Intergenerational Effects of Welfare Reform on Educational Attainment

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Abstract

This paper estimates the impact of the fundamental welfare reforms of the 1990s on the educational attainment of children in low-income families. Using administrative records and individual survey data spanning the early 1990s to the mid-2000s, we find large positive effects of welfare reform: income gaps in school enrollment and dropout rates narrow by more than 20 percent. Unlike the significant and growing relative gains in the years following state welfare reforms, we find no evidence of relative gains for low-income adolescents in the years preceding the reforms. These findings are robust under alternative definitions of the treatment and control groups and after controlling for contemporaneous economic and policy changes.

1. Introduction

It is a widely held policy belief that improving the schooling of children from low-income families is an essential step toward severing intergenerational links between poverty and dependence on public assistance (Pepper 2000; Solon 1992). A corollary is that social programs aimed at improving the well-being of low-income families should be evaluated in large part according to their educational effects. This paper evaluates the impact of the welfare reforms of the 1990s—arguably, the most dramatic reforms in the history of the U.S. welfare system—

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on the educational attainment of children from low-income families, a long-term outcome that has been omitted from most evaluations.

The dramatic changes to the U.S. welfare system embodied in the state welfare waivers of the early 1990s and in the federal Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (Pub. L. No. 104-193) were aimed at promoting adult employment and reducing long-term dependence on public assistance. While the details varied across states, common policy innovations included time limits, job subsidies, work requirements, and increased funding for child support. Most of the existing scholarly work in the welfare reform literature has focused on adults and found substantial increases in the labor force participation of single mothers and reductions in public assistance case loads (Blank 2002; Moffitt 2003; Grogger and Karoly 2005). These observed changes in maternal behavior, along with potential changes in family structure, parenting quality, and self-motivation, can lead to considerable changes in the educational investments for and schooling outcomes of children in low-income families.

This paper is the first to measure the impact of welfare reform on the educational attainment of children in low-income families using large, nationally representative samples. Our outcome measures span the period from the early 1990s to the mid-2000s and are drawn from two sources. Our primary attainment measures are constructed using the Common Core of Data (CCD), an administrative census of school district records from the Department of Education (National Center for Education Statistics 1991–2003). In a robustness analysis, we confirm the main findings using self-reported enrollment rates for children ages 13–18 in the Current Population Survey (CPS).

We estimate the net effect of welfare reform in a generalized difference-in-differences framework in which trends in the educational attainment of children in higher income households are used to impute counterfactual trends for what would have happened to youths in low-income households absent welfare reform. In our initial analysis, we associate state welfare reforms with improved educational outcomes for low-income children. We then extend the analysis to consider dynamic effects and to test for preexisting trends. We find statistically indistinguishable time trends for low-income and higher income children before welfare reform but significant and growing relative gains for low-income children after welfare reform. This finding is robust to alternative definitions of low- and higher income groups and to controlling for the adoption of school accountability programs—the most important contemporaneous policy change likely affecting low-income students differentially.

This paper contributes to the new literature on the educational effects of welfare reform on several fronts. First, we provide the first evidence of beneficial effects for both male and female adolescents. Evaluations of the welfare reform experiments find evidence of improvements in both self-reported and teacher-reported academic achievement measures and test scores for young children randomized into reform groups (Duncan and Chase-Lansdale 2001; Zaslow et

al. 2002; Morris, Duncan, and Clark-Kauffman 2005).[1] Similarly, Miller and Zhang (2009) use data from the National Assessment of Educational Progress to find substantial relative gains in mathematics test scores for low-income fourth-grade students in the years after national welfare reform. Although these childhood gains are important, a natural concern is that they may erode with time.[2] Indeed, some experimental studies find increases in self-reported dropout, expulsion, and suspension rates for adolescents in the welfare reform treatment group (Gennetian et al. 2004). This finding of adverse effects of welfare interventions, however, is not consistent with the finding of eighth-grade test score gains in Miller and Zhang (2009) and may be due to the limited schooling outcome measures or other drawbacks of the experimental studies that especially apply to older children. This paper uses national data on objective schooling measures for adolescents to address the question of persistence.

In contrast to the experimental literature, a major contribution of this study is the use of large national samples of educational attainment data spanning the period from 1991 to 2005. We estimate the effects of the actual statewide and national welfare reforms that were implemented. In the experimental studies, the geographic variation is limited, and the policies under study tend to employ weaker work requirements than the statewide and national reforms; the experiments are also likely to miss any general equilibrium effects from widespread reforms (Grogger and Karoly 2005). In addition, the experimental setup, in which welfare recipients are randomized into treatment and control groups, prevents researchers from assessing entry effects if welfare reform causes some adults to voluntarily avoid or curtail their participation. This is particularly problematic for studies focused on adolescents, who are themselves potential welfare recipients.[3] By limiting the sample to current welfare participants, experimental approaches miss a key channel for the impact of reforms. The low-income group in this paper is defined to include former, current, and potential welfare recipients and to facilitate the capturing of the important entry and exit effects.

This research also advances the literature using observational data and studying national reform by combining an analysis of both administrative and survey data on large, national samples for an extended period. Previous studies of welfare reform and adolescents have largely relied on self-reported educational outcomes from surveys. Kaestner, Korenman, and O'Neill (2003) use National Longitudinal

[1] The welfare experiments are the Florida Family Transition Program; the National Evaluation of Welfare to Work Strategies in Atlanta (Georgia), Grand Rapids (Michigan), and Riverside (California); the Minnesota Family Investment Program; Milwaukee New Hope; and the Canadian Self-Sufficiency Project. Studies of child outcomes are still pending for three additional programs: the Connecticut Jobs First Program, the Indiana Welfare Reform Evaluation, and the Iowa Family Investment Program.

[2] For example, Barnett (1995) and Karoly et al. (1998) report evidence that test score gains from the Head Start program dissipate by the third grade. Meanwhile, Garces, Duncan, and Currie (2002) find significant long-term gains for whites as a result of Head Start participation, including higher rates of school completion.

[3] Indeed, Kaestner, Korenman, and O'Neill (2003) argue that welfare reform lowered welfare participation rates for teenage girls.

Surveys of Youth from the 1979 and 1997 entry cohorts to track changes in relative outcomes for teenage girls from more and less advantaged backgrounds over a period that includes welfare reform. They present evidence of relative improvements for disadvantaged girls, which contrasts with the lack of significant associations found by Hao and Cherlin (2004), who compare adjacent cohorts within the National Longitudinal Surveys of Youth 1997 entry cohort. The lack of positive effects noted by Hao and Cherlin may be the result of their limited sample sizes and the short-term nature of their estimates, which were computed immediately after welfare reforms.[4]

Offner (2005) and Dave, Reichman, and Corman (2009) provide the studies closest to our analysis of survey data from the October CPS (U.S. Bureau of Labor Statistics 1990–2005) presented in Section 5. Both focus on teenage girls. Offner uses March CPS data for more than 20,000 individuals to associate, in a difference-in-differences framework, welfare reform with lower dropout rates and out-of-wedlock fertility rates for teenage girls from disadvantaged backgrounds. The results are consistent across each of the comparison groups (based on income, gender, and motherhood). Although the focus of Dave, Reichman, and Corman (2009) is women 21–49 years of age, that paper also contains an analysis of teenage girls performed using October CPS data. The result is complementary: unmarried teenage girls in low-education (without adults who are college graduates), non-two-parent households exhibit a decline in high school dropout rates relative to a control group of unmarried 15–20-year-old males in similar households.

This paper is the first to use administrative data on schooling outcomes to validate the estimates from self-reported survey data. It is also the only paper in the literature to go beyond difference-in-differences models comparing periods before and after welfare reform to exploit the available panel data to estimate dynamic effects of welfare reform. This is accomplished by expanding the empirical model to include terms for several years before and after the policy change. As in Miller and Zhang (2009), the impact of welfare reform is not immediately expressed in educational outcomes but increases in size during the first decade after reforms. This may occur because of accumulating gains through the educational production process or because of the importance of early childhood exposure. The dynamic framework also allows us to test the validity of the difference-in-differences framework by estimating changes in relative outcomes preceding welfare reform. The lack of preexisting trends provides support for assigning a causal interpretation to the benefits associated with welfare reform in observational studies.

Finally, to our knowledge, this is the only paper to estimate educational benefits from welfare reform to adolescent males. As described in Section 2, the dynamic

[4] In related work, Dunifon, Hynes, and Peters (2006) suggest that the lack of systematic associations between child outcomes and state welfare policies in their analysis of the Survey of Income and Program Participation may be due to the short time horizon.

effects of welfare reform can occur through direct incentive effects as well as indirect mechanisms that operate through changes in the home environment and accumulate over time. The direct incentive effect should be stronger for young women, who can potentially become single mothers and who face a much greater risk of ever receiving welfare benefits. However, the incentive effects can still affect the educational choices of young men, since these men are likely both the sexual partners of the women being affected by welfare reform and the fathers of their children. The indirect effects that occur through changes in maternal behavior and the home environment should have more similar effects on males and females. Our finding of educational benefits for teenagers of both sexes is consistent with the finding (in Miller and Zhang [2009] as well as in experimental studies of younger children) of test score gains for younger children of both sexes and provides evidence of lasting effects from changes in the home environment. The fact that gains increase over time is also consistent with the evidence for younger children and with the primary mechanism for the effect being changes in the home environment, the benefits of which increase with more years of exposure.

The rest of the paper is organized as follows. Section 2 provides background on welfare reform and educational attainment, and Section 3 describes the attainment data. Sections 4 and 5 discuss results, and Section 6 concludes.

2. Welfare Reform and Schooling Background

In standard human capital models (Becker 1964; Ben-Porath 1967), utility-maximizing individuals invest in schooling up to the point at which the benefit from an additional year of schooling (including higher discounted future earnings, better health, and higher nonpecuniary values) is equal to the cost (for example, forgone earnings, direct monetary costs, and disutility from studying). Optimal schooling varies across individuals as a result of differences in innate ability, family and school inputs, and socioeconomic environment. Welfare reform changed maternal behavior and family environment as well as the future economic rewards from working for children in low-income households. These indirect (through home environment) and direct (through work incentives) channels may independently or collectively affect the expected costs and benefits of school investment, thereby shifting the educational outcomes of low-income individuals.

Increased maternal employment has a theoretically ambiguous effect on the level of family inputs devoted to children's education. On the one hand, maternal employment can hinder school performance by reducing the time available for home production, such as supervising and disciplining children, reading to them, and assisting with homework.[5] Slower learning progress accumulated over time

[5] Since Coleman et al. (1966) was published, a long line of scholars in a variety of disciplines have studied the role of families in the academic achievement of children.

can lead to large skill gaps by adolescence and early-dropout decisions.[6] Lagging behind peers in school performance can also lead to greater disutility from schooling and earlier exit from school. On the other hand, working mothers may feel more secure and confident, resulting in greater productivity at home. They may also provide their children with improved stability and daily routine, serve as better role models for them, and instill in them a greater desire for financial independence and academic achievement. Working mothers may also acquire useful human capital on the job, such as learning about child rearing from coworkers.[7] Increased family income may also boost children's school performance through improved nutrition and reading materials at home, although the earned income gains were largely offset by reduced welfare benefits.[8] Improved school performance can then lead to increased schooling quantity.

