Should Food Stamps Be Free?
Impacts and Implications of the Purchase Requirement Removal *

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Abstract

Free provision of in-kind transfers weakens screening on private value (willingness-to-pay) but may improve targeting on social value. We study this trade-off empirically using a 1979 reform to the U.S. Food Stamp Program that increased the subsidy on program food purchases from roughly 65% to its current 100%. Leveraging geographic variation in exposure to the reform, we estimate that enrollment increased by about 2 percentage points (roughly 30%), with marginal enrollees substantially poorer than existing participants. Using the reform as an instrument for participation, we find no evidence the food stamp enrollment improved infant health; our 95% confidence intervals rule out birth-weight gains above 2%. Welfare losses from reduced screening on private value appear to exceed welfare gains from improved targeting on social value, suggesting that it can be suboptimal for in-kind transfer programs to forgo screening on willingness-to-pay.

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Many in-kind transfers are provided free of charge. This includes the U.S. food stamp program, which is currently the second-largest means-tested transfer program in the United States, and the only one where nearly all low-income households are eligible. Known today as the Supplemental Nutrition Assistance Program (SNAP), it provides free food up the maximum allowed benefit to more than 40 million recipients (Carrington et al., 2013; Hoynes and Schanzenbach, 2015). But the program didn't always work this way. When the Food Stamp Program was originally introduced in the 1960s, it offered only about a 40 percent food subsidy rather than the current 100% subsidy; that is, households would pay $60 to receive $100 of food stamps.

Should food stamps be free? Cost-sharing would improve targeting on *private* value by screening out households unwilling to pay for food stamps, but perhaps reduce targeting on *social* value by screening out households with high social valuations of food stamps, such as cash-constrained households whom policymakers may want to reach for redistributive purposes. Whether free provision or positive cost-sharing is socially desirable is thus an empirical question. Evidence on cost-sharing for health products in developing countries illustrates that this tension between private and social value is relevant in practice: willingness to pay predicts private use (Ashraf et al., 2010), but cost sharing can screen out households whose take-up is socially valuable due to externalities (Cohen and Dupas, 2010).

Moreover, the key conceptual trade-offs involved in the move to free food stamps — and the empirical objects needed to resolve them — apply more broadly to current policy discussions about cost-sharing and in-kind transfers. Many major in-kind transfer programs in the U.S. today fully subsidize consumption of in-kind goods; for example, the Low Income Heating Assistance Program (LIHEAP) and the Women Infants and Children (WIC) nutrition program provide free energy and food. On the other hand, the National School Lunch Program imposes copays on households between 130% and 185% of the FPL (U.S. Department of Agriculture, Food and Nutrition Service, 2022). U.S. states have started charging premiums for Medicaid, and some OECD health programs rely on positive cost-sharing even for low-income households .[1]

We analyze the major 1979 reform which eliminated the Food Stamp Program's "purchase requirement," converting the program from an average 65% subsidy on food in 1978 to the current, 100% subsidy on food. The trade-off between targeting on private versus social value was central to the contemporaneous debate over eliminating the purchase requirement. The original partial subsidy was intended to ensure financial responsibility and screen on willingness to pay. Advocates pushed to eliminate the purchase requirement after reporting showed that the neediest households lacked the liquidity to buy enough food stamps to avoid hunger. Yet there is remarkably little empirical evidence on the impacts of the decision to eliminate cost-sharing in

[1]Beginning in 2027, California will shift low-income undocumented adults on Medicaid from a zero-premium design to $100 monthly premiums. When defending this reform, Governor Gavin Newsom appealed to the positive screening benefits of cost-sharing, arguing that beneficiaries should have "some skin in the game" through contributions (Associated Press, 2025).

this canonical in-kind transfer program.

We begin by laying out a simple conceptual framework in which the empirical social welfare value of an increase in the subsidy from 65 percent to 100 percent can be expressed as a function of empirical objects: the population shares of always takers and compliers, and for each group their revealed private valuations, social welfare weights, magnitude of any externalities or internalities which can cause households' revealed private valuations to differ from the social value, and the magnitude of any fiscal externalities for enrollment in food stamps.

To estimate each of these objects, we use both the 1979 move to free food stamps as well as the previously studied county-by-county roll out of the positive cost-sharing food stamp program in the late 1960s and early 1970s (Almond et al., 2011). For the empirical analysis of the purchase requirement, we exploit geographic variation in exposure to the 1979 subsidy increase that arises from variation in the pre-reform share of the population that was eligible for but not enrolled in food stamps. We find that the move to free food stamps increased enrollment from about 7 percentage points nationwide to about 9 percentage points (a 30% increase). Moreover, new enrollees (the compliers) were worse off economically than existing participants (always-takers); they had substantially lower incomes and assets than those who always enrolled. For instance, before 1979, about half of program enrollees were below the federal poverty level, compared with more than 80% of compliers who take up in response to the 1979 policy reform.

We also use both reforms as instruments for food stamp enrollment to estimate potential externalities on infant health for always-takers and compliers. Infant health is a natural externality to examine, as influential work has found that the initial roll-out of the partially subsidized food stamp program had positive impacts on infant health (Almond et al., 2011), and health externalities are a central focus of earlier experiments on cost-sharing in developing countries (Ashraf et al., 2010; Cohen and Dupas, 2010).[2] Using the free food stamp reform, we estimate insignificant impacts of food stamp enrollment among compliers on infant birth weight both overall as well as among Black infants, whom Almond et al. (2011) found experienced larger infant health gains from the roll out. Our point estimates are that food stamp enrollment reduces birth weight by a statistically insignificant 0.6% (standard error = 1.2%) and the share of low birth weight births by a statistically insignificant 0.4 percentage points (standard error = 1.4) off of a baseline rate of 7.3 percent. For birth weight, our standard errors allow us to rule out that food stamps increase birth weight by more than 2 percent. For Black infants, we estimate a statistically insignificant 0.9% increase in birth weight (standard error = 1.2%) and a statistically insignificant decline in the share that are low birth weight of 0.3 percentage points (standard error = 1.8), off of a baseline rate of 13 percent. Re-examining the county-by-county roll out of the program previously studied by Almond et al. (2011), Hoynes and Schanzenbach (2009), and Hoynes and

[2]Infant health constitutes an externality if all associated social benefits, including the direct impacts on children, are not internalized in the adults' take-up decision.

Schanzenbach (2012) allows us to also estimate potential externalities on infant health for those who previously enrolled under the partial subsidy program (always takers). Our estimates of the infant health impacts of the roll out are broadly consistent with the results reported in Almond et al. (2011). Because the impacts on birth weight are much more precisely estimated when using the free food stamp reform, when we pool results from both reforms we find that food stamps has no statistically significant impact on infant health, either overall or when limited to Black infants; our precision-weighted estimates of the combined effect of the initial roll out and the increased enrollment under the 1979 reform can rule out an increase in birthweight of more than 1.5% overall, and an increase in black birthweight of more than 3.5%.

In the final part of the paper, we incorporate the evidence into our conceptual framework to assess the welfare implication of making food stamps free. We use the empirical data on the shares of compliers (first stage from the 1979 reform) and externalities (infant health impacts across reforms), and calibrate the fiscal externalities based on income effects. To estimate private valuations, we adopt a revealed preference approach for private valuations based on enrollment shares under different subsidy levels. We estimate an average private valuation per dollar of food stamps among enrollees of 58–65 cents when food stamps is free and 68–76 cents with a 65 percent subsidy. This illustrates how cost sharing can improve targeting on private value. As noted, however, cost sharing also worsened targeting on social value, with those enrolled under the 65 percent subsidy better off economically than the newly enrolled under the free program. On net, we find that the magnitude of the positive targeting on social value is not enough to outweigh the negative targeting on private value: cost sharing produced more welfare per dollar spent than free stamps. We also investigate how alternative assumptions about tastes for redistribution, fiscal externalities, or behavioral biases would change the underlying trade-off. Because the gap in private value is large relative to the gap in observed poverty, large unobserved differential behavioral biases or strong tastes for redistribution would be required to overturn our results.

Contribution. From a positive perspective, our paper contributes to the existing literature on the causal impact of food stamp participation for infants and children. Despite SNAP's current size and policy relevance, existing work evaluating its impact and welfare value comes primarily from studies of the program's initial county-by-county roll out in the late 1960s and early 1970s, when food stamps had about a 65 percent subsidy (e.g., Almond et al., 2011; Hoynes et al., 2016; Bitler and Figinski, 2024; Barr and Smith, 2023; Bailey et al., 2024). This literature — which has been very influential in modern policy discussions (e.g., National Academies of Sciences et al., 2019) — found that food stamp enrollment during pregnancy increased birth weight (Almond et al., 2011), and that enrollment in childhood improved subsequent adult health, life expectancy, human capital, economic self-sufficiency, and the quality of the neighborhood one lives (Hoynes et al., 2016; Bitler and Figinski, 2024; Bailey et al., 2024) and reduced criminal behavior (Bailey et

al., 2024; Barr and Smith, 2023).[3] By contrast, our results paint a less optimistic picture. We find that the large increases in enrollment in the late 1970s did not produce detectable improvements in infant health, and precision-weighted estimates of the combined effect of the roll out of food stamps with the elimination of the purchase requirement also produce null effects. These findings raise broader questions about the effect of the food stamp program on infant health in its first two decades.

From a normative perspective, our revealed-preference estimate of the private transfer value of food stamps of \$0.58–0.65 per dollar of food stamps is also toward the low end. Existing estimates using labor supply responses to food stamp eligibility (Hendren and Sprung-Keyser, 2020) or survey evidence from Whitmore (2002) on the willingness to pay for food stamps (Bailey et al., 2024) suggest a private transfer value that is \$0.62 cents and \$0.80 cents, respectively. The Congressional Budget Office's Supplemental Poverty Measure imputes food stamps' transfer value at \$1-per-dollar (Congressional Budget Office, 2025), as does influential research on distributional national accounts (Piketty et al., 2018; Blanchet et al., 2022; Auten and Splinter, 2024). Our revealed-preference approach complements influential calibrations of food stamps' valuation to recipients in Hendren and Sprung-Keyser (2020) and Bailey et al. (2024); we compare our findings in more detail in Section 6. The strengths and limitations of the revealed-preference approach are also explored in research on revealed-preference valuations for social insurance (e.g., Finkelstein et al., 2019; Landais and Spinnewijn, 2021; Haller and Staubli, 2023; Naik, 2025).

Finally, and most broadly, the trade-offs that we analyze in the design choice of cost-sharing in in-kind transfer programs such as food stamps relate closely to the lively "self-targeting" literature examining whether non-pecuniary costs to enrollment ("ordeals") screen out recipients with low private value (Nichols and Zeckhauser, 1982) or, instead, exclude those with highest social value (often referred to as "need") (Mullainathan and Shafir, 2013; Herd and Moynihan, 2025).[4] Cost-sharing for in-kind transfers raises an analogous but distinct targeting margin, operating through the relationship between social value and private willingness-to-pay rather than through the relationship between social value and non-pecuniary barriers to take-up. This central trade-off between targeting on private and social value appears most directly in Shepard and Wagner (2025)'s study of health insurance, where enrollees' social and private valuations may diverge due to adverse selection. Our framework also relates to work on targeting based on observables versus unobservables including willingness to pay (Ito et al., 2023), and the targeting value of

[3] There is much more limited evidence on the causal impact of food stamps after the roll-out, likely stemming from the difficulties of finding quasi-experimental variation in this uniform, national program. However, there is some evidence that food stamp enrollment in the mid-1990s and later improves childhood health; this evidence is based on quasi-experimental variation in SNAP purchasing power stemming from changes in local food prices (Bronchetti et al., 2019) and variation in SNAP eligibility stemming from changing state policies regarding eligibility for documented immigrants (East, 2020).

[4] A partial list of recent empirical applications includes Alatas et al. (2016), Deshpande and Li (2019), Finkelstein and Notowidigdo (2019), Wu and Meyer (2023) and Rafkin et al. (2025), among many others.

transferring benefits in-kind rather than in cash (Lieber and Lockwood, 2019).

The rest of the paper proceeds as follows. Section 1 provides an overview of the food stamp program and the 1979 reform. Section 2 describes our conceptual welfare framework and how we will measure each element empirically. Section 3 describes our data and empirical strategies. Section 4 presents our reduced form empirical analysis of the impact of 1979 reform that made food stamps free on enrollment rates, characteristics of compliers, and birth weight. Section 5 presents IV analyses of the impact of food stamps on birth weight using both the initial roll-out of food stamps and the 1979 reform. Section 6 presents implications of our estimates for empirical welfare analysis. The last section concludes.

1 Background: The Food Stamp Program and the 1979 Reform

The Food Stamp Act of 1964 initiated a staggered roll-out of the Food Stamp Program across counties nationwide, which was completed a decade later in 1974.[5] However, unlike the current food stamp program (now called the Supplemental Nutrition Assistance Program, or SNAP) the original program required participating households to buy their stamps. This was known as the "purchase requirement." The amount of the purchase requirement increased with household income and initially averaged about 60 cents (i.e. a 40% subsidy).

Starting in the late 1960s, advocates from the "hunger lobby," argued that the purchase requirement presented a barrier to participation for the poorest eligible individuals and called for its elimination (Citizens' Board of Inquiry, 1968). In 1970, the average subsidy was raised to 65%, and in 1971 the purchase requirement was eliminated entirely (i.e. the subsidy rate was set to 100%) for those with sufficiently low income (Richardson, 1979). However, with the worsening economy, calls for further food stamp reforms started in 1975. The ultimate result was the Food And Agriculture Act of 1977 (implemented in January 1979) which eliminated the purchase requirement and increased the subsidy to its current 100% level (USDA Food and Nutrition Service, 2025).

The elimination of the purchase requirement was considered sufficiently transformative that Congress felt the need to repeal the Food Stamp Act of 1964 and to implement the new Food Stamp Program as part of the larger 1977 Food and Agriculture Act (Congressional Research Service, 1977). Although the food stamp program has continued to evolve since 1979—perhaps most notably with the creation and roll out of electronic benefit transfers (EBT) starting in 1988—the basic structure of the program created by the 1979 elimination of the purchase requirement and with it the creation of a 100 percent subsidy on food up to a maximum transfer amount has remained in place to the current day. Yet the limited empirical analyses of the 1979 reform that we

[5]The information in this section draws heavily on Richardson (1979), Congressional Budget Office (1977), and USDA Food and Nutrition Service (2025). Appendix A provides additional details on the evolution of the program.

have been able to find focus only on its impact on program participation, using either time series (e.g., Morentz, 1980) or cross-sectional (e.g. Brown, 1988) analysis.

To understand the implications of the elimination of the purchase requirement, Figure 1 illustrates the consumer choice sets and the possible behavior of eligible households under both the current free food stamp regime (panel a) and the previous, purchase requirement regime (panel b).[6] In both cases, eligible individuals with income I face a choice of whether or not to participate in the food stamp program (which affects their budget set), and how to allocate their budget between food expenditures (x-axis) and expenditures on all other goods (y-axis). Participation involves some costs in the form of applying for and documenting eligibility (Schanzenbach, 2009; Finkelstein and Notowidigdo, 2019) and (in the period we are studying) retrieving food stamps from the local food stamps office (Congressional Budget Office, 1977); we model this as a monetary participation cost (k) that may vary across individuals, whom we allow to have heterogeneous types denotes by θ. These types of participation costs contribute to incomplete take-up of the free food stamps program (Schanzenbach, 2009; Finkelstein and Notowidigdo, 2019)

Under the free food stamps program (panel a), if individuals choose to participate (at private cost k_θ), they receive a 100 percent food subsidy for any food purchased, up to a maximum food stamps allotment (i.e. government transfer) of $\$F > 0$; after that, participants face the full (unsubsidized) price of food for any additional food purchases (which we normalize to 1). The red line shows a budget set when the individual does not participate in the free food stamp program and the blue line shows a budget set when she does. For simplicity, the figure shows only two types of eligible individuals: person x chooses sufficiently little food in the absence of the program that she is better off not participating under the program rather than incurring the participation cost k_θ.[7] Person z who chooses to participate but who would have chosen to purchase more food than F in the absence of the program;[8] such individuals are referred to as "inframarginal" participants since, for them, food stamps are (at least in principle) equivalent to a cash transfer of F. Naturally there may also be "marginal" participants who would have spent less on food in the absence of the program than they received in food stamps (and for whom program participation thus lowers the marginal price of food), but we do not include them in the figure since, in practice, most participants are infra-marginal; this will be important for some of the welfare analyses below.[9]

[6]The illustration follows that of, e.g., Moffitt (1989) and Hoynes and Schanzenbach (2009) but adds a (monetized) participation (or take-up) cost of enrolling in the program (k_θ). For expositional clarity, it abstracts from some program details; Appendix A provides additional details. We discuss potentially relevant abstractions below in the context of our empirical strategy and the interpretation of the empirical results.

[7]Of course, there may also be people who consume a lot of food but still choose not to participate due to sufficiently high transaction costs k_θ; these are not depicted simply to keep the figure less cluttered.

[8]More precisely, an inframarginal participant is one who would consume more than their food stamp allotment if they received the food stamps in cash instead.

[9]Estimates from the contemporary program suggest that 5 to 25 percent of enrollees would have spent less on food in the absence of the program than they received in food stamps (Trippe and Ewell, 2007; Econometrica, 2012; Hoynes

Panel (b) adds the budget set under the pre-1979 purchase requirement (shown in black). Participating households were required to pay cash (purchase) their food stamps. Specifically the household paid the government R and in return receive a dollar value of food stamps $R + F$ from the government.[10] The program parameters F and R together determine the magnitude of the food subsidy $s = \frac{F}{R+F}$ that the individuals received on the first $F + R$ food that they consumed, after which they faced the unsubsidized marginal price for additional food of 1. The limit as the subsidy $s \to 1$ (i.e., as the purchase requirement $R \to 0$) is the contemporary food stamps program, in which stamps are free and (equivalently) the subsidy is 100 percent.

The figure illustrates three distinct types of people affected by the removal of the purchase requirement. Person x is a never taker: she would consume sufficiently little food relative to F in the absence of the program that she chose not to participate under the purchase requirement and continues not to participate under the free stamps program. Person z is an "always taker": she participates under the purchase requirement and continue to participate under the free food stamp program.[11] Person v is a complier: she chose not to participate under the purchase requirement, but would participate under the free food stamp program because of the reduction in the average price of food she faces.[12] We continue to depict only infra-marginal participants since, in practice, under the purchase requirement, most participants were also infra-marginal.[13]

2 Welfare Analysis of Free Food Stamps

2.1 Conceptual Framework

To illustrate the key objects that determine whether average social welfare per dollar of government spending is higher under free food stamps ($s = 1$) than under positive cost sharing ($0 \leq s \leq 1$), we begin by following Hendren and Sprung-Keyser (2020) and defining the marginal value of

et al., 2015; Hastings and Shapiro, 2018). Specifically, survey data of food expenditures for households on SNAP suggest that between 15 and 25 percent of participants are not inframarginal (Trippe and Ewell, 2007; Econometrica, 2012; Hoynes et al., 2015) while transaction-level data from a large US grocery retailer on SNAP-eligible spending by households who ever use SNAP in months that they are not on SNAP suggest that 5 percent are not infrmaraginal (Hastings and Shapiro, 2018).

[10] As we describe in more detail in Appendix A, the household could in fact pay $0 \leq r \leq R$ for various values of r, and in return receive a dollar value of food stamps $f = r(R + F)/R$ from the government. In practice, however, participating households essentially all chose the maximum allotment $R + F$.

[11] Because, as noted above, essentially all participants under the purchase requirement in practice chose the maximum allotment $R + F$, we do not consider inframarginal participants locating on the first piece-wise linear part of the purchase requirement budget set facing a marginal price of food of less than 1.

[12] Because the elimination of the purchase requirement (weakly) expands the budget set, there are no defiers.

[13] The estimates from the time period of the purchase requirement come from administrative records on the share of households purchasing less than the maximum allotment and suggest that only about 6 to 8 percent of enrollees would have spent less on food in the absence of the program than they received in food stamps (Reese et al., 1974; Clarkson, 1976; MacDonald, 1977; Moffitt, 1989).

public funds (MVPF) per dollar of food stamp transfers (F) as:

$$\text{MVPF} = \frac{\text{NWTP}}{1 + \text{FE}} = \frac{V + D}{1 + \text{FE}}. \tag{1}$$

The numerator is the average enrolled household's "normative willingness to pay" for food stamps (NWTP). The denominator is the mechanical cost of providing a dollar of food stamps plus the average fiscal externality ($\text{FE} \lessgtr 0$) from a behavioral response to food stamps on the government budget, such as reductions in earnings which reduce income tax revenue.

NWTP itself can be decomposed into private valuations V and distortions $D \lessgtr 0$. The value V is an individual's perceived private willingness to pay for food stamps, per dollar. The value $D \lessgtr 0$ denotes any distortions that lead private valuations to depart from how much a household "should" be willing to pay based on their normative utility. Distortions D may reflect externalities on enrolled households — such as failures of efficient intra-household bargaining in which parents do not internalize the positive health benefits that food stamps might have on their children — and internalities which represent mistakes or behavioral biases which cause households' revealed valuations to differ from their normative willingness to pay (Allcott and Taubinsky, 2015; Bernheim and Taubinsky, 2018).[14] Note that absent any such distortions, one does not need causal impacts of food stamps (such as effects on health or test scores) to estimate its value to recipients; they are internalized by the household and reflected in V.[15]

To define social welfare $W(s)$ from a food stamp program with subsidy s, we introduce heterogeneous types $\theta \sim G$ and let valuations, distortions, and fiscal externalities vary by θ. Types also have potentially heterogeneous social marginal welfare weights λ_θ. We denote by $\theta \sim G(s)$ the distribution of types conditional on enrolling in the food stamps program at purchase requirement subsidy $0 < s = \frac{F}{R+F} \leq 1$. $G(s)$ may differ from the distribution of types in the overall population $\theta \sim G$ due to both differences in who is eligible for food stamps and who enrolls conditional on eligibility (i.e., take-up). $G(s)$ may differ from $G(s')$ because different types enroll depending on the purchase subsidy. The corresponding density $g(\theta, s)$ thus encodes θ's type-specific take-up rate as well as θ's underlying share of the population. Allowing for this heterogeneity gives a *welfare-weighted* MVPF of food stamps with subsidy s:

$$W(s) = \frac{\int_{\theta \in G(s)} \lambda_\theta (V_\theta + D_\theta) dG(s)}{\int_{\theta \in G(s)} (1 + \text{FE}_\theta) dG(s)}. \tag{2}$$

The welfare ratio $W(s)$ is the social value of providing an additional \$1 of food stamps, at no cost to recipients, to the population enrolled under subsidy s. By holding the recipient population

[14] Examples of internalities include present focus, mistakes in optimizing food stamps use due to mental accounting, or misperceptions about food stamps' value or eligibility. We abstract from potential externalities onto non-enrolled households.

[15] Section 3.1 of Banerjee et al. (2024) discusses this point in depth in the context of transfer programs.

fixed and evaluating the dollar at each enrollment composition separately, $W(s)$ isolates how the types who enroll under each subsidy level — their private valuations, distortions, welfare weights, and fiscal externalities — affect the social return to food stamp spending. The reform abstracts from the mechanical fiscal effects of collecting purchase requirements from inframarginal enrollees and fiscal externalities from new enrollment by raising the benefit.

Our key question of interest is whether average social welfare per dollar of government expenditures is higher under free stamps ($s = 1$) than under the positive-cost sharing regime that existed prior to the elimination of the purchase requirement ($s = 0.65$), or in other words whether:

$$W(1) \lesseqgtr W(0.65)$$

Equation (2) clarifies the key parameters — private valuations, distortions, social marginal welfare weights, and fiscal externalities under each subsidy level — that enter welfare analysis. It will therefore provide a guide for the key objects in our empirical analysis. Of course, one could also ask whether a given food stamp regime increases social surplus by examining whether $W(s) \lesseqgtr 1$. It is possible that free food stamps increase social surplus even if average social welfare per dollar of government spending is reduced under free food stamps — i.e., $W(0.65) > W(1) > 1$. In fact, this is what we will find empirically.

The welfare framework also provides intuition for the key qualitative welfare trade-off involved in raising the subsidy level: reduced targeting on private value but potentially improved targeting on social value. Consider two different subsidy regimes with $s_2 > s_1$ The higher subsidy enrolls individuals with lower private value (i.e., $\int V_\theta dG(s_2) < \int V_\theta dG(s_1)$). Intuitive, individuals who are not willing to enroll at a subsidy level s_1 (i.e., a subsidized price of food of $1 - s_1$) but will enroll at a higher subsidy level s_2 must have a private valuation from enrolling (net of transaction costs) of at most $1 - s_1$ per dollar of food stamps, while those who were willing to enroll at the lower subsidy s_1 must have private value net of transaction costs of at least $1 - s_1$ per dollar of food stamps. However, the higher subsidy may also enroll individuals with higher social value. Social value may differ from private value due to social welfare weights (λ_θ), fiscal externalities (FE_θ), and/or distortions (D_θ).

Moreover there is no a-priori theoretical sign for how social value varies for the individuals who newly enroll at the higher subsidy compared to those who were already willing to enroll at the lower subsidy. Whether or not a higher subsidy increases average welfare per dollar of government expenditure depends on whether it enrolls individuals with sufficiently higher social value to offset their lower private value. To see this more clearly, consider a simplified situation in which the only potential wedge between private and social value comes from the social welfare weights λ_θ; in other words, fiscal externalities are constant across types θ and

weakly positive ($FE_\theta = FE \geq 0$) and distortions are zero ($D_\theta = 0$). [16] In this case, the higher subsidy $s_2 > s_1$ increases social welfare per dollar of government expenditure if and only if $W(s_2) = \int \lambda_\theta(V_\theta) dG(s_2) > \int \lambda_\theta(V_\theta) dG(s_1) = W(s_1)$. The higher subsidy increases average welfare per dollar of government spending if and only if it enrolls individuals of higher social welfare weights $\int \lambda_\theta dG(s_2) > \int \lambda_\theta dG(s_1)$ and this difference in large enough to outweigh the fact that the higher subsidy enrolls individuals of lower private value ($\int V_\theta dG(s_2) < \int V_\theta dG(s_1)$). This is similar to the analysis in Shepard and Wagner (2025)'s study of ordeals in health insurance markets, where private and social value do not coincide because of adverse selection. Appendix B provides a graphical illustration of this tradeoff.

2.2 From the Framework to Empirics

Our primary goal is to estimate $W(1)$ and $W(0.65)$ and to compare them. This involves estimating the five core empirical ingredients that enter Equation (2): $dG(s)$, V_θ, D_θ, λ_θ and FE_θ. We will use the elimination of the purchase requirement in 1979 (i.e., the change in subsidy from 0.65 to 1) as well as the previously-studied county-by-county roll out of the program in the 1960s and early 1970s to estimate each of these elements. For empirical implementation, we will assume that there are two types: always-takers who enroll in food stamps before the elimination of the purchase requirement ($\theta = a$), and compliers who only enroll once the purchase requirement is eliminated ($\theta = c$). Note that $dG(1)$, the distribution of types who enroll when the subsidy is 1, is the sum of the shares of each type, which we denote by σ_a and σ_c. Thus the key empirical objects of interest can be written:

$$W(1) = \frac{\lambda_c(V_c + D_c)\sigma_c + \lambda_a(V_a + D_a)\sigma_a}{1 + FE_c\sigma_c + FE_a\sigma_a}. \tag{3}$$

and

$$W(0.65) = \frac{\lambda_a(V_a + D_a)}{1 + FE_a}. \tag{4}$$

Other empirical objects of interest, however, can be estimated with a subset of the above ingredients. This includes the average perceived value of food stamps among people who take up the program under its current 100% subsidy form:

$$V(1) = \int V_\theta dG(1) = \sigma_c V_c + \sigma_a V_a. \tag{5}$$

The value of $V(1)$ is itself important for policy, as average perceived willingness to pay for food stamps enters any welfare or policy analysis of the program (and, as noted in the introduction,

[16] These restrictions simplify the exposition without affecting the core intuition; they also consistent with our ultimate empirical findings.

is often assumed to be 1 in practice). In addition, if we assume (as the empirical evidence will suggest) that distortions are zero and fiscal externalities are constant across the two types, we can analyze the trade off between targeting on private versus social value simply by comparing the difference in social value of compliers and always-takers: $\lambda_c V_c - \lambda_a V_a$.

Implementation. To estimate the type shares σ_a and σ_c, we will look at enrollment prior to the 1979 elimination of the purchase requirement, and the enrollment response to the elimination of the purchase requirement. To estimate social marginal welfare weights λ_c and λ_a, we will look at observable characteristics of compliers and always takers. We will use the removal of the purchase requirement (respectively, the initial roll-out of food stamps) as an instrument for food stamp participation; this will allow us to produce causal estimates of the impact of food stamps on infant health D_c (respectively, D_a) and on adult earnings FE_c (respectively, FE_a).

Finally, we will use a revealed-preference approach based on the enrollment responses to increases in the food stamp subsidy to estimate households' perceived private valuations V_c and V_a, defined as the cash transfer that would make a household indifferent between enrolling in the program and receiving food stamps versus not enrolling and receiving cash. Key to our implementation is the fact that, as discussed in Section 1, most households enrolled in food stamps are infra-marginal. Therefore, absent any transaction costs or stigma associated with enrolling in or using food stamps, we assume households would privately value each dollar of food stamps as equivalent to \$1 in cash. However, in practice, participation in the program among eligible individuals is incomplete — both under the original partial subsidy (Richardson, 1979; Congressional Budget Office, 1977) and when fully subsidized (Hoynes and Schanzenbach, 2015) — and at least some of this incomplete take-up reflects the transaction costs of enrolling (Congressional Budget Office, 1977; Schanzenbach, 2009; Finkelstein and Notowidigdo, 2019). In the presence of transaction costs, the average private valuation of free food stamps could be as low as \$0, if transaction costs are sufficiently high that individuals are just indifferent between enrolling and not. The empirical enrollment responses to increases in the food stamp subsidy allows us to identify additional points on the demand curve for food stamps and thus tighten these bands.[17]

Of course, our revealed-preference approach estimates household's *perceived* valuations (from the perspective of the "decision" utility function that they maximize), which may differ from their true valuations (from the perspective of their "normative" utility function) due to optimization frictions such as misperceptions; the "true" valuation from is captured by $V_\theta + D_\theta$.[18] However,

[17]This approach is in the spirit of the revealed-preference, or demand-based approach, used to measure the value of other social programs including unemployment insurance (Landais and Spinnewijn, 2021), disability insurance (Haller and Staubli, 2023), health insurance (Finkelstein et al., 2019), social assistance (Naik, 2025), and school vouchers (Kamat and Norris, 2025).

[18]The evidence that recipients' marginal propensity to consume food out of food stamps is higher than out of

while optimization failures could introduce a wedge between perceived and true valuation, they do not threaten the qualitative comparison of $W(1)$ versus $W(0.65)$ as long as these biases do not differ across the always-takers and compliers. By contrast, liquidity constraints do not introduce a wedge between perceived and true valuation; if a household faces liquidity constraints - and hence declines to enroll at a positive purchase price $s < 1$ because it has a high marginal utility of consumption out of current resources - the private valuation parameter V_θ correctly reflects the value of food stamps given the household's budget and borrowing constraints. Distributional concerns arising from differences in resources instead enter through the social marginal welfare weights λ_θ.

3 Data and Empirical Strategy

3.1 Data Sources, Variable and Sample Definitions.

We use data from 1961 through 1984 to study both the initial roll-out of the food stamp program and the 1979 elimination of the purchase requirement.

Food stamp measures. We have two county-year measures of the size of the food stamp program from 1961–84 . Our primary measure is counts of the number of participants in the food stamp program from books published by the Food and Nutrition Service. The data from 1961 through 1979 come from the replication package for Almond et al. (2011); we digitized the data on participant counts for 1980–84 from books that we found in the Cornell libraries (U.S. Deptartment of Agriculture, 1980-1984). For some analysis, however, we also examine the total dollars transferred through the food stamp program; this comes from data books published by the Bureau of Economic Analysis' Regional Economic Information System (REIS) (U.S. Department of Commerce, n.d.).

To transform these objects into per-capita measures, we use data from the 1970 census on county-level counts of population and of population at or below the poverty line; for some analyses we will also use 1970 census data on the share of black population in the county, and the state poverty rate for black and for white populations, as county-level race-specific poverty rates are not available in public-use data.[19]

Our baseline measure of food stamp participation (FS_{ct}) in county c and year t is therefore the *food stamp enrollment rate*, defined as the count of the number of people enrolled in food stamps in

cash despite the fact that food stamps are infra-marginal has been interpreted as evidence that they engage in mental accounting (Hastings and Shapiro, 2018; Song, 2022; Chorniy and Notowidigdo, 2025). Such mental accounting could also drive a wedge between perceived and true valuation if the recipient does not realize that they will engage in mental accounting.

[19]The 1972 "County and City Data Book" publishes summary statistics of data collected in the U.S. 1970 Census of Population and Housing. State-level race-specific poverty rates come from the 1970 1% IPUMS USA sample.

county c and year t divided by the census population count in county c in 1970:

$$\text{EnrollmentRate}_c^t = \frac{\text{Number of Food Stamp Enrollees in Year } t}{\text{Number of People in County in 1970}} \tag{6}$$

Likewise, we will also examine *food stamp spending per capita*, defined as food stamp spending in county c and year t divided by the census population count in county c in 1970.

Although we cannot measure *race-specific* county-year food stamp enrollment rates directly in the data, for some analyses we will impute them using the county-year overall enrollment rate, annual national data from the PSID on race-specific take-up rates, and 1970 census data on county-level race-specific population and state-level race-specific poverty rates; the key assumption behind this imputation that the national ratio of take-up rates for non whites and whites holds across counties. Appendix C.1 describes this approach in more detail.

Finally, it will also be useful for some analyses to define the county-level *food stamp eligibility rate* using the 1970 census:

$$\text{EligibilityRate}_c^{1970} = \frac{\text{number of people in poverty 1970}}{\text{number of people in county in 1970}} \tag{7}$$

Characteristics of enrollees. To examine how the characteristics of food stamp enrollees and how this changes over time, we use annual, national data from the Panel Study of Income Dynamics (PSID) from 1970 through 1981; the PSID is an annual, longitudinal survey of about 4,800 U.S. households with information on demographics (including race, income, age and family structure), from 1970 through 1981.

Infant health. Our outcome variables for infants come from birth-level data that are available nationwide from the National Center for Health Statistics Vital Statistics starting in 1968 (National Center for Health Statistics, 1968-1987).[20] For live births, the data contain birth weight — which will be our primary focus — as well as gestational age and infant death through 12 months. They also contain exact date of birth, state and county of birth, maternal race and marital status. Our primary focus will be on birth-weight, and on the share of births that are low birth weight, defined as birth weight under 2,500 grams.

Sample Definitions. For the analysis of the 1979 elimination of the purchase requirement, we start from the universe of 3,143 "county equivalents" (henceforth, counties) in 1970 and consider

[20] Prior to 1972, states reported outcomes for a random 50% of births; after 1972 some states started reporting 100% of births and by 1985 all states were reporting 100% of births.

county years 1972 through 1984.[21] We make several sample restrictions. Most consequentially, we limit to the approximately three-quarters of population-weighted counties that had introduced their food stamp program in 1970 or earlier; we do this so that our analysis from 1972 on is limited to counties that are plausiby in 'steady state' after the initial roll out. In addition, we exclude about 6% of population-weighted counties because they are missing food stamp participation counts or birth records in any year 1972–84. Our baseline analysis sample thus consists of 1,788 counties representing 71% of the 1970 population (see Appendix Table A1 for details). For infant health estimates by race (i.e., white vs. non-white), we further restrict to a balanced panel of counties that have at least 25 births in each race-county-year cell; this results in a sample of 606 counties, or 55.78% of the U.S. 1970 population. For estimates by race (white and black), we additionally drop counties that lack data on the 1970 race-specific population shares, and we drop county-race-year cells that have fewer than 25 births.

For the analysis of the initial county-by-county roll out, we follow Almond et al. (2011) and limit the analysis to counties that rolled out the Food Stamp Program in July 1969 or later; this ensures that we observe some pre-period infant health outcomes (since that data only begins in 1968). The resulting sample includes 1,504 counties which represents 38.10% of 1960 U.S. population. For estimates by race (white and black), we additionally drop counties that lack data on the 1960 race-specific population shares, and we drop county-race-year cells that have fewer than 25 births. The resulting sample of white births spans 1,490 counties that represent 38.08% of 1960 U.S. population, and non-white birth sample spans 634 counties that represent 29.5% of 1960 U.S. population.

3.2 Analyzing the Elimination of the Purchase Requirement

3.2.1 Empirical strategy.

Our empirical strategy exploits the fact that different counties were differentially exposed to the elimination of the purchase requirement. We define exposure based on the difference between the share of people in the county who are eligible for food stamps and the share of people in the county who are enrolled in food stamps. The intuition behind this exposure-based strategy is that areas that had more exposure to the reform - i.e., a greater share of the population that was eligible for food stamps but had not enrolled - may be more affected by the 1979 reform that eliminated the purchase requirement.

More specifically, we define:

[21] County equivalents are counties and other primary divisions like parishes or independent cities, as in the Census.

$$\text{Exposure}_c^z := \text{EligibilityRate}_c^{1970} - \text{EnrollmentRate}_c^z \tag{8}$$

$$:= \frac{\text{number of people in poverty 1970}}{\text{number of people in county in 1970}} - \frac{\text{number of people on FSP in year } z}{\text{number of people in county in 1970}} \tag{9}$$

All of the components of the exposure measure come from the 1970 census except for the count of individuals enrolled in food stamps which is defined for a fixed year $z < 1979$. All else equal, Exposure_c^z is increasing in the poverty rate (i.e., eligibility rate) and decreasing in the enrollment rate; we will show below that in practice the instrument operates primarily through variation in the poverty rate, rather than variation in the enrollment rate conditional on the poverty rate.

We define our baseline exposure measure (Exposure_c^z in equation (8)) using the 1970 poverty rate for the eligibility rate, and calculating the enrollment rate based on the enrollment count in 1972 (i.e., $z = 1972$) and the 1970 county population.Due to measurement error from combining different data sources in the exposure measure's numerator and denominator, we estimate a small share of counties with negative exposure (3.2% of population-weighted counties); we censor these estimates at zero and for symmetry, we censor the top 3.2% of the exposure distribution to the 96.8th percentile.[22] We select 1972 to define exposure so as to be able to examine trends across counties with differential exposure for a reasonably long period prior to the 1979 reform, however the choice is inconsequential; Appendix Figure A10 shows that exposure as measured in 1972 is highly correlated with exposure as measured in 1975 or 1977.

