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The Effect of Food Stamp Cashout on Food Expenditures

An Assessment of the Findings from Four Demonstrations

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ABSTRACT

Using findings from four demonstrations, we examine estimates of the effect on household food expenditures of converting food stamps into a cash transfer. We provide arguments for why the estimate produced by one of the demonstrations should be regarded as an outlier. The disparity in the difference-in-means estimates from the remaining three demonstrations is reduced when each is normalized by dividing by the average value of benefits in the respective demonstration site. The normalized estimates imply a reduction in food expenditures of between 18 and 28 cents for each dollar of food stamps cashed out. At the aggregate level, these estimates imply that nationwide cashout would result in a reduction in household food spending of between 4.2 and 6.5 billion dollars, whereas the associated reduction in the cost of administering the Food Stamp Program would be about 0.3 billion dollars.

I. Introduction

Are food stamps more effective than cash assistance in increasing expenditures on food by low-income households? Beginning with Southworth's (1945) theoretical analysis of methods to subsidize food consumption, a large literature addressing this question has accumulated. More recent articles on this

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topic (Basiotis et al. 1983; Senauer and Young 1986; Devaney and Fraker 1989) have tended to be empirical in nature, rather than theoretical. The principal empirical research strategy has been that of estimating the marginal propensities to spend on food out of food stamps and cash income by regression methods applied to nonexperimental data. In drawing inferences about the possible effect of cashing out food stamps, this literature has assumed that ordinary cash income has the same effect on food expenditure as food assistance in cash.

In a review of 19 such studies, Fraker (1990) reports that most estimates of the increase in food expenditure resulting from an additional dollar of food stamp coupons range from 17 to 47 cents, compared with estimates of 5 to 13 cents for an additional dollar of ordinary cash income. These estimates have two implications. First, they suggest that each additional dollar of food stamp benefits increases food expenditure by much less than a dollar. Second, when combined with the assumption that cash food assistance and ordinary cash income are equivalent, these estimates imply that the cashing out of food stamp benefits would result in a large reduction in food expenditures: for each dollar of food stamp benefits cashed out, substantially less would be spent on food.

It should be noted that the latter conclusion is at odds with the main prediction of the Southworth-type neoclassical model—that the cashing out of food stamp benefits would affect the food expenditure behavior of only those families whose food stamp benefits exceed their desired levels of food expenditures. Survey data have consistently shown that these are only a small minority among households that receive food stamps, which implies that cashout should have only a minor impact on food expenditure on average.

In addition to its theoretical interest, the issue of the effectiveness of cash food assistance in increasing expenditures on food has important implications for welfare policy. The cost of administering a coupon-based program is believed to be substantially greater than that of administering a program that delivers benefits in the form of a cash equivalent, such as checks. If the difference in effectiveness is small between coupons and a cash-equivalent benefit, then the potential savings in administrative costs may warrant the conversion from food stamp coupons to food stamp checks. Additional potential benefits of cash issuance may accrue to recipient households in the form of reductions in the stigma associated with obtaining and using food stamp benefits, and the development of the budgeting and shopping skills that are needed to successfully manage personal finances outside of the welfare system.

A substantial limitation of the body of empirical research discussed above is that it is based on nonexperimental data in which only cash income and food stamps are observed. Until recently, there has been little experience in using cash alternatives to the usual food coupons in distributing benefits to eligible low-income households under the Food Stamp Program (FSP).¹ To overcome

1. Prior to the cashout experiments that are the subject of this article, the only U.S. experiences with the issuance of food stamp benefits in the form of checks rather than coupons were in Puerto Rico, where households have been receiving food assistance in the form of checks since 1982, and in Utah, Vermont, and portions of six other states, where a 1981 demonstration program provided elderly and disabled persons with food stamp benefits in the form of a check. Evaluations of these examples of food stamp cashout by Butler, Ohls, and Posner (1985) and Devaney and Fraker (1986) found no statistically

this limitation, in the late 1980s the Food and Consumer Service (FCS) of the U.S. Department of Agriculture, which administers the FSP at the federal level, authorized a number of demonstrations of food stamp cashout—the issuance of food stamp benefits in the form of checks rather than the traditional coupons. These demonstrations provided the opportunity to collect and analyze data on the food consumption behavior of food stamp recipients in the presence of actual cash food assistance benefits. FCS sponsored evaluations of four of the demonstrations—two in Alabama and one each in Washington and in San Diego County, California.

