentally-generated control groups is similar to the strategies used by Lalonde (1986), Heckman and Hotz (1989), Heckman, Ichimura, and Todd (1997, 1998a, 1998b), Dehejia and Wahba (1999), and Hotz, Imbens and Mortimer (1999).

We then use the estimated models to address the question of differential impacts of alternative programs. The models estimate the effect of treatment for a vector of observed characteristics. We use the predictions of the model to estimate the effect of applying the treatment in Riverside County to the treated population in the other three counties. Given the 9-year post-enrollment data on outcomes that we have, we can assess both the short- and longer-run differential impacts of these two types of treatment strategies for the various county-specific treatment populations. These predicted effects for two programs, *for a given population*, can then be compared to assess the differential effects of types of services (treatments) used in the respective programs. We show that these matching estimates differ—sometimes substantially—from the simple comparison of the net treatment effects in each county, suggesting that effect heterogeneity is substantively important.

The remainder of the paper is organized as follows. In Section 2, we provide a brief description of California's GAIN program and the original MDRC evaluation. We begin by showing that the populations selected (i.e., who were subject to randomization) were very different across the counties; Riverside, and to a lesser extent, San Diego, chose to enroll in their GAIN programs nearly all cases, the other counties choosing to enroll in their GAIN program only long

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variates.

term welfare recipients who are potentially the most difficult to serve. Thus, if there is treatment heterogeneity, it is likely to affect our comparison of program effects across counties. In Section 3, we present within-county experimental estimates of the gross impacts of the county-specific GAIN programs on the employment, earnings, and welfare participation during the 9-year post-enrollment period. In Section 4, we provide a more detailed discussion of our strategy for estimating the differential impacts of the Riverside GAIN program relative to the GAIN programs used in Alameda, Los Angeles, San Diego counties and present estimates for the same outcomes and follow-up period as analyzed in Section 3. Finally, we offer some conclusions about the implications of our findings in Section 5.

2. The GAIN Program, the MDRC Evaluation and GAIN Evaluation Counties

In this section we provide a brief description of the structure of the GAIN program and how it was implemented in the four urban counties we consider in this paper. We also describe the structure of the MDRC GAIN Evaluation.

The GAIN program began in California in 1986 and, in 1989, became the state's official Job Opportunities and Basic Skills Training (JOBS) Program, authorized by the Family Support Act, the nation's attempt to reform the welfare system prior to PRWORA. The GAIN program represented a compromise between two groups in the State's legislature with different visions of how to reform the welfare system. One group favored the "work-first" approach, i.e., use of a relatively short-term program of mandatory job search, followed by unpaid work experience for participants who did not find jobs. The other group favored the "human capital" approach, i.e., a program providing a broader range of services designed to develop the skills of welfare recipients.

In crafting the GAIN legislation, these two groups compromised on a program that contained work-first as well as basic skills and education components in what became known as the GAIN Program Model.[10] The GAIN model consisted of the following sequence of steps. At the time of initial (or continuing) determination of eligibility for welfare, county staff also determined whether the head of a welfare household was subject to GAIN[11] and, if so, registered her (usually, although sometimes him) for GAIN. (Staff also offered to register adults on welfare who were exempted but wished to volunteer for the program.) A county's GAIN registrant was required to attend an orientation meeting to learn about the county's particular GAIN program and their obligations under this program. Each registrant was administered a screening test to measure a registrant's basic reading and math skills. (The same test was used in each of the counties.) Based on their score on this test and whether she had a high school diploma or a GED, she was sent to one of two service tracks. A registrants with a low test score and who did not have a high school diploma or GED was deemed "in need of basic education" and was to be routed through a sequence of services that included access to Adult Basic Education (ABE) or English as a Second Language (ESL) programs. Those not judged to be in need of basic education were to bypass these basic education services. A registrant, in either group, was then to be channeled into job search activities in an attempt to get her employed. If the registrant did not find a job, they were to be provided access to vocational, on-the-job training and work experience activities, in an attempt to enhance a registrant's human capital and, thus, to improve their chances of securing a job.

While the GAIN legislation set out a clear set of goals for the program and the above model for the delivery of services, it also gave California's 58 counties substantial discretion and

[10] See Riccio and Friedlander (1992) for a more complete description of this model.

[11] Heads of households on welfare were mandated to register for GAIN, except for female heads with children under the age of 6. See Riccio, *et al*. (1989) for a more complete description of the criteria for mandated participation

flexibility in designing their programs. In particular, counties had discretion over the types of welfare recipients they registered for their GAIN programs and the relative weight they placed on quick labor market entry versus skill development.[12] County GAIN programs differed along both of these dimensions. With respect to the types of welfare recipients registered for GAIN, some counties, like Riverside and San Diego, operated "universal" programs in that all welfare applicants and recipients were registered for GAIN, while others, like Alameda and Los Angeles, registered mostly long-term welfare recipients, who presumably were more difficult to serve.

MDRC conducted a randomized evaluation of the impacts and cost-effectiveness of the GAIN program in six research counties (Alameda, Butte, Los Angeles, Riverside, San Diego, and Tulare). From the latter part of 1988 to the middle of 1990, each county chose whom to register for GAIN. MDRC then randomly assigned some of these registrants to an experimental group, which was eligible to receive GAIN services and subject to its participation mandates, and the remainder of these registrants to a control group, whose members were not eligible for GAIN services or mandates but could seek (on their own initiative) alternative services in their communities. Note that because the counties followed different practices with respect to choosing registrants, both the experimental and control populations will vary across the research counties. The controls were embargoed from any GAIN services from the date of their random assignment until June 30, 1993.[13] MDRC collected data on both experimental and control group members in each of the research counties, including some background and demographic characteristics and on a set of outcomes after random assignment. (Most of these data were obtained from state and county administrative data systems.) They also monitored the operations of the programs in each

[12] See Riccio and Friedlander (1992), chapter 1.

[13] For a period of two years following the lifting of this embargo, control group members were not required to participate in GAIN but could if they asked to enroll.

of the six research counties. MDRC issued a series of reports on program operation[14] and on the impacts of GAIN programs in these counties over the three- to five-year post random assignment period.[15]

Descriptive statistics and sample sizes for the participants in the MDRC evaluation in the four analysis counties (Alameda, Los Angeles, Riverside and San Diego) are provided in Table 1. Separate statistics are provided for GAIN registrants who were members of female-headed households on AFDC at the time of random assignment (AFDC-FG cases) and two-parent households on AFDC (AFDC-U cases). The statistics in Table 1 will be discussed below. We note that our samples for three of the four counties (all but Los Angeles) are slightly smaller than the original samples used by MDRC due to our inability to find records for some sample members in California's Unemployment Insurance Base Wage system[16] or because we were missing information on the educational attainment of the sample member. The number of cases lost in these three counties is very small, never larger than 1.8% of the total sample,[17] and does not appear to differ by experimental status. Finally, note that in most of the counties and especially for AFDC-FG cases, a much larger fraction of cases were assigned to the experimental group than to the control group.

As noted above, there are differences in what types of AFDC cases were registered for GAIN. For the four counties we analyze, the programs in Riverside and San Diego counties sought to register all welfare cases in GAIN while the programs in Alameda and Los Angeles

[14] See Wallace and Long (1987), Riccio, *et al*. (1989) and Riccio and Friedlander (1992).

[15] See Riccio and Friedlander (1992), Riccio, *et al*. (1994) and Freedman, *et al*. (1996).

[16] The California Economic Development Department (EDD) administers the State's UI system.

[17] The losses from the original MDRC samples were as follows: Alameda: 0.6% for AFDC-FGs and 6.0% for AFDC-Us; Los Angeles: 0.02% for AFDC-FGs and 0.0% for AFDC-Us; Riverside: 1.1% for AFDC-FGs and 1.8% for AFDC-Us; San Diego: 1.1% for AFDC-FGs and 1.0% for AFDC-Us.

counties focused on long-term welfare recipients.[18] The consequences of these differences in selection criteria can be seen in Table 1. Panel A of Table 1 displays the characteristics of the AFDC-FG cases, i.e., households with a single adult (usually a female) on welfare, while Panel B provides comparable information for AFDC-U cases, i.e., households with a married couple. In Alameda and Los Angeles, over 95% of the cases had been on welfare a year prior to random assignment; in San Diego and Riverside, fewer (for some cells much fewer) than 65 percent had been.

These differences in selection criteria also contributed to substantial differences in the employment histories and individual characteristics of the registrant populations across these four counties. As shown in the two Panels of Table 1, the registrants in Alameda and Los Angeles counties had, on average, much lower levels of earnings prior to random assignment relative to those in Riverside and San Diego. These differences in past income across counties are most dramatic for AFDC-U cases, where the average past earnings of GAIN registrants in Riverside and San Diego counties were between 5 and 7 times greater than those for Alameda and Los Angeles registrants. Furthermore, the registrants in Alameda and Los Angeles were, on average, older, had lower levels of educational attainment, and were more likely to be assessed as "in need of basic education" when they entered the GAIN program than the average registrants in Riverside and San Diego. The differences in characteristics of GAIN registrants displayed in the two panels of Table 1 also suggest the possibility of differences in the overall low-income and welfare-prone populations that reside in each of these counties.

The across-county differences in earnings, labor force and welfare participation rates for GAIN registrants also may have resulted from differences in the labor market conditions pre-

[18] For example, Alameda County, which began its GAIN program in the third quarter of 1989, began by registering cases that had been receiving AFDC since 1989, subsequently registering more recent recipients. The GAIN pro-

vailing at the time of random assignment. In Table 2, we present data on the labor market conditions in the four counties for the years during which GAIN registrants were enrolled into the MDRC GAIN Evaluation. While it is unclear the extent to which these differences in labor market conditions can account for the across-county differences in earnings, employment and welfare participation of GAIN registrants noted in Table 1, the diversity in the labor markets of these counties is quite apparent. Table 2 reveals noticeable differences in the structure and state of the labor markets in these counties around the enrollment period. For example, both Alameda and Los Angeles Counties had higher shares of employment in the manufacturing sector than did either Riverside or San Diego Counties. Around the time of the enrollment into the GAIN Evaluation, employment in Riverside County was growing at a much higher rate (7.4 to 8.5%) than was the case in any of the other three counties.

There also were marked differences in the policies and practices that were followed in these counties. Senior leadership in the four counties had very different views about what services to provide to welfare recipients and the likely cost-effectiveness of those services. The GAIN legislation provided the counties with enough discretion to allow them to allocate resources consistent with those views thus leading to very different programs. In particular, counties differed in the emphasis placed on work-first versus human capital and skill development in their GAIN programs. Riverside's program stood apart from other counties in degree to which staff emphasized moving registrants into the labor market quickly. This difference in Riverside's work-first orientation is reflected in the distribution of program activities over the first three years of GAIN's operation (see Table 3). (The shaded quarters in this—and the next—table show the quarters in which the random assignment of registrants into the MDRC experimental evalua-

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gram in Los Angeles County initially only registered those cases that had been on welfare for 3 consecutive years.

tion were conducted for each of the four counties.)

The activities in the table are organized into groups, one representing job search-related activities, another consisting of basic skills and educational activities, and a third including activities which provided registrants with direct work experience. Clearly, Riverside disproportionately channeled its registrants into job search activities relative to basic skills activities. (None of the county programs made extensive use of work-experience activities in the early stages of their operation.) Riverside's emphasis on job search activities stands in contrast to the other three counties, especially Alameda and Los Angeles, where registrants were much more likely to be in basic skills activities in any given month.

The data in Table 3 are consistent with other indicators of Riverside's emphasis on getting GAIN registrants quickly into jobs. For example, Riverside staff required that their registrants that were enrolled in basic skills programs continue to participate in Job Club and other job search activities. In a survey of program staff conducted by MDRC at the time of its evaluation, 95% of case managers in Riverside rated getting registrants into jobs quickly as their highest goal while fewer than 20% of managers in the other research counties gave a similar response.[19] In the same survey, 69% of Riverside case managers indicated that they would advise a welfare mother offered a low-paying job to take it rather than wait for a better opportunity, while only 23% of their counterparts in Alameda county indicated they would give this advice.

Overall, MDRC concluded, "What is perhaps most distinctive about Riverside's program, though, is not that its registrants participated somewhat less in education and training, but that the staff's emphasis on jobs *pervaded* their interactions with registrants throughout the program" (Riccio and Friedlander, 1992, p. 58). Riverside County's GAIN staff were instructed to com-

[19] See Table 3.1 in Riccio and Friedlander (1992) for the results of this survey.

municate a strong “message” to all registrants, including those in education and training activities, that gaining employment was central, that it should be sought expeditiously, and that jobs should not be turned down even if they were low-paying. In contrast, program staff in the other research counties placed less emphasis on getting registrants into a job quickly. For example, Alameda’s GAIN managers and staff “believed strongly in ‘human capital’ development and, within the overall constraints imposed by the GAIN model’s service sequences, its staff encouraged registrants to be selective about the jobs they accepted and to take advantages of GAIN’s education and training to prepare for higher-paying jobs.”[20]

A final indicator of the differences in the way Riverside’s GAIN program operated, relative to the programs in the other counties, can be seen in Table 4. This table displays the average monthly GAIN enrollments, by county, as a percentage of each county’s AFDC caseload. Compared to the programs in Alameda and Los Angeles, Riverside consistently provided GAIN services to more of its caseload. This pattern for these three counties is consistent with the fact that the latter two counties enrolled more of their registrants in basic skills and education programs compared to those focused on job search. The former programs are, on average, much more expensive, on a per case basis, compared to the latter activities. We note from Table 4 that San Diego actually enrolled an even higher proportion of its AFDC caseload in GAIN activities than any other county, including Riverside. This reflected the fact that San Diego officially “enrolled” a large number (and percentage) of its AFDC participants in GAIN even though most of these registrants did not participate in any activities. Rather, they remained in a queue, waiting until slots in services, provided by an outside contractor, became available.

All of the evidence provided above clearly suggests that the prevailing “treatment” in

[20] See Riccio, *et al.* (1994), p. xxv.

Riverside County's GAIN program—both in terms of way it distributed its registrants across activities and in the pervasive message it provided to them—was one that had a "work-first" orientation, while the other county programs we consider in this paper, especially the Alameda and Los Angeles programs, disproportionately provided their registrants with a human capital, skill development oriented treatment.

3. Estimating County-Specific Effects of GAIN Programs

In this section we discuss the experimental estimates of the impacts of GAIN services and mandates to which GAIN registrants were subject during the early 1990s on their employment, earnings and welfare participation for up to nine years after random assignment. While well known, we briefly characterize the properties of experimental estimators of such impacts in anticipation of our discussion of the estimation of the differential effects of work-first versus human capital development treatments in Section 4. We then present the estimates of the short- and long-run impacts for each of the four counties using the MDRC GAIN Evaluation samples.

3.1 Identifying Within-County Treatment Effects

To help fix ideas, we define the following notation. Let D denote an indicator of the county (and its GAIN program), where $d \in \{A,L,R,S\}$ for the four counties in our study. Random samples, of size, N_d, are drawn from the GAIN program registrants in each county d and let i denote a household in these samples. Let t denote the period (year) after a household has been randomly assigned. Let T denote the treatment indicator for the treatments under the MDRC GAIN evaluation, where $T \in \{0, w, h\}$, where 0 denotes no GAIN services, w denotes the work-first oriented treatment and h denotes the human-capital development treatment. Let $Y_{it}(k)$ denote the potential outcomes for household i in t periods after their (random) assignment to treatment k, so that $Y_{it}(0)$ is the potential outcome associated for the no-treatment case, $Y_{it}(w)$ is the potential outcome associated with the work-first treatment and $Y_{it}(h)$ is the potential outcome for the human-capital development treatment as of period t. Finally, let X_i denote a vector of background characteristics and pre-treatment variables for household i.