3.2.2 Event study framework.

We use an event study framework to estimate the impact of the 1979 elimination of the purchase requirement at the county-year (c, t) level on both measures of food stamp participation (FS_{ct}) and birth outcomes (y_{ct}); these will inform our estimates of, respectively, the share of compliers (σ_c) and their average distortions (D_c):

$$\text{FS}_{ct} = \sum_{t \neq 1978} \delta_t \left(D_t \times \text{Exposure}_c^z \right) + \alpha_c^1 + \gamma_t^1 + u_{ct} \tag{10}$$

$$y_{ct} = \sum_{t \neq 1978} \beta_t \left(D_t \times \text{Exposure}_c^z \right) + \alpha_c^2 + \gamma_t^2 + \varepsilon_{ct} \tag{11}$$

where D_t is an indicator for the year t, α_c are county fixed effects, γ_t are calendar-year fixed effects, and Exposure_c^z is a time-invariant, county-level measure of exposure to the 1979 elimination of the purchase requirement. We weight by the 1970 population in all our regressions and report

[22] Appendix Figure A8 shows the raw exposure measure and how we censored it. In everything else that follows we use the censored exposure measure.

standard errors clustered by county.

The key coefficients of interest are the δ_t's and β_t's, which are the coefficients on the interaction of the calendar-year indicators (D_t) with exposure to the 1979 reform. For ease of interpretation, we will often summarize the event study estimates of the average annual impact of the reform in the three years after the reform relative to the three years prior to the reform by:

$$\beta_{\text{avg}} = \frac{1}{3}[(\beta_{79} + \beta_{80} + \beta_{81}) - (\beta_{76} + \beta_{77} + \underbrace{\beta_{78}}_{\equiv 0})] \tag{12}$$

and similarly for δ_{avg}.

The key identifying assumption behind interpreting estimates of the δ_t's and β_t's from Equations (10) and (11) as reflecting the causal impact of the 1979 elimination of the purchase requirement is that there are no shocks to outcomes that coincide exactly with the 1979 removal of the purchase requirement and are correlated with the county's exposure to this reform. We present supportive evidence of the plausibility of this assumption by documenting similar pre-trends in FS_{ct} and y_{ct} prior to the 1979 reform in counties that were differentially affected by that reform. Of course this does not guarantee that these counties would have continued to be on similar trends in the absence of the reform, and the assumption becomes less plausible the further in time we get passed the reform. We concentrate our empirical analysis on the three-year window after the reform (1979–1981) because in 1981 Congress passed three major pieces of legislation affecting food stamps in 1981, which restricted eligibility and reduced benefit inflation-indexing (Congressional Quarterly, ed, 1982).

4 Impacts of Eliminating the Purchase Requirement

4.1 Descriptive Patterns.

Cross-sectional Variation in Exposure. Figure 2 panel (a) shows the distribution of Exposure_c^{1972} across (population-weighted) counties. Exposure ranges from 0 to 0.2, with a mean of 0.06; moving from the 10th to the 90th percentile of the exposure distribution increases exposure by about 0.1 units. The county-level heat map of exposure in panel (b) reveals greater exposure in the South, consistent with the historical evidence on hunger counties (Citizens' Board of Inquiry, 1968), as well as in the Great Plains region. Even so, it also shows meaningful within-state variation. Of course, since exposure is decreasing in the take-up rate, i.e. the share of eligible individuals who are enrolled (see equation 8 and Appendix Figure A16 panel b), this introduces potential mean version concerns which we will investigate below when looking at the impact of the reform on enrollment rates.

Cross-sectional Variation in Birth Outcomes. Our two main outcome variables are log birth weight and share low birth weight (i.e., birth weight under 2,500 grams). We focus on log birth weight to facilitate interpretation, although results are similar in levels. We focus on share low birth weight to capture impacts on the most at-risk part of the distribution. Figure 3, panel (a) shows the distribution of the 1978 share of births that are low birth weight across (population-weighted) counties. The share low birth weight ranges from 0 to 0.2, with a mean share of 0.08; moving from the 10th to 90th percentile of the distribution increases the share low birth weight by 0.04 percentage points. The county-level heat map in panel (b) reveals higher shares of low birth-weight births in the South Eastern and South Western United States, but again exhibits meaningful within-state variation. Panel (c) shows that counties with greater exposure to the 1979 purchase requirement reform tended to have a higher share of births with low birth weight; this positive correlation is natural since counties with higher poverty rates tended to have higher exposure (see equation 8 and Appendix Figure A16 panel a). However it raises potential concerns about mean reversion when looking at the impact of the reform on birth weight which we will investigate below.

Time Patterns of Enrollment and Birthweight by Exposure. Before turning to our main estimating equations, we examine the raw time series of enrollment rates and birth outcomes in counties with different levels of exposure to the 1979 reform. Figure 4 presents the time series from 1972 through 1984 of food stamp enrollment rates and birth outcomes separately by quartile of the exposure instrument, where higher quartiles correspond to greater exposure. The biggest difference in exposure is between the third quartile (average exposure of 0.06) and the fourth quartile (average exposure of 0.14); by contrast average exposure is 0.03 in the second quartile and 0.01 in the first quartile.

We start with the time series in enrollment rates (Panel a). Before 1979, counties trend roughly in parallel. In 1979, counties in the highest quartile of the exposure measure exhibit about 5 percentage point increase in the enrollment-rate relative to pre-reform years. Other counties show smaller gains in 1979, and these gains decrease in the level of exposure. Contrasting with the impacts on take-up, log birth weight (Panel b) and share low birth weight (Panel c) exhibit no discernible trend break in 1979 in either high- or low-exposure counties. These trends, therefore, suggest that the elimination of the purchase requirement raised take-up without affecting birth outcomes.

4.2 Impact on Food Stamp Enrollment Rate

The elimination of the purchase requirement induces a sharp increase in food stamp enrollment. Figure 5 panel (a) plots the estimated impact of the reform (i.e., the δ_t coefficients from equation (10). The figure shows a sharp jump in enrollment after the elimination of the purchase requirement

in counties that were more exposed to the reform relative to counties that were less exposed. Relative to the three years prior to the reform, the average increase in the enrollment rate three years after the reform in a county that was completely exposed relative to one that had no exposure was 0.325 (s.e. 0.027).

Recall from Figure 2 panel (a) that moving from the 10th to the 90th percentile of the exposure variable raises its value by 0.1. The estimates thus imply that relative to a 10th percentile exposure county, a 90th percentile exposure county experienced on on average an increase in the enrollment rate of 3.25 percentage points. Put differently, since average exposure prior to the reform was 0.06, the estimates imply that the elimination of the purchase requirement on average increased food stamp enrollment by about 2 percentage points, or about 25 percent relative to an average enrollment rate in 1976-1978 of about 7 percentage points.

Trends in enrollment rates between higher and lower exposure counties appear fairly flat between 1972 and 1975 (rising only modestly), and then fall faster in higher exposure counties between 1975 and 1978. A mean-reversion story would suggest the opposite: higher-exposure counties have lower enrollment rates to start with, so regression to the mean would push toward estimating lower enrollment rates relative to lower exposure counties.

Instead, a possible source of differential pre-trends is that food stamp enrollment rates are highly counter-cyclical (see Appendix Figure A11) and counties with different exposure may be experiencing different local business cycles. Consistent with this, the impact of exposure on enrollment seems to be driven primarily by variation across counties in their poverty rate (i.e., share of the county that is eligible for food stamps) rather than the take-up rate (i.e., share of the eligible individuals who enroll in food stamps). Specifically, Appendix Figure A15A includes year $\times$ poverty-rate decile fixed effects as controls when estimating the relationship between exposure to the reform on the enrollment rate in Equation (10). There is no longer an impact of exposure on the enrollment rate. By contrast, when we instead add year $\times$ take-up decile fixed effects, where take-up is defined as the count of food stamp enrollees as a share of the poverty rate, the impact of exposure on the enrollment rate remains strong (Panel B).

In Appendix C.2 we investigate and correct the modest pre-trends using the proxy approach in Freyaldenhoven et al. (2019). Specifically, we proxy for local business cycles using employment in industries that have small shares of workers on food stamps. This correction purges the pre-trends and has negligible impacts on the magnitude of the first stage.

Food Stamp Spending. Evidence on food-stamp spending per capita also corroborates the enrollment impacts. Appendix Figure A18 shows the estimates based on Equation 10.[23] They indicate that the moving from 0 to 100 percent exposure increased food stamp spending on average by $40 per capita, suggesting an average effect of the reform (given average exposure of

[23]We convert county-year food stamp spending into 1970 dollars using the CPI-U and normalize it by 1970 county population to create a measure of food stamp spending per capita for each county-year.

0.06) of $2.4 per capita. Since the average enrollment rate in the post period was around 9%, this suggests that eligible households experienced an average increase in food stamps of about $27, or . We eliminate the pre-trend in this outcome using the proxy approach (Appendix C).

Other aspects of the 1979 food stamp reform. Our description of the 1979 reform has abstracted from the fact that (as noted in Section 1), prior to the reform, the amount of the purchase requirement was increasing with household income, and therefore the subsidy was decreasing with household income (see Appendix Figure A2). The 1979 reform, which eliminated the purchase requirement for all eligible households (see Appendix Figure A3), therefore represented a bigger increase in the subsidy for higher income eligible households (see Appendix Figure A4, Panel B). This point would bias against our finding that the removal of the purchase requirement improved social targeting (i.e., increased enrollment disproportionately among lower-income eligible individuals).

The 1979 reform also included several other changes beyond the elimination of the purchase requirement. In principle, these could complicate our interpretation of the impact of the move to free food stamps. In practice, we describe these other aspects of the reform and examine their quantitative importance in Appendix A, concluding that these other changes were small and unlikely to meaningfully affect enrollment; as noted by other scholars (Berry, 1982; Rosenfeld, 2010), the elimination of the purchase requirement was the centerpiece of the reform. Note that the elimination of the purchase requirement did not affect the transaction costs of procuring food stamps; recipients still had to go to the local food stamp office to pick up their food stamps once a month.

4.3 Targeting on Social Value

We next use time-series evidence to produce estimates of the social marginal welfare weights of compliers and always-takers (λ_a and λ_c).

Our identification strategy for estimating first-stage impacts already suggests that compliers may be needier (have higher social marginal welfare weights) than always-takers. That is because counties with a higher share in poverty experienced larger increases in enrollment from the increase in the food stamp subsidy (Figure A15).

The time-series national patterns on the characteristics of food stamp enrollees compared to non-enrollees are also consistent with such evidence. Figure 7 shows the time series pattern of enrollment rates (Panel A) and the income characteristics of enrollees relative to non-enrollees (Panels B and C). Panel A corroborates that PSID enrollment rose in 1979–81 compared to 1976–78 by about 1.5 percentage points, an estimate that is quite similar to the 2 percentage point increase we estimated using the event study design.

Characteristics of enrollees diverge sharply beginning in 1979 (Panels B and C). The share in the lowest poverty rate band (0 to 50 percent) increases after the reform, while the share between 100 and 150 percent of poverty declines sharply.[24] Likewise a standardized index of durable household assets declines for enrollees but does not show a similar decline for non-enrollees.[25] New enrollees are also more disadvantaged on characteristics like food spending and unemployment (Figure A36). We summarize these measures of need as social marginal welfare weights in Panel D, but defer discussion until Section 6.

In Table 4, we use these average contrasts, together with standard complier algebra and the first-stage estimate in Figure 5, to characterize the always takers and compliers who do not enroll with the 65 percent subsidy but do enroll with the 100 percent subsidy. Specifically, we use our first-stage estimate in Figure 5. The results indicate that compliers are much needier than the always-enrollees. Complier income[26] is about 60% that of always takers, and compliers are about 10 times more likely to be below 50 percent of the poverty line (31 percent compared to 9 percent for always takers).

While naturally one must be cautious drawing a causal interpretation from time series patterns, several pieces of evidence suggest they reflect complier characteristics. First, unlike the highly counter-cyclical enrollment pattern in Panel A (see also Figure A11), trends in enrollee and non-enrollee characteristics are typically flat prior to the reform, diverging in 1979 in opposite directions for those enrolled and not enrolled in food stamps. Second, the targeting appears concentrated in measures of poverty; other demographic characteristics such as share of enrollees who are non-white or share of enrollees who are recent mothers are constant (Figure A35).

Although we abstracted from this in the description of the purchase requirement in Figure 1 panel b, recall that before 1979, the subsidy declines with income, so lower-income households get a larger subsidy. After 1979, all eligible households receive a full subsidy (100%). As a result, the 1979 reform generates a larger increase in the subsidy for higher-income households (see Appendix Figure A4 panel b). This makes the direction of the targeting particularly striking.

4.4 Impact on Birth Weight

The elimination of the purchase requirement is not associated with any statistically or economically significant improvement in birthweight. Figure 5 panel (b) shows no evidence of an increase in log birth weight in higher exposure counties relative to lower exposure counties. If anything, log

[24] In practice, eligibility is limited to individuals below the poverty line. We attribute the existence of enrollment among those between 100 and 150 percent of poverty in the data to unavoidable measurement error in our poverty rate construction. These relate to well-known issues with poverty measurement in survey data (Meyer et al., 2015).

[25] The index is a simple average of standardized values of four asset variables: home value, number of rooms in the home, whether one owns a home and whether one owns a car.

[26] The PSID reports total family income, including transfer income other than AFDC income. We convert nominal income to real income expressed in 1979 dollars using the CPI.

birth weight decreases moderately in higher exposure counties. The difference in the estimated average coefficients post reform relative to pre reform is -0.0019 log points (standard error = 0.0040), suggesting that moving from the 10th to the 90th percentile of exposure (a 0.1 change in exposure) is associated with a statistically insignificant decline in birth weight of about 0.02 percent (s.e. 0.04 percent). The upper bound of the 95% CI therefore allows us to rule out an increase in birth weight of more than 0.1 percent associated with moving from a 10th to 90th percentile exposure county.

There is likewise little evidence of an impact on the share low birth weight (Panel c). The difference in the estimated average coefficients post reform relative to pre reform is -0.0012 (s.e. 0.0044). This estimate suggests that moving from the 10th to the 90th percentile of exposure is associated with a statistically insignificant decline in the share low birth weight of 0.12 percentage points (s.e. 0.44 pp). The 95% CI allows us to rule out a decline in the share low birthweight of more than about 0.1 percentage points, or about 14% percent relative to the 1978 mean share low birth weight of 7.2 percentage points. Looking across the birthweight distribution more broadly, Figure 6 shows no evidence of a statistically significant impact of the reform anywhere in the birth weight distribution.[27] For example, the left-most estimate indicates that an increase in exposure of 0.1 is associated with a statistically insignificant decline of 0.89% relative to the pre-reform share of births below that threshold (with an upper end of the 95% confidence interval of 2.20%) in the share of births below 1,500 grams.

Our estimates of the distortion D_θ depend on the full population; however, estimates of the impact of food stamps by race are important in the social-policy literature. We therefore also analyzed impacts of the elimination of the purchase requirement separately for white and Black births. Appendix Figure A23 shows no evidence of statistically significant impacts on birth weight for either racial group.[28] In contrast to Almond et al. (2011) who found statistically significant improvements in Black birth weight associated with the initial roll out of food stamps, our results point to economically small and statistically insignificant impacts for both Blacks and whites. For whites, impacts on log birth weight and share low-birthweight are "wrong-signed"; for Blacks, 95% CIs rule out that moving from the 10th to 90th percentile of exposure raised birth weight by more than 0.27 percent and share low birthweight by more than 0.3 percentage points.

[27] The only marginally significant impact is a reduction in births between 4,000 to 4,499 grams, where the point estimate suggests that an increase in exposure of 0.1 is associated with a reduction in births of 0.86% between these thresholds relative to the pre-reform (1976–1978) share of births (upper bound of 95% confidence interval of 0.04%

[28] For this analysis, we restrict to a balanced panel of counties that have at least 25 births in each race-county-year cell. This results in a sample of 606 counties, or 55.78% of U.S. 1970 population. Appendix figure A14 shows results for each race group if we do not limit the sample to the union of the race-specific samples. That is, separately by race, we restrict the sample to the balanced panel of counties that have at least 25 births in each county-year. This results in a sample of white births that spans 1,733 counties (70.7% of the 1970 population) and a sample of black births that spans 613 counties (55.8% of the 1970 population). Again we find no evidence of statistically significant impacts on birth weight for either racial group.

5 Impact of Food Stamp Enrollment on Birth Weight

The lack of a detectable impact of the removal of the purchase requirement on birth weight suggests little evidence of distortions – at least on the infant health margin – among compliers (D_c). To gain insight on potential distortions among the always takers (D_a) we re-analyze the impact of the initial introduction of the food stamp program on birth weight, using the staggered, county-by-county roll out design pioneered by Almond et al. (2011). Since our primary object of interest is not the reduced form impact of either reform on birth weight but rather the impact of reform-induced food stamp enrollment on birth weight we also analyze both reforms in a common, instrumental variable framework that relates food stamp enrollment to birth outcomes. This facilitates comparisons between D_c and D_a in common units; it also allows us to pool both reforms to analyze the impact of food stamps on birth weight under a treatment-effect homogeneity assumption.

5.1 Estimating equations.

5.1.1 Reduced Form Roll-out analysis.

We analyze the staggered, county-by-county roll-out of the food stamps program across counties (c), and time by creating and stacking datasets corresponding to periods d between 1961 and 1975. Food stamp enrollment data are available at the calendar year, so in first stage analysis of enrollment, periods d correspond to calendar years t. Infant health outcomes are available at the monthly level, so in the the reduced form analysis of infant health, periods d correspond to calendar year-months m. In a given dataset d, we include all periods for counties that rolled out in period d (treatment) and all periods prior to roll-out for counties that did not roll out in period d (control). We then stack (append) the datasets, resulting in a county-period-dataset structure where control-group counties enter multiple times across datasets.

In this structure, we run the following regression to estimate the effects clustering at the county level:

$$FS_{ctd} = \alpha_{cd} + \tau_{td} + \sum_{r \neq -1} \beta_r D_{crd} + \epsilon_{ctd} \tag{13}$$

$$y_{cmd} = \alpha_{cd} + \tau_{md} + \sum_{r \neq -1} \beta_r D_{crd} + \epsilon_{cmd} \tag{14}$$

where r denotes time relative to the roll-out of the food stamp program in county c, FS_{ctd} is a measure of the food stamp program (e.g., enrollment rate); y_{cmd} is an outcome (e.g., birthweight); α_{cd} is a county-by-dataset fixed effect and τ_{td} (τ_{md}) are calendar time-by-dataset fixed effect (year or year-month). The key coefficients of interest are the β_r coefficeints on the D_{crd} indicators for whether the food stamp program was in effect in county c at relative period r for dataset d. D_{crd}

is therefore only 1 in counties that are treated in the period of the corresponding dataset, and is zero for all other counties. For all our analyses we restrict the sample to the three years before and three years after the roll-out (i.e. relative years –3 to +2), cluster our analyses at the county level, and weight the analyses using the weights in Wing et al. (2024). Appendix C.3 provides additional details.

5.1.2 IV Analyses.

We analyze the relationship between birth weight and food stamp enrollment, using the various reforms as instruments for food stamp enrollment. The IV analyses requires the exclusion restriction that the reforms only affects birth outcomes via its impact on food stamp participation.[29]

Roll-out Instruments. Our second-stage estimating equation is:

$$y_{crd} = \lambda \text{FS}_{crd} + \alpha_{cd} + \tau_{md} + e_{cmd} \tag{15}$$

where y_{crd} is a birthweight outcome in county c, relative time r, and dataset d and FS_{crd} denotes the food stamp enrollment rate, normalized by that county's 1960 population. The parameter α_{cd} is a county-by-dataset fixed effect and τ_{td} is calendar year-by-dataset fixed effect. We estimate this equation by instrumenting for FS_{crd} with the following (pooled) first-stage equation:

$$\text{FS}_{crd} = \delta \text{Post}_{crd} + \alpha^1_{cd} + \tau^1_{td} + e^1_{ctd} \tag{16}$$

where the instrument, Post_{crd} is an indicator that is 1 if relative time is ≥ 0; and τ_{td} is calendar year-by-dataset fixed effect. For the roll-out IV, the exclusion restriction requires that the timing of the roll-out itself is not related to other forces that could affect birthweight.

Elimination of purchase requirement instruments. Our second-stage equation relates food stamp participation to outcomes:

$$y_{ct} = \lambda \text{FS}_{ct} + \alpha_c + \gamma_t + e_{ct}, \tag{17}$$

which we estimate by instrumenting for FS_{ct} with the following first stage equation:

$$\text{FS}_{ct} = \delta(\text{Post}^s_t \times \text{Exposure}^z_c) + \alpha^1_c + \gamma^1_t + u_{ct}. \tag{18}$$

[29]The IV analysis also requires monotocity; the elimination of the purchase requirement weakly increased the probability that a household enrolls in food stamps. While the purchase-requirement reform affected eligibility, monotonicity can still hold if eligibility reductions were not larger in more-exposed counties. Since eligibility changes occurred only for relatively less poor households , this assumption is less concerning.

The excluded instrument is $\text{Post}_t^s \times \text{Exposure}_c^z$, where Post_t^s is an indicator that is 1 if calendar year t is 1979 or later. For both of the above equations, we limit the sample to the 3 years before and 3 years after the introduction of the purchase requirement, so Post_t^s is an indicator variable that is 1 for years 1979–81 at 0 for the remaining years 1976–78. We cluster at the county level and weight by 1970 county population.

Both reforms as instruments. Finally, we pool the roll-out and purchase requirement reforms in the IV analysis, imposing a homogeneous treatment effects assumption across reforms. Specifically, we append to the the datasets described above for the roll out analysis a flat (not stacked) sample of county-years for 1976-1981 (i.e. year -3 to 2 relative to the 1979 reform) and treat this as an additional dataset d; estimates are precision-weighted across the two reforms.

5.2 Reduced Form Impact of the Food Stamp Roll-Out on Birth Weight

Figure 8 shows the impact of the roll-out reform on food stamp enrollment (panel a) and birth weight (panels b and c). The first-stage estimates in Panel (a) indicate that the roll-out led to a 5.7 pp increase in food-stamps take up in the whole population, an estimate that is similar to that in Almond et al. (2011). However, we find no meaningful impacts on birth weight. For log birthweight (panel b), our 95% confidence intervals rule out that the roll out of the food stamp program increased birthweight by more than 0.2 log points on average. For share low birthweight (Panel c), 95% CIs rule out more than a reduction of 0.41 percentage points. Results look similar if we use population weights (rather than Wing et al., 2024) weights), a two way fixed effects (TWFE) estimator[30] on a 'flat' rather than stacked data set, or a TWFE estimator on a balanced panel of counties that rolled out food stamps in 1970 or later, ensuring an additional pre-period year is observed for them all.[31]

Examining impacts of the roll-out on birthweight separately by race, we find some evidence of improvements for Blacks, but no evidence of impacts for whites (Appendix Figure A29). In our baseline specification, the average effect of the food stamps roll-out on log birthweight for Blacks (panel a) is 0.46 log points (standard error 0.28), with even larger (and now statistically significant) effects outside the three-year sample window; meanwhile, 95% CIs can rule out 0.2 log point improvements for whites. Moreover, the average effect of the roll out on the share low birth weight is a statistically significant 1 percentage point decline (standard error = 0.004) for Blacks (panel b), with even larger estimates outside the three year window; meanwhile 95% CIs can rule out a decline in the share low birth weight of more than 0.3 percentage points for whites.

[30] TWFE estimating equations specified in Appendix Section C.3.

[31] Appendix Figure A33 summarizes comparison across these four specifications. Appendix Figures A28, A30, and A31 present the underlying event studies for, respectively, the stacked data set using population weights, the TWFE estimator, and the TWFE estimator on a balanced panel.

However, these results for Blacks are sensitive to changes in the specification; Appendix Figure A33 shows that in some specification but not others, there is evidence of an improvement in black birth weight, but there is no evidence in any specification of improvements in white birth weight or overall birthweight.[32]

In Appendix C.4 we discuss in more detail how our findings compare to the original Almond et al. (2011) estimates of the impact of the food stamp roll out on birth weight. Overall, our findings by race qualitatively replicate their results. However, whether or not we estimate health improvements for Blacks varies depending on weights or other aspects of the empirical strategy. Regardless, as Blacks are a minority share of the population, the population estimate of D_a aggregated across races is always close to zero.

5.3 IV analyses of the impact of food stamps on birth weight

Table 1 presents the results, for each reform separately and also stacked. In Column 1, we show the first stage for each reform, rescaling the purchase requirement first-stage to reflect a 1 s.d. change in exposure. The first-stage F is large for both reforms (F-statistic 147 for the purchase requirement.) Columns 3 and 4 show the IV results for the purchase requirement and roll-out respectively. These results are nulls both for log birth weight (panel a) and share low birthweight (panel b), which is unsurprising since the reduced form is a null and the first stage is strong. For the elimination of the purchase requirement, the IV estimate in column 3 is that enrollment in food stamps reduces birth weight by 0.6% (s.e. 1.3); the 95% CI rules out that enrollment raises birth weight by more than 2%. In words, this means that a household newly enrolling in food stamps (equivalently, assigning 100% of a county to enroll in food stamps from 0%) causes no more than a 2% increase in birth weight. For the roll-out instrument (column 4), the 95% confidence intervals rule out a 6% increase in birth weight from enrollment in food stamps.[33] Turning to share low-birthweight in panel (b), the IV estimate from the elimination of the purchase requirement (column 3) is a statistically insignificant decline in the share low birth from enrollment in food stamps of 0.3 percentage points (standard error = 1.4); for the role out, the point estimate is larger (a 2.9 percentage point reduction in the share low birth weight) but less precise (standard error = 3.7).

The final two columns uses both reforms as instruments for food stamp enrollment. The first stage estimate for the purchase requirement (rescaled based on the average exposure in the sample) is about 35 percent that of the roll-out (0.0185 ÷ 0.0525). However, the reduced-form estimates from the purchase requirement is much more precise, leading to an IV estimate with

[32] Appendix figures A32 present the underlying event studies for one of the three additional specifications reported in Appendix Figure A33, specifically the TWFE estimator.

[33] These IV estimates using the roll out are similar if we use different weighting, exclude the year of roll-out from the IV, or averaging over a 5-year window versus a 3-year window (Table A4, A3, and A5).

standard errors less than half the size (Column 3). Because standard errors are smaller for the purchase-requirement, the precision-weighted pooled estimates in Column 2 are more similar to the purchase requirement. We find little evidence that food stamps improves infant health on average. We estimate that enrollment in food stamps reduces birth weight by a statistically insignificant 0.5%, and 95% confidence intervals rule out declines of 3 percent or more. Likewise we estimate that food stamps decreases the share low birthweight by 0.5 pp, with 95% CIs ruling out declines of more than 3 pp (relative to a baseline mean of 7.3 percent).

IV Impacts By Race. Tables 2 and 3 show the analogous race-specific first stage and IV estimates for log birthweight and share low birthweight respectively. Consistent with Almond et al. (2011), we find larger improvements in infant health for black infants than white infants. However the only statistically significant IV estimate we find is that food stamp enrollment is associted with an 18.2 percentage point (standard error = 0.086) decline in the share low birth weight for black infants (a point estimate that is greater than the baseline mean share of 13.3 percentage low birth weight). The analogous IV estimate of the impact of food stamp enrollment on the share of Black infants that are low birth weight when using the purchase requirement instrument is null (point estimate of a 0.26 percentage point decline in share low birth weight, with a standard error of 1.8) as is the precision-weighted pooled estimates.[34]

6 Empirical Welfare Analysis

The preceding sections provide estimates of all of the empirical objects that we need to estimate and compare social welfare under free food stamps and under the pre-reform 65-cent subsidy (i.e., $W(1)$ and $W(0.65)$ in Equations (3) and (4), respectively).

6.1 Estimates of welfare objects.

Causal estimates of food stamps' impacts: distortions and fiscal externalities. Based on the findings, we set distortions equal to zero ($D_a = D_c = 0$). Of course, while we are unable to reject the null of no external health benefits to infants, there may be other benefits to children of food stamp recipients that we do not measure. Note however as long as any distortions are the same across always takers and compliers, even if they are positive this will not affect our comparison of $w(1)$ and $W(0.65)$; in other words, the key assumption is that $D_a = D_c$, not that they are zero.

For fiscal externalities, we focus (as in Hendren and Sprung-Keyser, 2020) on the potential negative fiscal externality of lost tax revenue via a food-stamp-induced labor supply reduction.

[34] Figure A24 shows the event study estimates of each reform on food stamp enrollment rate by race. Recall that we need to impute race-specific enrollment rates based on county-year overall enrollment rates, annual national data on race-specific take-up rates, and 1970 census data on county-level race-specific population and state-level race-specific poverty rates. See Appendix C.1 for more details.

As we discuss in Appendix D.1, reasonable calibrations suggest that any lost tax revenue from reduced earnings is likely small; moreover, long-run fiscal externalities could in fact be positive if (as in Bailey et al. (2024)) food stamp receipt as a child increases earnings as an adult. As a result, for our primary analysis we simply assume $\text{FE}_\theta = 0$. However, we also consider calibrations in which $\text{FE}_\theta > 0$. Importantly, the qualitative comparison between $W(1)$ and $W(0.65)$ is not affected by the level of FE_θ but only to the extent that the magnitude of the fiscal externality differs between compliers and always takers (i.e., $\text{FE}_a \neq \text{FE}_c$, based on Equations 3 and 4).

Distribution of types. We obtain σ_a and σ_c from the first-stage enrollment-impact of the 1979 subsidy increase. The enrollment impact of the removal of the purchase requirement on the average county is 1.85 percentage points (Table 1, column 1). The pre-reform average enrollment rate (calculated as the average food stamp enrollment across (population-weighted) counties in the 1976-1978 pre-reform period) is about 7.6 percent. Therefore the share of compliers is given by $\sigma_c = 1.85 \div (1.85 + 7.6) \approx 0.19$, while the share of always takers is $\sigma_a = 0.81$.[35]

Social marginal welfare weights. We obtain social marginal welfare weights λ_a and λ_c by converting our targeting estimates in Section 4 into a social valuation based on assumptions about household utility. We assume that households' utility is CRRA in household equivalized income with a risk aversion parameter of 3 (so that marginal utility is income^{-3}); we adjust household income for number of adults and children using the equivalence scale in Citro and Michael (1995) and impose a household income floor of the 5th percentile of total income in 1970. . We normalize the social marginal welfare weights by assuming that it is 1 for the average household in society; this is consistent with a utilitarian social planner and the financing of any new spending in the food stamp program via a non-distortionary tax on this household.

Figure 7d presents the social marginal welfare weights of enrolled households under these assumptions. A welfare weight of 1 corresponds to the welfare weight of the average household in society from 1970–1986. Consistent with decreasing average income, welfare weights of enrollees rise from about 4 to more than 5 over this period. In Table 4, we show that compliers' average welfare weight is 6.7 versus always-enrollees' weights of 4.1.[36] One way of thinking about this last estimate is that compliers derive about $1.6\times$ ($\approx 6.7 \div 4.1$) as much marginal utility from taking up food stamps as always-enrollees.

[35]This calculation assumes that, absent the purchase requirement reform, there would have been no change in enrollment. Consistent with this, the enrollment rate in the lowest exposure group in Figure 4 (with average exposure of 0.01) rises slightly after the reform but remains similar to pre-reform levels overall.

[36]These estimates of social marginal welfare weights are high, for both always-takers and compliers. This is a standard consequence of CRRA utility, which decreases very quickly in income, and assuming that the budget constraint is closed via a non-distortionary tax on the average person in society.

Estimates of private marginal benefits. As discussed in Section 2, absent further information the average private value of a dollar of food stamps for those enrolled with a subsidy rate of s (i.e. $V(s)$) could be anywhere between \$0 and \$s; thus under the current free food stamp regime ($s = 1$), the private value $V(1)$ to enrollees could be anywhere between 0 and 1. However, we can use the enrollment response to changes in the subsidy to food purchases with food stamps to trace out additional points on the demand curve and tighten the bounds.

Intuitively, if we observe enrollment both under a lower subsidy s_1 and a higher subsidy $s_2 > s_1$, the enrollment response to the increase in the subsidy identifies distinct bounds on valuations *both* for the individuals who would have enrolled even under the lower subsidy (always takers, whose share σ_a we have estimated from enrollment under s_1) and those who only enroll under the higher subsidy (compliers, whose share σ_c we have estimated from the enrollment response to the reduced subsidy s_2). For the always-takers, V_a is now bounded from below by $1 - s_1$ rather than 0; since they were willing to enroll with subsidy s_1, their participation cost per dollar of food stamps received cannot be higher than s_1. Likewise for the compliers, V_c is now bounded from above by $1 - s_1$ rather than s_2; since they were not willing to enroll with a subsidy of s_1, their participation cost per dollar of food stamps received must be at least s_1. Observing the enrollment response to an increase in the subsidy from s_1 to s_2 thus allows us to tighten the bounds from $V(s_2) \in [0, s_2]$ to $V(s_2) \in [\sigma_a \cdot (1 - s_1) + \sigma_c \cdot (1 - s_2),\ \sigma_a \cdot 1 + \sigma_c \cdot (1 - s_1)]$.[37]

If we could observe demand at all subsidy levels ($s \in [0, s_2]$ we could use this approach to point-identify demand and hence $V(s_2)$. In practice, since our empirical application has a pre-reform subsidy of $s_1 = 0.65$ to a post-reform subsidy of $s_2 = 1$, we know $V_a \in [0.35, 1]$ and $V_c \in [0, 0.35]$. Using these bounds and our estimates of $\sigma_c = 0.19$ and $\sigma_a = 0.81$, the elimination of the purchase requirement allows us to tighten the bounds from $V(1) \in [0, 1]$ to $V(1) \in [0.28, 0.88]$ per dollar. If we further assume that demand is piece-wise linear, so that valuations lie at the midpoint of the bounds: $V_a = 0.675$ and $V_c = 0.175$, so that $V(1) = 0.58$, or an average value of 58 cents per dollar of food stamps. Figure 9a shows this piecewise linear demand curve for food stamps; it is the empirical version of the red Marginal Private Benefit line from Figure A1. The vertical axis shows the price to obtain \$1 net of take-up costs (equivalently, 1 – the subsidy). The horizontal axis shows the quantity enrolled at each price. With data from both the free-stamps ($s_2 = 1$) and purchase-requirement ($s_1 = 0.65$) regimes, we observe three points on the demand curve: no enrollment with no subsidy $s = 0$ (since there is no food stamp program this holds by definition), all post-reform enrollees enrolled at a price of 0 ($s_2 = 1$), and $\sigma_a = 0.81$ enrolled at a

[37] The ideal variation for identifying $V(s)$ would fix the bundle of food stamps at F and only manipulate the subsidy level (s). In practice, however, we use variation in the purchase requirement R; this is similar to variation in prices but not equivalent, because changing R also changes the bundle of food stamps received (R plus the bonus F). In Appendix D, we formally define the private valuation V_θ in terms of equivalent variation and describe straightforward conditions under which our identifying variation in the purchase requirement R is sufficient to identify V_θ. We also provide a microfoundation for these conditions.

price of 0.35 ($s_1 = 0.65$).[38]

Moreover, in Appendix C.5 we exploit a 1970 policy that increased the subsidy from $s_0 = 0.40$ to $s_1 = 0.65$ to add another point to the demand curve. Exploiting a version of an exposure instrument similar to Equation (8) — but based on enrollment prior to the reform — we estimate that the subsidy increase increased the enrollment rate by about 1 pp in aggregate. Figure 9b plots the implied demand curve with the additional data point from the bonus reform. The result is tighter nonparametric bounds of $V(1) \in [0.46, 0.83]$ and - assuming piece-wise linear demand, a revised estimate of $V(1) = 65$ cents per dollar of food stamps, rather than 58 cents.

Of course, piecewise-linearity is a strong assumption. We therefore also consider shape restrictions to relax piecewise-linearity without explicit parametric assumptions. Following Kang and Vasserman (2025), we define the *robustness parameter* r to allow demand curves whose slopes within each observed interval may deviate from the piecewise-linear demand slope by up to $r \times 100$ percent in either direction. Intuitively, the parameter r rules out demand curves that are anywhere "too flat" or "too steep" relative to the observed variation.[39] When $r \to 0$, we recover the piecewise-linear benchmark, under which admissible demand curves are the piecewise-linear functions interpolating the observed points. When $r \to 1$, we recover the nonparametric bounds above, allowing any monotone demand curve consistent with the observed data (the light blue or green regions in Figure A27).

Figure 9C shows bounds on the willingness to pay for food stamps $V(1)$ as a function of the robustness parameter r, under three scenarios: without observing data from the purchase requirement (red series), with data from the purchase requirement (blue series), and with data from the purchase requirement and bonus reform (green series). The shape restriction reduces the bounds in all three scenarios. The value of food stamps is point identified as the area under the piecewise-linear demand curve when $r = 0$. When $r = 0.5$, the value of food stamps is tightly restricted to lie between [0.58, 0.71] if we include the 1969 bonus reform and [0.48, 0.68] if we exclude it.[40]

[38] Relative to other work using revealed-preference to obtain the value of social *insurance* (e.g., Landais and Spinnewijn, 2021), being able to observe this third point is a feature of our setting. In the insurance case, willingness to pay has no upper bound. Then, extrapolation into infra-marginal demand always requires parametric assumptions.

[39] More formally, consider an interval of the demand curve defined by the subsidy and take-up tuples $(1 - s_i, q_i)$ and $(1 - s_j, q_j)$. Let β be the slope of the line connecting these points: $\beta = \frac{q_j - q_i}{s_i - s_j}$. Then r is defined by

$$\left| \frac{dq}{ds} - \beta \right| \leq r \, |\beta|, \tag{19}$$

where $\frac{dq}{ds}$ denotes the local slope of any demand curve $q(s)$ within the interval.

[40] The robustness parameter $r = 0.5$ is reasonable benchmark, as it lies halfway between the polar values of $r = 0$ (piecewise-linear) and $r = 1$ (completely non-parametric). As another benchmark, one could fit a polynomial $q(s)$ to the three points in panel A and compare the deviation in slope between this polynomial and the piecewise-linear fit to each segment. Given the observed points on the demand curve, the minimum value of r which satisfies Equation (19) for this function $q(s)$ is close to $r = 0.5$, suggesting our choice of $r = 0.5$ is a reasonable benchmark for how much a natural alternative demand curve might vary between these points.

6.2 Should Food Stamps Be Free?

As discussed in Section 2 there are two ways to think about this question. First, we can ask whether $W(1)$ larger than 1, that is, whether free stamps raises surplus relative to no food stamps, if the program is financed by a tax on the average household in society. The empirical estimates indicate that the answer is yes. Specifically, $W(1) = \mathbb{E}[\lambda_\theta V_\theta] = 4.1 \times 0.675 \times 0.81 + 6.7 \times 0.175 \times 0.19 \approx 2.4 > 1$. However, this result is somewhat mechanical, as CRRA utility always implies high welfare weights that push toward transferring to low-income households. Thus, this answer to the question does not isolate our core trade-off between private and social value.

The other way to pose the question is to ask whether free food stamps are more (welfare-weighted) cost-effective than the pre-reform 0.65 cent subsidy. As discussed in Section 2, qualitatively, this occurs if and only if compliers have more social value than always-takers; in other words, $W(1) > W(0.65) \iff \frac{\lambda_c(V_c+D_c)}{1+\text{FE}_c} > \frac{\lambda_a(V_a+D_a)}{1+\text{FE}_a}$. An advantage of such qualitative comparisons is that they are less sensitive to how we normalize the welfare weights. Instead, what matters is the magnitude of welfare weights relative to each other ($\frac{\lambda_c}{\lambda_a}$). Our estimates imply that $\lambda_c V_c < \lambda_a V_a$. Specifically, compliers' social value $\lambda_c V_c = 6.7 \times 0.175 \approx 1.2$. Always-takers' social value $\lambda_a V_a = 4.1 \times 0.675 \approx 2.8$. Thus the full subsidy is not (welfare-weighted) cost effective.