These evaluations consistently found that cashout resulted in reductions in the cost of administering the FSP. However, the findings regarding effects on household food expenditures appear to be far more heterogeneous. Carlson (1993) summarizes the evidence from the four evaluations as follows:

Cashout appears to reduce household food expenditures, but the size of the reduction remains uncertain. Three of the evaluations found statistically significant reductions in food expenditure. The reduction in San Diego was relatively modest, the reduction in Alabama ASSETS was substantially larger, and the reduction in Washington fell between the two. In the Alabama “pure” test, however, there were virtually no differences between households with checks and coupons.

The purpose of this article is to review the evidence produced by the four evaluations and to offer explanations of why the results appear to differ so widely. We show that the discrepancies can largely be reconciled, and a tighter range of estimated effects of cashout can be obtained. The latter is important if the results of these evaluations are to be used to forecast the likely effect of the extension of cashout to other localities or to the entire caseload.

We begin by discussing the designs of the four demonstrations and their evaluations. We then summarize the estimates of the impact of cashout that were presented in the original evaluation reports and explore possible explanations for the discrepancies among these estimates.

II. The Designs of the Cashout Demonstrations and Their Evaluations

Each of the four demonstrations of food stamp cashout sponsored by FCS in the late 1980s had one of two basic evaluation designs: an experimental design with random assignment of households to treatment and control status; or a quasi-experimental design with matched treatment and comparison sites. In addition, in two demonstrations cashout was the only policy change and was implemented only on a fraction of the FSP caseload, while in the other two demonstrations cashout was part of a more comprehensive change in the delivery of welfare benefits and was implemented on the entire caseload.

significant effects on household food expenditures. Because of the unique populations studied, the findings cannot be generalized with confidence to the entire food stamp caseload.

A. Demonstrations of “Pure” Cashout

Two of the demonstrations entailed only a single policy change—the issuance of food stamp benefits in the form of checks rather than coupons. These demonstrations, referred to as the “pure” food stamp cashout demonstrations, were conducted in San Diego County and in 12 counties in Alabama. In San Diego, 20 percent of new and continuing food stamp households were randomly selected to receive their benefits in the form of checks rather than coupons. Approximately 600 of these households were selected to serve as the treatment sample for the impact evaluation. A control sample of 600 households that received the traditional coupons was also randomly selected. In the 12 Alabama counties, only 4 percent of the caseload was selected to receive cash benefits and was fully used for the evaluation. The Alabama treatment and control samples each included approximately 1,200 households.

B. Demonstrations of Cashout as a Component of Comprehensive Welfare Reform

In each of the other two demonstrations, food stamp cashout was just one component of a comprehensive package of reforms to the welfare system. This included reforms of cash welfare programs, medical assistance, job training, and child care, as well as food assistance. These demonstrations entailed the implementation of the reforms in selected demonstration counties, each of which had been paired with an economically and demographically similar comparison county in which the reforms were not implemented. Five such pairs of counties participated in the Washington Family Independence Program (FIP), while three matched pairs of counties participated in the Alabama Avenues to Self-Sufficiency through Employment and Training (ASSETS) program.² In the FIP demonstration counties, all eligible applicants for both food stamp benefits and Aid to Families with Dependent Children were issued food stamp benefits in the form of checks. In the ASSETS demonstration counties, the entire food stamp caseload was issued food stamp checks. The impact evaluations were based on data for approximately 800 households in Washington and 1,400 households in Alabama, divided roughly equally between demonstration and comparison counties.

C. Advantages and Limitations of the Two Designs

There are two key advantages in the design of the pure cashout demonstrations for the purpose of obtaining unbiased estimates of the impact of cashout on food expenditure. First, the design entailed the random assignment of recipient households to treatment (checks) or control (coupons) status, which in principle should result in treatment and control samples that differ systematically only with respect to the receipt of the treatment. In particular, there should be no systematic differences between the samples in characteristics that might influence the outcome of interest and thereby bias estimates of the treatment effect.³ By contrast,

2. The treatment and comparison counties involved in the Alabama ASSETS demonstration did not overlap with those chosen for the Alabama pure cashout demonstration.

3. The degree to which this was achieved in the cashout demonstrations is discussed later in this section when we review sample characteristics from the four sites.

the matched treatment/comparison site design is more vulnerable to the biasing effect of preexisting differences between treatment and comparison sites. Another advantage of the design of the pure cashout demonstrations, in addition to the use of random assignment at the household level, is that cashout was the sole policy change, thus eliminating the risk that the observed outcome might be entirely attributed to cashout when, in fact, it was the result of multiple policy changes.

