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The Effect of Welfare on Child Outcomes: What We Know and What We Need to Know

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Abstract

This survey discusses 8 large federal welfare programs that affect children. The available evidence is incomplete but suggests a consistent story: Programs that target services directly to children have the largest measured effects, while it is more difficult to detect possibly positive effects of unrestricted cash transfers. There are also striking and largely unexplained differences between demographic groups in the effects of some programs. The survey concludes with five questions for future research: 1) Do welfare programs have long-term effects on children?; 2) Why do programs appear to have differential effects on different demographic groups?; 3) How do programs interact; 4) How exactly do successful programs work? and 5) Are programs cost effective? These questions indicate that though we know much more than we did even 5 years ago about the effects of welfare on children, there is still much work to be done if we are to make informed decisions about public policy.

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There is broad support for the idea that welfare should benefit poor children. Yet most research on welfare programs, as well as much of the debate about welfare reform, has focused on the way that parents respond to incentives created by welfare, rather than on its effects on children. Less work has been devoted to the fundamental question of whether any of the web of programs supporting poor families benefit children.

If it can be shown that they do, then there are many other questions to be addressed: First, are the benefits short or long term? Second, which types of programs or combinations of programs are most effective; for example, do cash or in-kind programs produce bigger benefits for children? Third, do welfare programs have different effects on different groups, and if so why? Fourth, how exactly do successful programs work? And finally, can efficacious programs pass the more stringent test of cost effectiveness?

This review focuses on the 8 large federal programs shown in Table 1: Aid to Families with Dependent Children (AFDC) which has been replaced with the new Temporary Aid for Needy Families program (TANF); the Earned Income Tax Credit; Housing Assistance; Food Stamps; the Supplemental Feeding Program for Women, Infants, and Children (WIC); School Nutrition programs; Medicaid and Head Start.

The programs are evaluated with respect to their effects on the health and educational achievement of children. Where possible, documented effects on long-term outcomes are noted. The first section of the paper gives a brief discussion of "How We Know What We Know" about these programs. The evidence regarding the effects of cash programs and in-kind programs is then reviewed in sections II and III respectively.

The evidence indicates that contrary to much current publicity, the system is not entirely "broken" when judged using the metric of child well-being: there are specific programs that produce important benefits for children. Nevertheless, not all programs are equally effective, and benefits are not equally distributed across children. Hence, a review of "What We Know" about these programs can provide a useful starting point for welfare reform, as well as highlighting gaps in "What We Need to Know" in order to carry out intelligent reform. The last section of the paper discusses fruitful directions for future research and the importance of enhanced data collection efforts.

I: How We Know What We Know

A comprehensive review of the program evaluation literature is far beyond the scope of this review. However, since several

different methods are used in the studies discussed in this essay, some comment on methodology is in order. A somewhat fuller, non-technical discussion can be found in Currie (1995), or see Heckman (1990).

The fundamental problem facing researchers and policy makers is that the children of welfare recipients may have bad outcomes for reasons that have nothing to do with the receipt of assistance *per se*. It is possible that a program could have substantial benefits for poor children and still leave many children disadvantaged relative to better-off peers.

Evidently, parents of children on welfare are worse off than other parents in observable ways: They are poorer, likely to have less education, and may also have health problems. Many data sets available to researchers contain at least crude measures of these observable variables so that observed differences between parents on welfare and other parents can be accounted for using standard regression models.

To take a simple example, suppose that children of high-school dropouts have lower scores on standardized tests than children of college graduates. Then if mothers on welfare are more likely to be high-school dropouts than college graduates, a simple comparison of the two group's average scores might tell you more about the

effects of maternal education than about the effects of welfare. A simple way to "control" for the effects of education in order to focus on the effects of welfare might involve drawing a sample of high-school dropouts, and comparing children of welfare mothers to other children within this group. Any differences between the welfare children and the others could then be attributed to welfare use and not to maternal education. Multiple regression techniques simply allow one to control for the effects of several observable variables at the same time.

The problem becomes much more difficult however, if parents on welfare also differ from other parents in ways that are not observed. For example, they may lack motivation, or be discouraged by previous misfortune. Failure to properly control for these differences could lead one to incorrectly infer that it was being on welfare that was associated with negative child outcomes, rather than these underlying conditions. Some underlying problem, such as maternal depression, might cause *both* welfare dependence and negative child outcomes.

There are basically two approaches to this issue of unobserved characteristics. First, one may design a social experiment, randomly assigning eligibles to a "treatment" group and a "control" group. Random assignment assures that on average, the two groups

will have the same observed and unobserved characteristics. In principal, one can then assess the effect of the treatment simply by comparing mean outcomes for the two groups, just as one would do in a drug trial. The key advantage of an experimental evaluation is its transparency.

One disadvantage of social experiments is that they may be very expensive. But there are several disadvantages in addition to high cost (Heckman, 1990). These include differential attrition between treatments and controls (which causes the "treatment" group to become less and less like the comparison group over time); the fact that subjects assigned to the control group may not accept their fate passively (for example subjects denied training in a government program might sign up for an alternative program); and the fact that it may be difficult to use the experiment to examine differential effects of the treatment on different groups.

Non-experimental evaluations attempt to control statistically for unobserved variables associated both with participation in the program, and with the outcome of interest. One method of doing this is to find a third set of variables, called "instruments" that are associated with participation in the program, but not with the important unobserved variables. For example, a researcher interested in the effects of participation in Medicaid on child

health might argue that the generosity of state AFDC benefits is associated with participation in the Medicaid because of the link between AFDC recipiency and Medicaid eligibility, but that the level of AFDC benefits does not have any effect on child health other than through its effect on participation in Medicaid. If this assumption were true then the level of AFDC benefits would qualify as an "instrumental variable".

This instrument would be used (along with other observable characteristics of the mother) to predict Medicaid participation, and predicted participation would be substituted for actual participation in the model explaining child health. The idea is that predicted participation will depend only on observable characteristics and differences in state AFDC benefit levels, and not on the unobserved characteristics of the mother. The procedure is analogous to an experiment in which AFDC benefit levels are varied across states, Medicaid participation responds, and only this source of variation in participation rates is used to identify the effects of Medicaid on health.

