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**The Long-Term Gains from GAIN:**
**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 emphasize 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; at a lower cost per recipient.

This paper reexamines the GAIN programs from three complementary perspectives. First, we extend the earlier analysis through nine years post-randomization, and find, that the stronger impacts for Riverside County's work first program tend to shrink, and the weaker impacts for the human capital programs tend to grow. 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, allowing us to conclude that the work first programs were better than the human capital accumulation programs in the short run, but that this relative advantage disappears at longer intervals. Finally, these results suggest that—at least in this welfare context—non-experimental methods successfully recover true program effects; i.e., those found using conventional random assignment methods.

## 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. While PRWORA entailed many changes,[1] a key set of provisions directed states to reorient their welfare programs toward encouraging work, and not cash assistance, as the way 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 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 their employment rates and skill levels of welfare recipients. What has changed in the most recent welfare legislation is the relative emphasis on getting recipients into jobs quickly versus allowing for the

[1] Some of the major changes in the U.S. welfare system mandated by PRWORA, include: (a) Federal provision of funding for cash assistance and employment and training programs for welfare recipients in form of block grants to states, rather than funding that "matched" state funding for all recipients; (b) elimination of the entitlement of cash assistance from Federal government to eligible individuals; (c) 5-year limit on receipt of federally-funded cash aid by families; (d) imposition of work requirements on adult recipients after two years on assistance; (e) elimination of federal mandate to additional benefits whenever recipients had another child; (f) restrictions on eligibility for SSI and granting of state-option for provision of Medicaid, TANF funds and other assistance to non-citizen, but legal immigrants.

more deliberate acquisition of basic and vocational skills in state “welfare-to-work” programs. To achieve this objective, states have increasingly relied on “work-first,” “quick-job-entry,” or “labor force attachment” (LFA) strategies that move welfare recipients into unsubsidized employment as quickly as possible by providing them with 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.

Although the movement of states, as well as the federal government, away from basic skills and vocational training programs toward programs that encourage quick entry into jobs has been motivated by several factors, one important justification has come from the findings of several recent evaluations of various state welfare-to-work programs. One of the earliest and most prominent of these studies is the evaluation of California’s Greater Avenues to Independence (GAIN) program that was 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 a “treatment” group that was to receive services in a county based and designed “welfare-to-work” program or to a control group in which these services were denied. Counties were given considerable discretion in the types of recipients they served, as well as in the way they structured their programs. Thus, in effect, the MDRC study was an evaluation of six separate programs, 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 into GAIN, Riverside’s program increased annual employment rates and earnings by 31% and 55%, respectively, over those of non-participants and reduced annual AFDC/TANF participation rates by 10%.[2] In contrast to the

[2] See estimates in Table 3, below.

programs run in the other analysis counties that emphasized human capital acquisition (usually involving longer periods of basic education and training), Riverside emphasized a structured 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 of placing people into jobs quickly, even if at low 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 the program are measured. MDRC has produced estimates of GAIN impacts for three and five years post-enrollment period and this may be too short a period to measure the full impacts of such programs. In particular, the returns from human capital or skill development strategies may take longer to realize than those from the work-first strategies. Work-first program is likely to look good in the short-run simply because it is so participants tend to find jobs and leave welfare sooner; however since the program was minimal (perhaps100 hours of classroom time, in a period of four to six weeks),

[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 results, the Wilson administration in California pushed to have all of the state's counties adopt the Riverside work-first approach in its GAIN programs.

we might not expect these effects to last beyond the first spell of employment. In contrast, while basic skills and vocational training tend to be more expensive and take longer to complete (often a year or more); if effectively targeted, they potentially develop skills that are more "permanent" in nature and enable their recipients to obtain better and more secure jobs for longer periods of time. Simply because the treatment lasts longer, any benefits will take longer to materialize.

The second reason for caution in inferring the relative effectiveness of alternative training strategies relates to the design of the GAIN evaluation. As noted above, 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, their experimental design does not allow one to draw inferences about the *differential* impact of alternative programs—e.g, work-first versus human-capital—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 there may be across-county differences in the populations served by a particular program, or differences in labor market conditions, that compromise the ability to conclude that differences in the within-county estimates of different programs would hold for the same populations.

In this paper we present estimates of the impacts of the work-first and human capital approaches over longer periods of time than are found in the existing literature. We analyze nine years of post-enrollment data on the employment, earnings and welfare participation of the members of the experimental and control groups in four of the six counties used in the MDRC evaluation of the California GAIN program. We focus on data for Alameda, Los Angeles, Riverside and San Diego counties. We omit the other two counties included in the original MDRC evaluation, Butte and Tulare, because their rural economies are less comparable to those in the other four counties. 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.

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 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 receives 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.[6] 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-

[6] 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

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 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 prevailing 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. Subject to passage of these tests, we then estimates the across-county differences in the outcomes of subjects randomly assigned to receive GAIN services in order to estimate the differential impacts of the work-first type treatment provided in Riverside relative to the more human-capital type treatments used in the other 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. 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).

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 of it. We begin by showing that the populations treated (i.e, who got to randomization) were very different across the counties; Riverside chose to treat nearly all cases, the other counties choosing to treat only the hardest to serve. In Section 3, we present within-county experimental estimates of the gross

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Hamilton, et al. (1997).

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 complete 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 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 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 that to develop the skills of welfare recipients.

In crafting the GAIN legislation, these two groups compromised on a program that contained both work-first and basic skills and education components in what became known as the GAIN Program Model.[7] 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 deter-

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

mined whether heads of welfare households were subject to GAIN[8] and, if so, registered them for GAIN. (Staff also offered to register adults on welfare who were exempted but wished to volunteer for the program.) A county's GAIN registrants were 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 they had a high school diploma or a GED, they were sent to one of two service tracks. Registrants with low test scores and who did not have a high school diploma or GED were deemed "in need of basic education" and were 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. Registrants, in either group, were then to be channeled into job search activities in an attempt to get them placed in a job. If the registrants 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 in an attempt 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 a great deal of discretion and flexibility in designing their programs. In particular, counties they 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.[9] County GAIN programs differed along both of these dimensions. With respect to the types of welfare recipients registered for

[8] 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

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

GAIN, some counties operated "universal" programs in that all welfare applicants and recipients were registered for GAIN, while some counties registered mostly long-term (presumably lower skill and harder to serve) welfare recipients.

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 choose who 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 rest 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 experimentals and the controls will vary across the research counties. The controls were embargoed from GAIN from the date of their random assignment until June 30, 1993.[10] MDRC 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 of the six research counties. MDRC issued a series of reports on program operation[11] and on the impacts of GAIN programs in these counties over the five-year post random assignment period.[12]

We analyze data for four of the six MDRC research counties, namely Alameda, Los Angeles, Riverside and San Diego. We do not include the other two counties—Butte and Tulare—in

[10] 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.

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

our analysis because they had smaller populations and welfare caseloads (and, thus, smaller analysis samples), were more rural, and had less diversified economies compared to the other four counties. Descriptive statistics and sample sizes for the participants in the MDRC evaluation 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 the samples we use in our analysis for Alameda, Riverside and San Diego (but not Los Angeles) counties 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[13] or because we were missing information on the educational attainment of the sample member. The number of cases lost in these three counties are small[14] and do 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 compared to the control group.

As noted above, there were differences in what types of AFDC cases were registered for GAIN. For the four counties we analyze, Riverside and San Diego counties sought to register all welfare cases in GAIN while Alameda and Los Angeles counties began their GAIN programs by focusing on long-term welfare recipients.[15] 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

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

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

[14] The losses from the original MDRC samples were as follows: Alameda: 0.6% for AFDC-FGs and 6.0% 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.

[15] 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-

cases, i.e., female-headed households on welfare, while Panel B provides comparable information for AFDC-U cases, i.e., cases headed by husband and wives. 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 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 corresponding 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 low-income and welfare-prone populations that reside in each of these counties. Whatever the source of these differences, Table 1 provides strong evidence that the GAIN-registrant populations in these counties are different. We return to the importance of these differences for interpreting the experimental results of our re-analysis of the MDRC GAIN evaluation data in Section 3 below.

There also were marked differences in the policies and practices that were followed in these counties. County programs were shaped by resource allocation, administrators' views

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

about how best to institute the participation mandate, how to meet employment, earnings improvement and welfare dependence goals, and how to do this in a cost-effective manner. 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 2). (The shaded quarters in this—and the next—table show the quarters in which the random assignment of registrants into the MDRC experimental evaluation 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 2 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.[16] 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 communicate a strong "message" to all registrants, including those in education and training activities that employment was central, that it should be sought expeditiously, and that opportunities to obtain low-paying jobs should not be turned down. 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."[17]

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 3. 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 a greater fraction of the 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

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

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 3 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 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. Experimental Results

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. Estimates for AFDC-FG cases are presented in Table 4 and the corresponding estimates for AFDC-U cases are presented in Table 5. 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

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

number of quarters in the calendar year that she received AFDC/TANF benefits.[18,19] MDRC has published estimates of similar outcomes for 3 and 5 years post random assignment.[20] (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.[21])

Our access to data on outcomes for four additional years after random assignment allows us to assess the longer-term consequences of being exposed to the GAIN programs in the four 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 permanence of the impacts found in the previous 3-year and 5-year analyses. Furthermore, analyzing impacts over a nine-year period is a allows us to better assess the extent to which there are differences in the temporal "re-

[18] 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).

[19] 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.

[20] 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.

[21] 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

turns structure" of 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 prospect that the returns to a work-first strategy may be realized sooner than those associated with skill development ones. (We note that which strategy one prefers also depends upon the how one "discounts" the future and the relative costs of each.)

We first consider the long-term impacts of the four GAIN programs for AFDC-FG cases presented in Table 4. Consider first the impacts on employment, given in Panel A. Regardless of whether 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 either the first three or five years post random assignment. Over the first five years, the GAIN registrants in Riverside had annual employment rates that were, on average, 11 percentage points (31% higher) than members of the control group and worked 0.37 more quarters per year (40% higher) than did control group members. Furthermore, 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 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. For years 6 through 9, Riverside's GAIN registrants experience only a 2.8 percentage point advantage in annual rates of employment and 0.12 quarters worked over their control group counterparts and these impacts are less reliably esti-

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relative to those produced by MDRC.

mated compared to the impacts found in years 1 through 5.[22] 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 6 through 9 relative to those in the first five years. Recall 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 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 4.3 percentage points greater and the number of quarters worked per year is 0.14 larger than the corresponding averages for control group members. Furthermore, these impacts in the latter four years 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 four 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 initial (1-year post-enrollment) on the

[22] 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-

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.[23] 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. (More on this below.)

The impacts of GAIN programs on earnings and our indicator of poverty for AFDC-FG households are displayed in Panel B of Table 4. 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 $1,360 per year in the first five years to $569 over the last five years. The comparable averages for San Diego are $597 and $465, respectively. Furthermore, the annual estimates of GAIN impacts on earnings and our poverty indicator tend to be much less precisely estimated in these two counties compared to the statistical precision found for impact estimates during years 1 through 5. At the same time, we note that the impacts on earnings in Riverside are sizeable, even as many as six or more years after individuals were randomly assigned in that county. With respect to the effects of the GAIN programs in Alameda and

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

[23] See Freedman, *et al*. (1999) for early results from the evaluation of Los Angeles Job-First GAIN program that is currently be conducted by MDRC.

Los Angeles counties on earnings and our poverty measure, while we do find that the size of the impacts increase in years 6 through 9, relative to the effects for the first five years after random assignment, these estimates are seldom statistically significant in any of the nine years.

In Panel C of Table 4, we present 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 lower rate of AFDC/TANF participation than the control group in the first five years after random assignment and a 3.1 percentage point differential in years 6 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 reliability estimated, especially in the last two years of the follow-up period.