On the other hand, the FIP and ASSETS demonstrations, with their full-saturation implementation of cashout, made it possible for the evaluations of these demonstrations to capture *community effects* on the outcome measures. In the context of cashout, community effects are changes made in response to cashout by individuals or institutions other than the actual food stamp recipients that in turn could cause food stamp recipients to alter their food expenditure behavior. For example, the raising of rents by landlords after learning that the food stamp recipients who live in their apartments have higher cash incomes could possibly result in a community effect, especially if housing patterns are such that food stamp recipients are spatially concentrated. Demonstrations that entail the random assignment of a small fraction of FSP recipients to cashout status are generally incapable of capturing community effects because the policy change is generally imperceptible to the community at large.

D. Data and Analytic Approach

The evaluations of the four cashout demonstrations each included a cross-sectional survey of the recipients of food stamp checks and coupons, which took place several months after the initial issuance of food assistance checks. The survey obtained information on household economic and demographic characteristics, as well as expenditures on food. In the FIP evaluation and the two evaluations of pure cashout, the measure of food expenditure was constructed on the basis of detailed information provided by the surveyed households on their use of food at home during the seven days preceding the interview. Specifically, the expenditure measure is the money value of all purchased food used by a household from its home food supply during the reference period. In the remainder of the article, we use the term “food expenditure” to refer to what would more accurately be referred to as “the money value of purchased food used at home.” The evaluation of the Alabama ASSETS demonstration collected data on aggregate expenditures during the month preceding the interview on food used at home.

The analytic approach used in all four evaluations was that of computing the differences in cross-sectional mean values of food expenditure (and other outcomes) between the samples of cash and coupon recipients. These cross-sectional differences between the treatment group and the control or comparison group are used to make inferences about the effects of cashout. This analytic approach is strongest in evaluations in which the two groups are generated by random assignment of individual households. In the case of random assignment at the site level, a more robust statistical design entails collecting data on food expenditures at the demonstration and comparison sites both *before and after* the implementation of cashout. This makes it possible to compute *pretest-posttest* changes in key

outcomes at the microlevel, and to use a difference-in-differences estimator, rather than relying solely on posttest cross-sectional differences between the sites. The latter are biased if there are systematic differences in food expenditures between treatment and comparison sites that exist independently of cashout. Moreover, even in the case of random assignment of individual households, the availability of pretest and posttest observations increases the precision of the estimated effects (holding sample size constant) and dampens differences between treatment and control samples that may arise due to chance. Unfortunately, limitations on data collection resources made it impossible to implement a pretest versus posttest design in any of the cashout evaluations.

Table 1 provides summary information about the household samples for each of the four cashout demonstrations. There are very substantial differences in household characteristics across the four demonstrations, reflecting differences in their settings and in the target groups used in the demonstrations. For instance, AFDC participation ranged from 100 percent at the Washington site, where the demonstration was limited to AFDC participants, to below 30 percent at the Alabama sites. To take another example, the percentage of Hispanics ranged from about 30 percent in San Diego to essentially zero in the Alabama sites. However, while there is substantial variation across sites, for the most part the treatment and the control or comparison groups are very similar *within* sites, particularly for the two randomized experiments, as one would expect.

Complete details on the designs of the four cashout demonstrations, data collection and analysis procedures, and empirical findings are provided in the individual evaluation reports, which are available from the National Technical Information Service.⁴ In the next section we summarize the major results from evaluations of these demonstrations that pertain to expenditures on food used at home. The individual evaluation reports provide estimates for a broader range of outcomes, including effects on the nutritional quality of food used, reliance on food banks and soup kitchens, and program costs.

III. The Impact of Cashout on Food Expenditures

Among other research objectives, the evaluations of the four cashout demonstrations sought to answer the question, "How large is the reduction in expenditure on food by food stamp recipients that is caused by the introduction of cashout?" The basic strategy adopted by each of the evaluations was to compare the mean expenditure on food used at home by check recipients with the corresponding mean value for coupon recipients.⁵ One-tailed t tests were con-

4. Copies of the four evaluation reports can be obtained by calling NTIS (703-487-4650) and referring to the following acquisition numbers: Ohls et al. 1992, #PB94-207792; Fraker et al. 1992, #PB94-208899; Davis and Werner 1993, #PB94-207271; and Cohen and Young 1993, #PB95-232336.

