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## Chapter 9

# Welfare Reform and Indirect Impacts on Health

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**B**eginning in the early 1990s, many states used waivers to reform their Aid to Families with Dependent Children (AFDC) programs. This state experimentation resulted in landmark legislation that eliminated AFDC in 1996 and replaced it with Temporary Assistance for Needy Families (TANF). TANF—like the earlier AFDC program—provides cash grants to low-income families with children and is a key element of the nation's economic safety net. The roots of this reform lie in long-time concern that AFDC led to reductions in work, decreases in marriage, and increases in nonmarital births among low-income women.

These important policy changes, known collectively as *welfare reform*, were implemented with a desire to increase work among low-skilled single-parent families, reduce dependency on welfare, reduce births outside marriage, and increase the formation of two-parent families. In the wake of welfare reform, welfare caseloads declined by 50 percent between 1990 and 2000 (U.S. Department of Health and Human Services 2007), and the employment rate of low-skilled single parents with children increased by 13 percentage points, from 74 percent to 87 percent (Eissa and Hoynes 2006). An enormous literature has developed which evaluates the impact of welfare reform on caseloads and labor supply, as well as on income, poverty, fertility, marriage, and family and child well-being.<sup>1</sup>

Importantly, these goals of welfare reform had little to do with health or health insurance directly. Despite this lack of direct connection to health, however, we argue that welfare reform may have important indirect impacts on health. Understanding if and how welfare reform impacts health is extremely important given the preexisting inverse relationship between income and health. Welfare recipients are worse off than the general population. This both complicates the task of deciphering the effects of welfare reform and makes the possible negative health impacts of welfare a topic of extra concern. For example, Kaplan et al. (2005) show that current and former welfare recipients are more likely to smoke; be obese; have

higher rates of hypertension, diabetes, and elevated glycosylated hemoglobin levels; and have worse self-reported health status compared to other women of the same age and race.

Key policy changes in welfare reform occurred over this period. The central changes in the TANF program include lifetime time limits for receiving cash assistance, work requirements, financial sanctions, and enhanced earnings disregards.<sup>2</sup> At the same time, there were concurrent changes in public health insurance for poor families through the expansions of Medicaid and introduction of the State Children's Health Insurance Program (SCHIP).

There are multiple pathways by which welfare reform may affect health-related outcomes. One pathway is through health insurance—reform leads to reductions in welfare participation, which is expected to reduce health insurance coverage (employer-provided coverage may increase but by less than Medicaid coverage declines). The other pathways are more indirect; for example, welfare reform may impact families' economic resources, time endowment, and levels of stress which may then affect health care utilization and health status.

The early studies on this issue documented very low rates of health insurance coverage following federal reform. For example, Bowen Garrett and John Holahan (2000) found that one year after leaving welfare, one-half of women and almost one-third of children are uninsured.<sup>3</sup> This "leaver" analysis provides an important profile of the well-being of families departing the welfare rolls. However, an analysis of welfare leavers is largely descriptive and not adequate for identifying the impact of welfare reform. There are many forces that can lead to transitions off welfare (for example, labor market opportunities, changes in living arrangements, and welfare reform). Leaver studies are not designed to separate out these forces and identify the impacts of welfare reform and, therefore, provide largely descriptive evidence.

The literature on the impacts of welfare reform on health includes nonexperimental estimates (typically state-panel models using variation in the timing and presence of reform across states) and experimental estimates (randomized experimental evaluations of state waiver programs). These two approaches have important and distinct advantages and disadvantages. Nonexperimental (or observational) studies have the advantage of measuring impacts on the overall population, but they are subject to concerns about identification due to sample selection and endogenous policies. Experimental studies have the advantage of randomization, but the results apply to the experimental context—typically one state, one set of policies, and one group of welfare recipients. Overall, a review of the literature shows that welfare reform led to reduction in health insurance coverage, with small and often insignificant impacts on health care utilization and health status. Some studies find evidence of a modest decrease in utilization and small changes in health behaviors. They suggest that welfare-to-work programs need not have large negative health effects.

We augment the literature review with an analysis of data from separate state welfare-reform experiments in Connecticut, Iowa, Florida, Minnesota, and Vermont. Each of these states represents reforms prior to TANF. However, all ran-

domized experimental evaluation of welfare reform were of state waivers, while there were no evaluations of TANF. We present estimates of the impact of reform on health insurance, health utilization (child doctor or dentist visits and whether the child has a place to go for routine care), and health status (parent-rated child health status, whether the case head is at risk for depression, and whether the focal child scores poorly on a Behavioral Problem Index). These five states were chosen because their experimental evaluations provided the most comprehensive data on health and their welfare reform policies were the most similar to the eventual federal TANF policies. It is important to note, however, that our results reflect the specific policies that were implemented by these states and do not necessarily reflect "average" TANF policies. With that caveat, our analysis of these five states finds that reform led to small changes in health insurance coverage, mixed evidence on health care utilization, and suggestive evidence of improvements in child health status for children between two to nine years old at the beginning of the experiments.

An important drawback of this analysis and indeed much of the existing literature on welfare reform and health is that most of the available data is quite limited. The experimental literature, while able to avoid issues of selection bias that are common to observational studies, is restricted to looking at health outcomes about which information was collected during surveys administered to participants. These surveys tended to ask about health insurance coverage in some detail, and a number also collected information about some health outcomes. Yet the experimental surveys did not collect data on many other important health outcomes of interest, such as whether the children are suffering from developmental delays, asthma, or chronic ear conditions; whether the children or recipients are obese or overweight; whether recipients suffer from substance abuse or sexually transmitted diseases; whether the recipients have negative health behaviors such as smoking; and whether the recipients have chronic conditions such as asthma, hypertension, or diabetes. A number of health surveys (which could be used for nonexperimental analyses) do collect information on these outcomes and others of interest, but they either do not contain information allowing one to identify whether women are in a group likely to be affected by reform, do not contain information for a consistent panel of states and years spanning reform, or do not have large enough samples to plausibly identify the effects of reform. Consequently, we are left with the outcomes on which we and the previous literature have focused.

## WELFARE REFORM IN THE 1990S

Beginning in the early 1990s, many states were granted waivers to make changes to their AFDC programs. About half of the states implemented some sort of welfare waiver between 1993 and 1995 (Office of the Assistant Secretary for Planning and Evaluation 2001). Following this period of state experimentation, the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) was enacted

in 1996, replacing AFDC with TANF. PRWORA originally indicated that all states had to have TANF programs in place by July 1, 1997, although subsequently this deadline was relaxed (Administration of Children and Families 2002). All states implemented PRWORA in a seventeen-month period between September 1996 and January 1998 (Crouse 1999; Administration for Children and Families 1997).

The main goals of welfare reform were to increase work, reduce dependency on welfare, reduce births outside marriage, and to increase the formation of two-parent families. While waiver and TANF policies varied considerably across states, the reforms were generally welfare tightening and pro-work. More specifically, the welfare-tightening elements of reform include work requirements, financial sanctions, time limits, family caps, and residency requirements.<sup>4</sup> The loosening aspects of reform include liberalized earnings disregards (which promote work by lowering the tax rate on earned income while on welfare), increased asset limits, increased transitional Medicaid coverage, and expanded welfare eligibility for two-parent families. Importantly, welfare reform—both the goals and resulting policies—had little directly to do with health or health insurance.

During this period of welfare reform, however, other policies expanded public health insurance for low-income families. Historically, eligibility for Medicaid for the nonelderly and nondisabled was tied directly to receipt of cash public assistance. In particular, the AFDC income-eligibility limits adopted by a state would also be used for Medicaid, and AFDC conferred automatic eligibility for Medicaid. Thus, a family that received AFDC benefits would also be eligible for health insurance through Medicaid. Conversely, if a family left AFDC, its members generally would lose Medicaid coverage.<sup>5</sup> However, in a series of federal legislative acts beginning in 1984, states were required to expand Medicaid coverage for infants, children, and pregnant women beyond the AFDC income limits, leading to large increases in eligibility (Gruber 1997). These are known as the poverty-related or Omnibus Reconciliation Act (OBRA) Medicaid expansions. By 2001, these expansions mandated that all children in families with income up to the federal poverty limit were eligible for Medicaid, provided that they met other requirements.

PRWORA further weakened the link between AFDC and Medicaid by requiring states to cover any family that meets the pre-PRWORA AFDC income, resource, and family composition eligibility guidelines (Haskins 2001). This so-called 1931 program (named after the relevant section of the Social Security Act, as amended by PRWORA) also allowed states to expand eligibility for parents beyond the 1996 AFDC and Medicaid limits. Anna Aizer and Jeffrey Grogger (2003) report that by 2001, about half of the states had taken advantage of this program and expanded Medicaid access for parents above the welfare income cutoffs.

In addition to the time limits, work requirements, and sanctions, PRWORA also contained language restricting immigrant access to means-tested transfer programs including Medicaid. Specifically, immigrants arriving after August 1996 (when the law was passed) are prohibited from receiving any means-tested transfers (including Medicaid) for five years. Initially the law also restricted access to immigrants arriving before 1996, but this was never enacted. In the wake of these

policy changes and the likely confusion over the coverage of earlier arriving immigrants, Medicaid caseloads declined significantly for foreign-born individuals compared to natives (Borjas 2003; Kaushal and Kaestner 2005). As discussed by George Borjas (2003), many states responded by providing immigrant access to Medicaid using newly created, state-funded "fill-in" programs. These policy changes suggest that the impacts of welfare reform may be larger among foreign-born low-income families.

Lastly, in 1997, Congress established the State Children's Health Insurance program (SCHIP), which allows states to expand public health insurance to children beyond the then applicable income eligibility limits in TANF and Medicaid. The idea was to expand coverage for children in families whose family income was above the eligibility income limit for Medicaid but who were uninsured. States could choose to implement SCHIP by expanding Medicaid, by creating a separate SCHIP program, or by doing both. There were also SCHIP resources allocated for outreach to achieve higher take-up rates. The funding for the program came from state funds with matching funds from the federal government, although federal funds were limited to a fixed block grant. States were also allowed to charge premiums, with the amount capped as a share of income for the lowest income SCHIP recipients. This expansion ensured that the bulk of uninsured children in families with income up to 200 percent of the poverty level would be eligible for publicly funded health insurance; many states even expanded eligibility to income levels beyond 200 percent of poverty. The fact that the contraction of welfare programs took place during a time of expansion of public health insurance for children suggests a potential cushioning of any adverse impacts of welfare reform on children. For this reason, it is important to understand the impacts of SCHIP.

## WELFARE REFORM AND EXPECTED IMPACTS ON HEALTH

Despite the lack of a direct connection between welfare reform and health, there are many indirect pathways through which welfare reform may affect health outcomes.

First, welfare reform reduces welfare caseloads, leading to a decline in Medicaid coverage. The AFDC caseload has declined more than 60 percent since its peak in 1994 (U.S. Department of Health and Human Services 2007).<sup>6</sup> During this time period, the number of nondisabled adults and children on Medicaid also fell. Between 1995 and 1997, the number of nondisabled adults on Medicaid fell by 10.6 percent, with larger reductions among cash welfare recipients (Ku and Bruen 1999). The non-cash-assistance Medicaid caseload (especially children), on the other hand, grew, reflecting the separation of AFDC eligibility from Medicaid eligibility.

This expected loss in public coverage may be offset by elevated rates of private coverage due to increases in mothers' employment or coverage from other family

members (that is, a *crowd-in effect*). However, these low-skill workers are likely to be employed in industry-occupation cells with traditionally low rates of employer-provided health insurance (Currie and Yelowitz 2000). In sum, welfare reform is predicted to be associated with a decrease in Medicaid coverage, an increase in private insurance, and likely a decrease in overall insurance.

This pathway of decreased insurance coverage may lead to changes in health. For example, a decline in insurance may lead to less health service utilization; for example, it may lead to less preventive care and prenatal care (Nathan and Thompson 1999). This decline in health care utilization may subsequently impact health outcomes. Importantly, there is an ongoing debate about the magnitude of the causal effects of health insurance coverage on health. Most observational studies show a positive and significant association between health insurance and health. However, as summarized in the recent review by Helen Levy and David Meltzer (2004), these observational studies are limited due to issues with endogeneity and selection. Instead, these authors argue that the best evidence about a causal link between health insurance and health comes from the quasi-experimental analysis of government policy expansions and the RAND health insurance experiments.<sup>7</sup> These studies show a much weaker, but still a generally positive, link between health insurance and health compared to the observational studies. The positive link is stronger for more vulnerable or disadvantaged populations.<sup>8</sup>

Welfare reform may also impact families' economic resources. While the evidence is less clear on this topic, research suggests that welfare reform has led to an overall increase in the incomes of low-skill families.<sup>9</sup> However, Marianne Bitler, Jonah Gelbach, and Hilary Hoynes (2006) find that reform has heterogeneous impacts across the income distribution, and there is some evidence of reductions at the lowest income levels. These changes in a family's economic well-being could then have impacts on health care utilization and health status (as well as on health insurance coverage).

Furthermore, reform-induced increases in employment may lead to changes in a parents' time endowment, which in turn can affect choices about health care utilization, diet, and health (Haider, Jackowitz, and Schoent 2003). Welfare reform could also lead to increases (or decreases) in stress, which in turn can affect health.

Discussion of these pathways illustrates that the impacts of welfare reform on health insurance coverage and health care utilization are more direct than the impacts on health status. This interpretation is consistent with Michael Grossman's (2001) health production model. In particular, health is a durable capital stock that will change slowly with investment (that is, with time, nutrition, exercise, and health services). Health services, on the other hand, are investment goods consumed each period. Therefore they would be expected to change more quickly in response to changes in prices, income, and time constraints. This has important implications for how to interpret and what to expect from empirical analyses of welfare reform on health. We might expect a somewhat immediate impact of reform on health insurance, while it may take months or years for welfare reform to impact health status.