The difficulty with instrumental variables techniques is that the key assumptions may not satisfied. Suppose that states with more generous AFDC benefits also have higher income populations and thus that higher incomes are associated with better child health.

Then unless one takes account of this relationship, one will tend to find a spurious positive relationship between participation in Medicaid and child health. Alternatively, suppose that states raised AFDC benefit levels in response to poor child health. Then one might observe a spurious negative relationship between predicted Medicaid participation and child health.

An alternative approach involves assuming that the relevant omitted characteristics are fixed within a family, or for the same child over time. Suppose for example that the relevant unobserved variable is maternal attitudes towards education, and that this remains fixed over some period of time. Suppose further that one sibling participated in Head Start and one did not. Then comparing the sibling who participated to the one that did not provides a measure of the effect of Head Start that is not affected by the fact that on average, mothers of Head Start children may have more positive (or negative?) views of education than other similarly situated mothers. Of course, the problem with this approach is that the relevant variable may not be fixed within households or over time.

The studies discussed below all rely on one of these methodological approaches. Their conclusions are only as valid as the assumptions underlying the chosen approach. It is in cases

where the same result has been obtained using different assumptions and data sources that we can be most confident of the conclusions.

II: What We Know About Cash Programs

a) Aid to Families with Dependent Children

The term "welfare" has usually identified with the Aid to Families with Dependent Children (AFDC) program. This oldest and largest of the federal welfare programs provided cash transfers to (predominantly female-headed) families with children. This is the program that recent welfare reforms (The Personal Responsibility and Work Opportunity Reconciliation Act of 1996) effectively ended, replacing it with the new Temporary Aid for Needy Families (TANF) program. TANF differs from AFDC because it ends the "entitlement" of all needy families to welfare benefits; because it introduces time limits on welfare benefits; and because it provides states with much more latitude in developing their own welfare programs. Nevertheless, since most of what we know about cash welfare programs comes from studies of AFDC, and because many states will respond to TANF by only gradually altering their AFDC programs, it is of interest to summarize this literature here.

Like TANF, AFDC was administered at the state level within federal guidelines. As a result, program characteristics varied

widely from state to state. For example, as of January 1993, the maximum monthly AFDC grant for a one-parent family of 4 persons varied from $164 in Alabama to $923 in Alaska (U.S. House of Representatives, 1993). On average the federal government pays 54% of benefit costs, as shown in Table 1. The continuous erosion of real AFDC benefit levels over the past 15 years provides compelling evidence of the unpopularity of this program: the average monthly AFDC benefit declined from $483 (1993$) in 1980 to $373 in 1993, even though the average family size remained constant at 3 persons (U.S. House of Representatives, 1994).

One of the problems involved in evaluating the effects of AFDC on children is that the benefits of a cash transfer program can be expected to be diffuse. Small increases in household expenditures on a wide range of items may produce overall benefits for children without affecting any one indicator a great deal. A second problem is that although income is often used as a short-hand summary of a household's socioeconomic status, it is in practice extremely difficult to separate the effects of income from the effects of other family background characteristics including neighborhoods (Mayer, 1996).

Most research about the effects of AFDC on children, focuses on the fact that daughters of women who participate in AFDC are

themselves more likely to participate (c.f. Gottschalk, 1990; Murray, 1984). What is less clear is whether the relationship is causal, or whether it merely reflects the fact that the children of the poor are more likely to be poor -- older studies tended to conclude that the relationship was not causal, but studies using more recent data have questioned this conclusion. See Moffitt (1992) for a fuller discussion of this issue.

There has been comparatively little research linking maternal AFDC participation to other child outcomes, but the empirical issues are the same. First, it is necessary to control for some measure of income as well as for AFDC status since otherwise the estimated effects of participation are likely to reflect the relative poverty of AFDC mothers. Second, within the group of poor women, one would like to control for the fact that women choose whether or not to go onto AFDC. Blank and Ruggles (1996) show that only 60% of eligible women actually take up welfare benefits. Those who do are likely to differ from those who do not in many unobservable respects.

Hill and O'Neill (1994) find that, when instrumental variables methods are used to take account of unobserved variables that might be correlated with AFDC status, AFDC participation has no effect on children's scores on a standardized test of vocabulary, given

income. Currie (1995) confirms that their results hold up even when sibling comparisons are used to account for unobserved maternal background characteristics. Currie and Cole (1993) use data from the 1979 to 1988 waves of the National Longitudinal Survey of Youth (NLSY) to examine the effect of AFDC participation during pregnancy on the utilization of prenatal care and birthweight. They use both sibling comparisons and instrumental variables methods to take account of unobserved variables that might be correlated with both participation in the AFDC program and outcomes[1] and find that AFDC participation has no additional significant effect on birthweight given income. Together, these studies suggest that income from AFDC has much the same effect on children as family income from any other source.

b) The Earned Income Tax Credit: A Comparison to the Negative Income Tax

The slack in the growth of AFDC payments over time has been taken up by the growth in expenditures on the Earned Income Tax Credit (EITC), which doubled between 1975 and 1990. The EITC was introduced in 1975 as a means of granting tax relief to low-income

[1] They instrument AFDC participation using state-level variation in program characteristics.

tax payers. Because it is administered through the tax system, the EITC is not always viewed as a welfare program. However, unlike most tax credits, the EITC is "refundable", that is, if the amount of the credit exceeds the tax-payer's federal income tax liability, then the difference is refunded. Table 1 shows that in fact most EITC expenditures are outlays of this kind rather than foregone tax dollars. The EITC differs from traditional cash welfare programs primarily because the majority of recipients work, and benefits are available to all kinds of families. Thus, it creates fewer perverse incentives than AFDC.