The three Panels of Table 6 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 4. 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 five years after random assignment. Second, as noted in Freedman, *et al*. (1996), the GAIN program in Los Angeles, rather than those in Riverside or San Diego counties, shows the largest impacts during the first five years after random assignment, although the impacts in this county fall off markedly after 8 and 9 years after random assignment. Turning next to the GAIN impacts on earnings and poverty for AFDC-U households (Panel B of Table 5), 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 6 through 9—an average impact of $912 per year—relative to those for the first five years—an average impact of $266 per year—although none of these estimates are statistically significant at conventional levels of significance. Finally, the results in Panel C of Table 6 for welfare participation show that the most 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 10.1 percentage point lower rate of participation in AFDC/TANF than their control group counterparts in the first five years after random assignment and a 15.9 percentage point lower rate in years 6 through 9. While the reductions for GAIN registrants are lower in Los Angeles County, they do improve between the first five years (3.6 percentage point reduction) and the subsequent four years (3.8 percentage point reduction).

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 in between short-run (3 to 5 years) relative to the long-run (5 to 9 years) after random assignment. 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. Estimating the Differential Effects of GAIN Programs

In this section, we discuss comparisons of the estimates of the training effects across counties. Interpreting such comparisons as causal cannot be justified by reference to the experimental design, and therefore is inherently weaker and more reliant on untestable assumptions than a causal interpretation for within-county estimates. Nevertheless, such comparisons are of great interest, and they are often made in informal discussions and interpretations of the GAIN studies. Here we discuss some of the problems in these comparisons, as well as the assumptions that validate them and investigate the plausibility of them in the current setting. In the end, we argue that there is a strong case to be made that after some statistical adjustments the comparisons between counties can be interpreted causally.

### *4.1 Problems in Cross-county Comparisons*

The results in the previous section show the effects of the training programs implemented in four of the six GAIN counties. The experimental design (randomized assignment within each of the sites) implies that under arguably weak assumptions these estimates can be given a causal interpretation. In particularly, we rely on there being no interference between the individuals. If a substantial fraction of the labor market was involved in these programs, one might be concerned that general equilibrium effects could compromise the simple treatment-control average difference estimates of the causal effect of the training program. See Heckman, Lochner and Taber (1998) for a discussion of such general equilibrium effects. Given that in fact in each county only a small fraction of the overall labor force was enrolled in these programs, it may be reasonable to assume that any such interference would only have a minor effect on the results. Randomization then guarantees that the comparisons of trainees and controls are unbiased for the average effects of the training. This does not change if we adjust the estimates for individual differences using regression techniques. These adjustments can make the inferences more precise but do not affect their bias. The estimates, however, are only valid for the specific program and the specific population, and do not necessarily have any validity for comparisons between counties.

In this section we attempt to go beyond the comparisons validated by the randomization, and consider comparisons between counties. Specifically we consider comparisons between Riverside on the one hand and Alameda, Los Angeles, and San Diego on the other hand. The reason for concentrating on these comparisons is twofold. First, as discussed before, the larger benefits of the training program in Riverside compared to those in other counties (see Tables 4 and 5 and the discussion in Section 3) have attracted considerable attention. Second, these four counties differ considerably in the focus of their training activities as described in Section 2.

For these comparisons we are interested in the question what the average outcome would have been for participants in, say, Los Angeles, had they been subject to the program as implemented in Riverside. This is the question that is of crucial important to administrators deciding which model to follow. Of course knowing the answer to this question does not solve the problem of how to implement such a model, but without a firm answer it is not clear whether there is even any advantage to be gained by attempting to do so.

Randomization over sites rather than within sites would imply that comparisons between counties with different training programs could be given a causal interpretation. MDRC is currently conducting such an experiment in their National Welfare-to-Work Strategies. Such experiments do not solve all problems, however. For example, they only allow 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.

Without randomization over sites the problems in comparing program results in different locations are very similar to those in justifying a causal interpretation of treatment-control differences in non-experimental evaluations. Since the seminal paper by LaLonde (1986) there is widespread skepticism concerning the ability to obtain credible estimates of causal effects in such studies. More recently Dehejia and Wahba (1999), and Heckman, Ichimura, Smith and Todd (1997, 1998a) suggest adjustment for sufficiently detailed observable characteristics may lead to credible estimates.

In principle there are two reasons why the training program may have a different effect in Riverside versus the other three counties. See for a general discussion of these issues Hotz, Imbens and Mortimer (1999). First, the populations subject to the programs may differ in ways that affects the efficacy of the training. For example, if training is less effective for individuals with more education, the fact that in Alameda the highest grade completed is 11.2 years, and in Riverside is 10.7, may explain some of the differences in the estimates of the effect of the training program. In addition to individual characteristics, we interpret differences in the population here as including general labor market conditions. If the local unemployment rate in Riverside is considerably below that in Alameda during the period of the study, this could explain relatively high rates of post-program employment.

A second reason for finding differences in program effects between counties is that the programs may be different. Here we can distinguish two possible differences in the programs. One possibility is that the focus in Riverside on job search rather than basic skills acquisition explains part of the difference in results. Another possibility is that notionally the same program means different things in different sites. Receiving job search assistance in Riverside may be very different from receiving job search assistance in Alameda. The message in Riverside that finding a job was the first and most important objective is suggestive that such differences may be important.

In our analysis we hope to eliminate the first difference that based on differences in the populations, and estimate the part of the difference in program efficacy due to differences in the actual training provided. Without individual-level observations on the training received we will not be able to directly distinguish between the differences due to common efficacy of the different training components (e.g., job search versus basic skills) versus the relative efficacy of the same training components (relative efficacy of job search). Both effects will show up as differences in the relative efficacy of the programs in the two counties after adjusting for population differences.

### *4.2 Methodology*

In this section we discuss our method for obtaining estimates of the relative effects of the training programs in the four GAIN counties, and for our sensitivity analyses to validate the comparisons. For each of the three comparisons (Riverside versus Alameda, Los Angeles, and San Diego), we use the same six outcomes as before, yearly employment indicators, yearly earnings, number of quarters employed each year, indicators for earnings exceeding full-time minimum wage earnings, yearly indicators for AFDC/TANF receipt, and number of quarters on

AFDC/TANF each year. Again we look at these outcomes for the nine years following randomization. We study the AFDC-FG (single parent) and AFDC-U groups separately.

The main approach we use is least squares adjustment for differences in pre-randomization characteristics between pairs of sites. The key assumption is that within subpopulations with the same before random assignment characteristics (including recent labor market histories, and the same local labor market conditions), there are no systematic differences in outcomes between the sites other than those due to differences in the programs; i.e., with these controls, we have selection on observables.

Specifically, to estimate the effect of the training program in Riverside relative to Los Angeles we estimate a regression model

$$Y_i = \beta_0 + \beta_1 R_i + \beta_2' X_i + \varepsilon_i \tag{1}$$

using only data from trainees from both Riverside and Los Angeles. The coefficient on the Riverside dummy measures the effect of the program in Riverside relative to Los Angeles. We estimate the relative effect of the Riverside program both with and without additional covariates $X_i$. These covariates include 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), variables describing labor market histories (lagged earnings for quarters one to ten prior to randomization, indicators for positive earnings in these quarters), lagged AFDC receipts (for quarters one to four prior to randomization, indicators for positive AFDC receipts in these quarters and indicators for positive AFDC/TANF receipts in the four quarters prior to randomization}.

Second, we run the same regression using only the control observations from Riverside and Los Angeles (or Alameda or San Diego). Here the second reason for the differences in outcomes for the two sites, differences in the mix of program components, or differences in notionally identical programs, is not present because in all sites individuals assigned to the control group were excluded from all GAIN services. Any differences we find for this group are, therefore, due to the first reason: differences in pre-random assignment characteristics or the environment. These are the differences we attempt to eliminate by adjusting for pre-randomization variables and local labor market conditions. If these methods are successful, there should therefore be no difference in average outcomes of individuals in the control groups between the sites and the estimate for $\beta_1$ should be close to zero, both substantively and statistically. For other examples of evaluating procedures by applying them to groups for whom the effects are known (typically zero, as in this case), see Heckman and Hotz (1989), Rosenbaum (1995), Hotz, Imbens and Mortimer (1999).

Finally, we run a difference-in-differences version of this regression,

$$Y_i = \beta_0 + \beta_1 R_i + \beta_2 T_i + \beta_3 R_i T_i + \beta_4' X_i + \beta_5' X_i T_i + \varepsilon_i \qquad (2)$$

where $T$ is an indicator for receipt of training, and we are interested in the coefficient on the interaction between the training indicator and the Riverside dummy. We estimate this regression using both trainees and controls from both sites and focus on the estimate of $\beta_3$. Again we run this regression both with and without pre-randomization characteristics.

The last set of estimates, the difference-in-differences estimates, are arguably the most credible, as they eliminate remaining additive differences between Riverside and the other sites. 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. If, on the other hand, the control estimates are not close to zero, one might be concerned that the differences between Riverside and the other sites are not necessarily additive, and the difference-in-differences estimates would be less credible. A 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.

A final comment concerns the credibility of the causal interpretation of estimates of the relative effect of the Riverside program on trainees for the case where control difference are in fact eliminated by adjusting for pre-randomization variables. It is of course always possible that there are unobserved variables that invalidate such comparisons. It is useful, however, to think what properties such variables would need to have, in an informal version of the sensitivity analyses proposed by Rosenbaum and Rubin (1983). First of all, it would have to have a different distribution in Riverside and, say, Los Angeles. Second, it would have to affect controls and trainees differently. Randomization implies that the distribution even of unobservables is balanced in the two groups at that point, and by assumption the difference does not show up in the control group. In other words, this unobserved variable does not affect earnings or other labor market outcomes directly, but only through receipt of the training. Although we cannot rule out the presence of such variables, it does appear unlikely that such a variable would not affect outcomes without training, and we therefore view the tests involving comparisons of controls as strongly indicative of the validity of the across-county comparisons.

## 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 6 and those for AFDC-U cases in Table 7. 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 Panels A through C of Table 6 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 in the first post-randomization year shows that 22.0% more trainees in Riverside are employed than trainees in Alameda. However, even the difference for controls, 5.6% is significantly different from zero (at the 5% level). This suggests that the populations enrolled in GAIN differed, so simply comparing outcomes for the controls is not appropriate. The difference between the trainee and control differences is 16.4%.

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 changes the estimated differences between Riverside and Alameda to 19.8% for trainees and 1.0% for controls. Note that the difference between Riverside and Alameda for controls is essentially eliminated, both substantially and in terms of statistical significance. Note also that the point estimate of the net effect of Riverside relative to Alameda has shifted from 16.4% to 18.7%. This shift is evidence for the importance of heterogeneous program effects.

For the subsequent eight years a general pattern emerges. 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. The adjusted difference-in-difference estimates are only significantly different from zero in the early years, with the employment prospects for trainees enhanced in Riverside relative to Alameda. In contrast, in later years the effects of the program are higher in Alameda than in Riverside, although not significantly so.

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 them 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 five years and barely negative but insignificant effects in the last four years.

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. 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 1% level; i.e., in the long-run (i.e., eight or more years), Alameda's treatment group (who received more education and training) has higher earnings

For the fourth outcome, the indicator for earnings above the full-time minimum wage level, the story is again similar, but the effects are 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, but no single year estimates 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 quarter 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 a later, small, advantage for Alameda, but none are significantly different from zero after adjusting for covariates. It should be kept in mind here that in contrast to the earnings data, where we have ten quarters of pre-randomization outcomes, we only adjust for four quarters of pre-randomization AFDC/TANF receipts.

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, and they remain significantly different from zero. The difference-in-differences estimates are all small and none are 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 3.7% to 12.2%, and which are significant at the 1%

level in seven out of the nine years. Adjusting these for pre-randomization differences lowers the estimates, now ranging from –1.3% to 6.0%, and only significant at the 5% level in three of the nine years. In contrast, the trainee differences are significant both before and after adjustment. It is interesting to note that in year seven the unadjusted estimate for the trainee differences suggests a significantly higher employment rate in Riverside, whereas the adjusted estimate suggests a significantly lower employment rate. 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, but in later years relatively employment rates are higher in Los Angeles than in Riverside, significantly so at the 10% level in years seven and nine.