## EMPIRICAL IDENTIFICATION OF THE EFFECTS OF WELFARE REFORM ON HEALTH

Three challenges to identifying the impact of TANF are often raised in the literature (Blank 2001). First, at the same time that welfare reform occurred, the American economy also boomed. As documented by James Hines, Hilary Hoynes, and Alan Krueger (2001), the economic expansion of the 1990s led to important gains for disadvantaged families, especially in the last years of the decade. For example, the unemployment rate for African Americans fell to the lowest level ever recorded, and low-skill groups experienced the first increase in real wages since the 1970s. These gains in the economic position of disadvantaged families may, of course, have independent impacts on health. Second, all states implemented TANF between September 1996 and January 1998. This relatively short implementation period leaves less scope for identifying impacts of TANF through differences in the timing of TANF implementation across states. Identifying the impacts of welfare waivers, however, is considerably more straightforward, as there is variation across states and over time in the implementation of waivers. Third, welfare reform is multidimensional and consists of many different policy changes. In the end, there is no single waiver program or TANF program; rather, there are fifty individual state TANF programs. This makes it difficult to learn about the importance of any specific policy change.

In the face of these challenges, there are several different methodologies used in the literature. The first and most common approach is nonexperimental or observational. A typical approach is to use state-panel models such as

$$Y_{ist} = \alpha + \delta R_{ist} + \beta X_{ist} + \gamma L_{ist} + \theta_s + \nu_i + \epsilon_{ist}. \quad (9.1)$$

Here the main data source is the outcome variable  $y$  which is measured by state  $s$  and time period  $t$ . These data might be state averages or data on individuals (denoted by  $i$  subscript) from a household survey. Welfare reform is captured by  $R_{ist}$  and the parameter of interest is the treatment effect  $\delta$ . One might also include controls for state-level labor-market and other policy variables ( $L_{ist}$ ), individual covariates  $X_{ist}$  (if applicable), as well as state ( $\theta_s$ ) and time ( $\nu_i$ ) fixed effects. In one common version of this model,  $R_{ist}$  is a dummy variable equal to one if waivers or TANF are implemented for this state-year observation. In this case, identification comes from variation in the presence and timing of reform across states.

Because of the lack of variation in the timing of TANF implementation across states, many studies extend the above model to a difference-in-difference model:

$$Y_{ist} = \alpha + \delta_1 R_{ist} + \delta_2 TREAT_{ist} * R_{ist} + \delta_3 TREAT_{ist} + \beta X_{ist} + \gamma L_{ist} + \theta_s + \nu_i + \epsilon_{ist}. \quad (9.2)$$

The parameter of interest is here  $\delta_2$ , and is identified using the difference in trends postreform between a treatment and control group. The treatment group identifies those likely to be impacted by welfare (for example, low-educated female heads of household and their children). Various control groups are used in the literature;

single women without children, higher-income single women with children, married women with children, single men. Other nonexperimental studies add variation in the waiver and TANF reform variables by using detailed characteristics across states such as the length of the time limit or the severity of the sanctions. One challenge for studies using these methods is correctly characterizing the many reforms that states implemented.

Another variation of the basic model above is to replace the reform variable  $R_{st}$  with a measure of the welfare caseload (or per capita caseload) in the state-year cell,  $C_{st}$ . This approach seeks to take advantage of the variation in the declines in welfare caseloads across states and over time. The literature has shown that welfare reform accounts for only part of the fall in caseloads; other important factors are labor-market opportunities and other policies such as the Earned Income Tax Credit (Council of Economic Advisers 1997, 1999, Wallace and Blank 1999, Ziliak et al. 2000; Klerman and Haider 2004). Thus, these studies also control for such factors. Studies that use welfare caseloads to summarize the effects of reform may miss effects of reform which do not result in caseload changes. Another possible problem with using caseloads to identify the causal impact of reform on other outcomes is that the caseload and the outcomes of interest may be affected by unobserved variables.

The second approach is experimental. By federal law, all states implementing welfare waivers were required to evaluate their waivers, mostly using experimental methods. In these experimental evaluations, individuals were randomly assigned into the treatment (welfare reform) and control (AFDC) groups. Using the data from these experiments, the treatment effect of reform can be simply calculated as the difference between mean outcomes in the treatment and control groups. Importantly, all experimental analyses relate to welfare waiver programs; there is no experimental evidence of the effects of state TANF programs.<sup>10</sup> Generally, welfare waivers were less punitive and less severe compared to the TANF policies. Time limits, for example, which are a central feature of TANF, were only used in a few state waiver programs (prominent examples include Connecticut and Florida). We would expect, therefore, that the impacts of state waivers would be smaller than the federal welfare reform which replaced AFDC with TANF.

There are also results from "leaver analyses," which consist of national or state-level studies that examine the characteristics of families leaving welfare. The leaver studies provide an accurate snapshot of the experiences of those families that have left welfare. However, there is no counterfactual in these studies (no control group, no before-period data, and no comparison to exits from welfare in the prereform period), and thus they cannot identify the impacts of welfare reform (Blank 2002). First, there is no way to identify why the families left welfare—it was due to welfare reform or to other factors? Second, a significant fraction of the decline in welfare caseloads is due to reductions in initial entry into welfare (Grogger, Haider, and Klerman 2003), and the leaver studies do not capture this group.

Overall, the experimental and nonexperimental approaches have advantages and disadvantages. Nonexperimental analyses have the advantage of being nationally representative, but the usual identification concerns exist. That is, under-

lying trends in the outcome variables of interest could lead to spurious estimates of policy effects. A further disadvantage of nonexperimental analyses, especially as it relates to health outcomes, is that one is limited by the available data at the state level. An observational analysis requires measuring the outcome variable consistently across states and over time in a representative sample. Some household surveys such as the Current Population Survey (CPS) or the Survey of Income and Program Participation (SIPP) have the state and time coverage, but they offer very limited data beyond health insurance coverage (as is the case with CPS) or only ask about health outcomes intermittently (as is the case with SIPP) for which the health outcome data are collected in topical modules). A number of health surveys collect information on a much wider set of health outcomes; however, these either do not contain information allowing one to identify whether women are in a group likely to be affected by reform, do not contain information for a consistent panel of states and years spanning reform, limit public access to relevant geographic data, or do not have large enough samples to plausibly identify the effects of reform.

Experimental studies have the appeal of random assignment, but they have limitations such as the limited coverage of TANF policies (as opposed to waivers), the inability to obtain nationally representative estimates, and the inability to account for effects of changes in entry behavior that result from welfare reform. Further, as is often noted in discussions of experimental methods for evaluating the effects of programs, effects may differ when a small-scale program is ramped up to a larger scale. Evaluators may be better funded or have a strong incentive to ensure that program participants understand the rules of the treatment. This may not be the case when the program is implemented everywhere. An advantage of the experimental analyses in the context of this study is that many state welfare waiver experiments collected data that allow for a somewhat richer analysis of health outcomes than would be possible with the CPS, for example. However, the small sample sizes in these surveys are a limitation compared to the large sample sizes in typical nonexperimental analyses.

## WHAT DO WE KNOW FROM THE EXISTING LITERATURE?

Our review summarizes evidence from both experimental and nonexperimental analyses.<sup>11</sup> We organize our summary into two sections: the first examines the impacts of welfare reform on health insurance, and the second examines the impacts of reform on health care utilization and health status.

The nonexperimental literature utilizes national survey data that allows for identification of state-year cells. Such national datasets include the Behavioral Risk Factor Surveillance System (BRFSS), CPS, National Health Insurance Survey (NHIS), SIPP, and Vital Statistics detailed natality files.<sup>12</sup> The main source of data for experimental evaluations of welfare waivers is state administrative data for women participating in the experiments. These data, for example, are used to cal-

culate impacts of reform on employment, earnings, welfare use, public-assistance payments, and, in a few cases, Medicaid enrollment. Relevant for this project, however, these administrative data have (in some experiments) been augmented by surveys measuring additional family and child outcomes (including health insurance coverage, utilization, and health status). In addition to the state welfare experiments, we also draw on the experimental evaluation of the Canadian Self Sufficiency Project (SSP), which, like TANF, is an income-support program with a time limit. We discuss impacts of SSP here for two reasons: First, SSP was associated with larger cash increases during the treatment before time limits than were most American programs. Second, the SSP data cover a longer follow-up period than the American experimental data. Both of these features may make possible the detection of long-term health effects of SSP if they exist.

### Health Insurance Coverage

Health insurance coverage is by far the most analyzed outcome in the welfare-reform and health literature. The studies analyze the impact of reform on public health insurance coverage (usually Medicaid, or, in some cases for children, Medicaid or SCHIP), private health insurance coverage (such as employer-provided coverage or individually purchased coverage), and any insurance coverage. Reform should be expected to lead to overall reductions in health insurance—through decreases in public coverage and increases in private coverage—as families move off welfare and into work.

*Welfare Reform Led to Small Reductions in Health Insurance Coverage* The literature is generally consistent with the prediction that reform is associated with a reduction in health insurance coverage. Among the nonexperimental studies, Bitler, Gelbach, and Hoynes (2005) use the BRFS and find that state waivers and TANF implementation led to reductions in any insurance coverage for single women, with the largest impacts for Hispanic single women. The study uses a state-pooled-panel model with dummies for waivers and TANF implementation; it estimates a difference-in-difference model (with married women as controls) to control for other contemporaneous impacts on health insurance. John Cawley, Mathis Schroeder, and Kosali Simon (2005, 2006) extend this work by examining effects of reform on monthly health insurance coverage using the SIPP. They find an increase in the propensity to be uninsured, with somewhat smaller effects for children compared to their mothers. Robert Kaestner and Neeraj Kaushal (2004) use the CPS to estimate a difference-in-difference model comparing single low-educated mothers and their children to low-educated single women without children and low-educated married women with children. They find that declines in the AFDC caseload are associated with reductions in Medicaid, increases in employer-provided health insurance, and overall increases in uninsured for single mothers and their children. They measure welfare reform using the AFDC and TANF caseload (the idea being that reform leads to reductions in the caseload, which

leads to changes in health insurance and other outcomes). These estimates may reflect factors other than reform that lead to changes in the caseload or may miss effects of reform not captured in caseload declines.

The results using household survey data are consistent with Medicaid caseload analyses. Leighton Ku and Bowen Garrett (2000) examine the impact of pre-PRWORA welfare waivers on Medicaid caseloads. They find that waivers led to (statistically insignificant) declines in the adult and child Medicaid caseload.

In contrast to the above studies, Thomas DeLeire, Judith Levine, and Helen Levy (2006) conclude that welfare reform leads to *increases* in health insurance coverage for low-educated women. They use the CPS to examine the impacts of waiver and TANF implementation. They argue that reform could lead to increases in insurance if there are spillover effects of reform on nonrecipients. Indeed, because of these possible spillovers, they consider the “treatment” group to be all women regardless of marriage or presence of children.<sup>13</sup>

Jeffrey Grogger, Lynn Karoly, and Jacob Klerman (2002) review the experimental literature and find small, typically insignificant, and somewhat mixed impacts of welfare reform on the health insurance coverage of adult recipients and their children. In these studies, surveys are used to measure health insurance coverage at some point after random assignment (typically three to four years, depending on the particular study). These results are not necessarily at odds with the nonexperimental literature. Recall that the welfare experiments evaluated state waiver programs that tended to be less severe (for example, few had time limits and sanctions were less severe) compared to the eventual TANF programs. This leads to smaller reductions in caseloads and, hence, smaller reductions in Medicaid.

Overall, the balance of evidence—especially when focusing on the impacts of TANF—is toward finding decreases in insurance following reform. It is difficult to compare specific estimates across the studies (due to different measurement of public coverage and differences in samples and control groups), but the measured impacts are consistently relatively small. For example, Bitler, Gelbach, and Hoynes (2005) find that TANF led to an insignificant 4 percentage point reduction in insurance coverage among low-educated single women with children. This is in stark contrast to the very large rates of uninsured reported in the leaver studies (Garrett and Holahan 2000). However, leaver studies are not useful for estimating the impacts of the policy change that is the focus of this study.

*Medicaid Expansions and SCHIP Introduction Mitigated Declines in Insurance Coverage* Recall that concurrent with welfare reform there was a widespread expansion of public health insurance for low-income children through expansions in Medicaid and the introduction of SCHIP. The Medicaid expansions, which took place between the mid-1980s and the mid-1990s, led to relatively large increases in health insurance coverage among children in low-income families (Gruber and Simon 2007). The evidence suggests, however, that expanding public health insurance leads to a significant “crowd-out” of private coverage leading to smaller reductions in the uninsured than might be expected. Further, these *crowd-out effects* are larger higher up the income distribution among those eligible. SCHIP also leads to



increases in insurance coverage, but the magnitude is somewhat lower than the Medicaid expansions (Bansak and Raphael 2007; Cunningham, Hadley, and Reschovsky 2002; Gruber and Simon 2007; Hudson, Selden, and Bantuin 2005; Lo Sasso and Buchmueller 2004; Duderstadt et al. 2006; Wolfe et al. 2006).

While most of expansions targeted children, the Medicaid 1931 program allowed states to expand eligibility for parents beyond the pre-PRWORA AFDC income eligibility limits. Aizer and Grogger (2003), as well as Susan Busch and Noelia Duchovny (2005), use the CPS to examine parental Medicaid expansions through the 1931 program. Aizer and Grogger (2003) find that these Medicaid expansions led to increases in health insurance coverage of women (with some crowd-out of private-insurance coverage). They also find that expanding parental coverage leads to increases in the health insurance coverage of children; this possibly arises from an increase in benefits relative to costs associated with taking up coverage.

It seems clear that in the absence of these expansions to public health insurance programs, any possibly negative impacts of welfare reform on health insurance (along with any commensurate impacts on health outcomes) would be larger in magnitude. It is also important that any analysis of welfare reform include controls for these state-level expansions to health insurance (Bitler, Gelbach, and Hoynes 2005).

*Welfare Reform Led to Larger Reductions in Health Insurance Among Immigrants* PRWORA imposed a five-year waiting period for TANF and Medicaid for new immigrants (those arriving after 1996). It is widely believed that there was confusion about this provision; in particular, it was unclear whether it applied to all immigrants. This suggests that the impacts of reform would be larger among the foreign-born population. Bitler, Gelbach, and Hoynes (2005), Namratha Kandula and colleagues (2004), and Neeraj Kaushal and Robert Kaestner (2005, 2007) show that welfare reform led to larger reductions in health insurance among the foreign-born or Hispanic populations compared to the entire low-income population. Borjas (2003), on the other hand, finds that more restrictive Medicaid policies did not reduce health insurance coverage among immigrants, because the loss in public coverage was offset by increases in private-insurance coverage. Heather Royer (2005) finds some evidence that more restrictive policies have reduced public coverage, but her findings about overall coverage are inconclusive.<sup>14</sup>

## Health Utilization and Health Outcomes

Fewer studies provide evidence on health care use and health outcomes. The BRFS allows for measuring outcomes for adult women. It includes health care utilization (for example, it includes indicators for recent checkups, Pap smears, breast exams, and whether one needed care but found it unaffordable), health behaviors (for example, smoking, drinking, and exercise), and health status (for example, obesity, lost work days, and self-reported health status). Another source of

nonexperimental data is the detailed natality files, which, as a census of birth certificates, includes data on prenatal care and birth outcomes (for example, birth weight and gestation). Many state waiver experiments include surveys designed to obtain richer family and child outcomes. The NHIS also collects detailed health information, but researchers must use restricted-use data to link these outcomes with state-level data on TANF or caseloads. Individual-level panel data sets such as the Fragile Families and Child Wellbeing Study collect detailed health data but follow a single cohort (in this example, the cohort is parents giving birth in hospitals with a large share of nonmarital births).