Furthermore, welfare reform may have increased the financial returns to schooling for low-income individuals to the extent that it made long-term dependence on public assistance a less attractive or viable alternative to paid employment.[9] Low-income adolescents may remain in school longer because of this direct incentive. Welfare reform, through its new eligibility requirements, may also provide parents with financial incentives to keep their children in school or stay in school themselves if they are minors and do not have a high school diploma or a general educational development certificate. These different pathways lead to opposing effects, and the net effect of welfare reform on schooling is inherently an empirical question.

We use a difference-in-differences framework to estimate the net effect of welfare reform on the educational attainment of low-income children:

$$E_{ist} = \boldsymbol{\beta}_1 \times \mathrm{LI}_i + \boldsymbol{\beta}_2 \times \mathrm{Postreform}_{st} + \boldsymbol{\beta}_3 \times \mathrm{LI}_i \times \mathrm{Postreform}_{st} + \boldsymbol{\beta}_X \times X_{ist} + \varepsilon_{ist}, \quad (1)$$

where s indexes state, t indexes year, and i indexes the unit of observation. The

[6] In Becker (1964) and Ben-Porath (1967), individuals benefit more from schooling when they are more productive in translating time in school into additional units of human capital, and hence they stay in school longer. There is substantial evidence that students who do better in school, through either grades or scores on standardized achievement tests, tend to go further in school. See, for example, Rivkin (1995) and Hanushek, Rivkin, and Taylor (1996).

[7] There is experimental evidence that the children of women assigned to welfare policies that promote adult education and training show improved school readiness and fewer academic problems (Magnusen and McGroder 2002). However, our study measures the effects of the national and statewide reforms, which focus more on employment. Indeed, Jacobs and Winslow (2004) detail how the work-first approach of Temporary Assistance for Needy Families may have reduced higher education enrollment among single mothers.

[8] Prior studies of maternal employment and child human capital produce mixed evidence. Negative effects are concentrated among children in more affluent, two-parent families with more educated mothers (Ruhm 2004; Baum 2003). For financial resources, Blau (1999) finds only trivial direct effects of family income on child development. Waldfogel (2007) reports no positive association between U.S. welfare reform and low-income family spending on education or children's clothing.

[9] See, for example, Bruce and Waldman (1991) on the Samaritan's dilemma associated with cash transfers.

term LI measures *i*'s low-income status.[10] The state-year-specific postreform time trend is estimated using higher income children, while the treatment effect of welfare reform is measured in the interaction between Postreform and the low-income indicator LI. The term X_{ist} is a vector of control variables.

In an expanded, dynamic version of the model, we allow the postreform effect to vary with years since the reform, and we also estimate differential trends before the reform:

$$E_{ist} = \beta_1 \times \mathrm{LI}_i + \sum_j \beta_2^j \times \mathrm{YSR}_{st}^j + \sum_j \beta_3^j \times \mathrm{LI}_i \times \mathrm{YSR}_{st}^j + \beta_X \times X_{ist} + \varepsilon_{ist}, \tag{2}$$

where YSR_{st}^j is a vector of indicators for the jth year since state implementation of welfare reform, with negative values of j indicating years before reform. Since all states experienced welfare reform during the sample period, the omitted YSR category is zero for the year of reform, and changes are relative to that baseline year. The coefficients of primary interest in the dynamic model comprise the vector β_3, the differential trends in educational attainment between low- and higher income students surrounding welfare reforms.

The assumption underlying the model is that the groups experience otherwise similar changes in educational environment during the period, so the observed trend in educational attainment for higher income adolescents provides an appropriate counterfactual estimate for what would have happened to low-income adolescents in the absence of welfare reform. Conditional on covariates,[11] we assume that welfare reform is the only systematic factor that has a differential impact on the poor.[12] Although the assumption is not directly testable during the reform period, it is supported by data prior to welfare reform, as discussed in Sections 4 and 5.

The timing of welfare reform is defined for each state as the year in which the state first instituted major reforms to its cash transfer system. Data on welfare

[10] The unit of observation is a school district in the Common Core of Data (CCD) and an individual in the Current Population Survey (CPS). Low-income status is the share of students in a school district who are low income in the CCD data and an indicator variable in the CPS. Hence, slightly different forms of equations (1) and (2) are estimated in Sections 4 and 5 for the two data sets.

[11] In some models, the covariates include interactions between observable controls and LI. Including these additional terms does not alter the estimates of the treatment effects.

[12] Meyer and Rosenbaum (2001) show that other policy changes between the mid-1980s and the mid-1990s, such as the earned income tax credit (EITC) and Medicaid expansions, training programs, and child care, along with welfare waivers, substantially contributed to the increased employment of single mothers. Data limitations do not allow us to definitively isolate the impacts of these policy changes on the educational attainment of low-income children. However, this may not alter the interpretation of our main results. First, the impact of these policies is likely to have occurred by the mid-1990s, whereas summary statistics in Section 3 suggest that educational attainment by low-income children started to catch up after 1996. Second, even if one attributes our findings to the combined effects of welfare reform and these related policy changes, the basic structural implication remains unchanged: increased parental work was beneficial to children in low-income households. Finally, some specifications in Section 4 control for the expansion of the EITC, and this does not affect the estimated effects of welfare reform.

policies are from Crouse (1999) and U.S. Department of Health and Human Services (1997). Table A1 lists each state's timing of welfare reform. For states that adopted waivers to Aid to Families with Dependent Children program rules, we define the reform date as the earliest statewide waiver date. For other states, we use the date that they switched from Aid to Families with Dependent Children to Temporary Assistance for Needy Families.

To isolate the impact of welfare reform, we control for factors that could independently influence school enrollment and dropout rates and that may be correlated with reforms. Temporary upward shifts in labor demand can increase the opportunity cost of not working and thereby increase dropout rates.[13] We use state-year macroeconomic indicators—unemployment rate and income per capita—to control for the labor market conditions. Increased school resources may increase the benefit of schooling and reduce dropout rates.[14] We therefore control directly for educational inputs using spending per pupil and the pupil-teacher ratio.

We also address the potentially confounding effect of a contemporaneous change in education policy that may have affected low-income students differentially: school accountability reform. Following Hanushek and Raymond (2005), we define an accountability system as a mechanism for publicly disseminating information on standardized test performance for each school, along with a way to aggregate and interpret the school performance measure. States are classified as consequential states if they both report results and attach consequences to school performance or as report card states if they provide only a public report. Consequential accountability may provide stronger incentives to schools than report card accountability. States began introducing school accountability systems in the early 1990s, and by 2003, 31 states had consequential accountability. The timing of adoption of state accountability programs is reported in Table A1. Table A2 illustrates two useful facts about the timing of state welfare and school accountability reforms. First, states did not generally adopt the two reforms simultaneously. Second, the states with early accountability reform are not the states with early use of welfare waivers. Without this variation, it would be impossible to empirically distinguish the effects of the two sets of reforms.

The passage of the No Child Left Behind Act (NCLB) (Pub. L. 107-110) in January 2002 demanded strong accountability of schools in all states. Between January and June 2003, states submitted their plans for implementing an accountability system under NCLB to the Department of Education. By June 2003, all were approved. Therefore, we consider 2003 to be the year when the 19 report card states and Washington, D.C., introduced consequential accountability. In

[13] Card and Lemieux (2000) find that the local unemployment rate is an important explanatory variable for lower school enrollment rates in the 1970s. Neumark and Wascher (1995) find that increases in the minimum wage reduced the proportion of teenagers enrolled in school and increased the proportion of teenagers who were neither enrolled in school nor employed.

[14] However, the role of spending in improving educational outcomes remains controversial (Hanushek 1986).

the analysis that follows, we control for the presence of a consequential accountability system and for the number of years that have elapsed since its adoption, and we allow these variables to have different effects on low-income children.[15] To address concerns that provisions in NCLB that relate to high school graduation rates created incentives for schools to undercount dropouts (Swanson and Chaplin 2003), we also estimate the models in Section 4 using a shorter period that ends before NCLB.

3. Measures of Educational Attainment

We estimate the impact of welfare reform using measures of educational attainment from both administrative and survey sources. Our primary measures, described in Section 3.1, are administrative records of school district dropout and high school completion rates. We present the main results obtained using these measures in Section 4. As a validation exercise, in Section 5, we also present results using an external and complementary measure of attainment: self-reported school enrollment, which we obtain from October CPS files and describe in Section 3.2.

3.1. School Dropout and High School Completion Rates

We capture educational progress using school dropout and high school completion rates as our primary outcomes. We construct our measures using administrative school district panel data from the CCD survey of the U.S. Department of Education (National Center for Education Statistics 1991–2003). Although the survey covers all school districts in the country, data are made available only for districts that satisfy the department's minimum reporting standards. Dropout rates are available separately by grade and are averaged over grades 7–12 and 9–12 from 1991 to 2003; high school completion rates are available from 1994 to 2003. We aggregate CCD school-level data to compute the share of students in each district who are eligible for free lunches through the national school lunch program (those with a household income below 130 percent of the federal poverty level). Free-lunch eligibility (FLE) is the only student income measure in CCD; its broad definition means that the low-income group includes many children whose families are not eligible for welfare payments and would not be directly affected by the reforms.[16] This will dilute the treatment group and introduce attenuation bias in the measured impact. The advantage

[15] Report card accountability programs have no significant effects on educational attainment, either alone or along with consequential programs; they also do not affect estimated effects of welfare reform.

[16] Using data from the U.S. Department of Agriculture and the U.S. Department of Health and Human Services, we calculate that the national average ratio between the number of families receiving welfare and the number of children eligible for free or reduced-price lunches is .3 in 1996 and .13 in 2005. In addition, approximately 85 percent of students eligible for free or reduced-price lunches are eligible for free school lunches.

of using FLE as the definition is that the low-income group thus defined is likely to include children formerly and currently eligible for welfare assistance, as well as children who are potentially eligible, which allows us to avoid the issue of sample selection due to entry into or exit from welfare.

The grade G dropout rate for school year T (the school year beginning in the fall of year T) is defined as the ratio of the number of students who are enrolled in grade G in school year T but are not enrolled in any grade at the beginning of school year $T + 1$ to the number of students enrolled in grade G in school year T. Students who graduated from high school or transferred to another school are not counted as dropouts.[17] For example, the seventh-grade dropout rate for school year 2000 is the percentage of students who were enrolled in grade 7 in school year 2000 but were not enrolled at the beginning of school year 2001 relative to the number enrolled in grade 7 in school year 2000, and the dropout rate over grades 7–12 in school year 2000 is the percentage of students who were enrolled in grades 7–12 in school year 2000 but were not enrolled in any grade at the beginning of school year 2001 relative to the number enrolled in grades 7–12 in school year 2000. The dropout rate for grades 7–12 can be interpreted as an overall measure of a district's effectiveness at keeping students enrolled. High school completers are defined as students who receive a high school diploma or a certificate of attendance or completion at the end of the summer of a school year. General Education Development certificate recipients are not counted. The high school completion rate is the number of high school completers divided by the sum of the number of high school completers, the number of grade 12 dropouts in the current year, and the number of dropouts from grades 11, 10, and 9 in the preceding 1, 2, and 3 years, respectively.[18]

Table 1 reports summary statistics for the dropout and completion rates over all years and all school districts. We restrict our analytic sample to states with at least 4 years of valid observations.[19] The dropout rate increases monotonically from grade 7 to grade 12, in large part reflecting the age distribution of the

[17] Administrative calculations of dropout and completion rates are flawed, in large part because school districts have limited ability to track student migration. To improve data quality for these key educational outcomes, some have proposed that students be assigned unique national identification numbers at school entry that remain with them throughout their schooling careers (Orfield et al. 2004).