If it is difficult to identify the effects of cash transfers under AFDC, the problems involved in identifying the effects of the EITC are even more formidable. The fundamental problem is that the amount of the credit depends on the parents' earnings, and earnings are likely to reflect many unobserved factors relevant to child well-being. However, the EITC is in many respects similar to the "Negative Income Tax" (NIT), an income guarantee program that was subjected to exhaustive scrutiny through four large-scale social experiments, although it was never implemented.[2] The four

[2] Under a NIT, a family that earns no income is guaranteed a minimum income, **G**. Families with earnings, **Y**, receive a payment **D**, where $\mathbf{D=G-t_1Y}$. The quantity $\mathbf{B=G/t_1}$ is referred to as the breakeven level of income since workers who earn more than **B** receive no payments. If income is equal to the wage multiplied

experiments were conducted in New Jersey and Pennsylvania, Seattle and Denver, Gary Indiana, and rural areas of North Carolina and Iowa. It is important to note that the North Carolina and Gary samples were much poorer than the others.

The income guarantees paid out under the NIT program were large relative to cash transfers that have been made under the EITC. The average payments in the Seattle/Denver experiment, for example, ranged from $919. to $2031. (1972 dollars) depending on the treatment group. By way of comparison, the poverty line for a family of 3 persons was $3,099 in 1972. In 1992, the maximum EITC was $1,384 and the poverty line $11,280. Since NIT participants were randomly assigned to "treatment" and "control" groups, the NIT experiments provide a unique opportunity to assess the effects of income transfers *per se* on the well-being of children in poor families.

Despite the large transfers, findings about the effects of the NIT are inconsistent across studies and experimental populations.

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by hours worked, and workers face a tax rate **t**, then workers on the NIT earn $\mathbf{w}(1-\mathbf{t}-\mathbf{t}_1)$ for every hour of work, whereas workers with incomes above **B** earn $\mathbf{w}(1-\mathbf{t})$. That is, workers on the NIT face a higher tax rate. The EITC differs from the NIT in that the EITC has no income guarantee. Also, since at first the size of the credit increases with earnings, the EITC lowers effective marginal tax rates for the poorest rather than raising them. After a certain level of income the credit begins to be phased out, creating a higher implicit tax rate.

In addition, econometric estimates are sometimes at odds with those derived from simple comparisons of treatments and controls. For example, Kehrer and Wolin (1979) find that the mean birthweight of infants born to the treatment group in the Gary experiment was actually lower than the birthweight of the controls. Yet estimates from their structural model suggest that the infants of treatments had higher birthweights in 9 out of 12 maternal age groups.

O'Conner, Madden, and Pringle (1976), examine the effect of the NIT on child nutrition using data from the rural experiment. Among subjects in North Carolina, they found positive and significant treatment effects on nutrient intakes. However, the treatment did not appear to have any significant effect in Iowa, a finding that the authors attribute to the relative poverty of the North Carolina sample.

Maynard and Crawford (1976) found that elementary school children from NIT families in North Carolina showed statistically significant improvements in attendance, standardized tests, and grades. However, there were no effects for elementary school children in Iowa. Once again, this pattern of results is attributed to the fact that the children in North Carolina were more disadvantaged than those in Iowa. Maynard and Murnane (1979) found that in the Gary experiment the NIT treatment had positive

effects on reading scores of young children but that these effects were statistically significant only among children whose families had been in the program for 3 or more years.

Finally, in an analysis of data from the New Jersey experiment, Mallar (1977) found that teenagers whose parents were enrolled in NIT were 20% to 90% more likely to complete high school depending on the NIT plan. However, Venti (1984) found only an 11% increase in the probability of completing highschool for youth in the Seattle/Denver experiment. This lower estimate seems more probable in view of the relatively short duration of the experiments, and the many long-term factors (such as achievement in early grades) that have been linked to educational attainment. These results may also be related to the fact that, in all 4 experiments, youths in treatment households were less likely to be employed than controls (Robins, 1985).

These studies suggest that the relatively large income transfers made to families under the NIT had a positive effect on the nutritional status and educational attainment of children in the poorest families. However, the magnitudes vary greatly from study to study. Perhaps unsurprisingly, studies of the consumption effects of the NIT also show that families spent much of the subsidy on goods that may not have been directly related to the

well-being of their children. For example, the NIT appears to have had a negative effect on the labor supply of married women[3], and positive effects on housing expenditures and purchases of consumer durables (Robins, 1985; Michael, 1978).[4]

II. What We Know About In-Kind Programs

A parallel "in-kind" welfare system has grown up alongside the cash system. This system aims to directly provide for a child's "basic needs": decent housing, food, medical care, and quality early education. Table 1 shows that expenditures on virtually all of these programs have shown study growth over time (the exception being the School Lunch Program). Table 2 indicates that in contrast to stagnant AFDC caseloads, caseloads for most in-kind programs have been increasing.

Initial evaluation of these in-kind programs is more straight-forward than the evaluation of cash transfer programs because we can ask whether the program has an impact on the specific child outcome it was designed to affect. For example, we can ask whether

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[3]No convincing evidence of a link between maternal employment and children's well-being has been found. See Blau and Grossberg (1990) and Desai *et al*. (1989)).

[4] The NIT may also have increased the probability of marital dissolution, although this finding remains controversial (c.f. Cain and Wissoker, 1990; Hannan and Tuma, 1990).

receipt of housing assistance is associated with improvements in housing, or whether household participation in the Food Stamps program improves a child's diet.

We might then wish to ask whether the program has additional effects on related child outcomes. For example, better nutrition could influence a child's cognitive abilities. Also, subsidies to food and housing may influence child outcomes more generally by relaxing the family's budget constraint (see Moffitt, 1989 and Citro and Michael, 1995 for discussions of the valuation of in-kind benefits).[5] However, since the effects of income transfers are discussed above, I will focus in this section on any effects of participation in in-kind programs on the specific outcomes that the programs were designed to affect. In practice, this restriction eliminates very few studies from consideration.[6]

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[5] The National Research Council (Citro and Michael, 1995) concludes that for simplicity's sake, "near-cash" benefits such as food stamps and housing assistance should be counted at their dollar value when comparing the resources available to different households and they have a discussion of various procedures for valuing housing benefits. However, the panel also recommends that health insurance be excluded from these comparisons because it is too hard to come up with a meaningful estimate of its value to households in different circumstances.