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 seven out of the nine years), 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 itself. 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 to counter 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 these. Whereas the raw differences for annual AFDC participation range from -2.8% to –15.1%, larger in absolute value than 10% in five of the nine years, and significantly different from zero at the 1% level in all nine years, after adjusting the differences range from -3.6% to 1.0%, 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, positive in one year and negative in the others, in the later years. For the number of quarters spent on AFDC the story is similar. The raw differences for controls range from -0.26 to -0.72, all significant at the 1% level, and the adjusted differences range from -0.19 to 0.01, 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. In terms of averages for and Los Angeles. For example, in Riverside 22% had positive earnings in the last pre-randomization quarter, in San Diego 27%, in Alameda only 14% and in Los Angeles 12%. In Riverside 69% received AFDC in the last pre-randomization quarter, in San Diego 75%, in Alameda a full 99%, and in Los Angeles 96%. These pre-randomization differences are consistent with the process analysis evidence that, as in Riverside, San Diego also tried to run a nearly universal GAIN program. These differences also suggest that covariate adjustment is less important for the Riverside-San Diego comparisons than form the Riverside-Alameda and Riverside-Los Angeles comparisons. This is in fact what we find. In general raw differences between Riverside and San Diego controls are close to zero and often insignificant, and this does not change much with the adjustment.

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 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 relative benefit from attending the training in Riverside rather than San Diego.

For earnings the raw differences between controls is larger and more significant. It is not entirely removed by adjusting for covariates, although the size and significance levels are generally smaller. 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 -0.17 to -0.07. After adjusting for pre-randomization differences the range is -0.38 to -0.15. The difference-in-difference estimates are also negative in all nine years for both outcomes after adjusting, and 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 nine years 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 and some of the earnings outcomes in Sand Diego where substantial differences with Riverside remain after least squares adjustment. The ability 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 program versus the three others. Generally we find substantial positive effects of the Riverside (increasing employment rates and earnings, and lowering AFDC receipts) in the first three to five years post-randomization; the period covered by the MDRC evaluations; however the effects taper off and sometimes becoming significantly negative in the last two to three years in Alameda and Los Angeles. 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.

The results also have the important, but terrifying, implication that short-term evaluation of training programs can be misleading. Despite the demands of policy makers for quick results, there may be no substitute for long-term and costly follow-up. 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. 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, let us 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 latter two in the GAIN program on long-term AFDC recipients because of insufficient funds to enroll all eligibles. 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. Note also that the fractions are so large as to bring ceiling effects into play. There simply is not much observed variation in the history of welfare receipt in these counties.

## 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 look at 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, with in the later years the programs in counties such as Alameda and Los Angeles 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] | $216 | $860 | $224 | $884 | $458 | $1,419 | $593 | $1,498 |
| Earnings 4 Qtrs. Before Rand. Assign.[1] | $267 | $1,030 | $220 | $881 | $620 | $1,619 | $817 | $1,899 |
| Earnings 8 Qtrs. Before Rand. Assign.[1] | $223 | $1,020 | $184 | $809 | $734 | $1,857 | $836 | $1,978 |
| 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] | $1,907 | $526 | $1,873 | $663 | $1,192 | $1,043 | $1,159 | $903 |
| AFDC Benefits 4 Qtrs. Before Rand. Assign.[1] | $1,823 | $550 | $1,867 | $662 | $995 | $1,024 | $1,008 | $928 |
| 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] | \$235 | \$1,017 | \$298 | \$779 | \$960 | \$2,012 | \$997 | \$1,951 |
| Earnings 4 Qtrs. Before Rand. Assign.[1] | \$156 | \$611 | \$284 | \$664 | \$1,464 | \$2,609 | \$1,348 | \$2,485 |
| Earnings 8 Qtrs. Before Rand. Assign.[1] | \$171 | \$617 | \$235 | \$586 | \$1,582 | \$2,846 | \$1,411 | \$2,697 |
| 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] | \$2,671 | \$689 | \$2,502 | \$828 | \$946 | \$1,181 | \$1,251 | \$1,124 |
| AFDC Benefits 4 Qtrs. Before Rand. Assign.[1] | \$2,602 | \$655 | \$2,488 | \$773 | \$699 | \$1,077 | \$1,005 | \$1,128 |
| 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: 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 3: 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 4: Experimental Estimates of Annual Impacts of GAIN, Cases Enrolled in GAIN as AFDC-FG

### Panel A: Employment

| | Alameda | | | | Los Angeles | | | | Riverside | | | | San Diego | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *Yr 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 | 28.3 | 28.8 | -0.5 | -2% | 26.3 | 25.3 | 1.1 | 4% | 50.3 | 34.4 | 15.9*** | 46% | 46.0 | 42.4 | 3.7** | 9% |
| 2 | 31.7 | 27.5 | 4.2 | 15% | 26.1 | 25.1 | 1.0 | 4% | 50.6 | 36.4 | 14.1*** | 39% | 46.4 | 41.9 | 4.4*** | 11% |
| 3 | 32.3 | 28.0 | 4.4* | 16% | 25.9 | 22.9 | 2.9** | 13% | 46.0 | 35.1 | 10.9*** | 31% | 42.9 | 38.1 | 4.8*** | 13% |
| 4 | 35.3 | 31.9 | 3.4 | 11% | 25.3 | 23.9 | 1.4 | 6% | 41.3 | 34.6 | 6.7*** | 19% | 41.2 | 38.3 | 2.9* | 8% |
| 5 | 36.3 | 35.6 | 0.7 | 2% | 28.8 | 26.1 | 2.7* | 11% | 40.4 | 33.1 | 7.3*** | 22% | 40.8 | 38.2 | 2.6* | 7% |
| 6 | 39.4 | 36.6 | 2.8 | 8% | 33.4 | 27.5 | 5.9*** | 21% | 39.4 | 32.7 | 6.7*** | 21% | 40.5 | 38.2 | 2.3 | 6% |
| 7 | 43.4 | 41.9 | 1.5 | 3% | 35.7 | 30.2 | 5.5*** | 18% | 39.4 | 36.6 | 2.7 | 7% | 40.4 | 38.9 | 1.5 | 4% |
| 8 | 45.9 | 45.1 | 0.8 | 2% | 36.7 | 33.1 | 3.6** | 11% | 39.0 | 36.7 | 2.2 | 6% | 40.6 | 39.6 | 1.0 | 3% |
| 9 | 46.6 | 48.8 | -2.2 | -4% | 38.3 | 36.1 | 2.2 | 6% | 39.6 | 40.1 | -0.5 | -1% | 42.1 | 44.3 | -2.2 | -5% |
| Ave., Yrs. 1-5 | 32.8 | 30.3 | 2.4 | 8% | 26.5 | 24.7 | 1.8 | 7% | 45.7 | 34.7 | 11.0 | 31% | 43.5 | 39.8 | 3.7 | 9% |
| Ave., Yrs 6-9 | 43.8 | 43.1 | 0.7 | 2% | 36.0 | 31.7 | 4.3 | 14% | 39.3 | 36.5 | 2.8 | 8% | 40.9 | 40.3 | 0.7 | 2% |
| **Number of Quarters Employed in Year** | | | | | | | | | | | | | | | | |
| 1 | 0.67 | 0.70 | -0.03 | -4% | 0.64 | 0.63 | 0.01 | 1% | 1.21 | 0.77 | 0.44*** | 57% | 1.13 | 1.03 | 0.09** | 9% |
| 2 | 0.80 | 0.73 | 0.07 | 9% | 0.73 | 0.71 | 0.02 | 3% | 1.42 | 0.95 | 0.48*** | 50% | 1.33 | 1.14 | 0.18*** | 16% |
| 3 | 0.94 | 0.82 | 0.12 | 14% | 0.75 | 0.68 | 0.08* | 11% | 1.35 | 0.97 | 0.38*** | 40% | 1.29 | 1.11 | 0.18*** | 16% |
| 4 | 1.04 | 0.93 | 0.11 | 12% | 0.76 | 0.71 | 0.05 | 7% | 1.25 | 0.97 | 0.27*** | 28% | 1.26 | 1.16 | 0.11** | 9% |
| 5 | 1.11 | 1.05 | 0.05 | 5% | 0.85 | 0.78 | 0.08 | 10% | 1.21 | 0.96 | 0.25*** | 27% | 1.25 | 1.18 | 0.08 | 7% |
| 6 | 1.23 | 1.08 | 0.14 | 13% | 1.00 | 0.83 | 0.16*** | 20% | 1.22 | 1.01 | 0.21*** | 21% | 1.28 | 1.19 | 0.09 | 7% |
| 7 | 1.35 | 1.27 | 0.08 | 6% | 1.10 | 0.92 | 0.18*** | 20% | 1.21 | 1.10 | 0.11* | 10% | 1.28 | 1.19 | 0.09* | 8% |
| 8 | 1.48 | 1.41 | 0.07 | 5% | 1.16 | 1.03 | 0.13** | 12% | 1.23 | 1.12 | 0.11* | 10% | 1.32 | 1.26 | 0.06 | 5% |
| 9 | 1.57 | 1.59 | -0.01 | -1% | 1.22 | 1.15 | 0.08 | 7% | 1.27 | 1.23 | 0.04 | 3% | 1.37 | 1.40 | -0.02 | -2% |
| Ave., Yrs. 1-5 | 0.91 | 0.85 | 0.06 | 7% | 0.75 | 0.70 | 0.05 | 7% | 1.29 | 0.92 | 0.37 | 40% | 1.25 | 1.12 | 0.13 | 11% |
| Ave., Yrs 6-9 | 1.41 | 1.34 | 0.07 | 6% | 1.12 | 0.98 | 0.14 | 15% | 1.23 | 1.12 | 0.12 | 11% | 1.31 | 1.26 | 0.05 | 5% |

NOTES: * denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level. Significance levels were note determined for either “Ave., Yrs. 1-5” or “Ave., Yrs. 6-9” differences between experimentals and controls.