[18] Our high school completion rate measure is closely related to Heckman and LaFontaine's (2010) preferred estimator with CCD data, which is calculated by dividing the number of diplomas issued in a given year by the number of students enrolled in the eighth grade 5 years earlier. Our measure totals the dropout counts (effectively, the changes in enrollment) from grades 9–12 over the previous 4 years and is thus similar to using a ninth-grade enrollment base, although it accounts for transfers across districts and excludes students retained at any time between grades 9 and 12. Results are unchanged if we use completion rate calculated relative to eighth-grade enrollment 5 years earlier or relative to ninth-grade enrollment 4 years earlier.

[19] The excluded states (California, Colorado, Indiana, Michigan, New Hampshire, and Washington for dropout analysis; Alaska, Florida, Kansas, New Hampshire, North Carolina, Ohio, South Carolina, Tennessee, and Washington, D.C., for completion analysis) mostly appear in the data set only at the end of the sample period. Hence, they cannot contribute to the before- and after-event analysis of welfare reform. Results are qualitatively unchanged if we include all usable observations.

Table 1
Summary Statistics of Dropout and Completion Rates

Variable	*N*	Mean	SD	Median	99th Percentile	Min	Max
Dropout rate, by grade:							
7	80,976	.40	2.60	0	6.30	0	100
8	81,545	.62	2.98	0	8.01	0	100
9	76,131	2.68	4.90	1.20	19.05	0	100
10	77,147	3.64	5.19	2.40	20.00	0	100
11	77,797	4.28	5.35	3.23	20.70	0	100
12	78,181	4.33	5.74	3.20	22.42	0	100
7–12	74,725	2.48	4.09	1.70	13.61	0	100
9–12	75,905	3.72	4.61	2.80	17.70	0	100
Completion rate:							
Total	45,159	85.68	11.05	87.75	100	.85	100
Diploma receipt	45,145	84.45	12.06	86.96	100	.85	100
ShFLE	83,317	.27	.19	.24	.85	0	1

Note. Dropout rates, completion rates, and share of students with free-lunch eligibility (ShFLE) are averaged over all years and all districts. Dropout rates are conditional on enrollment in the previous grade in the previous school year; high school completers do not include General Educational Development certificate recipients.

different grades. The vast majority (99 percent) of school districts have dropout rates below 14 percent for grades 7–12 and below 18 percent for grades 9–12. Nevertheless, the average dropout rates in grades 9 and 10 are quite high, at 2.68 and 3.64 percent, respectively, despite the fact that most states have a legal school exit age of 16 years or older. This may be due to either high grade retention in these grades or delayed school entrance (Deming and Dynarski 2008). The average high school completion rate is 86 percent. The majority of high school completers receive a high school diploma, and only 1.3 percent complete high school with a certificate.

Also reported in Table 1 is a summary of the share of students eligible for free school lunches (ShFLE). Averaged across districts, 26 percent of students are eligible for free school lunches, and in the median school district, 22 percent of students are eligible for free lunches. In addition, in the wealthiest 10 percent of districts, less than 5 percent of students are eligible, and in the poorest 10 percent of districts, more than 50 percent of students are eligible.

Figures A1 and A2 display time trends in school dropout and high school completion rates. From 1991 to 2003, there is a general decrease in school dropout rates over all grades, and the 4-year high school completion rate also increases slightly between 1994 and 2003. In addition, states with larger shares of their student populations classified as low income have relatively more dropouts and fewer high school completers, as illustrated by Figures A3 and A4.[20]

Since welfare reform directly affects only low-income students, we can estimate

[20] In Figure A4, dropout rates are averaged by state for 1991–2003; completion rates are for 1994–2003.

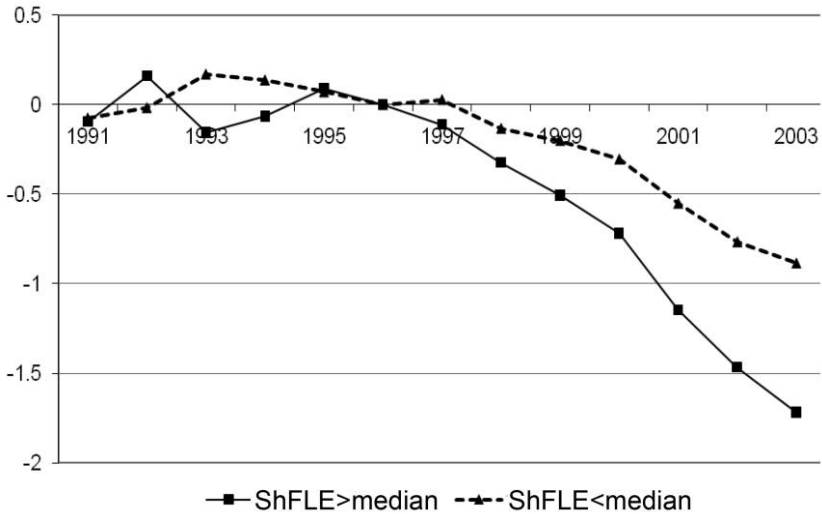

Figure 1. Deviation from the 1996 dropout rate for grades 7–12, by income (National Center for Education Statistics 1991–2003).

its impact on schooling by comparing differential trends in outcomes between FLE and non-FLE students around the time of reform. Because our data are aggregated at the school district level, we compare differential trends in outcomes across districts according to their shares of students eligible for free lunches ($ShFLE_{it}$). Figure 1 illustrates the differential trends in dropout rates for school districts with higher (above the median of 23 percent) and lower (below the median) shares of FLE students. Each curve plots the change in the enrollment-weighted average dropout rate for grades 7–12 for that group of districts relative to its value in 1996, the year that the Personal Responsibility and Work Opportunity Reconciliation Act was passed. Before 1996, dropout rates for the two groups of school districts track each other quite closely, whereas after 1996, there is an unmistakable divergence between the districts with low $ShFLE_{it}$ and high $ShFLE_{it}$. Districts with higher income show relatively stable dropout rates over the period. In sharp contrast, lower income districts see an abrupt break from the pre-1996 trend; their dropout rates decrease consistently and considerably after 1996. This pattern of dramatic relative changes is confirmed in the formal regression analysis that follows.

3.2. School Enrollment

As a validity check for analysis performed at the school district level, we also estimate our model using self-reported school enrollment data for young adults from the CPS October supplement files from 1990 to 2005 (U.S. Bureau of Labor

Table 2
School Enrollment Rates by Year and Household Income

	Weighted Enrollment Rate			Percentage Change	
Year	SLI	SLE	Gap	SLI	SLE
1990	93.16	84.14	9.02		
1991	94.12	83.94	10.18	1.0	−.2
1992	94.20	86.69	7.50	.1	3.3
1993	94.37	86.30	8.07	.2	−.4
1994	94.50	86.36	8.14	.1	.1
1995	94.39	84.93	9.46	−.1	−1.7
1996	93.86	84.53	9.33	−.6	−.5
1997	94.82	86.41	8.42	1.0	2.2
1998	94.38	84.45	9.93	−.5	−2.3
1999	93.79	85.45	8.34	−.6	1.2
2000	93.57	84.95	8.62	−.2	−.6
2001	93.47	85.96	7.52	−.1	1.2
2002	93.98	87.27	6.71	.5	1.5
2003	93.56	87.18	6.39	−.4	−.1
2004	94.29	88.14	6.14	.8	1.1
2005	94.80	89.02	5.78	.5	1.0
Average	94.08	85.95	8.13	.1	.4

Note. Weighted average school enrollment rates (percentage currently enrolled) are presented by year and income, averaged over all ages (13–18 years). SLI = subsidized-lunch ineligible; SLE = subsidized-lunch eligible.

Statistics 1990–2005). The main data set includes more than 168,000 observations of children ages 13–18 for whom schooling and household income information is available.

The primary measure of low-income family status is an indicator of eligibility for subsidized (free or reduced-price) school lunches (SLE) assigned according to the condition that household income is below 185 percent of the federal poverty level.[21] We also use narrower definitions of low-income status: a household income below 130 percent of the poverty level (corresponding to the cutoff for FLE), income below the poverty level, a mother with fewer than 12 years of schooling completed, and SLE combined with the condition that the mother is unmarried. Each of the low-income groups defined above is likely to contain children who are formerly and currently eligible for welfare assistance as well as children who are potentially eligible, which therefore allows us to avoid the issue of sample selection due to entry into or exit from welfare.

In Table 2, the basic time patterns of school enrollment rates during the sample period are shown for children ages 13–18 in low-income (SLE) and higher income

[21] Total household income and the number of household members are obtained from the CPS October files (U.S. Bureau of Labor Statistics 1990–2005). They are compared to the federal poverty level for the given family size in that calendar year to determine income status. Income is not observed continuously in the data but is reported in ranges. Low-income status is defined conservatively, using the highest point of the income category as reported income. Results do not change if the midpoint or lower bound of the reported range is used instead.

(subsidized-lunch-ineligible, or SLI) households, based on a household income that is below 185 percent of the poverty level cutoff. Considering the modest income gains of welfare recipients, the low-income (SLE) group likely contains past, current, and potential welfare-eligible children in all years, while the higher income (SLI) group likely excludes them.[22] The table also reports school enrollment rates in October of each year using CPS household weights. School enrollment rates for higher income children show fluctuations but no clear trend, while those for lower income children have a tendency to increase. The unweighted means are very similar, with differences less than .1 percent. Low-income children are approximately 8 percent less likely to be enrolled in school, but that gap narrows from 9 to 6 percent over the 16-year sample period. The average annual percentage growth rate is .4 percent for low-income children but only .1 percent for higher income children. The regression analysis that follows explores the timing of the narrowing relative to state-level adoption of welfare reforms and confirms that the narrowing remains significant after controlling for changes in observables.

In Figure A5, the raw trends in enrollment rates between 1995 and 2005 are illustrated for children of different ages and family income levels. Weighted average school enrollment percentages are given by age, year, and low-income status. There is substantial heterogeneity in enrollment rates by age, with near full enrollment for those ages 15 and younger. Rates of school enrollment are lower for older children, and gaps between low-income and higher income children are more pronounced. Over time, enrollment increases for SLE individuals ages 16–18 and for SLI individuals age 18.

In our analysis, we do not distinguish between high school and other school enrollment, but instead use the age cutoff of 18 to define our sample. We impose the upper age limit to ensure that our household income measure captures parental resources. Young adults enrolled in college are counted in the enrolled group.[24] (Results using alternative age cutoffs are discussed in Section 5.) Although our sample does include some college students (8 percent of SLI students and 4 percent of SLE students), we are unable to analyze the effects of welfare reform on college decisions in any detail. The results in Section 5 should be interpreted as largely applying to high school enrollment, although it should be borne in mind that gains at the secondary level may carry over to later schooling.

[22] Rough estimates of year 2000 income for single mothers who are former welfare recipients are between 105 percent and 120 percent of the federal poverty level, including income from the EITC (Haskins 2001).

[24] The enrolled group includes all respondents who were enrolled in a regular school at the time of the survey. Regular school includes day and night schools, public, parochial, and other private schools and any schooling that leads to a high school diploma, college degree, or professional degree.