In other words, we empirically detect a trade-off between targeting on private value and social value — compliers' social marginal welfare weights exceed always-takers' ($\lambda_c > \lambda_a$), but compliers' private values are less than always-takers' ($V_c < V_a$). Indeed, the net welfare impact of providing food stamps to compliers is about 40% that of always-takers — $\lambda_c V_c \approx 1.2$, while $\lambda_a V_a \approx 2.8$. Compliers' much-lower private valuations swamp their somewhat-higher social marginal welfare weights.

Quantitatively, we can also use the above objects to compare the magnitudes of $W(0.65)$ and $W(1)$. Based on the previous estimates, $W(0.65) = \lambda_a V_a = 2.77$. Meanwhile, as noted above, $W(1) = 2.4$. Our welfare-weighted MVPF $W(1) = 2.77$ is a portable number for the literature, though not necessarily comparable to existing non-welfare-weighted MVPFs.

So, should food stamps be free? It depends on the counterfactual. If the new food stamps are financed by taxing the average person in society, and our assumptions about social marginal welfare weights hold, the answer is yes ($W(1) > 1$, so they increase surplus). But compared with the initial policy that imposed cost sharing, making food stamps free appears less cost effective ($W(1) < W(0.65)$. We indeed detect a trade-off between targeting on private and social value; empirically, the reduced targeting on private value outweighs the improved targeting on social value.

Sensitivity. Table 5 reports welfare estimates under alternative assumptions. Our main goal is to show which forces matter qualitatively and how the welfare comparison changes with the underlying empirical ingredients. To that end, we report $W_c \equiv \frac{\lambda_c(V_c+D_c)}{1+\text{FE}_c}$, the welfare ratio for

compliers, and compare it to $W_a \equiv \frac{\lambda_a(V_a+D_a)}{1+\text{FE}_a} = W(0.65)$, the welfare ratio for always-takers, as well as $W(1) = \sigma_a W_a + \sigma_c W_c$. As discussed, free stamps dominate cost sharing ($W(1) > W(0.65)$) if and only if $W_c > W_a$. However, comparing W_c and W_a is often more informative for interpreting magnitudes and comparative statics: the share of compliers (σ_c) is small relative to the share of always-takers (σ_a), so changes in empirical inputs other than σ_c often have only a modest effect on the magnitude of $W(1)$ mechanically.

In the baseline, $W_a = 2.77$ while $W_c = 1.17$, implying that compliers generate less than half the social value of always-takers. This gap helps explain why the welfare ranking is fairly stable across assumptions, and it clarifies which assumptions are capable of reversing it.

Welfare weights. Reducing compliers' welfare weights to equal always-takers' makes free stamps even less desirable (Row 2). But even doubling the welfare weight gap (so compliers' weights are 9.1, more than double always-takers') only moves W_c to 1.62, shrinking the gap between W_a and W_c by about a third (Row 3).

First stage. Shifting shares of types (σ_θ) identified from the magnitude of the first stage cannot reverse $W(0.65) > W(1)$, since in our baseline specification $\frac{\lambda_c(V_c+D_c)}{1+\text{FE}_c} < \frac{\lambda_a(V_a+D_a)}{1+\text{FE}_a}$. Halving or doubling our first stage estimate moves $W(1)$ between 2.25 and 2.6 (Rows 4–5).

Adjustments to V_a. Our primary estimates excluded the demand variation from the bonus reform, using the three-point rather than four-point curves in Figure 9. This choice was conservative, as the four-point demand curve yields a higher V_a than the three-point curve. Adding this variation raises $W(0.65)$ to 3.11 and widens the gap between $W(1)$ and $W(0.65)$ (Row 6).

Rows 7–10 explore alternative assumptions about V_a and V_c within the intervals defined by observed prices. Setting both valuations at the top of the admissible interval ($V_a = 1$, $V_c = 1 - s = 0.35$) or both at the bottom ($V_a = 0.35$, $V_c = 0$) does not change the conclusion (Rows 7–8). The only case favoring free stamps is setting always-takers at the lowest admissible valuation and compliers at the highest admissible valuation ($V_a = V_c = 0.35$, Row 9). This case implies *all* households to have knife-edge valuations of exactly 0.35, which is inconsistent with take-up prior to the bonus reform moved the subsidy from 40 to 65 cents. Including the bonus reform and assigning the most favorable valuations to compliers ($V_c = 0.35$) while giving pre-bonus enrollees the least favorable valuations ($V_a = 0.6$ for pre-bonus, $V_a = 0.35$ for post-bonus) makes W_c and W_a have approximately equal welfare valuations (Row 10). Therefore, as some households' valuations lie strictly within the admissible interval, cost-sharing still dominates.

Fiscal externalities. Fiscal externalities from income effects must be small because taxes on low-income households are low. Therefore incorporating FEs under various assumptions, including assigning only always-takers the maximum FE possible under Equation (36), makes little difference (Rows 11–13). Breaking our result would thus require FEs beyond labor supply, which primarily accrue to always-takers.

Distortions. An important limitation is that we could miss either (i) unobserved household

impacts beyond infant health, or (ii) behavioral biases, could drive a wedge between decision willingness to pay V_θ and normative willingness to pay. Then, distortions $D_\theta \neq 0$ as we have assumed. If anything, the infant-health evidence suggests larger distortions for always-takers than for compliers, which would actually strengthen our conclusions. Still, we consider alternative assumptions about distortions D_θ and conclude that they probably would not reverse our results.

First, in Row 14 we consider a plausible upper bound on distortions: $D_\theta = V_\theta$. In Row 15, we assign the upper-bound only to compliers. Even then, $W_a > W_c$, although the gap shrinks considerably ($W_c = 2.34$ while $W_a = 2.77$).

Second, we perform a back-of-the-envelope calibration of a specific behavioral bias. Consider a two-period setting where future FSP benefits are normatively worth \$1 of net present value minus ordeals costs. These ordeal costs accrue contemporaneously. Households are present-biased with present-bias parameter β_θ, so that $D_\theta = (1 - \beta_\theta) \times 1$; intuitively, present-biased households excessively discount the benefits of future stamps, resulting in private valuations that are too small relative to normative utility. A reasonable estimate from the laboratory literature is $\beta = 0.7$ (e.g., Frederick et al., 2002). If $\beta_a = \beta_c = 0.7$, the welfare values are $W_a = 4.01$ and $W_c = 3.18$, so the gap closes by half but remains positive (Row 16). Even with substantial heterogeneity pushing toward free-stamps — e.g., if compliers are twice as present focused as always-takers ($\beta_a = 0.85$ and $\beta_c = 0.7$) — the gap between W_a and W_c remains positive (Row 17). Overturning our findings requires always-takers to be neoclassical ($\beta_a = 1$) while compliers are biased ($\beta_c = 0.7$), shown in Row 18. In this case, $W_a = 2.77$, $W_c = 3.18$, and free stamps produce more value than cost sharing. It seems unlikely that biases would be so strongly differential across types, but of course we cannot rule it out.[41]

6.3 Relation to the Literature and Discussion

Relation to the Literature. Our framework clarifies how the existing empirical literature on food stamps, particularly the program roll out, provides estimates that enter welfare analysis. Estimates of V have been based on the share of households estimated to have inframarginal food consumption (Whitmore, 2002); or the willingness of adults to distort their labor supply in order to qualify (Hendren and Sprung-Keyser, 2020). Estimates of the impact of food stamps on food consumption (Hoynes and Schanzenbach, 2009; Hastings and Shapiro, 2018; Song, 2022) can also reveal private valuations under a specific model of household behavior. Estimates of the health benefits of enrollment to children and the long-run impacts on these children as adults (e.g Almond et al., 2011; Hoynes et al., 2016; Bailey et al., 2024) have implicitly been interpreted as estimates of D; evidence of internalities such as mental accounting and present bias by food stamp

[41]The take-up response to the bonus reform also poses a challenge to a present bias model, present bias should upweight the immediate cost of 35 cents.

participants (Hastings and Shapiro, 2018; Chorniy and Notowidigdo, 2025) or agency problems within the family more broadly (Bergstrom, 1989) may provide justification for this interpretation. There are estimates of FE based on the reduction in adults earnings to qualify for food stamps (Hoynes and Schanzenbach, 2012). Finally, there are estimates of $\text{cov}(g(\theta, 1), \lambda_\theta V_\theta)$ based on the consumption distribution of enrollees relative to the eligible population (e.g Rafkin et al., 2025; Finkelstein and Notowidigdo, 2019).

At a high level, our revealed-preference approach to estimating V differs from the calibration approach taken in Hendren and Sprung-Keyser (2020) or Bailey et al. (2024), who compute the impact of food stamps on outcomes and extract the WTP outcome-by-outcome. Calibration can be transparent but involves strong stances on how much households "should" be willing to pay based on food stamps' impacts on outcomes. Besides revealed-preference or calibration, one can use survey experiments to elicit WTP for an in-kind transfer directly (Krueger and Kuziemko, 2013; Caspi and Rafkin, 2025). The advantage of such experiments is that they do not require a market to exist, and experimental manipulations can check for behavioral biases directly. However, they can lack credibility if participants report expressive or affective responses; the elicitation is hypothetical; or data are otherwise noisy because of measurement error.

Another revealed-preference approach could infer demand for food stamps from trades in black markets. There, according to Whitmore (2002)'s survey, food stamps trade for about 65 cents per dollar. However, this price reflects the value for the marginal household induced to trade, and not the willingness to pay among inframarginals, and is thus only indirectly related to the policy-relevant willingness to pay. Households who trade on black markets could differ from the average, perhaps because they are especially desperate.

One attractive feature of using the enrollment response to variation in the subsidy to estimate willingness to pay is that it analyzes individuals' response to a decrease in the amount of cash they need to pay for food stamps, which is directly the object of interest. By contrast, other work that looks at how enrollment in benefits programs responds to changes in benefit sizes (Anderson and Meyer, 1997; Pukelis, 2024; Naik, 2025; McQuillan and Moore, 2025) or to changes in ordeal costs requires the additional step of needing to translate the ordeal or benefits into dollar values (e.g., Deshpande and Li, 2019; Finkelstein et al., 2019), precisely the object that we are trying to estimate. Estimating V based on the response to a change in the subsidy avoids this since the reform is already defined in terms of the change in monetary cost or price. More generally, any revealed-preference estimate of transfer-program value requires model structure to convert observed choices into a willingness to pay, unless the identifying variation involves changes in the price of the program in dollars (rather than changes in benefits, ordeals, or other costs not denominated in dollars). A virtue of our setting — or others in which there are changes in subsidies or prices — is that we have clean price variation denominated in dollars, which then eliminates the necessity of such structure. Of course, like these other, enrollment-response

methods for estimating V, we still must be willing to accept this response as revealing value.

Implications of $V(1) = 0.65$. Our primary estimate is $V(1) = 0.65$, or that households value food stamps at about 65 cents per dollar transferred. This value is much lower than the benchmark of 1 typically assumed in the literature and has important implications for policy. For instance, some accounting estimates of U.S. poverty count food assistance spending as equal to cash (e.g., Congressional Budget Office, 2025). Marking down the value of this transfer by a third would have significant implications. As one benchmark, we calculate that the total transfer value across the eight programs in Rafkin et al. (2025) would fall by about 5% if we mark down SNAP by 0.65. This number rises to 11% if we consider the transfer value of the seven transfers excluding Medicaid, another large in-kind program that recipients may not value dollar-for-dollar (Finkelstein et al., 2019). The 11% estimate would grow further if we also marked down food assistance in school meals or Special Supplemental Nutrition Program for Women, Infants, and Children (WIC), and if other programs (including Medicaid) have take-up costs which reduce the face value of enrollment. The estimate would fall if today's transaction costs are less than the past's.

If households are strongly inframarginal and would consume more than $R + F$, these estimates also imply 35 cents of FSP per dollar are dissipated by ordeals. These magnitudes for ordeals far exceed what one gets from monetizing time costs associated with applying — about 2 cents per dollar transferred in Finkelstein et al. (2019). Our estimates are more comparable to envelope theorem-based estimates of ordeal costs from benefit take-up elasticities (17 cents per dollar in SNAP in Rafkin et al., 2025). Applied to 2024 SNAP benefits of $94 billion, our ordeal costs would imply that $33 billion in deadweight loss from hassling inframarginal enrollees. So much deadweight loss from ordeals would substantially offset ordeals' possible targeting benefits and push toward making transfer programs automatic (Herd and Moynihan, 2025; Rafkin et al., 2025).

Stepping back, a recurring puzzle in this literature is that transfers are large enough that the ordeals needed to rationalize non-take up seem implausibly high.[42] One way out of the puzzle is to instead interpret non-take up as reflecting behavioral biases, for instance due to misperceptions or lack of information about benefit programs. Because the reforms we study ostensibly hold information fixed, the take-up responses to price variation — and positive benefit take-up elasticities in Pukelis (2024), McQuillan and Moore (2025), Naik (2025), and other work — pose a challenge to non-classical explanations like lack of information. The potential importance of ordeals accords with the not-enormous magnitudes of information-provision experiments for benefit take-up (e.g., Finkelstein et al., 2019). Naturally, optimization failures may also contribute to FSP non-take up, especially among today's non-enrollees at a subsidy rate of 1 who do not enter $V(1)$.

[42] Deshpande and Li (2019) finds that disability-insurance recipients must value their time at $100 per hour in order to rationalize the first-stage impact on take-up from DI office closings, and notes similar results for Alatas et al. (2016).

7 Conclusion

When in-kind transfers are free, even the neediest households can access the good — but they may not derive much private value from it. This trade-off arises in many settings, from health technologies in developing countries (Ashraf et al., 2010; Cohen and Dupas, 2010) to cost-sharing in Medicaid, School Lunch, and beyond. It is particularly relevant for the food stamps program, which was free when rolled out. We find that making food stamps free caused a large increase in enrollment without raising infant health. Even under conservative assumptions, new enrollees' low private valuations outweigh their high social valuations.

We draw several broader conclusions. First, our null effects on infant health during the elimination of the purchase requirement raise questions about food stamps' health impacts more generally. Second, we estimate private valuations of food stamps at 58 to 65 cents per dollar — well below the benchmarks used by the CBO and other agencies, implying meaningfully lower rates of effective government transfers. Third, low private valuations imply larger ordeals than commonly assumed. These findings speak to core debates in the design of transfer programs.

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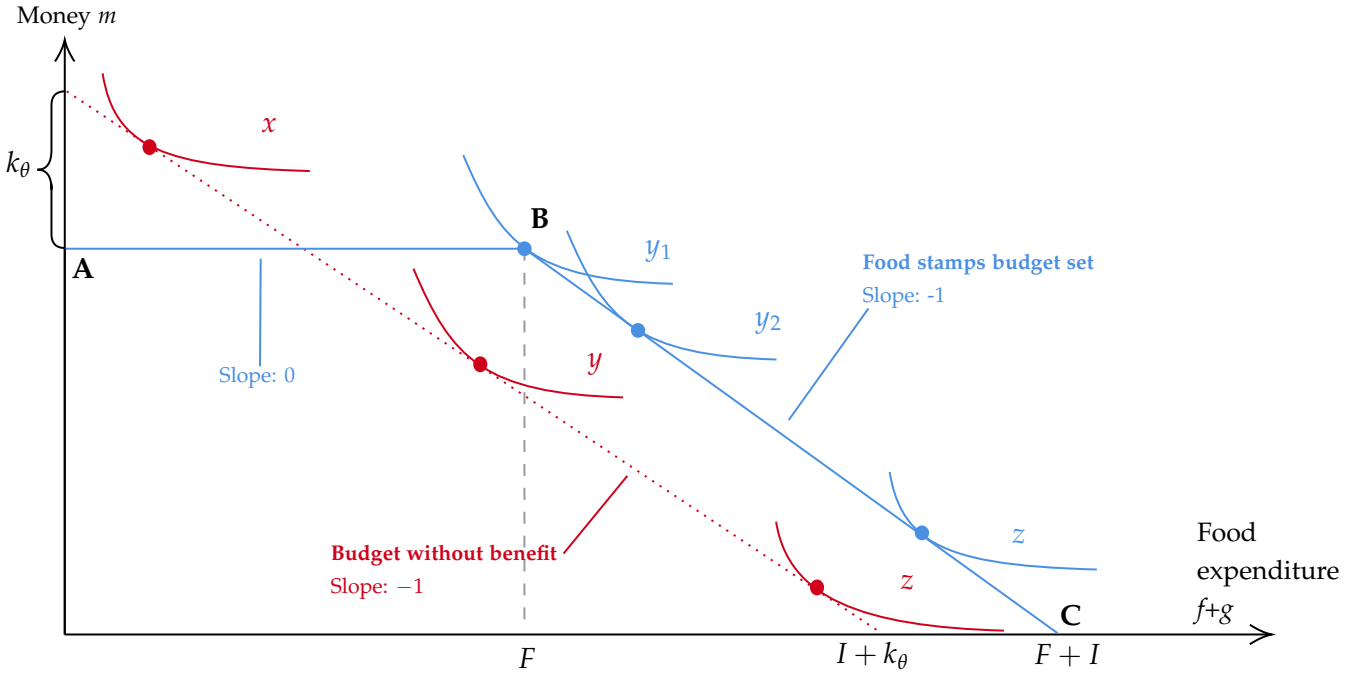
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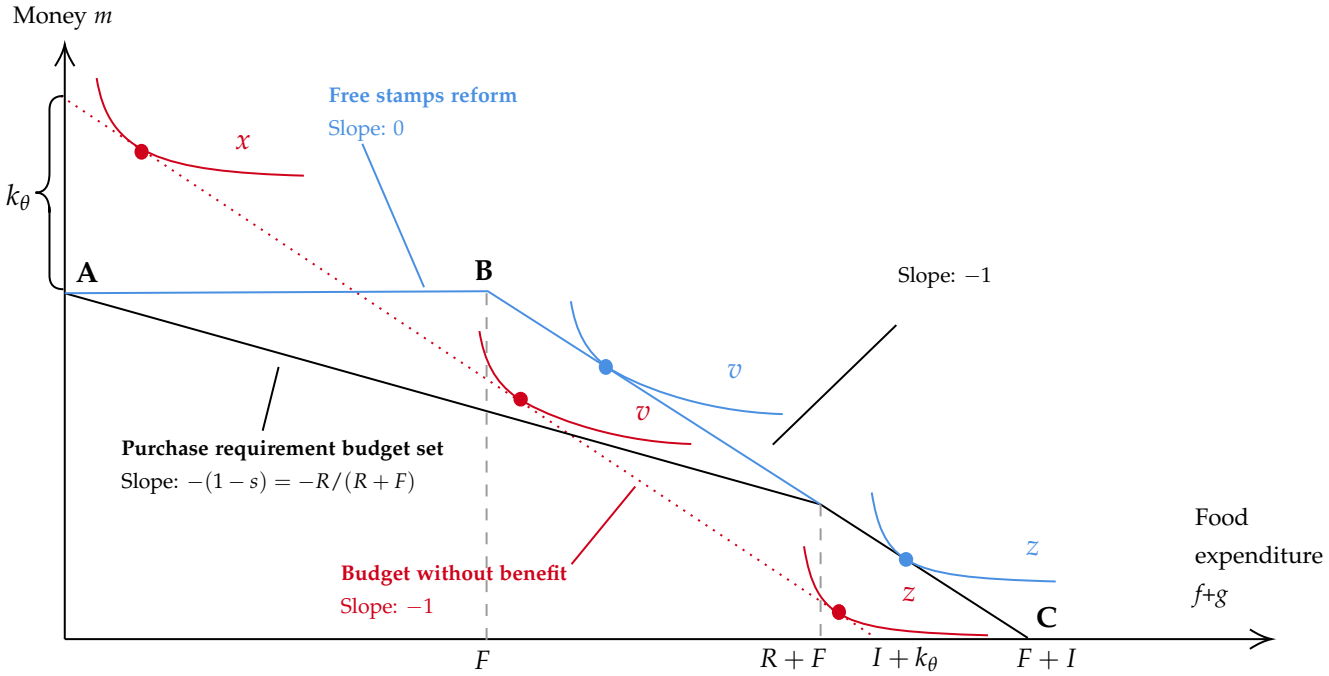
8 Figures

Figure 1: Budget Sets

(a) Contemporary, Free Food Stamp Program (No Purchase Requirement)



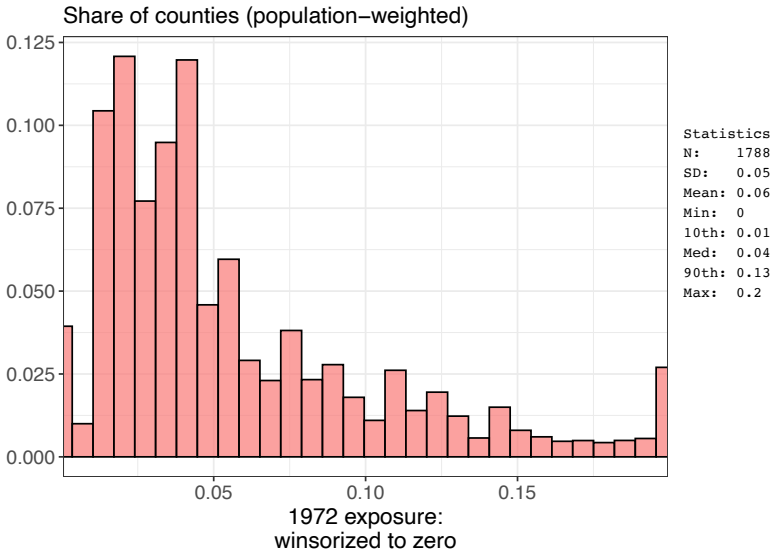
(b) Impact of the Elimination of the Purchase Requirement



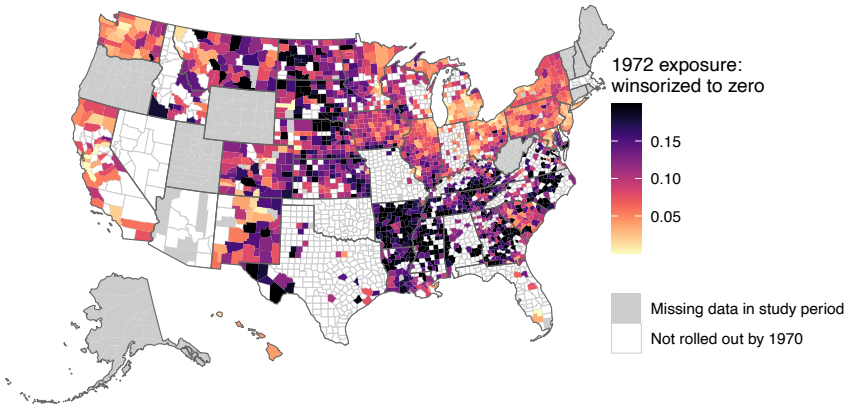
Note: Panel A shows a standard presentation of the budget sets in the contemporary food stamps program (e.g., Gruber, 2005). The main difference from the textbook presentation is that we show a household that does not participate (x) because of high take-up costs. Panel B shows budget sets and indifference curves under the purchase requirement.

Figure 2: Variation in Exposure

(a)

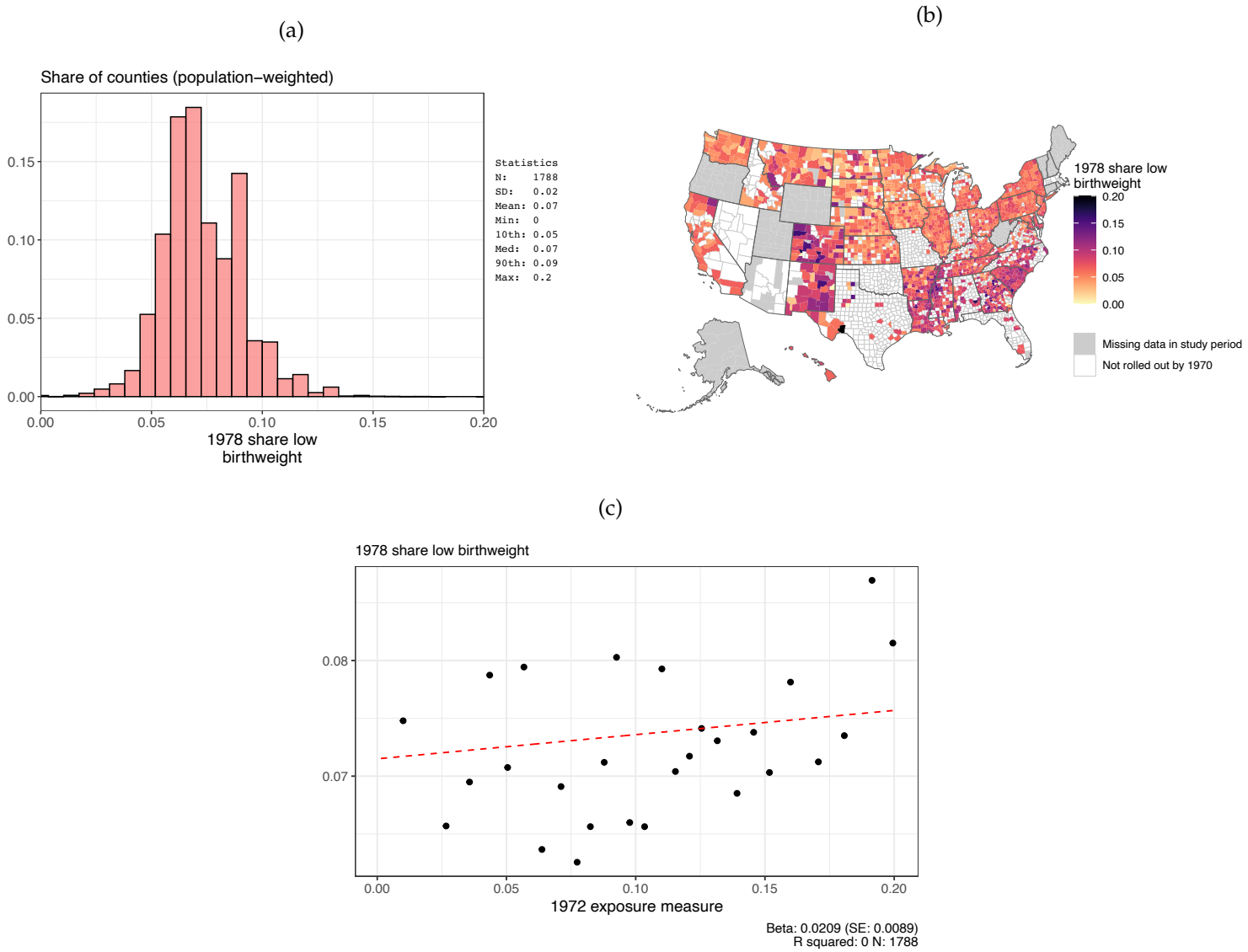


(b)



Notes: Figure shows a histogram of exposure across (population-weighted) counties (panel a) and a heat map of exposure across counties (panel b). Exposure is measured based on 1972 FSP enrollment counts, and analysis is limited to the counties that had rolled out their food stamp program by 1970. The exposure measure is winsorized to the 1970 population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, $\pm 3.2\%$.

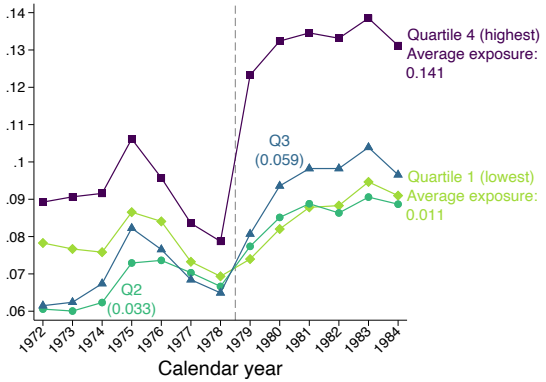
Figure 3: Variation in 1978 county-level share low birthweight



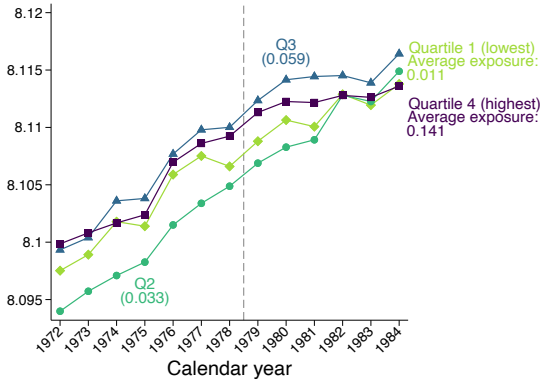
Notes: Figure describes the 1978 county-level share of births $< 2{,}500$ grams (low birthweight). Panel (a) shows a population-weighted histogram of share low birthweight, and panel (b) shows a heat map of share low birthweight across counties. Panel (c) shows a binned scatterplot of 1978 share low birthweight against the 1972 unwinsorized exposure measure, weighted by 1970 county population. A linear fit illustrates the population-weighted correlation between the two county-level measures. The sample across panels is limited to the panel that is balanced for the participation and birthweight outcome variables. The sample is also limited to counties that had rolled out FSP by 1970, for which both the 1972 and later-year exposure measures are defined (N=1,788 counties).

Figure 4: Outcomes by quartile of 1972 exposure measure

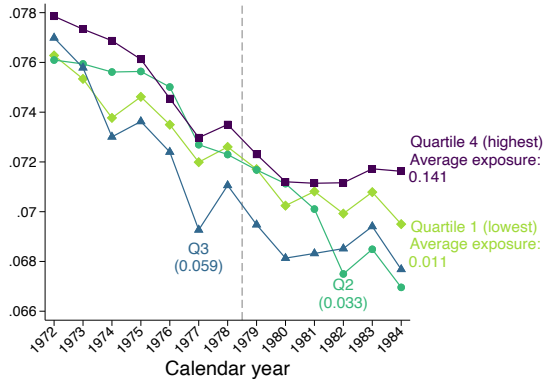
(a) Food Stamp Program enrollment rate



(b) ln(birthweight)



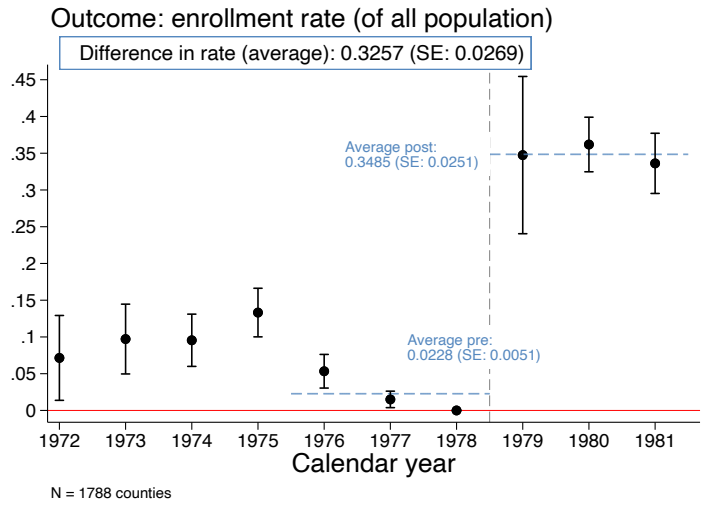
(c) share low birthweight (< 2,500 g)



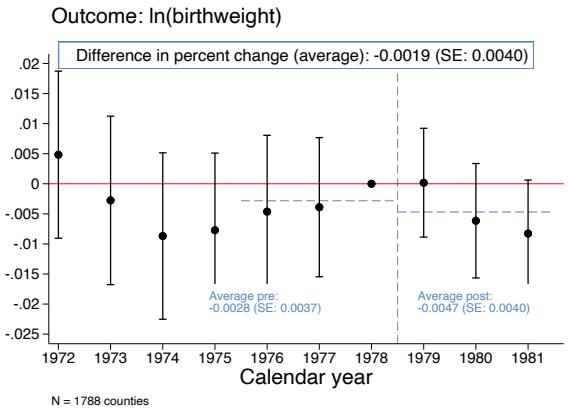
Notes: Figures show population-weighted average county-level outcomes over time by population-weighted quartile of the 1972 exposure measure. Darker shades in the series color scale correspond with increasing quartile of exposure. The average exposure measure within each quartile is displayed within series labels. In both panels, the exposure measure is winsorized to the population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, $\pm 3.2\%$. Sample is limited to counties where FSP was rolled out in 1970 or earlier, and further limited to the panel that is balanced for the participation and birthweight variables (1,787 counties). Sample period starts two years after the rollout cutoff year to allow the last counties rolling out to reach a steady state. Note that weighted quartiles are defined over this defined set of counties.

Figure 5: 1979 Reform impact on Food Stamp Enrollment Rate

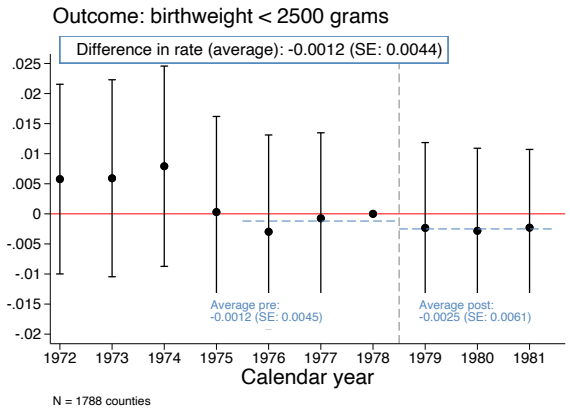
(a) First stage: impact on FSP enrollment



(b) Reduced form: impact on ln(birthweight)

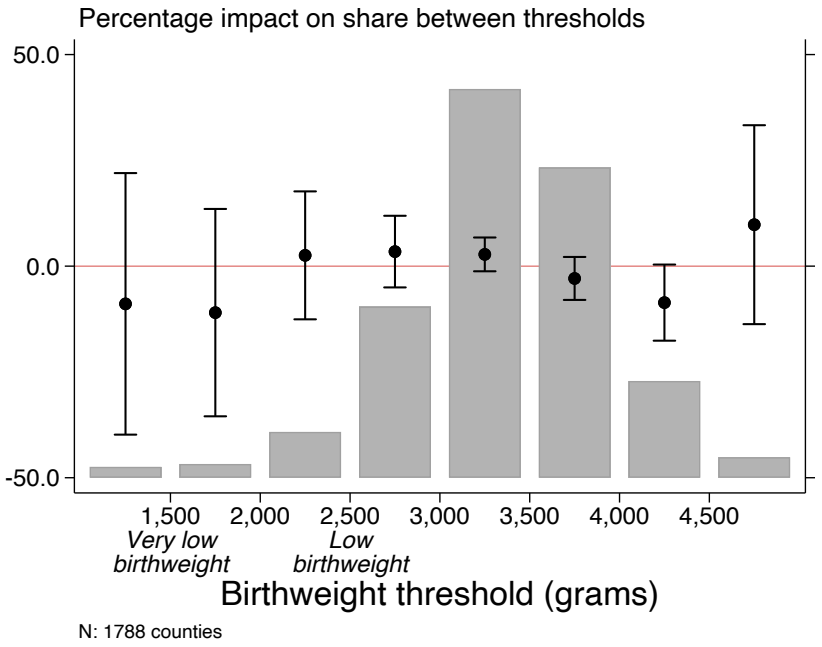


(c) Reduced form: impact on share low birthweight



Notes: Figure shows the results of estimating the first stage (Equation 10) in (a) and the reduced form equation (11) in (b), (c), using a continuous 1972 exposure measure on FSP enrollmentand infant birthweight outcomes. The exposure measure is winsorized to the 1970 population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, $\pm 3.2\%$. Sample is limited to counties that rolled out FSP by 1970, and further limited to the panel that is balanced for the participation and birthweight variables (N = 1,788 counties). Sample period starts two years after the rollout cutoff year to allow the last counties rolling out to reach a steady state. Regressions are all weighted by 1970 county-level population. Standard errors are clustered at the county level.

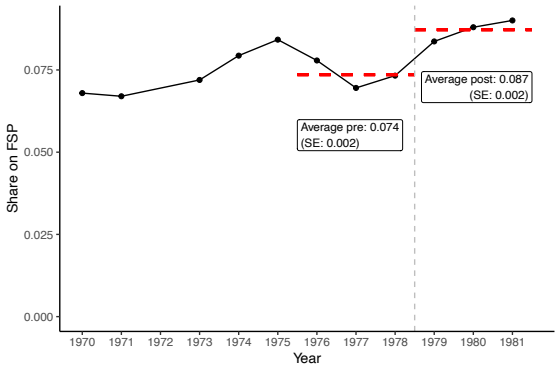
Figure 6: Percentage impacts of 1979 reform on share within birthweight ranges



Notes: Figure shows scaled estimates of β_{avg} (as defined in equation 12) from estimating equation (11) where the outcome is the share of births that fall within a specified number of grams. Because the share of births in 1976-1978 in various birth-wight ranges varies greatly (as shown in the light gray bars), we scale each estimate of β_{avg} by the share of births in 1976-1978 that fall within each range. We define the ranges as inclusive of the lower threshold and exclusive of the upper threshold. For example, we define the first outcome range as "$<$ 1,500 grams" followed by "$\geq$ 1,500 and $<$ 2,000." Similarly, we define the last range as "$\geq$ 4,500." The exposure measure is winsorized to the 1970 population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, $\pm$3.2%. Sample is limited to counties that rolled out FSP by 1970, and further limited to the panel that is balanced for the participation and birthweight variables (N = 1,788 counties). Regressions are weighted by 1970 county-level population and standard errors are clustered at the county level.