## Table 4: AFDC-FG Cases (Continued)

### Panel B: Earnings and Poverty

| Yr 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 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Annual Earnings (1999$)** | | | | | | | | | | | | | | | | |
| 1 | $1,491 | $1,440 | $50 | 3% | $1,426 | $1,507 | -$81 | -5% | $2,611 | $1,461 | $1,150*** | 79% | $2,622 | $2,308 | $314* | 14% |
| 2 | $2,323 | $1,803 | $520 | 29% | $1,959 | $2,007 | -$48 | -2% | $4,169 | $2,552 | $1,618*** | 63% | $4,261 | $3,589 | $672*** | 19% |
| 3 | $3,275 | $2,378 | $897** | 38% | $2,218 | $2,105 | $113 | 5% | $4,367 | $2,833 | $1,534*** | 54% | $4,604 | $3,718 | $886*** | 24% |
| 4 | $3,652 | $2,866 | $786* | 27% | $2,297 | $2,254 | $43 | 2% | $4,330 | $3,075 | $1,255*** | 41% | $4,788 | $4,240 | $547* | 13% |
| 5 | $4,109 | $3,485 | $624 | 18% | $2,630 | $2,557 | $73 | 3% | $4,366 | $3,122 | $1,243*** | 40% | $4,876 | $4,311 | $565* | 13% |
| 6 | $4,583 | $3,795 | $788 | 21% | $3,017 | $2,759 | $258 | 9% | $4,552 | $3,524 | $1,027*** | 29% | $5,057 | $4,553 | $504 | 11% |
| 7 | $5,099 | $4,171 | $928* | 22% | $3,315 | $3,009 | $306 | 10% | $4,455 | $3,874 | $581* | 15% | $5,199 | $4,456 | $743*** | 17% |
| 8 | $5,963 | $5,258 | $704 | 13% | $3,708 | $3,341 | $366 | 11% | $4,533 | $4,051 | $482 | 12% | $5,388 | $5,022 | $366 | 7% |
| 9 | $6,752 | $6,379 | $373 | 6% | $4,186 | $3,936 | $250 | 6% | $4,943 | $4,757 | $185 | 4% | $5,799 | $5,554 | $245 | 4% |
| Ave., Yrs. 1-5 | $2,970 | $2,395 | $575 | 23% | $2,106 | $2,086 | $20 | 0% | $3,969 | $2,608 | $1,360 | 55% | $4,230 | $3,633 | $597 | 16% |
| Ave., Yrs 6-9 | $5,599 | $4,901 | $698 | 16% | $3,556 | $3,261 | $295 | 9% | $4,621 | $4,052 | $569 | 15% | $5,361 | $4,896 | $465 | 10% |
| **Earnings above Full-Time Min. Wage (%)** | | | | | | | | | | | | | | | | |
| 1 | 3.4 | 3.7 | -0.3 | -8% | 3.7 | 4.3 | -0.5 | -13% | 4.9 | 3.1 | 1.8** | 58% | 6.2 | 5.3 | 0.9 | 17% |
| 2 | 6.9 | 5.7 | 1.2 | 21% | 5.8 | 5.9 | -0.1 | -2% | 12.6 | 6.6 | 6.0*** | 91% | 13.6 | 12.0 | 1.6 | 13% |
| 3 | 10.1 | 7.2 | 2.9* | 40% | 7.1 | 7.1 | 0.1 | 1% | 14.1 | 9.0 | 5.1*** | 56% | 15.0 | 12.4 | 2.6** | 21% |
| 4 | 11.1 | 8.5 | 2.6 | 30% | 8.0 | 7.8 | 0.3 | 3% | 14.0 | 9.8 | 4.2*** | 43% | 16.0 | 14.2 | 1.8 | 13% |
| 5 | 13.4 | 11.3 | 2.1 | 18% | 8.9 | 8.3 | 0.6 | 7% | 14.4 | 9.7 | 4.7*** | 49% | 15.9 | 14.0 | 1.9 | 13% |
| 6 | 13.9 | 13.0 | 0.9 | 7% | 10.1 | 9.7 | 0.4 | 4% | 15.1 | 10.8 | 4.3*** | 40% | 16.5 | 14.6 | 1.9 | 13% |
| 7 | 17.1 | 12.5 | 4.6** | 37% | 10.3 | 10.4 | -0.1 | -1% | 14.7 | 12.7 | 2.0 | 16% | 17.0 | 13.9 | 3.1*** | 23% |
| 8 | 19.6 | 17.0 | 2.6 | 15% | 11.8 | 10.8 | 1.0 | 9% | 14.9 | 13.1 | 1.8 | 14% | 17.8 | 17.1 | 0.7 | 4% |
| 9 | 22.1 | 22.5 | -0.4 | -2% | 13.7 | 13.1 | 0.7 | 5% | 16.2 | 16.3 | 0.0 | 0% | 19.2 | 17.7 | 1.5 | 8% |
| Ave., Yrs. 1-5 | 8.9 | 7.3 | 1.7 | 20% | 6.7 | 6.7 | 0.1 | -1% | 12.0 | 7.7 | 4.4 | 59% | 13.3 | 11.6 | 1.7 | 15% |
| Ave., Yrs 6-9 | 18.2 | 16.2 | 2.0 | 14% | 11.5 | 11.0 | 0.5 | 4% | 15.2 | 13.2 | 2.0 | 17% | 17.6 | 15.8 | 1.8 | 12% |

NOTES: * denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.
Significance levels were note determined for either “Ave., Yrs. 1-5” or “Ave., Yrs. 6-9” differences between experimentals and controls.

### Table 4: AFDC-FG Cases (Continued)

### Panel C: AFDC/TANF Participation

| Yr 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 Received AFDC/TANF Benefits in Year (%)** | | | | | | | | | | | | | | | | |
| 1 | 98.5 | 98.3 | 0.2 | 0% | 99.2 | 99.0 | 0.2 | 0% | 95.6 | 96.3 | -0.7 | -1% | 97.0 | 97.5 | -0.5 | 0% |
| 2 | 86.4 | 88.0 | -1.6 | -2% | 84.0 | 87.4 | -3.3*** | -4% | 66.6 | 74.8 | -8.2*** | -11% | 69.1 | 72.3 | -3.1** | -4% |
| 3 | 73.0 | 76.5 | -3.5 | -5% | 72.2 | 76.4 | -4.3*** | -6% | 52.8 | 61.3 | -8.6*** | -14% | 55.7 | 59.6 | -4.0** | -7% |
| 4 | 63.1 | 68.9 | -5.7** | -8% | 61.8 | 66.4 | -4.6*** | -7% | 46.0 | 51.2 | -5.3*** | -10% | 47.5 | 50.1 | -2.6 | -5% |
| 5 | 56.3 | 63.6 | -7.3** | -11% | 53.2 | 56.4 | -3.2** | -6% | 39.3 | 45.4 | -6.1*** | -13% | 40.6 | 44.7 | -4.1*** | -9% |
| 6 | 49.4 | 57.2 | -7.8*** | -14% | 46.1 | 49.4 | -3.4** | -7% | 34.3 | 37.3 | -3.1* | -8% | 35.7 | 38.7 | -3.1** | -8% |
| 7 | 44.1 | 49.8 | -5.7** | -11% | 39.5 | 42.8 | -3.3** | -8% | 30.3 | 34.0 | -3.8** | -11% | 31.0 | 33.5 | -2.6* | -8% |
| 8 | 38.0 | 40.1 | -2.1 | -5% | 33.7 | 35.6 | -1.8 | -5% | 25.5 | 28.7 | -3.3** | -11% | 26.4 | 29.2 | -2.8** | -10% |
| 9 | 29.6 | 29.6 | 0.0 | 0% | 27.0 | 28.6 | -1.6 | -6% | 20.1 | 22.6 | -2.5* | -11% | 21.2 | 23.8 | -2.6** | -11% |
| Ave., Yrs. 1-5 | 75.5 | 79.1 | -3.6 | -5% | 74.1 | 77.1 | -3.0 | -4% | 60.0 | 65.8 | -5.8 | -10% | 62.0 | 64.8 | -2.8 | -5% |
| Ave., Yrs 6-9 | 40.3 | 44.2 | -3.9 | -8% | 36.6 | 39.1 | -2.5 | -6% | 27.5 | 30.7 | -3.1 | -10% | 28.6 | 31.3 | -2.8 | -9% |
| **Number of Quarters in Year on AFDC/TANF** | | | | | | | | | | | | | | | | |
| 1 | 3.71 | 3.79 | -0.08* | -2% | 3.73 | 3.80 | -0.07*** | -2% | 3.26 | 3.43 | -0.17*** | -5% | 3.38 | 3.46 | -0.08** | -2% |
| 2 | 3.11 | 3.29 | -0.18** | -6% | 3.05 | 3.26 | -0.21*** | -7% | 2.16 | 2.55 | -0.39*** | -15% | 2.31 | 2.49 | -0.18*** | -7% |
| 3 | 2.62 | 2.89 | -0.28*** | -10% | 2.61 | 2.84 | -0.23*** | -8% | 1.75 | 2.14 | -0.39*** | -18% | 1.92 | 2.10 | -0.18*** | -9% |
| 4 | 2.26 | 2.64 | -0.37*** | -14% | 2.22 | 2.45 | -0.23*** | -10% | 1.53 | 1.79 | -0.26*** | -15% | 1.65 | 1.79 | -0.14** | -8% |
| 5 | 2.07 | 2.38 | -0.31*** | -13% | 1.90 | 2.08 | -0.17*** | -8% | 1.32 | 1.55 | -0.23*** | -15% | 1.42 | 1.57 | -0.15** | -9% |
| 6 | 1.81 | 2.11 | -0.30*** | -14% | 1.65 | 1.84 | -0.19*** | -10% | 1.18 | 1.33 | -0.15** | -12% | 1.25 | 1.35 | -0.10* | -7% |
| 7 | 1.55 | 1.79 | -0.24** | -13% | 1.42 | 1.57 | -0.15** | -10% | 1.01 | 1.18 | -0.17*** | -14% | 1.08 | 1.17 | -0.09 | -8% |
| 8 | 1.38 | 1.34 | 0.03 | 2% | 1.19 | 1.27 | -0.09 | -7% | 0.84 | 0.99 | -0.15*** | -16% | 0.90 | 1.01 | -0.10** | -10% |
| 9 | 1.03 | 1.00 | 0.03 | 3% | 0.93 | 0.99 | -0.06 | -6% | 0.63 | 0.73 | -0.10** | -14% | 0.71 | 0.80 | -0.09** | -12% |
| Ave., Yrs. 1-5 | 2.75 | 3.00 | -0.25 | -9% | 2.70 | 2.88 | -0.18 | -7% | 2.00 | 2.29 | -0.29 | -14% | 2.14 | 2.28 | -0.15 | -7% |
| Ave., Yrs 6-9 | 1.44 | 1.56 | -0.12 | -6% | 1.30 | 1.42 | -0.12 | -8% | 0.92 | 1.06 | -0.14 | -14% | 0.98 | 1.08 | -0.10 | -9% |

NOTES: * denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.
Significance levels were note determined for either “Ave., Yrs. 1-5” or “Ave., Yrs. 6-9” differences between experimentals and controls.