5. Each of the four evaluations also conducted a supplementary multivariate regression analysis of the effect of cashout on food expenditures. In each case, the regression estimate of the cashout effect was essentially the same as the simple difference in mean expenditure values. In no case did the statistical test based on the regression analysis lead to a conclusion that was qualitatively different from that based on the simple difference in mean values.

Table 1
Sample Characteristics for Four Demonstrations of Food Stamp Cashout

	San Diego Pure Cashout	Alabama Pure Cashout	Washington FIP	Alabama ASSETS
Percentage AFDC				
Treatment group	88	26	100	18
Comparison group	89	27	100	22
Mean cash income				
Treatment group	\$873	\$446	\$646	\$636
Comparison group	\$888	\$441	\$687	\$676
Monthly food stamp benefit				
Treatment group	\$116	\$169	\$193	\$174
Comparison group	\$116	\$169	\$176	\$169
Percentage Hispanic				
Treatment group	31	—	6	—
Comparison group	32	—	10	—
Percentage African American				
Treatment group	20	68	5	51
Comparison group	23	69	11	65
Percentage with elderly member				
Treatment group	1.9	25	0.3	23
Comparison group	2.3	24	2.4	24
Percentage where household head completed high school				
Treatment group	56	41	73	26
Comparison group	58	40	67	22
Percentage with children				
Treatment group	92	56	99	57
Comparison group	93	61	98	62
Percentage with earned income				
Treatment group	20	29	23	34
Comparison group	22	30	27	34
Average household size				
Treatment group	3.3	3.0	3.2	2.6
Comparison group	3.4	2.9	3.3	2.8

Source: Ohls et al. (1992), pp. 36, 40, and 41 (San Diego pure cashout); Fraker et al. (1992), pp. 56 and 59 (Alabama pure cashout); Cohen and Young (1993), pp. 27 and 29 (Washington FIP); Davis and Werner (1993), pp. 27–29 (Alabama ASSETS).

Table 2
Mean Weekly Expenditure per Adult Male Equivalent on Food to Be Used at Home

Cashout Demonstration	Check Recipients	Coupon Recipients	Absolute Difference	Percentage Difference
San Diego pure cashout	\$29.63	\$31.82	-\$2.19*	-6.9%
Alabama pure cashout	29.43	29.50	-0.07	-0.3
Washington FIP	24.71	29.31	-4.60*	-15.7
Alabama ASSETS	21.03	26.95	-5.92*	-21.9

Source: Ohls et al. (1992), p. 48; Fraker et al. (1992), p. 64; Cohen and Young (1993), p. 34; Davis and Werner (1993), p. 34.

* Difference is statistically significant at the 95 percent confidence level.

ducted on the differences in means in order to determine whether the empirical evidence could support a conclusion that cashout resulted in a reduction in food expenditures.

The estimates presented in Table 2 are in terms of weekly food expenditures per adult male equivalent (AME).⁶ The four evaluations produced a wide range of estimates of the effect of cashout on food expenditures. The evaluation of the Alabama "pure" cashout demonstration (hereafter, simply the Alabama demonstration) found no evidence of any reduction in household food expenditures, whereas cashout in the context of the Alabama ASSETS demonstration (hereafter, simply the ASSETS demonstration) was found to result in a (statistically different from zero) \$5.92 reduction in weekly food expenditures per AME. This represents an average reduction of 22 percent, relative to the food expenditure of coupon recipients. The evaluations of the San Diego and Washington FIP demonstrations found statistically significant cashout-induced reductions in food expenditures between these two extreme values: a \$2.19 reduction in San Diego (6.9 percent of the average expenditure by coupon recipients) and a \$4.60 reduction in Washington (15.7 percent).

From a policy perspective, these estimates are so diverse that, as they stand, they provide little basis to draw guidance regarding the likely magnitude of the reduction in food expenditures if cashout were to be adopted in some other location, or in the nation as a whole. The zero effect estimate produced by the

6. Age- and sex-specific recommended dietary allowances for food energy (National Research Council, 1989) were used to compute household size in adult male equivalent persons. The procedure weights each household member by the recommended dietary allowance (RDA) for that member based on a given nutrient, typically food energy, relative to the RDA for that nutrient for an adult male age 23 to 50 years. For instance, if a child has, say, half the food energy requirements of an adult male, the child is assigned a weight of .5 in these calculations. The sum of the household member weights within a household gives household size in adult male equivalents.

Alabama evaluation even introduces doubt as to whether cashout would result in any reduction in food expenditures.