*Welfare Reform Had Small, Mixed Impacts on Health Care Utilization and Outcomes* The nonexperimental literature finds small, mixed, and often insignificant impacts on health. Janet Currie and Jeffrey Grogger (2002), as well as Robert Kaestner and Won Chan Lee (2005), use the detailed natality data to show that declines in welfare caseloads during the waiver period (Currie and Grogger) and TANF period (Kaestner and Lee) are associated with declines in prenatal care and small increases in the incidence of low birth weight for low-education women.

Bitler, Gelbach, and Hoynes (2005) use the BRFS to show significant but small reductions in health care utilization, such as the probability of having gotten a checkup, Pap smear, or breast exam in the last year. They also find (insignificant) increases in the likelihood of needing care but finding it unaffordable. Kaestner and Elizabeth Tarlov (2006) also use the BRFS and find no association between reductions in welfare caseloads and health behaviors (smoking, drinking, diet, and exercise) or health status (weight, days in poor health, and general health status).

The experimental estimates of the impact of reform on health are summarized in several reviews (Grogger and Karoly 2005; Grogger, Karoly, and Klerman 2002; Morris et al. 2001; Gennethan et al. 2002). (Estimates are also available from the final reports for each state's experimental evaluation.) Much of the experimental evidence examines the impact on children ages five to twelve years.<sup>15</sup> Health-utilization measures include when the child last saw a dentist or doctor, whether any children have had emergency room visits since random assignment, whether the child has a place to go for routine care, and whether various types of medical care were unaffordable. Health outcomes include parent-rated child general health status as well as indexes of maternal depression and child behavior problems. The estimates from these child surveys are mixed, with an equal number of unfavorable and favorable impacts of reform on health (Grogger and Karoly 2005). The Canadian SSP study examines somewhat different outcomes, focusing on injuries, long-term health limitations, parents' emotional well-being, and general health. The impacts of SSP are quite consistently positive; however, few are statistically significant.

*The Impacts Varied by Demographic Group and by Type of Welfare Reform* Bitler, Gelbach, and Hoynes (2005) find that TANF led to larger reductions in health care utilization (that is, recent checkups, Pap smears, and breast exams) among Hispanic women compared to similar black and white women. Much of the other evidence



that varies by demographic group or type of reform compares immigrants to other women (Kausshal and Kaestner 2005, 2007). Jean Knab, Irv Garfinkel, and Sara McLanahan (chapter 10, this volume) study the effects of reform on unmarried mothers using the Fragile Families and Child Wellbeing study. They look at a range of maternal health and health behaviors, but their analysis is limited by the fact that the data follow a single cohort of mothers in eighteen cities over time.

The experimental studies of child well-being find that any improvements in behaviors tend to be concentrated among young children, while there are more likely to be negative impacts on behaviors for adolescent children (Morris et al. 2001; Gennethan et al. 2002). The experimental literature also finds that improvements are more likely to be present with welfare reforms that lead to increases in income (such as those with generous earnings disregards). Examples of more generous reforms include the state reforms in Connecticut and Minnesota, as well as the Canadian SSP program.

## ILLUSTRATING IMPACTS OF REFORM FROM EXPERIMENTAL DATA

To get a better understanding of the results from the literature, here we present our own estimates on the impact of reform from five state welfare waiver evaluations. Each state waiver (but none of the TANF programs) was evaluated using randomized experiments. Further, state waivers varied significantly in terms of their policy scope, and many of the state waivers did not include time limits or enhanced earnings disregards (which were two of the key policies included in TANF).

Here, we analyze public-use data from state waiver experiments in five states: Connecticut (CT-JF), Florida (FL-FTP), Iowa (IA-FIP), Minnesota (MN-MFIP), and Vermont (VT-WRRP). The primary reason for choosing these states was that they were among a relatively small number of states whose evaluations include data on health. Most state evaluations relied on administrative data on employment and welfare participation, while these five states (and a few others) supplemented this administrative data by fielding a survey to a subset of treatment and control participants. This is the main source of data used for the literature on family and child well-being (Gennethan et al. 2002; Morris et al. 2001). We chose to study Connecticut and Florida in particular because they included time limits as part of their waiver experiments. Overall, these five states provide a good range of welfare-reform policies: they range from more generous (CT-JF and MN-MFIP) to less generous (FL-FTP and VT-WRRP), and they include states with time limits (CT-JF and FL-FTP) and without time limits (IA-FIP, MN-MFIP, and VT-WRRP). This is useful for evaluating alternative sorts of welfare reforms. However, they are less useful purely as a TANF evaluation exercise.

The outcomes we explore include health insurance coverage, health care usage (that is, whether the child has seen a doctor in the past two years, whether the child has seen a dentist in the past two years, whether the child has a place to go

for routine care, and whether the family is not able to afford the doctor or dentist), and health status (that is, parent-rating of the child overall health status, whether the mother is at risk for depression, and whether the focal child scores poorly according to a Behavioral Problem Index). Table 9.3 contains information on the outcomes by state of experiment. While some of the experimental surveys collected other health or health related outcomes (such as whether a child had been to the emergency room or clinic since randomization, whether a child had had any accidents since randomization, or whether the family was food insecure), we do not show results for these additional outcomes because these data are not available for all the experiments, the events varied little across the groups, or the events were very rare in the population.

### Description of the Policies in the Five States

Table 9.1 presents the policies for AFDC and the welfare waivers in the five states (AFDC is the control group program in each case). We document three central policies that are required in TANF programs: time limits, work requirements, and financial sanctions. We also include earnings disregards, as quite commonly they were made more generous in TANF programs and they are very important for determining how reform affects family income.

Very few welfare waivers included time limits. In our set of states, CT-JF and FL-FTP have time limits. There are several other states that included time limits, one of which—Indiana—had public-use data available. However, we excluded Indiana's reform due to limited data on health outcomes. All of the state waivers had work requirements that were stricter than the preexisting AFDC program. The states varied in terms of who was exempt from work requirements (typically, this is based on the age of the youngest child in the family), as well as whether the program was focused on employment (having a "work first" policy) or instead aimed recipients toward education and training.

The earnings disregards determine the rate at which benefits are reduced as earnings increase. In the AFDC program, after three months of working and receiving welfare, all earnings over a basic deduction level were "taxed" at 100 percent. This high benefit reduction rate played a central role in the adverse work incentives in the prereform system. All of the states (except VT-WRRP) offered more generous disregard policies than did AFDC. The most generous states in our sample are CT-JF (where all earnings below the poverty line were disregarded) and MN-MFIP. FL-FTP and IA-FIP had somewhat less generous reforms. Highlighting the earnings disregards is important because this liberalization leads not only to increases in benefits, but also to an increase in the break-even income point (which implies increases in welfare participation, at least before time limits hit). Thus, we have an opposite prediction for the effects of reform in the short run compared to our long-run prediction of reform causing a decrease in welfare participation.

Financial sanctions (which are triggered when a client does not comply with the

TABLE 9.1 / Policies in Welfare-Reform Experiments and Preexisting AFDC Program

	Connecticut Jobs First (JF)	Florida Family Transition Program (FTP)	Minnesota Family Investment Program (MFIP)	Iowa Family Investment Program (FIP)	Vermont Welfare Restructuring Project (WRP)
General		Two-tiered system based on job readiness	Two-tiered system for long-term and short-term recipients. Two treatments-incentives only and full treatment	Control group subject to TANF rules in 1997	Two treatments: incentives only and full (we only consider full treatment)
Time limit	Twenty-one months with six-month extensions	Twenty-four months (of every sixty) for job ready; thirty-six months (of every seventy-two) for others	None	None	None
Work requirements	Mandatory work first, exempt if child younger than one year	Mandatory job search and employment for job ready; education and training for others; exempt if child younger than six months	Mandatory employment and training for long-term; exempt if child younger than one year	Employment and training; exempt if child younger than six months (eliminated in 1997)	Half-time work required after thirty months on aid
Earnings disregards	All earnings disregarded up to poverty line	\$200 + 50% of remaining earnings	38% of earnings disregarded up to 140% of poverty; maximum grant increased by 20% if working	40% of earnings disregarded (all earnings disregarded for first four months of work if "new worker" through 1997)	\$150 + 25% of remaining earnings
Financial sanctions	Cut in grant for first and second offense; three month suspension for third	Adult portion of grant eliminated until compliant (until June 1997)	10% reduction in grant	Three months reduced benefits, six months no benefits	None
Selected other policies	Two years transitional Medicaid	One year transitional Medicaid	One year transitional Medicaid	One year transitional Medicaid	Three years transitional Medicaid
Benefit level, family of three at start of experiment	\$636	\$303	\$532	\$426	\$640

Sources: Bloom et al. (2000, 2002); Fraker et al. (2002); Gennetian, Miller, and Smith (2005); Scrivener et al. (2002).

work requirements (or other rules) also varied across the states, with the most stringent policy in FL-FTP. Finally, the pre-existing AFDC policy provided twelve months of transitional Medicaid assistance to families leaving welfare. This was expanded by CT-JF (to two years) and VT-WRP (to three years). The final row of the table shows how the states vary in terms of the level of the maximum welfare grant at the time of random assignment. FL-FTP and IA-FIP have less generous maximum grants while CT-JFs and VT-WRP's grants are quite generous.

The experiments in VT-WRP and MN-MFIP had two treatments: incentives only and full treatment. The incentives-only policies included the enhanced earnings disregards but not the work requirements. In our analysis below, we analyze both treatments in MN-MFIP, but for VT-WRP we present the full treatment only. (The VT-WRP incentives-only program was only mildly more generous than the preexisting AFDC program, and thus would not be expected to have significantly different impacts than AFDC.) It is also important to note that FL-FTP had a two-tiered policy that assigned one treatment "job ready" (which included a shorter time limit and a work-first employment program) and another "non-job ready" (which included a longer time limit and more emphasis on education and training). We evaluate the average treatment effect across both FL-FTP groups.

Overall, CT-JF and FL-FTP are the most "TANF-like" of our sample programs due to the presence of the time limit. CT-JF and MN-MFIP are the states whose waivers were most likely to lead to increases in income and welfare use (at least before time limits bind in CT-JF) due to the enhanced earnings disregards. VT-WRP was probably the most "gentle" of the reforms, with a weaker work requirement, no time limit, and the longest transitional Medicaid benefits. Again, there is an important caveat: these states provide a good range of possible welfare reform policies but are less useful as a pure TANF evaluation exercise.

## Description of Evaluations and Our Samples in the Five States

Table 9.2 describes the details of each of the five experiments and the samples that we use in our analysis. We begin with the timing of the experiment (random assignment and follow-up period), the geographic range of the experiment (state-wide or partial state), and the sample size for the single-parent component of the evaluation (used in the final reports in each state). Most of the state caseloads consist primarily of single-parent families, and this is reflected in the evaluations that also primarily focus on single-parent families.

All of the impacts on health come from the surveys which are given to a (random) subset of the full sample.<sup>16</sup> We indicate in table 9.2 the timing of the surveys, the cohorts that faced the surveys, and the response rate on the surveys. The surveys tend to be fielded to specific cohorts between three and four years after random assignment. For example, in CT-JF there is survey data on 2,424 single-parent recipients who entered the experiment between April 1996 and February 1997. This number is a bit more than half of the full sample size for the evaluation. The

information on health comes from the adult survey and the focal-child survey (with the exception being VT-WRP, which does not have a focal-child survey). The focal child is a child in the family who is between the ages of five and twelve at the time of the survey. Only one child is chosen as the focal child (randomly if there is more than one child of the correct age), and no child-survey information exists if there is no child in that age range. This explains why the number of observations for the child survey is less than the number for the adult survey.<sup>17</sup>

It is important to note that the timing of the survey (at three to four years after random assignment) is rather medium term. First, we might not expect much to change until after the time limits, which in the case of FL-FTP (and to a lesser extent in the case of CT-JF) are first reached close to the survey dates. Further, to comprehensively understand the impact of welfare reform on health status, we need to use data that span a very long follow-up period (which these surveys do not). On the other hand, we may expect that health insurance (and probably health care utilization) will respond more immediately. However, the expansions in transitional Medicaid assistance in several of these states may dampen any negative impacts on health insurance.

We also indicate in the table the samples that we use in our analysis. We have focused on samples of parents who were single at the time of random assignment.<sup>18</sup> For some states, this is simply the full sample (CT-JF and FL-FTP), as the public-use data are only for single parents. In MN-MFIP, we present estimates for single parents who were long-term welfare recipients living in urban counties. This is the group that was highlighted in the state's final report.<sup>19</sup> Because we consider both incentives only as well as full treatment in MN-MFIP, we report sample sizes for both treatments. We chose our IA-FIP sample to include single females in early cohorts.<sup>20</sup> Finally, for VT-WRP, we include only those receiving the "full" treatment.

A list of the outcome variables and how they are defined in each sample is provided in table 9.3.