## Table 5: Experimental Estimates of Annual Impacts of GAIN, Cases Enrolled in GAIN as AFDC-U

### Panel A: Employment

| | Alameda | | | | Los Angeles | | | | Riverside | | | | San Diego | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *Yr 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 | 29.2 | 19.5 | 9.7 | 50% | 39.7 | 29.7 | 10.0*** | 34% | 58.5 | 48.9 | 9.6*** | 20% | 56.1 | 50.5 | 5.6*** | 11% |
| 2 | 34.8 | 20.7 | 14.1** | 68% | 39.2 | 29.6 | 9.6*** | 32% | 53.7 | 43.6 | 10.1*** | 23% | 52.2 | 44.4 | 7.8*** | 17% |
| 3 | 29.2 | 18.3 | 10.9* | 60% | 36.9 | 27.0 | 9.9*** | 37% | 47.3 | 41.5 | 5.8*** | 14% | 46.5 | 43.0 | 3.5* | 8% |
| 4 | 23.6 | 20.7 | 2.9 | 14% | 34.6 | 27.2 | 7.3*** | 27% | 44.3 | 38.3 | 6.0*** | 16% | 43.9 | 41.4 | 2.5 | 6% |
| 5 | 32.6 | 19.5 | 13.1* | 67% | 37.4 | 28.8 | 8.6*** | 30% | 39.5 | 34.8 | 4.8** | 14% | 42.0 | 40.2 | 1.8 | 4% |
| 6 | 30.3 | 23.2 | 7.2 | 31% | 39.0 | 30.7 | 8.3*** | 27% | 38.3 | 35.8 | 2.5 | 7% | 40.5 | 40.8 | -0.3 | -1% |
| 7 | 32.6 | 26.8 | 5.8 | 21% | 36.9 | 36.0 | 0.9 | 3% | 37.6 | 35.2 | 2.4 | 7% | 41.2 | 42.0 | -0.8 | -2% |
| 8 | 37.1 | 35.4 | 1.7 | 5% | 37.7 | 37.5 | 0.2 | 1% | 34.8 | 35.1 | -0.3 | -1% | 40.2 | 42.3 | -2.1 | -5% |
| 9 | 37.1 | 34.1 | 2.9 | 9% | 40.3 | 41.2 | -0.9 | -2% | 37.1 | 39.0 | -1.9 | -5% | 41.8 | 42.3 | -0.5 | -1% |
| Ave., Yrs. 1-5 | 29.9 | 19.8 | 10.1 | 52% | 37.6 | 28.5 | 9.1 | 32% | 48.7 | 41.4 | 7.2 | 17% | 48.2 | 43.9 | 4.2 | 9% |
| Ave., Yrs 6-9 | 34.3 | 29.9 | 4.4 | 16% | 38.5 | 36.3 | 2.1 | 7% | 36.9 | 36.3 | 0.7 | 2% | 40.9 | 41.9 | -0.9 | -2% |
| **Number of Quarters Employed in Year** | | | | | | | | | | | | | | | | |
| 1 | 0.76 | 0.62 | 0.14 | 23% | 1.16 | 0.92 | 0.24*** | 26% | 1.46 | 1.17 | 0.29*** | 24% | 1.47 | 1.30 | 0.16*** | 13% |
| 2 | 0.87 | 0.68 | 0.18 | 27% | 1.23 | 0.94 | 0.29*** | 31% | 1.48 | 1.20 | 0.27*** | 23% | 1.52 | 1.29 | 0.22*** | 17% |
| 3 | 0.78 | 0.56 | 0.21 | 38% | 1.17 | 0.87 | 0.30*** | 35% | 1.34 | 1.15 | 0.19*** | 16% | 1.40 | 1.29 | 0.11 | 8% |
| 4 | 0.78 | 0.63 | 0.14 | 22% | 1.17 | 0.84 | 0.33*** | 39% | 1.22 | 1.08 | 0.14** | 13% | 1.35 | 1.26 | 0.08 | 6% |
| 5 | 0.99 | 0.61 | 0.38* | 62% | 1.18 | 0.88 | 0.30*** | 34% | 1.17 | 1.01 | 0.16** | 16% | 1.26 | 1.23 | 0.03 | 3% |
| 6 | 1.01 | 0.67 | 0.34 | 51% | 1.25 | 0.97 | 0.28*** | 28% | 1.12 | 1.04 | 0.08 | 8% | 1.26 | 1.29 | -0.02 | -2% |
| 7 | 1.04 | 0.79 | 0.25 | 32% | 1.22 | 1.12 | 0.10 | 9% | 1.11 | 1.05 | 0.05 | 5% | 1.29 | 1.28 | 0.01 | 1% |
| 8 | 1.09 | 1.01 | 0.08 | 8% | 1.28 | 1.27 | 0.01 | 1% | 1.08 | 1.10 | -0.02 | -2% | 1.29 | 1.33 | -0.05 | -3% |
| 9 | 1.10 | 1.20 | -0.09 | -8% | 1.34 | 1.38 | -0.04 | -3% | 1.14 | 1.20 | -0.06 | -5% | 1.35 | 1.38 | -0.03 | -2% |
| Ave., Yrs. 1-5 | 0.83 | 0.62 | 0.21 | 34% | 1.18 | 0.89 | 0.29 | 33% | 1.33 | 1.12 | 0.21 | 19% | 1.40 | 1.28 | 0.12 | 9% |
| Ave., Yrs 6-9 | 1.06 | 0.92 | 0.14 | 21% | 1.27 | 1.18 | 0.09 | 9% | 1.11 | 1.10 | 0.01 | 2% | 1.30 | 1.32 | -0.02 | -2% |

NOTES: * denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level. Significance levels were note determined for either “Ave., Yrs. 1-5” or “Ave., Yrs. 6-9” differences between experimentals and controls.

## Table 5: AFDC-U Cases (Continued)

### Panel B: Earnings and Poverty

| Yr 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 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Annual Earnings (1999$)** | | | | | | | | | | | | | | | | |
| 1 | $1,145 | $1,355 | -$210 | -16% | $1,716 | $1,427 | $289* | 20% | $4,042 | $2,966 | $1,076*** | 36% | $3,717 | $3,266 | $450* | 14% |
| 2 | $1,631 | $1,644 | -$12 | -1% | $2,120 | $1,811 | $309 | 17% | $5,352 | $4,330 | $1,022** | 24% | $5,265 | $4,668 | $597 | 13% |
| 3 | $1,775 | $1,387 | $388 | 28% | $2,039 | $1,743 | $296 | 17% | $4,800 | $4,059 | $741* | 18% | $5,161 | $5,040 | $121 | 2% |
| 4 | $2,126 | $1,924 | $202 | 11% | $2,122 | $1,731 | $391 | 23% | $4,459 | $4,097 | $362 | 9% | $4,947 | $4,842 | $105 | 2% |
| 5 | $3,067 | $2,103 | $964 | 46% | $2,391 | $1,964 | $428 | 22% | $4,477 | $3,845 | $632 | 16% | $4,742 | $4,661 | $82 | 2% |
| 6 | $3,422 | $2,601 | $820 | 32% | $2,524 | $2,288 | $236 | 10% | $4,415 | $3,771 | $644 | 17% | $4,864 | $4,939 | -$75 | -2% |
| 7 | $3,713 | $2,560 | $1,153 | 45% | $2,640 | $2,693 | -$52 | -2% | $4,184 | $4,041 | $143 | 4% | $5,013 | $5,121 | -$108 | -2% |
| 8 | $4,276 | $3,191 | $1,085 | 34% | $2,982 | $3,037 | -$55 | -2% | $4,366 | $4,208 | $158 | 4% | $5,359 | $5,565 | -$206 | -4% |
| 9 | $4,565 | $3,977 | $588 | 15% | $3,341 | $3,788 | -$447 | -12% | $4,716 | $4,883 | -$167 | -3% | $6,093 | $6,153 | -$60 | -1% |
| Ave., Yrs. 1-5 | $1,949 | $1,683 | $266 | 14% | $2,078 | $1,735 | $342 | 20% | $4,626 | $3,860 | $767 | 21% | $4,766 | $4,495 | $271 | 7% |
| Ave., Yrs 6-9 | $3,994 | $3,083 | $912 | 31% | $2,872 | $2,951 | -$79 | -1% | $4,420 | $4,226 | $194 | 5% | $5,332 | $5,445 | -$112 | -2% |
| **Earnings above Full-Time Min. Wage (%)** | | | | | | | | | | | | | | | | |
| 1 | 1.1 | 3.7 | -2.5 | -69% | 1.6 | 1.5 | 0.1 | 7% | 11.0 | 7.3 | 3.7*** | 50% | 10.0 | 7.7 | 2.4** | 31% |
| 2 | 4.5 | 3.7 | 0.8 | 23% | 3.7 | 3.6 | 0.1 | 2% | 16.7 | 14.3 | 2.4 | 17% | 16.1 | 13.7 | 2.4* | 18% |
| 3 | 5.6 | 4.9 | 0.7 | 15% | 3.5 | 3.5 | 0.1 | 2% | 15.1 | 11.2 | 3.8** | 34% | 16.4 | 16.4 | 0.0 | 0% |
| 4 | 6.7 | 4.9 | 1.9 | 38% | 4.1 | 3.0 | 1.0 | 34% | 14.2 | 13.9 | 0.3 | 2% | 15.5 | 14.9 | 0.7 | 4% |
| 5 | 9.0 | 7.3 | 1.7 | 23% | 5.2 | 3.7 | 1.4 | 38% | 15.6 | 11.9 | 3.7** | 31% | 14.9 | 15.1 | -0.2 | -1% |
| 6 | 10.1 | 11.0 | -0.9 | -8% | 5.4 | 5.7 | -0.2 | -4% | 14.9 | 11.6 | 3.2** | 28% | 14.8 | 15.1 | -0.2 | -2% |
| 7 | 10.1 | 7.3 | 2.8 | 38% | 6.8 | 6.5 | 0.3 | 5% | 14.2 | 13.6 | 0.6 | 5% | 15.4 | 16.2 | -0.7 | -5% |
| 8 | 13.5 | 8.5 | 4.9 | 58% | 6.8 | 7.1 | -0.3 | -4% | 15.5 | 14.6 | 0.9 | 6% | 16.3 | 15.7 | 0.6 | 4% |
| 9 | 12.4 | 11.0 | 1.4 | 13% | 7.9 | 8.3 | -0.4 | -5% | 16.0 | 16.8 | -0.8 | -5% | 18.7 | 19.2 | -0.5 | -2% |
| Ave., Yrs. 1-5 | 5.4 | 4.9 | 0.5 | 6% | 3.6 | 3.1 | 0.5 | 17% | 14.5 | 11.7 | 2.8 | 27% | 14.6 | 13.5 | 1.1 | 10% |
| Ave., Yrs 6-9 | 11.5 | 9.5 | 2.1 | 25% | 6.7 | 6.9 | -0.1 | -2% | 15.1 | 14.2 | 1.0 | 8% | 16.3 | 16.5 | -0.2 | -1% |

NOTES: * denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.
Significance levels were note determined for either “Ave., Yrs. 1-5” or “Ave., Yrs. 6-9” differences between experimentals and controls.

## Table 5: AFDC-U Cases (Continued)

## Panel C: AFDC/TANF Participation

| | Alameda | | | | Los Angeles | | | | Riverside | | | | San Diego | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *Yr 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 Received AFDC/TANF Benefits in Year (%)** | | | | | | | | | | | | | | | | |
| 1 | 97.8 | 96.3 | 1.4 | 1% | 99.5 | 98.5 | 1.0* | 1% | 90.8 | 90.2 | 0.6 | 1% | 95.3 | 96.3 | -0.9 | -1% |
| 2 | 80.9 | 89.0 | -8.1 | -9% | 84.5 | 88.2 | -3.8** | -4% | 55.6 | 61.4 | -5.8*** | -9% | 61.8 | 70.2 | -8.4*** | -12% |
| 3 | 65.2 | 79.3 | -14.1** | -18% | 74.3 | 79.5 | -5.2** | -7% | 46.2 | 52.0 | -5.9*** | -11% | 50.6 | 55.8 | -5.2** | -9% |
| 4 | 53.9 | 72.0 | -18.0** | -25% | 64.6 | 70.1 | -5.5** | -8% | 39.4 | 43.3 | -3.9* | -9% | 45.6 | 50.1 | -4.5** | -9% |
| 5 | 52.8 | 64.6 | -11.8 | -18% | 59.3 | 63.8 | -4.4* | -7% | 36.3 | 37.9 | -1.6 | -4% | 39.0 | 43.7 | -4.7** | -11% |
| 6 | 40.4 | 62.2 | -21.7*** | -35% | 51.7 | 56.8 | -5.1** | -9% | 32.3 | 33.2 | -0.9 | -3% | 35.5 | 37.8 | -2.4 | -6% |
| 7 | 33.7 | 54.9 | -21.2*** | -39% | 45.4 | 51.7 | -6.3** | -12% | 28.4 | 30.2 | -1.7 | -6% | 31.1 | 34.0 | -2.9 | -8% |
| 8 | 30.3 | 43.9 | -13.6* | -31% | 42.7 | 45.6 | -2.9 | -6% | 24.5 | 25.9 | -1.5 | -6% | 26.3 | 28.9 | -2.5 | -9% |
| 9 | 25.8 | 32.9 | -7.1 | -22% | 39.2 | 39.8 | -0.7 | -2% | 20.0 | 19.8 | 0.2 | 1% | 22.6 | 24.8 | -2.2 | -9% |
| Ave., Yrs. 1-5 | 70.1 | 80.2 | -10.1 | -14% | 76.4 | 80.0 | -3.6 | -5% | 53.7 | 57.0 | -3.3 | -7% | 58.5 | 63.2 | -4.7 | -8% |
| Ave., Yrs 6-9 | 32.6 | 48.5 | -15.9 | -31% | 44.8 | 48.5 | -3.8 | -7% | 26.3 | 27.3 | -1.0 | -3% | 28.9 | 31.4 | -2.5 | -8% |
| **Number of Quarters in Year on AFDC/TANF** | | | | | | | | | | | | | | | | |
| 1 | 3.70 | 3.67 | 0.03 | 1% | 3.75 | 3.80 | -0.05 | -1% | 2.84 | 2.98 | -0.14** | -5% | 3.12 | 3.31 | -0.19*** | -6% |
| 2 | 2.97 | 3.32 | -0.35 | -11% | 3.14 | 3.32 | -0.18** | -5% | 1.73 | 2.03 | -0.30*** | -15% | 2.03 | 2.34 | -0.31*** | -13% |
| 3 | 2.43 | 2.99 | -0.56** | -19% | 2.72 | 2.97 | -0.25*** | -8% | 1.48 | 1.75 | -0.27*** | -16% | 1.76 | 1.98 | -0.22*** | -11% |
| 4 | 2.01 | 2.71 | -0.70** | -26% | 2.43 | 2.67 | -0.24** | -9% | 1.31 | 1.46 | -0.14* | -10% | 1.57 | 1.76 | -0.19** | -11% |
| 5 | 1.82 | 2.43 | -0.61** | -25% | 2.23 | 2.42 | -0.19* | -8% | 1.20 | 1.29 | -0.10 | -8% | 1.36 | 1.53 | -0.16** | -11% |
| 6 | 1.43 | 2.29 | -0.87*** | -38% | 1.94 | 2.17 | -0.23** | -10% | 1.07 | 1.18 | -0.11 | -9% | 1.22 | 1.37 | -0.15** | -11% |
| 7 | 1.24 | 1.94 | -0.70** | -36% | 1.71 | 1.94 | -0.23** | -12% | 0.94 | 1.04 | -0.09 | -9% | 1.08 | 1.22 | -0.14** | -12% |
| 8 | 1.12 | 1.65 | -0.52* | -32% | 1.61 | 1.72 | -0.11 | -6% | 0.80 | 0.87 | -0.07 | -8% | 0.92 | 1.04 | -0.12* | -12% |
| 9 | 0.92 | 1.10 | -0.18 | -16% | 1.44 | 1.47 | -0.02 | -2% | 0.65 | 0.63 | 0.02 | 4% | 0.77 | 0.82 | -0.06 | -7% |
| Ave., Yrs. 1-5 | 2.58 | 3.02 | -0.44 | -16% | 2.86 | 3.04 | -0.18 | -6% | 1.71 | 1.90 | -0.19 | -11% | 1.97 | 2.18 | -0.22 | -10% |
| Ave., Yrs 6-9 | 1.18 | 1.74 | -0.57 | -30% | 1.68 | 1.82 | -0.15 | -8% | 0.87 | 0.93 | -0.06 | -6% | 1.00 | 1.11 | -0.12 | -10% |