Access to experimental data allows us to estimate the *average gross treatment effect* (AGTE) of treatment k on those treated, which is defined to be

$$\alpha_t(k) \equiv E\left(Y_{it}(k) - Y_{it}(0) \middle| T_i = k\right) = E\left(\Delta_{it}(k) \middle| T_i = k\right), \tag{1}$$

where $\Delta_{it}(k)$ ($\equiv Y_{it}(0) - Y_{it}(k)$) is household i's gain as of period t from treatment k relative to receiving no GAIN services. The *conditional* (on X) version of this treatment effect is given by,

$$\alpha_t(k|x) \equiv E(\Delta_{it}(k))|T_i = k, X_i = x), \tag{2}$$

for all $k \in \{w,h\}$. As noted above, the design of the MDRC GAIN Evaluation was to randomly assign GAIN registrants in each County d to either the prevailing treatment in that county, $T_i = k_d$ or to a control group that received no services, $T_i = 0$. This design implies that

$$Y_{it}(T) \perp T_i | D_i = d, \tag{C-1}$$

where $z \perp y$ denotes that z is (statistically) independent of y, where in (C-1) the independence is conditional on the county of residence ($D_i = d$). Note that (C-1) implies that

$$E\left(Y_{it}(0) \middle| T_i = k, D_i = d\right) = E\left(Y_{it}(0) \middle| T_i = 0, D_i = d\right), \tag{C-1$'$}$$

i.e., the mean value of $Y(0)$ for those who receive treatment k in County d is equal to the mean value of the outcomes as of period t for control group members in that same county. Note that the latter is typically unobserved by the econometrician while the latter is observable with data from an experiment with random assignment of treatments. MDRC's evaluation design guarantees that treatment received by individual i is independent of that unit's potential outcomes, for all i *within* a particular County d. Given (C-1) (or (C-1′)), the difference between average outcomes for treatment group members (receiving k_d) in County d and average outcomes for the control group, also in County d and receiving treatment 0, identifies the AGTE for that county's registrant population. That is

$$\alpha_{td}(k_d) = E(Y_{it}(k_d) | T_i = k, D_i = d) - E(Y_{it}(0) | T_i = 0, D_i = d)\,, \tag{3}$$

where $\alpha_{td}(k_d)$ denotes the AGTE for County d, for all $d \in \{A,L,R,S\}$.

As discussed in Section 2, GAIN registrants were randomly assigned to either receive some subsequently-determined set of GAIN services or no services at all, where the services the "experimental" subjects received were based on the GAIN model and the types of services a particular county emphasized. Recall that Riverside's GAIN program emphasized work-first treatments ($T = w$), the GAIN programs in Alameda and Los Angeles Counties emphasized human-capital development treatments ($T = h$), while San Diego's GAIN program was a mixture of these two types of programs. Moreover, we do not have data on precisely what set of services each GAIN experimental subject actually received. Therefore, in the next section we present estimates of the gross effects of receiving any set of services versus no services for each of the counties instead of estimates of specific sets of treatments. In this sense, we are approximating the AGTE for the "prevailing" treatment that was used in a particular county in place of a more-specific set (or sequence) of services actually received. That is, we use differences in the sample means of outcomes between experimentals ($\bar{Y}_t(k_d)$) and controls ($\bar{Y}_t(0)$) to estimate (3) for each county.

3.2 Long-Run Estimates of County-Specific GAIN Impacts

Estimates for AFDC-FG cases are presented in Table 5 and the corresponding estimates for AFDC-U cases are presented in Table 6. We provide estimates for six different outcomes: (1) ever employed during year; (2) number of quarters worked per year; (3) annual labor market earnings; (4) whether a GAIN registrant's earnings exceeded the income of a full-time worker earning the minimum wage; (5) whether the registrant received AFDC/TANF benefits during the year; and (6) the number of quarters in the calendar year that she received AFDC/TANF benefits.[21,22] For each of these outcomes we have nine years of post-random assignment data. We average them over three-year periods to reduce the number of entries in tables that follow. MDRC has published estimates of similar outcomes for 3 years post random assignment and 5-year post-random assignment results were released in a working paper.[23] (The estimates presented in Tables 4 and 5 for the first five post random assignment years do not correspond exactly to the estimates produced by MDRC, due to relatively minor differences in samples used (see discussion above) and, more importantly, the use of a different "dating" convention in forming post-random assignment years.[24])

Our access to data on outcomes for six additional years after random assignment allows us to assess the longer-term consequences of being exposed to the GAIN programs in the four

[21] The employment and earnings outcomes were constructed with data from the State's UI Base Wage files provided by the California Employment Development Department (EDD). These data contain quarterly reports from employers on whether individuals were employed in a UI-covered job and their wage earnings for that job. These quarterly data were organized into four-quarter "years" from the quarter of enrollment in the MDRC GAIN evaluation. The "Ever Employed in Year" outcome was defined to be = 1 if the individual had positive earnings in at least one quarter during that year and = 0 otherwise. The "Annual Earnings" outcome was the sum of the four-quarter UI-covered earnings recorded for an individual in the Base Wage file. All income variables were converted to 1999 dollars using cost-of-living deflators. Finally, the indicator variable for whether an individual's UI-covered earnings exceeded that the earnings from working full-time (2,000 hours per year) at the prevailing Federal minimum wage rate ($5.15 per hour).

[22] The AFDC/TANF variables were constructed using data from the California statewide Medi-Cal Eligibility Data System (MEDS) files, which contain monthly information on whether an individual received AFDC (before 1998) or TANF (starting in 1998) benefits in California during a month. These monthly data were organized into 3-month "quarters" from the quarter of enrollment in the MDRC GAIN evaluation and then organized into "years" since enrollment, as was done with the employment and earnings data. The "Ever Received AFDC/TANF Benefits in Year" variable was defined to be = 1 if the individual received AFDC or TANF benefits in at least one month during that year and = 0 otherwise.

[23] See Riccio, *et al*. (1994) for 3-year impact estimates and Freedman, *et al*. (1996) for estimates based on five years of follow-up data.

[24] In their analysis, MDRC defined the first year of post-random assignment to be quarters 2 through 5, year two as quarters 6 through 9, etc. In our analysis, we define year one as quarters 1 through 4, year two as quarters 5 through 8, etc. This difference in definitions results in relatively minor differences between our years 1 through 5 estimates

counties we consider. As noted in the Introduction, analyzing the longer-term consequences of GAIN is important for several reasons. By analyzing the effects of these county-level programs over a longer follow-up period, one is better able to assess the duration or permanence of the impacts found in the previous 3-year and 5-year analyses. Furthermore, analyzing impacts over a nine-year period allows us to better assess the extent to which there are differences in the temporal pattern of the returns to the quick-job-entry versus skill development training strategies used by the different counties in their GAIN programs. This issue is potentially important, as noted above, because the longer training periods of the HCD approach imply that effects are likely to be negative in the short-term (during the training period and shortly thereafter). On the other hand, the skills developed might have longer lasting effects than the primarily motivational LFA approach. (We note that the ranking of the strategies also depends upon the how one "discounts" the future benefits as well as on the relative costs of each.)

We first consider the long-term impacts of the four GAIN programs for AFDC-FG cases presented in Table 5. Consider first the impacts on employment. Regardless of whether one uses annual employment rates or the number of quarters employed in a year, one finds that the impacts of Riverside's program are consistently larger, and statistically significant, relative to the effects for the other three counties over the first three-year period after random assignment. Over the first three years, the GAIN registrants in Riverside had annual employment rates that were, on average, 13.6 percentage points (39%) higher than members of the control group and worked 0.43 more quarters per year (48%) higher than did control group members. The employment impacts of the GAIN programs in the GAIN programs of the other three counties are considerably lower than those for Riverside and often are not statistically significant. This relative success of

relative to those produced by MDRC.

the Riverside program in improving the employment outcomes of GAIN registrants illustrates why this program, and its work-first orientation, has been heralded nationally as a model welfare-to-work program.

In the longer run, however, the employment impacts of the Riverside GAIN program diminish in magnitude and statistical significance. In years 4 through 6 after random assignment, Riverside's GAIN registrants experience a 6.9 percentage point annual average gain in annual rates of employment (down from 13.6 percentage points) and 0.25 quarters worked (down from 0.43 quarters) over their control group counterparts. For years 7 through 9, the Riverside GAIN registrants have an average annual gain of only 1.5 percentage points in annual rates of employment and 0.08 quarters worked per year relative to the control group and these latter impacts estimates are no longer significantly different from zero.[25] The employment effects of the GAIN programs in Alameda and San Diego also decline in magnitude and statistical significance and the impacts attributable to GAIN in these counties remain substantially smaller than those for Riverside. However, the GAIN impacts on the two measures of annual employment for the Los Angeles program grow in magnitude in years 4 through 9 relative to those in the first three years. Recall from Table 1 that the GAIN program in Los Angeles concentrated its services on long-term welfare recipients at the time our sample members were randomly assigned and, from Table 4, that this program, at that time, was oriented toward the providing its registrants with basic education and skill development programs. On average, the annual employment rates of the GAIN registrants in Los Angeles are 3.3 (3.8) percentage points greater per year and the number of quarters worked per year is 0.10 (1.3) larger than the corresponding averages for control group members in years 4 through 6 (years 7 through 9) after random assignment. These later-year im-

[25] We also note that the average employment rates and quarters worked per year for experimentals in Riverside consistently decline in magnitude over the nine-year. This is in contrast to the other 3 counties, where comparable out-

pacts impact estimates for Los Angeles are all statistically significant and are larger than found for any of the other three county programs, including Riverside's.

It is possible that the larger impacts of GAIN on employment effects found in Los Angeles County over the latter three years of our post-enrollment data may be the result of changes in that County's GAIN program that were initiated in 1995. (These changes were in effect during years 6 through 9 of our analysis period.) In 1995, Los Angeles County re-oriented its GAIN program toward a "work-first" or "job-first" program, adopting a program model similar to that used in Riverside. All members of our sample that continued to reside in Los Angeles County and remained on welfare—as well as all other GAIN-mandated adults in the County's program—would have been eligible for this new program during years 6 through 9. Moreover, recent evidence from a random-assignment evaluation of the 2-year post-enrollment impacts of Los Angeles's re-oriented GAIN program indicate that it had positive employment effects on AFDC-FG adults, similar to the initial effects found for the Riverside program.[26] Consistent with the possible impacts of Los Angeles's reoriented program is the fact that the employment rates and quarters worked for both experimental and control group members in our sample increased in years 6 through 9, relative to earlier years. While we cannot rule out this explanation for the larger employment effects in Los Angeles, we find little evidence that the change in the Los Angeles GAIN program had noticeable affects on the other outcomes (earnings and welfare participation) in years 6 through 9. It would also be difficult to explain in this interpretation why the gain in the later years is larger for the experimental group than the control group. (More on this below.)

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comes for experimentals in each of the other three counties increased over the nine-year follow-up period.

[26] Freedman, *et al*. (1999, 2000) provide the official results from MDRC's evaluation of Los Angeles Job-First GAIN program.

The impacts of GAIN programs on earnings and our indicator of poverty for AFDC-FG households are displayed in Table 5. As with the impacts on employment, we find that the differences in earnings and in the incidence of earnings being greater than our "threshold" for poverty—namely, that a sample member's annual earnings exceeded the income generated by working full time at the minimum wage—between experimentals and controls tends to decline in both Riverside and San Diego over the nine-year follow-up period. In the case of Riverside, annual earnings gains go from an average of $1,416 per year in the first three years to an annual average of $411 over the last three years. The comparable averages for San Diego are $616 and $446, respectively. Nonetheless, we note that the impacts on earnings in Riverside are sizeable and remain so, even six to nine years after individuals were randomly assigned in that county. With respect to the effects of the GAIN programs in Alameda and Los Angeles counties on earnings and our poverty measure, our estimates of three-year averages over the nine-year period are seldom statistically significant, although we do find that the magnitude of the impacts almost always increase in years 4 through 9 relative to those for years 1 through 3.

We also present, in Table 5, estimates of the impacts of the GAIN programs on welfare participation over the nine years after random assignment for AFDC-FG GAIN registrants. As is clear from the estimates in this panel, the GAIN participants in each of the counties consistently have lower rates and quarters of welfare participation than their control group counterparts over the nine-year period and these differences are statistically significant in many of the years after random assignment, including the latter four years. Clearly, the welfare reductions are largest for Riverside, with GAIN registrants who averaged a 5.8 percentage point average annual lower rate of AFDC/TANF participation than the control group in the first three years after random assignment and a 4.8 (3.2) percentage point differential in years 4 through 6 (years 7 through 9). While the welfare reductions attributable to the GAIN program in San Diego are smaller in magnitude than in Riverside, the effects for this county also are statistically significant in almost every year. Finally, while the GAIN registrants in Alameda and Los Angeles GAIN programs also experienced evidence of welfare reductions, the effects in these two counties tended to be smaller in magnitude and less reliably estimated, especially in the last two years of the follow-up period.

In Table 6, we present the corresponding estimates of long-term impacts for the GAIN registrants for the AFDC-U cases in Alameda, Los Angeles, Riverside and San Diego counties. There are several notable differences in the findings for two-parent AFDC households compared to those found for single-parent (and largely female-headed) AFDC households recorded in Table 5. With respect to the impacts on employment, most of the gains for GAIN registrants in each of these counties, at least the ones that are precisely estimated, occur in the first six years after random assignment. Second, the GAIN program in Los Angeles, rather than those in Riverside or San Diego counties, shows the largest impacts during the first six years after random assignment,[27] although the impacts in this county fall off markedly after 7 and 9 years after random assignment. Turning next to the GAIN impacts on earnings and poverty for AFDC-U households in Table 6, we find that virtually none of the annual impact estimates are precisely estimated for any of the years. It is notable, however, that the impacts on earnings for GAIN registrants in Alameda county are substantially larger in years 4 through 9—an average impact of \$654 per year in years 4 through 6 and \$935 in 7 through 9—relative to those for the first three years—an average impact of \$54 per year—although none of these estimates are statistically significant at conventional levels of significance.

Finally, the results in Table 6 for welfare participation show that, for AFDC-U, the most

[27] Freedman, *et al*. (1996) note this in their 5-Year GAIN Impact Analysis working paper.

persistent reductions in welfare over the nine years following random assignment occur for the GAIN participants in Alameda and Los Angeles counties, the two counties that emphasized basic education and skill development in their programs. Moreover, the reductions in welfare dependence actually improve over time for these two counties. For example, the GAIN registrants in Alameda had, on average, a 6.9 percentage point lower rate of participation in AFDC/TANF than their control group counterparts in the first three years after random assignment and a 17.2 (13.9) percentage point lower rate in years 4 through 6 (years 6 through 9). While the reductions for GAIN registrants are lower in Los Angeles County, they do "improve" from a 2.7 percentage point reduction in the first three years to an average reduction of 5.0 and 3.3 percentage points per year in years 4 through 6 and 7 through 9, respectively.

In summary, our examination of the long-term experimental estimates of the impacts of the GAIN programs in these four counties indicate some noticeable differences between experimental effects in the years immediately following random assignment (years 1 through 3) compared to experimental effects at longer intervals after randomization (years 7 through 9). Furthermore, the results on the longer run impacts of GAIN are less supportive of the view that the Riverside GAIN program dominates those in the other counties we analyze. However, drawing the latter conclusion, while tempting, is subject to the flaw that was noted in the Introduction, namely, that the experimental design does not support conclusions about the *differential* effects of programs based solely on evidence from within-county random assignment evaluations. In the next section, we discuss an econometric strategy to get at these differential effects and discuss how to validate its reliability, using data for the within-county control groups.

4. Identifying the Differential Effects of GAIN Programs

In this section, we discuss how to estimate, or identify, the differential effects of treatments. As noted above, the results from the MDRC GAIN Evaluation have been interpreted as suggesting that the work-first oriented program used in Riverside County is relatively more effective than the human-capital development programs used in the other counties, especially Alameda and Los Angeles Counties. Implicitly, the MDRC GAIN results have been interpreted as if they answered the following question: What would the average outcome have been for participants in, say, Los Angeles, had they been subject to the program as implemented in Riverside? Clearly, answering this question is of crucial important to the administrators in these counties and the rest of the nation for deciding which welfare-to-work model to implement.

To answer this question, one needs to determine the differential effects of the work-first oriented treatment used in Riverside relative to the human-capital oriented treatments in the other counties. As we have suggested in the Introduction, the within-county experimental design employed in the GAIN Evaluation does not immediately lend itself to estimating differential effects. The within-site experimental design used in the MDRC GAIN evaluation only allows comparisons of average sites against an average of alternative sites, rather than a comparison of two specific sites against each other, as we attempt to do here. Randomization of treatments across sites would be required to obtain experimental estimates of the differential effects of these treatments. Without randomization over sites the problems in comparing program results in different locations are similar to, although distinct from, those in justifying a causal interpretation of treatment-control differences in non-experimental evaluations. Below, we characterize more precisely why this is so.