[6] An exception that deserves mention is Meyers *et al*. (1993) who found that in a sample of poor children in Boston, those who received housing assistance were less likely to be anemic. The study did not control for selection into public housing.

a) Housing Assistance

In contrast to AFDC and food stamps, housing assistance is not an entitlement: When funds allocated to the program run out, people who are eligible must be wait-listed. It is estimated that about half of federal expenditures on housing assistance directly benefit children while the elderly are the other large group of beneficiaries.

Most expenditures are on rental assistance programs rather than on low-rent public housing (which is what many people think of as "public housing"). And since 1982, most new authorizations for rental housing assistance have been for Section 8 programs (Pedone, 1988). The Section 8 existing housing program provides rent subsidies to families who find an apartment of their own choosing, as long as the rent is below the "Fair Market Rent" established by the Department of Housing and Urban Development (HUD), and the unit meets minimum quality standards. Rental assistance typically reduces a family's rental payments to 30% of its income, after deductions for certain expenses are taken into account.

Deficient housing is hazardous to children. For example, lead poisoning is three times more common among poor children than among non-poor children and is directly related to housing conditions. The risk of accidental death is also three times higher for poor

children, and some of this increased risk may be due to hazards in the home (Starfield, 1985). In 1989, 18% of poor households (2.2 million households), lived in housing with severe or moderate physical problems compared to 7% of non-poor households.[7]

It is not known whether, in general, housing assistance enables families in deficient housing to move to adequate housing. A 1988 HUD study found that more than half of public housing households lived in projects that needed moderate to substantial rehabilitation just to meet HUD's own standards. The estimated cost of bringing these units up to standard would have exceeded $20 billion 1986 dollars (Lazere *et al.*, 1991).

Section 8 programs require families to locate a landlord willing to participate, and to arrange with the landlord for inspections and repairs within a fixed period of time. One case study of 56 single mothers in eastern Massachusetts in 1985 and 1986, found that after waiting an average of 2 years to receive a certificate, 24 women returned them unused because they were unable to find housing that met program requirements within the allotted time (Mulroy, 1988). On the other hand, there is some evidence

[7] Problems HUD classifies as severe include lack of basic plumbing facilities, serious heating breakdowns, and rat infestations. An example of a moderate deficiency, is the use of unvented gas, oil, or kerosene heaters as primary heating equipment.

that recipients of vouchers pay higher rent (Kennedy and Finkel, 1987; Apgar 1990) and move to better neighborhoods (Johnson, 1986).

The often dismal social conditions in many public housing projects must be weighed against any improvements in the physical housing stock. However, it is very difficult to identify the effects of neighborhoods and schools because any relationship we observe between neighborhood characteristics and individual outcomes could reflect the characteristics of the individual or of his or her family that placed them in these neighborhoods in the first place.

The Gautreaux program sheds light on this issue. Under the program, residents in public housing projects can apply for Section 8 housing certificates and move to private apartments. Some apartments are in predominantly white suburbs, while others are in the inner city. Although the persons admitted to the program are not a random sample of public housing residents[8], Rosenbaum (1982, 1992) asserts that the program assigns apartments in an

[8] Applicants are screened to make sure that they have paid their rent regularly, and that they have adequate housekeeping abilities. The program does not serve families with more than 4 children because few large housing units are available in the suburbs. In addition, the act of applying for an apartment in an unknown location may indicate that a person is strongly motivated to improve his or her circumstances.

approximately random manner, since people get whatever is available when they reach the top of the waiting list. He finds that 7 years after their move, children who had moved to the suburbs were 15% less likely to have dropped out of school, 16% more likely to be in a college-track program, and 34% more likely to be employed than those who had moved within the inner city. All of these differences are statistically significant at the 90% level of confidence.

These findings suggest that voucher programs can have a positive effect on the life chances of children if they enable families to find housing in better neighborhoods. On the other hand, they suggest that the disamenities associated with large public housing projects may have significant negative effects. However, the study is marred by high rates of attrition from the sample. HUD is currently conducting an experimental evaluation of a program similar to Gautreaux in four cities.[9] An experimental evaluation that took care to minimize attrition could shed great light on the possible beneficial effects of housing vouchers, and on the issue of the effects of neighborhoods more generally.

Despite their bad reputations, housing projects may be better

[9] Personal communication Lawrence Katz, Dept. of Economics, Harvard University.

than much of the housing available to poor families who do not have access to voucher programs. By combining data from the 1990 Census and the Current Population Surveys, Currie and Yelowitz (1997) are able to examine the effects of residence in public housing projects on housing quality as measured by the extent of overcrowding and the density of the housing complex. They also examine the effect on the probability that a child has been retained in grade, an important index of educational attainment. They find significant positive effects on all three outcomes.

b) The Food Stamp Program

Food stamps are issued in the form of booklets of coupons that may be used to purchase all foods except alcohol, tobacco, and hot foods "intended for immediate consumption." In contrast to AFDC, food stamps are available to all families who meet federally determined income-eligibility requirements, though AFDC recipients are automatically eligible.

The value of a family's food stamp allocation is typically much less than what the family spends on food. Hence, it is likely that the increase in the family's food expenditures will be less than the value of the food stamps because families can spend the same amount on food that they would have in the absence of the program, and use the "freed up" money for something else. In fact,

economic theory suggests that Food Stamp families may spend a little more on food because they feel wealthier, but that there should be no difference between the effects of Food Stamps and the effects of cash transfers to the family. However, recent experimental studies of food stamp "cashouts" conclude that families spend more than the expected amount of their food stamp income on food.[10]

In these cashouts, Food Stamp Program (FSP) participants were randomly selected to receive the cash value of their food stamps rather than the coupons. Fraker, Martini, and Ohls (1996) summarize the results of four of these demonstrations. In three of the four, reductions in food expenditures ranged from 7% to 22%. In one site, there was no effect on food expenditures. However, in this site, the change was introduced with little publicity, and recipients continued to receive separate checks from AFDC and the FSP, rather than one combined check. Fraker *et al*. argue that these differences can explain the fact that the switch to cash had little impact at this site.