IV. Accounting for the Variation in Findings from the Four Demonstrations

There are several plausible sources of discrepancy in the estimates of the effect of cashout on food expenditures obtained from the four demonstrations. These sources fall into two categories: those related to differences in the way cashout was implemented, and those related to differences in the economic circumstances of food stamp recipients, independent of the cashout demonstrations.

A. Differences in the Way Cashout Was Implemented

The first and most evident discrepancy in the findings from the four demonstrations is the absence of any cashout effect in the Alabama demonstration vis-à-vis a positive and significant reduction in food expenditures in the other three demonstrations. However, the way cashout was implemented in the Alabama demonstration deviated sharply from its implementation in the other three demonstrations, including the one conducted in San Diego. The major differences are in the planned duration of cashout and in the way cash food assistance benefits were issued in the four sites.

The San Diego, FIP, and ASSETS demonstrations were designed to continue for four years or longer. With much publicity, state and local political leaders and welfare officials introduced these demonstrations as incorporating important long-run reforms to the FSP. In contrast, pure cashout in Alabama was designed from the onset to be a short-term demonstration, and was introduced with little fanfare. It actually lasted for only eight months. Given the brief duration of this demonstration, many check recipients may have decided to use the check benefits in the same way that they had previously used food stamp coupons. A household's adoption of new budgeting, shopping, and food-use patterns would make more sense if the cost of learning the new patterns could be amortized over a longer period of time.

The relationship between the issuance of food stamp checks and the issuance of other assistance benefits also varied across the four sites, with the Alabama demonstration again deviating sharply from the pattern in the other demonstrations. In Alabama, food stamp checks were issued independently of other assistance checks. In contrast, in the San Diego, FIP, and ASSETS demonstrations, a household that participated in both food stamps and AFDC was issued one check for the combined amount of the two benefits. A notice accompanying the check provided a breakdown of the benefit into its component parts, but during interviews conducted as part of the evaluation of the San Diego demonstration, most recipients of a combined check were unable to report the approximate amount of their food stamp benefit. This intermingling of the food stamp and AFDC benefits may have compromised the ability of recipients to reserve the

food stamp benefit for the purchase of food, thus resulting in a larger reduction in the use of purchased food at home than would have been the case if the two benefits were issued independently. The percentages of food stamp participants who also participated in the AFDC program and therefore received a combined benefit check are 100 percent in the FIP demonstration, 83 percent in the San Diego demonstration, and 19 percent in the ASSETS demonstration.

To summarize, the limited duration of cashout and the issuance of separate food stamp and AFDC benefit checks are important ways in which the Alabama pure cashout demonstration differed from the other three demonstrations. These design features are ones that would tend to result in a small cashout effect in the Alabama demonstration, and may explain why the estimate of that effect is an outlier when compared with findings from the other three demonstrations.

But even when the finding of no cashout effect on food expenditures from the Alabama demonstration is put aside, we are still left with a set of rather heterogeneous findings. The San Diego results are at the low end of the spectrum, with a 7 percent reduction in food expenditures, while the ASSETS results are at the opposite extreme, with a 22 percent reduction. Two other design features of these demonstrations may account for part of this divergence.

The first is a design difference between the San Diego demonstration and the other two demonstrations. The design of the San Diego demonstration was one that would capture few, if any, community effects of cashout, because only 20 percent of the caseload was cashed out at the time the evaluation was conducted. This could potentially generate smaller cashout effects in the San Diego demonstration relative to those in the FIP and ASSETS demonstrations, which had full-saturation designs, capable of producing undiluted community effects. The more plausible types of community effects of cashout would act to reinforce its negative impact on food expenditures. In the example cited earlier, the fact that cashout increases the cash at the disposal of recipients could induce some landlords to increase rents, thus reducing the resources available for food purchases. These second-order effects are more likely to be manifested when the entire food stamp caseload is cashed out and, hence, the general public's awareness of cashout is greatest. To be sure, neither the estimates based on the small-scale San Diego demonstration nor those based on the full-saturation ASSETS and FIP demonstrations could be said to be biased, in the sense of lacking *internal* validity, by their failure to fully capture community effects. However, the full-saturation demonstrations should in principle generate a pattern of results more similar to that of a generalized implementation of cashout (higher *external* validity).