## Results

We present our results in six figures. In each case, we present an unconditional "percent effect" estimator (this is simply one hundred times the treatment-group mean minus the control-group mean divided by the control-group mean). This is weighted to be representative of the full experimental population at that point in time where sampling probabilities varied (for CT-JF, IA-FIP, and MN-MFIP). An alternative estimator, used often in the evaluation literature, is the standardized "effect size" (this is the treatment-group mean minus the control-group mean divided by the standard deviation of the control group). For those who prefer that measure, we present companion tables for each of the figures that present the effect size (as well as the difference, standard error of the difference [calculated to be robust to heteroskedasticity], the control-group mean, and the number of observations). Note that in our experiment, there is no need to differentiate between intent to treat and average treatment effects. Everyone in the treatment group is treated:

(text continues on page 255)

TABLE 9.2 / Welfare-Reform Experiments and Samples

	Connecticut Jobs First (JF)	Florida Family Transition Program (FTP)	Minnesota Family Investment Program (MFIP)	Iowa Family Investment Program (FIP)	Vermont Welfare Restructuring Project (WRP)
Timing of experiment (RA: random assign- ment FO: follow-up)	RA: 1/1996 to 2/1997 FO: 4 years	RA: 5/1994 to 2/1995 FO: 4 years	RA: 4/1994 to 3/1996 (urban coun- ties through Q3 1995) FO: 2 to 4 years (through 6/1998)	RA: 9/1993 to 3/1996 FO: 6 to 7 years	RA: 6/1994 to 12/1996 FO: 6 years
Geographic range	Statewide waiver Evaluation in two offices	Partial state waiver Evaluation in one county	Partial state waiver Evaluation in seven counties (three ur- ban counties)	Statewide waiver Evaluation in nine counties	Statewide waiver Evaluation in six districts
Sample size for evaluation	4,803 single- parent cases	2,815 single-parent cases	9,217 single-parent cases, 2,615 long- term urban recipients	7,823 single- parent cases	5,469 single-parent cases, 4,381 single parents for full WRP
Timing of survey	Collected three years after RA to cohort enter- ing experiment between 4/1996 and 2/1997	Collected four years after RA to cohort entering ex- periment between 8/1994 and 2/1995	Collected three years after RA to cohort entering experiment between 4/1994 and 10/1994	Five to six years after RA to co- horts entering before 4/1996 for recipients	Collected forty-two months after RA to cohortentering ex- periment between 10/1994 and 6/1995
Survey Response rate	80%	80%	80%	72%	80%
Sample used in our analysis	All single-parent cases	All single-parent cases	Long-term single- parent recipients in incentives-only ur- ban group (on wel- fare at least twenty- four of past thirty- six months): N = 1,769; Long-term single-parent recipi- ents in full urban group: N = 1,780	Single females eight- teen and older or sixteen to eighteen at RA with a pre- school child: N = 1,996; (Note: sur- vey sample as here completing sur- vey between four years, ten months to five years, eleven months after RA)	Full-WRP single- parent: cases, N = 4,381
Maximum number of observations when using adult survey data	2,424	1,729	718 (incentives only) 724 (full MFIP)	1,201	842
Maximum number of observations when using focal-child sur- vey data	1,469	1,108	573 (incentives only) 587 (full MFIP)	683	NA (no focal-child survey)

Source: Bloom et al. (2000, 2002); Fraker et al. (2002); Gennetian, Miller, and Smith (2005); and Scrivener et al. (2002).

TABLE 9.3 / Construction of Health Outcomes

	Connecticut Jobs First (JF)	Florida Family Transition Program (FTP)	Minnesota Family Investment Program (MFIP)	Iowa Family Investment Program (FIP)	Vermont Welfare Restructuring Project (WRP)
<b>1. Insurance coverage</b> (Figure 9.3 for head and 9.4 for children)					
Public health insurance (month before survey)	Adult head covered by public insurance. Any child of head covered by public insurance	Same as CT-JF	Same as CT-JF	NA	Same as CT-JF
Other non-public health insurance (month before survey)	Adult head has no public coverage and has some other coverage	Same as CT-JF	Same as CT-JF	NA	Same as CT-JF
Any health insurance (month before survey)	Adult head has public or non-public coverage. Any child of head has some coverage	Same as CT-JF	Same as CT-JF	NA	Same as CT-JF
Ever no coverage (any period of no coverage since random assignment)	Adult head had at least one spell of no coverage. Any child had at least one spell of no coverage	Same as CT-JF	Same as CT-JF	NA	NA
<b>2. Health care utilization for focal child sample (child aged five to twelve in household)</b> (Figure 9.5)					
Dentist visit past two years	Focal child had a dental visit during two years preceding survey	Same as CT-JF	Same as CT-JF	Same as CT-JF	NA
Doctor visit past two years	Focal child had a doctor visit during two years preceding survey	Same as CT-JF	Same as CT-JF	Same as CT-JF	NA
Place for routine care	Focal child has place to go for routine care	Same as CT-JF	Same as CT-JF	Same as CT-JF	NA
Family cannot afford dentist	For focal-child sample families, someone needed to see a dentist during past year but could not afford to do so	Same as CT-JF	Same as CT-JF	Same as CT-JF	NA
Family cannot afford doctor	For focal-child sample families, someone needed to see a doctor during past year but could not afford to do so	Same as CT-JF	Same as CT-JF	Same as CT-JF	NA

TABLE 9.3 / (continued)

	Connecticut Jobs First (JF)	Florida Family Transition Program (FTP)	Minnesota Family Investment Program (MFIP)	Iowa Family Investment Program (FIP)	Vermont Welfare Restructuring Project (WRP)
3. Child and mother health outcomes for focal child sample (child aged five to twelve in household) (Figure 9.6)					
Mother at risk for depression	Mother has score of 16 or higher on 20-item Center for Epidemiological Studies Depression Scale (worst score is 60)	Same as CT-JF	Same as CT-JF	Same as CT-JF	NA
Child behavioral problem index in top 25th	Focal child's Behavioral Problem Index was in the worst 25 percentile range	Same as CT-JF	Same as CT-JF	Same as CT-JF	NA
Focal child has excellent or very good health	Focal child health is excellent or very good (rather than good, fair, or poor)	Same as CT-JF	Same as CT-JF	Same as CT-JF	NA

Sources: Authors' compilation of reports and public use data documentation.

everyone faces the new welfare-reform program. This characteristic is in contrast to, for example, the Moving to Opportunity Program, where the treatment is voluntary (Kling, Liebman, and Katz 2007).<sup>21</sup>

To begin, figure 9.1 presents the impacts of welfare reform on quarterly employment, quarterly welfare participation, and quarterly income. These estimates are important "first-stage" outcomes. For example, we may expect states with smaller reductions in welfare participation to have smaller reductions in health insurance coverage. Treatment-group members in states whose reforms led to large increases in income may show fewer adverse or more beneficial health outcomes compared to treatment-group members in states whose reforms led to decreases in income.

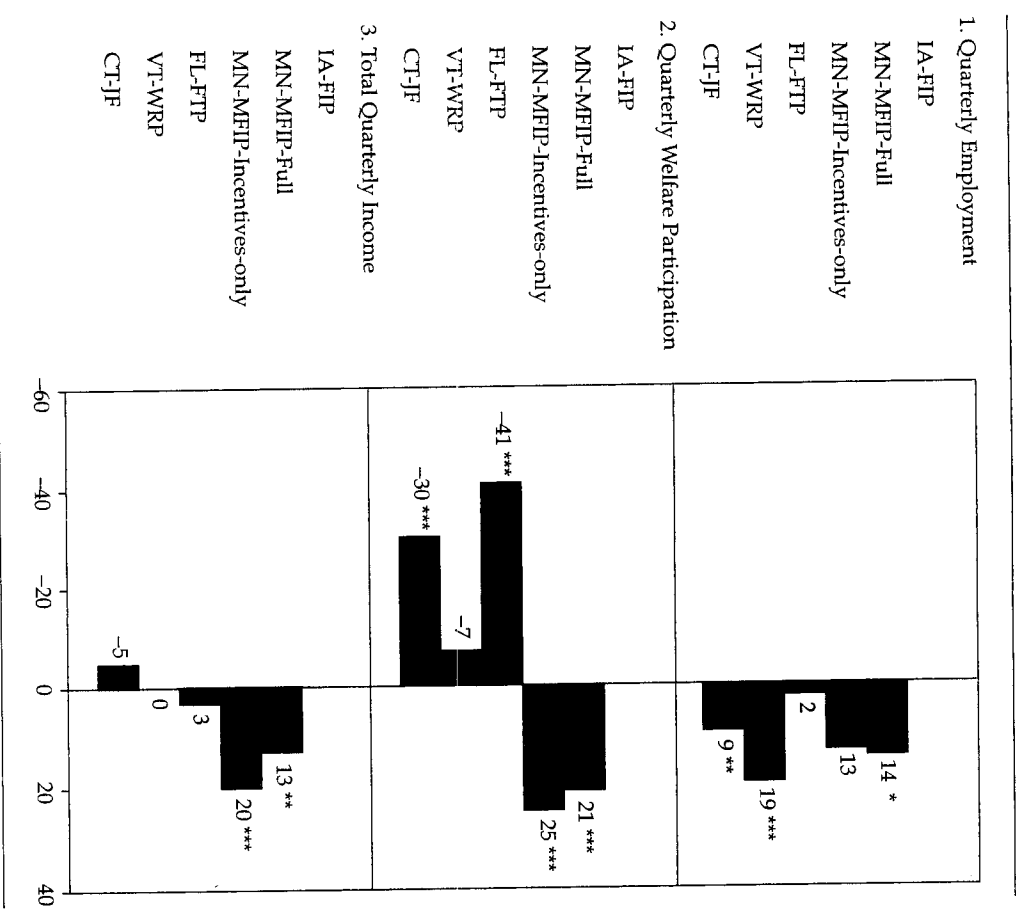
Figure 9.1 (and the companion table, table 9.4) presents these first-stage outcomes measured at the quarter that the survey was fielded (outcomes are not available for IA-FIP).<sup>22</sup> Information about employment and welfare participation at the time of the survey may present an incomplete picture of these important first-stage outcomes. For example, it may be important to know about longer-term welfare and employment exposure to understand impacts on health measured at the time of the survey. Figure 9.2 and table 9.5 provide a more comprehensive characterization of these impacts by presenting differences (between the treatment and control groups) in the outcomes averaged over all quarters between random assignment and the time of the survey. While an argument could be made in support of either time frame, we focus on the entire period up to the survey to reflect the fact that the health care utilization data refer to some look-back period and the health status variables are stock measures that adjust over a longer time period.

Figures 9.1 and 9.2 consist of three panels, where each panel corresponds to a different outcome: quarterly employment, welfare participation, and income (which includes earnings, cash assistance, and food stamps, as well as for MN-MFIP only General Assistance). Within each panel, we present percent effects for each of the states for which the outcome is available. There are a maximum of six estimates—one each for CT-JF, FL-FTP, IA-FIP, and VT-WRP, and two for MN-MFIP (incentives-only treatment and full treatment). Each estimate is shown as a bar; at the end of the bar we provide the percent effect along with the significance of the treatment control differences. (\* denotes significant at the 10 percent level, \*\* significant at the 5 percent level, and \*\*\* significant at the 1 percent level.) Later figures differ only in how many panels are presented. The sample for the estimates in figures 9.1 and 9.2 is persons completing the survey who also have administrative data for all three outcomes.<sup>23</sup>

The results for figure 9.1 show that at the time of the survey, employment is higher in all states, with significant increases in CT-JF, VT-WRP, and MN-MFIP-Full. Welfare participation is significantly lower in CT-JF and FL-FTP, reflecting the period after the time limit. MN-MFIP shows higher welfare participation and higher income, reflecting the generous reform without time limits.

The results for figure 9.2, reflecting the average impact during the period between random assignment and the survey, show that all of the programs led to statistically significant increases in quarterly employment relative to AFDC. Effects on employment seem to be larger in the states with more generous earnings disre-

FIGURE 9.1 / Impacts of Welfare Reform on Employment, Welfare, and Income from Experimental Studies, Outcomes Measured at the Quarter of Survey (Percent Effects)



Source: Authors' compilation of public use data.

Note: The impacts are reported at the quarter when the survey was conducted. For CT-JF the survey was done thirty-six months after random assignment began; for VT-WRRP, forty-two months; for FL-FTP, forty-eight months; for MN-MFIP, thirty-six months; and for IA-FIP, five to six years. Effect sizes reported are the treatment-control difference divided by the control mean. Significance levels (\*\*\*) 1 percent, \*\* 5 percent, and \* 10 percent) are for treatment-control differences.

TABLE 9.4 / Impacts on Employment, Welfare, and Income, Quarter of Survey

	Difference	Std. Err. Difference	Mean (Controls)	Std. Dev. (Controls)	Percent Effect	Effect Size	N
1. Quarterly Employment							
IA-FIP	NA						
MN-MFIP-Full	0.071*	0.040	0.50	0.50	14.13%	0.142	724
MN-MFIP-Incentives	0.064	0.041	0.50	0.50	12.72%	0.128	718
FL-FTP	0.009	0.024	0.54	0.50	1.69%	0.018	1,729
VT-WRRP	0.102***	0.034	0.53	0.50	19.28%	0.204	842
CT-JF	0.050**	0.022	0.57	0.50	8.69%	0.100	2,414
2. Quarterly Welfare Receipt							
IA-FIP	NA						
MN-MFIP-Full	0.117***	0.040	0.56	0.50	20.97%	0.235	724
MN-MFIP-Incentives	0.137***	0.039	0.56	0.50	24.65%	0.276	718
FL-FTP	-0.082***	0.017	0.20	0.40	-40.77%	-0.204	1,729
VT-WRRP	-0.029	0.034	0.42	0.49	-6.86%	-0.058	842
CT-JF	-0.121***	0.021	0.40	0.49	-30.26%	-0.248	2,414
3. Average Quarterly Income							
IA-FIP	NA						
MN-MFIP-Full	337.97***	146.04	2616.34	1829.27	12.92%	0.185	724
MN-MFIP-Incentives	512.71***	158.42	2616.34	1829.27	19.60%	0.280	718
FL-FTP	49.53	89.20	1799.48	1759.93	2.75%	0.028	1,729
VT-WRRP	2.26	129.79	2527.20	1869.25	0.09%	0.001	842
CT-JF	-144.57	107.24	2974.01	2384.00	-4.86%	-0.061	2,414

Source: Authors' compilations of public use data.

Note: Shown are average quarterly employment rates, averages for any cash welfare receipt during quarter (to be comparable to the employment rates), and average quarterly income (cash welfare plus food stamps plus general assistance [MN only] plus earnings) for the quarter during which the survey was done (except for IA where we do not report values because no quarterly number is available). Statistics are for all observations completing the adult survey that also had data for all the outcomes. Numbers are weighted to be representative of survey design where relevant. Standard errors are robust to heteroskedasticity. Percent effect is one hundred times the treatment-control difference divided by control mean, effect size is treatment-control difference divided by control standard deviation. Significance levels (\*\*\*) 1 percent, \*\* 5 percent, and \* 10 percent) are for treatment-control differences.

gards (MN-MFIP and CT-JF). Welfare participation is significantly higher than under AFDC in MN-MFIP and somewhat higher in CT-JF, reflecting these states' more generous disregards; in the case of CT-JF, it also reflects the fact that more of the period was before the time limits. Welfare participation is significantly lower in FL-FTP.<sup>24</sup> Finally, panel 3 presents impacts on quarterly income from administrative sources. Total quarterly income was significantly higher for the treatment-group members in CT-JF and MN-MFIP, and it was approximately the same for the other states.