In an effort to obtain estimates of differential impacts using data on cross-county comparisons, we outline how matching, or regression-adjustment, estimation methods could be used to estimate the differential effects of alternative program treatments, such as work-first versus human-capital development ones and the conditions required for one to use such methods with the four-county data available in the MDRC Evaluation. While such methods are inherently more controversial than those from a properly designed experiment in which different treatments are randomly assigned to subjects from a particular, population, recent studies by Dehejia and Wahba (1999), and Heckman, Ichimura, Smith and Todd (1997, 1998a) suggest such adjustments with sufficiently detailed observable characteristics may lead to credible non-experimental estimates of average gross treatment effects (AGTEs). Herein, we examine whether the effectiveness of these methods extend to estimating the differential effects of alternative treatments. In our assessment, we discuss how one can use the data on the experimentally-generated control groups for these four counties to validate, in part, these conditions for use of regression-adjustment and matching methods.

Recalling the discussion in Section 3.1, we now consider the identification of differential treatment effects. Adopting the notation used in that section, we first define the *average differential treatment effect* (ADTE) of treatment k relative to treatment k' as,

$$\gamma_t(k,k') \equiv E\left(Y_{it}(k) - Y_{it}(k')\right) = E\left(\Delta_{it}(k) - \Delta_{it}(k')\right), \tag{4}$$

where the second equality in (4) follows from the definition of $\Delta_{it}(j)$. The conditional analogue of (4) is given by

$$\gamma_t(k,k'|x) \equiv E\left(Y_{it}(k) - Y_{it}(k')\middle|X_i = x\right) = E\left(\Delta_{it}(k) - \Delta_{it}(k')\middle|X_i = x\right), \tag{5}$$

for k and $k' \in \{w,h\}$, $k \neq k'$.

There are at least two reasons why data from the MDRC GAIN county-specific experimental evaluations do not necessarily allow us to identify ADTEs. First, as noted above, the treatments received by households varied by their county of residence, with Riverside's GAIN program emphasizing work-first treatments ($T = w$) while the programs in Alameda and Los Angeles Counties emphasized human-capital development treatments ($T = h$).[28] More formally, this implies that

$$T_i \not\perp D_i. \tag{C-2}$$

In an extreme, we might consider T_i is a deterministic function of D_i, i.e.,

$$\Pr(T_i = k_d \,|D_i = d) = 1 \text{ and } \Pr(T_i = k'_{d'} \,|D_i = d) = 0, \tag{C-2′}$$

for all $k_d, k'_{d'} \neq 0$. This characterization of the dependence between T and D seems reasonable since, as noted above, Riverside's entire GAIN program was work-first oriented while the GAIN programs in Alameda and Los Angeles Counties were oriented toward human capital development, at least over the periods of enrollment in the MDRC GAIN Evaluation. Throughout the remainder of this paper we will assume that condition (C-2′) holds. Second, the populations—and their potential incomes, $Y_t(k)$—may differ across counties, i.e.,

$$Y_{it}(k) \not\perp D_i, \tag{6}$$

for all $k \in \{0, w, h\}$. As a consequence, there is no guarantee that the average differences in outcomes for treatments k_d and $k'_{d'}$, specific to two counties d and d', respectively, will identify the ADTE for these two treatments. To see this, we write the potential outcome associated with treatment k that is received in County d as follows:

$$Y(k_d) = Y_{it}^d(0) + \Delta_{it}^d(k), \tag{7}$$

where both $Y_{it}^d(0)$ and $\Delta_{it}^d(k)$ are county-specific and are treated as random variables. From the available data, one can identify the differences in mean outcomes for those receiving treatments k_d and $k'_{d'}$,

$$E\left(Y_{it}^d(k_d)\middle|T_i = k_d, D_i = d\right) - E\left(Y_{it}^{d'}(k'_{d'})\middle|T_i = k'_{d'}, D_i = d'\right),$$

[28] San Diego's GAIN program was a mixture of these two types of programs.

across Counties d and d'. Using the characterization of the outcomes associated with these treatments in (7), it follows that

$$\begin{aligned}
&E\left(Y_{it}(k_d)\middle|T_i=k_d,D_i=d\right)-E\left(Y_{it}(k'_{d'})\middle|T_i=k'_{d'},D_i=d'\right)\\
&\quad=E\left(Y_{it}^{d}(0)+\Delta_{it}^{d}(k_d)\middle|T_i=k_d,D_i=d\right)-E\left(Y_{it}^{d'}(0)+\Delta_{it}^{d'}(k'_{d'})\middle|T_i=k'_{d'},D_i=d'\right)\\
&\quad=\left\{E\left(\Delta_{it}^{d}(k_d)\middle|T_i=k_d,D_i=d\right)-E\left(\Delta_{it}^{d'}(k'_{d'})\middle|T_i=k'_{d'},D_i=d'\right)\right\}\\
&\qquad+\left\{E\left(Y_{it}^{d}(0)\middle|T_i=k_d,D_i=d\right)-E\left(Y_{it}^{d'}(0)\middle|T_i=k'_{d'},D_i=d'\right)\right\}.
\end{aligned}\tag{8}$$

In general, the expression in (8) is not equal to (4), the ADTE for treatments k and k'. Their equality requires that the following two additional conditions hold:

$$E\left(Y_{it}^{d}(0)\middle|T_i=k_d,D_i=d\right)-E\left(Y_{it}^{d'}(0)\middle|T_i=k'_{d'},D_i=d'\right)=0\,,\tag{A-1}$$

for all d, d' and t i.e., there is no difference in the no-treatment outcomes across counties d and d', and

$$E\left(\Delta_{it}^{d}(k)\right)=E\left(\Delta_{it}(k)\right),\tag{A-2}$$

for all d and t, i.e., the expected gross treatment effect of treatment k does not vary with the county of residence d.

The within-county experimental design employed in MDRC's evaluation of the GAIN programs in its analysis counties does not guarantee that conditions (A-1) and (A-2) hold. Moreover, the descriptions of the GAIN registrants and programs across the four counties presented in Section 2 do not lend support to either of these conditions. Recall the pre-enrollment differences in earnings, labor force participation and welfare receipt between the GAIN registrants in Riverside relative to those in Alameda or Los Angeles Counties and the differences in personal characteristics (Table 1) and the differences in labor market conditions across counties at the time of enrollment into the MDRC GAIN evaluation (Table 2). These differences suggest that (A-1) will not hold, i.e., that there are differences in the populations, and thus, the $Y_{it}(0)$s, across counties, irrespective of county differences in GAIN programs. Condition (A-2) stipulates that the gross gains for the *same* treatment must be the same in all counties. If the average gross gain from a treatment, such as human capital development, depends on a registrant's educational attainment and language skills and there are differences in the distribution of these initial skills in the registrant population differ across counties, Condition (A-2) will be violated. Alternatively, if the effectiveness of a particular treatment, such as work-first treatments, depends on economic conditions, i.e., the availability of jobs in a labor market, Condition (A-2) would be violated. Again, the differences in educational backgrounds of registrants and labor market conditions across the four counties in the MDRC GAIN Evaluation discussed in Section 2 suggest that (A-2) is unlikely to hold in our data.

To deal with the lack of credibility in maintaining (A-1) and (A-2) for the GAIN evaluation data, we consider the use of matching and/or regression-adjustment methods.[29] Such methods are predicated on the availability of a sufficiently rich set of observable background characteristics and outcome histories for GAIN registrants and measures of labor market conditions that can be used to adjust for the differences across counties so that conditional versions of (A-1) and (A-2) hold. More precisely, while (A-1) and (A-2) need not hold, suppose that the following conditional versions of them do:

$$E\left(\left(Y_{it}^{d}(0)\middle|T_i = k, D_i = d\right) - E\left(Y_{it}^{d'}(0)\middle|T_i = k'_{d'}, D_i = d'\right)\middle|X_i\right) = 0 \tag{A-1$'$}$$

and

$$E\left(E\left(\Delta_{it}^{d}(k)\right) - E\left(\Delta_{it}^{d'}(k)\right)\middle|X_i\right) = 0\,, \tag{A-2$'$}$$

for all $k \in \{w, h\}$. Assumptions (A-1′) and (A-2′) are sufficient to justify the use of non-paramet-

[29] See Rubin (1979) and Heckman, LaLonde and Smith (1999) for discussions of these methods and the conditions that support them.

ric methods and, in certain cases, parametric regression techniques to identify the ADTEs in (4) and (5).[30] By matching individuals in one county with comparable individuals (based on X) in the other, one can eliminate both the population differences that exist in the absence of any treatments as well as the county-specific factors that lead to differences in gross gains associated with a particular treatment across counties.

As with any non-experimental methodology, maintaining assumptions like (A-1′) and (A-2′) are inherently controversial, because their validity typically cannot be ensured or verified. Note, however, that in our case the validity of (A-1′) can be examined empirically, given that we have data on controls from the within-county experiments conducted by MDRC. In particular, Condition (C-1′), implies that

$$E\left(Y_{it}^{d}(0)\middle|T_i = k_d, D_i = d\right) = E\left(Y_{it}^{d}(0)\middle|T_i = 0, D_i = d\right) \qquad \text{(C-1′)}$$

for all d and, thus, that the mean outcomes for control group members in each county is a consistent estimator of $E\left(Y_{it}^{d}(0)\middle|T_i = k_d, D_i = d\right)$. (Note that this result holds for observable subgroups of the GAIN registrant populations as well. As a result, we can test whether Assumption (A-1′) holds in our data. (A similar test for Assumption (A-2′) cannot be performed. Thus, this assumption will need to be maintained. However, in our view, maintaining it is much less controversial so long as (A-1) holds.) Furthermore, it follows from (C-1′) and (8) that one can use the data on expected outcomes for control group members in counties d and d' to identify ADTE for the treatments, k_d and $k'_{d'}$ used in these two counties, so long as Assumption (A-2′) holds.

[30] Stronger forms of these conditions are often invoked in the matching literature. In particular, in place of (A-1′) and (A-2′), the following conditional independence assumptions are often used:

$$Y_{it}(0) \perp D_i \middle| X_i \qquad \text{(A-1″)}$$

and

$$\Delta_{it}(k) \perp D_i \middle| X_i \,. \qquad \text{(A-2″)}$$

More formally, (C-1′) and (8) imply that

$$\begin{aligned}&\left[E\left(Y_{it}(k_d)\middle|T_i=k_d,D_i=d\right)-E\left(Y_{it}^{d}(0)\middle|T_i=0,D_i=d\right)\right]\\&\quad-\left[E\left(Y_{it}(k'_{d'})\middle|T_i=k'_{d'},D_i=d'\right)-E\left(Y_{it}^{d'}(0)\middle|T_i=0,D_i=d'\right)\right]\\&=E\left(\Delta_{it}^{d}(k_d)\middle|T_i=k_d,D_i=d\right)-E\left(\Delta_{it}^{d'}(k'_{d'})\middle|T_i=k'_{d'},D_i=d'\right)\\&=\gamma_t(k,k').\end{aligned} \tag{9}$$

Thus, the availability of the within-county experimental data implies an alternative way of identifying the ADTEs for these two treatments so long as (A-2) holds.

In the empirical analysis that follows, we make use of parametric regression methods, rather than non-parametric matching techniques, to condition on the *X*'s as suggested by assumptions (A-1′) and (A-2′).[31] We focus on contrasting the effects of the Riverside work-first oriented program relative to those for the more human-capital development oriented programs in each of the other three counties (Alameda, Riverside, and San Diego). For one of these contrasts, let R_i = 1 denote a registrant being enrolled in Riverside's program and 0 for those residing in the comparison county. Let T_i be defined here as equal to one if the registrant was in the experimental group in a particular county and equal to zero otherwise. Below, we present estimates for two alternative regression specifications. In the first, we use data only on experimental subjects (T_i = 1) from Riverside and a comparison county to estimate the following regression model:

$$Y_{it}=\beta_{0t}+\beta_{1t}R_i+\beta'_{2t}X_i+\varepsilon_{it}, \tag{10}$$

where ε_{it} is a stochastic disturbance assumed to have mean zero. The coefficient on the Riverside dummy (β_{1t}) measures the effect of the program in Riverside relative to the program in the comparison county (Los Angeles, Alameda, or San Diego) on outcome *Y* as of period *t*. The estimate

For our purposes, we only require the conditional mean independence assumptions in (A-1′) and (A-2′).

[31] While not presented herein, we also used non-parametric matching techniques, controlling for the same set of *X*'s listed above, to estimate the differential treatment effects between Riverside and the various comparison counties.

of this coefficient provides a consistent estimator of γ_t in (4) if assumptions (A-1′) and (A-2′) hold. In estimating this linear regression function, we include in X_i personal characteristics (indicator for female, indicators for five levels of education, Hispanic, black, an indicator for having one child, an indicator for having children under the age of five), past earnings and labor force participation indicators (for quarters one to ten prior to randomization), indicator for past receipt of welfare and the amounts of AFDC benefits received (for quarters one to four prior to randomization), for the head of household each household, as well as variables describing individual labor market histories at the time of random assignment. We refer to the estimates of β_{1t} below as the "regression-adjusted difference in means for experimentals." We also present estimates β_{1t} for a variant of (10) that does not include the X_i in order to depict the consequences of controlling for these observables. Below, we refer to the estimates for this variant as the "unadjusted difference in means for experimentals."

Estimating the specification in (10) (that includes X) with data for members of the experimental groups from Riverside and a comparison county presumes that both conditions (A-1′) and (A-2′) holds. As noted above, the availability of county-specific controls groups that were determined by random assignment, allows us scope for testing the validity of (A-1′). To test this assumption, we estimate the specification in (10) using data for members of the controls groups of Riverside and the comparison county. Given this data on control groups, condition (A-1′) implies that $\beta_{1t} = 0$ in (10). That is, if our regression-adjustment method is successful, there should therefore be no difference in average outcomes of individuals in the control groups between the sites and the estimate for β_1 should be close to zero, both substantively and statistically.[32] Below, we

The estimates, especially the inferences drawn, are quite similar to the regression-based estimates reported below.

[32] For other examples of evaluating procedures by applying them to groups for whom the effects are known (typi-

present estimates of β_{1t} for the various outcomes of control group members for each of the Riverside-comparison County contrasts for specifications of (10) that include and do not include X. The resulting estimates of β_{1t} that do not control for X are referred to as "unadjusted difference in means for controls" and those that do are labeled as "regression-adjusted difference in means for controls."

Failure to reject the null hypothesis that $\beta_{1t} = 0$ for outcomes of the controls groups in Riverside and a comparison county even though the null is not true, i.e., a Type II error may be committed in testing $\beta_{1t} = 0$. To guard against the consequences of this type of error while, at the same time, exploiting the availability of data on randomly generated control groups within each of the counties, we also present estimates for two "difference-in-differences" estimators. The first is the sample analogue of (9), i.e., the difference-in-differences of the mean outcomes for experimentals and controls for Riverside and the comparison county. This estimate corresponds to the coefficient, β_{3t}, in the following regression:

$$Y_{it} = \beta_{0t} + \beta_{1t} R_i + \beta_{2t} T_i + \beta_{3t} R_i T_i + \nu_{it} . \tag{11}$$

This estimator is a consistent estimator for the ADTE for the treatments used in these two counties so long as condition (A-2) holds. We refer to the estimates for this estimator in the tables below as the "unadjusted difference-in-differences." We also present a difference-in-difference estimator of the ADTEs that controls for X, i.e.,

$$Y_{it} = \beta_{0t} + \beta_{1t} R_i + \beta_{2t} T_i + \beta_{3t} R_i T_i + \beta_{4t}' X_i + \beta_{5t}' X_i T_i + \nu_{it} , \tag{12}$$

where the β_{3t} corresponds to γ_t. Below, we refer to estimates of β_{3t} as the "regression adjusted difference-in-differences" estimates.

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cally zero, as in this case), see Lalonde (1986), Heckman and Hotz (1989), Rosenbaum (1995), Hotz, Imbens and Mortimer (1999).