4. Results for Administrative Dropout and High School Completion Rates

4.1. Overall Effects of Welfare Reform

Here we report estimates of the overall effects of welfare reform on the relative performance of low-income school districts, using administrative data. We begin with the modified difference-in-differences model of equation (1), where the treatment group variable is a continuous measure of the share of enrolled students who qualify for free lunches ($ShFLE_{it}$)[25] and the dependent variable is the school dropout rate for students in grades 7–12. Models are estimated using the weighted least squares method.[26]

The treatment effects of welfare reform on dropout rates are captured by the coefficients on the interaction of ShFLE and Reform reported in Table 3. Two sets of robust standard errors are reported for each estimate. The first, in parentheses, is clustered at the school district level to account for possible serial correlations in ε_{ist}. The second, in brackets, is from a more general model that also allows for arbitrary error correlations across districts within the same state, by clustering at the state level. The state-level clustering produces larger standard errors in all of the columns. Hence, to be conservative in testing hypotheses, we focus on state-level clustering in the subsequent tables.

Column 1 of Table 3 shows results from the basic fixed effects model, including the full set of district and year fixed effects. Welfare reform is associated with a significant improvement in schooling outcomes for low-income students: school districts with 10 percentage points more FLE students experienced a relative reduction of .06 percentage point in the dropout rate following welfare reform (significant at the 1 percent level with district clustering and at the 5 percent level with state clustering). In column 2, the treatment effect estimate is essentially unchanged after the addition of controls for district-level school input variables (pupil/teacher ratio and spending per pupil) and changing state macroeconomic and educational characteristics (educational attainment of adults in the state, state income per capita, and state unemployment rate) (see summary statistics in Table A3). These results, as well as other estimates shown below that use the CCD data, are not driven by outliers; overall and for each grade level, the estimates are unchanged if we exclude the 1 percent of school districts with the highest dropout rates.

Column 3 of Table 3 supplements the model with controls for state-level

[25] Results are unchanged if we use $ShFLE_{i,t=1990}$ instead of the time-varying $ShFLE_{it}$.

[26] When the unit of observation is an aggregate measure, it is common practice to weight each observation by the number of individual elements it contains (in this case, the number of students), to improve efficiency because larger cells are subject to less sampling error. However, as Dickens (1990) shows, the method inefficiently causes overweighting of larger cells if there are important cell-specific error components. Our estimates use approximately efficient weights derived from Dickens' iterative procedure. The distribution of district sizes is quite skewed, and the efficient weights reduce the undue influence of very large districts, such as New York City, which has 1 million students. Results are the same if we use ordinary least squares estimation instead.

Table 3

Differential Changes in Dropout Rates after Welfare Reform by School District Income

	None (1)	Basic (2)	Accountability (3)	Charter (4)	EITC (5)	MinWage (6)	TeenBirth (7)
ShFLE × Reform	−.5755	−.5724	−.7787	−.7680	−.7807	−.818	−.5805
	(.1168)**	(.1184)**	(.1729)**	(.1750)**	(.2153)**	(.2203)**	(.2296)*
	[.2557]*	[.2908]+	[.3041]*	[.2920]*	[.4534]+	[.5120]	[.4818]
Observations	73,084	73,043	73,043	73,043	73,043	73,043	73,043
R^2	.01	.02	.02	.02	.02	.02	.02

Note. The dependent variable is the percentage of dropouts in grades 7–12, obtained from Common Core of Data school district files (National Center for Education Statistics 1991–2003). Reform is a state-by-year indicator for the period after the first statewide welfare reform or statewide adoption of Temporary Assistance for Needy Families, whichever was earlier. Each column is a separate regression model. All models include district and year fixed effects. The Basic controls include state macroeconomic (adult educational attainment, income per capita, and unemployment rate) and district educational inputs (spending per pupil and pupil/teacher ratios in public schools). The Accountability controls include the Basic variables plus controls for school accountability reform and the number of years since accountability reform and interactions with ShFLE. The Charter controls include Accountability variables and controls (in states with at least 2 percent enrollment in charter schools by 2005) for state legislation allowing charter schools and years since that legislation, as well as interactions with ShFLE. The earned income tax credit (EITC) model includes Accountability variables and additional controls for state and federal EITC generosity (measured by the combined state and federal EITC phase-in rate) and interactions with ShFLE. The MinWage and TeenBirth models supplement the EITC variables with controls for the relevant (state or federal) minimum wage and the state-level teenage birth rate, respectively, as well as their interactions with ShFLE. Standard errors clustered at the school district level are in parentheses; standard errors clustered at the state level are in brackets.

+ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

consequential school accountability reform (if any) or state adoption of NCLB reforms (including indicators for the presence of a reform and a linear measure of months since its enactment) and interactions of the accountability variables and ShFLE. The interactions allow for differential impacts of accountability reform; in fact, we find smaller gains for lower income students following accountability reform.[27] Controlling for accountability reform leaves the main estimates for welfare reform undiminished. To address concerns that national reforms under NCLB were more comprehensive and included stronger provisions than under state accountability (in particular, with regard to dropout and completion rates), we conducted a further robustness analysis in which we estimated the model in column 3 using a shorter period, terminating our sample in 2002. These results (not reported in Table 3) are again similar: a .07-percentage-point relative decline in dropout rates for districts where ShFLE is 10 percentage points higher. These estimates also provide some reassurance that the estimated impact of welfare reform is not driven by systematic changes in the reporting of dropout rates (and not actual dropout rates) in the administrative data in response to NCLB incentives (Swanson and Chaplin 2003).

The remaining columns of Table 3 report treatment effect estimates for additional robustness checks. The estimate is unchanged at −.8 in columns 4 and 5 when we add controls for state charter school policy[28] or for the generosity of the earned income tax credit (EITC)[29] and interactions with ShFLE. In column 6, we add controls for the state-level minimum wage (and interaction with ShFLE), and the point estimate increases slightly. In column 7, the estimate decreases slightly when we add controls for the teenage birth rate in the state-year and its interaction with ShFLE.

The aforementioned analysis made use of the differential timing in start dates for state welfare reforms. However, state welfare reforms generally included a package of policy changes, and the specific policy rules varied substantially across states. In a separate analysis, we attempted to disentangle the impacts of several specific welfare reform policies that may have had strong effects on the behavior of welfare recipients and their children: time limits, sanctions for violating work requirements, earnings disregards, and school requirements for dependent children. When included in equation (1) individually and interacted with ShFLE, every policy except for the school requirement has a negative and significant

[27] The estimated effect of accountability reform is a .38-percentage-point reduction in dropout rates (SE = .23; significant at the 10 percent level). The interaction term of accountability reform and ShFLE is positive (.79) and statistically insignificant. The estimates for years since reform (.041) and its interaction with ShFLE (−.055) are also statistically insignificant.

[28] To be specific, we add a variable that indicates the presence of a charter school law that led to at least modest charter enrollment (2 percent of total enrollment in the 2004–5 school year) by the end of our sample period. We also add a variable for years since state charter legislation. For more information, see Sable and Hill (2006).

[29] The generosity of the EITC is measured by the combined state and federal EITC phase-in rate. The data source is University of Kentucky, Center for Poverty Research, State-Level Data of Economic, Political, and Transfer-Program Information for 1980–2010 (http://www.ukcpr.org/AvailableData.aspx).

effect on dropout rates, with estimates ranging from −.8 to −1.1. When all policies are included in the same model, their interactions with ShFLE are jointly significant, but only time limits have an individually significant effect. This result reflects the well-known challenge of isolating the effects of different features of welfare policies (Blank 2002; Dunifon, Hynes, and Peters 2006): not only are there a large number of policy dimensions, but state policy choices also tend to be either highly positively correlated or to mix relatively weak and strong requirements.

4.2. *Dynamic Effects of Welfare Reform*

To explore the dynamics of the treatment effect and test for preexisting trends, we estimate the following form of equation (2):

$$E_{ist} = \alpha_i + \alpha_t + \boldsymbol{\beta}_1 \times \text{ShFLE}_{it} + \sum_j \boldsymbol{\beta}_2^j \times \text{YSR}_{st}^j + \sum_j \boldsymbol{\beta}_3^j \times \text{ShFLE}_{it} \times \text{YSR}_{st}^j + \boldsymbol{\beta}_X \times X_{ist} + \varepsilon_{ist}. \quad (3)$$

Each observation is a school district (i) located in a state (s) for a time period (t). The dependent variable is the district's dropout or high school completion rate; α_i and α_t are school district and year fixed effects, respectively. The term YSR_{st}^j is a vector of indicators for 5 or more years before reform, 3–4 years before, 1–2 years before, 1–2 years after, 3–4 years after, 5–6 years after, and 7 or more years after. The treatment effect of welfare reform exposure is captured in the series of $\boldsymbol{\beta}_3^j$ coefficients that measure differential changes in the dropout rate relative to the baseline year of welfare reform between districts with no FLE students and districts with 100 percent FLE students. A positive sign on $\boldsymbol{\beta}_3$ indicates a relatively higher dropout rate for a district with a larger FLE share, relative to the reform year.

The first column of Table 4 shows the time pattern of treatment effects for dropout rates in grades 7–12 around the time of welfare reform in a specification with no controls beyond the fixed effects. The generally insignificant ShFLE interactions with years before welfare reform imply that changes in dropout rates in the years preceding welfare reform, relative to the reform year, were substantially similar across districts with different FLE shares. In the years after welfare reform, however, the dropout rate relative to the base year is significantly lower for districts with larger shares of FLE students. This pattern strengthens the temporal link between welfare reform and educational outcomes shown in Section 4.1. In addition, the increasing magnitude of the ShFLE interaction with years after welfare reform suggests increasing gains. As more time elapses between welfare reform and outcome measurement, districts with large FLE shares experience even greater relative declines in their dropout rates.