At the opposite end of the spectrum, the high estimates for the ASSETS demonstration could be in part a consequence of the fact that the comparison (coupon) counties in the ASSETS demonstration were not well matched with the treatment (check) counties. Davis and Werner (1993) note that there were large preexisting differences in housing costs between the treatment and comparison counties; rents were 50 percent higher in the treatment counties. Since there is no evidence of income differences, expenditures other than rent must have been lower in the treatment counties prior to cashout, possibly including expenditures on food. Thus, there is reason to believe that expenditures on food in the ASSETS treatment counties were lower than in the comparison counties before cashout was

implemented. To the extent that the estimate of the effect of cashout in the ASSETS demonstration reflects these preexisting conditions, it is biased in the direction of being too large in absolute value.

B. Differences in the Economic Circumstances of Food Stamp Recipients

Despite being a federal program, the FSP differs across localities because of differences in the economic circumstances of the low-income households it serves, including those arising from differences in pay scales and the availability of jobs and in eligibility requirements and benefit levels for other transfer programs that may interact with the FSP. Variation in economic circumstances affects the size distribution of FSP benefits in the total resources (food stamp benefits plus cash income) available to food stamp households. We investigated two aspects of the economic circumstances of food stamp recipients that vary by locality and may influence the effect on household food expenditures of cashing out food stamps:

1. The fraction of food stamp recipient households whose desired level of food spending is less than the food stamp benefits they receive tends to be high where cash incomes are low (and consequently food stamp benefits are high). These “constrained” households presumably would like to redirect some of their food stamp benefits to the purchase of nonfood items. Cashout gives them the opportunity to do so; thus, one would expect the effect of cashout to be larger in localities where the fraction of constrained households is larger.
2. The share of total food expenditures financed by food stamp benefits may also vary substantially by locality. Where food stamp recipients have relatively high cash incomes, food stamp benefits are lower and can be used to finance a smaller share of food purchases. Cashing out food stamps might therefore be expected to have a smaller impact in these areas.

According to Southworth’s neoclassical model, the fraction of constrained households should play a major role in generating the effect of cashout. In its pure form, this model predicts that the presence of constrained households is the *only* basis for a cashout effect, because households that are already spending some of their cash income on food, in addition to using all of their food stamp benefits to buy food, would not change their behavior when the benefits are cashed out. An implication of this model is that, if the fraction of constrained households varied significantly across sites, so would the effect of cashing out food stamps. However, the data generated by the demonstrations do not support this explanation; rather, they reject the central prediction of the model. Data from the control groups show that 25 percent of households in the Alabama demonstration report food expenditures below the value of their food stamp benefits, whereas only 10 percent do so in the San Diego sample. The qualitative difference in the estimated effects of cashout in these two sites is the opposite of that which the Southworth model would predict based on these percentages of constrained households. Therefore, one can only conclude that variation in the

Table 3
Mean Weekly Food Stamp Benefit and Food Expenditure per Adult Male Equivalent, for Households Receiving Coupons

Cashout Demonstration	Mean Food Stamp Benefit (A)	Mean Expenditure on Food (B)	Ratio of Mean Benefit to Mean Expenditure on Food (A ÷ B)
San Diego pure cashout	\$12.00	\$31.82	0.38
Alabama pure cashout	18.35	29.50	0.62
Washington FIP	20.95	29.31	0.71
Alabama ASSETS	20.08	26.95	0.75

Source: Column A is from tabulations of data from the San Diego and Alabama Food Stamp Cashout Demonstrations; Cohen and Young (1993), p. 83; and Davis and Werner (1993), p. 29. Column B is from Table 1 of this article.

fraction of constrained households cannot account for the variation in the estimated effect of cashout across the demonstration sites.

This suggests that cashout affects the food spending behavior of unconstrained as well as constrained households. Fraker (1990) discusses several possible explanations for why unconstrained households might be affected by cashout, despite such behavior being inconsistent with neoclassical model. But whatever the explanation, the size of the effect is likely to be related to the size of a household's food stamp benefit relative to its expenditures on food. If food stamp benefits finance only a small share of a household's food expenditures, the effect of switching to checks on total food expenditures is likely to be small, and conversely.

The implication of this relationship is that, in areas of the country where the ratio of food stamp benefits to food expenditures is small, cashout is unlikely to cause large reductions in food expenditures, since households are already using alternative means of payment to purchase most of their food. As shown in Table 3, the ratio of the mean food stamp benefit to the mean expenditure on food by coupon recipients is only about half as large in the San Diego sample (.38) as in the FIP and ASSETS samples (.71 and .75, respectively). Much of this difference is due to the fact that California provides relatively large AFDC benefits, which, because they are included in the computation of food stamp net income, result in relatively low food stamp benefits for California households that participate in both food stamps and AFDC.⁷ Approximately 83 percent of food stamp recipients in San Diego also receive AFDC benefits.