Because the latter difference-in-differences estimator in (12) (β_{3t}) exploits the experimental data on control groups and only relies on condition (A-2) holding, it is arguably a more credible non-experimental estimator of the differential effects of the alternative treatments used in Riverside and the comparison counties. In essence, this estimator eliminates the additive differences between Riverside and the other sites (via the control group outcomes) as well as adjusts for the observable Xs. It should be kept in mind, however, that if the differences in average outcomes for controls between Riverside and Los Angeles (or Alameda or San Diego) are eliminated by adjusting for pre-randomization variables—that is, if the coefficient on the Riverside dummy is close to zero in the control regression—the trainee-only estimates should be close to the difference-in-differences estimates (but the former estimated more precisely than the latter). If, on the other hand, the control estimates are far from zero, one might be concerned that there is some important unobserved effect on outcomes or that the differences between Riverside and the other sites are not necessarily additive, and the difference-in-differences estimates would be less credible. The second concern with the difference-in-differences estimates is that the statistical significance levels may be affected by the relative scarcity of the control groups. For example, in Riverside there are 4,358 trainees, but only 1,025 controls. If we can successfully control for cross-county differences in the control group, then we may prefer the adjusted difference for the trainees in a bias/variance tradeoff similar to the one concerning the decision to include additional controls with limited explanatory power in a linear regression framework.

5. Estimates of Differential Effects of GAIN Programs

In this section, we present estimates of the differential effects of the GAIN programs run in Riverside County, relative to those in Alameda, Los Angeles and San Diego counties. Results for AFDC-FG cases are presented in Table 7 and those for AFDC-U cases in Table 8. The Appendix presents results separately for the in need of basic education and not in need of basic education groups. In what follows, we discuss the estimates for sets of differential effects, namely Riverside vs. Alameda, Riverside vs. Los Angeles, and Riverside vs. San Diego.

5.1 Riverside versus Alameda

The results in the left set of columns in Table 7 presents the results for the Riverside-Alameda comparison for the AFDC-FG group. The first set of results presents estimates for the differences between Riverside and Alameda for yearly employment indicators. The simple difference in outcomes for trainees averaged over the first three post-randomization years shows that 18.2% more trainees in Riverside are employed than trainees in Alameda. However, even the difference between Riverside and Alameda for controls, 7.3% is significantly different from zero (at the 1% level). Consistent with the descriptive results on differences in the enrolled population across counties, this suggests that the populations enrolled in GAIN differed, so simply comparing outcomes for the trainees is not appropriate. The difference-in-differences estimate, equal to the difference between the trainee and control differences is 10.9%.

Allowing for heterogeneity, we include covariates in the levels and as interactions with the treatment effects to estimate the net effect of the Riverside GAIN program relative to the Alameda. Adjusting for covariates, the estimated differences between Riverside and Alameda averaged over the first three years changes to 16.0% for trainees and 3.7% for controls. Note that the difference between Riverside and Alameda for controls is halved, and is no longer significantly different from zero even at the 10% level. The point estimate of the net effect of Riverside relative to Alameda increases from 10.9% to 12.3%.

For the other two time periods, years 4-6 and years 7-9, the differences between trainees in Riverside and Alameda are substantial, and many cases significantly different from zero. Adjusting for pre-randomization differences does not affect these estimates much. For the controls some of the raw differences between Riverside and Alameda are large and significant, but adjusting for pre-randomization differences makes these differences substantially smaller and largely insignificant. Unlike the first three years, the adjusted difference-in-difference estimates are not significantly different from zero in the last six years. The estimated net effect declines from 12.3% for the first three years to 5.2% for years 4-6 and -0.4% for years 7-9.

For the second outcome, the number of quarters employed in a year, the same pattern emerges. The raw differences for control are often significantly different from zero, but adjusting for pre-randomization characteristics eliminates a substantial part of these differences and renders them largely insignificant. The trainee differences remain robust to adjusting for covariates. The difference-in-differences estimates follow the same pattern of significant and positive effects in the first three-year period (0.43), going down in years 4-6 (to 0.24 years) and becoming negative in years 7-9 (-0.01).

The third outcome is total yearly earnings. The general pattern is repeated again. Control differences between Riverside and Alameda are substantial and significant before adjusting for covariates, but much smaller and all insignificant after adjustment. For the three time periods the unadjusted differences are \$403, \$-141, and \$-1,032, which reduces after adjusting for covariates to \$107, \$88 and \$-185. Again the trainee differences are more robust. In this case the pattern of initial positive relative effects for Riverside followed by later negative relative effects is repeated, but now the negative effects in the final years are significant at the 5% level. The difference-in-differences estimates decline sharply, from \$876 in the first period, to \$210 in the second, and -\$911 in the third three-year period.

For the fourth outcome, the indicator for earnings above the full-time minimum wage level, the story is again similar, but the effects are now imprecisely estimated. There are differences for the unadjusted controls, but adjusting them for pre-randomization differences makes them smaller and insignificant. Differences between trainees remain, with the program in Riverside in early years significantly more successful than in Alameda in the early years. In the later years, there is a reversal—Alameda's outcomes are better, and in the last three years the differences for the trainees are significantly different from zero.

The story for positive annual AFDC/TANF receipt is different. Here raw differences are very large and significant for controls, and adjustment for covariates does not entirely eliminate them, although it makes these differences smaller and considerably less significant. In contrast to the earnings measures, the differences between Riverside and Alameda for trainees are not robust to adjusting for covariates. In a typical three-year period the raw difference is on the order of 15% lower participation in Riverside, but after adjusting this is reduced to about 8% lower participation. Difference-in-difference estimates still follow the same pattern of the earnings results of an early advantage for Riverside (a bigger reduction in AFDC participation rates) followed by sharp decline, with the effects in later years close to zero. It should be kept in mind here that, in contrast to the earnings data, where we have ten quarters of pre-randomization outcomes including both the binary indicators and the amount, we have for AFDC/TANF receipt only the binary indicator, but not the amount, and only for four pre-randomization quarters. Perhaps, just as to adjust for employment and earnings, we need detailed histories of employment and earnings, to successfully adjust for AFDC/TANF receipt we would need detailed histories of receipt and payment amounts.

For the last outcome, the number of quarters with positive AFDC receipt the story is similar to that for annual AFDC receipt. Control differences are largely but not completely eliminated by adjusting for covariates (they remain significantly different from zero). The difference-in-differences estimates are, other than in the first three-year period, all small and not significantly different from zero.

5.2 Riverside versus Los Angeles

Next we discuss the Riverside versus Los Angeles comparison. Starting again with the annual employment indicators, we find that there are substantial differences between controls in Riverside and Los Angeles, ranging from 4.7% to 10.9%, and which are significant at the 1% level in all three time-periods. Adjusting for pre-randomization differences lowers the adjusted difference between controls across the two counties, now ranging from 0.7 to 4.4%, and significant at the 5% level in only one period. In contrast, the trainee differences are significant both before and after adjustment. It is interesting to note that in years 7-9 the unadjusted estimate for the trainee differences suggests a significantly higher employment rate in Riverside, whereas the adjusted estimate suggests a significantly lower employment. The adjusted difference-in-differences estimates suggest a pattern similar to that in the Riverside-Alameda comparisons: initial differences are large and in favor of Riverside, followed by a substantial and steady decline, leading to relatively employment rates that are higher in Los Angeles than in Riverside, although not significantly so.

The second outcome, the number of quarters employed each year follows the same pattern. There are highly significant differences between controls in Riverside and Los Angeles prior to adjustment (significant at the 1% level in two of the periods), but these differences are much smaller and insignificant after adjustment (none significant at the 10% level). For trainee differences we again find that the beneficial effect of the Riverside program relative to Los Angeles disappears after six years and turns into a significant comparative advantage for Los Angeles. Difference-in-difference estimates are positive and significant in early years and negative but not significant in later years.

For the level of earnings the pattern repeats. Raw control differences are largely eliminated by adjustment for pre-randomization variables, whereas for trainees the adjustment shows in later years significant advantages for the Los Angeles program in contrast to the early benefits of the Riverside program. For the final earnings-based measure, an indicator for earnings above the full-time minimum wage level the raw differences for the controls are already small and largely insignificant; the regression adjustments make them even smaller and less significant, but also eliminate a substantial part of the differences for trainees.

For the two AFDC outcomes the results for the Riverside-LA comparison are somewhat different from those for the Riverside-Alameda comparison. As before, we do find large difference in unadjusted differences for the control groups. However, the least squares adjustment eliminates virtually all of the differences. Whereas the raw differences for annual AFDC participation range from -12.8% to -7.2% significantly different from zero at the 1% level in all three periods, after adjusting the differences range from -2.8% to 0.3%, none significant even at the 10% level. The difference-in-difference estimates suggest a significantly bigger decrease in Riverside than Los Angeles in the early years, with an insignificant effect in later years. For the number of quarters spent on AFDC the story is similar. The raw differences for controls range from -0.56 to -0.31, all significant at the 1% level, and the adjusted differences range from -0.16 to -0.07, with two significantly different from zero at the 10% level.

5.3 Riverside versus San Diego

Finally we discuss the Riverside versus San Diego comparisons. Here we have more difficulty eliminating the differences between average control outcomes than in the other comparisons. This is somewhat surprising given that before adjustment, the population in San Diego looks more comparable to that in Riverside than the other two counties. Adjustment does make the differences smaller, but some do remain significantly different from zero.

However, for the two employment measures the general pattern of the coefficients is similar to that for the other two comparisons. The difference-in-differences estimates are much larger in the first few years but then decline and they are not significantly different from zero in the later years. Unlike in the comparisons with Alameda and Los Angeles the differences are never negative, always suggesting a benefit from attending the training in Riverside relative to San Diego.

For earnings the raw differences between controls is large and significant. Furthermore, the difference is not removed by adjusting for covariates. The difference-in-differences estimates are all positive after adjusting for covariates.

The two AFDC outcomes follow an interesting pattern. The raw differences for controls are all small and insignificant. This does not change if we adjust for pre-randomization variables. However, for the trainees the adjustment does make a considerable difference. The size of the difference and the significance goes up substantially. This is true both for annual AFDC receipts and for the number of quarters with positive AFDC receipts. For example, for the number of quarters with positive AFDC receipts the raw differences for trainees range from -0.15 to -0.07. After adjusting for pre-randomization differences the range is -0.30 to -0.19. The difference-in-difference estimates are also negative in all three periods for both outcomes after adjusting, and are often highly significant.

5.4 Conclusions from Cross-County Comparisons

Generally we find that there are substantial and significant differences in raw averages between sites, for all three comparisons, and all six outcomes over the three three-year periods post-randomization. Adjusting for a rich set of pre-randomization variables reduces and in most cases essentially eliminates these differences for the control groups. Exceptions are the AFDC/TANF receipts in Alameda County and some of the earnings outcomes in San Diego County where substantial differences from Riverside County remain even after least squares adjustment.

The ability for most comparisons and outcomes to adjust away the differences for controls suggests that the adjusted differences for trainees (and the adjusted difference-in-differences estimates which therefore are close to the trainee differences) can be interpreted as estimates of the causal effect of the Riverside County program versus the three others. Generally we find substantial positive effects of the Riverside County program (increasing employment rates and earnings, and lowering AFDC receipts) in the first three years post-randomization; the period covered by the MDRC evaluations. The effects, however, taper off and sometimes becoming significantly negative in the last two three-year periods in Alameda County and Los Angeles County. This interpretation is consistent with the effects of job search assistance being shorter lived than the effects of basic skills training, although without individual level data on the nature of the training it is difficult to further investigate this interpretation. It is also largely consistent with the interpretation that after the embargo ended, the control groups received training. This seems particularly likely in Riverside County were the program was nearly universal in the period after randomization ended; but less likely in the other three counties were the GAIN programs appear to have been smaller and the commitment to universal participation enrollment was weaker. We note, however, that such delayed treatment for the controls would not explain the sign change in the Riverside-LA and Riverside-Alameda comparisons found for employment and earnings outcomes.

The results underline the important, but discomforting, conclusion that short-term evaluation of training programs can be misleading. The relative ranking of programs is not stable over time. Simple extrapolations of early results to later results do not appear to be possible. The relation of short-term results to longer-term results appears to vary with program content in ways consistent with a priori expectations. Thus, despite the demands of policy makers for quick results with which to design new legislation and programs, there may be no substitute for long-term and costly follow-up, and thus for program design-evaluation-redesign cycles lasting a decade or more. Conventional follow-up periods of three, or even five, years may simply be too short.

Finally, let us return to the Riverside versus Alameda comparisons for AFDC receipt that was one of the most problematic comparisons. For these outcomes, adjusting for pre-randomization differences did not eliminate differences for the controls. For the Riverside-LA and Riverside-San Diego comparisons covariance adjustment was adequate. To gain some insight into these difficulties, consider the fraction on AFDC in each of the last four quarters prior to randomization. In Riverside these fractions are 63%, 64%, 65%, and 77% in chronological order. For San Diego these fractions are 57%, 59%, 60% and 71%, very comparable to the Riverside numbers. For Los Angeles the fractions are 98%, 99%, 99%, and 99%, much higher than in Riverside and San Diego. Finally, in Alameda the numbers are 97%, 97%, 97%, and 98%, again much higher than in Riverside and San Diego. These differences between Riverside and San Diego on the one hand, and Los Angeles and Alameda on the other hand, reflect the focus of the GAIN program in the latter two counties on long-term AFDC recipients. This focus was a result of insufficient funds to enroll all eligibles given the high per case cost of the their HCD strategy.

This difference in selection rules in turn is reflected in the large raw differences post-randomization between Riverside and both Los Angeles and Alameda. These differences are in fact largest between Riverside and Alameda, and the adjustment for four quarters of pre-randomization AFDC indicators does not appear to be sufficient to eliminate them, although it does reduce them substantially.

6. Conclusions

In this paper we analyze data from the GAIN experimental evaluations of job training programs. Whereas previous researchers have had only five years of post-randomization outcomes available, we have observations on nine years of earnings and welfare receipts after randomization. This allows us to explore the long-term effects of these programs. We find that the early superiority of the Riverside program with its stress on job search assistance rather than basic skills training is lessened over time. In the later years the programs in counties, such as Alameda and Los Angeles, are doing as well as, or even slightly better, than Riverside.

We also make a case that credible comparisons can be made between the countries. Although such comparisons cannot be justified by the randomization alone, we exploit the presence of control groups to validate such comparisons between trainees. We find that in the early years the program in Riverside did indeed lead to better outcomes, although the relative benefits of the Riverside program do disappear over time. Our analyses show the importance of having detailed characteristics of the individuals even in randomized experiments. The results presented here are also encouraging for the ability of non-experimental methods to reproduce the results of experimental results, if enough detailed information on individual characteristics (e.g., histories of employment, earnings, and welfare receipt) is available.

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Table 1: Background Characteristics and Pre-Randomization Histories of GAIN Evaluation Participants

Panel A: AFDC-FG Cases

	Alameda		Los Angeles		Riverside		San Diego	
Variable	*Mean*	*Std. Dev.*	*Mean*	*Std. Dev.*	*Mean*	*Std. Dev.*	*Mean*	*Std. Dev.*
Age	34.72	8.62	38.52	8.43	33.64	8.20	33.80	8.59
White	0.18	0.38	0.12	0.32	0.52	0.50	0.43	0.49
Hispanic	0.08	0.26	0.32	0.47	0.27	0.45	0.25	0.44
Black	0.70	0.46	0.45	0.50	0.16	0.36	0.23	0.42
Other Ethnic Groups	0.04	0.20	0.11	0.31	0.05	0.22	0.09	0.29
Female-Head	0.95	0.22	0.94	0.24	0.88	0.33	0.84	0.37
One Child	0.42	0.49	0.33	0.47	0.39	0.49	0.43	0.50
More than One Child	0.57	0.50	0.67	0.47	0.58	0.49	0.53	0.50
Child 0 to 5 Years	0.31	0.46	0.10	0.30	0.16	0.37	0.13	0.34
Highest Grade Completed	11.18	2.52	9.54	3.55	10.67	2.54	10.66	3.04
In Need of Basic Education	0.65	0.48	0.81	0.40	0.60	0.49	0.56	0.50
Earnings 1 Qtr. before Rand. Assign.[1]	$213	$852	$221	$875	$453	$1,405	$587	$1,485
Earnings 4 Qtrs. Before Rand. Assign.[1]	$264	$1,018	$216	$866	$613	$1,602	$808	$1,879
Earnings 8 Qtrs. Before Rand. Assign.[1]	$220	$1,008	$181	$796	$726	$1,839	$827	$1,958
Employed 1 Qtr. Before Rand. Assign.	0.14	0.34	0.12	0.33	0.22	0.41	0.27	0.44
Employed 4 Qtrs. Before Rand. Assign.	0.14	0.34	0.13	0.33	0.25	0.43	0.29	0.45
Employed 8 Qtrs. Before Rand. Assign.	0.13	0.33	0.11	0.32	0.27	0.44	0.28	0.45
AFDC Benefits 1 Qtr. Before Rand. Assign.[1]	$2,498	$687	$2,477	$876	$1,616	$1,416	$1,585	$1,234
AFDC Benefits 4 Qtrs. Before Rand. Assign.[1]	$2,477	$748	$2,564	$910	$1,395	$1,432	$1,425	$1,310
On AFDC 1 Qtr. Before Rand. Assign.	0.99	0.08	0.96	0.20	0.69	0.46	0.75	0.43
On AFDC 4 Qtrs. Before Rand. Assign.	0.98	0.15	0.96	0.20	0.56	0.50	0.62	0.49
Number of Experimental Observations	597		2,995		4,405		6,978	
Number of Control Observations	601		1,400		1,040		1,154	
Total Number of Observations	1,198		4,395		5,445		8,132	

[1] In 1999$.