In the second column, we add state- and district-level control variables. The results are qualitatively similar, but the magnitudes of the treatment effects are larger. For example, in 1–2 years after the implementation of welfare reform, districts with an ShFLE that is 10 percentage points higher experience a .07-

Table 4

Differential Time Trends in Dropout Rates by Grade

	Grades 7–12	Grades 7–12	Grade 7	Grade 8	Grade 9	Grade 10	Grade 11	Grade 12
ShFLE × (5 or more years before reform)	−.2891	−.1852	.0069	.3248	−1.2648	−1.2324	−.1331	1.428
	(.4815)	(.4359)	(.4419)	(.5132)	(.8115)	(.6626)$^{+}$	(.5595)	(.7692)$^{+}$
ShFLE × (3–4 years before reform)	−.5169	−.4433	.0068	−.191	−2.0696	−1.4601	.0525	−.4235
	(.9830)	(.9750)	(.3829)	(.4803)	(1.4543)	(1.4244)	(1.3493)	(1.5430)
ShFLE × (1–2 years before reform)	−.1286	−.17	.2049	.1288	−.3281	−.1974	.1069	−.57^{+}
	(.2580)	(.2412)	(.1784)	(.2428)	(.3618)	(.3857)	(.2900)	(.3231)
ShFLE × (1–2 years after reform)	−.363	−.6821*	−.4619**	−.7127*	−1.1087**	−.5839	−.3118	−.0442
	(.2833)	(.2963)	(.1584)	(.2674)	(.3869)	(.4073)	(.4343)	(.5913)
ShFLE × (3–4 years after reform)	−1.022*	−1.4944**	−.9026**	−1.033**	−2.4663**	−1.3406*	−.6799	−.2171
	(.4191)	(.4542)	(.2314)	(.3413)	(.5826)	(.5931)	(.5439)	(.6952)
ShFLE × (5–6 years after reform)	−.8243^{+}	−1.5224**	−.9116**	−1.5157**	−3.297**	−1.7884**	−.4136	.3226
	(.4799)	(.4582)	(.2667)	(.3476)	(.6174)	(.5732)	(.5791)	(.9485)
ShFLE × (7 or more years after reform)	−2.2576*	−3.0007**	−1.311**	−2.5815**	−6.2201**	−4.7701**	−2.2185**	−1.8707^{+}
	(.9012)	(.6538)	(.3384)	(.5161)	(.9337)	(1.0532)	(.8207)	(1.0947)
p-Value on all prereform terms	.85	.85	.69	.84	.24	.24	.97	.12
p-Value on all postreform terms	.01	.00	.00	.00	.00	.00	.04	.07
Accountability controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	73,084	73,043	79,724	80,341	74,975	76,021	76,717	77,177

Note. Dropout rates are from the Common Core of Data school district files (National Center for Education Statistics 1991–2003). All regressions include district and year fixed effects. Accountability controls are state macroeconomic (adult educational attainment, income per capita, and unemployment rate) and district educational inputs (spending per pupil and pupil-teacher ratios in public schools) and indicators for school accountability reform and interactions with ShFLE. Standard errors, clustered at the state level, are in parentheses. The *p*-values are for the separate *F*-tests for which all prereform and postreform terms are zero.

$^{+}$ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

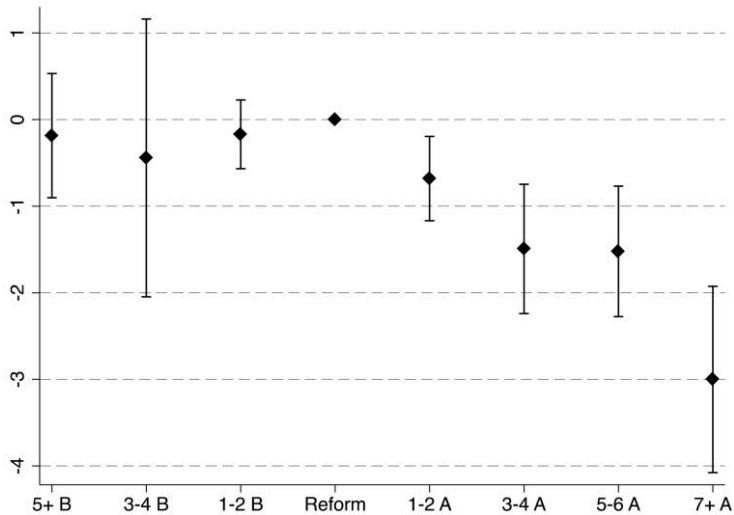

Figure 2. Differential trend in the dropout rate in grades 7–12, by share of free-lunch eligibility around the welfare reform year.

percentage-point larger decline in dropout rates relative to the year of welfare reform. These districts experience a .15-percentage-point larger decline 5–6 years after welfare reform and a .3-percentage-point larger decline 7 or more years after welfare reform, with a standard deviation of the dropout rate that is close to .1.[30] This corresponds to a narrowing of the initial gap of 18 percent after 5 years and 36 percent after 7 years. The *F*-test of the hypothesis that all the prereform coefficients are zero cannot be rejected at conventional significance levels, whereas the *F*-test of the hypothesis that all the postreform coefficients are zero rejects the null hypothesis at 1 percent significance. The pattern is illustrated in Figure 2,[31] where we plot the point estimates of the interactive terms and their 90 percent confidence interval bands. There is a clear break from trend around the time of welfare reform.

The dropout rate for grades 7–12 reflects the overall tendency of students in the six included grade levels to drop out of school. Table 4 also examines the dropout pattern for each of the grades separately. The effects of welfare reform

[30] The coefficient on the interaction between ShFLE and the indicator for 7 or more years after reform is identified by 15 states with early welfare waiver use that have waivers for 7 or more years by the end of the sample period. Similarly, the coefficients on the interactions between LI and indicators for 9–10 years and 11 or more years after reform in Section 5 are identified by the same group of early waiver states.

[31] In Figures 2 and 3, coefficient estimates and 90 percent confidence intervals are for interactions between the district share of students with free-lunch eligibility and indicators for years before and after statewide waiver or welfare reform adoption. Models include the full set of controls, and standard errors are clustered at the state level.

appear to increase between grades 7 and 9 and then decrease in size. The postreform estimates for grades 11 and 12 are statistically insignificant, except at 7 or more years after reform. Extrapolating from low-income districts to low-income students, we infer that welfare reform reduced the relative dropout rates of low-income students in grades 7–10 but did not alter the dropout propensity of students in grades 11 and 12. This is not surprising given that the dropout rate is defined conditional on enrollment in the previous school year. Low-income students who plan to drop out before completing high school are likely to leave in earlier grades; those who reach grade 11 or grade 12 reveal themselves, by self-selection, to be more determined and academically more prepared to finish high school.[32] For the prereform period, while dropout rates for grades 7 and 8 do not vary with FLE share relative to the baseline year, the negative (although not jointly significant) estimates for grades 9 and 10 suggest that, if anything, the gap in dropout rates between low-income and high-income students may have been growing in the years before welfare reform.

Although not reported in Table 4, the estimates for control variables link consequential school accountability systems with lower dropout rates (−.44; SE = .22), but their impact is smaller for districts with larger FLE shares (1.01; SE = .41), consistent with the interaction effect in Table 3. In addition, in states with higher unemployment rates, dropout rates are lower, especially for grades 10–12, which suggests a possible demand-side influence. Estimates for other control variables are insignificant.

Table 5 reports dynamic results for our alternative educational attainment measure: the high school completion rate. As a comparison, we repeat the results for the dropout rate for grades 7–12 from column 2 of Table 4. The results for 4-year high school completion rates, presented in column 2, are almost a mirror image of those for aggregate dropout rates, as illustrated by Figures 2 and 3. Prior to welfare reform, high school completion rates relative to the reform year do not vary with FLE share. Following welfare reform, completion rates relative to the base year are significantly higher for districts with larger shares of FLE students; the point estimates are jointly significant at the 1 percent level, and the individual estimates are statistically significant for observations made 3–4 years and 7 or more years after reform. The effect of welfare reform on high school completion is important because of the critical role of a high school diploma in the labor market and because completing high school gives students the option to obtain further education.

In the last two columns of Table 5, we consider the impact of welfare reform on the educational attainment of low-income students on the basis of our alternative definition of school district income: the share of children ages 5–17 in poverty in a school district based on 1995 census data. For both dropout and high school completion rates, the basic pattern of treatment effect estimates is

[32] Current Population Survey data confirm that low-income students are more likely to be retained (that is, they are more likely to report larger differences between their age and grade level in October) and are less likely to be enrolled in grades 11 or 12.

Table 5

Differential Time Trends in Dropout and High School Completion Rates, by Definition of Low Income

	ShFLE		95pov5–17	
	Dropout Rate, Grades 7–12	Completion Rate	Dropout Rate, Grades 7–12	Completion Rate
Low-income share × (5 or more years before reform)	−.1852		−.0673	
	(.4359)		(.6766)	
Low-income share × (3–4 years before reform)	−.4433	1.2202	−.0346	1.8115
	(.9750)	(1.3246)	(1.3182)	(1.9490)
Low-income share × (1–2 years before reform)	−.17	.0984	−.2631	−.6966
	(.2412)	(.9877)	(.3441)	(1.2400)
Low–income share × (1–2 years after reform)	−.6821*	.8692	−.9327*	1.5727
	(.2963)	(.9399)	(.4339)	(1.3188)
Low-income share × (3–4 years after reform)	−1.4944**	2.6319+	−2.116**	5.3202*
	(.4542)	(1.3569)	(.7078)	(2.2226)
Low-income share × (5–6 years after reform)	−1.5224**	2.15	−2.0451*	3.0259
	(.4582)	(1.4135)	(.8323)	(2.7718)
Low-income share × (7 or more years after reform)	−3.0007**	5.229**	−4.7353**	7.169*
	(.6538)	(1.5558)	(1.2063)	(3.0842)
p-Value on all prereform terms	.85	.58	.89	.59
p-Value on all postreform terms	.00	.01	.00	.06
N	73,043	44,635	72,657	44,494

Note. Dropout and completion rates (0–100 percent) are from Common Core of Data school district files (National Center for Education Statistics 1991–2003). All regressions include district fixed effects. Control variables are state macroeconomic (adult educational attainment, income per capita, and unemployment rate), district educational characteristics (spending per pupil and pupil/teacher ratios in public schools), and state school accountability reform (and interactions with low-income share). Standard errors, clustered at the state level, are in parentheses. The *p*-values are for the separate *F*-tests for which all prereform and postreform terms are zero. 95pov5–17 = the fraction of children aged 5–17 years who are living in poverty in the school district in 1995.

+ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

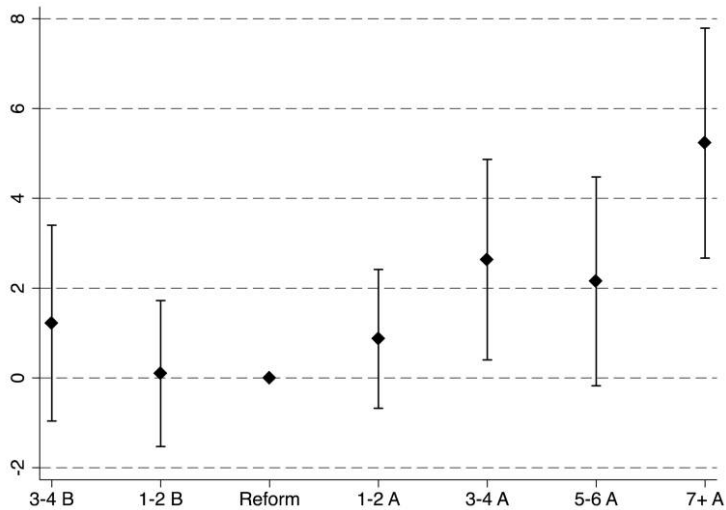

Figure 3. Differential trend in 4-year high school completion rate by share of free-lunch eligibility.

the same as when low income was defined by FLE. However, the magnitude of the postreform estimates is considerably higher, perhaps because the impact of welfare reform is more intense for children from welfare-prone families than for other low-income children.

In addition to estimating effects by grade level, it is interesting to measure separate effects for boys and girls, in part because the direct-incentive hypothesis would predict larger effects for girls. Unfortunately, dropout rates in grades 9–12 are available by sex only for 1998–2001, which means that there is no pre-reform baseline for most states. Nevertheless, preliminary analysis using the limited range of years suggests that welfare reform had similar positive effects on boys and girls, as measured by point estimates on the interactive terms. However, because girls have lower average dropout rates than boys, this translates into larger proportional effect for girls—the ratio between the estimate for the interaction between ShFLE and the indicator for 7 or more years after reform and the mean dropout rate in 1998 is .44 for boys and .62 for girls. This is consistent with low-income girls experiencing an additional direct incentive effect from welfare reform, over and above any changes in family environment.