Two intriguing hypotheses have been advanced to explain why food stamp income might have a different effect on food

[10] Non-experimental studies of this issue have proved inconclusive. See Fraker (1990a, 1990b) and Korenman and Miller (1992) for examples, and Currie (1995) for a discussion.

expenditures than cash income. First, it is possible that households view FSP benefits as a more permanent source of income than other sources--thus they are more likely to spend the money rather than saving it for a "rainy day". Second, women with children may be more likely to spend a given amount of income on food than men, and the female head of household may have more control of Food Stamp coupons (which are likely to be issued in her name) than she has over the household's cash income.[11] Neither

[11] Some circumstantial evidence pertinent to this hypothesis comes from the Washington State Welfare Reform Demonstration Program. AFDC recipients in demonstration counties had the option of choosing to receive their AFDC and Food Stamp benefits in the form of a single consolidated check rather than continuing to receive Food Stamp coupons. Over 20% of these women opted to continue receiving the coupons.

theory has been subjected to an empirical test.

c) The Supplemental Feeding Program for Women, Infants, and Children (WIC)

In addition to the Food Stamp Program, the federal government offers several feeding programs that give food directly to needy children and their mothers. The WIC program provides nutritional counseling and food supplements to pregnant and lactating mothers and their infants as well as to low-income children up to age 5. All participants must be certified to be nutritionally "at risk". WIC is funded by appropriation and the size of each year's appropriation limits the number of people that can be served. WIC is currently operated out of some 8,330 sites, and serves approximately 60% of those eligible (Jones, 1992). The law requires that the WIC program provide foods containing protein, iron, calcium, vitamin A, and vitamin C. Food packages must be appropriately tailored to meet the needs of each category of recipient.[12] In fiscal year 1991, the average monthly WIC package was valued at $31.67.

[12] The categories are children 0 to 3 months of age, 4 to 12 months, women and children with special dietary needs, children from 1 to 5, pregnant and nursing mothers, and postpartum non-nursing mothers.

Many studies find that WIC has positive effects on the utilization of prenatal care and on measures of infant health including birthweight, the incidence of low birthweight, gestational age, and infant mortality.[13] Schramm (1985) and Devaney *et al*. (1990) examine the effects of WIC on the Medicaid costs of newborns. The results are of particular interest because they can be used to compare the costs and benefits of the WIC program. Schramm found that in 1980 a dollar spent on WIC reduced Medicaid costs in Missouri by approximately $.80 in the first 30 to 45 days after birth. Devaney *et al*. examine Medicaid costs in the first 60 days after birth in 5 states and found that reductions in Medicaid costs over this period more than offset the costs of providing WIC.

Unfortunately, only two WIC studies, by Metcoff *et al.* (1985) and Caan *et al*. (1987), have used random assignment to generate a comparison group. If WIC participants are worse off than non-participants because places are scarce and only the neediest are admitted into the program, then studies that compare WIC participants and non-participants will under-estimate the effects of the program. Conversely, if WIC participants are more highly motivated or better informed than non-participants, then studies of this type may over-estimate the program's effects. Without knowing

[13] See Devaney, Bilheimer and Schore (1991) for a review.

more about the selection mechanism underlying participation it is difficult to assess the probable direction of this bias.

Still, given that the program is locally administered, the factors governing selection into the WIC program are likely to differ considerably over time and across sites. Hence, the fact that estimated effects are remarkably constant across states and over time suggests that the positive results are not entirely driven by selection. This conclusion is reinforced by a recent study by Brien and Swann (1997) who use both instrumental variables and sibling comparison methods to analyze data from the 1988 National Maternal and Infant Health Survey. They find significant effects of WIC on birth outcomes and on maternal behaviors (such as reductions in drinking while pregnant) among blacks, but they are unable to detect any effect among whites.

Studies of the effects of WIC on the nutrient intakes of children generally find positive effects (c.f. Fraker (1990)), but these studies are also plagued by possible selection bias. One way to control for bias is to follow the same child over time. The Centers for Disease Control (1978) report the results of a study that followed child WIC participants in 6 states over a two year interval. The study found that after 3 WIC visits the percent of children who were anemic fell by more than half. In addition, the

fraction of 6 to 23 month old children below the tenth percentile of length-for-age fell from 21% to 15% after three WIC visits.

Hicks *et al*. (1982) focus on 21 pairs of siblings from rural Louisiana. Because of the design of the WIC program in that state, the younger child in each pair was eligible for supplementation beginning in the third trimester of pregnancy, while the older child became eligible for WIC only after the first year. The results show that the "early supplementation" group had significantly higher scores on a range of cognitive tests.

d) School Nutrition Programs

The federal government supports 6 other programs that provide meals or monthly food supplements to low-income children. The largest is the National School Lunch Program (NSLP). The NSLP is an entitlement that operates by reimbursing schools for each meal served. School lunches are provided free to children with family incomes less than 130% of the federal poverty line, and are subsidized if the family income falls between 130 and 185% of the poverty line. In 1990, lunches were served to approximately 12.8 million students, and 10.3 million students received free lunches. The School Breakfast Program (SBP) serves fewer, typically needier, students.

The effects of school nutrition programs are controversial. Older studies found that participants had higher 24 hour nutrient intakes than non-participants, and that SBP participants were more likely to eat breakfast than non-participants (Hanes *et al*., 1984). However, more recent studies show higher intakes of some nutrients, but also higher intakes of fat and cholesterol (Gordon, Devaney and Burghardt, 1995).