These findings may suggest various patterns for the impacts on health insurance coverage, health care utilization, and health status, depending on the importance of the various pathways for reform to affect these outcomes. For example, if the most important factor leading to public-insurance coverage is ongoing welfare participation, figures 9.1 and 9.2 suggest that we would find increases in coverage with reform for MN-MFIP and possibly for CT-JF. If, instead, employment is important, there are other implications. We should point out again that increases in welfare participation are not generally expected with TANF. This difference between welfare participation in the experiments compared with what we expect from TANF reflects the fact that only two of our states have time limits; furthermore, CT-JF (one of the two time-limit programs) is highly unusual in its generous earnings disregard and extension of the transitional Medicaid benefits. For those most interested in evaluating TANF, the results for FL-FTP are the most relevant.

Figure 9.3 and table 9.6 present the estimates of the effect of reform on the head of household's health insurance coverage. Reform led to increases in public-insurance coverage in MN-MFIP and CT-JF—this seems to be a direct result of longer stays on welfare (as shown in figure 9.2). Public-insurance coverage fell (though not significantly) in the other states. The bottom line is that reform leads to a significant increase in the head of household's overall insurance coverage in CT-JF, an insignificant increase for MN-MFIP, and negative, small, and insignificant effects for the other states. One interesting outcome available in some states is the presence of spells of uninsurance since the time of random assignment. This outcome shows large and significant decreases (a positive outcome) for MN-MFIP, perhaps reflecting increased welfare participation (as shown in figures 9.1 and 9.2).

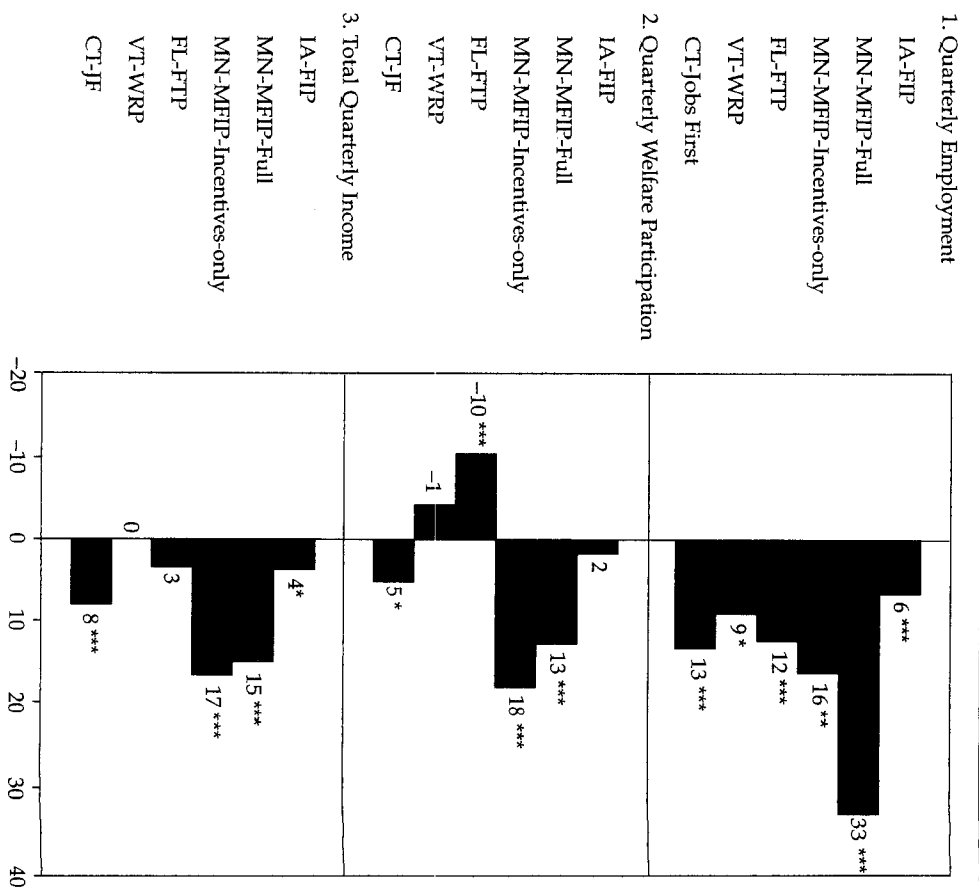
The results for children's insurance coverage, presented in figure 9.4 and table 9.7, show small (1 to 2 percent) and insignificant impacts on any insurance coverage. Similar to the results for adults, any insurance and public-insurance coverage increase for CT-JF and MN-MFIP (as well as for IA-FIP) and decrease for the other states (VT-WRO and FL-FTP). However, the effects are smaller and fewer are significant compared to the adults. We would expect smaller impacts on child coverage given the other available public-insurance programs.<sup>25</sup> Again, the measure of any spells of uninsurance for any child shows positive effects for Minnesota (that is, negative estimates).

Figure 9.5 and table 9.8 present estimates for utilization, access, and affordability of care for the sample of focal children ages five to twelve (and for their families). Few of the estimates are significant, and for most variables there are an equal number of positive and negative estimates. For example, the variable "focal child has seen a doctor in the past two years" has one significant positive estimate; the rest are insignificant and very close to zero. There are some large negative estimates for the outcome "someone in the family could not afford to see a dentist or doctor." However, none of these are significant. Further, one might expect smaller decreases (or increases) in utilization in states with smaller decreases (or increases) in insurance. No such pattern emerges from this figure.

Finally, figure 9.6 and table 9.9 present the results for health outcomes for the focal-child sample, including the mother's risk for depression (a positive effect is

(text continues on page 264)

FIGURE 9.2 / Impacts of Welfare Reform on Employment, Welfare, and Income from Experimental Studies: Averages from Random Assignment to Quarter of Survey (Percent Effects)



Source: Authors' compilation of public use data. Note: The impacts are reported for quarterly averages from the time of random assignment through the quarter when the survey was conducted. For CT-JF, the survey was done thirty-six months after random assignment began; for VT-WRP, forty-two months; for FL-FTP, forty-eight months; for MN-MFIP, thirty-six months; and for IA-FIP, five to six years (we report the six-year average). Effect sizes reported are the treatment-control difference divided by the control mean. Significance levels (\*\*\*) 1 percent, \*\* 5 percent, and \* 10 percent) are for treatment-control differences.

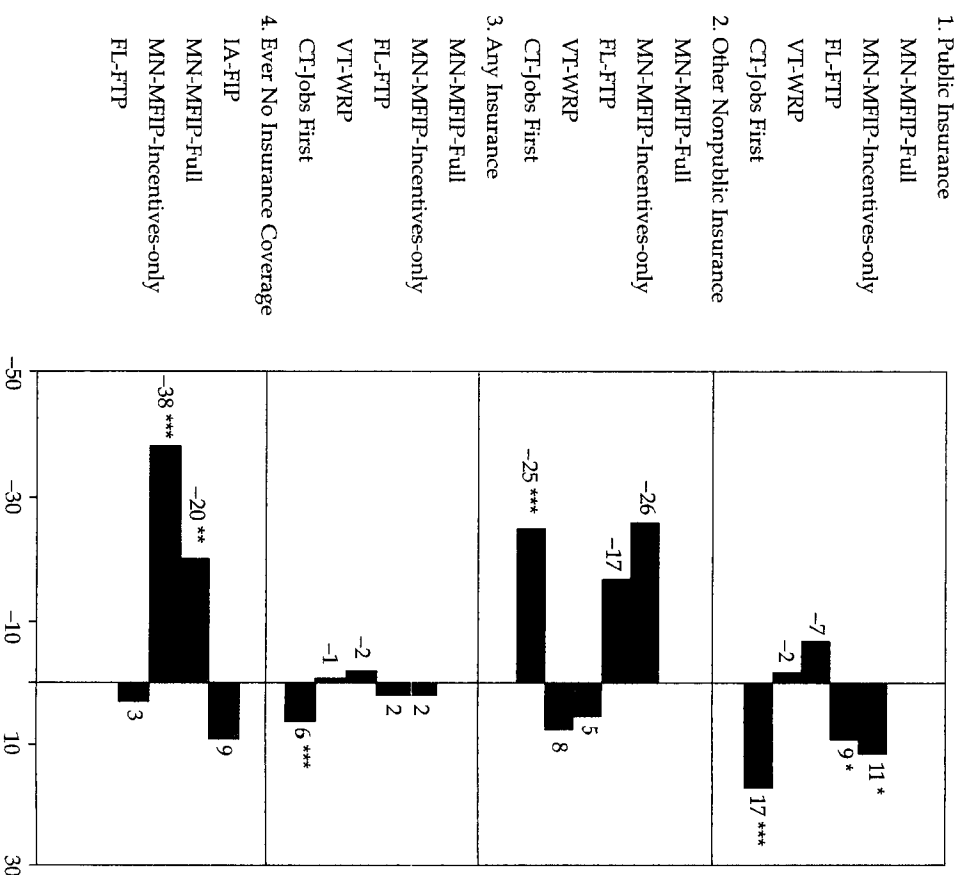
ABLE 9.5 / Impacts on Employment, Welfare, and Income, Averaged over Period from Random Assignment to Survey

	Difference	Std. Err., Difference	Mean (Controls)	Std. Dev. (Controls)	Percent Effect	Effect Size	N
<b>Quarterly employment</b>							
IA-FIP	0.033***	0.010	0.52	0.35	6.37%	0.095	7,823
MN-MFIP-Full	0.132***	0.029	0.40	0.36	32.92%	0.372	724
MN-MFIP-Incentives	0.065**	0.030	0.40	0.36	16.17%	0.183	718
FL-FTP	0.058***	0.016	0.47	0.34	12.36%	0.169	1,729
VT-WRRP	0.043*	0.025	0.46	0.37	9.28%	0.116	842
CT-JF	0.067***	0.017	0.51	0.38	13.16%	0.174	2,397
<b>Quarterly cash welfare receipt</b>							
IA-FIP	0.008	0.009	0.47	0.34	1.68%	0.023	7,823
MN-MFIP-Full	0.091***	0.025	0.72	0.34	12.76%	0.270	724
MN-MFIP-Incentives	0.127***	0.024	0.72	0.34	17.77%	0.376	718
FL-FTP	-0.044***	0.015	0.43	0.33	-10.20%	-0.133	1,729
VT-WRRP	-0.006	0.025	0.61	0.36	-0.98%	-0.017	842
CT-JF	0.029*	0.015	0.59	0.37	4.95%	0.079	2,397
<b>. Average quarterly income</b>							
IA-FIP	83.23*	46.66	2215.24	1651.09	3.76%	0.050	7,823
MN-MFIP-Full	366.82***	88.79	2443.30	1133.39	15.01%	0.324	724
MN-MFIP-Incentives	404.10***	97.66	2443.30	1133.39	16.54%	0.357	718
FL-FTP	58.85	55.91	1750.35	1101.99	3.36%	0.053	1,729
VT-WRRP	-2.84	72.42	2376.29	1030.16	-0.12%	-0.003	842
CT-JF	209.93***	71.43	2658.18	1517.52	7.90%	0.138	2,397

Source: Authors' compilations of public use data.

Note: Shown are average quarterly employment rates, averages for any cash welfare receipt during quarter to be comparable to the employment rates), and average quarterly income (cash welfare plus food stamps plus general assistance [MN only] plus earnings) for the period from random assignment to the quarter during which the survey was done (except for IA, for which it is an average over the entire follow-up period). Statistics are for all observations completing the adult survey that also had data for the full period, except for Iowa, where they are for approximately the same cohorts as the survey data (the IA public-use data does not contain the appropriate information to link the survey and administrative records). Numbers are weighted to be representative of survey design where relevant. Standard errors are robust to heteroskedasticity. Percent effect is one hundred times the treatment-control difference divided by control mean (also shown in figure 9.1), effect size is treatment-control difference divided by control standard deviation. Significance levels (\*\*\*) 1 percent, \*\* 5 percent, and \* 10 percent) are for treatment-control differences.

FIGURE 9.3 / Impacts of Welfare Reform on Head's Health Insurance from Experimental Studies (Percent Effects)



Source: Authors' compilation of public use data.

Note: The impacts are reported at the time of a follow-up survey administered to some recipients at some time after random assignment. For CT-JF, the survey was done thirty-six months after random assignment began; for VT-WRRP, forty-two months; for FL-FTP, forty-eight months; for MN-MFIP, thirty-six months; and for IA-FIP, five to six years. Effect sizes reported are the treatment-control difference divided by the control mean. Significance levels (\*\*\*) 1 percent, \*\* 5 percent, and \* 10 percent) are for treatment-control differences.