5. Results for Self-Reported School Enrollment Rates

Here we examine the effects of welfare reform on school enrollment, using data from the CPS's October supplement files from 1990 to 2005 (U.S. Bureau of Labor Statistics 1990–2005). This provides a useful comparison with other

observational studies in the welfare reform literature that use survey data (for example, Offner 2005; Dave, Reichman, and Corman 2009). It also allows us to confirm the district-level effects found in Section 4.2 using individual-level data and to test alternative definitions of the treatment and control samples. We estimate the following version of the dynamic model in equation (2) using individual-level data:

$$E_{ist} = \alpha_s + \alpha_t + \boldsymbol{\beta}_1 \times \mathrm{LI}_{it} + \sum_j \boldsymbol{\beta}_2^j \times \mathrm{YSR}_{st}^j + \sum_j \boldsymbol{\beta}_3^j \times \mathrm{LI}_{it} \times \mathrm{YSR}_{st}^j + \boldsymbol{\beta}_X \times \boldsymbol{X}_{ist} + \varepsilon_{ist}, \tag{4}$$

where the dependent variable is an indicator equal to one if an individual is enrolled in school. The terms α_s and α_t are state and year fixed effects, LI_i is an indicator equal to one if an individual is in the low-income group, and YSR_{st}^j is a vector of indicators defined as in Section 4.2 but ranging from 7 or more years before reform to 11 or more years after reform. Since all states experienced welfare reform during the sample period, the omitted category is for the year of reform, and the values of $\boldsymbol{\beta}_2^j$ for years before and after reform should be interpreted as changes relative to the baseline reform year. The control variables in $\boldsymbol{X}_{ist}$ include individual-level characteristics, such as race and gender, and a series of age fixed effects. They also include changing state-level macroeconomic and educational characteristics. To allow for arbitrary correlations in ε_{ist} across individuals and over time within states, we cluster the standard errors at the state level for all estimation and hypothesis testing.

The treatment effect of welfare reform is captured by the vector $\boldsymbol{\beta}_3$, which measures changes in the enrollment gap by income relative to the baseline year of welfare reform. For low-income children, a positive sign on $\boldsymbol{\beta}_3^j$ for the prereform years indicates a relative decline in enrollment in the years leading up to welfare reform; a positive sign on $\boldsymbol{\beta}_3^j$ for the postreform years indicates a relative increase in enrollment in the subsequent years.

The regression estimates of the differential time trends are reported in Table 6. Column 1 shows results from the basic linear probability model, including child age, state and year fixed effects, and the full set of covariates. The definition of low-income status in column 1 is SLE. The point estimates for the years after reform are positive, and after 3 years they are statistically significant at the 10 percent level or lower. They imply that school enrollment increased 2 percentage points more for low-income children in the years after welfare reform. The coefficient estimates for differential pretrends alternate in sign and are not generally significant. However, the estimate for 3–4 years before reform is positive and significant, which implies a prior trend toward increasing inequality that was reversed after welfare reform. This supports the claim that the relative gains observed in the postreform period were not the result of a continuation of preexisting differential trends for the two income groups. The estimates from a probit model are very similar. Column 2 reports the effects of a discrete change

Table 6
Effects of Welfare Reform on Self-Reported School Enrollment

	Ages 13–18			Ages 16–18		
	OLS (1)	Probit (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)
SLE × (7 or more years before reform)	.006	.002	.008	.002	.023	.01
	(.008)	(.005)	(.008)	(.016)	(.024)	(.034)
SLE × (5–6 years before reform)	−.002	.001	.000	−.002	.004	.031
	(.007)	(.004)	(.007)	(.014)	(.015)	(.020)
SLE × (3–4 years before reform)	.011*	.006*	.011*	.015	.016	.035*
	(.005)	(.003)	(.005)	(.010)	(.012)	(.016)
SLE × (1–2 years before reform)	−.002	.001	−.001	−.007	−.011	.012
	(.006)	(.003)	(.006)	(.010)	(.013)	(.015)
SLE × (1–2 years after reform)	−.002	.001	−.002	−.001	.004	.001
	(.008)	(.004)	(.008)	(.015)	(.018)	(.022)
SLE × (3–4 years after reform)	.011^{+}	.009**	.011	.022^{+}	.033*	.028
	(.006)	(.003)	(.007)	(.011)	(.014)	(.018)
SLE × (5–6 years after reform)	.012^{+}	.009**	.010	.022*	.03*	.038*
	(.006)	(.003)	(.007)	(.011)	(.014)	(.017)
SLE × (7–8 years after reform)	.019**	.01**	.016*	.033**	.041*	.046*
	(.007)	(.003)	(.008)	(.012)	(.016)	(.019)
SLE × (9–10 years after reform)	.016*	.007^{+}	.014^{+}	.029^{+}	.038*	.031
	(.007)	(.004)	(.008)	(.015)	(.019)	(.025)
SLE × (11 or more years after reform)	.021	.008	.019	.044	.045	.059
	(.008)*	(.004)$^{+}$	(.009)*	(.018)*	(.027)	(.033)$^{+}$
p-Value on all prereform terms	.06	.23	.04	.17	.10	.23
p-Value on all postreform terms	.01	.01	.08	.01	.03	.07
N	168,209	168,209	168,209	80,557	44,325	67,801
Pseudo-R^2	.18	.27	.18	.15	.17	.15

Note. The dependent variable is an indicator for school enrollment. All regressions include race, sex, age, state and year fixed effects and changing macroeconomic and educational characteristics. Column 3 includes interactions between SLE and the state controls. Standard errors, clustered at the state level, are in parentheses. The *p*-values are for the separate *F*-tests for which all prereform and postreform interaction terms are zero. OLS = ordinary least squares.

$^{+}$ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

from zero to one for each of the income-by-year interactions.[33] Column 3 includes a full set of interactions between the control variables ($\boldsymbol{X}_{ist}$) and the low-income indicator. Here again, the only significant prereform term is a positive estimate for the prior 3–4 years. The postreform interactions show relative gains on the order of a 2-percentage-point reduction in the enrollment gap after 7 years. This corresponds to a substantial 22 percent narrowing of the initial gap. In separate estimation, no consistent differences in treatment effects are detected for males versus females or whites versus nonwhites.[34]

The next columns of Table 6 demonstrate the robustness of the findings to changes in the estimation sample. During the sample period, the minimum age at which a child could legally drop out of school was at least 16 in all states.[35] Not surprising, when the sample is restricted to children ages 16 and older, as in column 4, the point estimates for postreform SLE interaction terms increase substantially in magnitude and remain statistically significant. In addition, excluding 18-year-olds from the sample leaves the results qualitatively unchanged.

Children from very high-income households may not form a relevant comparison group for very low-income children. Column 5 reports estimates from the linear probability model for children 16 and older using a narrower control group: children with family income under 300 percent of the federal poverty level. Column 6 limits the low-income group to children more likely to be affected by welfare reform: those in households headed by single women. In both cases, the estimated effects are similar to those in column 4: the postreform SLE interactions terms are all positive and are generally statistically significant, whereas the prereform SLE interaction terms are not jointly significant.[36]

All regressions include the full set of $\boldsymbol{X}_{ist}$ controls and state, year, and age fixed effects. The state- and time-varying controls are generally statistically insignificant, with the notable exception of the interaction term for low income and the number of years since school accountability reform. The estimates range from $-.04$ to $-.15$, which suggests that accountability reforms disproportionately increased enrollment for higher income adolescents. The estimated effects of welfare reform are insensitive to the exclusion of the accountability reform measures and their interactions with family income.

[33] Since the interpretation of marginal effects is somewhat complicated in nonlinear models with interaction terms (Ai and Norton 2003), we focus on the linear probability model.

[34] Coefficients for the interactions between the low-income indicator and indicators for years since waiver are generally, but not always, larger for girls than for boys. When the treatment effect is estimated for the interaction of the low-income indicator and a postreform indicator, the impact for girls is 1.4 percent and that for boys is 1.1 percent, with each significant at the 5 percent level. The difference in treatment effects is not statistically significant. For whites, the interaction of low income and postwaiver is .7 percent, and for nonwhites it is .5 percent; neither is significant at conventional levels.

[35] For state compulsory school attendance age, see National Center for Education Statistics (1995–2005). The oldest age of entry ranges from 5 to 8, and the youngest age of exit ranges from 16 to 18.

[36] When we limit the sample to households headed by single mothers, the sample size is reduced to only 38,404 observations. The estimated pattern is similar to that in column 7, but the point estimates are less precise.

Figures 4–7 depict the β_3 estimates from equation (4), estimated on the sample of 16–18-year-olds with alternative definitions of the low-income status indicator.[37] Bars indicate the 90 percent confidence intervals. Coefficient estimates and confidence intervals are for the interactions of the low-income indicator and the indicators for years before and after statewide waiver or welfare reform adoption. Regression models include the full set of controls, and standard errors are clustered at the state level. The sample includes children ages 16–18 in 1990–2005. Figure 4 shows the estimates in column 4 of Table 6. A similar time pattern emerges in Figure 5 when the low-income group is limited to those with FLE (income below 130 percent of the poverty level): the postreform effects are larger—more than 5 percentage points after a decade—and, again, significantly different from zero. Using the poverty level as the income cutoff level substantially reduces the low-income sample size and leads to noisier estimates of the treatment effect in Figure 6. Defining the disadvantaged sample as children whose mothers have fewer than 12 years of formal schooling also decreases the sample size, in part as a result of observations missing maternal education information. The estimates in Figures 6 and 7 confirm the break from trend around the time of welfare reform and the average cumulative effects of approximately 5 percent.

6. Conclusions

This paper presents the first analysis of the impact of the fundamental welfare reforms of the 1990s on the educational attainment of adolescents in low-income families. We draw educational attainment measures from two sources spanning the period from the early 1990s to the mid-2000s: administrative records of school dropout and completion rates from the school district records of the CCD and self-reported enrollment rates for youths ages 13–18 from the CPS. We then estimate the net effect of welfare reform in a reduced-form difference-in-differences framework, using trends in the educational attainment of youths in higher income households to impute counterfactual trends for what would have happened to those in low-income households absent welfare reform. The validity of this method is supported by the fact that the groups experience statistically indistinguishable time trends before the reforms. In the period after welfare reform, we find significant and growing gains in educational attainment for low-income youths. The result is robust to alternative measures of educational attainment and definitions of the low-income group and to controlling for contemporaneous changes in policy and the economy.

This evidence confirms and strengthens the findings of Miller and Zhang (2009) and the experimental literature that welfare reform improved the educational outcomes of children in low-income families. However, in contrast to some initial experimental findings (Gennetian et al. 2004) of small harmful impacts of welfare reform on older children, this paper presents new evidence

[37] Table A4 reports average cell sizes for different definitions of the low-income group.