Table 1: (Continued)

Panel B: AFDC-U Cases

Variable	Alameda *Mean*	Alameda *Std. Dev.*	Los Angeles *Mean*	Los Angeles *Std. Dev.*	Riverside *Mean*	Riverside *Std. Dev.*	San Diego *Mean*	San Diego *Std. Dev.*
Age	40.08	9.07	42.03	9.02	32.30	8.67	33.50	9.20
White	0.17	0.38	0.11	0.32	0.44	0.50	0.37	0.48
Hispanic	0.09	0.29	0.22	0.42	0.32	0.47	0.27	0.44
Black	0.18	0.38	0.04	0.20	0.08	0.28	0.10	0.30
Other Ethnic Groups	0.56	0.50	0.62	0.49	0.16	0.37	0.26	0.44
Female-Head	0.21	0.41	0.04	0.20	0.14	0.34	0.09	0.28
One Child	0.12	0.33	0.08	0.27	0.23	0.42	0.24	0.43
More than One Child	0.87	0.34	0.92	0.27	0.77	0.42	0.75	0.44
Child 0 to 5 Years	0.53	0.50	0.50	0.50	0.74	0.44	0.73	0.44
Highest Grade Completed	8.13	4.51	7.25	3.94	10.05	3.15	10.05	3.42
In Need of Basic Education	0.80	0.40	0.92	0.27	0.66	0.47	0.63	0.48
Earnings 1 Qtr. before Rand. Assign.[1]	$231	$996	$295	$772	$947	$1,987	$986	$1,931
Earnings 4 Qtrs. Before Rand. Assign.[1]	$155	$608	$279	$651	$1,449	$2,583	$1,333	$2,456
Earnings 8 Qtrs. Before Rand. Assign.[1]	$169	$607	$231	$575	$1,567	$2,817	$1,397	$2,670
Employed 1 Qtr. Before Rand. Assign.	0.11	0.32	0.21	0.41	0.36	0.48	0.38	0.49
Employed 4 Qtrs. Before Rand. Assign.	0.08	0.27	0.21	0.41	0.40	0.49	0.41	0.49
Employed 8 Qtrs. Before Rand. Assign.	0.12	0.33	0.20	0.40	0.40	0.49	0.39	0.49
AFDC Benefits 1 Qtr. Before Rand. Assign.[1]	$3,498	$897	$3,305	$1,094	$1,284	$1,606	$1,712	$1,539
AFDC Benefits 4 Qtrs. Before Rand. Assign.[1]	$3,531	$887	$3,411	$1,062	$982	$1,512	$1,422	$1,597
On AFDC 1 Qtr. Before Rand. Assign.	0.99	0.08	0.98	0.15	0.54	0.50	0.69	0.46
On AFDC 4 Qtrs. Before Rand. Assign.	0.99	0.08	0.98	0.15	0.36	0.48	0.51	0.50
Number of Experimental Observations	89		735		1,568		2,405	
Number of Control Observations	82		723		713		835	
Total Number of Observations	171		1,458		2,281		3,240	

[1]In 1999$.

Table 2: Annual Labor Market Conditions by County, 1989-90[1]

Year	Alameda	Los Angeles	Riverside	San Diego
Annual Unemployment Rate				
1988	4.8%	5.2%	6.5%	4.3%
1989	4.4%	5.5%	6.8%	3.9%
1990	3.9%	5.9%	6.3%	4.7%
Annual Employment Growth Rates				
1988	2.1%	0.9%	7.4%	2.0%
1989	2.1%	1.6%	7.4%	3.5%
1990	4.7%	4.0%	8.5%	6.0%
Annual Real Wage Income per Worker				
1988	$21,661	$23,070	$17,328	$18,820
1989	$21,569	$22,833	$17,453	$18,817
1990	$21,752	$22,957	$17,775	$18,919
Annual Growth Rates in Real Wage Income per Worker				
1988	0.4%	1.0%	-0.7%	0.0%
1989	-0.8%	-0.5%	-1.8%	-0.5%
1990	-0.4%	-1.1%	1.0%	0.1%
Share of Employment in Manufacturing				
1988	0.112	0.165	0.083	0.097
1989	0.113	0.172	0.086	0.098
1990	0.113	0.176	0.088	0.096
Share of Employment in Service Sector				
1988	0.293	0.333	0.279	0.289
1989	0.283	0.326	0.273	0.279
1990	0.282	0.324	0.275	0.279
Share of Employment in Public Administration				
1988	0.191	0.107	0.155	0.227
1989	0.193	0.107	0.156	0.228
1990	0.193	0.106	0.157	0.230

[1]Shaded entries denote years in which random assignment was conducted in various counties.

Table 3: Distribution of Average Monthly Participation in Various GAIN Activities[1,2]

Yr.:Qtr.	Job Club & Job Search Activities	All Other Job Search Activities	Basic Education Program	Vocational Training	OJT	PREP*	Supported Work & Transitional Employment
Alameda							
1988:Q3	0%	0%	0%	100%	0%	0%	0%
1988:Q4	0%	0%	0%	100%	0%	0%	0%
1989:Q1	21%	0%	53%	26%	0%	0%	0%
1989:Q2	34%	2%	37%	27%	0%	0%	0%
1989:Q3	35%	2%	36%	27%	0%	0%	0%
1989:Q4	33%	9%	44%	12%	0%	0%	0%
1990:Q1	29%	5%	44%	22%	0%	0%	0%
1990:Q2	45%	3%	38%	13%	1%	0%	0%
1990:Q3	24%	4%	32%	37%	0%	4%	0%
1990:Q4	19%	5%	38%	32%	0%	6%	0%
Los Angeles							
1988:Q3	N/A	N/A	N/A	N/A	N/A	N/A	N/A
1988:Q4	0%	0%	8%	92%	0%	0%	0%
1989:Q1	14%	0%	72%	14%	0%	0%	0%
1989:Q2	23%	1%	61%	15%	0%	0%	0%
1989:Q3	22%	2%	68%	8%	0%	0%	0%
1989:Q4	23%	4%	65%	8%	0%	0%	0%
1990:Q1	19%	7%	63%	12%	0%	0%	0%
1990:Q2	16%	5%	64%	15%	0%	0%	0%
1990:Q3	29%	6%	49%	15%	0%	0%	0%
1990:Q4	15%	3%	58%	24%	0%	0%	0%
Riverside							
1988:Q3	51%	9%	21%	20%	0%	0%	0%
1988:Q4	62%	7%	20%	10%	0%	0%	0%
1989:Q1	56%	3%	26%	14%	0%	0%	0%
1989:Q2	63%	5%	20%	12%	0%	0%	0%
1989:Q3	64%	3%	19%	14%	1%	0%	0%
1989:Q4	45%	2%	32%	21%	0%	0%	0%
1990:Q1	52%	3%	23%	22%	0%	0%	0%
1990:Q2	52%	1%	24%	23%	0%	0%	0%
1990:Q3	61%	3%	19%	17%	0%	0%	0%
1990:Q4	55%	4%	22%	19%	0%	0%	0%
San Diego							
1988:Q3	41%	1%	28%	28%	1%	2%	0%
1988:Q4	45%	1%	30%	22%	1%	2%	0%
1989:Q1	41%	1%	30%	24%	2%	2%	0%
1989:Q2	42%	2%	31%	21%	2%	2%	0%
1989:Q3	28%	5%	42%	23%	1%	2%	0%
1989:Q4	30%	6%	27%	28%	4%	5%	0%
1990:Q1	34%	8%	33%	21%	2%	3%	0%
1990:Q2	31%	6%	41%	15%	2%	4%	0%
1990:Q3	25%	8%	38%	22%	2%	5%	0%
1990:Q4	27%	7%	36%	19%	3%	8%	0%

[1]PREP stands for "Pre-Employment Preparation. This was California's form of Workfare, i.e., it was unpaid work experience.
[2]Shaded entries denote quarters in which random assignment was conducted in the various counties.

Table 4: Average per Month Enrollment in GAIN as Percentage of Total AFDC Enrollment*
[Source: GAIN25 Data]

Yr.:Qtr.	Alameda	Los Angeles	Riverside	San Diego
1988:Q3	0%	N/A	18%	32%
1988:Q4	0%	0%	21%	35%
1989:Q1	3%	2%	25%	40%
1989:Q2	5%	5%	26%	44%
1989:Q3	8%	7%	28%	46%
1989:Q4	10%	7%	33%	40%
1990:Q1	11%	6%	39%	48%
1990:Q2	10%	6%	40%	45%
1990:Q3	7%	5%	39%	43%
1990:Q4	8%	8%	39%	47%

*Shaded entries denote quarters in which random assignment was conducted in the various counties.

Table 5: Experimental Estimates of Annual Impacts of GAIN, Cases Enrolled in GAIN as AFDC-FG Full Sample

	Alameda				Los Angeles				Riverside				San Diego			
Yrs. after Enroll.	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*
Ever Employed in Year (%)																
1-3	30.8	28.1	2.7	10%	26.1	24.5	1.7	7%	49.0	35.3	13.6***	39%	45.1	40.8	4.3***	11%
4-6	37.0	34.7	2.3	7%	29.2	25.8	3.3***	13%	40.4	33.5	6.9***	21%	40.8	38.2	2.6*	7%
7-9	45.3	45.3	0.0	0%	36.9	33.1	3.8***	11%	39.3	37.8	1.5	4%	41.0	40.9	0.1	0%
Number of Quarters Employed in Year																
1-3	0.80	0.75	0.05	7%	0.71	0.67	0.04	5%	1.33	0.90	0.43***	48%	1.25	1.09	0.15***	14%
4-6	1.12	1.02	0.10	10%	0.87	0.77	0.10**	13%	1.23	0.98	0.25***	25%	1.26	1.17	0.09*	8%
7-9	1.47	1.42	0.05	3%	1.16	1.03	0.13***	13%	1.23	1.15	0.08	7%	1.32	1.28	0.04	3%
Annual Earnings (1999$)																
1-3	$2,333	$1,849	$484	26%	$1,843	$1,849	-$6	0%	$3,668	$2,253	$1,416***	63%	$3,781	$3,165	$616***	19%
4-6	$4,069	$3,342	$727	22%	$2,615	$2,493	$122	5%	$4,363	$3,201	$1,162***	36%	$4,849	$4,315	$534*	12%
7-9	$5,871	$5,206	$665	13%	$3,689	$3,386	$302	9%	$4,585	$4,174	$411	10%	$5,394	$4,948	$446	9%
Earnings above Full-Time Min. Wage (%)																
1-3	6.6	5.4	1.2	21%	5.4	5.7	-0.2	-4%	10.4	6.2	4.2***	69%	11.4	9.6	1.8**	19%
4-6	12.6	10.8	1.8	17%	8.8	8.5	0.3	4%	14.4	10.0	4.4***	44%	16.0	14.2	1.8*	12%
7-9	19.4	17.1	2.3	13%	11.8	11.2	0.6	5%	15.1	13.7	1.4	10%	17.8	15.9	1.9*	12%
Ever Received AFDC/TANF Benefits in Year (%)																
1-3	86.0	87.6	-1.6	-2%	85.1	87.6	-2.5***	-3%	71.7	77.5	-5.8***	-8%	73.9	76.5	-2.5***	-3%
4-6	56.3	63.2	-6.9***	-11%	53.7	57.4	-3.7**	-6%	39.8	44.6	-4.8***	-11%	41.3	44.5	-3.2**	-7%
7-9	37.2	39.8	-2.6	-6%	33.4	35.6	-2.2	-6%	25.3	28.5	-3.2**	-11%	26.2	28.9	-2.7**	-9%
Number of Quarters in Year on AFDC/TANF																
1-3	3.14	3.33	-0.18***	-5%	3.13	3.30	-0.17***	-5%	2.39	2.71	-0.32***	-12%	2.54	2.68	-0.15***	-6%
4-6	2.05	2.37	-0.33***	-14%	1.92	2.12	-0.20***	-9%	1.34	1.56	-0.22***	-14%	1.44	1.57	-0.13**	-8%
7-9	1.32	1.38	-0.06	-4%	1.18	1.28	-0.10*	-8%	0.83	0.97	-0.14***	-14%	0.90	0.99	-0.09**	-10%

NOTES: * denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.

Table 6: Experimental Estimates of Annual Impacts of GAIN, Cases Enrolled in GAIN as AFDC-U
Full Sample

Yrs. after Enroll.	Alameda Experi-mental	Alameda Control	Alameda Difference	Alameda % Dif-ference	Los Angeles Experi-mental	Los Angeles Control	Los Angeles Difference	Los Angeles % Dif-ference	Riverside Experi-mental	Riverside Control	Riverside Difference	Riverside % Dif-ference	San Diego Experi-mental	San Diego Control	San Diego Difference	San Diego % Dif-ference
Ever Employed in Year (%)																
1-3	31.1	19.5	11.6**	59%	38.6	28.8	9.8***	34%	53.2	44.7	8.5***	19%	51.6	46.0	5.6***	12%
4-6	28.8	21.1	7.7	36%	37.0	28.9	8.1***	28%	40.7	36.3	4.4**	12%	42.2	40.8	1.3	3%
7-9	35.6	32.1	3.5	11%	38.3	38.2	0.1	0%	36.5	36.4	0.1	0%	41.1	42.2	-1.1	-3%
Number of Quarters Employed in Year																
1-3	0.80	0.62	0.18	29%	1.19	0.91	0.28***	30%	1.42	1.17	0.25***	21%	1.46	1.30	0.17***	13%
4-6	0.93	0.64	0.29	45%	1.20	0.90	0.30***	34%	1.17	1.05	0.13**	12%	1.29	1.26	0.03	2%
7-9	1.08	1.00	0.08	8%	1.28	1.25	0.02	2%	1.11	1.12	-0.01	-1%	1.31	1.33	-0.02	-2%
Annual Earnings (1999$)																
1-3	$1,496	$1,442	$54	4%	$1,932	$1,638	$294	18%	$4,672	$3,736	$937***	25%	$4,654	$4,270	$384	9%
4-6	$2,837	$2,182	$654	30%	$2,316	$1,970	$347	18%	$4,398	$3,857	$541	14%	$4,792	$4,757	$35	1%
7-9	$4,139	$3,205	$935	29%	$2,950	$3,132	-$182	-6%	$4,365	$4,322	$43	1%	$5,420	$5,542	-$123	-2%
Earnings above Full-Time Min. Wage (%)																
1-3	3.7	4.1	-0.3	-8%	2.9	2.9	0.0	2%	14.0	10.8	3.2***	30%	14.0	12.5	1.5	12%
4-6	8.6	7.3	1.3	18%	4.8	4.1	0.7	16%	14.6	12.4	2.1	17%	14.9	14.8	0.1	1%
7-9	12.0	8.9	3.0	34%	7.1	7.2	-0.1	-2%	15.1	14.9	0.2	1%	16.6	16.9	-0.3	-2%
Ever Received AFDC/TANF Benefits in Year (%)																
1-3	81.3	88.2	-6.9*	-8%	86.1	88.8	-2.7**	-3%	64.2	67.9	-3.7**	-5%	69.3	74.1	-4.8***	-7%
4-6	49.1	66.3	-17.2**	-26%	58.5	63.6	-5.0**	-8%	36.0	38.1	-2.1	-6%	40.0	43.9	-3.8**	-9%
7-9	30.0	43.9	-13.9**	-32%	42.4	45.7	-3.3	-7%	24.3	25.3	-1.0	-4%	26.7	29.2	-2.5	-9%
Number of Quarters in Year on AFDC/TANF																
1-3	3.03	3.33	-0.30	-9%	3.21	3.36	-0.16***	-5%	2.02	2.26	-0.24***	-11%	2.30	2.54	-0.24***	-9%
4-6	1.75	2.48	-0.72***	-29%	2.20	2.42	-0.22**	-9%	1.19	1.31	-0.12*	-9%	1.39	1.55	-0.17**	-11%
7-9	1.09	1.56	-0.47*	-30%	1.59	1.71	-0.12	-7%	0.80	0.84	-0.05	-6%	0.92	1.03	-0.11*	-10%

NOTES: * denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.