7. Among households that participated in the demonstrations, the average monthly AFDC benefit was \$659 in San Diego (Ohls et al. 1992, age 42), \$125 in Alabama pure cashout (Fraker et al. 1992, page 59), \$381 in Washington FIP (Cohen and Young 1993, page 29), and \$109 in Alabama ASSETS (Davis and Werner 1993, page 29).

Because food stamps are not the major source of food purchasing power for recipients of food stamps in San Diego, we would not expect cashout to result in large reductions in their expenditures on food. In Alabama and Washington, where food stamp recipients rely on food stamps for approximately three-fourths of their food purchasing power, cashout has the potential to result in substantially larger reductions in food expenditures.

This discussion suggests an alternative way of presenting estimates of the cashout effect, one in which the difference in average food expenditure between check and coupon recipients is normalized by the average food stamp benefit for all recipient households, rather than by the average food expenditure for the control group, as was done in all of the evaluations of the cashout demonstrations. The results of this simple exercise are presented in Table 4. Obviously, this normalization does not change the interpretation of the Alabama result, which is still essentially zero. However, it does alter conclusions drawn from comparisons among the results from the other three demonstrations. As a fraction of average benefits, the reduction in food expenditure associated with cashout is 18 cents per dollar of benefits in San Diego, 21 cents in Washington state, and 28 cents in the ASSETS demonstration.⁸ The ratio of the largest of these reductions to the smallest is only 1.6, whereas the corresponding ratio of the percentage reductions in food expenditures is 3.1. Most of the difference in these ratios comes from the fact that the new figure for San Diego is much larger than the original figure. Food stamp benefits tend to be small in San Diego, so cashout resulted in a modest reduction in food spending of \$2.19 per adult male equivalent per week, on average. This reduction is only 7 percent relative to the level of spending by coupon recipients; however, it represents a decline in food expenditure of 18 cents for every dollar of benefits cashed out.

Another advantage of this normalization is that the difference-in-means estimates produced by the evaluations become easier to compare with the econometric estimates of the marginal effectiveness of food stamps obtained with nonexperimental data (reviewed by Fraker, 1990). The great majority of these estimates were based on some variation of the following linear model:

$$(1) \text{ Food expenditure} = a_0 + a_1(\text{FSP benefit amount}) \\ + a_2(\text{cash income}) + e,$$

where a_1 is the marginal propensity to spend on food out of FSP benefits, a_2 is the marginal propensity to spend on food out of cash income, and e represents all other determinants of food expenditure, whether observable or not. When data are available on randomly selected recipients of either food stamp coupons or cash food assistance (food stamp checks), such as the data generated by the four cashout demonstrations, this model can be written as:

$$(2a) \text{ Food expenditure among coupon recipients} = b_0 \\ + b_1(\text{coupon benefit amount}) \\ + b_3(\text{cash income}) + e$$

8. For reasons given in the preceding subsection, the estimate from the ASSETS demonstration that constitutes the top of this range is almost certainly too high and the estimate from the San Diego demonstration that constitutes the bottom of the range is probably a little low.

Table 4
Change in Food Expenditures Resulting from Cashout Normalized by the Average Food Stamp Benefit
(weekly amounts per adult male equivalent)

Cashout Demonstration	Absolute Difference in Mean Food Expenditure between Check and Coupon Households (A)	Mean Food Stamp Benefit for Check and Coupon Households (B)	Reduction in Food Expenditure per Dollar of Benefits Cashed Out (A ÷ B)
San Diego pure cashout	-\$2.19*	\$12.16	-\$0.18
Alabama pure cashout	-0.07	18.19	-0.00
Washington FIP	-4.60*	21.81	-0.21
Alabama ASSETS	-5.92*	20.85	-0.28

Source: Column A is from Table 2 of this article. Column B is from Ohls et al. (1992), p. E.6; Fraker et al. (1992), vol. II, p. E.6; Cohen and Young (1993), p. 83; and oral communication from Elizabeth Davis.

* Difference is statistically significant at the 95 percent confidence level.

$$(2b) \text{ Food expenditure among check recipients} = b_0 \\ + b_2(\text{check benefit amount}) \\ + b_3(\text{cash income}) + e,$$

where b_1 is the marginal propensity to spend food stamp coupons on food, while b_2 is the marginal propensity to spend cash food assistance on food.