Figure 7: Trends in Enrollment and Enrollee Characteristics, PSID

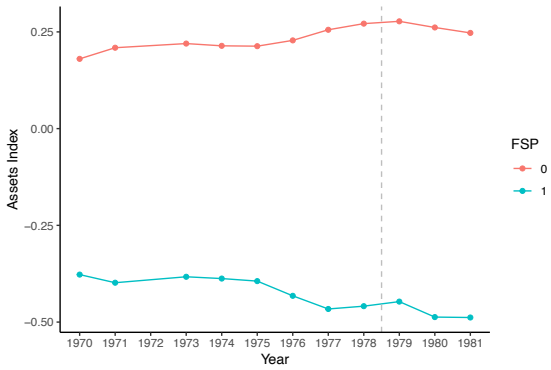
(a) Enrollment Rate



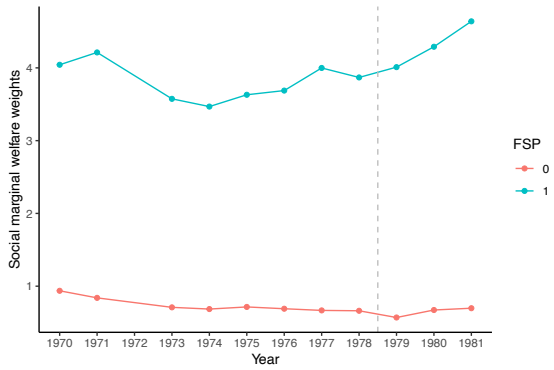
(b) Share in poverty bands



(c) Standardized index of durable assets



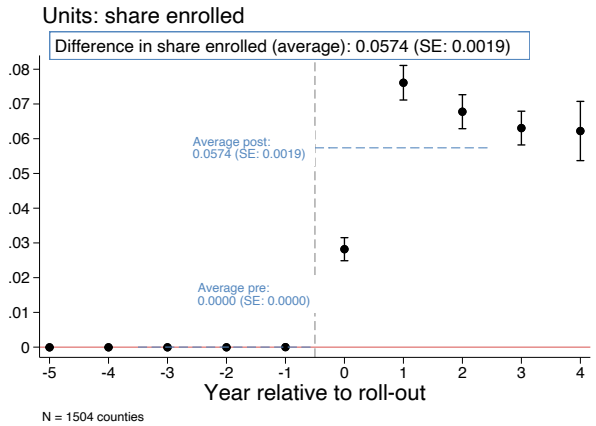
(d) Social marginal welfare weights



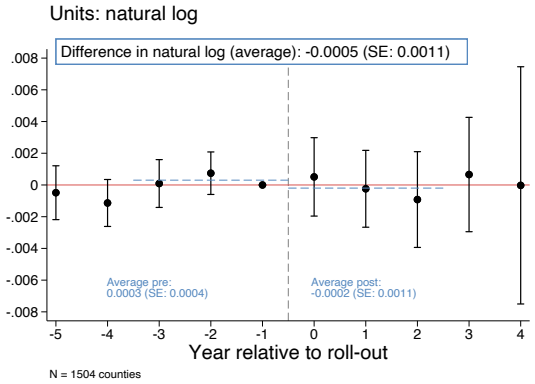
Notes: In the 1978 cross-section, 7.3% of the population (847 raw PSID observations) are participants, and 92.7% of the population (5,524 raw PSID observations) are nonparticipants. All shares within the FSP participant group and the FSP nonparticipant group are weighted using PSID weights. One unit is one household-year, so the same household could be included in the participant group one year and the nonparticipant group in another year depending on whether that household received any food stamps in a given year. The dashed line indicates 1978, the last period before the purchase requirement reform of 1979. FSP participation constructed as the amount saved with Food Stamps in a given year being greater than 0 ("amount saved" changes to "value of Food Stamps received" post-1979 reform). To summarize the reported enrollment shares in panel (a), average enrollment across the PSID sample in the pre-reform period (1976-1978) is 0.0735, and in the post-reform period (1979-1981), 0.0873. Poverty status is defined as family income less than Census needs standard (deflated to 1979 USD) which is based on family size, age of adults (elderly or not), and sex of head of household. Panel (c) then shows the share of households in each poverty band: 0-25%, 56-50%, and 51-100% of the poverty line. In panel (d) assets index is a simple average of standardized versions of 4 PSID variables: value of home (censored at 95%), number of rooms in home, owns home (yes/no), owns car (yes/no).

Figure 8: Impact of food stamps program roll-out: event study

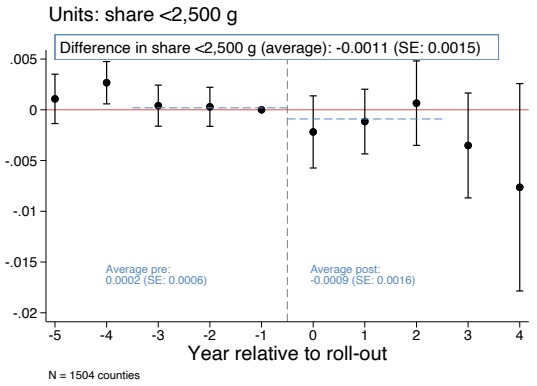
(a) First stage: impact on FSP enrollment



(b) Reduced form: impact on ln(birthweight)



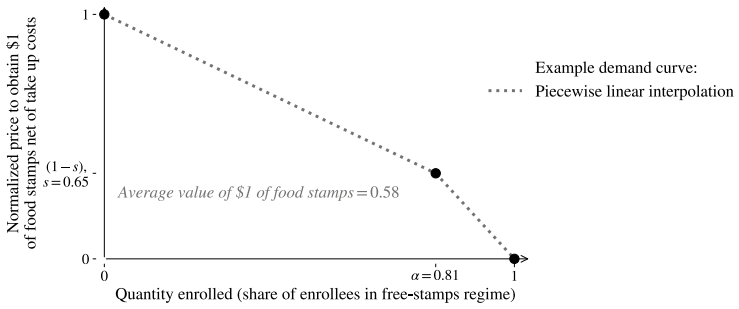
(c) Reduced form: impact on share low birthweight



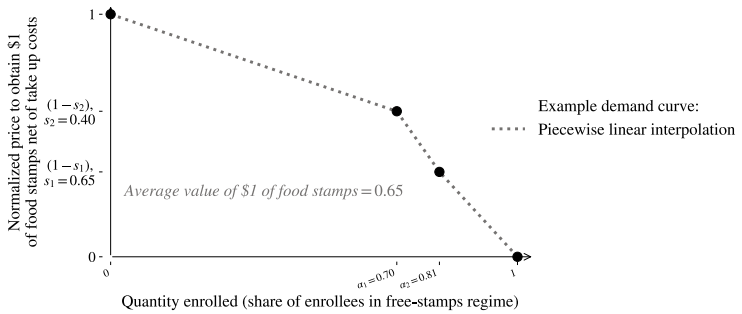
Notes: Figure shows estimates of β_r from equation 13 in panel a and from equation 14 in panels b and c. The regressions are weighted by dataset-normalized weights which are described above in equation 30. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the county level. Sample size, in terms of counties, is denoted in the figure legend. Since FSP enrollment is only reported annually, relative year corresponds to a calendar year for each county's rollout timing in panel a. Since birthweight data can be aggregated to the monthly level, relative year is constructed as the twelve months following each county's rollout timing. Therefore, relative year in panels b and c does not correspond to a calendar year.

Figure 9: The Value of Food Stamps: Demand Curves

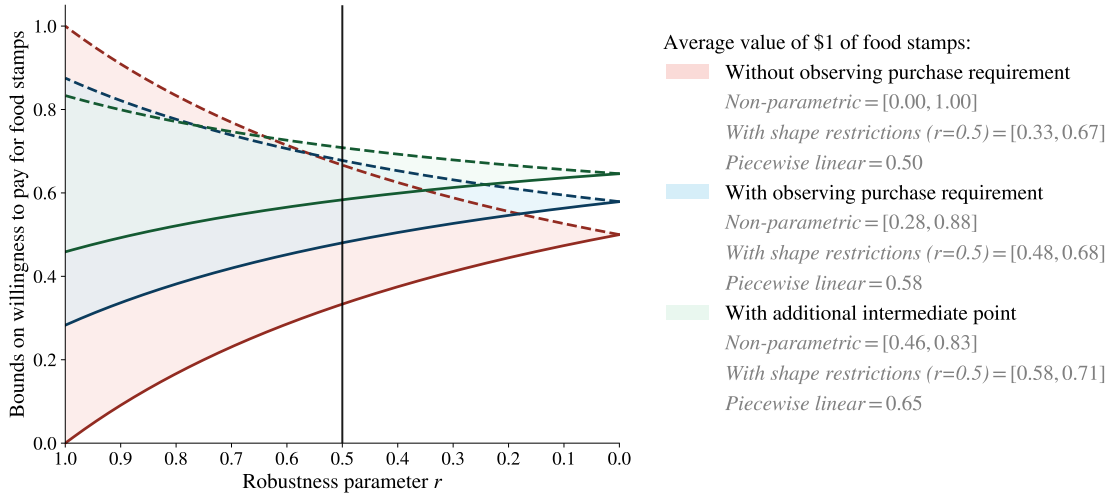
(a) With purchase requirement only



(b) With purchase requirement and bonus reform



(c) Robustness to parametric restrictions



Note: Panels A and B show points on the demand curve for food stamps. The points $(0,1)$ and $(1,0)$ reflect the assumptions that no one would enroll in food stamps absent any subsidy, and that individuals observed enrolling must weakly prefer participation. The points with a subsidy less than 100% comes from the take-up response to the elimination purchase requirement and the 1970 reform in the magnitude of the subsidy. We linearly interpolate between these points to form an estimate of the average value of food stamps among all enrollees. In Panel C, we use the framework in Kang and Vasserman (2025) to consider robustness to parametric choices on the demand curve. When the robustness parameter $r = 1$, we show the fully non-parametric bounds on the demand curves under monotonicity. When $r = 0$, we show the linear interpolations. When $r \in (0,1)$, demand curves whose slopes within each observed interval may deviate from the piecewise-linear demand slope by up to $r \times 100$ percent in either direction (Equation 19).

Table 1: Impact of food stamps program: stacked instrumental variable

	(1)	(2)	(3)	(4)
	First Stage	Stacked	Purchase requirement	Roll-out
Post rollout	0.0525***			
	(0.00153)			
Post reform	0.0185***			
	(0.00153)			
Panel A: ln(birthweight)				
Participation rate		-0.00519	-0.00586	0.00401
		(0.0116)	(0.0123)	(0.0283)
Hansen J p-value		0.748		
Baseline Mean	0.0224	8.104	8.107	8.103
Panel B: share low birthweight				
Participation rate		-0.00550	-0.00382	-0.0287
		(0.0129)	(0.0136)	(0.0368)
Hansen J p-value		0.524		
Baseline Mean	0.0224	0.0729	0.0727	0.0730
First-stage F	660.1			
p-value (joint test of instr.)	< 0.0001			
Observations	3313794	3313613	10728	3302885
Counties	2827	2827	1788	1504

Notes: Sample includes relative years -3 to +2 for FSP rollout and years 1976-1981 for the 1979 reform. Table shows precision-weighted estimates of δ from equations 18 and 16 in column (1). Column (2) shows precision-weighted estimates of λ from equations 17 and 15 for the pooled datasets (reform and rollout). Column (3) presents λ from equation 17 for the flat reform dataset alone, and then column (4), λ from equation 15 using the stacked rollout dataset alone. The regressions are weighted by county population in 1970 for rollout county-years and stacked dataset weights according to equation 30 for rollout county-years. The weights within each sample (rollout and reform) have been re-scaled to sum to 1. Standard errors clustered at the county level. Note that the "Post reform" impact is the exposure $*$ post coefficient re-scaled by the average 1972 exposure. The first-stage p-value reports the joint significance test of the excluded instruments. The Hansen J p-value reports the overidentification test of instrument exogeneity.

Table 2: Impact of food stamps program on ln(birthweight): stacked instrumental variable by race

	(1) First Stage	(2) Stacked	(3) Purchase requirement	(4) Roll-out
Panel A: Black sample				
Post rollout	0.0614***			
	(0.00257)			
Post reform	0.0390***			
	(0.00489)			
Participation rate (imputed)		0.0103	0.00905	0.0740
		(0.0120)	(0.0122)	(0.0594)
First-stage *F*	320.0			
p-value (joint test of instr.)	< 0.0001			
Hansen J p-value		0.281		
Baseline Mean	0.0620	8.033	8.039	8.031
Observations	1152078	1151976	3678	1148298
Counties	1120	1120	613	634
Panel B: White sample				
Post rollout	0.0119***			
	(0.000363)			
Post reform	0.00686***			
	(0.000916)			
Participation rate (imputed)		-0.0530	-0.0539	-0.0208
		(0.0345)	(0.0353)	(0.141)
First-stage *F*	569.8			
p-value (joint test of instr.)	< 0.0001			
Hansen J p-value		0.820		
Baseline Mean	0.0135	8.115	8.124	8.111
Observations	3245622	3245439	10398	3235041
Counties	2780	2780	1733	1490

Notes: Sample includes relative years -3 to +2 for FSP rollout and years 1976-1981 for the 1979 reform. Table shows precision-weighted estimates of δ from equations 18 and 16 in column (1) for each race-specific sample. Column (2) shows precision-weighted estimates of λ from equations 17 and 15 for the race-specific pooled datasets (reform and rollout). Column (3) presents λ from equation 17 for the flat reform race-specific dataset alone, and then column (4), λ from equation 15 using the stacked rollout race-specific dataset alone. The regressions are weighted by race-county population in 1970 for rollout county-years and stacked dataset weights according to equation 30 for rollout county-years. The weights within each sample (rollout and reform) have been re-scaled to sum to 1. Standard errors clustered at the county level. Note that the "Post reform" impact is the exposure $*$ post coefficient re-scaled by the average 1972 exposure within the sample. The first-stage p-value reports the joint significance test of the excluded instruments. The Hansen J p-value reports the overidentification test of instrument exogeneity.

Table 3: Impact of food stamps program on share low birthweight: stacked instrumental variable by race

	(1) First Stage	(2) Stacked	(3) Purchase requirement	(4) Roll-out
Panel A: Black sample				
Post rollout	0.0614***			
	(0.00257)			
Post reform	0.0390***			
	(0.00489)			
Participation rate (imputed)		-0.00592	-0.00256	-0.182**
		(0.0175)	(0.0179)	(0.0862)
First-stage *F*	320.0			
p-value (joint test of instr.)	< 0.0001			
Hansen J p-value		0.0394		
Baseline Mean	0.0620	0.132	0.130	0.133
Observations	1152078	1151976	3678	1148298
Counties	1120	1120	613	634
Panel B: White sample				
Post rollout	0.0119***			
	(0.000363)			
Post reform	0.00686***			
	(0.000916)			
Participation rate (imputed)		-0.0165	-0.0150	-0.0751
		(0.0360)	(0.0366)	(0.178)
First-stage *F*	569.8			
p-value (joint test of instr.)	< 0.0001			
Hansen J p-value		0.741		
Baseline Mean	0.0135	0.0636	0.0589	0.0658
Observations	3245622	3245439	10398	3235041
Counties	2780	2780	1733	1490

Notes: Sample includes relative years -3 to +2 for FSP rollout and years 1976-1981 for the 1979 reform. Table shows precision-weighted estimates of δ from equations 18 and 16 in column (1) for each race-specific sample. Column (2) shows precision-weighted estimates of λ from equations 17 and 15 for the race-specific pooled datasets (reform and rollout). Column (3) presents λ from equation 17 for the flat reform race-specific dataset alone, and then column (4), λ from equation 15 using the stacked rollout race-specific dataset alone. The regressions are weighted by race-county population in 1970 for rollout county-years and stacked dataset weights according to equation 30 for rollout county-years. The weights within each sample (rollout and reform) have been re-scaled to sum to 1. Standard errors clustered at the county level. Note that the "Post reform" impact is the exposure $*$ post coefficient re-scaled by the average 1972 exposure within the sample. The first-stage p-value reports the joint significance test of the excluded instruments. The Hansen J p-value reports the overidentification test of instrument exogeneity.

Table 4: PSID household characteristics of FSP enrollees

Variable	(1) Pre-period (always takers) (1976-1978)	(2) Post-period average (1979-1981)	(3) Complier Average (Estimated from Agg FS)
Total income	8045	7382	4671
	(121)	(110)	(2772)
At or below 50pct FPL	0.090	0.133	0.311
	(0.0057)	(0.0057)	(0.18)
51-100pct FPL	0.390	0.415	0.514
	(0.0097)	(0.0088)	(0.12)
101-150pct FPL	0.520	0.452	0.175
	(0.0099)	(0.0089)	(0.28)
Assets Index	-0.452	-0.475	-0.568
	(0.012)	(0.011)	(0.12)
Social marginal welfare weight	4.110	4.618	6.695
	(0.067)	(0.062)	(2.1)
Households (N)	2553	3146	

Notes: Table shows pre- and post-period household characteristics for households enrolled in FSP, measured three years before and after the 1979 purchase requirement reform. Columns report means weighted by PSID household weights, a post-pre difference, and the weighted means across all years (1976-1981) for all households enrolled in FSP in those years. Standard errors are shown in parentheses. Total household income is CPI-adjusted to 1979 dollars. "At or below 50% poverty" indicates households with income at or less than 50% of the Census poverty threshold as defined in the PSID. Additional rows describe the share of households in poverty bands at 51-100% and 101-150% of the FPL. Social marginal welfare weights are defined as the household's inverse-cubed censored income (censored at the 5th percentile of total income in 1970) relative to the average inverse-cubed income of all households across years 1976-1981. Column (3) reports mean outcomes among compliers, estimated means using the aggregate first stage, scaled by baseline average 1972 exposure, to generate a measure of the share of post-reform enrollees who join due to the reform.

Table 5: Welfare valuation

	Shares of types		Welfare weights		Private valuations		Distortions		Fiscal externalities		Welfare valuations		
	σ_a (1)	σ_c (2)	λ_a (3)	λ_c (4)	V_a (5)	V_c (6)	D_a (7)	D_c (8)	FE_a (9)	FE_c (10)	$\text{W}_a = W(0.65)$ (11)	W_c (12)	$W(1)$ (13)
1. **Primary estimate**	**0.804**	**0.196**	**4.11**	**6.69**	**0.675**	**0.175**	**0**	**0**	**0**	**0**	**2.77**	**1.17**	**2.46**
2. Equal welfare weights	0.804	0.196	4.11	4.11	0.675	0.175	0	0	0	0	2.77	0.72	2.37
3. Double welfare weight gap	0.804	0.196	4.11	9.28	0.675	0.175	0	0	0	0	2.77	1.62	2.55
4. Halve first stage	0.891	0.109	4.11	6.69	0.675	0.175	0	0	0	0	2.77	1.17	2.60
5. Double first stage	0.672	0.328	4.11	6.69	0.675	0.175	0	0	0	0	2.77	1.17	2.25
6. V_a from bonus reform	0.804	0.196	4.11	6.69	0.758	0.175	0	0	0	0	3.11	1.17	2.73
7. Upper bound for V_a, V_c (w/o bonus reform)	0.804	0.196	4.11	6.69	1.000	0.350	0	0	0	0	4.11	2.34	3.76
8. Lower bound for V_a, V_c (w/o bonus reform)	0.804	0.196	4.11	6.69	0.350	0	0	0	0	0	1.44	0.00	1.16
9. Lower bound for V_a (w/o bonus), upper bound for V_c	0.804	0.196	4.11	6.69	0.350	0.350	0	0	0	0	1.44	2.34	1.62
10. Lower bound for V_a (with bonus), upper bound for V_c	0.804	0.196	4.11	6.69	0.568	0.350	0	0	0	0	2.33	2.34	2.33
11. Calibrated FE $= 0.05V_\theta$	0.804	0.196	4.11	6.69	0.675	0.175	0	0	0.207	0.337	2.30	0.88	2.00
12. Maximum FE (tax and transfer rate)	0.804	0.196	4.11	6.69	0.675	0.175	0	0	0.129	0.129	2.46	1.04	2.18
13. Maximum FE, always-takers only	0.804	0.196	4.11	6.69	0.675	0.175	0	0	0.129	0.000	2.46	1.17	2.23
14. Distortion equal to V	0.804	0.196	4.11	6.69	0.675	0.175	0.675	0.175	0	0	5.55	2.34	4.92
15. Distortion equal to V, compliers only	0.804	0.196	4.11	6.69	0.675	0.175	0	0.175	0	0	2.77	2.34	2.69
16. Distortion from present bias: $\beta_a = 0.7, \beta_c = 0.7$	0.804	0.196	4.11	6.69	0.675	0.175	0.300	0.300	0	0	4.01	3.18	3.84
17. Distortion from present bias: $\beta_a = 0.85, \beta_c = 0.7$	0.804	0.196	4.11	6.69	0.675	0.175	0.150	0.300	0	0	3.39	3.18	3.35
18. Distortion from present bias: $\beta_a = 1.0, \beta_c = 0.7$	0.804	0.196	4.11	6.69	0.675	0.175	0	0.300	0	0	2.77	3.18	2.85

Note: In our primary estimate, social marginal welfare weights are estimated from the PSID and constructed by assuming CRRA utility with risk aversion parameter of 3. Private valuations assume piecewise-linear demand with valuations at the midpoint of identified bounds. Distortions reflect either the private values themselves or, in the present bias rows, ($D_\theta = 1 - \beta_\theta$). We present sensitivity analyses varying the first-stage estimate, welfare weights, and distortion assumptions.

- W_a corresponds to the welfare ratio for always takers, equivalent to $W(0.65)$ from Equation (4). W_c corresponds to to the welfare ratio for compliers only: $W_c = \frac{\lambda_c(V_c+D_c)}{1+\text{FE}_c}$.
- Rows 6–10 vary assumptions about the valuations. Row 6 uses information about the bonus reform (Figure 9). Rows 7–10 show upper and lower bounds on V_a and V_c, including or excluding the bonus reform as indicated.
- Our primary estimate for fiscal externalities is 0. In Rows 11, 12, and 13, we use a tax and transfer rate of 0.129 (Hendren and Sprung-Keyser, 2020) and income effect elasticity of 0.39 (Vivalt et al., 2024), which delivers an FE of about $0.05V_\theta$ (Equation 36).
- Rows 14–18 make assumptions about the distortions. Row 14 assumes $D_\theta = V_\theta$, or $D_a = 0$ and $D_c = V_c$ (Row 15). Rows 16–18 specify a simple back-of-the-envelope for behavioral bias as discussed in the text, with present-bias parameter $\beta \in \{0.7, 0.85, 1\}$ as shown in the row.

Appendices for Online Publication

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A Food Stamp Program: History of Major Reforms

Early program history In 1939, the first version of the food stamp program took the form of a food surplus redistribution program to combat the effects of unemployment. Those receiving benefits purchased orange stamps to spend on their normal food purchases, and in addition to each $1 of orange stamps, they would receive 50 cents of blue stamps to spend on items deemed to be surplus. By 1943, World War II efforts eliminated the surpluses that partially motivated the program, but over the four years of program activity, about 20 million people in the U.S. received benefits (Caswell and Yaktine, 2013).

In February 1961, the Kennedy administration began piloting a series of food stamp programs across 43 counties and cities that still required participants to purchase coupons but no longer limited food purchases to surplus goods. Then, in January 1964, President Johnson institutionalized the program through the Food Stamp Act of 1964 which delineated its purpose as improving nutrition for the poor. Although the funding and benefits for the program were set federally, the legislation left states to decide eligibility and operational schemes (USDA Food and Nutrition Service, 2025). Coupon allotments, or the amount of coupons a family could purchase, varied by household size, income, and region of the country, with separate benefits schedules for the North and the South. The Department of Agriculture set these allotments based upon responses to food consumption surveys with the goal of having eligible households spend about their normal food expenditures on purchasing stamps, allowing the difference between the allotted coupons and their purchase requirement to be the "bonus" (Richardson, 1979). Although the program greatly expanded throughout the remainder of the decade, benefits were largely unchanged. In early 1969, the mean rate of subsidization, in terms of bonus out of allotment, was 40% (Hoover, 1974).

Fueled by concerns of low program participation, benefits were increased, effective January 1970. One nationalized benefits schedule took the place of the region-specific allotments, the purchase requirements were decreased, and the bonus was increased (Richardson, 1979). Overall, the mean rate of subsidization jumped to 63.2% (Hoover, 1974). These edits to the benefits schedules were part of a larger set of amendments to the Food Stamp Act signed into law in 1971. The amendments introduced regular indexing of allotments to account for inflation and nationalized eligibility standards (except for Alaska, Hawaii, and territories) (Richardson, 1979), including establishing the categorical eligibility of households enrolled in public assistance programs (U.S. Congress, 1970). The changes prohibited the purchase requirement from exceeding 30% of of income, and for the first time, the purchase requirement was eliminated for low-income households with monthly income below $30 (Richardson, 1979).

Legislation in 1973 mandated that the Food Stamp Program be rolled out across all counties by July 1974. These amendments also increased the frequency of indexing food stamp allotments to twice per year and ensured access for certain groups, such as those in treatment or rehabilitation

centers. The number of program participants continued to grow throughout the early 1970s, reaching 18.5 million people in 1976 (USDA Food and Nutrition Service, 2025).

The status quo prior to the 1979 reform. By the mid-1970s, the program's structure was based on the goal that every household should have the opportunity to obtain a nutritionally adequate diet, and that households should not have to contribute more than about 30% of their own income towards monthly food consumption; the government would therefore make up the shortfall between the household's own income and the minimum cost of that nutritionally adequate diet (Congressional Budget Office, 1977; Richardson, 1979). The dollar value of that diet was called the household's monthly allotment; it was based on the U.S. Department of Agriculture's (USDA) Thrifty Food Plan, and was constant across household income but varied based on household size to account for economies of scale that allowed for bulk purchases in larger families. The allocation of the allotment between the participants (via the purchase requirement) and the government (via a transfer) depended on household income, with the purchase requirement increasing in income and the government transfer decreasing in income (Richardson, 1979).

Participating households would pay the government $0 \leq r \leq R$ (where R denotes the maximum participant purchase allowed) for various values of r, and in return receive a dollar value of food stamps $f = r(R+F)/R$ from the government. The total dollar value of food stamps was split between the individual contribution (r) and the government contribution rF/R up to a maximum government transfer of F that would occur if the household paid the maximum purchase requirement ($r = R$).

More specifically, households could choose $r \in (0, 0.25R, 0.5R, 0.75R, R)$ (Congressional Budget Office, 1977; Richardson, 1979) In practice, however, participating households essentially all chose the maximum allotment $R + F$.

The food subsidy the household faced varied with household income. Specifically, while as mentioned above the total monthly allotment - i.e. the dollar value of a nutritionally adequate diet - was the same across households of a given size ($F + R$), the split in the allotment between purchase requirement (R) and government transfer (F) varied with household income; therefore the food subsidy the household faced varies with household income. The subsidy was 100 percent for the poorest households who had no purchase requirement ($R = 0$), and increased by about \$1 with every \$3 increase in income (Congressional Budget Office, 1977). [43]

The 1979 reform: Elimination of the purchase requirement The 1979 reform eliminated the purchase requirement for everyone (i.e. set $R = 0$ for all eligible households) but left the

[43]Transfers from other government programs such as Aid for Families with Dependent Children (AFDC) counted as income so that AFDC recipients - who were over two-fifths of household recipients in 1975 - still had to pay the purchase requirement (Congressional Budget Office, 1977).

government transfer (essentially) F unchanged. As a result, the food stamp subsidy ($s = \frac{F}{R+F}$) because 100%. Prior to the reform, however, the purchase requirement was increasing with income (Appendix Figure A2). The reform eliminated the purchase requirement for everyone (Appendix Figure A3). Before 1979, the subsidy declines with income, so lower-income households get a larger subsidy. After 1979, all eligible households receive a full subsidy (100%). As a result, the 1979 reform generates a larger increase in the subsidy for higher-income households; Appendix Figure A4, panel (b) illustrates this clearly.

We have abstracted from this variation, focusing only on the average subsidy under the two regimes; the fact that we will find that the removal of the purchase requirement increased enrollment particularly among poorer individuals is especially striking in light of the fact that it involved a bigger subsidy increase for less poor households.

Other aspects of the 1979 reform. Although the elimination of the purchase requirement was the centerpiece of the 1979 reform, it included several other changes to the food stamp program (Berry, 1982; Rosenfeld, 2010). First, the reform included a small increase in the bonus (F), the magnitude of which was similar to prior years' inflation adjustments. Appendix Figure A4 shows the food stamp allotment (F) and subsidy rate ($\frac{F}{R+F}$) for a four-person household in 1972–1979 obtained from the 1976, 1978, and 1979 *Federal Register* (Food and Nutrition Service, USDA, 1976, 1978, 1979). These documents report the official Food Stamp coupon allotment tables used by caseworkers to determine household benefits. It shows some year-to-year variation in both the alottment and the subsidy schedule before 1979 due to inflation adjustments; however it shows that the dominant policy variation in the figure is in 1979, and comes from the increase in the subsidy (panel b) rather than the (typical) increase in the allotment (anel a).

Second, the 1979 reform changed some of the eligibility rules. It lowered the maximum net income level allowed for eligibility, and it also reduced the deductions that a family could take in calculating net income; both of these changes reduced eligibility among higher income individuals. At the same time, however, it also eliminated categorical eligibility for recipients of cash welfare (general welfare, AFDC, and SSI); after the reform, these indivdiuals needed to apply to the food stamp program separately, which may have decreased enrollment among lower income individuals.[44]

To examine the consequences of these changes in eligibility, we simulate eligibility for households in the PSID in 1976–68 and 1979–81 based on information about net income (income excluding standard and wage-based deductions), AFDC receipt, and the maximum allowable monthly income for Food Stamps eligibility in each year for four-person households.[45] The policy

[44]The reduction of deductions also resulted in smaller benefits for higher income households, but higher benefits for lower income households.

[45]After 1979 when the Food Stamp Act of 1977 went into full effect, benefits were determined not by published tables but by using the Thrifty Food Plan and net-income-based formulas. We obtain the maximum allowable monthly

reform's adjustments to eligibility appear to have produced a small (3 percentage point) reduction in the share of households eligible (Figure A5). To the extent that these changes bind, this would bias against our estimated impact of the 1979 reform in increasing enrollment; we would therefore underestimate the share of compliers and overestimate the average WTP (V) for food stamps. Given average take-up rate (i.e. enrollment rate among the eligible) prior to the reform for four person households of 64% in 1976-1978 in the PSID, with no change in take-up a 3 percentage point decline in eligibility should have about a 2 percentage point decline in the enrollment rate, while we estimate YY

We also looked more directly at the elimination of AFDC-based categorical eligibility. Evidence from the PSID suggests that this channel is quantitatively small. First, Figure A6 shows that the share of households participating in both FSP and AFDC is small — about 5%, compared with about 15% participating in FSP only — and remains flat over time. Second, in Figure A7 panel (a), we classify households in every year using the pre-reform categorical eligibility concept: households that would have only been eligible for food stamps due to categorical eligibility under the pre-1979 rules. Among these households, food stamp participation remains stable and stays above 75% even after 1979. Thus, most households that would previously have qualified through categorical eligibility continued to obtain benefits after the reform. Third, Figure A7 (panel b) shows that the share of food stamp recipients who appear to qualify only through pre-1979 categorical eligibility falls after 1979, but only modestly. Overall, these facts suggest that removing categorical eligibility was not a major driver of the reform's effects. If it were, one would expect much larger shifts in joint Food Stamp–AFDC participation or in the share of recipients appearing to rely solely on categorical eligibility.

Finally, while the reform did not change the basic "ordeals" associated with receiving food stamps — individuals still had to go to the local food stamp offices to pick up stamps – it did include several provisions aimed at increasing access among eligible individuals. For example, it allowed the use of mail, telephone or home visits for certifying a household's eligibility, and it required local stamps offices to provide outreach and nutrition education materials, as well as access to bilingual personnel and materials for applicants. It is unclear however, how quickly these increases in access were implemented or what if any impact they had.

net income for Food Stamp eligibility in 1980 and 1981 from The U.S. Department of Agriculture's overview of domestic food programs (Longen (1980)) and an All-County Information Notice from California Department of Social Services (1980).

B Welfare Framework: Graphical Analysis

Appendix Figure A1 provides a graphical illustration of the tradeoff between reduced targeting on private value and (potentially) increased targeting on social value from increasing the subsidy s. It does so under the simpifying assumption that the only potential wedge between private and social value comes from the social welfare weights λ_θ; in other words, fiscal externalities are constant across types θ and weakly positive ($\text{FE}_\theta = \text{FE} \geq 0$) and distortions are zero ($D_\theta = 0$). These restrictions simplify the exposition without affecting the core intuition; they also consistent with our ultimate empirical findings.

In each panel, the vertical axis is the "price" (p) of receiving \$1 in food stamps (or equivalently, $1 - s$, where s is the subsidy rate). The horizontal axis is quantity q, defined as the share enrolled in food stamps, normalized by the share enrolled when $p = 0$ (i.e., $s = 1$). The black line plots the *Marginal Social Cost (MSC)* per dollar of food stamps; it is horizontal because fiscal externalities are assumed constant, and it weakly exceeds 1 because fiscal externalities are weakly positive. The red curve plots the *Marginal Private Benefit (MPB)*: $\text{MPB}(q) = V_{(q)}$. That is, $\text{MPB}(q)$ is the willingness-to-pay per dollar of the marginal (i.e. q-th quantile) household to enroll. The orange curves plot the *Marginal Social Benefit (MSB)*: $\text{MSB}(q) = \lambda_{(q)}\text{MPB}(q) = \lambda_{(q)}\text{V}(q)$, where $\lambda_{(q)}$ denotes the marginal social welfare weight that rescales the marginal household at quantity q's private willingness to pay into a social value. Enrolling

Enrolling the q-th type *increases social surplus* if $\text{MSB}(q) > \text{MSC}$. If the MSB crosses the MSC curve only once and from above, then the *social surplus maximizing* enrollment level q^* satisfies $\text{MSB}(q^*) = \text{MSC}$ and the planner can implement q^* by setting the subsidy s^* such that $1 - s^* = \text{MPB}(q^*)$.

With zero distortions, $\text{MSB} = \lambda_{(q)}V_{(q)}$, and $V_{(q)}$ is weakly decreasing by definition. For a higher subsidy to increase surplus, $\lambda_{(q)}$ must be increasing so much that it offsets $V_{(q)}$. This is what we mean by a trade-off between private and social value. Free stamps increase surplus relative to positive cost sharing only if the social welfare weights of those who enroll at the higher subsidy are sufficiently large to outweigh their lower private valuations.

The figure illustrates that the tradeoff between targeting on private versus social value emerges from the distance between MPB (red curve) and MSB (orange curve). This is similar to the analysis in Shepard and Wagner (2025)'s study of ordeals in health insurance markets, where private and social value do not coincide because of adverse selection. The figure also shows the *Average Social Benefit* curve in blue, where $\text{ASB}(q) = \frac{1}{q}\int_0^q \text{MSB}(\tilde{q})d\tilde{q}$. Note that $ASB(q)$ is simply the numerator of social welfare $W(q)$, while average social cost is the denominator. Therefore,allocation q is more cost effective than allocation q' (i.e. Whether $W(q) < W(q')$ if and only if $\text{ASB}(q) > \text{ASB}(q')$. In other words, the figure illustrates that the *most cost-effective* quantity q^+ is the one that maximizes

ASB; it is implemented by subsidy s^+.[46]

The four different panels of the figure illustrate comparative statics in both the surplus maximizing subsidy s^* and the most cost-effective subsidy s^+. Panels A and B show examples in which both the social surplus maximizing allocation (s^*) and the cost effectiveness maximizing allocation (s^+) involve positive cost-sharing (i.e. $< 0s < 1$). A comparison of the two panels also illustrates some comparative statistics of s^* and s^+: in Panel B, the MSB curve (MSB_2) is flatter than in panel A and hence the optimal subsidies are also correspondingly higher. Specifically, for the first type to enroll, the MSB curves are the same in Panels A and B (i.e., $\text{MSB}_1(q=0) = \text{MSB}_2(q=0)$) but $\frac{\partial \text{MSB}_2(q)}{\partial q} - \frac{\partial \text{MSB}_1(q)}{\partial q} > 0$). Formally, we can think of the key comparative static for whether a higher subsidy increases total surplus as hinging on the slope of the MSB curve, $\frac{\partial \text{MSB}(q)}{\partial q}$

Panels C and D illustrate two cases in which MSB always lies above MSC, so that the surplus maximizing allocation is one in which food stamps are free: $s_3^* = s_4^* = 1$. However, in Panel C, the cost-effectiveness maximizing subsidy involves positive cost-sharing ($s_3^+ < 1$), whereas in Panel D, the cost-effectiveness maximizing subsidy is also free food stamps ($s_4^+ = 1$). The reason that the cost-effectiveness maximizing susidy is less than 100% in Panel C is that MSB_3 is decreasing for quantities beyond q_3^+, so that enrollees beyond q_3^+ dilute the average cost-effectiveness despite their having positive social surplus from enrolling ($MSB_3 > MSC$).[47] Our findings will most resemble Panel C, in which, relative to the prior positive cost-sharing regime ($s = 0.65$), free food stamps ($s = 1$) increase social surplus ($W(1) > 1$), but are not cost effective ($W(1) < W(0.65)$.

C Empirical Appendix

C.1 Imputing Race Specific Enrollment Rates

We do not directly observe race-specific county-year enrollment rates. Instead, we construct them using county-year overall enrollment rates, race-specific county-level population, race-specific poverty rates by state, and national race-specific take-up rates. The key assumption is that the ratio of national take-up rates for non-whites to whites applies uniformly across counties.

[46] One way to see the distinction between maximizing social surplus and maximizing cost-effectiveness is to observe that the subsidy that maximizes social surplus s^* maximizes the *total* surplus (i.e., maximizes the area between MSB and MSC). By contrast, the subsidy that maximizes cost-effectiveness s^+ maximizes the *average* social benefit per dollar spent (i.e., maximizes the height of the ASB curve). Even if surplus increases from enrolling a type (i.e. MSB > MSC), enrolling that type can reduce the average cost-benefit ratio if enrollment produces less welfare than the enrollment of prior types.

[47] Note that in Panel C, MSB_3 is non-monotonic, while Panel D shows a version where MSB_4 is always increasing. For MSB to have a positive slope everywhere and thus produce $W(1) > W(s < 1)$ as in Panel D is quite demanding. Yet, if MSB always exceeds MSC for this population, that suggests the government should already be doing much more redistribution to lower the λ_θ's in the target population. That is, according to an "inverse-optimum" logic, MSB would intersect MSC at a subsidy less than 1. Of course, there is no guarantee that existing policy is close to optimal.

Recall from Section 3.1 that the county-year food stamp enrollment rate is

$$\text{FS}_{ct} = \frac{e_{ct}}{\text{pop}_c}, \tag{20}$$

where e_{ct} is the number of people enrolled in food stamps in county c in year t and pop_c is the 1970 county population for our analysis of the 1979 reform (1960 population for our analysis of the roll-out). This can be decomposed as

$$\text{FS}_{ct} = \text{FST}_{ct} \times \text{pov}_c, \tag{21}$$

where $\text{FST}_{ct} = e_{ct}/p_c$ is the take-up rate (enrollees as a share of the poor) and $\text{pov}_c = p_c/\text{pop}_c$ is the 1970 poverty rate when studying the 1979 reform (or 1960 poverty rate, for the roll-out).

Our goal is to construct race-specific enrollment rates FS_{rct} for $r \in \{w, b\}$ (white and non-white). The strategy proceeds in three steps.

Step 1: Estimate the Relative Take-up Ratio δ. Using the PSID, we calculate national race-specific take-up rates annually from 1970 through 1981 (Appendix Figure A34, panel A). These are defined as the number of PSID households of race r enrolled in food stamps divided by the number of PSID households of race r with income below the census needs standard.[48] We define the relative take-up ratio as

$$\delta_t = \frac{\text{FST}_{bt}}{\text{FST}_{wt}}. \tag{22}$$

Since δ_t is approximately constant over time (Appendix Figure A34, panel b), we use its average value of $\delta = 1.6$.

Step 2: Approximate Race-specific County Poverty Counts. We approximate the race-specific county population in poverty as

$$p_{cr} = \text{pov}_{s(c)r} \times \text{pop}_{cr}, \tag{23}$$

where $\text{pov}_{s(c)r}$ is the state-level race-specific poverty rate from the census and pop_{cr} is the race-specific county population, calculated using county-level shares of population by race group and county population from the census.