Table 7: Differences Between Riverside and Other Counties in Annual Impacts of GAIN Cases Enrolled as AFDC-FG Full Sample

	Yrs. Since Enroll	Riverside – Alameda: *Differences in Means, Exp.*	Riverside – Alameda: *Differences in Means, Controls*	Riverside – Alameda: *Difference in Differences*	Riverside - Los Angeles: *Differences in Means, Exp.*	Riverside - Los Angeles: *Differences in Means, Controls*	Riverside - Los Angeles: *Difference in Differences*	Riverside - San Diego: *Differences in Means, Exp.*	Riverside - San Diego: *Differences in Means, Controls*	Riverside - San Diego: *Difference in Differences*
Ever Employed in Year (%)										
Unadjusted	1-3	18.2***	7.3***	10.9***	22.8***	10.9***	12.0***	3.8***	-5.5***	9.3***
	4-6	3.3*	-1.3	4.6	11.2***	7.6***	3.5*	-0.5	-4.8***	4.3**
	7-9	-6.0***	-7.4***	1.5	2.4**	4.7***	-2.3	-1.7**	-3.1*	1.4
Regression Adjusted	1-3	16.0***	3.7	12.3***	15.1***	4.4**	10.7***	7.2***	0.7	6.5***
	4-6	2.6	-2.6	5.2	5.4***	1.4	4.0	1.9*	-5.3**	7.2***
	7-9	-5.5***	-5.2*	-0.4	-3.3**	0.7	-4.0	-1.5	-3.7	2.1
Number of Quarters Employed in Year										
Unadjusted	1-3	0.53***	0.15**	0.38***	0.62***	0.22***	0.40***	0.08***	-0.20***	0.28***
	4-6	0.10	-0.04	0.14	0.36***	0.21***	0.15**	-0.04	-0.19***	0.16**
	7-9	-0.24***	-0.27***	0.04	0.07**	0.12*	-0.05	-0.09***	-0.13*	0.04
Regression Adjusted	1-3	0.43***	0.00	0.43***	0.39***	0.01	0.38***	0.20***	-0.05	0.25***
	4-6	0.09	-0.15	0.24*	0.18***	-0.01	0.19**	0.06*	-0.22***	0.28***
	7-9	-0.20***	-0.20*	-0.01	-0.11**	-0.04	-0.07	-0.07*	-0.18**	0.11
Annual Earnings (1999$)										
Unadjusted	1-3	$1,335***	$403	$931**	$1,825***	$403*	$1,422***	-$113	-$912***	$799***
	4-6	$294	-$141	$435	$1,748***	$709**	$1,040***	-$486***	-$1,114***	$628
	7-9	-$1,285***	-$1,032**	-$254	$896***	$788**	$109	-$809***	-$774*	-$35
Regression Adjusted	1-3	$983***	$107	$876**	$892***	-$272	$1,164***	$533***	-$521*	$1,054***
	4-6	$298	$88	$210	$696***	-$21	$717	$244	-$1,110**	$1,354***
	7-9	-$1,096**	-$185	-$911	-$309	-$8	-$301	-$352	-$1,040**	$688
Annual Earnings above FT-Min Wage Earnings (%)										
Unadjusted	1-3	3.8***	0.7	3.1**	4.9***	0.5	4.5***	-1.0**	-3.4***	2.4**
	4-6	1.7	-0.8	2.6	5.6***	1.5	4.0***	-1.6***	-4.2***	2.6*
	7-9	-4.4***	-3.5**	-0.9	3.3***	2.4*	0.8	-2.7***	-2.3	-0.5
Regression Adjusted	1-3	2.8***	0.0	2.8	1.6**	-1.6	3.2**	1.2**	-2.2*	3.4**
	4-6	2.3	-1.0	3.2	2.3***	-1.3	3.6**	0.9	-3.7**	4.6***
	7-9	-3.8**	-1.8	-2.0	-0.4	-0.9	0.4	-1.4*	-2.7	1.3
Ever Received AFDC/TANF Benefits in Year (%)										
Unadjusted	1-3	-14.3***	-10.2***	-4.2**	-13.5***	-10.1***	-3.4**	-2.3***	1.0	-3.3**
	4-6	-16.4***	-18.6***	2.1	-13.8***	-12.8***	-1.1	-1.4*	0.1	-1.6
	7-9	-12.0***	-11.4***	-0.6	-8.1***	-7.2***	-0.9	-0.9	-0.4	-0.5
Regression Adjusted	1-3	-6.0***	-0.8	-5.2**	-5.1***	0.3	-5.4***	-6.0***	-0.6	-5.3***
	4-6	-7.6***	-7.7**	0.1	-7.2***	-2.4	-4.8*	-6.3***	-0.4	-5.9**
	7-9	-4.7**	-5.3*	0.6	-3.8***	-2.8	-1.0	-4.5***	-0.4	-4.0*
Number of Quarters in Year on AFDC/TANF										
Unadjusted	1-3	-0.75***	-0.62***	-0.14	-0.74***	-0.59***	-0.15**	-0.15***	0.02	-0.17***
	4-6	-0.71***	-0.82***	0.11	-0.58***	-0.56***	-0.02	-0.10***	-0.01	-0.09
	7-9	-0.49***	-0.41***	-0.08	-0.35***	-0.31***	-0.04	-0.07**	-0.02	-0.05
Regression Adjusted	1-3	-0.39***	-0.17*	-0.22**	-0.33***	-0.07	-0.26***	-0.30***	-0.04	-0.27***
	4-6	-0.38***	-0.40***	0.02	-0.34***	-0.16*	-0.18*	-0.28***	-0.04	-0.24***
	7-9	-0.23***	-0.19*	-0.04	-0.19***	-0.14*	-0.04	-0.19***	-0.01	-0.17**

* denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.

Table 8: Differences Between Riverside and Other Counties in Annual Impacts of GAIN Cases Enrolled as AFDC-U Full Sample

		Riverside – Alameda			Riverside - Los Angeles			Riverside - San Diego		
	Yrs. Since Enroll	*Differences in Means, Exp.*	*Differences in Means, Controls*	*Difference in Differences*	*Differences in Means, Exp.*	*Differences in Means, Controls*	*Difference in Differences*	*Differences in Means, Exp.*	*Differences in Means, Controls*	*Difference in Differences*
Ever Employed in Year (%)										
Unadjusted	1-3	22.1***	25.2***	-3.1	14.6***	15.9***	-1.3	1.6	-1.3	2.9
	4-6	11.8***	15.1***	-3.3	3.7*	7.4***	-3.7	-1.5	-4.6**	3.1
	7-9	0.9	4.3	-3.4	-1.8	-1.8	0.0	-4.6***	-5.8***	1.2
Regression Adjusted	1-3	3.1	5.3	-2.3	1.9	3.7	-1.8	6.0***	0.6	5.4*
	4-6	-3.7	1.7	-5.4	-6.7**	-2.6	-4.1	0.6	-3.8	4.4
	7-9	-10.5**	-5.2	-5.4	-13.0***	-4.7	-8.3*	-5.2***	-9.1***	3.9
Number of Quarters Employed in Year										
Unadjusted	1-3	0.62***	0.55***	0.07	0.24***	0.26***	-0.03	-0.04	-0.12*	0.08
	4-6	0.25	0.41**	-0.16	-0.03	0.15*	-0.17*	-0.12**	-0.21***	0.10
	7-9	0.03	0.12	-0.09	-0.17**	-0.14*	-0.03	-0.20***	-0.21***	0.01
Regression Adjusted	1-3	0.07	0.10	-0.03	-0.08	0.05	-0.13	0.12**	-0.03	0.15
	4-6	-0.21	0.03	-0.25	-0.33***	-0.11	-0.22	-0.04	-0.19*	0.14
	7-9	-0.32*	-0.13	-0.19	-0.50***	-0.18	-0.33*	-0.22***	-0.35***	0.13
Annual Earnings (1999$)										
Unadjusted	1-3	$3,176***	$2,294***	$883	$2,741***	$2,098***	$643	$18	-$534	$552
	4-6	$1,561*	$1,675*	-$114	$2,081***	$1,887***	$194	-$394	-$900**	$506
	7-9	$226	$1,117	-$891	$1,416***	$1,190***	$226	-$1,054***	-$1,220***	$166
Regression Adjusted	1-3	$900	$322	$577	$511	-$101	$611	$573**	-$310	$883*
	4-6	-$612	$527	-$1,139	-$465	-$102	-$363	-$26	-$786	$760
	7-9	-$1,653*	-$398	-$1,255	-$1,232**	-$636	-$596	-$1,284***	-$1,760***	$476
Annual Earnings above FT-Min Wage Earnings (%)										
Unadjusted	1-3	10.3***	6.7**	3.6	11.1***	7.9***	3.2**	0.0	-1.7	1.7
	4-6	6.0*	5.1	0.9	9.8***	8.3***	1.5	-0.3	-2.3	2.0
	7-9	3.1	6.0	-2.8	8.0***	7.7***	0.3	-1.5	-2.0	0.6
Regression Adjusted	1-3	1.7	1.6	0.1	2.8*	-0.3	3.1	1.5	-1.8	3.3
	4-6	-2.3	0.2	-2.5	0.6	-0.3	1.0	0.6	-3.8*	4.4*
	7-9	-2.9	-1.1	-1.8	-1.4	-1.4	0.0	-3.0**	-4.6**	1.6
Ever Received AFDC/TANF Benefits in Year (%)										
Unadjusted	1-3	-17.1***	-20.3***	3.3	-21.9***	-20.9***	-1.0	-5.1***	-6.2***	1.2
	4-6	-13.1***	-28.1***	15.1**	-22.5***	-25.4***	2.9	-4.0***	-5.7**	1.7
	7-9	-5.7	-18.6***	12.9**	-18.2***	-20.4***	2.3	-2.4*	-3.9*	1.5
Regression Adjusted	1-3	-5.5	-3.4	-2.1	-8.6***	-0.7	-7.9**	-6.0***	-7.1***	1.0
	4-6	-1.5	-13.0**	11.6	-9.3***	-4.5	-4.8	-4.0**	-5.0*	1.0
	7-9	3.2	-9.5*	12.8*	-8.7***	-6.7*	-2.0	-1.2	-0.3	-0.9
Number of Quarters in Year on AFDC/TANF										
Unadjusted	1-3	-1.01***	-1.07***	0.06	-1.19***	-1.11***	-0.08	-0.29***	-0.29***	0.00
	4-6	-0.56***	-1.16***	0.60**	-1.01***	-1.11***	0.10	-0.19***	-0.24***	0.05
	7-9	-0.30*	-0.72***	0.42*	-0.79***	-0.86***	0.07	-0.13***	-0.18**	0.06
Regression Adjusted	1-3	-0.34**	-0.17	-0.17	-0.43***	-0.06	-0.37**	-0.29***	-0.26***	-0.03
	4-6	-0.05	-0.58***	0.53*	-0.43***	-0.30*	-0.13	-0.17***	-0.22**	0.05
	7-9	0.08	-0.36*	0.45*	-0.39***	-0.33**	-0.06	-0.07	-0.04	-0.03

* denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.

APPENDIX

Table A1: Experimental Estimates of Annual Impacts of GAIN, Cases Enrolled in GAIN as AFDC-FG

Panel A: "In Need of Basic Education" Subsample

Yrs. after Enroll.	Alameda				Los Angeles				Riverside				San Diego			
	Experi-mental	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*
Ever Employed in Year (%)																
1-3	26.3	23.9	2.4	10%	23.8	22.4	1.3	6%	43.3	32.5	__*10.8*__***	33%	38.4	35.6	2.8	8%
4-6	30.9	29.4	1.5	5%	27.0	23.5	3.5***	15%	34.8	28.6	6.2***	22%	34.4	33.6	__*0.8*__	2%
7-9	39.7	41.1	-1.4	-4%	34.6	31.3	3.3**	11%	34.3	32.9	1.4	4%	36.2	37.0	-0.9	-2%
Number of Quarters Employed in Year																
1-3	0.64	0.60	0.04	6%	0.63	0.60	0.03	4%	1.13	0.79	_0.34_***	43%	1.02	0.93	__*0.09*__	9%
4-6	0.91	0.83	0.08	10%	0.80	0.70	0.10**	14%	1.02	0.77	0.25***	33%	1.02	0.99	0.03	3%
7-9	1.27	1.26	0.01	0%	1.08	0.97	0.12**	12%	1.04	0.95	0.09	9%	1.12	1.10	0.02	2%
Annual Earnings (1999$)																
1-3	$1,666	$1,402	$264	19%	$1,469	$1,506	-$37	-2%	$2,753	$1,581	$1,172***	74%	$2,523	$2,284	__*$239*__	10%
4-6	$2,718	$2,261	$458	20%	$2,144	$2,086	$59	3%	$3,073	$1,925	$1,148***	60%	$3,025	$2,932	__*$93*__	3%
7-9	$4,095	$3,802	$293	8%	$3,089	$2,874	$215	7%	$3,124	$2,806	$318	11%	$3,447	$3,318	$129	4%
Earnings above Full-Time Min. Wage (%)																
1-3	3.8	4.1	__*-0.4*__	-9%	4.0	4.6	-0.5	-12%	7.0	3.5	3.5***	99%	6.8	6.3	__*0.5*__	9%
4-6	7.6	7.4	0.2	2%	6.9	7.1	-0.2	-3%	9.9	5.3	4.6***	87%	9.7	9.5	__*0.3*__	3%
7-9	14.1	13.1	0.9	7%	9.8	9.7	0.1	1%	10.2	8.5	1.7	20%	11.6	10.0	1.6	16%
Ever Received AFDC/TANF Benefits in Year (%)																
1-3	87.4	88.4	-1.0	-1%	86.0	88.0	-2.0**	-2%	72.8	78.5	-5.6***	-7%	76.3	79.4	-3.1**	-4%
4-6	58.8	64.3	-5.5*	-9%	54.9	58.3	-3.4**	-6%	42.4	48.6	-6.2***	-13%	46.5	50.9	-4.4**	-9%
7-9	38.6	42.2	-3.6	-8%	33.9	36.4	-2.5	-7%	27.2	31.1	-3.9**	-13%	30.2	33.5	-3.3*	-10%
Number of Quarters in Year on AFDC/TANF																
1-3	3.19	3.36	-0.17**	-5%	3.18	3.32	-0.14***	-4%	2.45	2.74	-0.29***	-11%	2.67	2.85	-0.19***	-6%
4-6	2.14	2.43	-0.29**	-12%	1.97	2.16	-0.20***	-9%	1.44	1.69	-0.25***	-15%	1.65	1.80	-0.16**	-9%
7-9	1.38	1.49	-0.11	-7%	1.20	1.31	-0.11*	-8%	0.90	1.06	-0.17**	-16%	1.05	1.16	-0.11	-9%

NOTES: * denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.
Italicized entries indicate means that are significantly different from those for the "Not In Need of Basic Education" subsample, at significance levels at or lower than 10% level.