Surprisingly, there have been few attempts to evaluate the effects of school nutrition programs on cognitive outcomes. In one of the more compelling studies, Meyers *et al*. (1988) examined 1092 third to sixth grade children in Lawrence, Massachusetts before and after the SBP was introduced at their school in 1987. They found that the Breakfast Program participants showed greater improvements on the Comprehensive Test of Basic Skills, relative to their initial scores, than non-participant children. SBP participation also reduced tardiness.

e) Medicaid

Medicaid is the main system of public health insurance for poor women and children. It is a federal-state matching entitlement program, administered at the state level. Table 1 shows that expenditures on children account for a relatively small

share of total Medicaid expenditures: The average expenditure on an AFDC child is $891 (1992$) compared to $3778 for an aged person (U.S. House of Representatives, 1994). Still, both expenditures and caseloads continue to grow as shown in Tables 1 and 2.

States were required to offer Medicaid coverage to AFDC recipients and until recent extensions of coverage to other groups, there was a very close link between AFDC recipiency and Medicaid eligibility. However, evidence that many children and pregnant women were not receiving adequate preventive care led Congress to expand Medicaid coverage for pregnant women and children beginning in 1984. States are now required to cover all pregnant women and children under 6 with family incomes less than 133% of the federal poverty line, regardless of family structure.[14] Beginning on July 1, 1991, states have been required to cover all children born after September 30, 1983 whose family incomes are less than 100% of the federal poverty line.

The recent 1997 Budget Reconciliation Act allocates $47 billion over the next 10 years to allow states to expand health insurance coverage to an even larger group of uninsured children, either through the Medicaid program or through separate state

[14]The coverage of pregnant women is limited to services related to the pregnancy.

initiatives. States must contribute 70% of what the state would have contributed under the matching provisions of the Medicaid program--that is, states can get federal money to expand health insurance coverage at a very favorable match rate. These new provisions make it more pressing than ever to determine the effects of public health insurance on children.

Currie and Thomas (1995) use panel data that follows the same child over time, and show that when children are covered by Medicaid they are more likely to have had *any* doctor visits in the past 6 months. Moreover, the effect of being covered by Medicaid is larger than the effect of being covered by private health insurance, which probably reflects the fact that Medicaid has no copayments or deductibles. This effect is the same for black and white children. However, white children also receive more visits for illness when they are covered by Medicaid than when they are uninsured, and this is not true for African-Americans. Thus, equivalent insurance coverage does not guarantee equal care.

Currie and Gruber (1996b) look at the effect of becoming *eligible* for Medicaid on the utilization of medical care and on child health. The effects of Medicaid eligibility are identified using the recent federally-mandated expansions of the Medicaid program to pregnant women and children described above. They find

that expansions of eligibility to pregnant women increased the fraction of women eligible from 12 to 43%. This increase was associated with an 8.5% decline in the infant mortality rate.

However, earlier extensions of Medicaid eligibility to very poor women who were already income-eligible for AFDC were much more cost effective than later expansions to higher-income women. The reason is that higher income women were less likely to become covered early in their pregnancies. Hence, they did not avail themselves of free preventative prenatal care available under the Medicaid program. There is evidence however, that hospitals enrolled eligible women in Medicaid at delivery so that costly services received by unhealthy newborns were paid for by the program. These results suggest that outreach programs designed to improve takeup could increase the cost-effectiveness of the Medicaid extensions to pregnant women.

Currie and Gruber (1996a) use the same methodology to look at the effects of extending eligibility to additional groups of low income children. They find that, although many newly eligible children did not take up coverage, becoming eligible for Medicaid reduced the probability that a child went without a doctor's visit in the past year, and also improved the quality of care as measured by the fraction of these visits that took place in doctor's offices

rather than hospital outpatient clinics or emergency rooms. These changes were linked to significant reductions in child mortality from internal causes, and had no effect on mortality from external causes (e.g. accidents). This is the pattern one might anticipate if the changes in mortality were linked to increases in the use of preventive care.

The complex relationship between formal takeup and benefits received is further explored by Currie (1995b) in a study that focuses on differences between children of immigrants and children of the native born. She shows that recent expansions of Medicaid eligibility had smaller effects on Medicaid coverage among immigrant children, but increased utilization of basic services by at least as much among immigrants as among non-immigrants.

The differences in patterns of takeup and utilization by race and natality are consistent with evidence from other countries that extensions of insurance coverage alone will not eliminate socioeconomic differences in health care utilization or health (Currie, 1995c). It is unlikely that lack of information alone can explain the differences, since black and immigrant parents are as likely as other parents to bring their children in for free preventive care when they become eligible for Medicaid. Similarly, purely cultural explanations that posit that some groups value

medical care less than others are difficult to reconcile with this evidence.

Disparities in the availability of private health insurance, in the transactions costs associated with enrolling in the Medicaid program, or in access to providers willing to accept Medicaid payments may all be important determinants of group differences. Currie, Gruber, and Fischer (1995) examine the last of these three factors and show using state-level data that increases in Medicaid fee ratios for obstetrician/gynecologists are associated with significant declines in infant mortality, presumably because of increases in either effective physician supply or in the quality of services provided.

The fraction of children with private health insurance fell over the period of the Medicaid expansions to such an extent that there was actually a small decrease in the fraction of children with any health insurance coverage (U.S. General Accounting Office, 1995). These trends lead one to suspect that public health insurance may have "crowded out" private insurance coverage. Cutler and Gruber (1996) estimate that as many as 50% of the people who became covered by the Medicaid expansions may previously have had private health insurance. While a switch from private to public insurance does not raise the fraction of children covered,

the Currie and Thomas (1995) results suggest that it may still improve the health of children by encouraging the utilization of preventive care.