Step 3: Back Out Race-specific Take-up and Enrollment Rates. The aggregate county-year

[48] The census needs standard is a year-specific poverty threshold established by the Census Bureau, based on sex of head of household, family size, number of children, and age of householder.

take-up rate can be written as a weighted average of race-specific take-up rates:

$$\text{FST}_{ct} = \frac{\text{FST}_{cbt}\, p_{cb} + \text{FST}_{cwt}\, p_{cw}}{p_{cb} + p_{cw}}. \tag{24}$$

Substituting $\text{FST}_{cbt} = \delta\, \text{FST}_{cwt}$ and solving yields

$$\text{FST}_{cwt} = \text{FST}_{ct} \cdot \frac{p_{cb} + p_{cw}}{\delta\, p_{cb} + p_{cw}}, \tag{25}$$

$$\text{FST}_{cbt} = \text{FST}_{ct} \cdot \frac{p_{cb} + p_{cw}}{p_{cb} + p_{cw}/\delta}. \tag{26}$$

Finally, race-specific county-year enrollment rates are

$$\text{FS}_{rct} = \text{FST}_{rct} \times \text{pov}_{cr}. \tag{27}$$

C.2 Freyaldenhoven et al. (2019) Correction

To address possible pre-trends, we use the proxy approach in Freyaldenhoven et al. (2019). This requires taking an explicit stance on the unboserved confound that is the source of pre-trends, and then identifying a proxy variable that follows the same time path as the unobserved confound producing the pre-trends but which is itself unaffected by the policy change.

As noted in the main text, a possible source of differential pre-trends is that food stamp enrollment rates are highly counter-cyclical (see Appendix Figure A11) and counties with different exposure may be experiencing different local business cycles. We therefore use as a proxy variable p_{ct} employment in industries without many workers on food stamps. Employment in such *unexposed* industries can proxy for the role of local business cycles but is plausibly unaffected by the purchase requirement reform (which can affect employment and earnings among food-stamp eligible individuals (Hoynes and Schanzenbach, 2012)). The Freyaldenhoven et al. (2019) procedure adjusts the focal variable — here, the first-stage outcome of food stamp enrollment — using leads of the proxy as instruments. Graphically, this procedure corresponds to subtracting the proxy from the focal variable. The identifying assumption is that absent the food stamp rollout, food stamp enrollment would move proportionally with employment in unexposed industries, and the rollout does not affect unexposed industries' employment. This assumption is supported if pre-trends in enrollment follow the same time pattern of unexposed employment.

We construct this proxy variable using data from County Business Patterns, specifically the datasets containing employment counts by Eckert et al. (2020) and Eckert et al. (2022).[49] We first identify the industries likely to be highly exposed to the food stamp reform. We follow Dube et al. (2010) in identifying low-wage sectors as retail, food services, and accommodations, and we

[49] For confidentiality reasons, employment counts are masked for many county-industry-years. In Eckert et al. (2020), the authors describe the process they used to impute masked employment counts.

sum county-level employment across these sectors for each year. We also add employment in agriculture, forestry, and fisheries because these are important low-wage sectors in the U.S. south. We then compute unexposed employment, which we use to proxy for macroeconomic trends, as employment across all industries, less employment across these exposed, low-wage industries. We also use employment across all industries as a secondary proxy; the identifying assumption could hold approximately in the broader industry sample if labor supply responses to food stamps are small as a share of aggregate employment.

Appendix Figure A19 presents the results, following the template in Freyaldenhoven et al. (2019). Panel A plots the event study in the proxy variable, running the same regression as in Equation (10) with log count employed as the outcome variable . Consistent with a recovering macroeconomy, employment rises in the years leading up to 1976. Because food stamp take-up is countercyclical, this force could plausibly increase take-up in these years and thus generate pre-trends in Figure 5. Panel B runs the same event study but normalizes the proxy into the same units as the food stamp enrollment outcome, flipping the sign of log employment and rescaling. In the Freyaldenhoven et al. (2019) framework, the researcher must choose a period to line up the proxy and outcome; this period is then omitted in adjusted estimates. We normalize the series so that the proxy and outcome coincide in 1975, the year in which the pre-trend is largest. In blue we show the same enrollment rate. The figure shows that the proxy closely tracks the pre-1979 pattern in enrollment, but not the enrollment jump in 1979. Finally, Panel C shows our adjusted first-stage estimates, subtracting the rescaled proxy from food stamp enrollment. This adjustment eliminates the pre-trend while leaving the post-period effect largely unchanged. The adjusted first-stage estimate in Panel C is 0.328 (s.e. 0.050), which is economically and statistically indistinguishable from the unadjusted first-stage estimate of 0.349 (s.e. 0.027) in Figure 5. We obtain similar results using total employment as the proxy (Figure A20). In sum, macroeconomic forces account for the modest pre-trend in the first stage, but not for the jump in 1979.

We perform the same analysis using both proxies for food stamps spending in Figure A21 and find similar results.

C.3 Roll-Out Analysis Details

Two-way Fixed Effect The canonical way to estimate the impact of the roll-out is via two-way fixed effects (TWFE). For counties c, years t, months m, and relative year r, we estimate the first stage and reduced form:

$$x_{ct} = \alpha_c + \tau_t + \sum_{r \neq -1} \beta_r D_{cr} + \epsilon_{ct} \tag{28}$$

$$y_{cm} = \alpha_c + \tau_m + \sum_{r \neq -1} \beta_r D_{cr} + \epsilon_{cm} \tag{29}$$

where x_{ctr} is a measure of the food stamp program (e.g., enrollment rate); y_{cmr} is an outcome (e.g., birthweight); α_c is a county fixed effect (or county-by-race) and τ_t, τ_m is a year- or year-month fixed effect; and D_{cr} is an indicator if FSP was in effect in county c at relative period r.

Since all counties are eventually treated, there is a standard time-period-cohort multicollinearity problem. To identify relative time indicators β_r, we adopt the practice of binning these indicators at +5 and –5. Economically, this restriction imposes that coefficients are constant far from the reform.

To address concerns about treatment-effect heterogeneity with TWFE estimators (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021), our primary approach builds on the TWFE model and instead uses a compares treated units only to "clean" controls using a stacked estimation approach (Deshpande and Li, 2019; Wing et al., 2024).

Stacked Dataset Weights. The stacked difference-in-differences weights described by Wing et al. (2024) aim to make control/treatment observations comparable across datasets so that each dataset contributes to overall estimates proportionally to its share of treated/control.

Control and treatment weights, at the dataset d and relative year r level, are constructed as:

$$w_{dr} = \begin{cases} \dfrac{Pop_d^{\text{treat}} / Pop_{\text{tot}}^{\text{treat}}}{Pop_{dr}^{\text{treat}} / Pop_r^{\text{treat}}}, & \text{if county–time is treated,} \\ \dfrac{Pop_d^{\text{treat}} / Pop_{\text{tot}}^{\text{treat}}}{Pop_{dr}^{\text{ctrl}} / Pop_r^{\text{ctrl}}}, & \text{if county–time is control.} \end{cases} \tag{30}$$

The subscripts tot denote the total across relative years.

Wing et al. (2024) show that these weights produce a population weighted ATT. Our specific weights differ slightly from Wing et al. (2024) (Table 1, Row 2) because we incorporate both population weights and the stacking weights, so we use populations of treated or control in the denominator.

C.4 Comparison to Almond et al. (2011).

Almond et al. (2011) estimate the effect of the county-level roll-out of the food stamp program on birthweight. They find that the roll-out increased birth weight by 2.64 grams (SE = 0.896) for whites and 4.12 grams (SE = 2.32) for blacks, corresponding to an increase of roughly 8% and 13% of mean birth weight, respectively. They also estimate that roll-out reduced the share of low-birthweight births by 0.06 percentage points (SE = 0.03) for whites and 0.16 percentage points (SE = 0.1) for blacks.[50]

Our re-analysis of the impact of the roll-out on birth weight largely follows theirs, but differs in some implementation choices and also incorporates some of the advances in the econometrics

[50] See Almond et al. (2011), Table 1, columns 2 and 6.

of event studies since their paper was published. In particular, our approach differs from Almond et al. (2011) in four main ways. First, Almond et al. (2011) apply very few sample restrictions, using all county-quarters in the period 1968 through 1977, except Alaska. We limit the sample to a balanced panel of county-years in the period -2 to +3 relative to roll-out and counties that rolled out in July 1969 or later to ensure all counties in the sample have an observable pre-period in the birth weight data, which begins in 1968. We also ensure counties in the sample have available data on food stamp enrollment across years 1970–1975 to maintain a consistent sample across all of our specifications.[51] Second, Almond et al. (2011) define treatment as of three months prior to birth (as a proxy for FSP having been rolled out at the beginning of the third trimester of gestation). Instead, we assign as treatment all births after food stamp program roll-out. Third, Almond et al. (2011) use a flat panel dataset of county-quarters. We use a stacked dataset design as in Deshpande and Li (2019). Fourth, Almond et al. (2011) include variations of linear county-by-time controls, as well as other control variables, which we exclude.

To explore the consequences of our various departures, we start by using publicly available data and the replication package provided by Almond et al. (2011) (hereafter: AHS) to replicate their original results. Tables A9 and A10 display the estimates of Almond et al. (2011) in columns 1 through 4 and our corresponding replication of their estimates in columns 5 through 8, for birthweight and share low birthweight respectively. Our replication estimates closely capture the original estimates, deviating from their numbers by less than 20% (sometimes considerably less). One reason our replication attempt is not numerically identical may be small differences in the birth certificate data we use; for example, we have several counties that lack data on birthweight which could be due to the imperfect digitization of scanned birth records. Alternatively, there may also be other differences in the use and availability of certain Almond et al. (2011) control variables.

We then proceed in Tables Tables A11 and A12 to interrogate the impact of the various changes described above. In each table, columns 1 through 4 report the results of our replication attempt using the same sample and design as Almond et al. (2011), and then proceed to toggle the changes described above across the columns for birth weight and share low birthweigth outcomes. [52] Specifically, in column (5) we use the baseline AHS specification but remove all covariates except for time and county fixed effects, while in columns (6) on we limit the sample to counties that rolled out in July 1969 or later and in columns (7), (9), (11) and (13)-(17) we further limit to data in relative years -3 to +2; otherwise columns (6) through (11) vary in the set of controls used.

Columns (12) through (17) used the stacked data analysis approach from equation 14. All of these analyses are limited to counties that roll out in July 1969 or later, and all but column 12

[51] Unlike birth weight data, county-level enrollment data is only available at the year level, which is why we start in 1970 rather than July 1969.

[52] For completeness, Table A13 applies the specification of Almond et al. (2011) and performs an identical set of specification alterations to the log birthweight outcome (the log transform does not appear in Almond et al. (2011)).

further limit the data to relative years -3 to +2. Columns 12, 13, 15 and 17 use the baseline (dataset) weights we use in teh main analyses, while columns 14 and 16 use the birth count weights used by Almond et al. (2011). Columns 14 and 15 add the sample limitations we use in our bsaeline described two paragraphs above while columns 16 and 17 add both the sample limitations used in our baseline and the treatment definition used in our baseline . Column (17) replicates our baseline specification.

Looking across all the specification generally, balancing the flat panel dataset used by Almond et al. (2011) by limiting the sample to counties that rolled out July 1969 or later and to relative years -3 to +2 decreases estimated impacts relative to Almond et al. (2011). The stacked design increases impacts of the food stamp program roll-out on black births, although standard errors also grow with the Wing et al. (2024) weights. Ultimately, our baseline estimate of the impact of roll out of the food stamp program shows a statistically significant decrease in the share of low births among black births (Appendix Table A10, column 17); however, that estimate is not robust to changes in the sample or specification.

C.5 1970 Reform

We discuss our empirical analysis of the 1970 "bonus reform." In 1968, Congress modified the program to increase the subsidy starting in January 1970. As Richardson (1979) describes, though this subsidy increase was a part of the set of amendments signed into law in 1971, the administration enacted the significant changes to benefits in January 1970. Hoover (1974) shows, using benefits schedules from the UDSA, that the bonus increases were published at the end of 1969. As Figure A39 shows, the subsidy increased near the bottom of the income distribution in both the schedules North and South. For instance, a four-person household with a monthly net income of \$50–59.99 was subsidized at a rate of 69% before the reform ($\approx 44 \div (44 + 20) \times 100$), and 90% after the reform ($\approx 96 \div (96 + 10) \times 100$). Near the top of the income distribution among eligibles, the subsidy was barely changed. For the very bottom, the bonus increased (e.g., from 58 to 104 for the four-person household), but since the purchase requirement was already very small for this group, the effective subsidy change was small. Overall, the subsidy changed from around 40% before the reform to about 65% after the reform (Figure A39).

The bonus reform occurred at a national level. To study the impact on take-up, we use an exposure design similar to Section 3. We modify Equation (8) to form exposure as follows:

$$\widetilde{\text{Exposure}}_c^z := \text{EligibilityRate}_c^{1960} - \text{EnrollmentRate}_c^z \tag{31}$$

$$:= \frac{\text{number of people in poverty 1960}}{\text{number of people in county in 1960}} - \frac{\text{number of people on FSP in year } z}{\text{number of people in county in 1960}} \tag{32}$$

and then estimate the same first-stage equation as in Equation (10).

This approach has several complications relative to Section 3. First, the reform occurred in a period during the food stamps roll-out. To address this issue, we restrict to counties that had rolled out the program by 1967 or earlier and augment Equation (10) with fixed effects for the roll-out date (specifically, years-since-rollout-by-calendar year fixed effects) and alter the baseline year to be 1969. Second, direct comparison to the 1979 elimination of the purchase requirement is imperfect since the subsidy changes in both reforms differ in terms of the exposed income groups. Our estimating equations are:

$$x_{ct} = \sum_{s \neq 1969} \delta_s \left(D_t^s \times \text{Exposure}_c^z \right) + \alpha_c^1 + \gamma_t^1 + \lambda_{o(c)t}^1 + u_{ct} \tag{33}$$

$$y_{ct} = \sum_{s \neq 1969} \beta_s \left(D_t^s \times \text{Exposure}_c^z \right) + \alpha_c^2 + \gamma_t^2 + \lambda_{o(c)t}^2 + \varepsilon_{ct}, \tag{34}$$

where $\lambda_{o(c)t}$ denotes county's rollout year by calendar year fixed effects.

Results. We first present visual evidence in Figure A40, akin to Figure 4 but with an extended pre-period. We see that all four quartiles of exposure have a jump in take-up in 1970. Consistent with a take-up response, a moderate gap emerges between the top three quartiles and the first quartile that was not present before the reform. On the same graph, the response to the 1979 reform based on this modified exposure measure appears much larger. However, because these graphs do not control for the date of roll-out, they must be treated with particular caution.

Event study graphs back up the descriptive evidence (Figure A41). In the years between 1965 and 1967, we see a relatively flat pre-trend. In 1970 and after, take-up grows and remains constant. Since moving from the 10th to the 90th percentile of exposure is about 0.3 units, this means that this difference in exposure would yield an increase of enrollment of about 2.9 percentage points ($\approx 0.0955 \times 0.3 \times 100$). For welfare, we care about the average impact, which we form by multiplying the average exposure of 0.106 by the average impact, giving a take-up response of 1 percentage point ($\approx 0.0955 \times 0.106 \times 100$). Notably, controlling for fixed effects in roll-out is required to correct for pre-trends trends in take-up in these counties (Panel C).

D Empirical Welfare Analysis

D.1 Fiscal Externalities on Earnings and Employment

Calibration. We calibrate the likely magnitude of the fiscal externality from food stamp enrollment on lost tax revenues via a food-stamp-induced labor supply reduction. Let $\psi = \frac{d \ln E}{d \ln I} > 0$ denote the elasticity of earnings (E) with respect to income (I) and let $\bar{\tau}$ denote the average tax-and-transfer rate applied to earnings changes. For households who take up the program, the income

effect on earnings from receiving $1 in food-stamp benefits is

$$\text{FE}_\theta = \bar{\tau}\psi\frac{E}{I}V_\theta \tag{35}$$

This equation assumes that income effects are scaled by how much recipients value food stamps.

An upper-bound estimate of the income effect $\psi = 0.39$ comes from Vivalt et al. (2024)'s Universal Basic income Experiment, which is similar to Golosov et al. (2024)'s estimate of $\psi = 0.31$ for Americans in the bottom income quartile using lottery winners. Other studies find smaller estimates of $\psi \in [0, 0.1]$ (Marinescu, 2018). We use an estimate of federal and state tax and transfer rate of $\bar{\tau} = 0.129$, from Hendren and Sprung-Keyser (2020) who use this constant rate across both contemporary and historical reforms. We also assume $\frac{E}{I} = 1$, that is, low-income households' income equals their earnings. Putting these numbers together, we have

$$\text{FE}_\theta = 0.129 \cdot 0.39 \cdot 1 \cdot V_\theta \approx 0.05V_\theta. \tag{36}$$

This means that negative fiscal externalities are bounded above by 5 cents per dollar of food stamps spent. Note that even if $\phi = 1$, the fiscal externality would only be 0.129, i.e the tax and transfer rate. Moreover, there are possibly other positive fiscal externalities (e.g. on the adult earnings of children enrolled in food stamps as in Bailey et al. (2024)).

D.2 Identification of Valuation

In this section, we formalize how variation in R identifies the valuation of food stamps. To lighten notation with identification arguments, we assume no behavioral biases distort choices from true (normative) preferences; allowing behavioral biases would simply mean that households optimize perceived utility ($\tilde{u}_\theta$) rather than true (normative) utility (u_θ), and valuation parameters would reflect perceived willingness to pay/equivalent variation as in Equation (2).

D.2.1 Willingness to Pay

Preferences and Constraints. Types $\theta \sim G$ have preferences $u_\theta(m, f)$ over consumption of a numeraire good m and food f. They face the budget constraint $m + f \leq I_\theta$, where I_θ are cash endowments and we normalize the price of food to 1.

The Food Stamp Program (FSP) has two parameters: (i) a required cash payment $R \geq 0$ (the purchase requirement under the pre-1979 program), and (ii) a government bonus $F > 0$.

Under the purchase requirement (PR), an enrollee pays R in cash and receives food stamps with face value $R + F$. After the PR is eliminated, an enrollee pays nothing and receives food stamps with face value F.

Enrolling entails a fixed enrollment cost $k_\theta \geq 0$. Using stamps may also involve stigma or transaction costs, where for each dollar of stamp face value redeemed, the household loses $\psi_\theta \geq 0$ dollars of the numeraire. The value $\psi_\theta = 0$ means there are no stigma or transaction costs.

Indirect Utilities. Assume households exhaust any food stamps they receive. If households do not take up the Food Stamp Program, indirect utility is

$$U_\theta^0(I_\theta) \equiv \max_{m,f \geq 0} u_\theta(m,f) \quad \text{s.t.} \quad m + f \leq I_\theta. \tag{37}$$

If she enrolls in the FSP with purchase requirement R, the household pays R in cash and receives $R + F$ dollars of stamps. She pays an enrollment cost of k_θ. Redeeming stamps of face value $R + F$ entails a loss $\psi_\theta(R + F)$. Therefore, indirect utility if households take up is

$$U_\theta^{\text{FSP}}(I_\theta; F, R) \equiv \max_{m,f \geq 0} \{u_\theta\big(m, f + (R + F)\big) - k_\theta\} \quad \text{s.t.} \quad m + f \leq I_\theta - R - \psi_\theta(R + F). \tag{38}$$

This expression nests both the PR and no-PR cases, as $U_\theta^{\text{FSP}}(I_\theta; F, 0)$ is the utility under the modern FSP with no purchase requirement.

Define willingness-to-pay for F in food stamps as the cash amount $V_\theta(R + F, R)$ that makes the household indifferent between not enrolling and enrolling under the purchase requirement with bonus $R + F$ and purchase requirement R:

$$U_\theta^0(I_\theta) = U_\theta^{\text{FSP}}(I_\theta - \widetilde{V}_\theta(R + F, R); F, R). \tag{39}$$

Further define normalized willingness-to-pay for the no-purchase requirement contract as

$$V_\theta \equiv \frac{\widetilde{V}_\theta(F, 0)}{F}. \tag{40}$$

The parameter $\widetilde{V}_\theta(R + F, R)$ is a notion of equivalent variation in absolute terms, comparing a world in which the household must exchange R in cash for an $R + F$ FSP bundle to a world where she is not allowed to. The parameter V_θ is a notion of equivalent variation *per dollar*, comparing a world in which the household must take up F in FSP for free (no purchase requirement) to a world where she is not allowed to. In other words, V_θ captures a household's willingness to pay for \$1 in food stamps, fixing that she does not also need to exchange cash for stamps as a part of the FSP.

D.2.2 Prices, Demand, and Identification

Next, we derive conditions under which take-up rates under different purchase-requirement regimes reveal the private value of food stamps.

More formally, households take up food stamps if $U_\theta^{\text{PR}} \geq U_\theta^0$. For enrolled households, this means that $\widetilde{V}_\theta(R+F) \geq R$; in words, households who enroll reveal that they value receiving $R+F$ in food stamps at least as much as R in cash. Let the per-dollar "demand curve" for food stamps be defined as

$$Q(p) \equiv \Pr(V_\theta \geq p). \tag{41}$$

We seek conditions under which PR variation reveals the per-dollar valuation of food stamps. That is, we seek conditions under which the following equations hold:

$$\Pr(\widetilde{V}_\theta(R+F,R) \geq R) = \Pr(V_\theta \geq p) \equiv Q(p). \tag{42}$$

With a fixed enrollment cost $k_\theta > 0$, the expression does not obtain automatically. Intuitively, fixed costs can be amortized into different per-dollar valuations as the magnitude of food stamps F changes. For instance, if a household is willing to pay $R = \$80$ to receive $R+F = \$200$ of food stamps, they may not be willing to pay $pF = \frac{80}{200} \times 100 = \$40$ to receive $F = \$120$ of FSP if fixed costs of enrollment are large enough.[53] However, a sufficient condition to generate this demand curve for food stamps is "proportional valuation."

Assumption 1 (Proportional valuation)**.** *Willingness-to-pay for food stamps scales proportionally with face value over the range induced by variation in the purchase requirement:*

$$\widetilde{V}_\theta(R+F,R) = \frac{R+F}{F}\,\widetilde{V}_\theta(F,0), \qquad \textit{for all } R \in [0, R_{\max}]. \tag{43}$$

To see why this assumption is sufficient, Equation (40) implies that proportional valuation is equivalent to assuming that $\frac{\widetilde{V}_\theta(R+F,R)}{R+F} = V_\theta$. Then, PR enrollment satisfies

$$\widetilde{V}_\theta(R+F,R) \geq R \iff V_\theta \geq \frac{R}{R+F} = p, \tag{44}$$

so that variation in R identifies $Q(p) = \Pr(V_\theta \geq p)$.

Assumption 1 is a strong restriction, but it arises under several simple environments. We highlight three cases in Appendix D. In the first two cases, all enrollment costs are variable, that is, $\psi_\theta > 0$ but $k_\theta = 0$. These cases could arise if households pick up stamps each week before going to the grocery store. The cases then differ in whether households always consume more than $R+F$ in food stamps ("strong inframarginality") or instead have a constant marginal rate of substitution between food and the numeraire. In the third case, we permit fixed take-up costs but derive parametric restrictions on utility such that proportional value still holds.

Identification From Price Variation. Under proportional valuation, variation in the purchase

[53] To see that this example is equivalent to Equation (42), note that the RHS of Equation (42) can be rewritten as $\Pr(\widetilde{V}_\theta \geq p) = \Pr(V(F,0) \geq pF)$.

requirement R induces variation in $p = \frac{R}{R+F}$ and traces out the demand curve $Q(p)$. The elimination of PR corresponds to $R = 0$ (so $p = 0$), identifying demand at zero price. More generally, tracing out $Q(p)$ identifies the distribution of V_θ.

If Assumption 1 fails, what are we identifying? We have variation in R, which lets us identify the curve $\Pr(\widetilde{V}(R+F, R) \geq R)$. This is still a legitimate welfare object, corresponding to willingness to pay for the real-world purchase-requirement contract. It is just harder to interpret since the FSP bundle changes with R. Therefore, we maintain Assumption 1, but failure of this assumption does not invalidate the exercise.

D.3 Microfoundations for Proportional Value Assumption

We highlight three leading cases that generate Assumption 1. In each of these cases, we assume inframarginality holds.

Assumption 2 (Inframarginality)**.** *Absent the program, food consumption is "inframarginal" in that food consumption weakly exceeds F (but not necessarily $R + F$). That is, $f^0 \geq F$, where*

$$f^0 = \arg\max_{f \geq 0} u_\theta(m, f) \quad s.t. \quad m + f \leq I_\theta. \tag{45}$$

(a) **Strong Inframarginality, No Fixed Cost.** Assume $k_\theta = 0$ and that, absent the program, baseline food consumption satisfies a "strong inframarginality" condition

$$f^0_\theta \geq R + F \quad \text{for all } R \leq R_{\max}. \tag{46}$$

Stamp dollars are then inframarginal and replace cash-financed food one-for-one, so

$$\widetilde{V}_\theta(F + R, R) = (1 - \psi_\theta)(R + F) \tag{47}$$

$$= \frac{R+F}{F} \widetilde{V}_\theta(F, 0), \tag{48}$$

thus satisfying proportional valuation.

(b) **Linear Marginal Value on the Relevant Range, No Fixed Cost.** Assume $k_\theta = 0$ and quasi-linear utility $u_\theta(m, f) = m + v_\theta(f)$, where $v_\theta(\cdot)$ is increasing and linear on the range of food levels that can arise under PR:

$$v_\theta(f) = \beta_0 f \quad \text{for all } f \in [0, F + R_{\max}], \qquad \beta_0 \geq 0.$$

Strong inframarginality need not hold. With per-dollar stamp-use loss ψ_θ,

$$\widetilde{V}_\theta(R+F,R) = (\beta_0 - \psi_\theta)(R+F) \tag{49}$$

$$= \frac{R+F}{F}\widetilde{V}_\theta(F,0). \tag{50}$$

(c) **Fixed Enrollment Cost With Offsetting Non-Inframarginality.** Allow $k_\theta > 0$ and suppose that, on the relevant support of R, stamps are non-strongly inframarginal:

$$R + F \geq f_\theta^0 \geq F.$$

A sufficient microfoundation is quasi-linear utility $u_\theta(m,f) = m + v_\theta(f)$, where $v_\theta(\cdot)$ has weakly decreasing marginal utility satisfying

$$v'_\theta(f) = \begin{cases} \psi_\theta + x + \dfrac{k_\theta}{F}, & f \in (0,F], \\ \psi_\theta + x, & f \in (F, F+R_{\max}], \end{cases}$$

for $x > 0$ with any concave extension elsewhere. Then for all $R \in [0, R_{\max}]$,

$$\widetilde{V}_\theta(R+F,R) = v_\theta(R+F) - \psi_\theta(R+F) - k_\theta \tag{51}$$

$$= \left(\psi_\theta + x + \frac{k_\theta}{F}\right)F + (\psi_\theta + x)R - \psi_\theta(R+F) - k_\theta \tag{52}$$

$$= x(R+F) \tag{53}$$

while

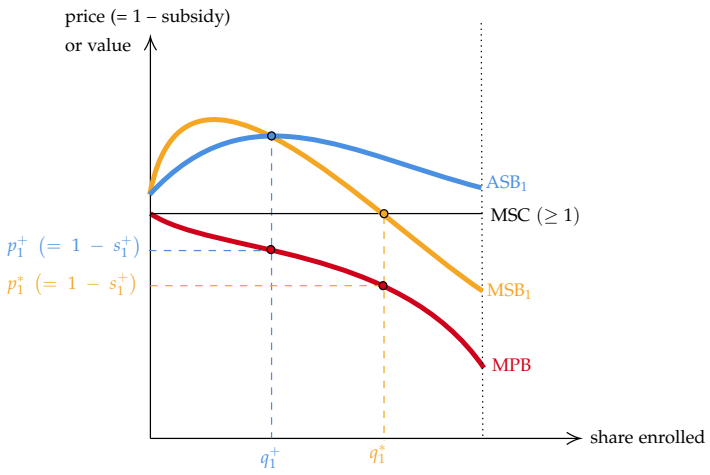
$$\widetilde{V}_\theta(F,0) = v_\theta(F) - \psi_\theta F - k_\theta = xF \tag{54}$$

so proportional valuation holds despite the fixed enrollment cost. Intuitively, this case describes the situation in which decreasing marginal utility of food beyond $R+F$ exactly offsets the fact that getting more food stamps amortizes the fixed costs of enrollment.

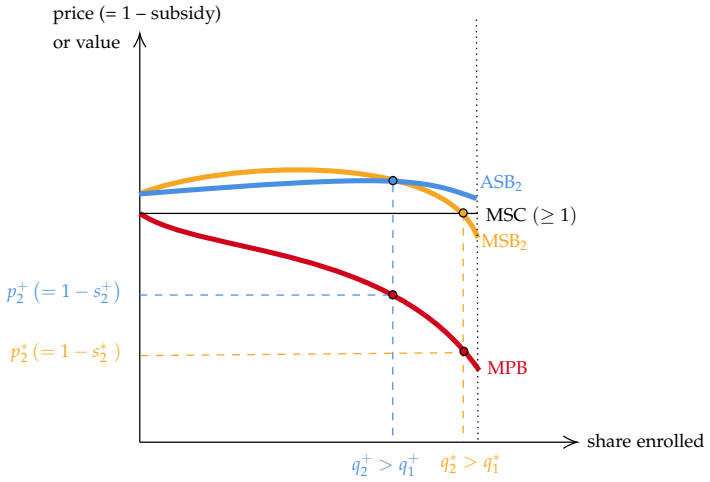
E Appendix Figures

Figure A1: Private versus Social Value

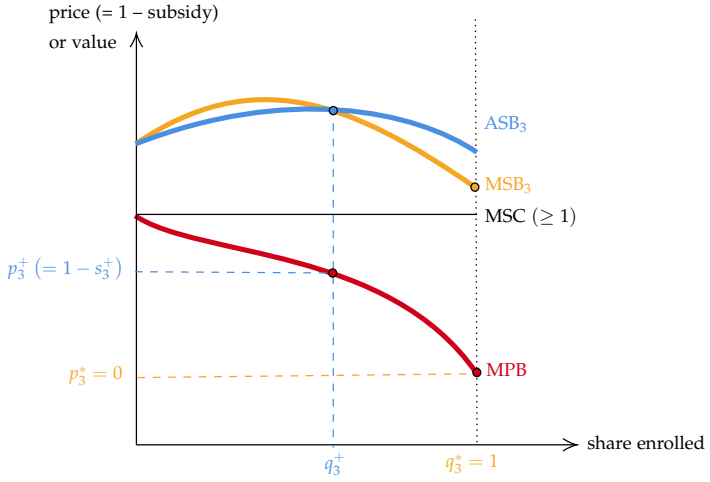
(a) Efficient and cost-effective quantities (< 1)



(b) Shifting out of efficient and cost-effective quantities (< 1)



(c) Efficient quantity $= 1$, cost-effective quantity < 1



(d) Efficient and cost-effective quantity $= 1$

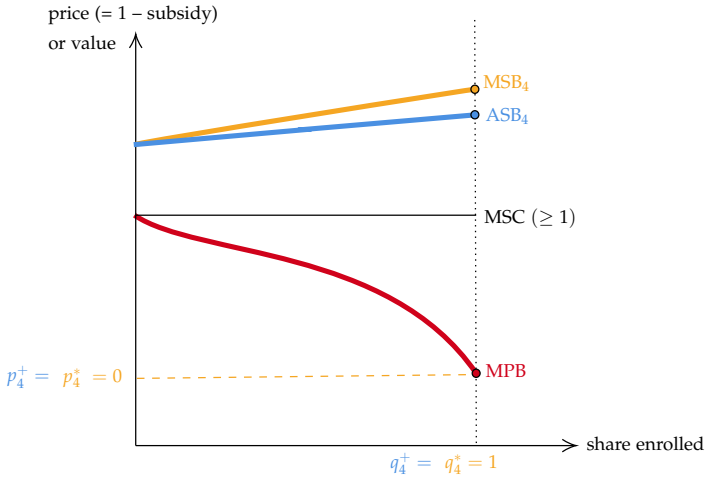


Figure A2: Benefits Table, January 1978 (Pre-EPR)

Monthly coupon allotments and purchase requirements—48 States and the District of Columbia

Monthly net income	For a household of—							
	1 person	2 persons	3 persons	4 persons	5 persons	6 persons	7 persons	8 persons
	The monthly coupon allotment is—							
	$52	$96	$138	$174	$206	$248	$274	$314
	And the monthly purchase requirement is—							
$0 to $19.99	0	0	0	0	0	0	0	0
$20 to $29.99	$1	$1	0	0	0	0	0	0
$30 to $39.99	4	4	$4	$4	$5	$5	$5	$5
$40 to $49.99	6	7	7	7	8	8	8	8
$50 to $59.99	8	10	10	10	11	11	12	12
$60 to $69.99	10	12	13	13	14	14	15	16
$70 to $79.99	12	15	16	16	17	17	18	19
$80 to $89.99	14	18	19	19	20	21	21	22
$90 to $99.99	16	21	21	22	23	24	25	26
$100 to $109.99	18	23	24	25	26	27	28	29
$110 to $119.99	21	26	27	28	29	31	32	33
$120 to $129.99	24	29	30	31	33	34	35	36
$130 to $139.99	27	32	33	34	36	37	38	39
$140 to $149.99	30	35	36	37	39	40	41	42
$150 to $169.99	33	38	40	41	42	43	44	45
$170 to $189.99	39	44	46	47	48	49	50	51
$190 to $209.99	40	50	52	53	54	55	56	57
$210 to $229.99	42	56	58	59	60	61	62	63
$230 to $249.99	42	62	64	65	66	67	68	69
$250 to $269.99	42	68	70	71	72	73	74	75
$270 to $289.99		74	76	77	78	79	80	81
$290 to $309.99		76	82	83	84	85	86	87
$310 to $329.99		76	88	89	90	91	92	93
$330 to $359.99		76	94	95	96	97	98	99
$360 to $389.99			103	104	105	106	107	108
$390 to $419.99			112	113	114	115	116	117
$420 to $449.99			120	122	123	124	125	126
$450 to $479.99			120	131	132	133	134	135
$480 to $509.99				140	141	142	143	144
$510 to $539.99				149	150	151	152	153
$540 to $569.99				150	159	160	161	162
$570 to $599.99				150	168	169	170	171
$600 to $629.99					177	178	179	180
$630 to $659.99					178	187	188	189
$660 to $689.99					178	196	197	198
$690 to $719.99						205	206	207
$720 to $749.99						214	215	216
$750 to $779.99						216	224	225
$780 to $809.99						216	233	234
$810 to $839.99						216	238	243
$840 to $869.99							238	252
$870 to $899.99							238	261
$900 to $929.99							238	270
$930 to $959.99								274
$960 to $989.99								274
$990 to $1,019.99								274
$1,020 to $1,049.99								274

Notes: Congress passed a new national benefits schedule every 6 months to index benefits to food price inflation. This schedule took effect in January, 1978. Every household of the same size received the same value stamps allotment, but higher income households paid higher purchase requirements for that stamps allotment. Thus, higher income households received smaller bonuses (i.e. the actual benefit value/transfer from the government), because the bonus is the allotment, at the top of the table, minus the household's purchase requirement, which is based on household income bracket. This table was screenshotted from Federal Register Vol 42 Iss 215. in dropbox, downloaded data, benefit schedules

Figure A3: Benefits Table, January 1979 (Post-EPR)

January 1, 1979 - Basis of Coupon Issuance - 1964 Act
48 States and District of Columbia

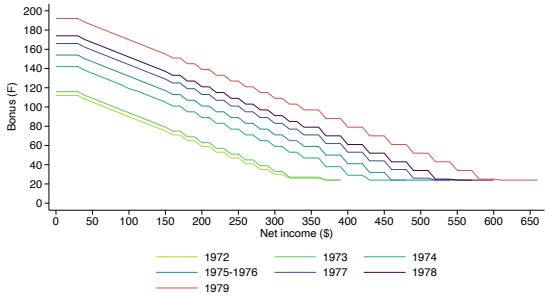
TABLE I

Monthly Net Income	Bonus by Household Size							
	One Person	Two Persons	Three Persons	Four Persons	Five Persons	Six Persons	Seven Persons	Eight Persons
0 - 19.99	58.00	106.00	152.00	192.00	228.00	274.00	302.00	346.00
20 - 29.99	57.00	105.00	152.00	192.00	228.00	274.00	302.00	346.00
30 - 39.99	54.00	102.00	148.00	188.00	223.00	269.00	297.00	341.00
40 - 49.99	52.00	99.00	145.00	185.00	220.00	266.00	294.00	338.00
50 - 59.99	50.00	96.00	142.00	182.00	217.00	263.00	290.00	334.00
60 - 69.99	48.00	94.00	139.00	179.00	214.00	260.00	287.00	330.00
70 - 79.99	46.00	91.00	136.00	176.00	211.00	257.00	284.00	327.00
80 - 89.99	44.00	88.00	133.00	173.00	208.00	253.00	281.00	324.00
90 - 99.99	42.00	85.00	131.00	170.00	205.00	250.00	277.00	320.00
100 - 109.99	40.00	83.00	128.00	167.00	202.00	247.00	274.00	317.00
110 - 119.99	37.00	80.00	125.00	164.00	199.00	243.00	270.00	313.00
120 - 129.99	34.00	77.00	122.00	161.00	195.00	240.00	267.00	310.00
130 - 139.99	31.00	74.00	119.00	158.00	192.00	237.00	264.00	307.00
140 - 149.99	28.00	71.00	116.00	155.00	189.00	234.00	261.00	304.00
150 - 169.99	25.00	68.00	112.00	151.00	186.00	231.00	258.00	301.00
170 - 189.99	19.00	62.00	106.00	145.00	180.00	225.00	252.00	295.00
190 - 209.99	13.00	56.00	100.00	139.00	174.00	219.00	246.00	289.00
210 - 229.99	10.00	50.00	94.00	133.00	168.00	213.00	240.00	283.00
230 - 249.99	10.00	44.00	88.00	127.00	162.00	207.00	234.00	277.00
250 - 269.99	10.00	38.00	82.00	121.00	156.00	201.00	228.00	271.00
270 - 289.99	10.00	32.00	76.00	115.00	150.00	195.00	222.00	265.00
290 - 309.99		26.00	70.00	109.00	144.00	189.00	216.00	259.00
310 - 329.99		20.00	64.00	103.00	138.00	183.00	210.00	253.00
330 - 359.99		20.00	58.00	97.00	132.00	177.00	204.00	247.00
360 - 389.99		20.00	49.00	88.00	123.00	168.00	195.00	238.00
390 - 419.99			40.00	79.00	114.00	159.00	186.00	229.00
420 - 449.99			31.00	70.00	105.00	150.00	177.00	220.00
450 - 479.99			22.00	61.00	96.00	141.00	168.00	211.00
480 - 509.99			18.00	52.00	87.00	132.00	159.00	202.00
510 - 539.99				43.00	78.00	123.00	150.00	193.00
540 - 569.99				34.00	69.00	114.00	141.00	184.00
570 - 599.99				25.00	60.00	105.00	132.00	175.00
600 - 629.99				24.00	51.00	96.00	123.00	166.00
630 - 659.99				24.00	42.00	87.00	114.00	157.00
660 - 689.99					33.00	78.00	105.00	148.00
690 - 719.99					28.00	69.00	96.00	139.00
720 - 749.99					28.00	60.00	87.00	130.00
750 - 779.99					28.00	51.00	78.00	121.00
780 - 809.99						42.00	69.00	112.00
810 - 839.99						33.00	60.00	103.00
840 - 869.99						32.00	51.00	94.00
870 - 899.99						32.00	42.00	85.00
900 - 929.99						32.00	36.00	76.00
930 - 959.99							36.00	67.00
960 - 989.99							36.00	58.00
990 - 1,019.99							36.00	49.00
1,020 - 1,049.99								40.00
1,050 - 1,079.99								40.00
1,080 - 1,109.99								40.00
1,110 - 1,139.99								40.00
1,140 - and up								40.00

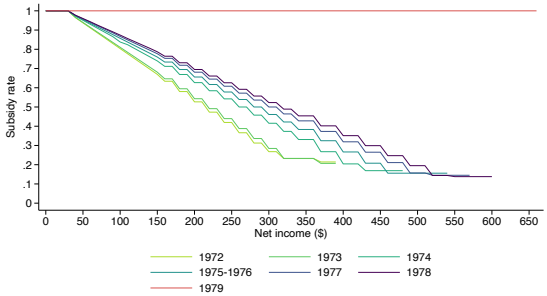
Notes: Congress passed a new national benefits schedule every 6 months to index benefits to food price inflation. This schedule took effect in January, 1979. After the elimination of the purchase requirement (EPR), the dollar value of food stamps a household received could be read off after knowing the household's size and income bracket. The benefit amount a household received was roughly equivalent to that household's bonus (allotment minus purchase requirement) under the purchase requirement system (see Figure A2). This table was screenshotted from Federal Register Vol 43 Iss 225. in dropbox, downloaded data, benefit schedules

Figure A4: Benefit schedules transfers and subsidy rate

(a) Food Stamp Allotment (F)

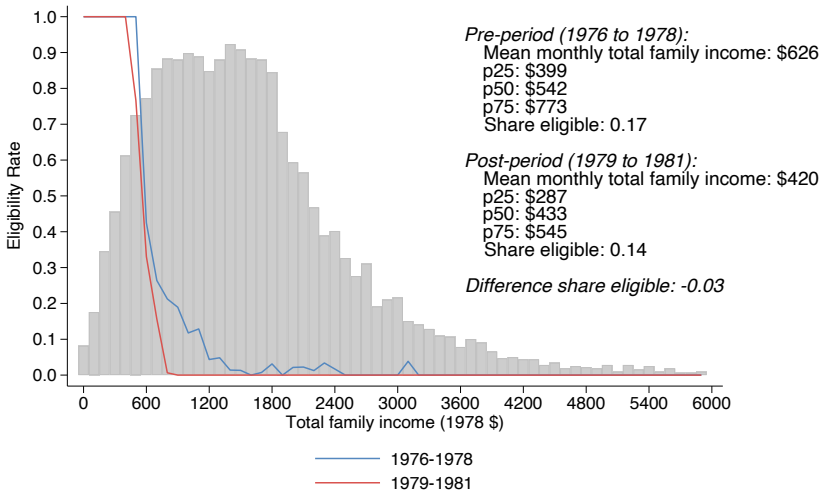


(b) Food Stamp Subsidy rate (F/F+R)



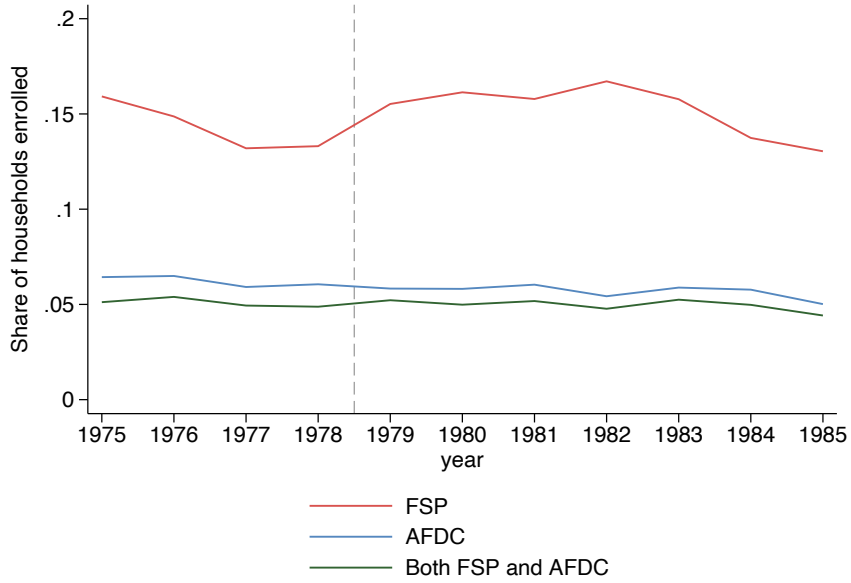
Notes: Panel (a) shows the bonus/transfer, in dollars, across benefits schedules in years 1972-1979 for a family of four. Panel (b) shows the subsidy rate across these years. Income bins have not been adjusted and thus the figures reflect the benefits received by a family of four within each income bin in terms of nominal dollars. Note that each point represents a $10-wide net income bin. For example, in panel (a), the point at net income $300 shows the bonus, in dollars, received by a family of four with monthly net income between $290.00-$299.99. Food stamp bonus schedules by net income are constructed from annual allotment tables published by the U.S. Department of Agriculture's Food and Nutrition Service in the Federal Register for 1971–1974 and 1976–1979 (see Food and Nutrition Service, USDA (1971, 1972, 1973, 1974, 1976, 1978, 1979)), supplemented by United States Food and Nutrition Service (1975), Longen (1980), and California Department of Social Services (1980).