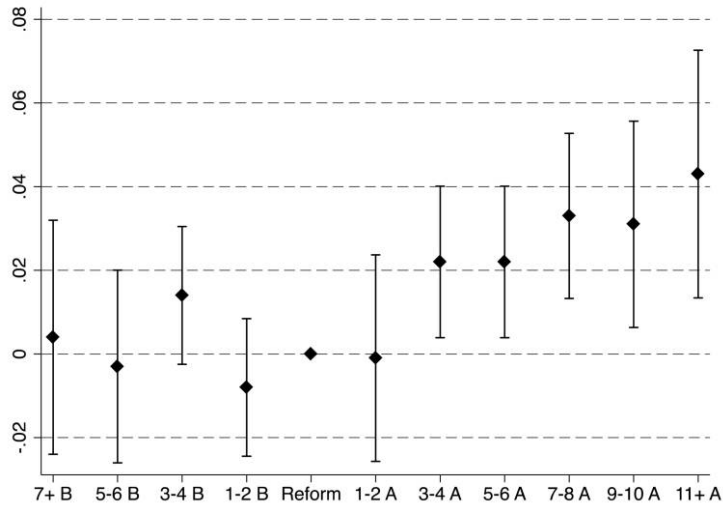

Figure 4. Differential trends in school enrollment, by subsidized lunch eligibility

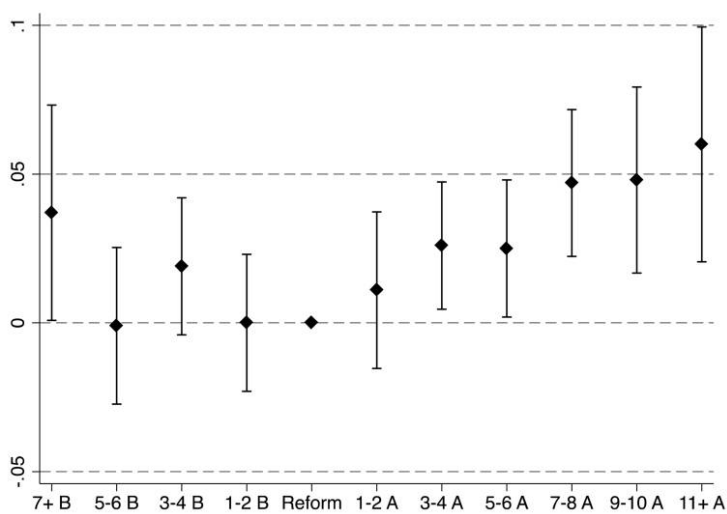

Figure 5. Differential trends in school enrollment, by free-lunch eligibility

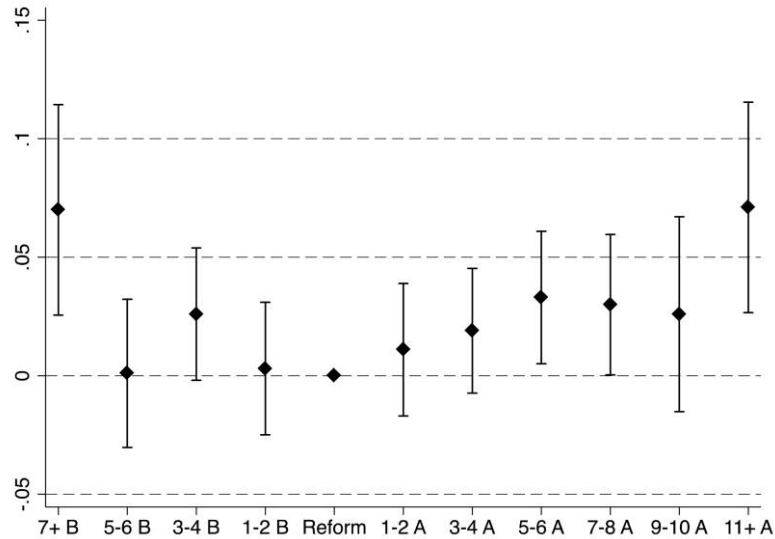

Figure 6. Differential trends in school enrollment, with poverty as the cutoff

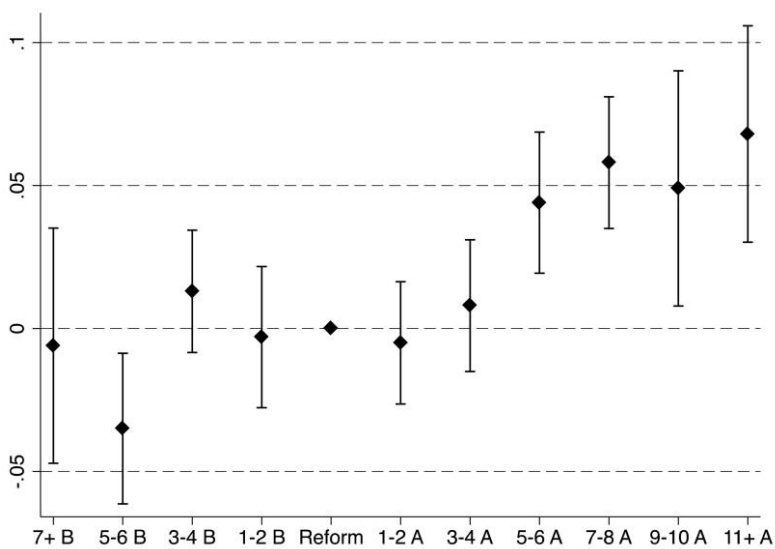

Figure 7. Differential trends in school enrollment, by when mother is a high school dropout

of gains for adolescents as well. Several differences between this paper and the experimental studies may explain the difference in results. First, the experimental studies consider shorter time horizons (2–5 years after reform). In our data, the gains from welfare reform are not immediate but increase gradually over time. Second, the experimental studies used different (often subjective) measures of schooling outcomes. Third, the experimental samples were limited to adolescents whose mothers were enrolled in welfare programs, thus neglecting the effects of reduced welfare dependence both by mothers of adolescents and by adolescents themselves. The importance of these entry effects is confirmed in observational studies that focus on teenage girls and find evidence of direct effects of welfare reform in lowering fertility and welfare participation rates (Offner 2005).

Beyond providing a new evaluation of welfare reform, this study also yields additional insight into the role of home environment and government programs outside of formal education on the educational attainment of individuals in low-income households. These findings contribute to our understanding of the determinants of school dropout and completion rates. These outcomes have received increasing policy attention in recent years, including provisions in NCLB, and their measurement and determinants have become a subject of controversy (see Heckman and LaFontaine [2010] and references therein). An important limitation of the current study is that our reduced-form estimation does not allow us to disentangle the distinct potential channels for the overall effects. Nevertheless, our finding of educational gains for both males and females, which increase with years of exposure, supports an important role for the indirect channel through home environment. Understanding the relative importance of family environment and direct employment incentives is an important area for future research.

Appendix

Additional Figures and Tables

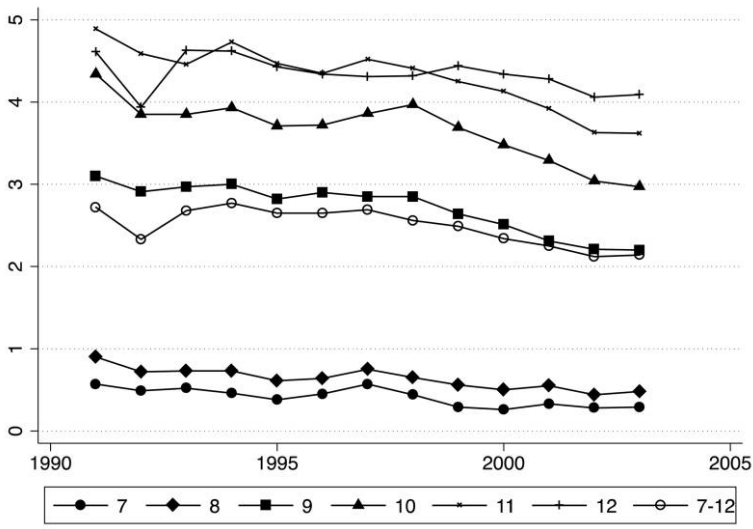

Figure A1. School dropout rates by grade and year

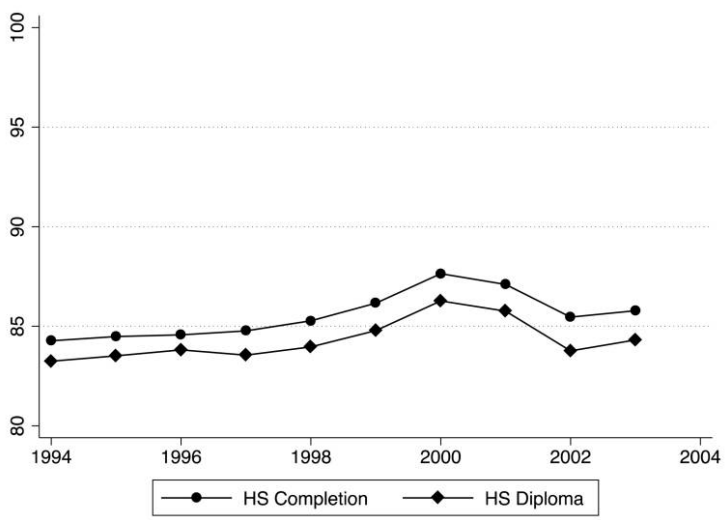

Figure A2. High school completion rates by year (National Center for Education Statistics 1991–2003).

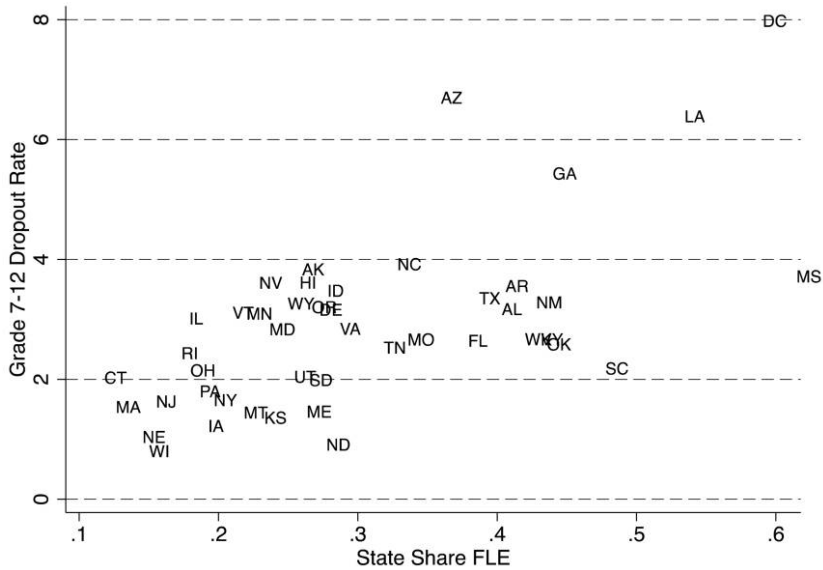

Figure A3. Statewide dropout rates and shares of free-lunch eligibility

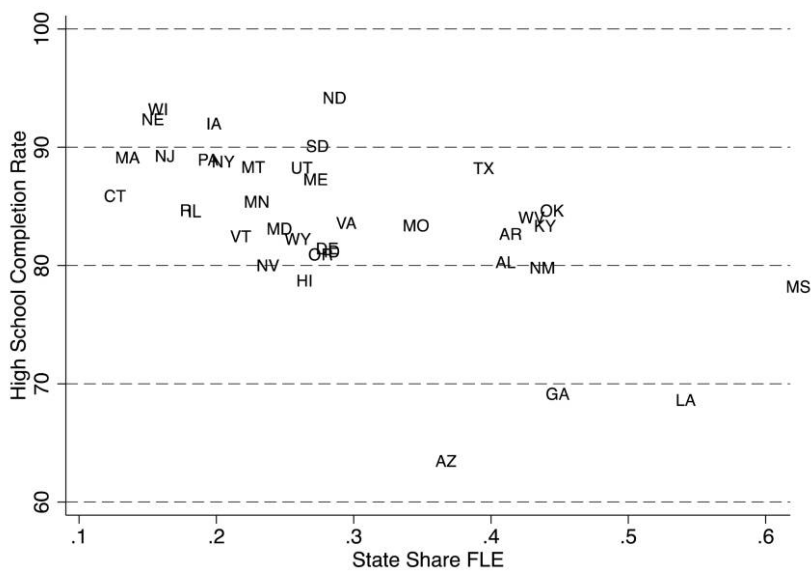

Figure A4. Statewide high school completion rates and shares of free-lunch eligibility (National Center for Education Statistics 1991–2003).