Table A1: (Continued)

Panel B: "Not In Need of Basic Education" Subsample

Yrs. after Enroll.	Alameda				Los Angeles				Riverside				San Diego			
	Experi-mental	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*
Ever Employed in Year (%)																
1-3	39.1	35.9	3.2	9%	35.9	32.8	3.1	9%	57.5	39.4	***18.1****	46%	53.8	47.1	***6.7****	14%
4-6	48.4	44.8	3.6	8%	38.2	35.5	2.6	7%	48.7	40.5	8.2***	20%	49.2	43.9	5.3**	12%
7-9	55.8	53.1	2.6	5%	46.3	40.6	5.7*	14%	47.0	45.0	1.9	4%	47.4	45.7	1.7	4%
Number of Quarters Employed in Year																
1-3	1.11	1.03	0.08	8%	1.04	0.95	0.08	9%	1.63	1.05	***0.58****	55%	1.54	1.29	0.25***	19%
4-6	1.52	1.38	0.13	10%	1.18	1.07	0.11	10%	1.55	1.30	0.25***	19%	1.57	1.40	0.18**	13%
7-9	1.84	1.73	0.12	7%	1.49	1.31	0.19	14%	1.53	1.44	0.09	6%	1.59	1.50	0.09	6%
Annual Earnings (1999$)																
1-3	$3,581	$2,701	$880	33%	$3,400	$3,274	$126	4%	$5,056	$3,236	$1,820***	56%	$5,406	$4,238	***$1,168****	28%
4-6	$6,595	$5,400	$1,195	22%	$4,569	$4,181	$389	9%	$6,321	$5,070	$1,251**	25%	$7,206	$6,002	***$1,204****	20%
7-9	$9,192	$7,877	$1,315	17%	$6,183	$5,512	$671	12%	$6,802	$6,178	$624	10%	$7,910	$6,935	$975*	14%
Earnings above Full-Time Min. Wage (%)																
1-3	11.9	7.9	***4.0****	50%	11.2	10.2	1.1	11%	15.5	10.0	5.5***	55%	17.4	13.7	***3.7****	27%
4-6	22.0	17.2	4.7	27%	16.7	14.1	2.6	18%	21.1	16.9	4.2**	25%	24.0	20.0	***4.0****	20%
7-9	29.5	24.8	4.7	19%	20.2	17.6	2.5	14%	22.5	21.2	1.2	6%	25.8	23.1	2.6	11%
Ever Received AFDC/TANF Benefits in Year (%)																
1-3	83.3	86.2	-2.8	-3%	81.6	85.8	-4.2**	-5%	69.9	76.0	-6.1***	-8%	70.9	72.9	-2.0	-3%
4-6	51.6	61.2	-9.6**	-16%	48.7	53.7	-5.0	-9%	35.9	38.9	-2.9	-8%	34.5	36.7	-2.2	-6%
7-9	34.6	35.3	-0.7	-2%	31.4	32.6	-1.2	-4%	22.3	24.6	-2.2	-9%	21.0	23.2	-2.2	-10%
Number of Quarters in Year on AFDC/TANF																
1-3	3.05	3.25	-0.20*	-6%	2.93	3.22	-0.29***	-9%	2.29	2.65	-0.36***	-14%	2.37	2.48	-0.11*	-5%
4-6	1.87	2.27	-0.40**	-17%	1.73	1.95	-0.22*	-11%	1.20	1.36	-0.17*	-12%	1.17	1.28	-0.11	-8%
7-9	1.21	1.17	0.03	3%	1.09	1.15	-0.07	-6%	0.72	0.83	-0.11	-13%	0.70	0.78	-0.09	-11%

NOTES: * denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.
Italicized entries indicate means that are significantly different from those for the "In Need of Basic Education" subsample, at significance levels at or lower than 10% level.

Table A2: Experimental Estimates of Annual Impacts of GAIN, Cases Enrolled in GAIN as AFDC-U

Panel A: "In Need of Basic Education" Subsample

	Alameda				Los Angeles				Riverside				San Diego			
Yrs. after Enroll.	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*
Ever Employed in Year (%)																
1-3	26.3	20.2	***6.1***	30%	37.9	29.0	8.9***	31%	50.5	42.3	8.2***	20%	49.4	42.6	6.9***	16%
4-6	23.9	19.2	4.8	25%	36.7	28.2	8.5***	30%	39.5	35.0	4.6**	13%	40.8	37.7	3.1	8%
7-9	29.1	31.3	***-2.2***	-7%	38.1	37.6	0.4	1%	35.7	36.6	-0.9	-2%	41.2	41.0	0.3	1%
Number of Quarters Employed in Year																
1-3	0.69	0.64	0.04	7%	1.17	0.93	0.25***	27%	1.33	1.12	0.21***	19%	1.40	1.23	0.17**	14%
4-6	0.79	0.57	0.22	38%	1.19	0.89	0.30***	34%	1.12	1.01	0.12	12%	1.26	1.14	***0.12***	11%
7-9	0.92	1.01	-0.09	-9%	1.28	1.24	0.04	4%	1.07	1.13	-0.06	-5%	1.31	1.29	0.01	1%
Annual Earnings (1999$)																
1-3	$992	$1,607	***-$615***	-38%	$1,870	$1,570	$300	19%	$4,073	$3,478	$595	17%	$3,748	$3,643	$106	3%
4-6	$2,377	$2,276	$101	4%	$2,239	$1,789	$450*	25%	$3,862	$3,486	$376	11%	$4,053	$3,509	***$544***	15%
7-9	$3,273	$3,410	***-$137***	-4%	$2,925	$2,848	***$77***	3%	$3,887	$4,080	-$193	-5%	$4,667	$4,440	$226	5%
Earnings above Full-Time Min. Wage (%)																
1-3	1.4	5.1	***-3.6***	-72%	2.7	2.4	0.3	12%	11.8	10.1	***1.6***	16%	10.3	10.0	0.3	3%
4-6	7.0	8.1	-1.0	-13%	4.5	3.5	0.9	26%	12.6	10.8	1.9	17%	12.2	9.7	***2.5****	26%
7-9	8.5	9.6	***-1.1***	-12%	6.8	6.3	0.5	8%	13.4	13.9	-0.4	-3%	14.3	13.4	0.9	7%
Ever Received AFDC/TANF Benefits in Year (%)																
1-3	82.2	87.4	-5.2	-6%	86.5	89.0	-2.5*	-3%	65.7	69.2	-3.5*	-5%	73.6	78.9	-5.3***	-7%
4-6	50.2	64.1	-13.9*	-22%	59.0	65.1	-6.1**	-9%	38.3	40.2	-1.9	-5%	45.2	52.0	***-6.7*****	-13%
7-9	33.3	42.9	-9.6	-22%	42.4	47.7	***-5.3*****	-11%	26.7	26.8	-0.1	0%	30.9	34.6	-3.7*	-11%
Number of Quarters in Year on AFDC/TANF																
1-3	3.09	3.34	-0.24	-7%	3.23	3.38	-0.15**	-4%	2.12	2.32	-0.21***	-9%	2.52	2.77	-0.25***	-9%
4-6	1.83	2.43	-0.60*	-25%	2.22	2.48	-0.27***	-11%	1.29	1.39	-0.10	-8%	1.59	1.89	***-0.29******	-15%
7-9	1.25	1.57	-0.32	-20%	1.59	1.78	***-0.19****	-11%	0.89	0.91	-0.01	-1%	1.09	1.24	-0.15*	-12%

NOTES: * denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.
Italicized entries indicate means that are significantly different from those for the "Not In Need of Basic Education" subsample, at significance levels at or lower than 10% level.

Table A2: (Continued)

Panel B: "Not In Need of Basic Education" Subsample

Yrs. after Enroll.	Alameda				Los Angeles				Riverside				San Diego			
	Experi-mental	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*	*Experi-mental*	*Control*	*Difference*	*% Dif-ference*
Ever Employed in Year (%)																
1-3	50.0	16.7	***33.3****	200%	47.9	26.2	21.8***	83%	58.5	49.5	9.0***	18%	55.4	51.4	4.0	8%
4-6	48.1	29.2	19.0	65%	41.7	35.9	5.8	16%	43.0	38.9	4.1	11%	44.5	45.8	-1.3	-3%
7-9	61.1	35.4	***25.7****	73%	41.0	44.1	-3.1	-7%	38.0	36.1	1.9	5%	40.8	44.2	-3.4	-8%
Number of Quarters Employed in Year																
1-3	1.26	0.54	0.72*	132%	1.38	0.76	0.61**	80%	1.61	1.27	0.33***	26%	1.57	1.40	0.16*	12%
4-6	1.46	0.92	0.55	60%	1.35	0.99	0.35	35%	1.27	1.12	0.15	13%	1.34	1.46	***-0.12***	-8%
7-9	1.72	0.98	0.74	76%	1.19	1.39	-0.20	-15%	1.19	1.10	0.09	8%	1.32	1.39	-0.08	-6%
Annual Earnings (1999$)																
1-3	$3,487	$765	***$2,722*****	356%	$2,820	$2,335	$485	21%	$5,860	$4,248	$1,612***	38%	$6,238	$5,264	$974*	19%
4-6	$4,649	$1,798	$2,851	159%	$3,423	$3,800	-$377	-10%	$5,459	$4,593	$866	19%	$6,084	$6,735	***-$651***	-10%
7-9	$7,555	$2,357	***$5,199****	221%	$3,301	$6,004	***-$2,703***	-45%	$5,314	$4,802	$512	11%	$6,737	$7,289	-$553	-8%
Earnings above Full-Time Min. Wage (%)																
1-3	13.0	0.0	***13.0*****	NA	6.3	7.7	-1.4	-19%	18.5	12.1	***6.4******	53%	20.5	16.5	3.9*	24%
4-6	14.8	4.2	10.6	256%	9.0	9.7	-0.7	-7%	18.4	15.8	2.7	17%	19.6	22.8	***-3.2***	-14%
7-9	25.9	6.3	***19.7****	315%	10.4	15.9	-5.5	-34%	18.4	17.0	1.4	8%	20.6	22.5	-1.9	-9%
Ever Received AFDC/TANF Benefits in Year (%)																
1-3	77.8	91.7	-13.9*	-15%	80.6	86.7	-6.1	-7%	61.2	65.3	-4.1	-6%	61.6	66.5	-4.8**	-7%
4-6	44.4	75.0	-30.6**	-41%	52.1	47.7	4.4	9%	31.4	34.0	-2.6	-8%	30.9	31.1	***-0.1***	0%
7-9	16.7	47.9	-31.3**	-65%	43.1	26.2	***16.9*****	65%	19.5	22.3	-2.8	-13%	19.4	20.6	-1.3	-6%
Number of Quarters in Year on AFDC/TANF																
1-3	2.78	3.27	-0.49	-15%	2.85	3.20	-0.35	-11%	1.81	2.12	-0.30***	-14%	1.92	2.19	-0.27***	-12%
4-6	1.44	2.65	-1.20**	-45%	1.99	1.77	0.21	12%	1.01	1.15	-0.15	-13%	1.02	1.02	***0.00***	0%
7-9	0.46	1.52	-1.06**	-70%	1.58	0.95	***0.63*****	67%	0.61	0.73	-0.11	-16%	0.63	0.69	-0.07	-10%

NOTES: * denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.
Italicized entries indicate means that are significantly different from those for the "In Need of Basic Education" subsample, at significance levels at or lower than 10% level.

Table A3: Differences Between Riverside and Other Counties in Annual Impacts of GAIN Cases Enrolled as AFDC-FG

Panel A: "In Need of Basic Education" Subsample

		Riverside – Alameda			**Riverside - Los Angeles**			**Riverside - San Diego**		
	Yrs. Since Enroll	***Differences in Means, Exp.***	***Differences in Means, Controls***	***Difference in Differences***	***Differences in Means, Exp.***	***Differences in Means, Controls***	***Difference in Differences***	***Differences in Means, Exp.***	***Differences in Means, Controls***	***Difference in Differences***
Ever Employed in Year (%)										
Unadjusted	1-3	17.0***	8.6***	8.4***	19.6***	10.1***	9.5***	4.9***	-3.1	8.0***
	4-6	3.9*	-0.8	4.7	7.8***	5.1***	2.7	0.4	-5.0**	5.4**
	7-9	-5.4**	-8.2***	2.8	-0.4	1.6	-1.9	-1.9*	-4.1*	2.2
Regression Adjusted	1-3	16.3***	2.7	13.6***	14.1***	4.6*	9.4***	7.6***	2.1	5.5**
	4-6	3.3	-4.9	8.2*	3.6***	1.6	2.0	2.1*	-7.5***	9.6***
	7-9	-4.6*	-8.2**	3.5	-4.9***	-1.9	-3.0	-2.3***	-6.3**	4.0
Number of Quarters Employed in Year										
Unadjusted	1-3	0.49***	0.19**	0.31***	0.50***	0.19***	0.32***	0.11	-0.14**	0.25***
	4-6	0.11	-0.07	0.17	0.22***	0.07	0.16**	0.00	-0.22***	0.22**
	7-9	-0.23***	-0.31***	0.08	-0.04	-0.01	-0.03	-0.08	-0.15*	0.07
Regression Adjusted	1-3	0.44***	-0.01	0.45***	0.34***	0.04	0.30***	0.20	-0.01	0.21**
	4-6	0.10	-0.27**	0.37**	0.11**	-0.05	0.16	0.05	-0.29***	0.34***
	7-9	-0.19**	-0.33**	0.13	-0.18***	-0.12	-0.06	-0.08	-0.23**	0.14
Annual Earnings (1999$)										
Unadjusted	1-3	$1,087***	$179	$908**	$1,284***	$75	$1,210***	$230*	-$703**	$933***
	4-6	$355	-$336	$690	$928***	-$161	$1,089***	$47	-$1,007***	$1,054***
	7-9	-$970***	-$996**	$26	$36	-$68	$104	-$323**	-$512	$189
Regression Adjusted	1-3	$1,000***	-$392	$1,392***	$793***	-$302	$1,095***	$634***	-$460	$1,093***
	4-6	$315	-$1,085**	$1,401**	$299	-$718*	$1,018**	$343*	-$1,170***	$1,513***
	7-9	-$985**	-$1,219**	$234	-$751***	-$762*	$11	-$278	-$892**	$614
Annual Earnings above FT-Min Wage Earnings (%)										
Unadjusted	1-3	3.2***	-0.6	3.9**	3.0***	-1.1	4.0***	0.2	-2.8**	2.9**
	4-6	2.3*	-2.2	4.4**	3.0***	-1.8	4.8***	0.2	-4.2***	4.3***
	7-9	-3.9***	-4.6***	0.7	0.4	-1.2	1.6	-1.5**	-1.6	0.1
Regression Adjusted	1-3	2.7**	-2.0	4.6**	1.2**	-2.1*	3.2**	1.3**	-2.7**	4.0***
	4-6	3.1**	-4.7**	7.7***	1.2	-3.7**	5.0***	1.4*	-4.2**	5.6***
	7-9	-3.3*	-5.5**	2.2	-1.8*	-3.3*	1.5	-1.4*	-2.5	1.0
Ever Received AFDC/TANF Benefits in Year (%)										
Unadjusted	1-3	-14.6***	-9.9***	-4.6*	-13.2***	-9.6***	-3.6**	-3.5***	-0.9	-2.5
	4-6	-16.4***	-15.7***	-0.7	-12.5***	-9.7***	-2.8	-4.1***	-2.3	-1.8
	7-9	-11.4***	-11.1***	-0.3	-6.7***	-5.3**	-1.4	-3.0***	-2.4	-0.6
Regression Adjusted	1-3	-6.1***	-0.7	-5.4*	-4.9***	-0.4	-4.6**	-6.7***	-2.7	-3.9*
	4-6	-6.7**	-4.1	-2.5	-7.6***	-2.1	-5.5	-7.9***	-3.5	-4.4
	7-9	-3.9	-6.0	2.1	-4.0***	-4.5	0.6	-6.4***	-4.6	-1.9
Number of Quarters in Year on AFDC/TANF										
Unadjusted	1-3	-0.74***	-0.62***	-0.12	-0.72***	-0.57***	-0.15**	-0.21***	-0.11	-0.10
	4-6	-0.70***	-0.74***	0.04	-0.53***	-0.47***	-0.06	-0.21***	-0.11	-0.09
	7-9	-0.48***	-0.42***	-0.06	-0.30***	-0.24***	-0.06	-0.15***	-0.10	-0.06
Regression Adjusted	1-3	-0.38***	-0.18	-0.20	-0.33***	-0.11	-0.22**	-0.33***	-0.19**	-0.14
	4-6	-0.35***	-0.31**	-0.04	-0.34***	-0.19*	-0.15	-0.36***	-0.19*	-0.17
	7-9	-0.21**	-0.21	0.00	-0.19***	-0.20*	0.00	-0.26***	-0.14	-0.12

* denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.