Figure A5: Simulated Food Stamps eligibility across income bins in PSID



Notes: Figure simulates FSP eligibility based upon household-level income data in the 1976-1981 PSID for four-person households and the 1978 and 1979 FSP eligibility schemes (in terms of deductions) as well as the maximum allowable monthly net income in each year. In years 1976-1978, households are deemed eligible through positive AFDC receipt or net income below the year-specific FSP maximum allowable monthly net income threshold (after simulated wage-based deductions). In years 1979-1981, households are deemed eligible if net income is below the year-specific FSP maximum allowable monthly net income threshold after the standard and wage-based deductions. Figure shows the share eligible across $100-wide income bins in terms of 1978 dollars. For example, the point at net income $600 shows the share of 1976–1981 four-person household-years with total family income (in 1978 dollars) in the range $600-$699.99 that are eligible for FSP based upon net income calculated from total family income in the PSID and wage-based deductions. Income is reported as an annual measure, so monthly income is calculated by dividing annual income by 12. The gray bars in the background of the figure depict the shares of 1976–1981 four-person household-years in each total family income bin (1978 dollars). Note that the labeled y-axis only describes the eligibility rates, and the bars are not on the same scale. The summary statistics written in the graph region describe the total family income (1978 dollars) of household-years in the pre/post period. Note that the share of eligible households falls slightly between the periods.

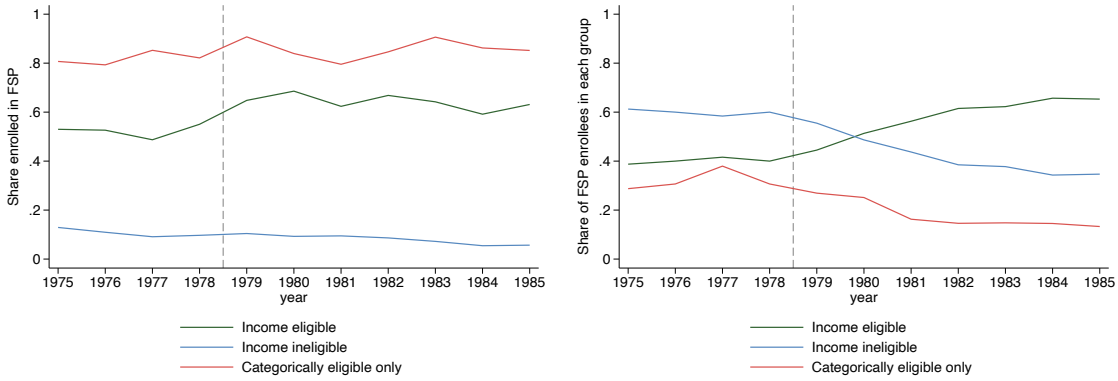
Figure A6: Share of households enrolled in FSP, AFDC



Notes: Figure shows the share of all households in the PSID with FSP transfers greater than 0, AFDC transfers greater than 0, and FSP and AFDC transfers greater than zero.

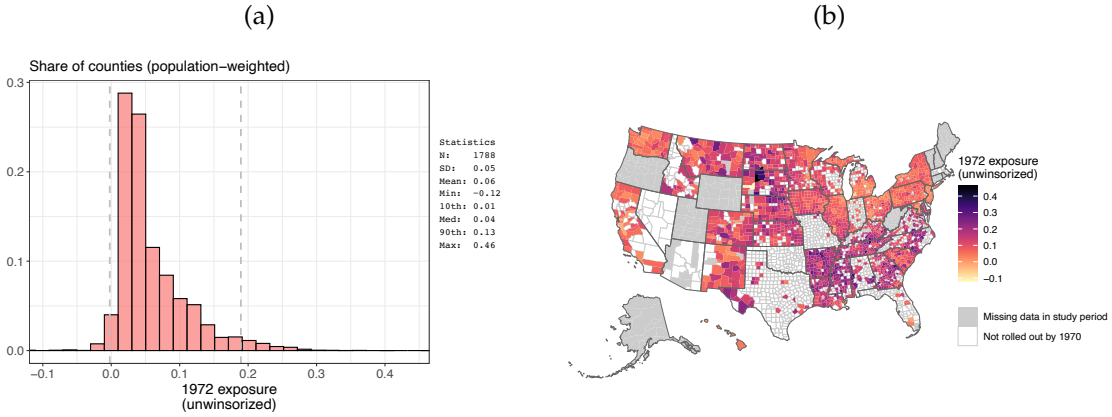
Figure A7: Take-up and FSP enrollee composition by simulated eligibility group

(a) P(FSP = 1 | Group x) (b) P(Group x | FSP = 1)



Notes: Figure uses simulated FSP eligibility based upon household-level income data in the 1976-1981 PSID for 4-person households and the 1978 FSP eligibility scheme (in terms of deductions and AFDC categorical eligibility), as well as the maximum allowable monthly net income in 1978. Specifically, to simulate eligibility, we calculate monthly net income (in 1978 dollars) by subtracting an earned income deduction according to the 1978 FSP eligibility deductions (10% of wages from labor up to a maximum of $30) from total family income. Then, we designate households with monthly net income below the maximum allowable monthly net income for 1978 FSP eligibility (¡$600) as "income eligible", and households with net income above this threshold as "income ineligible." Additionally, we designate households with positive AFDC transfers and with net income above the threshold as "categorically eligible only." We focus on 4-person households to be able to simulate eligibility using the maximum allowable monthly income that varies by household size. Panel (a) shows the FSP-enrolled share of 4-person households in the PSID that fall into each group. Panel (b) shows the share of FSP-enrolled 4-person households in the PSID that fall into each group.

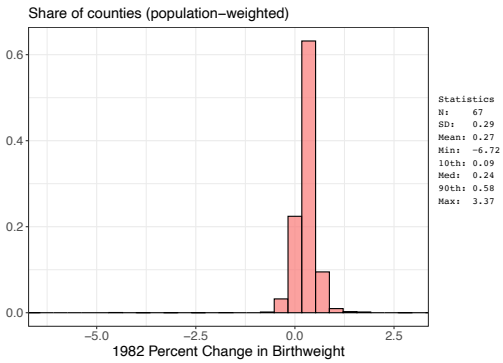
Figure A8: Variation in Unwinsorized Exposure



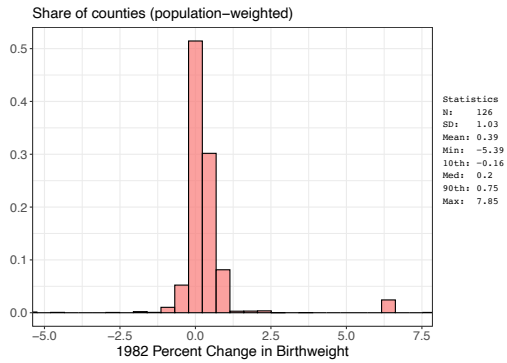
Notes: Figure shows a histogram of exposure across (population-weighted) counties (panel a) and a heat map of exposure across counties (panel b). Exposure is measured based on 1972 FSP enrollment counts, and analysis is limited to the counties that had rolled out their food stamp program by 1970. The exposure measure is unwinsorized. The baseline winsorized exposure measure is censored to the population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, $\pm 3.2\%$ as indicated by the vertical dashed lines in panel (a).

Figure A9: Distribution of 1982 percent change in birthweight by quartile of 1972 exposure measure

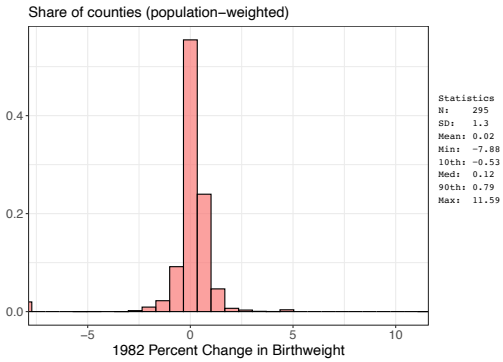
(a) Quartile 1 (lowest exposure)



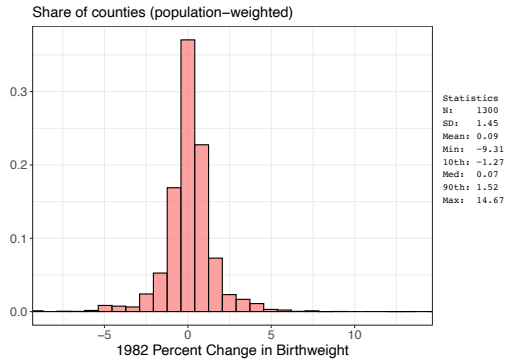
(b) Quartile 2



(c) Quartile 3



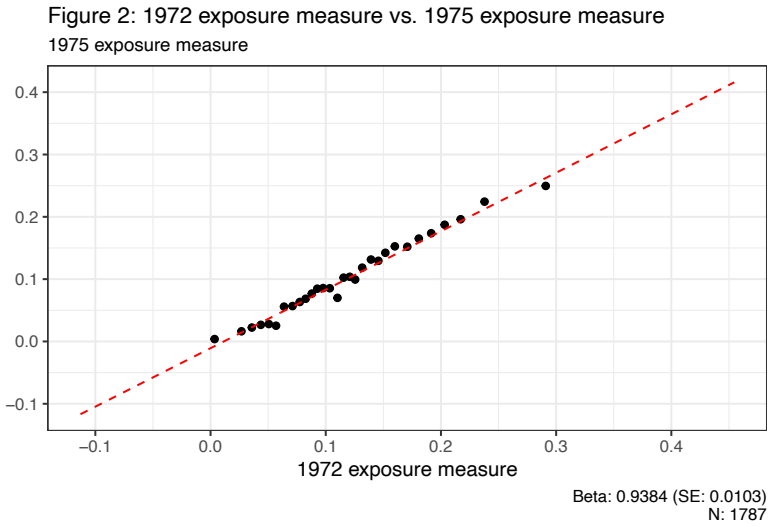
(d) Quartile 4 (highest exposure)



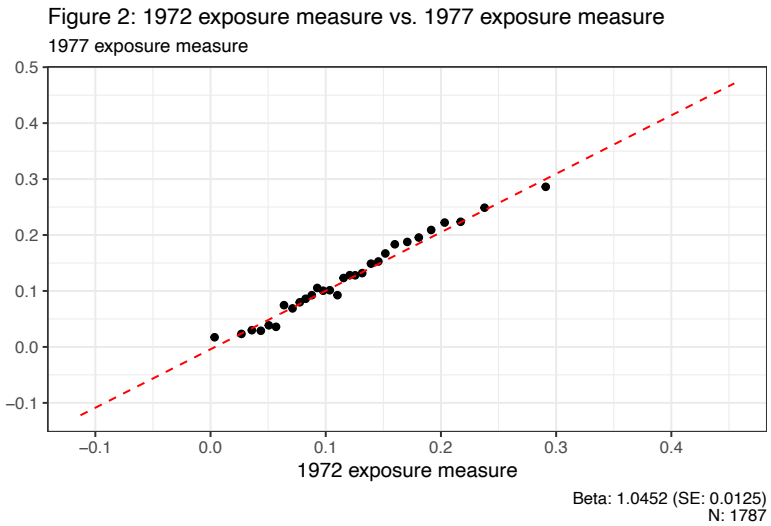
Notes: Figure shows the population-weighted histogram distribution of the county-level percent change in birthweight between years 1981 and 1982. Each panel displays the distribution of percent change in birthweight for each county in a population-weighted quartile of the 1972 exposure measure, where quartile 1 is the lowest exposure and quartile 4 is highest exposure. The sample, across the panels, is limited to counties that had rolled out FSP by 1970, and the sample is further limited to the panel that is balanced for the participation and birthweight outcome variables (N=1,788).

Figure A10: Correlation between year-specific exposure measures

(a)

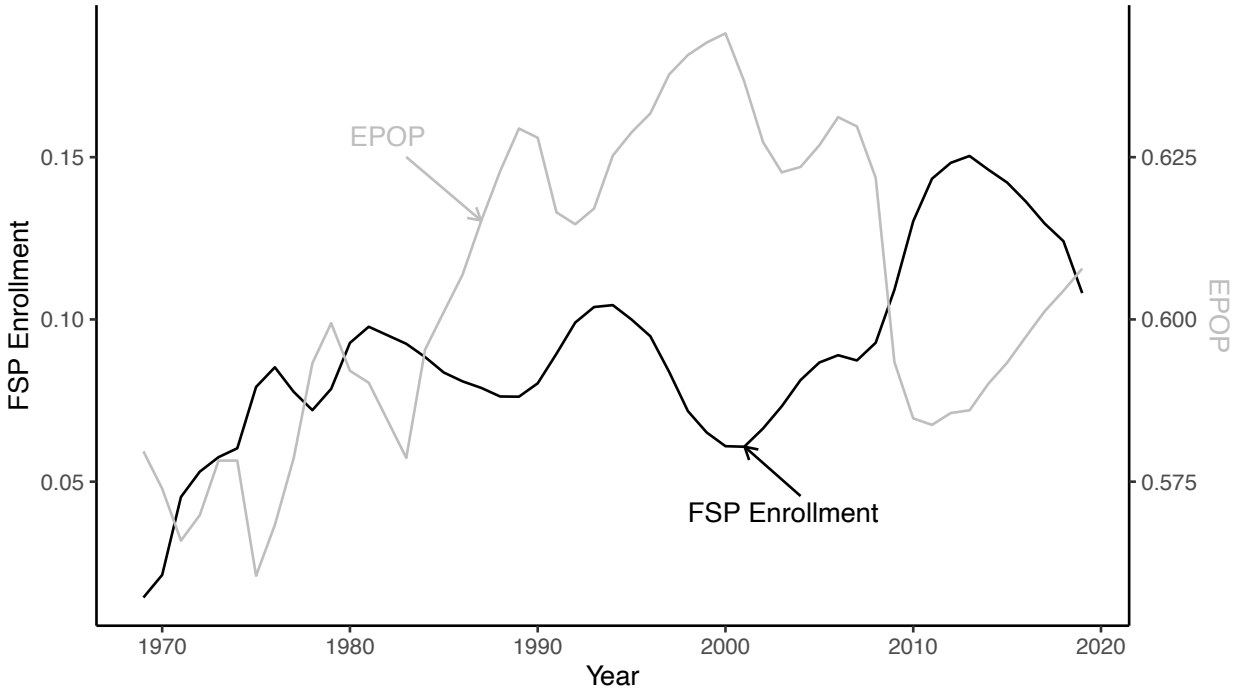


(b)



Notes: Figure shows a binned scatterplot of alternative year-specific exposure measure against the 1972 exposure measure (the baseline exposure measure for the main specifications), weighted by 1970 county population. Panel (a) describes the correlation of the 1972 exposure measure with the 1975 exposure measure, and (b) the 1977 exposure measure. Each point represents the average 1975, or 1977, exposure within a bin of 1972 exposure values. A linear fit illustrates the population-weighted correlation between the two exposure measures. Exposure measures are not winsorized. The sample is limited to the panel that is balanced for the participation and birthweight outcome variables. The sample is also limited to counties that had rolled out FSP by 1970, for which both the 1972 and later-year exposure measures are defined (N=1,787 counties).

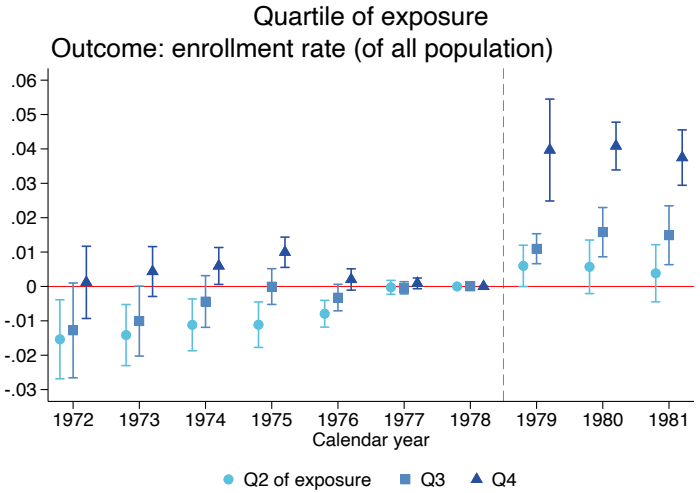
Figure A11: National Trends in Food Stamp Enrollment and Employment-to-Population Ratio



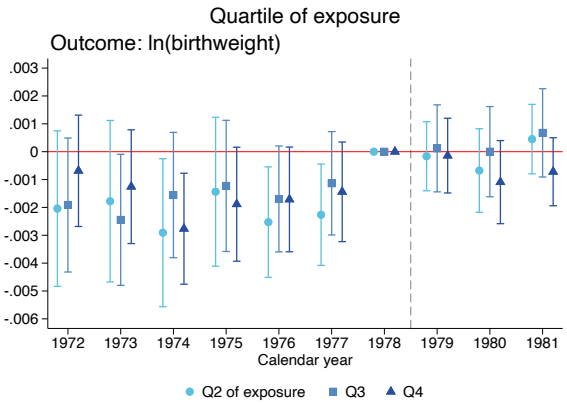
Notes: Figure shows a half-century of trends in Food Stamp Program (FSP) enrollment rate and employment-to-population ratio (EPOP) from 1969 to 2019. Data on the headcount of FSP enrollees comes from the USDA's FNS, data on U.S. population comes from the SEER database, and data on EPOP comes from the St.Louis Federal Reserve's FRED database. FSP is plotted in black and EPOP in grey. (Source: USDA Food and Nutrition Service (2025))

Figure A12: **By quartile** of 1972 exposure measure, 1979 reform impacts

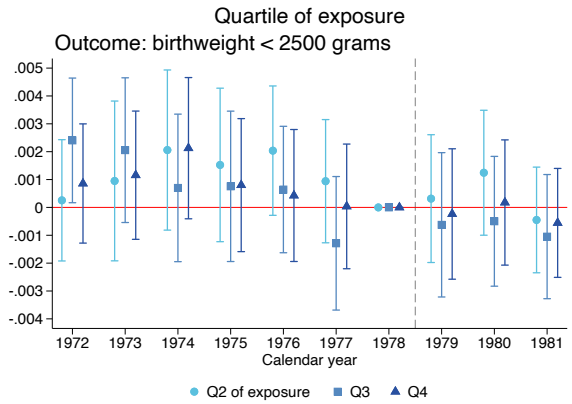
(a) First stage: impact on FSP enrollment



(b) Reduced form: impact on ln(birthweight)



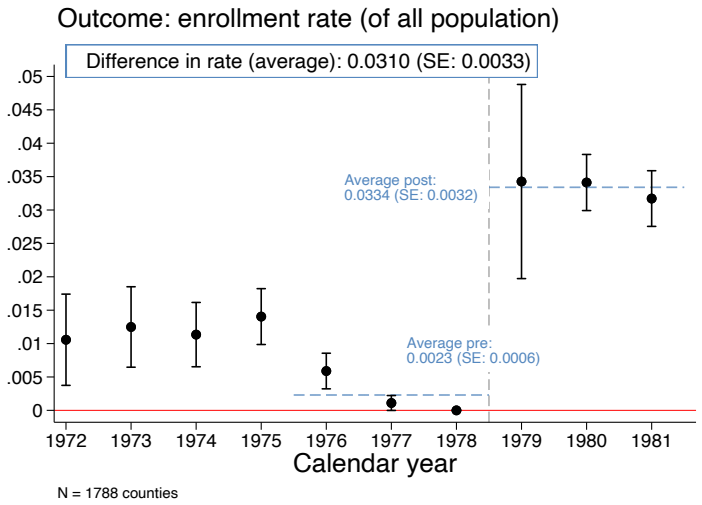
(c) Reduced form: impact on share low birthweight



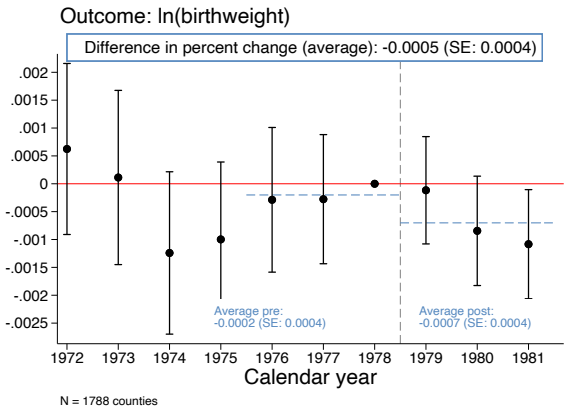
Notes: Panel (a) shows the results of estimating the first stage (Equation 10) using binary cuts of the 1972 exposure measure (where the lowest quartile of exposure comprise the comparison group) for FSP enrollment. Darker shades in the series color scale correspond with increasing quartile of exposure. Panel (b) and panel (c) show the results of estimating the reduced form equation (11) using binaries of the 1972 exposure measure (where the lowest quartile of exposure comprise the comparison group) and infant birthweight outcomes. In all panels, the exposure measure is winsorized to the population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, ±3.2%. Sample is limited to counties that rolled out FSP by 1970, and further limited to the panel that is balanced for the participation and birthweight variables (N = 1,788 counties). Sample period starts two years after the rollout cutoff year to allow the last counties rolling out to reach a steady state. Regressions are all weighted by 1970 county-level population. Standard errors are clustered at the county level. Note that weighted quartiles are defined over this defined set of counties.

Figure A13: **Highest quartile** of exposure measure, 1979 reform impacts

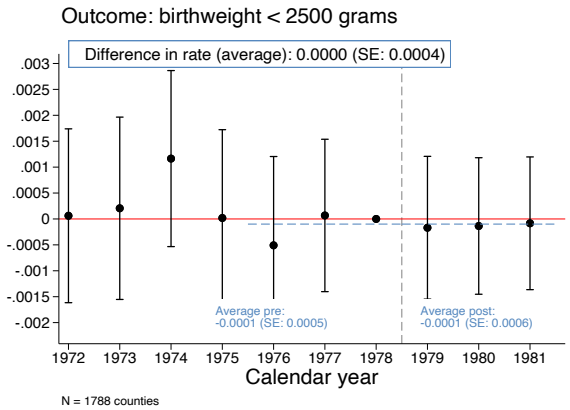
(a) First stage: impact on FSP enrollment



(b) Reduced form: impact on ln(birthweight)



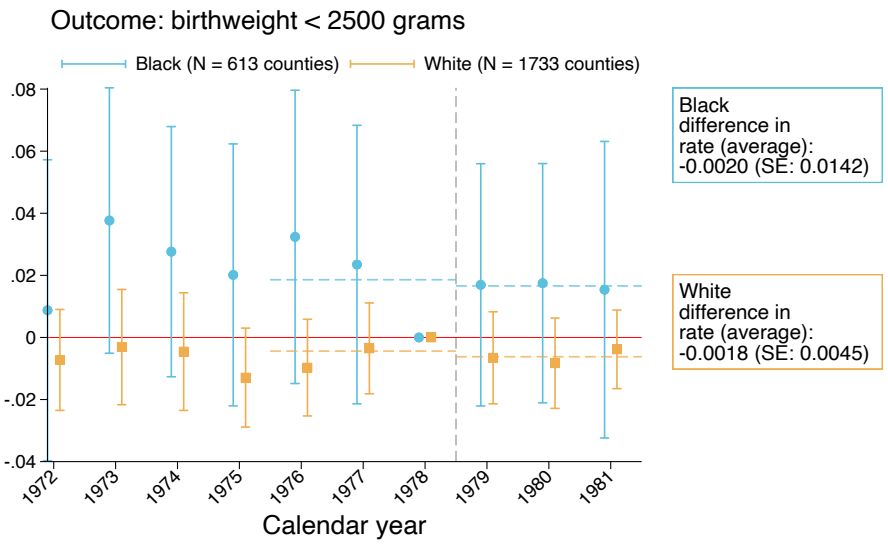
(c) Reduced form: impact on share low birthweight



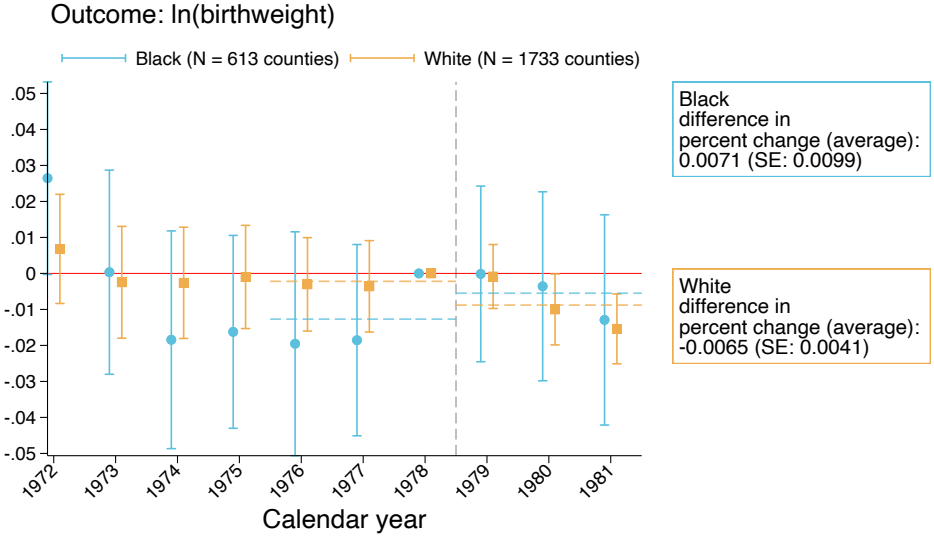
Notes: Panel (a) shows the results of estimating the first stage (Equation 10) using a binary of the 1972 exposure measure (where the lowest three quartiles of exposure comprise the comparison group) for FSP enrollment. Panel (b) and panel (c) show the results of estimating the reduced form equation (11) using a binary of the 1972 exposure measure (where the lowest three quartiles of exposure comprise the comparison group) and infant birthweight outcomes. In all panels, the exposure measure is winsorized to the population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, ±3.2%. Sample is limited to counties that rolled out FSP by 1970, and further limited to the panel that is balanced for the participation and birthweight variables (N = 1,788 counties). Sample period starts two years after the rollout cutoff year to allow the last counties rolling out to reach a steady state. Regressions are all weighted by 1970 county-level population. Standard errors are clustered at the county level.

Figure A14: 1979 Reform impacts on infant health outcomes by race

(a) Share low birthweight



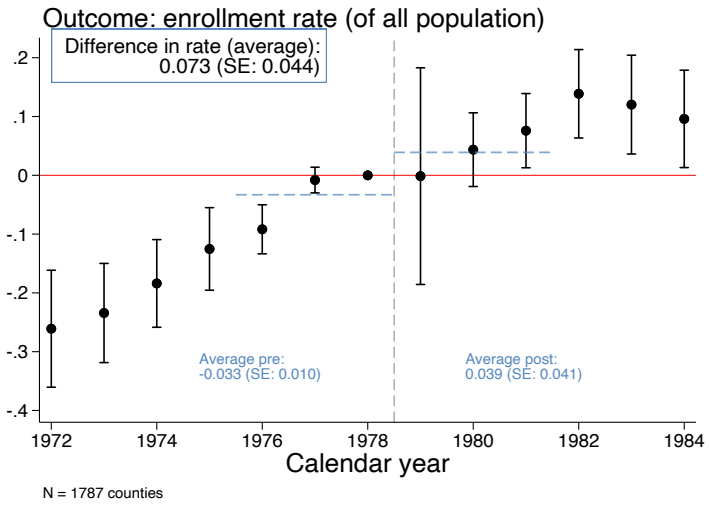
(b) Log birthweight



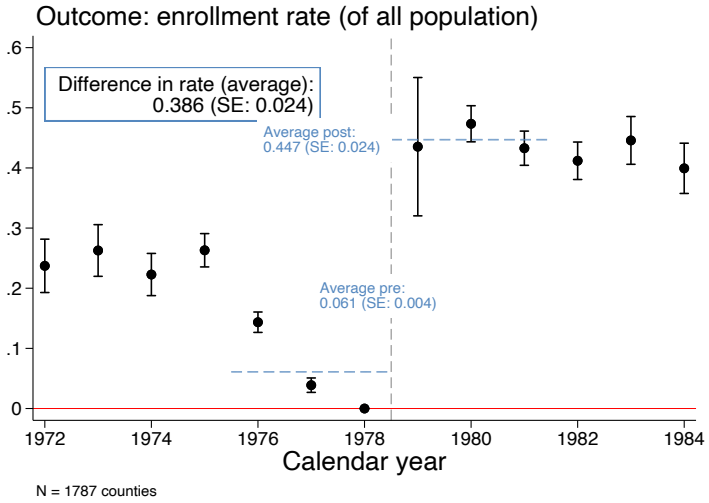
Notes: Figure shows the results of estimating the reduced form equation (11) using a continuous 1972 exposure measure on infant birthweight outcomes. The exposure measure is winsorized to the 1970 population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, ±3.2%. Sample is limited to counties that rolled out FSP by 1970 for estimations by race, and we drop county-race-years that have fewer than 25 births, and then limit the sample to the set of race-counties that have either white or black participation and birthweight data observed in each year across the panel. Sample period starts two years after the rollout cutoff year to allow the last counties rolling out to reach a steady state. Regressions are all weighted by 1970 county-level population. Standard errors are clustered at the county level.

Figure A15: 1979 Reform impacts on FSP enrollment, baseline fixed effects

(a) Baseline poverty decile × year fixed effects



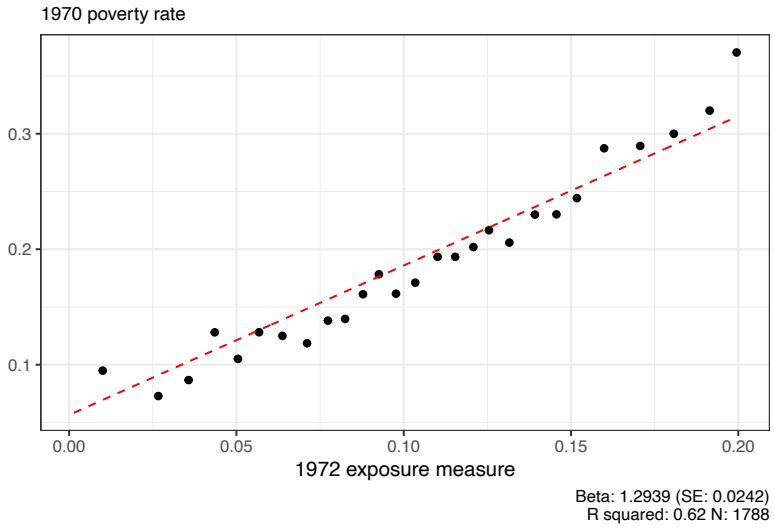
(b) Baseline take-up rate decile × year fixed effects



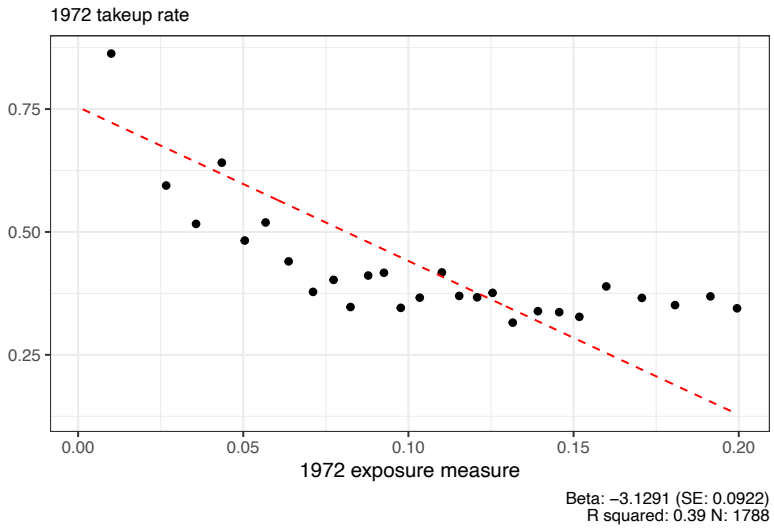
Notes: Figure shows the results of estimating the first stage equation 10 using a continuous 1972 exposure measure on FSP enrollment with additional decile by year fixed effects for county-level 1970 poverty rate decile in (a) and county-level 1972 FSP take-up rate decile (FSP participation divided by poverty rate) in (b). The exposure measure is winsorized to the 1970 population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, ±3.2%. Sample is limited to counties that rolled out FSP by 1970, and further limited to the panel that is balanced for the participation and birthweight variables (N = 1,788 counties). Sample period starts two years after the rollout cutoff year to allow the last counties rolling out to reach a steady state. Regressions are all weighted by 1970 county-level population. Standard errors are clustered at the county level.