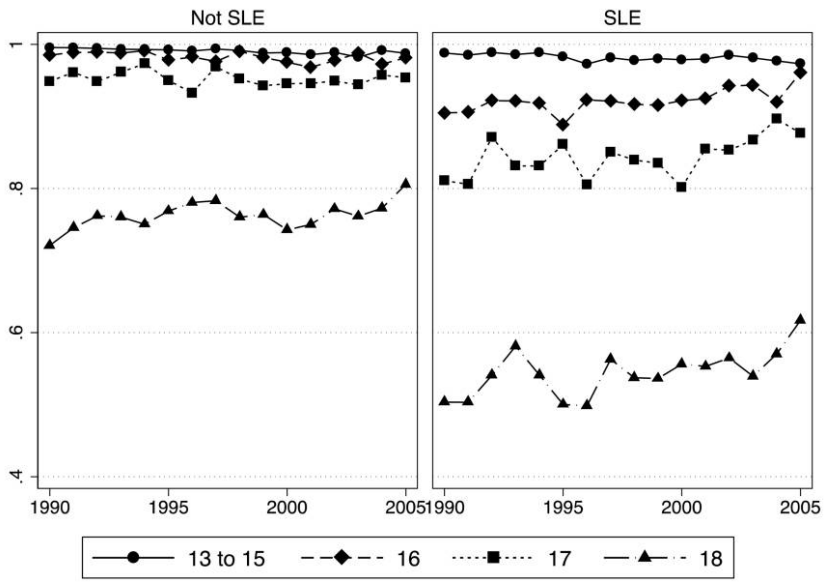

Figure A5. School enrollment rates by subsidized-lunch eligibility, age, and year (U.S. Bureau of Labor Statistics 1990–2005).

Table A1
Timing of Welfare and School Accountability Reforms

State FIPS	State or District	Earliest Statewide Waiver	TANF	Consequential Accountability	Report Card Accountability
1	Alabama		November 1996	1997	
2	Alaska		July 1997		2001
4	Arizona	November 1995	October 1996		2000
5	Arkansas	July 1994	July 1997	1999	
6	California	December 1992	January 1998	1999	
8	Colorado		July 1997		2002
9	Connecticut	January 1996	October 1996	1993	
10	Delaware	October 1995	March 1997	1998	
11	DC		March 1997		1997
12	Florida		October 1996	1999	
13	Georgia	January 1994	January 1997	2000	
15	Hawaii	February 1997	July 1997		2001
16	Idaho		July 1997		1997
17	Illinois	November 1993	July 1997		1999
18	Indiana	May 1995	October 1996		1995
19	Iowa	October 1993	January 1997	2003	
20	Kansas		October 1996		1995
21	Kentucky		October 1996	1995	
22	Louisiana		January 1997	1999	
23	Maine		November 1996		1999
24	Maryland	March 1996	December 1996	1999	
25	Massachusetts	November 1995	September 1996	1998	

Table A1 (*Continued*)

State FIPS	State or District	Earliest Statewide Waiver	TANF	Consequential Accountability	Report Card Accountability
26	Michigan	October 1992	September 1996	1998	
27	Minnesota		July 1997		1996
28	Mississippi	October 1995	July 1997		1994
29	Missouri	June 1995	December 1996		1997
30	Montana	February 1996	February 1997		1998
31	Nebraska	October 1995	December 1996		2001
32	Nevada		December 1996	1996	
33	New Hampshire		October 1996		1993
34	New Jersey	October 1992	July 1997	1997	
35	New Mexico		July 1997	2003	
36	New York		November 1997	1998	
37	North Carolina	July 1996	January 1997	1993	
38	North Dakota		July 1997	2003	
39	Ohio	July 1996	October 1996	1997	
40	Oklahoma		October 1996	1996	
41	Oregon	February 1993	October 1996	2000	
42	Pennsylvania		March 1997		1999
44	Rhode Island		May 1997	1997	
45	South Carolina		October 1996	1999	
46	South Dakota	June 1994	December 1996		1997
47	Tennessee	September 1996	October 1996	1996	
48	Texas	June 1996	November 1996	1994	
49	Utah	January 1993	October 1996	2003	
50	Vermont	July 1994	September 1996	1999	
51	Virginia	July 1995	February 1997	1998	
53	Washington	January 1996	January 1997		1998
54	West Virginia	February 1996	January 1997	1997	
55	Wisconsin	January 1996	September 1997	1993	
56	Wyoming		January 1997		1999

Sources. Dates of statewide waiver implementation and Temporary Assistance for Needy Families (TANF) implementation are from Crouse (1999) and U.S. Department of Health and Human Services (1997); dates of introduction of state accountability systems are from Hanushek and Raymond (2005), Fletcher and Raymond (2002), Goertz and Duffy (2001), and the Department of Education Web sites of Alaska, Idaho, Illinois, Iowa, New Hampshire, New Jersey, Ohio, Pennsylvania, and South Dakota.
Note. FIPS = Federal Information Processing Standard.

Table A2

Cross-State Correlation in Timing of Welfare and School Accountability Reforms

Accountability Reform	Welfare Reform		
	1992–93	1994–95	1996–97
1992–93			Connecticut
			North Carolina
			Wisconsin
1994–95			Kentucky
			Texas
1996–97	New Jersey		Alabama
			Idaho
			Nevada
			Ohio
			Oklahoma
			Rhode Island
			Tennessee
			Washington, D.C.
			West Virginia
1998–99	California	Arkansas	Florida
	Michigan	Delaware	Louisiana
		Massachusetts	Maryland
		Vermont	New York
		Virginia	South Carolina
2000–2001	Oregon	Georgia	Hawaii
2002–3	Illinois	Arizona	Alaska
	Iowa	Indiana	Colorado
	Utah	Mississippi	Kansas
		Missouri	Maine
		Nebraska	Minnesota
		South Dakota	Montana
			New Hampshire
			New Mexico
			North Dakota
			Pennsylvania
			Washington
			Wyoming

Sources. Dates of statewide waiver implementation and Temporary Assistance for Needy Families (TANF) implementation are from Crouse (1999) and U.S. Department of Health and Human Services (1997); dates of introduction of state accountability systems are from Hanushek and Raymond (2005), Fletcher and Raymond (2002), Goertz and Duffy (2001), and the Department of Education Web sites of Alaska, Idaho, Illinois, Iowa, New Hampshire, New Jersey, Ohio, Pennsylvania, and South Dakota.

Table A3

Control Variables: Summary Statistics

	N	Mean	SD	Min	Max
District-level controls:					
Expenditure per pupil	146,409	4,804	2,215	2,391	12,068
Pupil/teacher ratio	146,364	15.177	3.94	5.67	25.3
State-level controls:					
Expenditure per pupil	168,207	3,958	922.12	2,087.9	7,416.3
Pupil/teacher ratio	168,207	17.038	2.65	11.3	25.0
Share adults high school graduates	168,207	81.535	4.97	63.2	92.8
Share adults college graduates	168,207	23.869	4.56	11.1	46.4
Average state income	168,207	15,798	2,303.15	9,974.5	27,470.9
State unemployment rate	168,207	5.564	1.38	2.2	11.4
Individual characteristics:					
Age	168,207	15.406	1.69	13	18
White	168,207	.794		0	1
Free or reduced-price lunch	168,207	.347		0	1
Free-lunch eligible	168,207	.240		0	1
Poverty	168,207	.170		0	1
Mother is high school dropout	157,827	.203		0	1

Note. State-level controls are calculated using data from the U.S. Census and the U.S. Department of Education. Dollar values are in constant 1983 dollars. Individual characteristics are calculated from October Current Population Survey files (U.S. Bureau of Labor Statistics 1990–2005).

Table A4

Average Cell Sizes, by State and Definition of Low Income, for Students Ages 13–18

State FIPS	State or District	All	Household Income ≤185% FPL	Household Income ≤130% FPL	Household Income ≤100% FPL	Mother Is High School Dropout
1	Alabama	153	67	51	38	25
2	Alaska	174	44	29	19	13
4	Arizona	148	59	43	29	27
5	Arkansas	139	65	47	31	21
6	California	818	344	255	178	185
8	Colorado	156	42	26	17	17
9	Connecticut	125	23	15	11	10
10	Delaware	98	25	17	13	11
11	Washington, D.C.	70	32	23	17	13
12	Florida	394	142	95	66	50
13	Georgia	155	58	43	32	25
15	Hawaii	101	33	21	14	7
16	Idaho	170	59	38	25	19
17	Illinois	406	127	89	65	53
18	Indiana	148	40	24	15	14
19	Iowa	163	41	23	15	10
20	Kansas	169	50	29	20	10
21	Kentucky	132	52	37	28	17
22	Louisiana	135	66	51	40	23

Table A4 (*Continued*)

State FIPS	State or District	All	Household Income			Mother Is High School Dropout
			≤185% FPL	≤130% FPL	≤100% FPL	
23	Maine	136	43	26	17	7
24	Maryland	122	25	16	10	10
25	Massachusetts	242	64	43	30	23
26	Michigan	391	111	75	53	35
27	Minnesota	172	38	25	18	10
28	Mississippi	141	77	58	44	24
29	Missouri	141	45	28	18	15
30	Montana	160	65	43	29	14
31	Nebraska	168	53	33	20	10
32	Nevada	142	39	22	12	19
33	New Hampshire	126	22	12	7	6
34	New Jersey	308	68	43	29	26
35	New Mexico	153	77	59	43	30
36	New York	570	211	150	116	84
37	North Carolina	296	114	78	55	44
38	North Dakota	161	54	32	22	10
39	Ohio	406	118	81	56	38
40	Oklahoma	143	57	36	26	17
41	Oregon	131	41	27	17	12
42	Pennsylvania	385	112	73	48	34
44	Rhode Island	116	34	21	16	15
45	South Carolina	138	54	39	29	22
46	South Dakota	185	72	47	33	15
47	Tennessee	126	47	32	24	18
48	Texas	534	245	181	129	119
49	Utah	202	57	32	18	11
50	Vermont	106	31	18	11	8
51	Virginia	155	38	25	18	16
53	Washington	143	38	24	16	11
54	West Virginia	127	58	41	29	19
55	Wisconsin	179	42	27	18	14
56	Wyoming	159	50	31	20	11
Total		10,517	3,566	2,432	1,707	1,292

Note. Data are from October Current Population Survey files (U.S. Bureau of Labor Statistics 1990–2005). Values are the annual average (unweighted) number of sample observations in each state and income category. Total is the annual average number of observations across all states. FIPS = Federal Information Processing Standard; FPL = federal poverty level.

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