Table A3: (Continued)

Panel B: "Not In Need of Basic Education" Subsample

	Yrs. Since Enroll	Riverside – Alameda			Riverside - Los Angeles			Riverside - San Diego		
		Differences in Means, Exp.	*Differences in Means, Controls*	*Difference in Differences*	*Differences in Means, Exp.*	*Differences in Means, Controls*	*Difference in Differences*	*Differences in Means, Exp.*	*Differences in Means, Controls*	*Difference in Differences*
Ever Employed in Year (%)										
Unadjusted	1-3	18.4***	3.5	14.9***	21.6***	6.6**	15.0***	3.7***	-7.7***	11.4***
	4-6	0.3	-4.2	4.6	10.6***	5.0	5.6	-0.4	-3.4	3.0
	7-9	-8.8***	-8.1**	-0.7	0.7	4.5	-3.8	-0.4	-0.7	0.3
Regression Adjusted	1-3	16.0***	3.2**	12.8**	19.0***	3.5	15.5***	6.8***	-3.0	9.8***
	4-6	1.3	-2.5	3.8	9.8***	0.8	9.0	2.1	-3.7	5.8
	7-9	-7.1*	-2.4	-4.7	0.6	6.7	-6.1	0.1	-2.4	2.5
Number of Quarters Employed in Year										
Unadjusted	1-3	0.52***	0.02	0.50***	0.59***	0.10	0.49***	0.09**	-0.24***	0.33***
	4-6	0.03	-0.09	0.12	0.36***	0.22*	0.14	-0.03	-0.10	0.07
	7-9	-0.31**	-0.29**	-0.02	0.04	0.13	-0.09	-0.06	-0.06	0.01
Regression Adjusted	1-3	0.42***	-0.06	0.48**	0.55***	-0.05	0.60***	0.22***	-0.15	0.36***
	4-6	0.09	-0.06	0.15	0.36***	0.10	0.26	0.09	-0.17	0.26*
	7-9	-0.22	-0.08	-0.15	0.07	0.16	-0.08	-0.02	-0.19	0.17
Annual Earnings (1999$)										
Unadjusted	1-3	$1,475***	$535	$940	$1,656***	-$38	$1,693***	-$350	-$1,002**	$652
	4-6	-$274	-$330	$56	$1,752***	$889	$862	-$885***	-$932	$47
	7-9	-$2,390***	-$1,700*	-$691	$619	$666	-$47	-$1,108***	-$758	-$351
Regression Adjusted	1-3	$878*	$419	$459	$1,170***	-$545	$1,715**	$501*	-$839	$1,340**
	4-6	-$186	$1,079	-$1,265	$1,371**	$1,263	$108	$275	-$1,340	$1,615*
	7-9	-$1,833*	$641	-$2,474	$435	$1,321	-$886	-$235	-$1,514	$1,279
Annual Earnings above FT-Min Wage Earnings (%)										
Unadjusted	1-3	3.6*	2.1	1.5	4.3***	-0.1	4.4*	-1.9**	-3.6*	1.8
	4-6	-0.8	-0.3	-0.5	4.4***	2.8	1.6	-2.9***	-3.1	0.2
	7-9	-7.0**	-3.6	-3.5	2.3	3.6	-1.3	-3.3***	-1.9	-1.4
Regression Adjusted	1-3	2.5	1.8	0.6	3.0*	-1.6	4.5	1.4	-1.9	3.3
	4-6	-0.5	2.1	-2.5	4.2*	2.7	1.4	0.8	-4.0	4.8
	7-9	-6.1*	0.4	-6.6	2.1	2.5	-0.4	-0.7	-3.9	3.2
Ever Received AFDC/TANF Benefits in Year (%)										
Unadjusted	1-3	-13.5***	-10.2***	-3.3	-11.7***	-9.8***	-1.9	-1.0	3.1	-4.1*
	4-6	-15.7***	-22.3***	6.7	-12.8***	-14.8***	2.0	1.4	2.1	-0.7
	7-9	-12.3***	-10.7***	-1.6	-9.1***	-8.0***	-1.0	1.4	1.4	0.0
Regression Adjusted	1-3	-5.9**	-2.8	-3.1	-4.5**	1.1	-5.6	-5.2***	2.7	-7.9***
	4-6	-8.5**	-14.0***	5.5	-4.8*	-4.1	-0.7	-4.5***	5.6	-10.1***
	7-9	-5.9*	-3.4	-2.6	-1.8	3.1	-4.9	-2.1	5.8*	-7.9**
Number of Quarters in Year on AFDC/TANF										
Unadjusted	1-3	-0.76***	-0.60***	-0.17	-0.64***	-0.57***	-0.07	-0.08*	0.17**	-0.25***
	4-6	-0.68***	-0.91***	0.23	-0.53***	-0.58***	0.05	0.02	0.08	-0.06
	7-9	-0.48***	-0.34***	-0.14	-0.36***	-0.33***	-0.04	0.03	0.04	-0.02
Regression Adjusted	1-3	-0.42***	-0.24	-0.18	-0.31***	-0.02	-0.29*	-0.27***	0.20*	-0.46***
	4-6	-0.42***	-0.57***	0.15	-0.26***	-0.10	-0.16	-0.20***	0.22*	-0.42***
	7-9	-0.24**	-0.11	-0.13	-0.10	0.05	-0.15	-0.09*	0.18*	-0.27**

* denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.

Table A4: Differences Between Riverside and Other Counties in Annual Impacts of GAIN Cases Enrolled as AFDC-U

Panel A: "In Need of Basic Education" Subsample

	Yrs. Since Enroll	Riverside – Alameda			Riverside - Los Angeles			Riverside - San Diego		
		Differences in Means, Exp.	*Differences in Means, Controls*	*Difference in Differences*	*Differences in Means, Exp.*	*Differences in Means, Controls*	*Difference in Differences*	*Differences in Means, Exp.*	*Differences in Means, Controls*	*Difference in Differences*
Ever Employed in Year (%)										
Unadjusted	1-3	24.2***	22.1***	2.2	12.6***	13.2***	-0.7	1.1	-0.3	1.4
	4-6	15.6***	15.8***	-0.2	2.8	6.7***	-3.9	-1.3	-2.7	1.4
	7-9	6.6	5.3	1.3	-2.4	-1.1	-1.3	-5.5***	-4.4	-1.1
Regression Adjusted	1-3	1.4	3.0	-1.6	-0.2	3.9	-4.1	5.3***	3.8	1.5
	4-6	-3.1	6.0	-9.0	-8.5***	3.1	-11.7**	1.6	0.2	1.3
	7-9	-8.0	-7.8	-0.2	-14.6***	-2.7	-11.9**	-6.8***	-8.1**	1.3
Number of Quarters Employed in Year										
Unadjusted	1-3	0.65***	0.48***	0.16	0.16**	0.20**	-0.04	-0.07	-0.10	0.04
	4-6	0.34**	0.44**	-0.10	-0.06	0.12	-0.18	-0.14**	-0.13	-0.01
	7-9	0.16	0.12	0.03	-0.21***	-0.11	-0.10	-0.23***	-0.16*	-0.07
Regression Adjusted	1-3	-0.01	0.08	-0.09	-0.15*	0.11	-0.26	0.10*	0.09	0.01
	4-6	-0.23	0.14	-0.37	-0.38***	0.04	-0.41**	-0.03	-0.01	-0.02
	7-9	-0.29	-0.28	-0.01	-0.58***	-0.12	-0.46**	-0.27***	-0.31**	0.03
Annual Earnings (1999$)										
Unadjusted	1-3	$3,081***	$1,871**	$1,210	$2,203***	$1,908***	$295	$325	-$165	$490
	4-6	$1,484*	$1,210	$274	$1,623***	$1,697***	-$74	-$192	-$24	-$168
	7-9	$613	$669	-$56	$961***	$1,232***	-$270	-$780**	-$361	-$420
Regression Adjusted	1-3	$545	$173	$372	-$68	$239	-$307	$906***	$201	$704
	4-6	-$1,135	$312	-$1,447	-$745	$366	-$1,111	$353	$174	$179
	7-9	-$1,595	-$1,426	-$169	-$1,459***	-$276	-$1,184	-$1,178***	-$1,033	-$145
Annual Earnings above FT-Min Wage Earnings (%)										
Unadjusted	1-3	10.4***	5.1	5.3	9.1***	7.7***	1.4	1.5	0.2	1.3
	4-6	5.6	2.7	2.9	8.2***	7.2***	1.0	0.4	1.1	-0.6
	7-9	5.0	4.3	0.7	6.6***	7.5***	-0.9	-0.9	0.4	-1.3
Regression Adjusted	1-3	1.4	-0.1	1.4	1.0	1.1	-0.1	2.7**	0.2	2.5
	4-6	-4.3	-0.9	-3.4	-0.2	1.1	-1.3	1.8	-0.2	2.0
	7-9	-1.7	-5.0	3.3	-2.4	-0.5	-1.9	-2.6*	-3.1	0.5
Ever Received AFDC/TANF Benefits in Year (%)										
Unadjusted	1-3	-16.5***	-18.2***	1.7	-20.8***	-19.8***	-1.0	-7.9***	-9.7***	1.8
	4-6	-11.9**	-23.9***	12.0	-20.7***	-24.9***	4.2	-6.9***	-11.7***	4.8
	7-9	-6.6	-16.1***	9.5	-15.7***	-20.9***	5.2	-4.2**	-7.8***	3.7
Regression Adjusted	1-3	-3.2	-2.0	-1.2	-7.3***	-0.6	-6.7*	-8.9***	-10.4***	1.5
	4-6	-0.1	-10.3	10.2	-8.7***	-8.1*	-0.6	-7.0***	-10.6***	3.6
	7-9	3.2	-6.0	9.3	-7.2**	-9.0*	1.8	-2.2	-4.1	2.0
Number of Quarters in Year on AFDC/TANF										
Unadjusted	1-3	-0.97***	-1.01***	0.04	-1.11***	-1.05***	-0.06	-0.40***	-0.44***	0.04
	4-6	-0.54***	-1.04***	0.50*	-0.93***	-1.09***	0.16	-0.31***	-0.49***	0.19
	7-9	-0.36**	-0.67***	0.30	-0.70***	-0.88***	0.18	-0.20***	-0.34***	0.13
Regression Adjusted	1-3	-0.23	-0.16	-0.07	-0.38***	-0.05	-0.33**	-0.40***	-0.40***	0.00
	4-6	-0.01	-0.50*	0.49	-0.39***	-0.43**	0.04	-0.29***	-0.45***	0.17
	7-9	0.06	-0.29	0.35	-0.33***	-0.41**	0.08	-0.10	-0.18	0.07

* denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.

Table A4: (Continued)

Panel B: "Not In Need of Basic Education" Subsample

	Yrs. Since Enroll	Riverside – Alameda			Riverside - Los Angeles			Riverside - San Diego		
		Differences in Means, Exp.	*Differences in Means, Controls*	*Difference in Differences*	*Differences in Means, Exp.*	*Differences in Means, Controls*	*Difference in Differences*	*Differences in Means, Exp.*	*Differences in Means, Controls*	*Difference in Differences*
Ever Employed in Year (%)										
Unadjusted	1-3	8.5***	32.8***	-24.4*	10.6*	23.4***	-12.8	3.1	-1.9	4.9
	4-6	-5.1	9.7	-14.9	1.4	3.0	-1.7	-1.5	-6.9*	5.4
	7-9	-23.1**	0.7	-23.8	-2.9	-8.0	5.0	-2.8	-8.0**	5.3
Regression Adjusted	1-3	1.8	16.0	-14.2	4.4	6.8	-2.4	6.4**	-4.1	10.5**
	4-6	-10.1	-5.7	-4.4	-1.4	-17.1*	15.7	-0.8	-12.5**	11.7**
	7-9	-19.6*	-1.9	-17.7	-6.4	-10.3	3.9	-2.2	-13.6***	11.4**
Number of Quarters Employed in Year										
Unadjusted	1-3	0.35	0.73**	-0.39	0.23	0.51***	-0.28	0.04	-0.13	0.17
	4-6	-0.19	0.20	-0.40	-0.08	0.13	-0.20	-0.07	-0.34***	0.27*
	7-9	-0.54	0.12	-0.66	0.00	-0.29	0.29	-0.13	-0.30**	0.16
Regression Adjusted	1-3	0.15	0.19	-0.04	0.03	0.03	-0.01	0.14	-0.21	0.35**
	4-6	-0.37	-0.20	-0.16	-0.20	-0.41	0.21	-0.05	-0.53***	0.48**
	7-9	-0.41	0.17	-0.58	-0.10	-0.31	0.21	-0.12	-0.53***	0.41**
Annual Earnings (1999$)										
Unadjusted	1-3	$2,373	$3,483*	-$1,110	$3,040***	$1,913*	$1,127	-$378	-$1,016	$638
	4-6	$811	$2,796	-$1,985	$2,037	$793	$1,243	-$624	-$2,142**	$1,517
	7-9	-$2,241	$2,446	-$4,687	$2,013	-$1,202	$3,215*	-$1,423**	-$2,487***	$1,064
Regression Adjusted	1-3	$1,605	$980	$625	$1,670	-$1,021	$2,691	$19	-$1,276	$1,296
	4-6	$172	$582	-$410	$404	-$1,553	$1,957	-$572	-$2,816**	$2,244*
	7-9	-$2,485	$1,174	-$3,658	$400	-$2,012	$2,412	-$1,282*	-$3,527***	$2,245
Annual Earnings above FT-Min Wage Earnings (%)										
Unadjusted	1-3	5.5	12.1*	-6.6	12.3***	4.4	7.8	-2.0	-4.4*	2.4
	4-6	3.6	11.6	-8.0	9.4*	6.0	3.4	-1.2	-7.0**	5.9*
	7-9	-7.5	10.8	-18.3	8.0	1.1	6.9	-2.1	-5.5*	3.4
Regression Adjusted	1-3	0.9	6.7	-5.8	7.0	-6.3	13.3*	-0.8	-5.9*	5.1
	4-6	1.6	2.3	-0.7	4.4	-5.1	9.5	-1.1	-10.2***	9.1**
	7-9	-8.7	6.8	-15.4	4.0	-4.1	8.0	-3.0	-9.3**	6.3
Ever Received AFDC/TANF Benefits in Year (%)										
Unadjusted	1-3	-16.6**	-26.4***	9.8	-19.3***	-21.4***	2.1	-0.4	-1.2	0.8
	4-6	-13.0	-41.0***	28.0*	-20.7***	-13.7**	-7.0	0.5	3.0	-2.5
	7-9	2.9	-25.6***	28.5**	-23.5***	-3.8	-19.7***	0.2	1.7	-1.5
Regression Adjusted	1-3	-11.7	-12.5	0.8	-9.9*	-1.8	-8.1	-1.3	-1.7	0.3
	4-6	-6.9	-28.7**	21.8	-9.5	8.2	-17.6	1.0	4.8	-3.8
	7-9	6.6	-17.2	23.9	-15.5**	4.1	-19.6*	-0.1	7.3*	-7.4
Number of Quarters in Year on AFDC/TANF										
Unadjusted	1-3	-0.96***	-1.15***	0.19	-1.04***	-1.08***	0.04	-0.11	-0.07	-0.03
	4-6	-0.43	-1.49***	1.06**	-0.98***	-0.62***	-0.36	-0.01	0.13	-0.14
	7-9	0.15	-0.80**	0.94**	-0.97***	-0.22	-0.75***	-0.01	0.03	-0.05
Regression Adjusted	1-3	-0.64*	-0.45	-0.19	-0.52**	-0.09	-0.43	-0.12	-0.02	-0.10
	4-6	-0.22	-1.09**	0.86	-0.55**	0.15	-0.70*	0.01	0.19	-0.18
	7-9	0.30	-0.44	0.74	-0.68***	0.08	-0.75**	-0.04	0.23	-0.26

* denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.