NOTES: * denotes statistically significant at 10% level; ** denotes statistically significant at 5% level; *** denotes statistically significant at 1% level.
Significance levels were note determined for either "Ave., Yrs. 1-5" or "Ave., Yrs. 6-9" differences between experimentals and controls.

## Table 6: Differences Between Riverside and Other Counties in Annual Impacts of GAIN Cases Enrolled as AFDC-FG

### Panel A: Employment

| | Riverside - Alameda | | | Riverside - Los Angeles | | | Riverside - San Diego | | |
|---|---|---|---|---|---|---|---|---|---|
| *Yr after Enroll.* | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* |
| **Ever Employed in Year (%)** | | | | | | | | | |
| *Unadjusted Estimates* | | | | | | | | | |
| 1 | 22.0*** | 5.6** | | 24.0*** | 9.1*** | | 4.3*** | -8.0*** | |
| 2 | 18.9*** | 9.0*** | | 24.4*** | 11.3*** | | 4.2*** | -5.5*** | |
| 3 | 13.6*** | 7.1*** | | 20.1*** | 12.2*** | | 3.0*** | -3.0 | |
| 4 | 6.0*** | 2.7 | | 16.0*** | 10.7*** | | 0.1 | -3.7* | |
| 5 | 4.0* | -2.5 | | 11.5*** | 7.0*** | | -0.5 | -5.1** | |
| 6 | 0.0 | -3.9 | | 6.0*** | 5.2*** | | -1.1 | -5.5*** | |
| 7 | -4.0* | -5.3** | | 3.6*** | 6.4*** | | -1.1 | -2.3 | |
| 8 | -6.9*** | -8.4*** | | 2.3** | 3.7* | | -1.6* | -2.9 | |
| 9 | -7.0*** | -8.7*** | | 1.3 | 4.0** | | -2.5*** | -4.2** | |
| *Regression Adjusted Estimates* | | | | | | | | | |
| 1 | 19.8*** | 1.0 | 18.7*** | 14.9*** | 1.8 | 13.2*** | 6.5*** | -0.8 | 7.3*** |
| 2 | 17.2*** | 5.5* | 11.8*** | 17.6*** | 5.2** | 12.4*** | 7.9*** | 0.5 | 7.5*** |
| 3 | 10.7*** | 4.2 | 6.5 | 12.3*** | 6.0** | 6.4** | 6.9*** | 2.0 | 4.9* |
| 4 | 5.4** | 0.3 | 5.1 | 9.6*** | 5.0** | 4.6 | 2.9** | -2.1 | 5.0* |
| 5 | 3.3 | -4.2 | 7.5* | 6.6*** | 0.5 | 6.1** | 2.9** | -6.2** | 9.1*** |
| 6 | -1.3 | -3.7 | 2.3 | 0.0 | -1.1 | 1.1 | -0.1 | -7.6*** | 7.5*** |
| 7 | -5.5** | -4.4 | -1.1 | -2.9** | 2.1 | -5.1* | 0.0 | -3.5 | 3.5 |
| 8 | -5.7** | -6.9** | 1.2 | -2.7* | -1.3 | -1.4 | -1.6 | -3.3 | 1.7 |
| 9 | -5.7** | -4.0 | -1.7 | -4.2*** | 1.4 | -5.6* | -2.9** | -4.2 | 1.2 |
| **Number of Quarters Employed in Year** | | | | | | | | | |
| *Unadjusted Estimates* | | | | | | | | | |
| 1 | 0.54*** | 0.07 | | 0.57*** | 0.14*** | | 0.09*** | -0.26*** | |
| 2 | 0.63*** | 0.22*** | | 0.69*** | 0.24*** | | 0.10*** | -0.20*** | |
| 3 | 0.41*** | 0.14* | | 0.60*** | 0.29*** | | 0.07** | -0.14** | |
| 4 | 0.21*** | 0.05 | | 0.49*** | 0.27*** | | -0.01 | -0.18*** | |
| 5 | 0.11 | -0.10 | | 0.36*** | 0.18*** | | -0.04 | -0.22*** | |
| 6 | 0.00 | -0.07 | | 0.23*** | 0.18*** | | -0.05 | -0.18** | |
| 7 | -0.14* | -0.17** | | 0.11*** | 0.19*** | | -0.07** | -0.09 | |
| 8 | -0.25*** | -0.29*** | | 0.07* | 0.09 | | -0.09*** | -0.14** | |
| 9 | -0.31*** | -0.36*** | | 0.04 | 0.08 | | -0.11*** | -0.17** | |
| *Regression Adjusted Estimates* | | | | | | | | | |
| 1 | 0.40*** | -0.11 | 0.52*** | 0.30*** | -0.09 | 0.40*** | 0.16*** | -0.10 | 0.26*** |
| 2 | 0.55*** | 0.05 | 0.50*** | 0.49*** | 0.02 | 0.46*** | 0.24*** | -0.07 | 0.31*** |
| 3 | 0.32*** | 0.05 | 0.26** | 0.36*** | 0.11 | 0.26*** | 0.20*** | 0.01 | 0.19** |
| 4 | 0.20** | -0.08 | 0.28** | 0.30*** | 0.02 | 0.27*** | 0.10** | -0.15* | 0.25*** |
| 5 | 0.11 | -0.25** | 0.36*** | 0.21*** | -0.06 | 0.27*** | 0.08* | -0.25*** | 0.33*** |
| 6 | -0.04 | -0.10 | 0.06 | 0.03 | 0.01 | 0.02 | 0.01 | -0.25*** | 0.25*** |
| 7 | -0.17** | -0.16 | -0.01 | -0.09* | 0.02 | -0.11 | -0.01 | -0.13 | 0.12 |
| 8 | -0.22*** | -0.22* | -0.01 | -0.11** | -0.09 | -0.02 | -0.08** | -0.19** | 0.11 |
| 9 | -0.23*** | -0.21* | -0.02 | -0.12** | -0.04 | -0.08 | -0.11*** | -0.21** | 0.10 |

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

## Table 6: (Continued)

### Panel B: Earnings

| Yr after Enroll. | Riverside - Alameda | | | Riverside - Los Angeles | | | Riverside - San Diego | | |
|---|---|---|---|---|---|---|---|---|---|
| | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* |
| **Annual Earnings (1999$)** | | | | | | | | | |
| *Unadjusted Estimates* | | | | | | | | | |
| 1 | $1,120*** | $21 | | $1,185*** | -$46 | | -$12 | -$847*** | |
| 2 | $1,847*** | $748*** | | $2,211*** | $545** | | -$92 | -$1,038*** | |
| 3 | $1,092*** | $454 | | $2,149*** | $728*** | | -$237 | -$885*** | |
| 4 | $678* | $208 | | $2,033*** | $821*** | | -$458*** | -$1,165*** | |
| 5 | $257 | -$363 | | $1,736*** | $566* | | -$510*** | -$1,189*** | |
| 6 | -$31 | -$270 | | $1,535*** | $766** | | -$505*** | -$1,029*** | |
| 7 | -$643 | -$297 | | $1,140*** | $865*** | | -$744*** | -$582 | |
| 8 | -$1,430*** | -$1,207*** | | $825*** | $710** | | -$856*** | -$971** | |
| 9 | -$1,810*** | -$1,622*** | | $756*** | $821** | | -$857*** | -$797* | |
| *Regression Adjusted Estimates* | | | | | | | | | |
| 1 | $664*** | -$343 | $1,007*** | $405*** | -$467** | $873*** | $388*** | -$337* | $725*** |
| 2 | $1,447*** | $164 | $1,282** | $1,174*** | -$383 | $1,557*** | $657*** | -$740** | $1,397*** |
| 3 | $853** | $471 | $382 | $1,081*** | $37 | $1,044*** | $564*** | -$506 | $1,070*** |
| 4 | $574 | $367 | $207 | $964*** | $30 | $935** | $300 | -$908** | $1,208*** |
| 5 | $274 | -$167 | $440 | $748*** | -$160 | $908* | $247 | -$1,231*** | $1,478*** |
| 6 | $32 | $45 | -$13 | $380 | $113 | $267 | $189 | -$1,212*** | $1,401*** |
| 7 | -$542 | $243 | -$785 | -$8 | $79 | -$87 | -$34 | -$724 | $690 |
| 8 | -$1,286*** | -$590 | -$696 | -$400 | -$261 | -$139 | -$469** | -$1,376*** | $907* |
| 9 | -$1,522*** | -$228 | -$1,295 | -$520* | $205 | -$725 | -$580** | -$1,018** | $439 |
| **Annual Earnings above FT-Min Wage Earnings (%)** | | | | | | | | | |
| *Unadjusted Estimates* | | | | | | | | | |
| 1 | 1.5 | -0.6 | | 1.1** | -1.2 | | -1.3*** | -2.2** | |
| 2 | 5.8*** | 1.0 | | 6.8*** | 0.7 | | -1.0 | -5.4*** | |
| 3 | 4.1*** | 1.9 | | 7.0*** | 2.0* | | -0.8 | -3.4** | |
| 4 | 2.9* | 1.3 | | 5.9*** | 2.0* | | -2.1*** | -4.4*** | |
| 5 | 1.0 | -1.6 | | 5.6*** | 1.4 | | -1.5** | -4.3*** | |
| 6 | 1.1 | -2.2 | | 4.9*** | 1.1 | | -1.5** | -3.9*** | |
| 7 | -2.4 | 0.2 | | 4.4*** | 2.3* | | -2.3*** | -1.2 | |
| 8 | -4.7*** | -3.9** | | 3.2*** | 2.3* | | -2.9*** | -4.0*** | |
| 9 | -5.9*** | -6.2*** | | 2.5*** | 3.2** | | -3.0*** | -1.4 | |
| *Regression Adjusted Estimates* | | | | | | | | | |
| 1 | 0.4 | -0.6 | 0.9 | -1.4** | -1.2 | -0.2 | 0.1 | -0.9 | 1.1 |
| 2 | 4.5*** | -1.3 | 5.8** | 2.9*** | -2.5* | 5.4*** | 1.8** | -4.3*** | 6.0*** |
| 3 | 3.7** | 1.4 | 2.2 | 3.1*** | -1.4 | 4.5** | 1.9** | -1.9 | 3.7** |
| 4 | 2.7 | 0.5 | 2.2 | 2.3** | -1.4 | 3.7** | 0.5 | -3.2* | 3.8** |
| 5 | 1.6 | -2.8 | 4.4 | 2.4** | -1.6 | 4.0** | 1.1 | -4.0** | 5.1*** |
| 6 | 1.9 | -1.0 | 2.9 | 1.6 | -0.9 | 2.5 | 0.9 | -3.7** | 4.5** |
| 7 | -2.1 | 0.0 | -2.0 | 0.7 | -1.0 | 1.7 | 0.2 | -0.7 | 1.0 |
| 8 | -4.3** | -2.0 | -2.3 | -0.7 | -1.2 | 0.4 | -2.0** | -5.2*** | 3.2 |
| 9 | -5.2*** | -3.0 | -2.3 | -1.1 | 0.5 | -1.5 | -2.5*** | -1.8 | -0.7 |