Figure A16: Exposure measure correlated with poverty rate and takeup rate

(a) Correlation between exposure and poverty rate

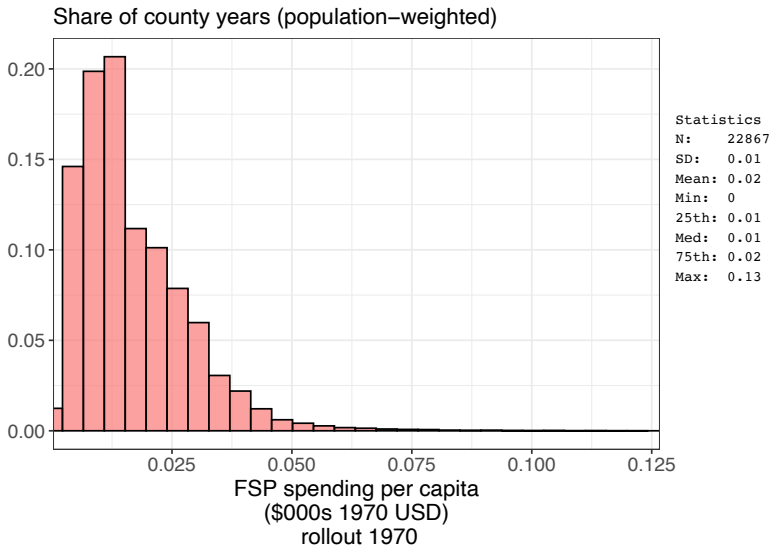


(b) Correlation between exposure and takeup rate



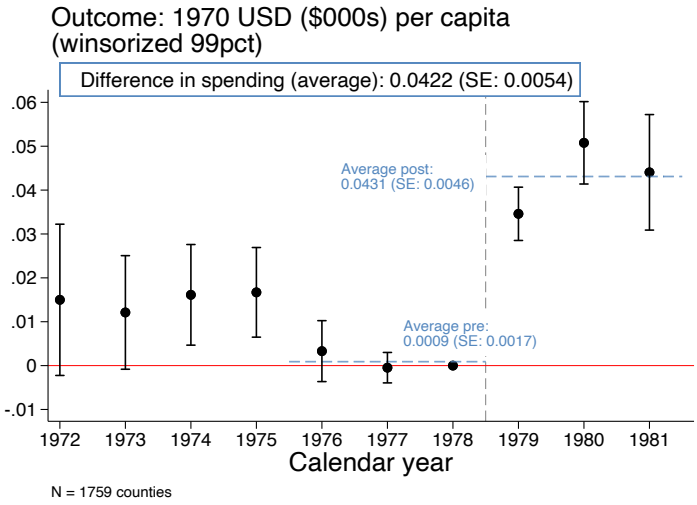
Notes: Figure shows a binned scatterplot of the 1972 exposure measure against poverty rate (a) and takeup rate (b), weighted by 1970 county population. Each point represents the average poverty or takeup rate within a bin of 1972 exposure values. A linear fit illustrates the population-weighted correlation between the two exposure measures. Exposure measures are not winsorized. The sample is limited to the panel that is balanced for the participation and birthweight outcome variables. The sample is also limited to counties that had rolled out FSP by 1970, for which both the 1972 and later-year exposure measures are defined (N=1,788 counties).

Figure A17: FSP spending per capita (thousands 1970 USD)



Notes: Figure shows the population-weighted distribution of FSP spending per capita across county-years in the sample between years 1972 and 1984. Each bar represents the share of county-years with FSP spending per capita falling within a given range of spending, measured in thousands of 1970 USD.

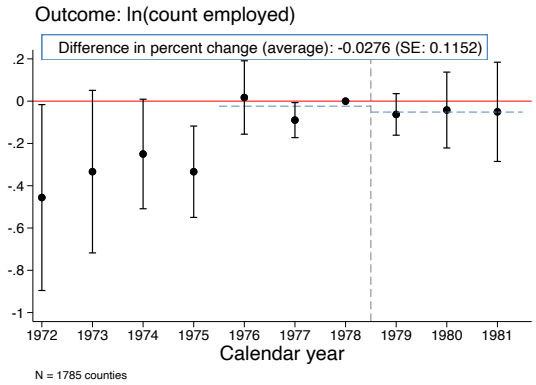
Figure A18: 1979 Reform impacts on FSP spending per capita



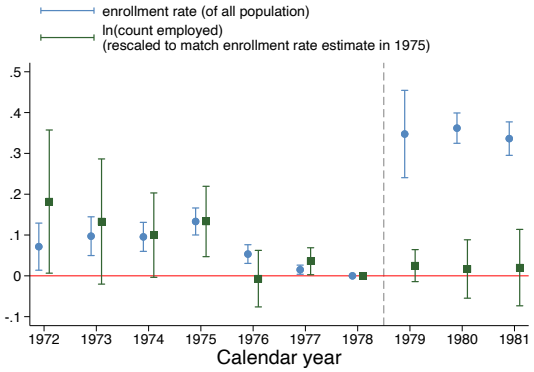
Notes: Figure shows the results of estimating the first stage equation (10) using a continuous 1972 exposure measure on county-level federal FSP transfers per capita in thousands of 1970 USD. The exposure measure is winsorized to the 1970 population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, ±3.2%. FSP spending per capita is winsorized at the 99th percentile using 1970 population weights. Sample is limited to counties that rolled out FSP by 1970, and further limited to the panel that is balanced for the participation, birthweight, and spending variables (N = 1,759 counties). Sample period starts two years after the rollout cutoff year to allow the last counties rolling out to reach a steady state. Regressions are all weighted by 1970 county-level population. Standard errors are clustered at the county level.

Figure A19: Adjusting for economic confounds using unexposed employment

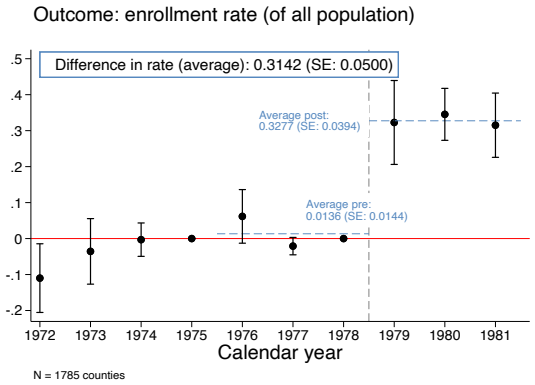
(a) Unexposed industry employment



(b) Overlaying participation and rescaled employment



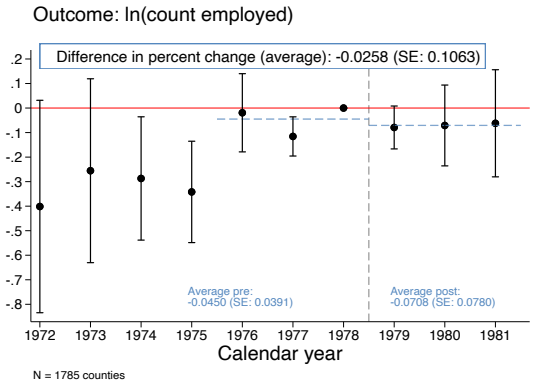
(c) Adjusted participation



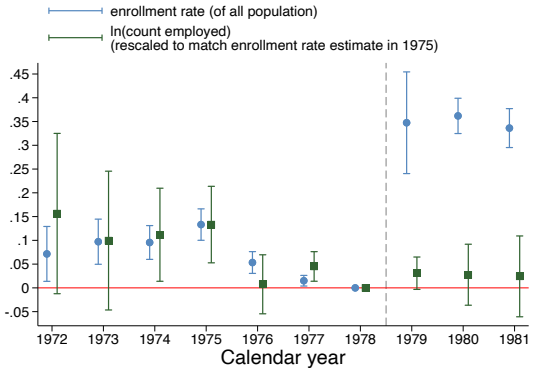
Notes: Unobserved general economic trends likely affect food stamp program enrollment, leading to pre-trends in enrollment and spending in the years before the reform. Employment in industries unlikely to be exposed to effects of the 1979 reform but is still affected by the same unobserved general economic trends. Therefore, we use leads of the treatment exposure as instruments for the employment component associated with the unobserved economic trends. Note that we construct unexposed industries using imputed CBP data and calculating aggregate employment less the sum of employment in agriculture, retail, and accommodation (highly exposed industries). We then take the natural log of this resulting employment count. Panels (a) shows the results of estimating the equation 11 for logged employment. Panel (b) takes the ratio of the first stage estimate from 10 to the employment estimate in 1975 from panel (a), and scales all employment estimates by that ratio, equating the estimates in year 1975. We choose year 1975 due to a greater estimated effect on enrollment relative to other pre-reform years, though this adjustment is robust to other choices of different additional normalizations in years where there is an estimated effect on both the first stage and employment (all but 1976, where dividing by zero becomes an issue in the ratio of estimates). Panel (c) shows the results of estimating the first stage equation for food stamp program enrollment, using the 1975 lead of the reform-exposure "treatment" as an instrument for the employment component associated with the unobserved confounder. This allows us to effectively "net out" the effects of the unobserved economic trends. All regressions are all weighted by 1970 county-level population. Standard errors are clustered at the county level.

Figure A20: Enrollment rate first stage: adjusting for economic confounds using aggregate employment

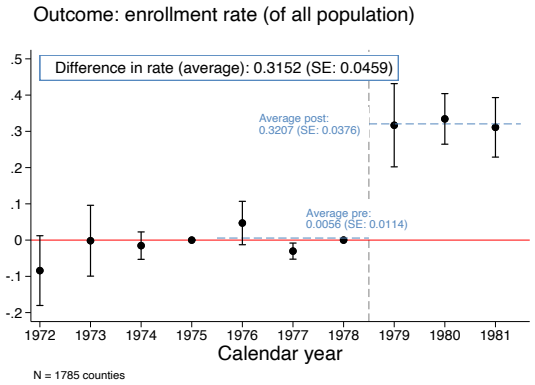
(a) Log aggregate industry employment



(b) Overlaying participation and rescaled aggregate employment



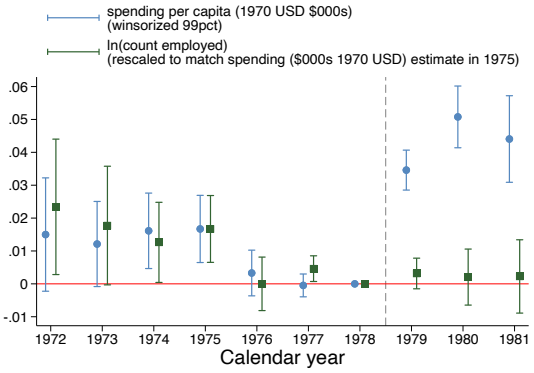
(c) Adjusted participation



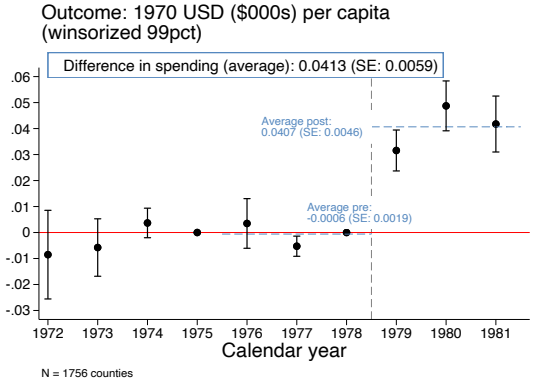
Notes: Unobserved general economic trends likely affect food stamp program enrollment and spending, leading to pre-trends in enrollment and spending in the years before the reform. Aggregate employment is unlikely to be exposed to effects of the 1979 reform but is still affected by the same unobserved general economic trends. Therefore, we use leads of the treatment exposure as instruments for the employment component associated with the unobserved economic trends. Note that we use imputed CBP data across all industries. We take the natural log of this employment count. Panels (a) shows the results of estimating the equation 11 for logged employment. Panels (b) shows the results of estimating the equation 10 (first stage) and the estimates from panel (a), but we then take the ratio of the first stage estimate to the spending estimate in 1975, and scale employment estimates by that ratio, equating the estimates in year 1975. takes the ratio of the first stage estimate to the employment estimate in 1975, and scales all employment estimates by that ratio, equating the estimates in year 1975. We choose year 1975 due to a greater estimated effect on enrollment relative to other pre-reform years, though this adjustment is robust to other choices of different additional normalizations in years where there is an estimated effect on both the first stage and employment (all but 1976, where dividing by zero becomes an issue in the ratio of estimates). Panels (c) shows the results of estimating the first stage equation for food stamp program enrollment, using the 1975 lead of the reform-exposure "treatment" as an instrument for the employment component associated with the unobserved confounder. This allows us to effectively "net out" the effects of the unobserved economic trends. All regressions are all weighted by 1970 county-level population. Standard errors are clustered at the county level.

Figure A21: Spending first stage: adjusting for economic confounds

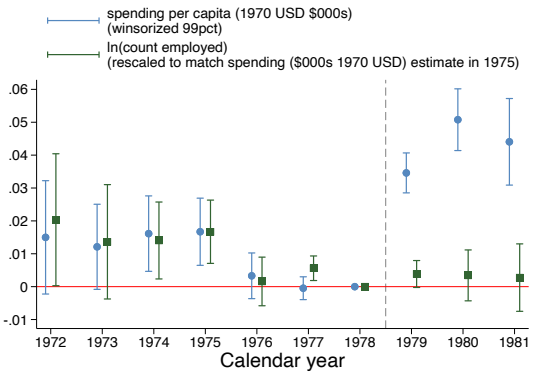
(a) Overlaying spending and rescaled unexposed employment



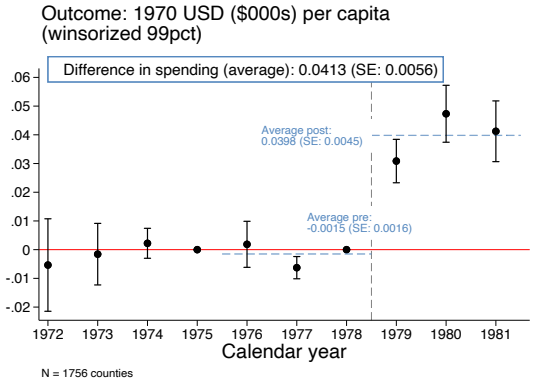
(b) Adjusted spending (unexposed employment proxy)



(c) Overlaying spending and rescaled employment



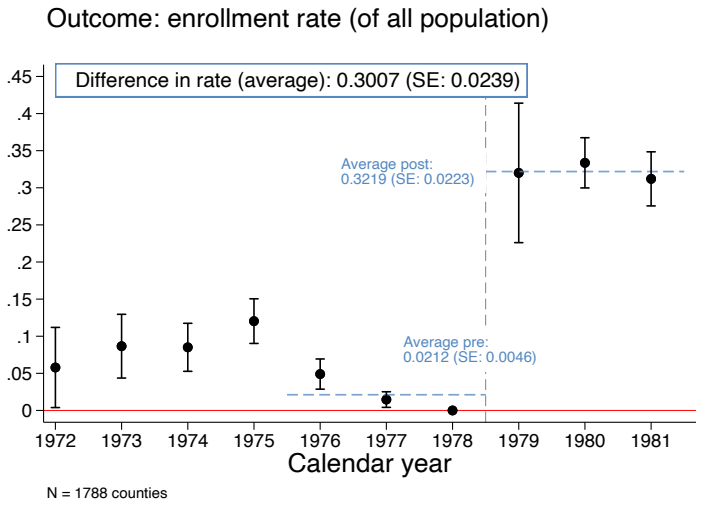
(d) Adjusted spending (aggregate employment proxy)



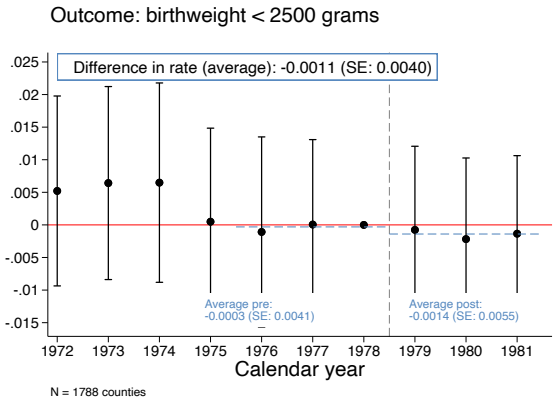
Notes: Unobserved general economic trends likely affect food stamp program enrollment, leading to pre-trends in enrollment and spending in the years before the reform. Employment in industries unlikely to be exposed to effects of the 1979 reform but is still affected by the same unobserved general economic trends. Therefore, we use leads of the treatment exposure as instruments for the employment component associated with the unobserved economic trends. Note that we construct unexposed industries using imputed CBP data and calculating aggregate employment less the sum of employment in agriculture, retail, and accomodation (highly exposed industries). We then take the natural log of this resulting employment count. Panels (a) and (c) shows the results of estimating the equation 10 (first stage) and 11 for logged employment. We then take the ratio of the first stage estimate to the spending estimate in 1975, and scales employment estimates by that ratio, equating the estimates in year 1975 in panels (a) and (c). We choose year 1975 due to a greater estimated effect on spending relative to other pre-reform years, though this adjustment is robust to other choices of different additional normalizations in years where there is an estimated effect on both the first stage and employment (all but 1976, where dividing by zero becomes an issue in the ratio of estimates). Panels (b) and (d) shows the results of estimating the first stage equation for food stamp program spending, using the 1975 lead of the reform-exposure "treatment" as an instrument for the employment component associated with the unobserved confounder. This allows us to effectively "net out" the effects of the unobserved economic trends. All regressions are all weighted by 1970 county-level population. Standard errors are clustered at the county level.

Figure A22: Unwinsorized 1972 exposure measure, 1979 reform impacts on FSP take-up and infant health outcomes

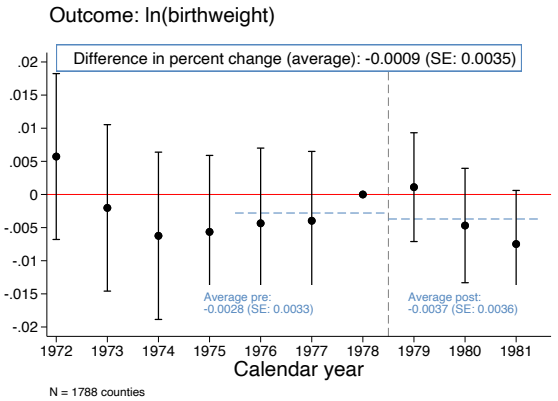
(a) First stage



(b) Share low birthweight



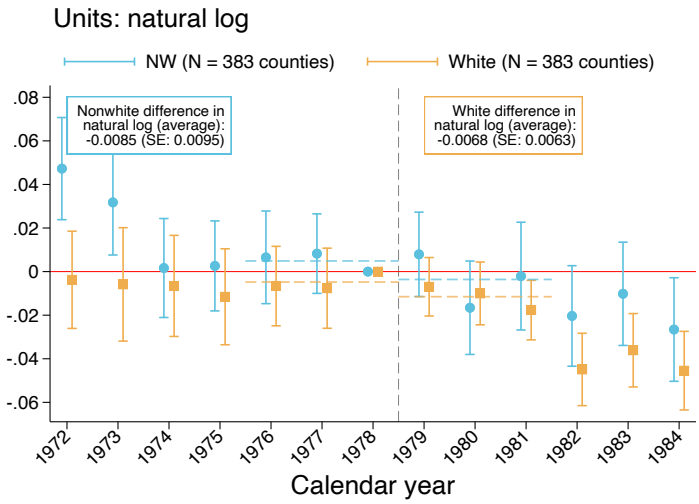
(c) Log birthweight



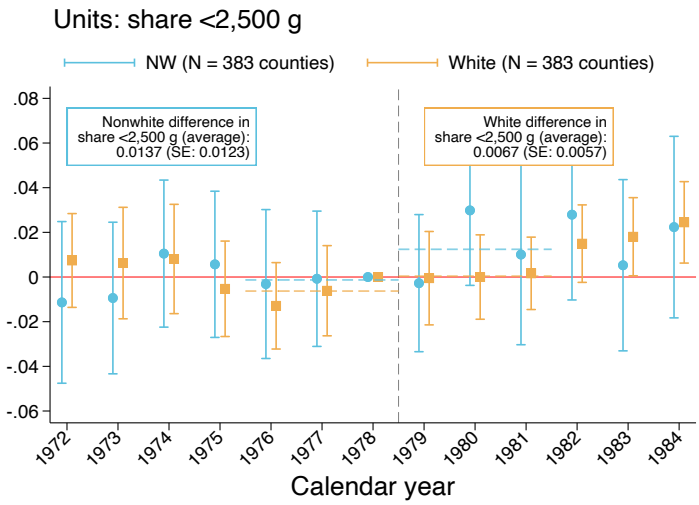
Notes: Figure shows the results of estimating the first stage (Equations 10 and 11) using a continuous 1972 exposure measure. The exposure measure is not winsorized. Sample is limited to counties that rolled out FSP by 1970, and further limited to the panel that is balanced for the participation and birthweight variables (N = 1,788 counties). Sample period starts two years after the rollout cutoff year to allow the last counties rolling out to reach a steady state. Regressions are all weighted by 1970 county-level population. Standard errors are clustered at the county level.

Figure A23: 1979 Reform impacts on infant health outcomes by race

(a) Reduced form: impact on ln(birthweight)

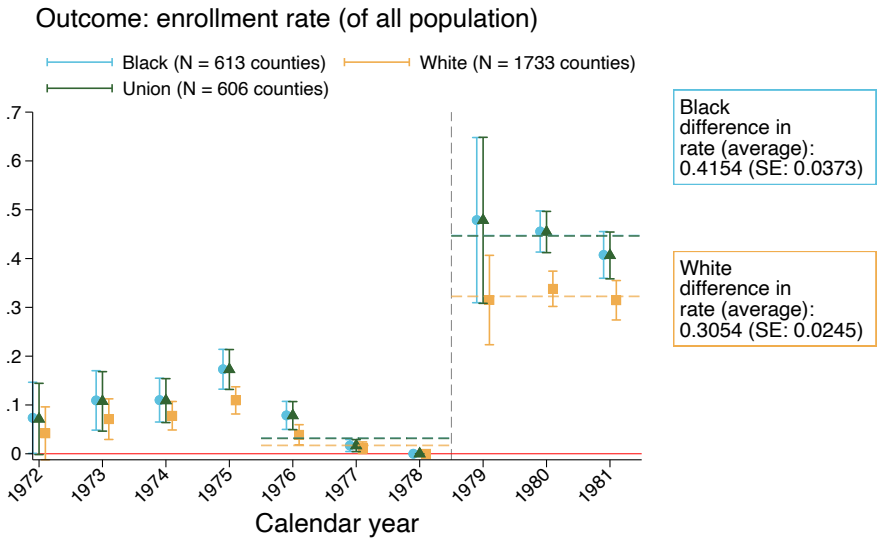


(b) Reduced form: impact on share low birthweight



Notes: Figure shows the results of estimating the reduced form equation 11 separately for each race group, using a continuous 1972 exposure measure on infant birthweight outcomes. The exposure measure is winsorized to the 1970 population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, ±3.2%. Sample is limited to counties that rolled out FSP by 1970 for estimations by race, and we drop county-race-year cells that have fewer than 25 births, and then limit the sample to the set of counties that have both white and black participation and birthweight data observed in each year across the panel. Sample period starts two years after the rollout cutoff year to allow the last counties rolling out to reach a steady state. Regressions are all weighted by 1970 county-level population. Standard errors are clustered at the county level.

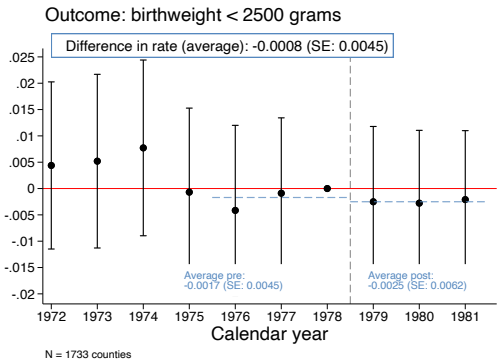
Figure A24: 1979 Reform impacts on FSP takeup by race



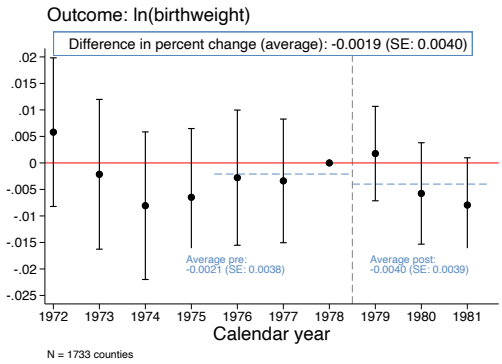
Notes: Figure shows the results of estimating the first stage (Equation 10) separately for each race group but with the births in those counties pooled (not race-specific births), and using a continuous 1972 exposure measure on FSP enrollment. The exposure measure is winsorized to the 1970 population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, ±3.2%. Sample is limited to counties that rolled out FSP by 1970. For estimations by race, and we drop county-race-years that have fewer than 25 births, and then limit the sample to the set of race-counties that have either white or black participation and birthweight data observed in each year across the panel. The union of these groups (i.e. the counties that have both white and nonwhite participation and birthweight observed in each year across the panel) is also represented by the green series. Sample period starts two years after the rollout cutoff year to allow the last counties rolling out to reach a steady state. Regressions are all weighted by 1970 county-level population. Standard errors are clustered at the county level.

Figure A25: 1979 Reform impacts on infant health outcomes (limited to counties in panels balanced for analysis by race)

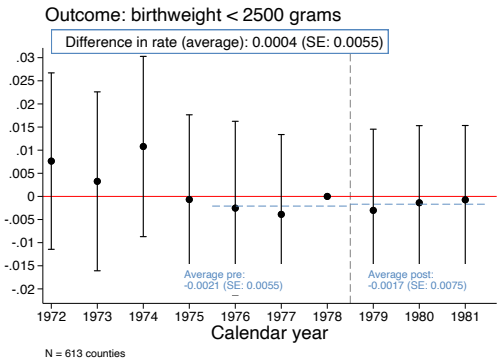
(a) Balanced for white racegroup



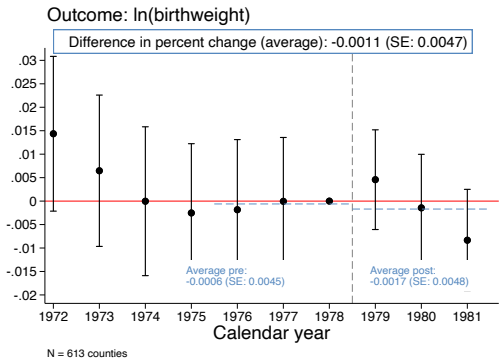
(b) Balanced for white racegroup



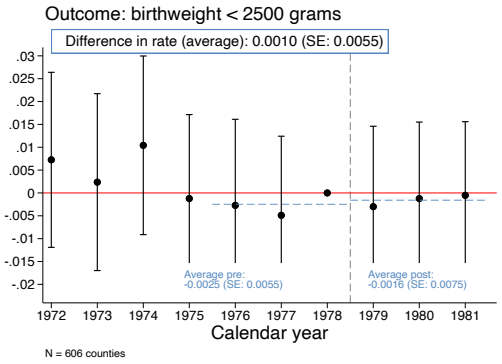
(c) Balanced for black racegroup



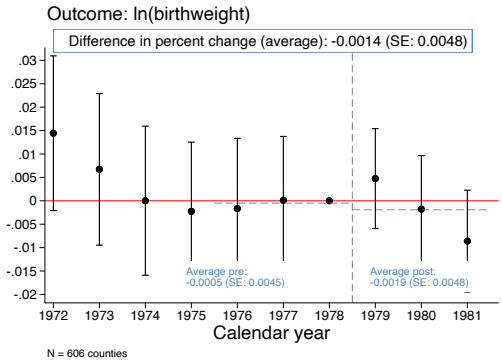
(d) Balanced for black racegroup



(e) Balanced for both racegroups

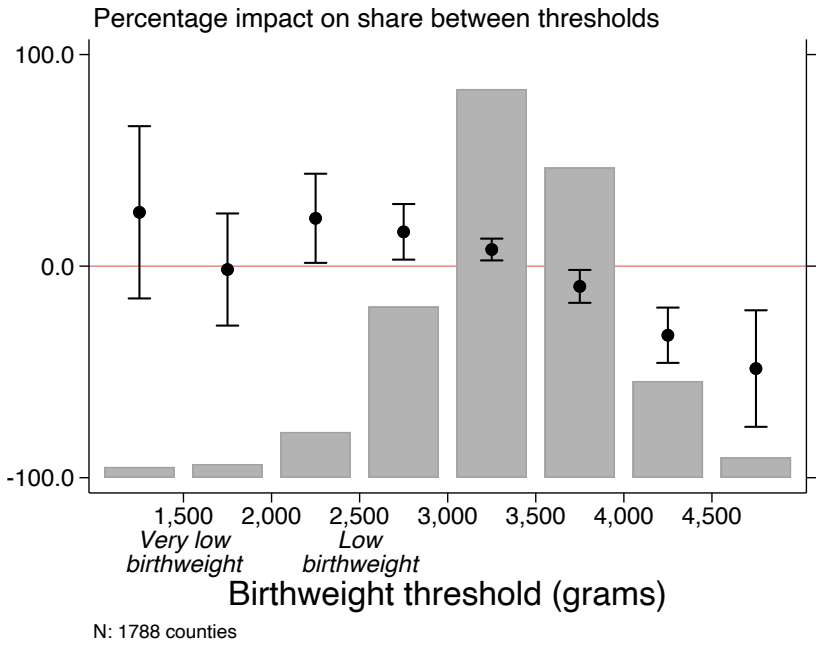


(f) Balanced for both racegroups



Notes: Figure shows the results of estimating the reduced form equation (11) using a continuous 1972 exposure measure on FSP enrollment. The exposure measure is winsorized to the 1970 population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, ±3.2%. Sample is limited to counties that rolled out FSP by 1970 for estimations by race, and we drop county-race-years that have fewer than 25 births, and then for panel (a), we limit the sample to the set of counties that have white participation and birthweight data observed in each year across the panel dataset. Similarly, for panel (b), we apply the same additional restriction but for the black racegroup panel, and in panel (c), for both racegroups. Note that this analysis is done on all births in the county (pooled racegroups) but for the set of counties present in each balanced-panel race-specific analysis. Sample period starts two years after the rollout cutoff year to allow the last counties rolling out to reach a steady state. Regressions are all weighted by 1970 county-level population. Standard errors are clustered at the county level.

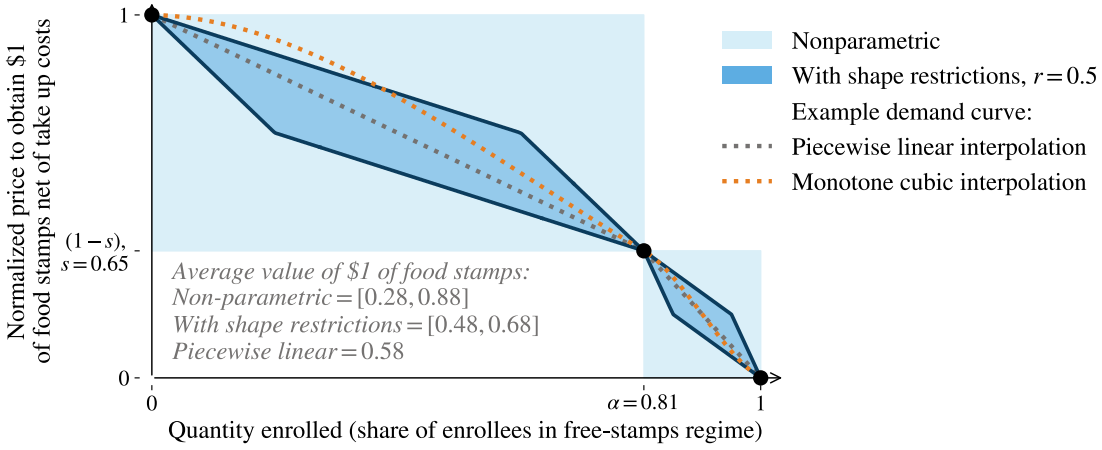
Figure A26: Percentage impacts of later post-reform period on share within birthweight ranges



Note: Figure shows the scaled results of estimating the reduced form equation (11) using a continuous 1972 exposure measure on infant birthweight ranges (left y-axis) and the share of births across the period 1976-1978 that fall within each range (relative shares depicted by gray bars). RF estimates are scaled by outcome means across the period 1976-1978. Therefore, the estimates show the percentage impact of the later post-reform period (1982-1984) on the fraction of births in the county-year cell that fall within each range of specified number of grams. We define the ranges as inclusive of the lower threshold and exclusive of the upper threshold. For example, we define the first outcome range as "$<$ 1,500 grams" followed by "$\geq$ 1,500 and $<$ 2,000." Similarly, we define the last range as "$\geq$ 4,500." The exposure measure is winsorized to the 1970 population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, $\pm$3.2%. Sample is limited to counties that rolled out FSP by 1970, and further limited to the panel that is balanced for the participation and birthweight variables (N = 1,788 counties). Sample period starts two years after the rollout cutoff year to allow the last counties rolling out to reach a steady state. RF regressions are all weighted by 1970 county-level population. RF regression standard errors are clustered at the county level.

Figure A27: Bounds on value of food stamps

(a) With purchase requirement only



(b) With purchase requirement and bonus reform

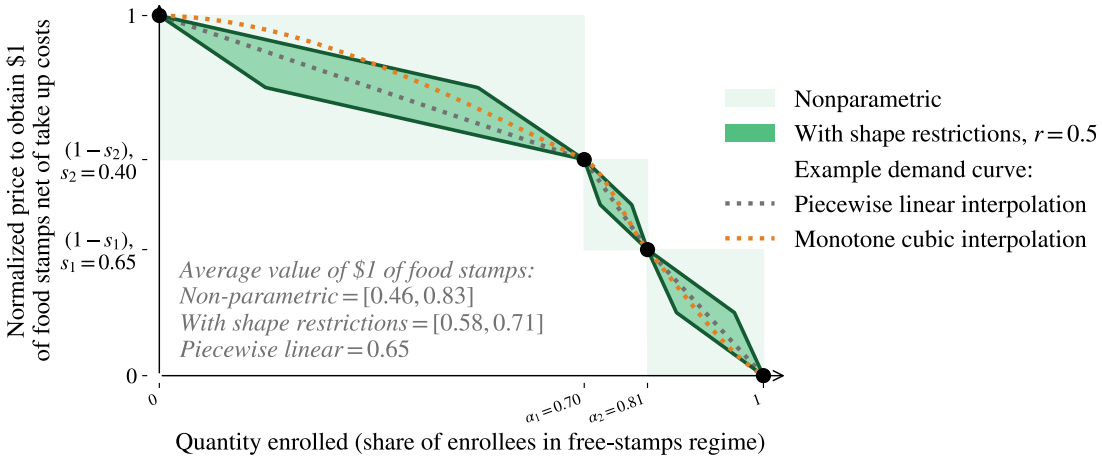
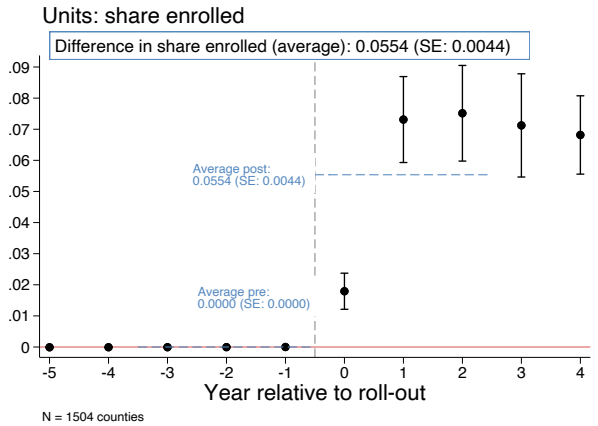
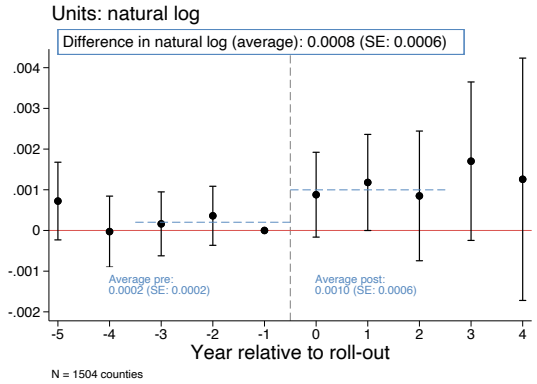


Figure A28: Effect of FSP rollout, population weights

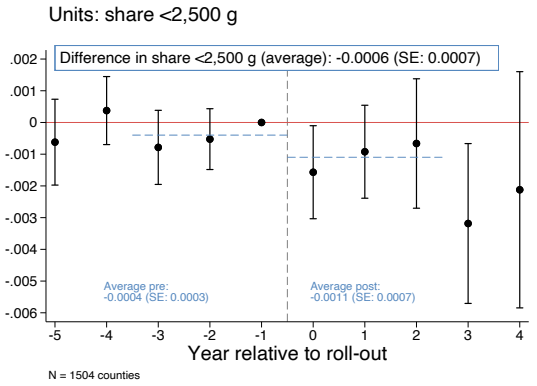
(a) First stage: impact on FSP enrollment



(b) Reduced form: impact on ln(birthweight)



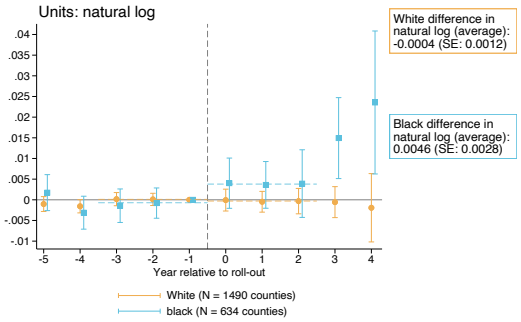
(c) Reduced form: impact on share low birthweight



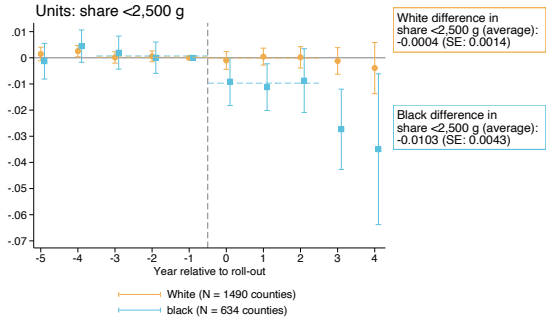
Notes: Figure show estimates of β_r from equation 13 in panel a and from equation 14 in panels b and c. The regressions are weighted by county population in 1960. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the county level. Sample size, in terms of counties, is denoted in the figure legend. Since FSP enrollment is only reported annually, relative year corresponds to a calendar year for each county's rollout timing in panel a. Since birthweight data can be aggregated to the monthly level, relative year is constructed as the twelve months following each county's rollout timing. Therefore, relative year in panels b and c does not correspond to a calendar year.