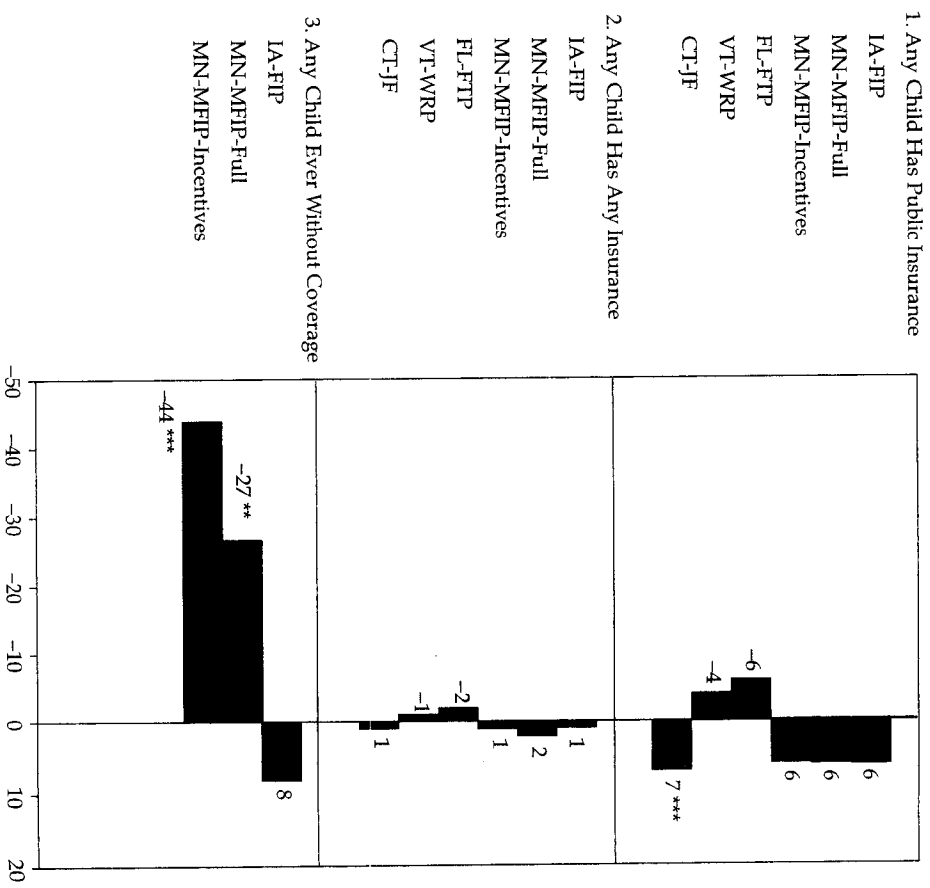
TABLE 9.6 / Impacts on Head's Health Insurance, Survey Data

	Difference	Std. Err., Difference	Mean (Controls)	Std. Dev. (Controls)	Percent Effect	Effect Size	N
<b>1. Public insurance</b>							
IA-FIP	NA	0.038	0.65	0.48	11.07%	0.152	712
MN-MFIP-Full	0.072*	0.038	0.65	0.48	8.93%	0.122	709
MN-MFIP-Incentives	0.058	0.023	0.37	0.48	-6.77%	-0.052	1,725
FL-FTP	-0.025	0.032	0.70	0.46	-1.71%	-0.026	840
VT-WRP	-0.012	0.021	0.60	0.49	16.69%	0.203	2,418
CT-JF	0.099***	0.021					
<b>2. Other nonpublic insurance</b>							
IA-FIP	NA	0.028	0.17	0.38	-25.77%	-0.117	707
MN-MFIP-Full	-0.044	0.028	0.17	0.38	-16.86%	-0.076	704
MN-MFIP-Incentives	-0.029	0.021	0.25	0.43	5.24%	0.030	1,723
FL-FTP	0.013	0.024	0.14	0.35	7.84%	0.031	837
VT-WRP	0.011	0.018	0.22	0.41	-25.23%	-0.133	2,402
CT-JF	-0.055***						
<b>3. Any insurance</b>							
IA-FIP	NA	0.030	0.84	0.37	1.82%	0.042	708
MN-MFIP-Full	0.015	0.030	0.84	0.37	1.96%	0.045	705
MN-MFIP-Incentives	0.016	0.023	0.62	0.49	-1.78%	-0.023	1,723
FL-FTP	-0.011	0.025	0.84	0.37	-0.71%	-0.016	837
VT-WRP	-0.006	0.017	0.82	0.39	5.65%	0.119	2,403
CT-JF	0.046***						
<b>4. Ever no insurance coverage</b>							
IA-FIP	0.049	0.032	0.54	0.50	9.13%	0.098	1,190
MN-MFIP-Full	-0.079**	0.039	0.39	0.49	-20.15%	-0.161	723
MN-MFIP-Incentives	-0.149***	0.037	0.39	0.49	-38.15%	-0.305	717
FL-FTP	0.011	0.023	0.38	0.49	2.87%	0.023	1,729
VT-WRP	NA						
CT-JF	NA						

Source: Authors' compilations of public use data.

Note: Shown are survey estimates of insurance coverage for the recipient for month before survey, or of having had any spell of non-coverage since random assignment. Statistics are for all observations completing the adult survey that had data for the outcome. Numbers are weighted to be representative of survey design where relevant. Standard errors are robust to heteroskedasticity. Percent effect is one hundred times the treatment-control difference divided by control mean (also shown in figure 9.2), effect size is treatment-control difference divided by control standard deviation. Significance levels (\*\*\* 1 percent, \*\* 5 percent, and \* 10 percent) are for treatment-control differences.

FIGURE 9.4 / Impacts of Welfare Reform on Child or Family Health Insurance from Experimental Studies (Percent Effects)



Source: Authors' compilation of public use data.

Note: The impacts are reported at the time of a follow-up survey administered to some recipients at some time after random assignment. For CT-JF, the survey was done thirty-six months after random assignment began, for VT-WRP, forty-two months; for FL-FTP, forty-eight months; for MN-MFIP, thirty-six months; and for IA-FIP, five to six years. Effect sizes reported are the treatment-control difference divided by the control mean. Significance levels (\*\*\* 1 percent and \*\* 5 percent) are for treatment-control differences. Values for IA are for any coverage within the family; those for other states are for any coverage for any child.

TABLE 9.7 / Impacts on Child or Family Health Insurance, Survey Data

	Difference	Std. Err. Difference	Mean (Controls)	Std. Dev. (Controls)	Percent Effect	Effect Size	N
<b>1. Public insurance</b>							
IA-FIP	0.031	0.033	0.49	0.50	6.32%	0.062	1,106
MN-MFIP-Full	0.045	0.036	0.72	0.45	6.34%	0.100	697
MN-MFIP-Incentives	0.044	0.036	0.72	0.45	6.14%	0.097	696
FL-FIP	-0.037	0.026	0.61	0.49	-6.06%	-0.076	1,471
VT-WRRP	-0.029	0.029	0.82	0.39	-3.58%	-0.076	774
CT-JF	0.055***	0.019	0.78	0.42	7.14%	0.132	2,135
<b>2. Any insurance</b>							
IA-FIP	0.006	0.026	0.80	0.40	0.80%	0.016	1,105
MN-MFIP-Full	0.017	0.027	0.86	0.34	1.97%	0.049	698
MN-MFIP-Incentives	0.008	0.029	0.86	0.34	0.90%	0.022	697
FL-FIP	-0.017	0.020	0.82	0.38	-2.07%	-0.045	1,468
VT-WRRP	-0.013	0.022	0.90	0.30	-1.45%	-0.044	772
CT-JF	0.005	0.010	0.95	0.22	0.57%	0.025	2,141
<b>3. Any child ever without coverage</b>							
IA-FIP	0.035	0.034	0.43	0.50	8.12%	0.071	1,004
MN-MFIP-Full	-0.094**	0.038	0.35	0.48	-27.08%	-0.197	698
MN-MFIP-Incentives	-0.154***	0.036	0.35	0.48	-44.32%	-0.323	697
FL-FIP	NA	NA	NA	NA	NA	NA	NA
VT-WRRP	NA	NA	NA	NA	NA	NA	NA
CT-JF	NA	NA	NA	NA	NA	NA	NA

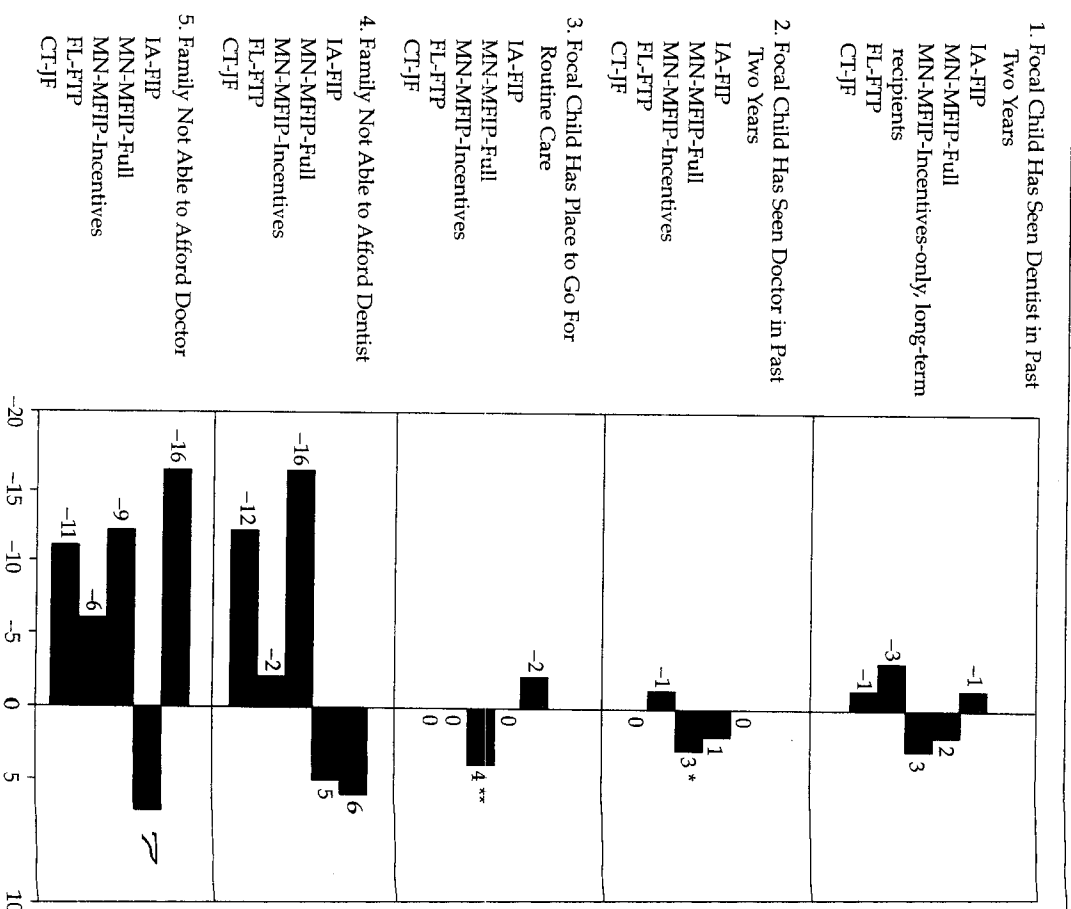
Source: Authors' compilations of public use data.

Note: Shown are survey estimates of insurance coverage for any child of the recipient for the month before observations, or of any child having had any spell of non-coverage since random assignment. Statistics are for all observations completing the adult survey that had data for the outcome and had a child in their household at the time of the survey. Numbers are weighted to be representative of survey design where relevant. Standard errors are robust to heteroskedasticity. Percent effect is one hundred times the treatment-control difference divided by control mean (also shown in figure 9.3), effect size is treatment-control difference divided by control standard deviation. Significance levels (\*\*\* 1 percent and \*\* 5 percent) are for treatment-control differences.

an adverse impact in this case), the child having behavioral problems (a positive effect is an adverse impact in this case), and for the parent reporting that the child was in excellent or very good health (a positive effect is a good outcome in this case).<sup>26</sup> These estimates consistently indicate that welfare reform leads to improvements in health status, although few estimates are statistically significant. For example, four of five estimates indicate that the risk of maternal depression decreases (the exception is CT-JF); four of five estimates indicate that the child behavior index improves (the exception is FL-FIP); and three of five estimates indicate that child health status improves (the exceptions are IA-FIP and MN-MFIP-Full). Again, the improvements tend to be most systematic for the most generous

(text continues on page 268)

FIGURE 9.5 / Impact of Welfare Reform on Child and Family Health-Care Utilization, Access and Affordability of Care from Experimental Studies (Percent Effects)



Source: Authors' compilation of public use data.

Note: The impacts are reported at the time of a follow-up survey administered to some recipients at some time after random assignment. For CT-JF, the survey was done thirty-six months after random assignment began; for VT-WRRP, forty-two months; for FL-FIP, forty-eight months; for MN-MFIP, thirty-six months; and for IA-FIP, five to six years. Effect sizes reported are the treatment-control difference divided by the control mean. Significance levels (\*\* 5 percent and \* 1 percent) are for treatment-control differences. Outcomes in panels 1, 2, and 3 are for focal child, those in panels 4 and 5 are for the family but for sample of focal children.

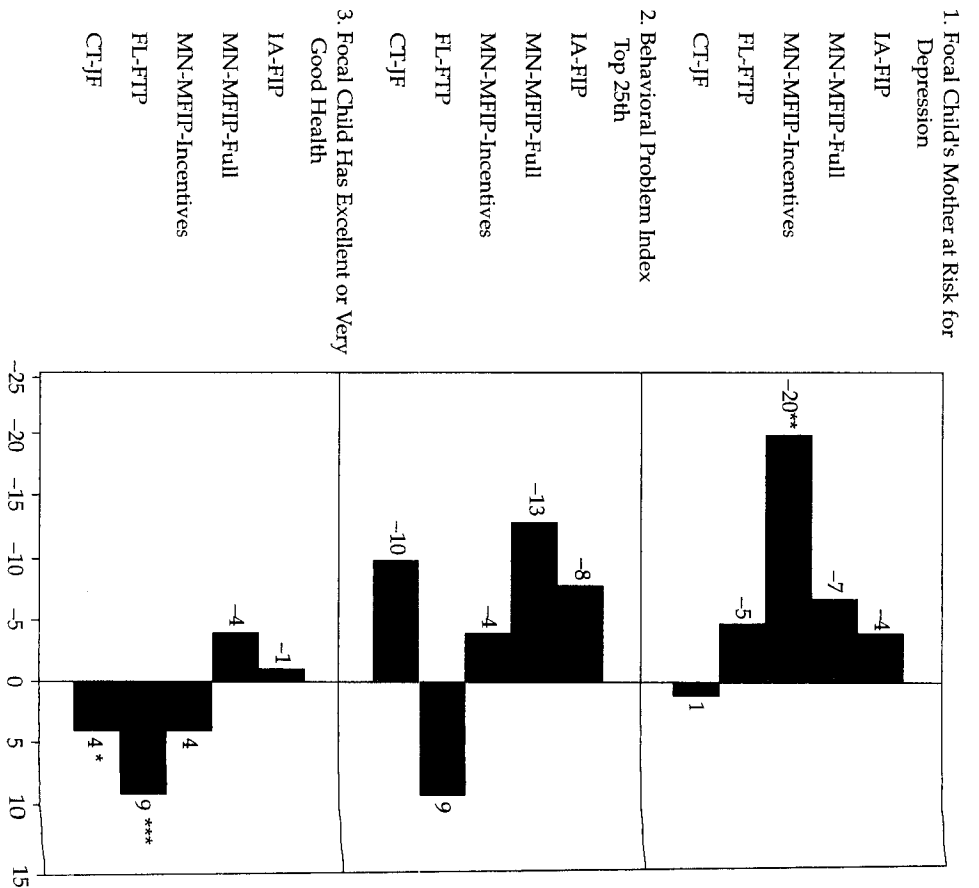
TABLE 9.8 / Impacts on Child and Family Health-Care Utilization, Access, and Affordability of Care: Survey Data