Other analysts (c.f. Dubay and Kenney, 1997) point out that private health insurance coverage was declining even among groups such as single men whom one would not expect to be affected by the expansions. If one asks what fraction of the total decline in private health insurance coverage is a result of substitution towards Medicaid, the answer is approximately 15%. Clearly, much research remains to be done on the causes and consequences of the decline in private health insurance coverage.

f) Head Start

Head Start is a federal-local matching grant program that aims to improve the skills of poor preschoolers so that they can begin schooling on a more equal footing with their more advantaged peers. Unlike Medicaid, it is not an entitlement program, and only about a third of eligible children are served (Stewart, 1992). Head Start has enjoyed widespread bi-partisan support over a long period, although evidence regarding long-term effects is inconclusive. Experimental studies that focus primarily on inner-city African-American children typically find an initial positive effect on children's cognitive achievement that fades out in 2 or

3 years.

Supporters of the program argue that a narrow focus on cognitive test scores is inappropriate, given that Head Start is intended to affect a range of outcomes (c.f. McKey *et al.*, 1985). Evidence from the Perry Preschool Project which found that Head Start children were less likely to drop out of high school, engage in crime, or to become pregnant as teenagers is often cited. However, since the project included only 58 treatments and 65 controls, was funded at about twice the rate of a typical Head Start program, and did not involve a national sample, it is not clear that the findings generalize.

Currie and Thomas (1995b) examine sibling comparisons from a national sample and find that children who were in Head Start have higher test scores at the end of the program than either stay-at-home siblings, or siblings who went to other preschools. The effects are of the same magnitude for both black and white children and indicate that Head Start closes 1/3 of the gap between these children and others. But consistent with the experimental studies, they find that the effects on black children fade out rapidly. These results suggest that the positive effects of Head Start may be undermined by subsequent deprivation among these children.

In contrast, the effects on the test scores of white children

do not fade out. Moreover, white children 10 and over are significantly less likely to have repeated a grade if they attended Head Start, and are thus less likely to have experienced the age/grade delay that often leads to high school non-completion. Both black and white children who attended Head Start were more likely to be immunized than stay-at-home siblings, although there was no effect on height-for-age, a measure of long-term nutritional status.

In related work, Currie and Thomas (1996) find that Head Start has large and lasting effects on the test scores of Latino students. A closer inspection of the data reveals that these positive effects are largest for Mexican-origin children and smallest for Puerto Rican children. However, due to sample size limitations it is not possible to sort out the effects of ethnicity and the effects of region. It is possible for example, that the ethnic differences reflect differences in the programs available in New York where Puerto Rican children tend to be located, and California and Texas where Mexican-origin children are concentrated, rather than any independent effect of ethnicity *per se*.

Currie and Thomas (1996b) ask whether differences in school quality can explain differences in the pattern of "fadeout" in test

scores between whites and blacks. Specifically, the initial positive effects of the Head Start program may be undermined if Head Start children were subsequently exposed to inferior schools. And since we see fadeout for blacks but not for whites, it would have to be the case that black Head Start children are attending worse schools than other black children but that the same was not true among whites.

They test this hypothesis using a sample of 8th graders from the National Educational Longitudinal Study of 1988 (NELS). Their work builds on earlier research by Valerie Lee and Susanna Loeb (1995) who showed using these data that the schools attended by Head Start children are of worse quality in some observable dimensions than the schools attended by other children. Even controlling for family income and parent's education, children who attended Head Start have lower test scores than other children. This result is to be expected if Head Start does not entirely compensate for early disadvantages.

However, among black children, the gap between Head Start children and other children is virtually eliminated when we compare children within the same school. That is, within schools, black Head Start children do no worse than other black children. But since they perform more poorly than other children on average, they

must be attending schools in which all black children do badly. If a "quality" school is defined as one in which children do well, then these results suggests that black children who attend Head Start go on to attend schools of significantly worse quality than other black children. In contrast, among non-hispanic white children there appeared to be little difference in the schools attended to by Head Start and other children.

III. What We Need to Know

The preceding discussion is summarized in Table 3. The table presents a matrix of programs and effects. Differences in the effects of programs across groups have been suppressed, although one theme that has emerged from the discussion so far is that they are important. The most striking feature of Table 3 is that there are many empty cells -- we clearly need to learn a great deal more about the effects of welfare before we can make informed public policy. In some cases, research has been limited by lack of appropriate data. In others, existing information has not yet been fully exploited. This section highlights some unanswered research questions, and discusses the extent to which better data collection efforts could help.

a) Effects of Welfare on Long-Run Outcomes

Ultimately, what many people care about is whether investments in children today will produce productive, well-socialized adults tomorrow. However, Table 3 highlights the fact that little is known about the effects of welfare on long-term outcomes. Lack of data places major limitations on this type of research. Many important outcomes can only be examined 10 to 15 years after childhood participation in welfare programs. There are few existing data sets that combine information about childhood participation in welfare, other family background characteristics, and the outcomes of interest.

One exception is the National Longitudinal Survey's Child-Mother file (NLSCM). The NLSCM contains information about the children of a sample of approximately 6300 women who were between the ages of 14 and 21 in 1978. Information about childhood participation in AFDC, the Food Stamp Program, Medicaid, Head Start and WIC is available. By the time the 1994 wave is released, there will be over 800 children over 16. Of course, since these children will have been born to young mothers, they will not be a nationally representative sample of 16 year olds. Still, this sample is a valuable resource. If future waves of the survey continue to be funded, it will grow in size and in representativeness, and allow us to address many questions about the relationship between welfare

and long-term outcomes such as schooling attainment, teen parenthood, and crime.

A second exception is a special supplement to the Panel Study of Income Dynamic (PSID) that was fielded in 1995. This module contains retrospective information about early childhood education and criminal activity which can be linked to data about welfare participation from the original PSID file. The PSID is currently undertaking an even more ambitious data collection effort, the 1997 Child Development Supplement. The survey of 3,500 zero to twelve year old children will have assessments of cognitive, behavioral and health status. Data are being collected from the mother, a 2nd caregiver, the absent parent (if relevant), teachers, school administrators, and the children themselves. The survey will also include time diaries for caregivers, children, and teachers, to examine inputs into child development. Finally, other inputs such as resources in the home and neighborhood will also be measured.