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

## Table 6: (Continued)

### Panel C: AFDC/TANF Participation

| Yr after Enroll. | Riverside - Alameda | | | Riverside - Los Angeles | | | Riverside - San Diego | | |
|---|---|---|---|---|---|---|---|---|---|
| | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* |
| **Ever Received AFDC/TANF Benefits in Year (%)** | | | | | | | | | |
| *Unadjusted Estimates* | | | | | | | | | |
| 1 | -2.9*** | -2.1** | | -3.6*** | -2.8*** | | -1.4*** | -1.2* | |
| 2 | -19.8*** | -13.2*** | | -17.4*** | -12.5*** | | -2.5*** | 2.5 | |
| 3 | -20.3*** | -15.2*** | | -19.4*** | -15.1*** | | -2.9*** | 1.7 | |
| 4 | -17.2*** | -17.6*** | | -15.8*** | -15.1*** | | -1.5 | 1.2 | |
| 5 | -17.0*** | -18.2*** | | -13.9*** | -11.0*** | | -1.4 | 0.7 | |
| 6 | -15.2*** | -19.9*** | | -11.8*** | -12.1*** | | -1.4 | -1.4 | |
| 7 | -13.8*** | -15.7*** | | -9.3*** | -8.7*** | | -0.7 | 0.5 | |
| 8 | -12.5*** | -11.3*** | | -8.2*** | -6.8*** | | -0.9 | -0.5 | |
| 9 | -9.6*** | -7.0*** | | -6.9*** | -6.0*** | | -1.1 | -1.2 | |
| *Regression Adjusted Estimates* | | | | | | | | | |
| 1 | -0.2 | 1.2 | -1.4 | -0.6 | 0.7 | -1.3 | -3.4*** | -2.4*** | -1.0 |
| 2 | -8.5*** | -1.4 | -7.0** | -5.3*** | 1.0 | -6.3** | -6.1*** | 0.8 | -6.9*** |
| 3 | -8.9*** | -1.9 | -7.0* | -8.9*** | -0.4 | -8.5*** | -8.2*** | -0.3 | -8.0*** |
| 4 | -7.6*** | -4.6 | -2.9 | -7.0*** | -3.1 | -3.9 | -6.7*** | 0.1 | -6.8** |
| 5 | -8.4*** | -6.8** | -1.7 | -7.1*** | 0.4 | -7.5** | -6.2*** | 0.4 | -6.6** |
| 6 | -6.3*** | -9.9*** | 3.6 | -6.5*** | -3.6 | -2.8 | -5.7*** | -1.3 | -4.4 |
| 7 | -6.2*** | -6.7** | 0.5 | -4.5*** | -0.9 | -3.6 | -4.7*** | -0.4 | -4.3 |
| 8 | -5.4** | -5.3 | 0.0 | -3.5** | -3.0 | -0.5 | -4.7*** | 0.5 | -5.2** |
| 9 | -1.8 | -2.0 | 0.2 | -2.2* | -3.3 | 1.0 | -3.8*** | -1.0 | -2.8 |
| **Number of Quarters in Year on AFDC/TANF** | | | | | | | | | |
| *Unadjusted Estimates* | | | | | | | | | |
| 1 | -0.45*** | -0.36*** | | -0.47*** | -0.37*** | | -0.12*** | -0.03 | |
| 2 | -0.95*** | -0.75*** | | -0.89*** | -0.72*** | | -0.15*** | 0.05 | |
| 3 | -0.87*** | -0.75*** | | -0.85*** | -0.69*** | | -0.17*** | 0.04 | |
| 4 | -0.74*** | -0.85*** | | -0.69*** | -0.66*** | | -0.12*** | 0.00 | |
| 5 | -0.74*** | -0.83*** | | -0.58*** | -0.53*** | | -0.10*** | -0.02 | |
| 6 | -0.64*** | -0.78*** | | -0.47*** | -0.51*** | | -0.07** | -0.02 | |
| 7 | -0.53*** | -0.61*** | | -0.40*** | -0.39*** | | -0.07** | 0.01 | |
| 8 | -0.54*** | -0.35*** | | -0.35*** | -0.28*** | | -0.07** | -0.02 | |
| 9 | -0.40*** | -0.27*** | | -0.30*** | -0.26*** | | -0.07** | -0.06 | |
| *Regression Adjusted Estimates* | | | | | | | | | |
| 1 | -0.18*** | -0.05 | -0.12 | -0.15*** | 0.01 | -0.16*** | -0.20*** | -0.07 | -0.13** |
| 2 | -0.49*** | -0.23** | -0.26* | -0.37*** | -0.09 | -0.28*** | -0.31*** | 0.00 | -0.31*** |
| 3 | -0.46*** | -0.20 | -0.27* | -0.44*** | -0.10 | -0.35*** | -0.39*** | -0.03 | -0.35*** |
| 4 | -0.39*** | -0.33** | -0.06 | -0.37*** | -0.15 | -0.22* | -0.32*** | -0.04 | -0.28*** |
| 5 | -0.41*** | -0.39*** | -0.03 | -0.32*** | -0.10 | -0.22* | -0.28*** | -0.03 | -0.24** |
| 6 | -0.32*** | -0.43*** | 0.11 | -0.27*** | -0.19* | -0.08 | -0.23*** | -0.03 | -0.21** |
| 7 | -0.26*** | -0.28** | 0.02 | -0.22*** | -0.11 | -0.11 | -0.21*** | 0.00 | -0.21** |
| 8 | -0.26*** | -0.14 | -0.12 | -0.16*** | -0.11 | -0.05 | -0.19*** | 0.01 | -0.20** |
| 9 | -0.15** | -0.08 | -0.07 | -0.14*** | -0.18** | 0.04 | -0.15*** | -0.04 | -0.12 |

* 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-U

### Panel A: Employment

| Yr after Enroll. | Riverside - Alameda | | | Riverside - Los Angeles | | | Riverside - San Diego | | |
|---|---|---|---|---|---|---|---|---|---|
| | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* |
| **Ever Employed in Year (%)** | | | | | | | | | |
| *Unadjusted Estimates* | | | | | | | | | |
| 1 | 29.3*** | 29.4*** | | 18.8*** | 19.2*** | | 2.4 | -1.6 | |
| 2 | 18.9*** | 22.9*** | | 14.5*** | 14.0*** | | 1.5 | -0.8 | |
| 3 | 18.1*** | 23.2*** | | 10.5*** | 14.5*** | | 0.8 | -1.5 | |
| 4 | 20.7*** | 17.6*** | | 9.7*** | 11.0*** | | 0.4 | -3.1 | |
| 5 | 7.0 | 15.3*** | | 2.1 | 6.0** | | -2.5 | -5.5** | |
| 6 | 7.9 | 12.6** | | -0.8 | 5.1** | | -2.3 | -5.1** | |
| 7 | 5.0 | 8.4 | | 0.8 | -0.8 | | -3.6** | -6.8*** | |
| 8 | -2.3 | -0.3 | | -2.9 | -2.4 | | -5.5*** | -7.2*** | |
| 9 | 0.0 | 4.8 | | -3.2 | -2.2 | | -4.7*** | -3.3 | |
| *Regression Adjusted Estimates* | | | | | | | | | |
| 1 | 4.5 | 3.4 | 1.1 | 0.5 | 2.1 | -1.6 | 4.8*** | -0.1 | 5.0 |
| 2 | -0.4 | 3.8 | -4.3 | 1.2 | 4.5 | -3.3 | 7.4*** | 2.2 | 5.2 |
| 3 | 4.5 | 8.0 | -3.5 | 3.2 | 3.8 | -0.7 | 5.7*** | -0.4 | 6.0 |
| 4 | 3.3 | 2.7 | 0.6 | -0.6 | 0.7 | -1.3 | 4.2** | 1.2 | 3.0 |
| 5 | -10.7* | 3.1 | -13.8 | -10.0*** | -3.0 | -7.0 | -0.3 | -4.6 | 4.3 |
| 6 | -4.5 | -1.5 | -3.0 | -10.0*** | -6.4 | -3.6 | -1.9 | -7.8** | 5.8 |
| 7 | -9.0 | -8.4 | -0.6 | -9.1*** | -10.5** | 1.4 | -3.8* | -8.4*** | 4.6 |
| 8 | -11.3** | -4.0 | -7.3 | -14.0*** | 0.1 | -14.2** | -6.8*** | -9.9*** | 3.2 |
| 9 | -10.2* | -3.3 | -6.9 | -15.5*** | -4.0 | -11.4** | -5.1** | -8.6*** | 3.6 |
| **Number of Quarters Employed in Year** | | | | | | | | | |
| *Unadjusted Estimates* | | | | | | | | | |
| 1 | 0.69*** | 0.55*** | | 0.30*** | 0.25*** | | -0.01 | -0.13* | |
| 2 | 0.61*** | 0.52*** | | 0.25*** | 0.26*** | | -0.04 | -0.09 | |
| 3 | 0.56*** | 0.59*** | | 0.16** | 0.28*** | | -0.07 | -0.15* | |
| 4 | 0.45*** | 0.44** | | 0.06 | 0.24*** | | -0.12** | -0.19** | |
| 5 | 0.18 | 0.40** | | -0.01 | 0.13 | | -0.09* | -0.22*** | |
| 6 | 0.11 | 0.37** | | -0.12* | 0.07 | | -0.14** | -0.24*** | |
| 7 | 0.06 | 0.26 | | -0.11 | -0.06 | | -0.18*** | -0.22*** | |
| 8 | -0.01 | 0.09 | | -0.20*** | -0.17* | | -0.21*** | -0.24*** | |
| 9 | 0.04 | 0.01 | | -0.20*** | -0.17* | | -0.21*** | -0.18** | |
| *Regression Adjusted Estimates* | | | | | | | | | |
| 1 | 0.00 | -0.09 | 0.09 | -0.13 | -0.07 | -0.06 | 0.09 | -0.08 | 0.17 |
| 2 | 0.08 | 0.14 | -0.06 | -0.06 | 0.17 | -0.23 | 0.18*** | 0.07 | 0.11 |
| 3 | 0.11 | 0.22 | -0.11 | -0.08 | 0.03 | -0.10 | 0.10 | -0.08 | 0.17 |
| 4 | 0.01 | 0.09 | -0.07 | -0.19* | 0.03 | -0.22 | 0.03 | -0.07 | 0.09 |
| 5 | -0.33* | 0.06 | -0.39 | -0.35*** | -0.14 | -0.20 | -0.04 | -0.18* | 0.13 |
| 6 | -0.35* | -0.07 | -0.29 | -0.47*** | -0.24 | -0.24 | -0.11* | -0.31*** | 0.20 |
| 7 | -0.32* | -0.13 | -0.19 | -0.39*** | -0.29* | -0.10 | -0.18*** | -0.31*** | 0.13 |
| 8 | -0.35* | -0.03 | -0.32 | -0.55*** | -0.09 | -0.45** | -0.24*** | -0.36*** | 0.12 |
| 9 | -0.24 | -0.24 | 0.00 | -0.56*** | -0.17 | -0.39* | -0.24*** | -0.38*** | 0.14 |