	Difference	Std. Err., Difference	Mean (Controls)	Std. Dev. (Controls)	Percent Effect	Effect Size	N
Focal child has seen dentist in past two years							
IA-FIP	-0.005	0.021	0.93	0.25	-0.54%	-0.020	683
MN-MFIP-Full	0.022	0.025	0.89	0.31	2.47%	0.071	570
MN-MFIP-Incentives	0.029	0.024	0.89	0.31	3.25%	0.094	558
FL-FIP	-0.023	0.023	0.85	0.36	-2.68%	-0.064	1,063
VT-WRP	NA						
CT-JF	-0.013	0.012	0.96	0.21	-1.41%	-0.065	1,459
Focal child has seen doctor in past two years							
IA-FIP	0.004	0.014	0.97	0.17	0.40%	0.023	683
MN-MFIP-Full	0.008	0.018	0.95	0.22	0.79%	0.034	570
MN-MFIP-Incentives	0.027*	0.016	0.95	0.22	2.83%	0.121	559
FL-FIP	-0.012	0.011	0.97	0.16	-1.22%	-0.072	1,065
VT-WRP	NA						
CT-JF	0.002	0.004	0.99	0.07	0.16%	0.021	1,461
Focal child has place to go for routine care							
IA-FIP	-0.021	0.015	0.97	0.17	-2.14%	-0.123	682
MN-MFIP-Full	0.001	0.019	0.95	0.23	0.11%	0.005	570
MN-MFIP-Incentives-only	0.034 <sup>†</sup>	0.016	0.95	0.23	3.59%	0.149	559
FL-FIP	0.004	0.018	0.90	0.30	0.41%	0.012	1,067
VT-WRP	NA						
CT-JF	-0.004	0.006	0.99	0.11	-0.37%	-0.035	1,460
Family not able to afford dentist							
IA-FIP	0.009	0.031	0.17	0.37	5.62%	0.025	682
MN-MFIP-Full	0.010	0.033	0.20	0.40	5.19%	0.026	587
MN-MFIP-Incentives	-0.031	0.032	0.20	0.40	-16.01%	-0.079	573
FL-FIP	-0.007	0.029	0.35	0.48	-2.14%	-0.016	1,107
VT-WRP	NA						
CT-JF	-0.019	0.019	0.17	0.37	-11.51%	-0.051	1,468
Family not able to afford doctor							
IA-FIP	-0.017	0.025	0.11	0.31	-15.68%	-0.055	682
MN-MFIP-Full	0.009	0.028	0.13	0.33	7.14%	0.027	587
MN-MFIP-Incentives	-0.012	0.027	0.13	0.33	-9.11%	-0.035	573
FL-FIP	-0.014	0.025	0.22	0.42	-6.43%	-0.035	1,107
VT-WRP	NA						
CT-JF	-0.014	0.017	0.12	0.33	-11.17%	-0.042	1,469

Source: Authors' compilations of public-use data.

Note: Shown are survey estimates for the focal child of the recipient of having seen a doctor or dentist during the *last* two years before the survey, having a place to go for routine care, and, for the focal-child sample, whether the family had someone who could not see a doctor or dentist because they could not afford it during the last year. Statistics are for all observations completing the focal-child survey that had data for the outcome. Numbers are reweighted to be representative of survey design where relevant. Standard errors are robust to heteroskedasticity. Percent effect is one hundred times the treatment-control difference divided by control mean (also shown in figure 9.4); effect size is treatment-control difference divided by control standard deviation. No focal-child survey was completed in Vermont. Focal-child sample is children 5 to 12. Significance levels (\* 10 percent) are for treatment-control differences.

FIGURE 9.6 / Impacts of Welfare Reform on Child and Mother Health Outcomes from Experimental Studies (Percent Effects)



Source: Authors' compilation of public use data.

Note: The impacts are reported at the time of a follow-up survey administered to some recipients at some time after random assignment. For CT-JF, the survey was done thirty-six months after random assignment began; for VT-WRP, forty-two months; for FL-FIP, forty-eight months; for MN-MFIP, thirty-six months; and for IA-FIP, five to six years. Effect sizes reported are the treatment-control difference divided by the control mean. Significance levels (\*\*\*) 1 percent, \*\* 5 percent, and \* 10 percent) are for treatment-control differences.

TABLE 9.9 \ Impacts on Child and Mother Health Outcomes, Survey Data

	Difference	Std. Err. Difference	Mean (Controls)	Std. Dev. (Controls)	Percent Effect	Effect Size	N
<b>1. Focal child's mother at risk for depression</b>							
IA-FIP	-0.012	0.038	0.30	0.46	-3.88%	-0.025	676
MN-MFIP-Full	-0.036	0.044	0.55	0.50	-6.51%	-0.072	525
MN-MFIP-Incentives	-0.112**	0.044	0.55	0.50	-20.27%	-0.226	507
FL-FITP	-0.018	0.029	0.39	0.49	-4.70%	-0.038	1,091
VT-WRRP	NA						
CT-JF	0.005	0.025	0.34	0.47	1.45%	0.010	1,436
<b>2. Behavioral Problem Index in top 25th percentile</b>							
IA-FIP	-0.023	0.037	0.28	0.45	-8.27%	-0.052	683
MN-MFIP-Full	-0.038	0.040	0.30	0.46	-12.73%	-0.083	510
MN-MFIP-Incentives	-0.012	0.041	0.30	0.46	-4.13%	-0.027	493
FL-FITP	0.023	0.027	0.26	0.44	8.70%	0.052	1,100
VT-WRRP	NA						
CT-JF	-0.028	0.023	0.28	0.45	-9.92%	-0.063	1,450
<b>3. Focal child has excellent or very good health</b>							
IA-FIP	-0.012	0.029	0.85	0.36	-1.39%	-0.033	683
MN-MFIP-Full	-0.029	0.036	0.78	0.42	-3.74%	-0.070	570
MN-MFIP-Incentives	0.031	0.034	0.78	0.42	4.01%	0.075	559
FL-FITP	0.069***	0.026	0.73	0.45	9.43%	0.154	1,068
VT-WRRP	NA						
CT-JF	0.033*	0.020	0.81	0.39	4.11%	0.086	1,466

Source: Authors' compilations of public use data.

Note: Shown are survey estimates for the focal child of the recipient of whether the mother was at risk for depression (score of sixteen or higher on twenty-item Center for Epidemiological Studies-Depression scale; worst score was 60), whether the focal child's Behavioral Problem Index score was in the worst twenty-fifth percentile range, and whether the mother reported the child's general health was "excellent" or "very good." Statistics are for all observations completing the focal-child survey that had data for the outcome. Numbers are weighted to be representative of survey design where relevant. Standard errors are robust to heteroskedasticity. Percent effect is one hundred times the treatment-control difference divided by control mean (also shown in figure 9.5), effect size is treatment-control difference divided by control standard deviation. No focal-child survey was completed in Vermont. Focal-child sample is children 5 to 12. Significance levels (\*\*\*) 1 percent, \*\* 5 percent, and \* 10 percent) are for treatment-control differences.

reforms: MN-MFIP has the largest improvements (perhaps related to their large increases in income) and CT-JF is close behind.

Given that we estimate effects for many outcomes, we need to be concerned about the possibility that the separate tests sometimes wrongly reject the null hypothesis of no impact. That is, we would expect some number of our many treat-

ment-control comparisons to differ at a statistically significant level simply because of randomness. With the many comparisons we present, the probability of falsely rejecting at least one null hypothesis of no significant difference in means across the treatment and control group is much higher than each individual test would suggest. To address this concern about multiple inference, we also constructed summary measures for the types of outcomes within each table for each state. These allow us to test the effect of the treatment on each set of outcomes. For each set of outcomes (for example, quarterly employment, welfare receipt, and income since random assignment for figure 9.2), the summary measure is defined as the average of the standardized outcomes (after having converted all outcomes to be positive when they are good and normalizing them by the control-group standard deviation). So, for the outcomes in figure 9.2, the summary measure would be the average of the quarterly employment, quarterly income, and one minus quarterly welfare receipt (assuming, as per the intent of reform, that ongoing welfare receipt is a negative thing), each normalized by its control-group standard deviation. This new averaged variable is then regressed on treatment status for each state. Tests on this summary measure are then robust to over-testing (one is less likely to inappropriately reject a null hypothesis of no effect with the summary measure than if one looked independently at the significance levels of the constituent individual tests).

This does not entirely resolve the issue of multiple inference because there are still five such summary measures. It is important to consider hypotheses about each of these summary measures as members of families of hypotheses. This involves calculating cutoffs for test statistics such that the probability is less than a set amount (for example, 0.05) that at least one of the tests in the family would exceed the cutoff under a joint null hypothesis of no effects (the probability of falsely rejecting one null hypothesis). One such familiar but quite conservative test (if the test statistics are highly correlated) is the Bonferroni adjustment, in which the adjusted p-value is the observed p-value times the number of outcomes examined. Such a conservative test may result in not rejecting the null hypotheses of no effect even when there are some significant differences. More powerful tests remove hypotheses from the family of nulls if they are rejected and also produce adjusted p-values. An alternative method from the biostatistics literature used in recent papers (Kling and Liebman 2004; Kling, Liebman, and Katz 2007; Anderson 2005) involves calculating family-wise error-adjusted significance levels, using the Westfall and Young free step-down resampling method (Westfall and Young 1999).<sup>27</sup> We have also implemented this method to adjust our summary measure p-values for the multiple inference, using one thousand draws from the null distribution of no impact of each summary measure (for more details, see algorithm 2.8 in Westfall and Young 1993).

The results of our five summary measures for each of the states are reported in table 9.10. The table reports the treatment-control difference in summary measures for each state and figure, along with the standard error, the family-wise error adjusted p-values for each state, and the number of observations for each summary measure.<sup>28</sup> Each summary measure is for a single table and state; each averages all

normalized reported outcomes for that treatment. The normalized outcomes are then all for positive outcomes (so the summary measure treatment-control difference is positive if the reform caused an improvement in the summary measure). For the employment, welfare, and income summary measure (figure 9.2), lack of welfare receipt is considered "good." For the adult health insurance coverage as well as the child and family health insurance coverage summary measures (figures 9.3 and 9.4), public coverage and the lack of any spells of coverage are considered "good." For the health care utilization summary measure (figure 9.5), being able to afford to see the doctor or dentist is considered "good." Finally, for the health status summary measure (figure 9.6), it is considered "good" if the child's mother is not at risk for depression and the child does not have a high Behavioral Problem Index measure.

Adjusting for the family-wise error rate definitely makes a difference in the overall interpretation of the results. For example, for the figure 9.2 summary measure (panel 1 of table 9.10), the treatment-control differences for IA-FIP, MN-MFIP-Full, FL-FITP, and CT-JF are all positive and significant at the 5 percent level if the p-value is unadjusted for the multiple testing (significance levels not shown in table). However, when multiple inference is controlled for, only FL-FITP and CT-JF have significant treatment-control differences in the summary measure, and only FL-FITP's is significant at the 5 percent level. For the adult health insurance measures in figure 9.3, the summary measure treatment-control difference is only statistically significant for CT-JF (and it is positive, suggesting an improvement in health insurance coverage for the head). None of the child and family health insurance summary measures (figure 9.4) are significant, although both the MN-MFIP-Incentives and CT-JF measures are both positive and come close to statistical significance ( $p = 0.107$  and  $0.103$  respectively). Again, none of the figure 9.5 or figure 9.6 summary measure treatment-control differences are statistically significant, although all but one are positive. Thus, considering all the measures within each domain suggests a similar interpretation to the one derived from considering them one at a time. CT-JF had a positive and significant effect on income, employment, and leaving welfare, as well as on better adult insurance-coverage outcomes. Effects for child and family insurance, utilization, and health status are small and insignificant in general.

### CONCLUSION

While there is a growing literature on the impacts of welfare reform on health insurance coverage, health care utilization, and health status, there are few clear findings. Most studies find that welfare reform leads to reductions in health insurance coverage, although some studies find the opposite. Results for utilization and health status are more mixed, but the balance is toward negative impacts which are small and rarely statistically significant.

To illustrate the findings in the literature review, we calculate estimates from five experimental evaluations of state welfare waivers (Connecticut, Florida, Iowa, Minnesota, and Vermont). We chose these five states because they had the best and

TABLE 9.10 / Summary Measure Impacts on Adult, Child, and Family Measures, Survey Data

	Difference	Std. Err. Difference	FWE Adjusted P-Value	N
<b>1. Summary measure: employment off welfare, and income, since RA</b>				
IA-FIP	0.041	0.020	0.181	7,823
MN-MFIP-Full	0.113	0.051	0.115	724
MN-MFIP-Incentives	0.021	0.054	0.698	718
FL-FITP	0.110***	0.030	0.000	1,729
VT-WRRP	0.046	0.048	0.671	842
CT-JF	0.078*	0.034	0.099	2,397
<b>2. Summary measure: head's HI coverage</b>				
IA-FIP	-0.049	0.032	0.409	1,190
MN-MFIP-Full	0.070	0.059	0.551	707
MN-MFIP-Incentives	0.118	0.059	0.125	704
FL-FITP	-0.021	0.051	0.900	1,723
VT-WRRP	-0.007	0.038	0.849	837
CT-JF	0.060*	0.025	0.099	2,402
<b>3. Summary measure: child/family HI coverage</b>				
IA-FIP	0.001	0.050	0.993	1,105
MN-MFIP-Full	0.114	0.062	0.235	697
MN-MFIP-Incentives	0.144	0.107	0.107	696
FL-FITP	-0.066	0.053	0.489	1,468
VT-WRRP	-0.052	0.060	0.671	771
CT-JF	0.067	0.032	0.103	1,134
IA-FIP	-0.013	0.045	0.993	681
<b>4. Summary measure: child/family utilization, access, and affordability</b>				
MN-MFIP-Full	0.015	0.058	0.857	570
MN-MFIP-Incentives	0.117	0.055	0.123	558
FL-FITP	-0.018	0.045	0.900	1,060
VT-WRRP	NA			
CT-JF	0.006	0.024	0.801	1,453
<b>5. Summary measure: child/mother health outcomes</b>				
IA-FIP	0.015	0.054	0.993	676
MN-MFIP-Full	0.030	0.062	0.857	509
MN-MFIP-Incentives	0.120	0.062	0.125	492
FL-FITP	0.065	0.044	0.435	1,048
VT-WRRP	NA			
CT-JF	0.051	0.036	0.263	1,421

Source: Authors' compilations of public use data.