Once again, this information can be linked to data about welfare participation from the main files, and followup on these children may help to identify long-term effects of participation. Fielding this type of supplement to existing data sources promises to be a cost-effective method of providing information on the link between the current outcomes of young adults and their

participation in various programs as children.

An additional issue that can be addressed is whether there are links between the short-term outcomes that have been examined in previous research and longer-term outcomes. If it is found that particular short-term outcomes are reliable "markers" for longer-term outcomes, then future evaluations of welfare programs may not require as much costly long-term followup of the participants.

b) Why Do Effects Appear to Vary with Race, Ethnicity, and Natality?

The PSID and NLSCM data sets will both support analyses stratified by race, ethnicity, and natality. However, in many cases the sample sizes are very small. In order to properly document differences in outcomes, or even in utilization, it will be necessary to add questions to existing large-scale data sets. For example, the Census asks questions only about the use of cash welfare, even though expenditures on in-kind programs constitute the largest and fastest growing share of the welfare bill.

A second problem is that large-scale individual level data sets typically lack information about neighborhoods and administrative procedures that could be used to test specific hypotheses about group differences. For example, one might believe that black children on Medicaid receive fewer visits for illness

than white children because the providers that serve them are over-crowded and it is more difficult to get additional appointments. It would be very useful to know the extent to which group differences are associated with the administration of welfare programs, rather than with differences in parental tastes or circumstances.

It is unlikely that many detailed questions of this type will be added to large-scale surveys, but it would be possible to match data from other sources to the surveys if finer geographical information were made available to researchers. While issues of confidentiality are important, the amount of information that could be gained if it were routinely possible to match survey data to say zip-code level data from other sources can hardly be under-estimated.

This type of matching is also greatly facilitated by the existence of a central agency that collects program information (and is willing to give it to researchers!). There is a real danger that further devolution of responsibility for welfare to the states will result in a loss of information about the administration of programs, making it more difficult to identify program effects using state-level variation in the programs.

c) How Do Programs Interact?

One glaring omission from this survey is that there has been no discussion of multiple program participation. Many children are covered by more than one program. For example, AFDC participants are covered by Medicaid and are automatically eligible for Food Stamps. As of 1990, half of AFDC children received free school lunches, 35% lived in public or subsidized rental housing, and 19% participated in WIC. Conversely, half of all Food Stamp recipients, 42% of Medicaid recipients, 38% of WIC recipients, and 24% of those in public housing also received AFDC. Moffitt (1992) estimates that in 1984, 26.4% of nonelderly single-parent families received AFDC, Medicaid, and Food Stamps, and 11% received at least one benefit in addition to AFDC.

It is impossible to say how multiple program participation affects the child outcomes discussed above since there has been little research on this topic. Some programs may be duplicative, while others may interact to produce more positive outcomes. For example, Currie and Thomas (1995b) found that children in Head Start were more likely to be immunized than other children, even though many Head Start children would have been eligible for free vaccinations under the Medicaid program in any case. Head Start may help families to enroll in Medicaid, help them locate a Medicaid provider, or it may bypass Medicaid altogether by

arranging for children to be immunized at the Head Start center.

An analysis of multiple program participation would assist us in answering the question of whether the current patchwork system of programs is an efficient way to provide welfare. The proliferation of programs increases possibilities for fraud, waste, and mismanagement. On the other hand, the evidence surveyed here suggests that targeting specific benefits directly to individual children has advantages in terms of ensuring that specific benefits are received. We need to know more about the balance between these benefits and costs.

d) How Do Successful Programs Work?

Data limitations place severe restrictions on our ability to look inside the "black box" of welfare programs. For example, we can show that expansions in Medicaid eligibility have been related to reductions in child mortality rates at the state level, but we do not know why. It could be due either to increased use of preventive care or to more intensive palliative care for sick children. The two possibilities have quite different implications for child well-being as well as for efficiency and program costs. Better information about what goes on during doctor visits, and about objective measures of child health status (short of mortality

statistics) could help us to address this question. It might be possible, for example, to add questions about anemia, lead poisoning, and anthropometrics (e.g. height-for-age, or weight-for-height) to the next National Health Interview Survey.

Still, the most likely scenario is one in which we chip away at these questions using an interative, multi-disciplinary approach: Analysis of large scale surveys can be used to develop broad hypotheses, which can then be tested using case studies. The case studies can then be used to develop more precise hypotheses about the survey data, and to suggest supplemental survey questions.

e) Cost-Effectiveness

Evidently, if a program has no effect at all on a desired outcome, then it cannot be considered cost effective. Many of the programs discussed above have passed this initial test--they can be shown to have positive effects. The question remains however, of whether they are cost effective, that is whether the benefits outweigh the costs. The figures discussed above for WIC are quite impressive in this regard. Cost effectiveness studies exist for other small-scale early intervention programs (not reviewed here), but have not generally been conducted for large-scale federal programs. Although it is unlikely that there will be agreement on

all of the costs and benefits that should be included in such an analysis, some rough calculations under varying assumptions would no doubt be useful to policy makers.

IV. Conclusions

This survey discusses 8 large federal welfare programs that affect children. The available evidence is incomplete but suggests a consistent story: Programs that target services directly to children have the measured largest effects, while unrestricted cash transfer programs have the smallest, perhaps because their benefits are more diffuse or because the amounts of money involved are typically quite small.

There are also striking and largely unexplained differences in the effects of some programs by race, ethnicity, and/or natality. These differences could reflect non-linearities in the effects of programs -- that is, one might expect larger effects for poorer than for richer children and children from some groups are more likely to be poor. Alternatively they may reflect differences in the programs available to children of different origins, or unobserved differences between participants from different groups that have not been adequately accounted for.

The survey concluded with five questions for future research:

1) Do welfare programs have long-term effects on children?; 2) Why do programs have differential effects by race, ethnicity and natality?; 3) How do programs interact; 4) How exactly do successful programs work?; and 5) Are programs cost effective? These questions indicate that though we know much more than we did even 5 years ago about the effects of welfare on children, there is still much work to be done if we are to make informed decisions about public policy.

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