If the data from the cashout demonstrations were generated according to this simple model, the difference between the average expenditure on food by coupon recipients and the average expenditure on food by check recipients—that is, the measure of cashout effect used by the four evaluations—could be written as:

$$(3) \text{ Difference in mean food expenditure} = \\ (b_1 - b_2)(\text{average food stamp benefit amount}),$$

since differences in cash income and other characteristics between coupon and check recipients are supposed to be zero, on average, due to the experimental design of the demonstrations. By dividing both sides of (3) by the mean benefit amount, we see that the difference in mean food expenditure, normalized by the mean benefit amount, can be interpreted as the difference between the marginal propensities to spend on food obtained from a linear model. In his review of 19 analyses based on nonexperimental data, Fraker (1990) reports that the difference between estimates of the marginal effects of food stamp coupons and ordinary cash income on household food expenditures ranges from .07 to .39, excluding several outliers. Setting aside the findings from the evaluation of pure cashout in Alabama, the normalized experimental results presented here are much more tightly distributed, ranging from .18 to .28, and fall squarely in the middle of the nonexperimental results. It should be noted that the experimental results are based on data on actual recipients of cash food assistance, while the nonexperimental results are based on data on coupon recipients only, and implicitly assume that ordinary cash income is equivalent to food assistance in the form of cash. Therefore, the results based on the demonstrations should provide more reliable estimates of the likely effect of cashout.

V. Summary and Conclusions

The original evaluations of the food stamp cashout demonstrations produced estimates that answer the question, “What is the effect of food stamp cashout on food expenditures by recipient households?” The evaluations of the San Diego, Washington FIP, and Alabama ASSETS demonstrations found that cashout resulted in reductions in food expenditures ranging from 7 percent to 22 percent of average food expenditures in each site. In contrast, the evaluation of the Alabama demonstration of pure cashout found no effect on food expenditures.

The lack of an effect in the Alabama pure cashout demonstration as compared with the others appears to be related to features of the design of the demonstrations. Cashout in the latter demonstrations was heralded as an important long-term improvement in the operation of the FSP; in the Alabama demonstration, it was introduced with little publicity as a short-term demonstration. Also, food stamp and AFDC benefits were combined in a single check in the three sites, but

were issued in separate checks in the Alabama demonstration. These differences are ones that would tend to depress the effect of cashout in the Alabama pure cashout demonstrations relative to effects in the other demonstrations.

We believe that the designs of the San Diego, Washington FIP, and Alabama ASSETS demonstrations are more likely to resemble a fully implemented policy of food stamp cashout than is the design of the Alabama pure cashout demonstration. However, even when the zero estimated effect from the evaluation of Alabama pure cashout is set aside, the estimates produced by the other evaluations vary so widely that it is not immediately possible to draw conclusions from them regarding the likely effect of cashout if it were adopted in other sites or nationwide.

We argue that the major avenue to reconcile these apparently diverging effects is to recognize that the impact of cashout, as measured in the cashout evaluations, should be roughly proportional to the size of the food stamp benefit, and that the average food stamp benefit varies considerably from state to state. To implement this idea, we simply normalize the estimates presented in the evaluation reports by dividing them by the average level of benefits in each site. This yields an approximate measure of the average reduction in food expenditures per dollar of benefits that are cashed out. While this measure still takes on a value of zero for the Alabama pure cashout demonstration, the values for the other three demonstrations range from 18 to 28 cents.

This normalized measure is more useful than that presented in the original evaluation reports for the development of benefit-issuance policies for the FSP. A reduction in food spending of 18 to 28 cents per dollar of food stamp benefits cashed out is too large to be regarded as insignificant from a policy perspective. According to projections included in the president's fiscal year 1996 budget request to Congress (Food and Consumer Service, USDA 1995), in fiscal year 1995 the FSP will issue benefits worth a total of \$23.3 billion, while incurring \$3.2 billion in administrative costs,⁹ of which benefit issuance will account for about 10 percent, or \$0.3 billion.¹⁰ The normalized cashout estimates that we have presented imply that between \$4.2 billion and \$6.5 billion of these benefits that will be used by recipient households to purchase food under coupon issuance would be used for other purposes under cashout. Thus, the aggregate amount of benefits that would be diverted from their intended purpose as a consequence of cashout would greatly exceed any possible savings in benefit-issuance costs or other costs of administering the FSP.

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9. The costs of administering the FSP are shared equally by the state governments and the federal government.

10. Reported issuance costs are actually several percentage points lower than 10 percent of total administrative costs (Ohls and Beebout 1993, page 85). The 10 percent estimate allows for some reporting of issuance costs in other administrative categories.

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