Note: Shown are survey estimates for summary measures for each state for each of the variables presented in figures 9.2 to 9.6. Each summary measure is the average of the outcomes on each figure (normalized by each outcome's control standard deviation), after converting each outcome to be positive when good (welfare participation is considered bad, but any kind of Health Insurance good). For the figure 9.2 summary measure, the sample is adults completing the survey with non-missing administrative data (for IA only, it is instead the same cohort as the survey). For the figure 9.3 summary measure, the sample is adults completing the survey with non-missing health insurance data. For the figure 9.4 summary measure, the sample is adults with a child in the household at the time of the survey completing the survey with non-missing child/family health insurance coverage data. For the figures 9.5 and 9.6 summary measures, the sample is survey recipients with a focal child completing the survey, with non-missing data on health care utilization, access, and affordability, or on health outcomes, respectively. Numbers are weighted to be representative of survey design where relevant. Standard errors are robust to heteroskedasticity. Difference is treatment-control difference in each summary measure. FWE adjusted p-value is p-value for comparison in row, adjusted for joint testing across all summary measures in the state. No focal-child survey was completed in Vermont. Focal child sample is children 5 to 12. Significance levels (\*\*\*) 1 percent and \* 10 percent) are for treatment-control differences, adjusted for family-wise errors.



most comprehensive data on health and they included the states with the most "TANF-like" welfare waiver policies. For example, Connecticut and Florida had time limits, which have proved to be very important features of the TANF program. Overall, the results suggest that reform leads to small changes in health insurance and possible improvements in health. The results for health care utilization are less conclusive.

A major limitation of this chapter reflects a weakness in the literature: we know a lot about the impact of welfare reform on health insurance, but we know little about the impact of welfare reform on health. The major challenge limiting the existing literature is obtaining the data that is required for a more comprehensive analysis. The experimental literature is restricted to looking at health outcomes, about which information was collected during surveys administered to participants. These surveys asked about health insurance coverage in some detail, included a few questions on health care utilization, and had quite limited information on health status (which, like other outcomes, was self-reported). Ideally, these experimental surveys would have collected before and after data from objective evaluations of participants' health. Yet the experimental surveys did not collect data on many important health outcomes of interest, such as whether the children are suffering from developmental delays, asthma, or chronic ear conditions; whether the children or recipients are obese or overweight; whether recipients suffer from substance abuse or sexually transmitted diseases; whether they have negative health behaviors, such as smoking; whether they have encountered domestic violence; or whether they have chronic conditions, such as asthma, hypertension, or diabetes. A number of health surveys do collect information on these outcomes or others of interest, but they either do not contain information allowing one to identify whether women are in a group likely to be affected by reform, do not contain information for a consistent panel of states and years spanning reform, or do not have large enough samples to plausibly identify the effects of reform. These features are required to evaluate welfare reform using observational data where it is necessary to make comparisons across groups facing different policies in different states at different times.

An additional limitation is that many of these most important health outcomes are ones which do not change very quickly as other conditions change. Thus, despite having been collected approximately three or four years after reform, the experimental surveys may still have been collected too soon to capture reform-related changes in these outcomes. Further, these delays in the impacts of policies make it difficult to attribute changes to reform in observational data sets.

With these caveats, we have several important conclusions from our analysis of the experimental data and our reading of the broader literature. First, work-promoting reforms do not necessarily lead to bad outcomes. There is little evidence that reforms led to significant reductions in health care utilization or worse health. Second and more speculatively, the *type* of welfare reform likely matters. Reforms that encouraged work while increasing benefits (such as those in Minnesota or Connecticut)—even in the presence of work requirements and time limits—may lead to more consistent positive impacts on health. Finally, investments in data collection resulted in important improvements and increases in our knowledge. Our

analysis (and many others' analyses) could not have been done without the randomized experimental data and the additional resources spent on surveys that provide a rich set of health, education, and well-being outcomes.

We have much to learn about the impacts of welfare reform on health. The study here is at best a short- to medium-term analysis, and thus it may be too early to inform us about the full impacts of reform. However, we need the appropriate data in order to complete this task. For example, one could field follow-up surveys to these experimental samples. There are other ways to improve access to data that facilitate both experimental and nonexperimental research. There are limited options for using household survey data to estimate the impacts of a state-varying policy such as welfare reform. The CPS is an option, but it only provides information on health insurance and, for some years, health status. The SIPP has richer outcomes but smaller samples, and no SIPP panel spanned all key reform periods. The NHIS is the obvious choice, but despite sample sizes comparable to the CPS, the public-use version of the NHIS data does not identify individual states; furthermore, the NHIS went through a redesign in the midst of TANF implementation. The BRFSS is another possibility, but the samples are smaller; also, there is no health information collected about children. Non-health surveys need to be expanded to include more information about health outcomes. Some health surveys might benefit from expanding the population surveyed, asking more about previous and concurrent use of programs; or including health outcomes that might respond to shorter-run changes in income, employment, or program participation. An example of the underlying demand for large-scale data with state identifiers and health outcomes is evidenced by the explosion of studies that use Vital Statistics natality data on births in the United States. The rich natality data permits analysis of birthweight, gestation, prenatal care, and other birth outcomes; these data also identify states and counties. Additionally, the natality data cover essentially the full population of births after 1985 and a large share before that year, enabling study of rare birth outcomes. Finally, it would be useful for experimental evaluators to provide researchers with links to administrative data on Medicaid claims. This would enable a richer characterization of health utilization and, possibly, health conditions.

The data used in this chapter are derived from data files made available to researchers by MDRC and Mathematica Policy Research (MPR). We thank MDRC and MPR for assistance with the public use data. The authors remain solely responsible for how the data are used or interpreted.

## NOTES

1. Rebecca Blank (2002) and Jeffrey Grogger and Lynn Karoly (2005) provide recent and comprehensive reviews of the welfare-reform literature.

2. The new welfare programs include other changes as well. Especially relevant for health, many states expanded "transitional" Medicaid coverage which is received when leaving welfare.
3. Other leaver studies documented similar rates of coverage (Elliswood and Lewis 1999; Guyer 2000; Moffitt and Shade 1997; Pollack et al. 2003).
4. Family caps prevent welfare benefits from increasing when a woman gives birth while receiving aid. Residency-requirement policies mandate that unmarried teen parents who receive aid must live in the household of a parent or other guardian.
5. States could and did set up Medically Needy programs that allowed them to provide Medicaid benefits to families above the AFDC income cutoff if they had high medical expenses. States were also required to provide Transitional Medicaid coverage for families who left AFDC due to an increase in earnings.
6. The literature on welfare reform includes an ongoing debate on what has contributed to this decrease in the welfare caseload. The leading candidate, besides welfare reform itself, is the booming economy of the late 1990s. Teen pregnancy rates also began to fall before TANF implementation.
7. The RAND Health Insurance Experiments, conducted in the 1970s, provide the only experimental evidence on the impact of the generosity of health insurance coverage on health care utilization and health outcomes.
8. For example, Currie and Jonathan Gruber (1996a, 1996b) found that expansions in Medicaid are associated with increased utilization and improved health outcomes among the poor. David Card, Carlos Dobkin, and Nicole Maestas (2006) found that the introduction of Medicare led to increases in insurance coverage and utilization, with larger impacts for more disadvantaged groups.
9. Several reviews offer recent summaries of the experimental and nonexperimental studies of welfare reform and family income (Blank 2002; Grogger and Karoly 2005; Moffitt 2002).
10. When federal law was passed, there were states that adopted their waiver programs as their TANF programs. Therefore some of the welfare waiver experimental studies do, then, estimate impacts of TANF.
11. The literature on the impacts of welfare reform is quite large. Here, we focus our review on what is known about the impacts of welfare reform on health. A related literature finds that prereform public assistance programs lead to improvements in health. Currie and Nancy Cole (1993) find that AFDC participation leads to improvements in birth outcomes (higher birth weight). Currie and Grogger (2002) find that higher pre-PRWORA welfare participation rates are associated with more prenatal care and improved birth outcomes. Price Fishback, Michael Haines, and Shawn Kantor (2007) find that increases in public assistance spending during the New Deal led to lower infant mortality, lower suicide rates, fewer deaths from diarrhea and infectious diseases, and higher birth rates.
12. The NHIS is less widely used in this area because the public-use data do not contain state identifiers. It is possible, through special arrangement, for researchers to access the file with state identifiers. Kauschal and Kaestner (2007) use the NHIS to examine impacts for immigrants using a difference-in-difference methodology and characterizing reform with caseloads.

13. Large spillover effects seem to be inconsistent with the small estimated impacts of welfare reform on marriage and fertility (Bilzer et al. 2004; Grogger and Karoly 2005).
14. Royer (2005) also examines impacts on pregnant immigrants. She finds a temporary reduction in prenatal care but no effect on birth outcomes.
15. DHHS funded a number of state experimental evaluations to allow them to examine longer-term impacts of reform on various school and health outcomes for children who were two to nine years old at the time of random assignment (and thus approximately five to twelve years old at the time of the surveys).
16. In states conducting the focal-child evaluations, single parents with children of the appropriate age were oversampled for the adult survey as well. The data for these states include sample weights to make the survey data representative of the overall population in the survey. (These weights adjust for initial differences in sampling ratios for Connecticut, Iowa, and Minnesota.)
17. The table mentions the maximum number of available observations. Because of item nonresponse, the actual numbers of observations are lower than this for many outcomes. We chose to use a different number of observations for each outcome to maximize sample. Item nonresponse is generally low for most of our outcomes.
18. Some of the evaluations do not include data for two-parent families. We wanted a sample that was consistent across states to the extent possible.
19. It also has the advantage that there were no changes in the random-assignment ratios across the time period, mitigating the need for controls beyond the treatment indicator. Long-term recipients are those who were on welfare for at least twenty-four of the past thirty-six months.
20. To be precise, we include single females eighteen and older, or between sixteen and seventeen who had a preschool-aged child, and who were in a cohort randomly assigned at least fifty-seven months before the survey. The final report includes separate outcomes for ongoing recipients and applicants in three cohorts. We wanted the applicant sample to have been exposed to the new program or AFDC for as long as the ongoing recipients. Thus, we restricted the analysis to the earlier applicants. This was not an issue for the four other states, as the surveys in the other evaluations were only administered to narrow cohorts of participants. Also, in 1997, Iowa implemented TANF and applied the new TANF policies to the control group. Thus the treatment-control program differences are much smaller for later cohorts.
21. In the MTO study, persons were randomly assigned to a treatment group (which was offered a housing voucher to move to a low-poverty neighborhood) and a control group (which did not receive an offer). There is a need to examine the intent to treat for MTO because only a subset of persons in the treatment group accepted the offer. Here, everyone has applied to obtain—and been deemed eligible for—welfare, although a small share of each group does not take up welfare.
22. In this and all subsequent figures, there is a companion table that provides the supporting data and an alternative estimator.
23. The exception to this is Iowa, where it is impossible to merge the administrative and survey information with the public-use data. For Iowa, we have tried to match our survey sample as closely as possible.
24. The difference between CT-JF and FL-FTF reflects the fact that CT-JF had a much more

generous earnings disregard. Further, the CT-JF time limit was a short twenty-one months on paper; however, in practice extensions to the time limit were fairly common. Florida's time limit was also relatively short (for the "work ready," twenty-four months out of sixty), but it was more stringent than CT-JF's; meanwhile, the survey in Florida was administered four years out.

25. For example, many of these low-income children would be eligible for Medicaid via the poverty or OBRA expansions (children under age fifteen in families with income up to 100 percent of the poverty level).
26. The mother's risk for depression is determined if her score is at least sixteen (out of a possible sixty) on the twenty-item Center for Epidemiological Studies Depression scale. The child having behavioral problems is determined by whether the child's Behavioral Problem Index score was in the worst 25 percent. General health is reported on a five-point scale (excellent, very good, good, fair, or poor).
27. The Bonferroni adjustment is a one-step method. Broadly speaking, step-down methods involve ordering the p-values from smallest to largest, rejecting relevant null hypotheses, and removing the rejected nulls from the set of others being considered. Resampling is related to using the bootstrap to obtain distributions for p-values under the null hypothesis that no differences are significant.
28. The numbers of observations reported are less than the maximum possible numbers of observations because an observation will be missing if it is missing for any of the outcomes. A small share of observations is missing for each set of outcomes.

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## Chapter 10

# The Effects of Welfare and Child Support Policies on Maternal Health and Well-Being

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In 1996 the U.S. Congress passed the Personal Responsibility and Work Opportunities Reconciliation Act (PRWORA), substantially reducing a family's rights to income support. PRWORA removed the entitlement to government-provided cash assistance and increased states' incentives to reduce welfare caseloads. At the same time it increased private responsibilities by encouraging greater work effort from mothers and more child support payments from nonresident fathers.

The PRWORA provisions raised concerns within the medical community and among other advocates interested in the health and well-being of at-risk families. The changes to cash welfare and child support policies had potential direct and indirect consequences for women's health. Most directly, by removing the entitlement to welfare, many feared that poor women would lose their health insurance coverage. While PRWORA included a provision to hold Medicaid eligibility constant, the administrative barriers to implementation for program staff and the confounding new rules suggested that many eligible women might lose coverage.

Less direct effects of welfare reform on maternal health were also of concern. Advocates feared that increased work requirements and stronger child support enforcement might increase maternal stress, leading to increases in mental health problems. They also expressed concern that stronger child support enforcement might expose mothers to more violence from fathers while stricter welfare requirements might make it harder for mothers to escape violent partners (Kaplan 1997). Finally, advocates feared that substance abusers and women with mental health problems would be disproportionately harmed by the new policies (American Psychological Association 2001; Metsch and Pollack 2005).

Research to date has generally focused on the impact of welfare reform on the health insurance coverage and health care utilization of low-income women. Some