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

## Table 7: (Continued)

### Panel B: Earnings and Poverty

| Yr after Enroll. | Riverside - Alameda: Differences in Means, Trainees | Riverside - Alameda: Differences in Means, Controls | Riverside - Alameda: Difference in Differences | Riverside - Los Angeles: Differences in Means, Trainees | Riverside - Los Angeles: Differences in Means, Controls | Riverside - Los Angeles: Difference in Differences | Riverside - San Diego: Differences in Means, Trainees | Riverside - San Diego: Differences in Means, Controls | Riverside - San Diego: Difference in Differences |
|---|---|---|---|---|---|---|---|---|---|
| **Annual Earnings (1999$)** | | | | | | | | | |
| *Unadjusted Estimates* | | | | | | | | | |
| 1 | $2,897*** | $1,611*** | | $2,326*** | $1,538*** | | $325 | -$300 | |
| 2 | $3,721*** | $2,687*** | | $3,232*** | $2,519*** | | $87 | -$338 | |
| 3 | $3,025*** | $2,672*** | | $2,762*** | $2,316*** | | -$361 | -$981** | |
| 4 | $2,333** | $2,173** | | $2,338*** | $2,366*** | | -$487* | -$745 | |
| 5 | $1,410 | $1,742* | | $2,085*** | $1,882*** | | -$266 | -$815* | |
| 6 | $993 | $1,169 | | $1,890*** | $1,483*** | | -$449 | -$1,168** | |
| 7 | $470 | $1,481 | | $1,543*** | $1,349*** | | -$829*** | -$1,080** | |
| 8 | $89 | $1,017 | | $1,384*** | $1,171*** | | -$993*** | -$1,357*** | |
| 9 | $151 | $906 | | $1,376*** | $1,096** | | -$1,377*** | -$1,270** | |
| *Regression Adjusted Estimates* | | | | | | | | | |
| 1 | $737 | -$494 | $1,231 | $331 | -$312 | $643 | $340 | -$669* | $1,008** |
| 2 | $1,209 | $601 | $608 | $756 | $342 | $415 | $908*** | $304 | $604 |
| 3 | $695 | $861 | -$165 | $375 | -$382 | $757 | $476 | -$569 | $1,045 |
| 4 | $327 | $592 | -$266 | -$42 | -$3 | -$40 | $203 | -$214 | $417 |
| 5 | -$904 | $1,102 | -$2,006 | -$571 | $8 | -$578 | -$13 | -$678 | $665 |
| 6 | -$1,452 | -$78 | -$1,373 | -$898* | -$379 | -$519 | -$267 | -$1,468** | $1,201* |
| 7 | -$1,395 | -$56 | -$1,339 | -$961* | -$603 | -$358 | -$762** | -$1,371** | $609 |
| 8 | -$1,825* | -$89 | -$1,736 | -$1,311** | -$272 | -$1,039 | -$1,294*** | -$1,760*** | $466 |
| 9 | -$1,696 | -$1,079 | -$617 | -$1,466*** | -$1,131 | -$335 | -$1,847*** | -$2,167*** | $320 |
| **Annual Earnings above FT-Min Wage Earnings (%)** | | | | | | | | | |
| *Unadjusted Estimates* | | | | | | | | | |
| 1 | 9.8*** | 3.6 | | 9.3*** | 5.8*** | | 0.9 | -0.4 | |
| 2 | 12.2*** | 10.6*** | | 13.0*** | 10.7*** | | 0.6 | 0.7 | |
| 3 | 9.4** | 6.3* | | 11.5*** | 7.8*** | | -1.4 | -5.2*** | |
| 4 | 7.4** | 9.0** | | 10.1*** | 10.8*** | | -1.4 | -1.0 | |
| 5 | 6.6* | 4.6 | | 10.5*** | 8.2*** | | 0.7 | -3.2* | |
| 6 | 4.7 | 0.7 | | 9.4*** | 6.0*** | | 0.0 | -3.4** | |
| 7 | 4.1 | 6.3 | | 7.4*** | 7.1*** | | -1.2 | -2.6 | |
| 8 | 2.0 | 6.0 | | 8.7*** | 7.5*** | | -0.8 | -1.1 | |
| 9 | 3.6 | 5.9 | | 8.1*** | 8.5*** | | -2.7** | -2.3 | |
| *Regression Adjusted Estimates* | | | | | | | | | |
| 1 | 1.3 | -2.6 | 4.0 | 1.8 | -2.4 | 4.2 | 0.5 | -3.4* | 3.8* |
| 2 | 2.3 | 4.6 | -2.3 | 3.8* | 2.2 | 1.6 | 3.2** | 3.0 | 0.2 |
| 3 | 1.6 | 2.8 | -1.1 | 3.1 | -1.2 | 4.3 | 1.3 | -4.9** | 6.2** |
| 4 | -0.4 | 2.8 | -3.3 | 1.5 | 0.4 | 1.1 | 0.3 | -1.4 | 1.8 |
| 5 | -3.1 | 2.3 | -5.3 | 0.4 | 0.9 | -0.5 | 1.2 | -4.8** | 6.0** |
| 6 | -3.8 | -4.3 | 0.6 | 0.0 | -2.0 | 2.0 | 0.3 | -5.9*** | 6.1** |
| 7 | -0.8 | 1.4 | -2.2 | -1.1 | -0.6 | -0.4 | -1.5 | -4.1* | 2.6 |
| 8 | -5.7 | -1.0 | -4.7 | -1.9 | -1.0 | -0.9 | -3.1** | -3.2 | 0.1 |
| 9 | -2.3 | -4.0 | 1.8 | -1.8 | -3.0 | 1.1 | -4.6*** | -6.3** | 1.7 |

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

## Table 7: (Continued)

## Panel C: AFDC/TANF Participation

| Yr after Enroll. | Riverside - Alameda | | | Riverside - Los Angeles | | | Riverside - San Diego | | |
|---|---|---|---|---|---|---|---|---|---|
| | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* | *Differences in Means, Trainees* | *Differences in Means, Controls* | *Difference in Differences* |
| **Ever Received AFDC/TANF Benefits in Year (%)** | | | | | | | | | |
| *Unadjusted Estimates* | | | | | | | | | |
| 1 | -6.9** | -6.2* | | -8.6*** | -8.3*** | | -4.5*** | -6.1*** | |
| 2 | -25.3*** | -27.6*** | | -28.9*** | -26.8*** | | -6.2*** | -8.7*** | |
| 3 | -19.0*** | -27.2*** | | -28.1*** | -27.5*** | | -4.5*** | -3.8 | |
| 4 | -14.5*** | -28.6*** | | -25.2*** | -26.8*** | | -6.2*** | -6.7*** | |
| 5 | -16.5*** | -26.8*** | | -23.0*** | -25.9*** | | -2.8* | -5.8** | |
| 6 | -8.1 | -29.0*** | | -19.4*** | -23.6*** | | -3.1** | -4.6* | |
| 7 | -5.3 | -24.7*** | | -17.0*** | -21.6*** | | -2.7* | -3.9 | |
| 8 | -5.8 | -18.0*** | | -18.2*** | -19.7*** | | -1.8 | -2.9 | |
| 9 | -5.9 | -13.2*** | | -19.2*** | -20.1*** | | -2.7** | -5.0** | |
| *Regression Adjusted Estimates* | | | | | | | | | |
| 1 | -0.6 | 0.2 | -0.8 | -2.2 | 1.3 | -3.5 | -5.2*** | -8.0*** | 2.8 |
| 2 | -8.7 | -8.5 | -0.2 | -9.4*** | -3.3 | -6.1 | -7.3*** | -9.1*** | 1.9 |
| 3 | -6.6 | -1.9 | -4.7 | -13.5*** | 0.1 | -13.5** | -5.6*** | -4.2 | -1.4 |
| 4 | -1.3 | -11.6 | 10.3 | -10.1*** | -3.7 | -6.4 | -6.4*** | -6.4* | 0.0 |
| 5 | -4.7 | -8.7 | 4.0 | -9.8*** | -2.5 | -7.2 | -2.3 | -3.6 | 1.3 |
| 6 | 3.6 | -18.0*** | 21.6** | -6.3** | -5.8 | -0.4 | -3.2* | -5.0 | 1.8 |
| 7 | 4.4 | -15.1** | 19.5** | -6.2** | -6.5 | 0.3 | -2.3 | -1.4 | -0.9 |
| 8 | 3.5 | -8.1 | 11.6 | -8.8*** | -5.6 | -3.3 | -1.3 | 1.3 | -2.6 |
| 9 | 4.2 | -4.5 | 8.7 | -9.2*** | -6.9 | -2.3 | 0.1 | -0.8 | 0.9 |
| **Number of Quarters in Year on AFDC/TANF** | | | | | | | | | |
| *Unadjusted Estimates* | | | | | | | | | |
| 1 | -0.86*** | -0.69*** | | -0.92*** | -0.82*** | | -0.28*** | -0.33*** | |
| 2 | -1.23*** | -1.29*** | | -1.41*** | -1.29*** | | -0.30*** | -0.31*** | |
| 3 | -0.95*** | -1.23*** | | -1.24*** | -1.22*** | | -0.28*** | -0.22** | |
| 4 | -0.70*** | -1.25*** | | -1.12*** | -1.21*** | | -0.26*** | -0.30*** | |
| 5 | -0.63*** | -1.13*** | | -1.03*** | -1.13*** | | -0.17*** | -0.23** | |
| 6 | -0.35* | -1.11*** | | -0.87*** | -0.98*** | | -0.15*** | -0.19** | |
| 7 | -0.29* | -0.90*** | | -0.77*** | -0.91*** | | -0.14** | -0.19** | |
| 8 | -0.32* | -0.77*** | | -0.81*** | -0.84*** | | -0.12** | -0.17** | |
| 9 | -0.27* | -0.47*** | | -0.80*** | -0.84*** | | -0.12** | -0.20** | |
| *Regression Adjusted Estimates* | | | | | | | | | |
| 1 | -0.17 | 0.04 | -0.20 | -0.15** | 0.01 | -0.15 | -0.22*** | -0.26*** | 0.04 |
| 2 | -0.45** | -0.26 | -0.19 | -0.54*** | -0.11 | -0.43** | -0.31*** | -0.29** | -0.03 |
| 3 | -0.36* | -0.30 | -0.07 | -0.57*** | -0.08 | -0.50** | -0.32*** | -0.23* | -0.10 |
| 4 | -0.15 | -0.55** | 0.40 | -0.47*** | -0.30* | -0.17 | -0.24*** | -0.30** | 0.06 |
| 5 | -0.08 | -0.50* | 0.42 | -0.42*** | -0.27 | -0.15 | -0.14** | -0.20* | 0.07 |
| 6 | 0.16 | -0.67*** | 0.83** | -0.32*** | -0.29 | -0.03 | -0.14** | -0.16 | 0.02 |
| 7 | 0.14 | -0.58** | 0.73** | -0.32*** | -0.37** | 0.05 | -0.12* | -0.06 | -0.06 |
| 8 | 0.08 | -0.32 | 0.39 | -0.38*** | -0.21 | -0.17 | -0.08 | -0.01 | -0.07 |
| 9 | 0.12 | -0.17 | 0.29 | -0.39*** | -0.36** | -0.03 | -0.01 | -0.04 | 0.03 |

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