Figure A29: Effect of FSP rollout by race, stacked dataset weights

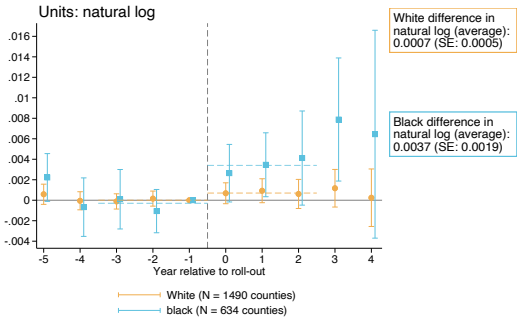
(a) Reduced form: impact on ln(birthweight)



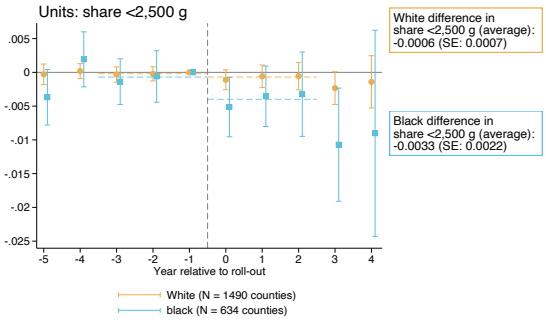
(b) Reduced form: impact on share low birthweight



(c) Reduced form: impact on ln(birthweight)



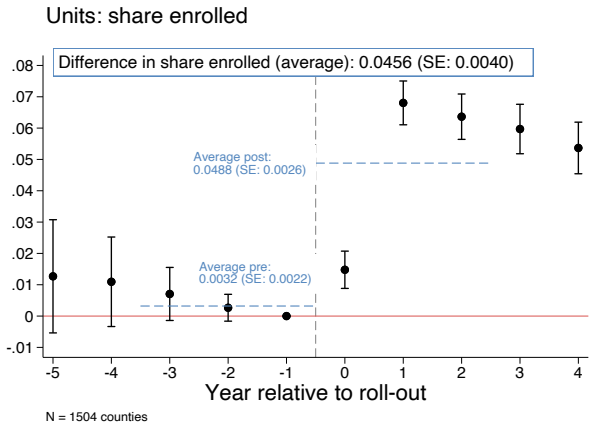
(d) Reduced form: impact on share low birthweight



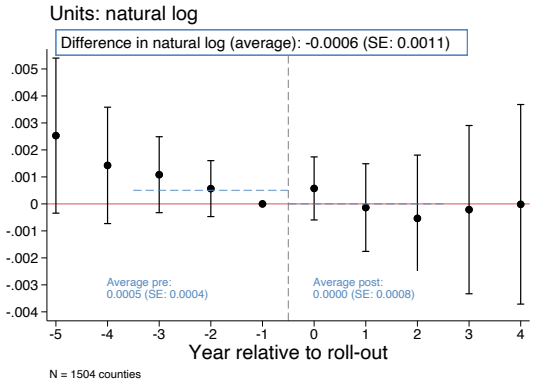
Notes: Figure show estimates of β_r from equation 14 using birthweight outcomes for white and black births, separately. The regressions are weighted by dataset-normalized weights which are described above in equation 30 in panels (a) and (b), but with county race population weights in panels (c) and (d). Vertical lines denote 95% confidence intervals computed using standard errors clustered at the county-race level. We drop counties that do not have race-specific birthweight data for any year in the range 1970-1975. Sample size, in terms of counties, is denoted in the figure legend.

Figure A30: Effect of FSP rollout, two-way fixed effects

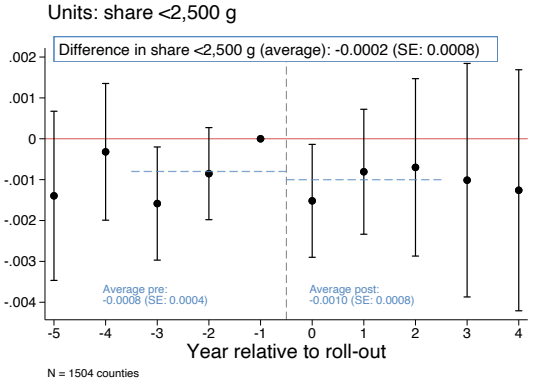
(a) First stage: impact on FSP enrollment



(b) Reduced form: impact on ln(birthweight)



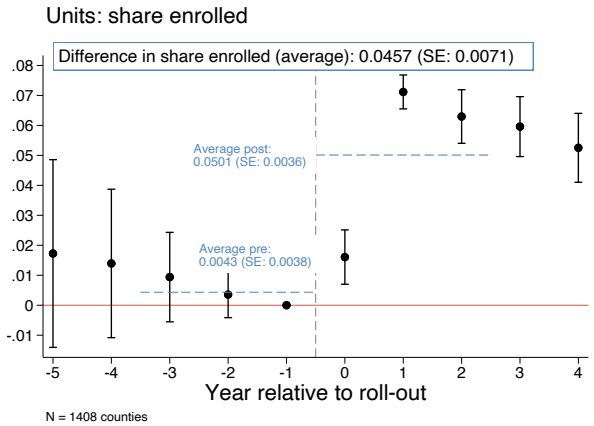
(c) Reduced form: impact on share low birthweight



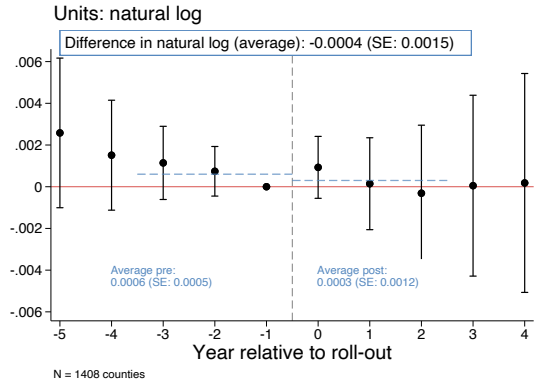
Notes: Figure shows estimates of β_r from equation 28 in panel a and from equation 29 in panels b and c, on a singular, flat (not stacked dataset) dataset. The regressions are weighted by county population in 1960. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the county level. Sample size, in terms of counties, is denoted in the figure legend. Since FSP enrollment is only reported annually, relative year corresponds to a calendar year for each county's rollout timing in panel a. Since birthweight data can be aggregated to the monthly level, relative year is constructed as the twelve months following each county's rollout timing. Therefore, relative year in panels b and c does not correspond to a calendar year. Relative years beyond five years in the pre and post period are grouped into single bins (-5, +5). Note that the flat (not stacked) county-period dataset, and fixed effects for county and period (year in panel (a) and month in panels (b) and (c)) are used in these estimations.

Figure A31: Effect of FSP rollout, two-way fixed effects on balanced panel

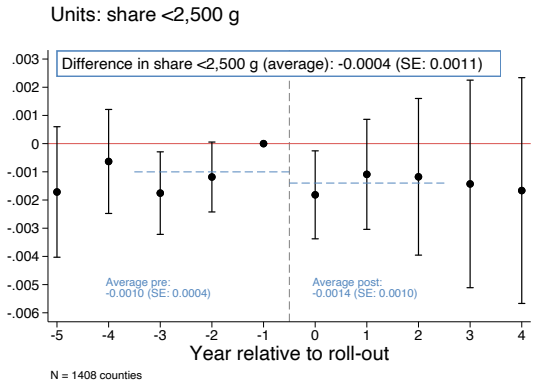
(a) First stage: impact on FSP enrollment



(b) Reduced form: impact on ln(birthweight)



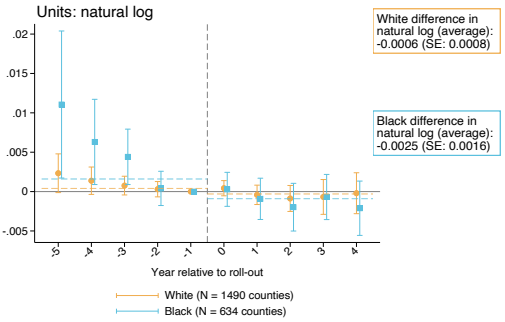
(c) Reduced form: impact on share low birthweight



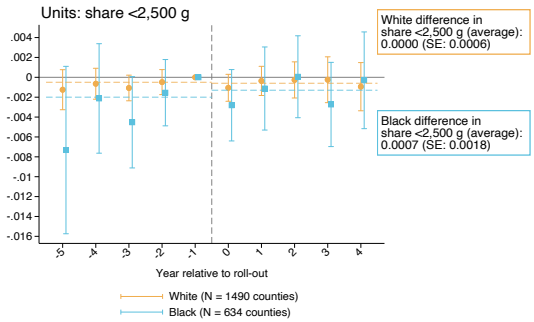
Notes: Figure shows estimates of β_r from Equation (28) in panel a and from Equation (29) in panels b and c, on a singular, flat (not stacked dataset) dataset. The regressions are weighted by county population in 1960. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the county level. Sample size, in terms of counties, is denoted in the figure legend. Since FSP enrollment is only reported annually, relative year corresponds to a calendar year for each county's rollout timing in panel a. Since birthweight data can be aggregated to the monthly level, relative year is constructed as the twelve months following each county's rollout timing. Therefore, relative year in panels b and c does not correspond to a calendar year. Relative years beyond five years in the pre and post period are grouped into single bins (-5, +5). Note that the flat (not stacked) county-period dataset, and fixed effects for county and period (year in panel (a) and month in panels (b) and (c)) are used in these estimations.

Figure A32: Effect of FSP rollout by race, two-way fixed effects

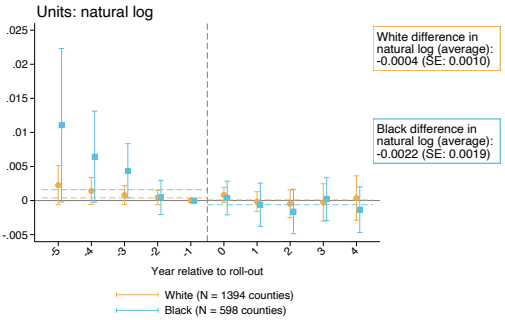
(a) Reduced form: impact on ln(birthweight)



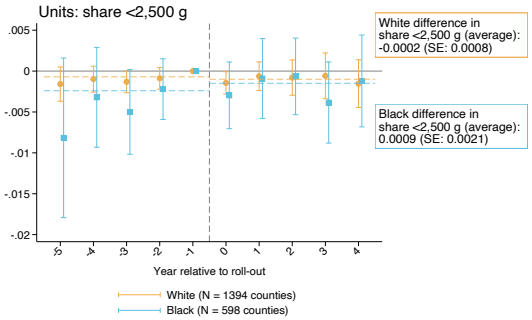
(b) Reduced form: impact on share low birthweight



(c) Reduced form: impact on ln(birthweight) (balanced panel)



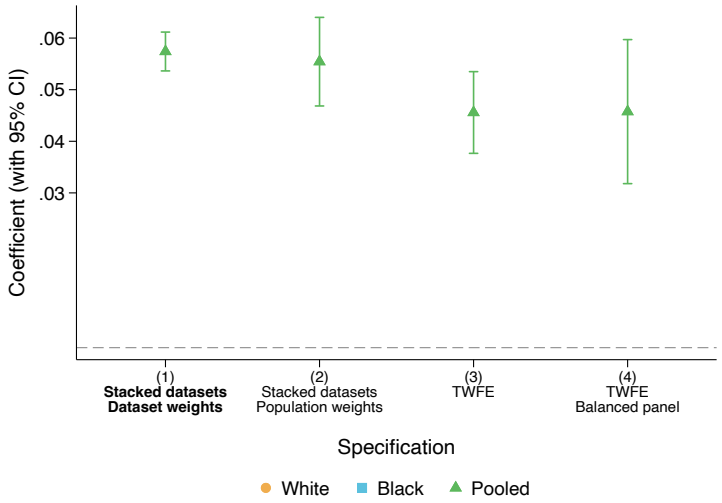
(d) Reduced form: impact on share low birthweight (balanced panel)



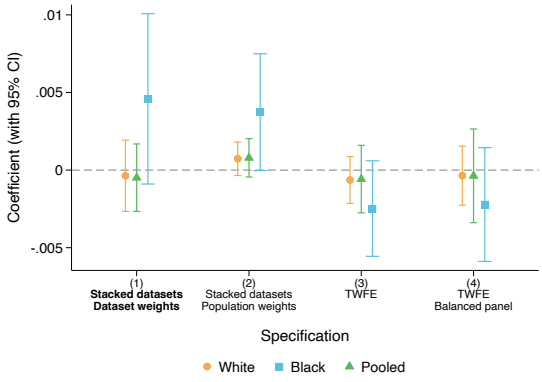
Notes: Figure show estimates of β_r from Equation (29) using birthweight outcomes for white and black births, separately. Note also that this specification is run on a singular, flat (not stacked dataset) dataset. The regressions are weighted by county-race population in 1960. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the county level. We drop counties that do not have race-specific birthweight data for any year in the range 1970-1975. In panels (c) and (d), we additionally limit to counties that rollout food stamps in 1970 or later. Sample size, in terms of counties, is denoted in the figure legend. Relative years beyond five years in the pre and post period are grouped into single bins (-5, +5). Note that the flat (not stacked) county-period dataset and fixed effects for county-race and period (month) are used in these estimations.

Figure A33: FSP rollout effects across specifications

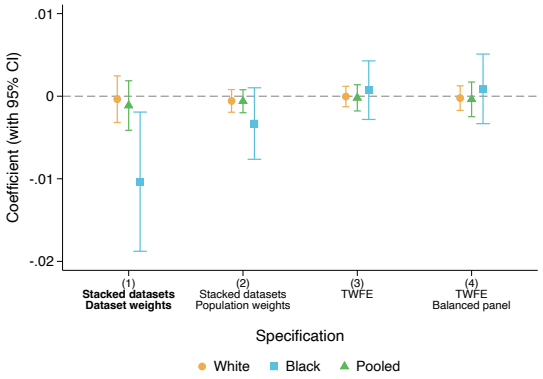
(a) First stage: FSP enrollment



(b) Reduced form: impact on ln(birthweight)



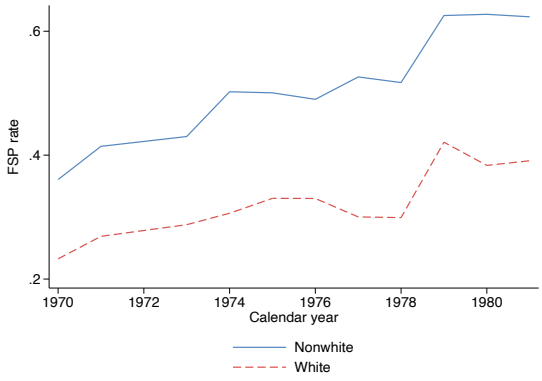
(c) Reduced form: impact on share low birthweight



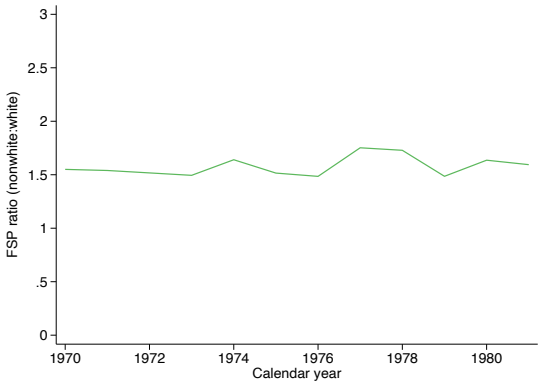
Notes: Figure compiles the 3-year difference coefficients and 95% CIs from four separate specifications for pooled races and then for each race group separately. Across specifications, estimations by race use county-race population in 1960 instead of county population in 1960. All specifications cluster standard errors at the county level. The baseline specification ("stacked datasets, dataset weights") uses the stacked event study estimators from Equations (13) and (14) and weights of the ratio of a dataset's share of the total treatment 1960 population and the share of treatment (or control) observations in a given relative year within a dataset of the total treatment (or control) population across that relative year. The "stacked datasets, population weights" specification uses the same estimating equations but instead weights by county population in 1960. the "TWFE" and "TWFE balanced panel" specifications use a flat (not stacked) dataset structure for Equations (28) and (29) and weight by county population in 1960; in the balanced panel version we limit to counties that rolled out the food stamp program in 1970 or later.

Figure A34: PSID FSP Takeup Summary

(a) Take-up rate by race (PSID).



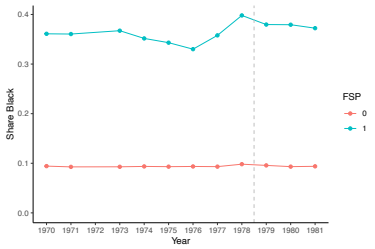
(b) PSID FSP takeup ratio by race



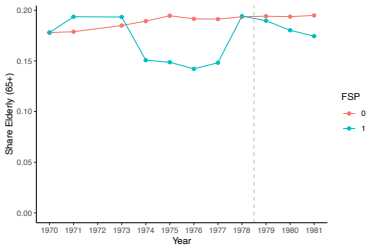
Notes: Panel (a) plots the national, annual take-rate rate of food stamp for whites and non whites separately, with take-up defined as the share of the eligible population that is enrolled. Panel (b) plots the ratio of those race-specific take-up rates (nonwhite:white) in each year between 1970 and 1981.

Figure A35: Demographic trends by FSP Participation

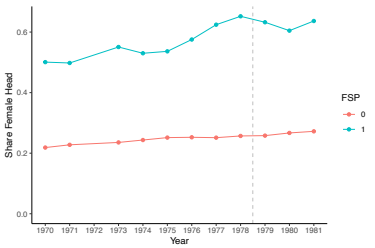
(a) Share black



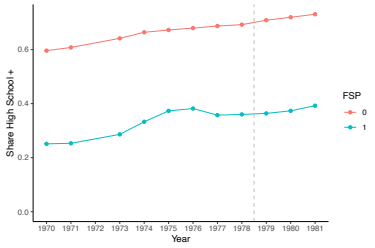
(b) Share elderly



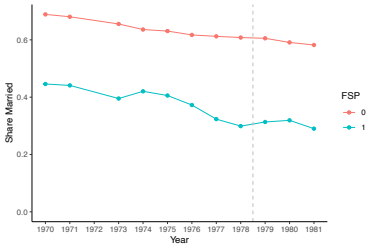
(c) Share with female head of household



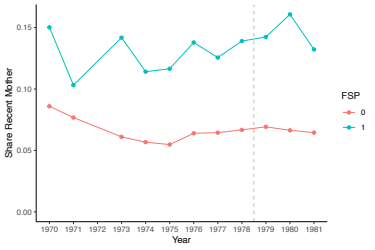
(d) Share high school education or more



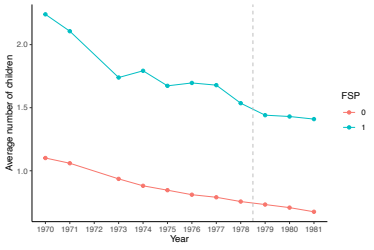
(e) Share married



(f) Share with a recent mother



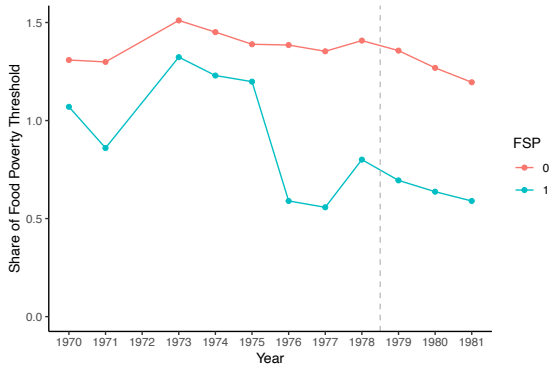
(g) Average count of children



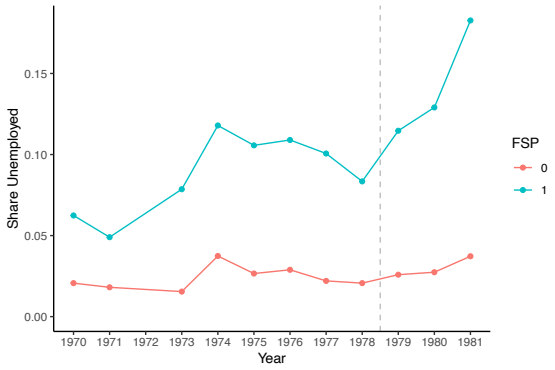
Notes: In the 1978 cross-section, 7.3% of the population (847 raw PSID observations) are participants, and 92.7% of the population (5,524 raw PSID observations) are nonparticipants. All shares within the FSP participant group and the FSP nonparticipant group are weighted using PSID weights. One unit is one household-year, so the same household could be included in the participant group one year and the nonparticipant group in another year depending on whether that household received any food stamps in a given year. The dashed line indicates 1978, the last period before the purchase requirement reform of 1979.

Figure A36: Income trends by FSP Participation

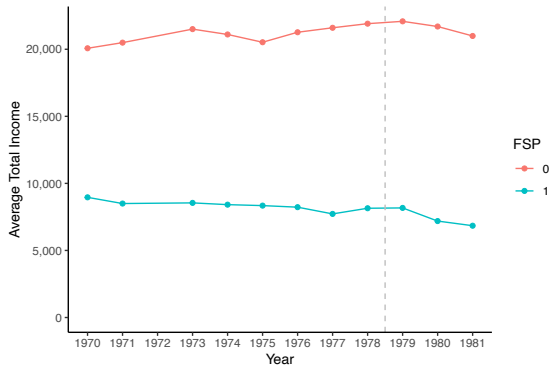
(a) Food spending as share of food poverty line



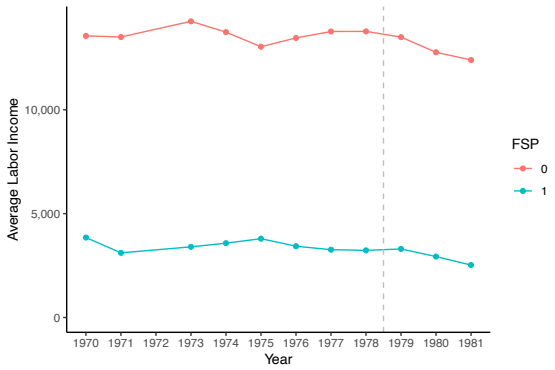
(b) Share unemployed



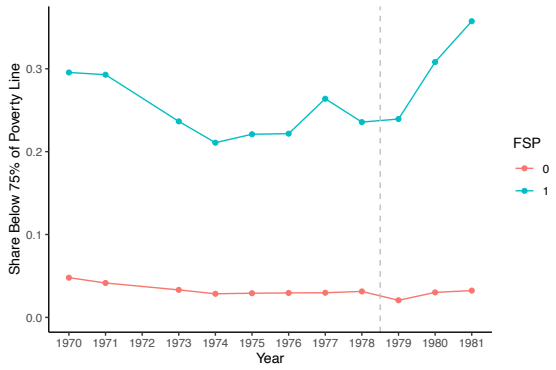
(c) Average total income



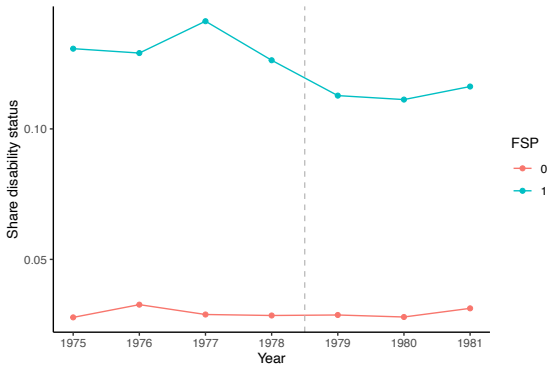
(d) Average labor income



(e) Share below 75% of poverty line



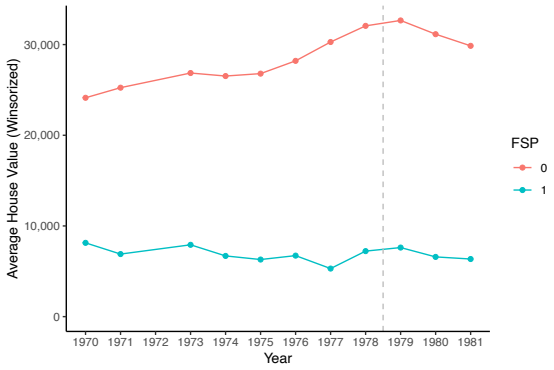
(f) Share with disability status



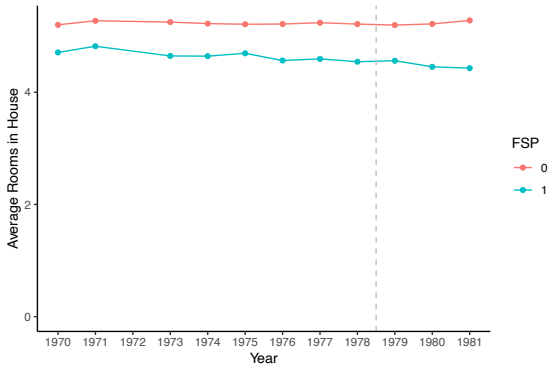
Notes: In the 1978 cross-section, 7.3% of the population (847 raw PSID observations) are participants, and 92.7% of the population (5,524 raw PSID observations) are nonparticipants. All shares within the FSP participant group and the FSP nonparticipant group are weighted using PSID weights. One unit is one household-year, so the same household could be included in the participant group one year and the nonparticipant group in another year depending on whether that household received any food stamps in a given year. The dashed line indicates 1978, the last period before the purchase requirement reform of 1979. Poverty status is defined as family income less than Census needs standard (deflated to 1979 USD) which is based on family size, age of adults (elderly or not), and sex of head of household.

Figure A37: Asset trends by FSP Participation

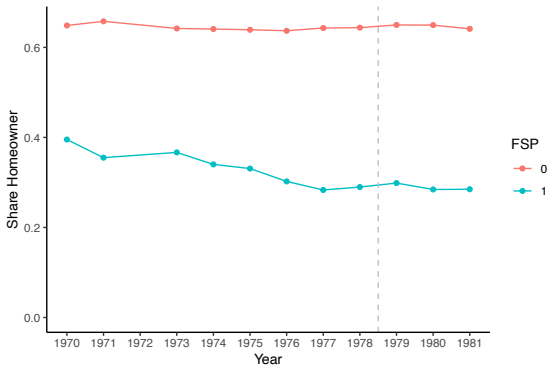
(a) Average home value



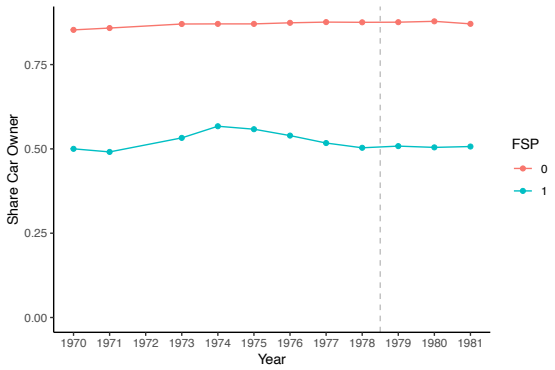
(b) Average count of rooms in home



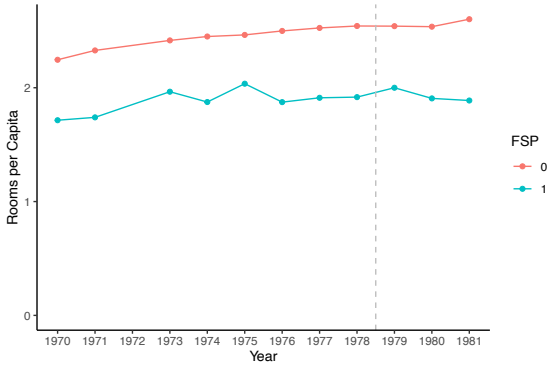
(c) Share owning home



(d) Share owning car



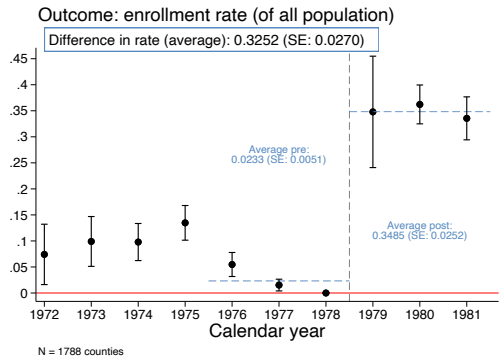
(e) Rooms per person in home



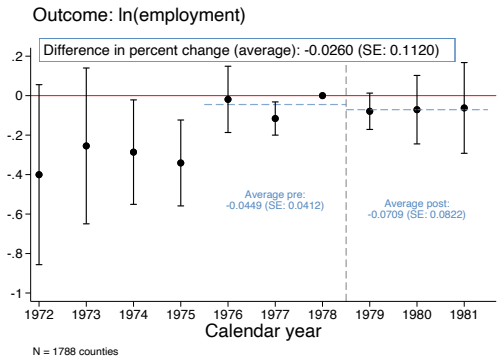
Notes: In the 1978 cross-section, 7.3% of the population (847 raw PSID observations) are participants, and 92.7% of the population (5,524 raw PSID observations) are nonparticipants. All shares within the FSP participant group and the FSP nonparticipant group are weighted using PSID weights. One unit is one household-year, so the same household could be included in the participant group one year and the nonparticipant group in another year depending on whether that household received any food stamps in a given year. The dashed line indicates 1978, the last period before the purchase requirement reform of 1979.

Figure A38: 1979 reform impacts on imputed employment

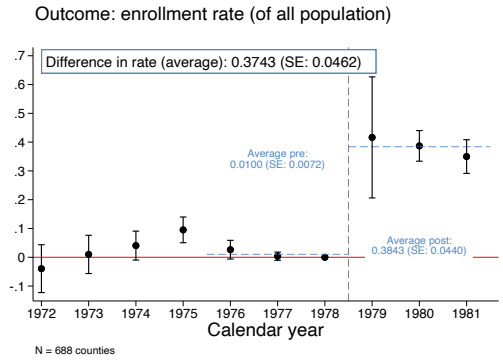
(a) First stage: impact on FSP enrollment (all industries)



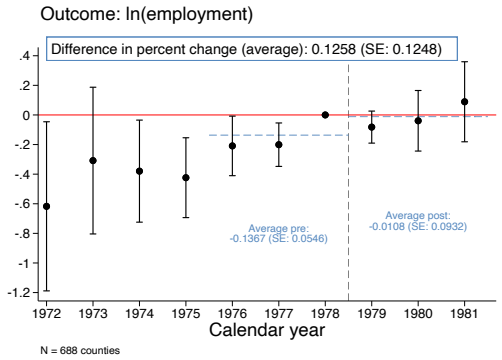
(b) Reduced form: impact on ln(employment) (all industries)



(c) First stage: impact on FSP enrollment (low-wages industry subset)



(d) Reduced form: impact on ln(employment) (low-wage industry subset)



Notes: Panels (a) and (c) show the results of estimating the first stage Equation (10). Panels (b) and (d) show the results of estimating a modified version of the reduced form Equation (11), where the log of the 1970 county population is added as a control on the RHS. Both estimations use a continuous 1972 exposure measure. The exposure measure is winsorized to the 1970 population-weighted percentile of 0 at the bottom of the distribution and the corresponding share at the top of the distribution, ±3.2%. The outcome ln(employment) comes from the imputed County Business Patterns datasets made by Eckert et al. (2020) and Eckert et al. (2022). The low-wage industry subset comprises 1972 Standard Industrial Classification codes 07-- (agricultural services, forestry, and fisheries), 7000 (hotels and other lodging places), and 52-- (retail trade). For both sets of industries (all and low-wage subset), the sample is limited to counties that rolled out FSP by 1970 and that have both FSP participation data and non-zero employment data in every year 1972-1981. For the low-wage industry sample, only county-years with employment data from each of the relevant SIC codes are included. Regressions are all weighted by 1970 county-level population. Standard errors are clustered at the county level.

Table A1: Sample restrictions

Restriction	Counties (N)	Share of 1970 population (%)
All counties	3,143	100%
Exclude counties missing census, participation, or health data in all years	2,876	94.38%
Limit to counties that rolled out FSP by 1970	1,818	71.42%
Panel balanced for participation and birthweight	1,788	71.14%

Notes: Table outlines the sample restrictions imposed upon the full set of U.S. county equivalents. Appendix Table A2 lists the counties that are dropped because they are missing some data element in all years. In addition, as indicated in the last row, a very small number of counties were dropped if they were missing food stamp enrollment or birthweight data in any year from 1972-1984.

Table A2: County equivalents dropped from the analysis sample

State	County-equivalent (count)	Reason
Entire states that lack of county-level participation (217 county equivalents)		
Alaska	29	Lacks rollout timing data
Maine	16	Lacks county-level participation
Oregon	36	Lacks county-level participation
New Hampshire	10	Lacks county-level participation
Rhode Island	5	Lacks county-level participation
Utah	29	Lacks county-level participation
Vermont	14	Lacks county-level participation
West Virginia	55	Lacks county-level participation
Wyoming	23	Lacks county-level participation

State	FIPS	County equivalent	Reason
Participation data reporting errors			
Georgia	13183	Long County	Reporting error
Idaho	16065	Madison County	Reporting error
Indiana	18139	Rush County	Reporting error
New Mexico	35021	Harding County	Reporting error
Virginia	51800	Suffolk City	Reporting error
Additional dropped county equivalents			
Arizona	04012	La Paz County	Lacks census data
Colorado	08014	Boulder County	Lacks census data
Florida	12086	Miami-Dade County	Lacks census data
Hawaii	15005	Honolulu County	Lacks census data
Nevada	32510	Clark County	Lacks census data
New Mexico	35006	Bernalillo County	Lacks census data
Virginia	51515	Bedford city	Lacks census data
Virginia	51550	Chesapeake city	Lacks census data
Virginia	51595	Emporia city	Lacks census data
Virginia	51600	Fairfax city	Lacks census data
Virginia	51620	Franklin city	Lacks census data
Virginia	51678	Lexington city	Lacks census data
Virginia	51683	Manassas city	Lacks census data
Virginia	51685	Manassas Park city	Lacks census data
Virginia	51735	Poquoson city	Lacks census data
Virginia	51775	Salem city	Lacks census data
Wisconsin	55078	Menominee County	Lacks census data
Colorado	08097	Pitkin County	Lacks FSP participation data
Connecticut	09013	Tolland County	Lacks FSP participation data
Connecticut	09015	Windham County	Lacks FSP participation data
Missouri	29186	Ste. Genevieve County	Lacks FSP participation data
New York	36005	Bronx	FSP Participation aggregated
New York	36047	Kings	FSP Participation aggregated
New York	36081	Queens	FSP Participation aggregated
New York	36085	Richmond	FSP Participation aggregated
Arizona	04015	Mohave County	Lacks births data
Arizona	04017	Navajo County	Lacks births data
Arizona	04019	Pima County	Lacks births data
Arizona	04021	Pinal County	Lacks births data
Arkansas	05081	Little River County	Lacks births data
Colorado	08087	Morgan County	Lacks births data
Kansas	20081	Haskell County	Lacks births data
Kansas	20087	Hodgeman County	Lacks births data
Louisiana	22039	Evangeline Parish	Lacks births data
Michigan	26161	Washtenaw County	Lacks births data
Minnesota	27161	Waseca County	Lacks births data
Missouri	29091	Howell County	Lacks births data
Missouri	29161	Phelps County	Lacks births data
Montana	30087	Rosebud County	Lacks births data
Nebraska	31161	Sheridan County	Lacks births data
New York	36121	Wyoming County	Lacks births data

New York	36123	Yates County	Lacks births data
North Dakota	38081	Sargent County	Lacks births data
Tennessee	47105	Loudon County	Lacks births data
Texas	48087	Collingsworth County	Lacks births data

Notes: Counties without complete census data lack population and/or poverty population information for 1960 and/or 1970. FSP participation data aggregates New York City boroughs, so we aggregate census population data similarly. Therefore, Bronx, Kings, Queens, and Richmond are not tracked as individual county equivalents but their population information is summed into one county equivalent.

Table A3: FSP rollout impacts, relative year 0 dummy

	(1) First stage	(2) RF log birthweight	(3) IV log birthweight	(4) RF share low birthweight	(5) IV share low birthweight
Post rollout	0.0763***	0.0000352		-0.000899	
	(0.00226)	(0.00176)		(0.00226)	
Participation rate			0.000459		-0.0117
			(0.0230)		(0.0295)
First-stage F	579.6				
Baseline Mean		8.103	8.103	0.0730	0.0730
Observations	3310026	3302885	3302885	3303089	3303089
Counties	1504	1504	1504	1504	1504

Notes: Sample includes relative years -3 to +2. Table shows estimates of δ from equation 16 in column (1). Columns (2) and (4) show estimates of β from equation 14. Columns (3) and (5) show estimates of λ from equation 15. All regressions include a dummy for relative year 0. The regressions are weighted by dataset-normalized weights. Standard errors clustered at the county level. Since FSP enrollment is only reported annually, relative year corresponds to a calendar year for each county's rollout timing. Since birthweight data can be aggregated to the monthly level, relative year is constructed as the twelve months following each county's rollout timing.

Table A4: FSP rollout impacts, population weights

	(1) First stage	(2) RF log birthweight	(3) IV log birthweight	(4) RF share low birthweight	(5) IV share low birthweight
Post rollout	0.0467***	0.000567		-0.00158*	
	(0.00204)	(0.000594)		(0.000893)	
Participation rate			0.0117		-0.0327*
			(0.0122)		(0.0184)
First-stage F	522.7				
Baseline Mean		8.095	8.095	0.0789	0.0789
Observations	8936287	8916359	8916359	8916905	8916905
Counties	1504	1504	1504	1504	1504

Notes: Sample includes relative years -3 to +2. Table shows estimates of δ from equation 16 in column (1). Columns (2) and (4) show estimates of β from equation 14. Columns (3) and (5) show estimates of λ from equation 15. The regressions are weighted by county population in 1960. Standard errors clustered at the county level. Since FSP enrollment is only reported annually, relative year corresponds to a calendar year for each county's rollout timing. Since birthweight data can be aggregated to the monthly level, relative year is constructed as the twelve months following each county's rollout timing.

Table A5: FSP rollout impacts, 5-year pre- and post-period

	(1) First stage	(2) RF log birthweight	(3) IV log birthweight	(4) RF share low birthweight	(5) IV share low birthweight
Post rollout	0.0525***	0.000211		-0.00148	
	(0.00166)	(0.00149)		(0.00193)	
Participation rate			0.00401		-0.0281
			(0.0283)		(0.0368)
First-stage F	995.5				
Baseline Mean		8.103	8.103	0.0730	0.0730
Observations	3310026	3302885	3302885	3303089	3303089
Counties	1504	1504	1504	1504	1504

Notes: Sample includes relative years -5 to +4. Table shows estimates of δ from equation 16 in column (1). Columns (2) and (4) show estimates of β from equation 14. Columns (3) and (5) show estimates of λ from equation 15. The regressions are weighted by dataset-normalized weights. Standard errors clustered at the county level. Since FSP enrollment is only reported annually, relative year corresponds to a calendar year for each county's rollout timing. Since birthweight data can be aggregated to the monthly level, relative year is constructed as the twelve months following each county's rollout timing.

Table A6: FSP rollout impacts by race, stacked dataset weights, relative year zero dummy

	(1) First stage	(2) RF log birthweight	(3) IV log birthweight	(4) RF share low birthweight	(5) IV share low birthweight
Panel A: Black sample					
Post rollout	0.0898***	0.00398		-0.0126*	
	(0.00370)	(0.00444)		(0.00657)	
Participation rate (imputed)			0.0443		-0.140*
			(0.0496)		(0.0737)
First-stage *F*	301.5				
Baseline Mean		8.031	8.031	0.133	0.133
Observations	1148298	1148298	1148298	1148298	1148298
Counties	634	634	634	634	634
Panel B: White sample					
Post rollout	0.0171***	-0.000332		-0.000247	
	(0.000557)	(0.00193)		(0.00247)	
Participation rate (imputed)			-0.0194		-0.0144
			(0.113)		(0.144)
First-stage *F*	492.6				
Baseline Mean		8.111	8.111	0.0658	0.0658
Observations	3235041	3235041	3235041	3235041	3235041
Counties	1490	1490	1490	1490	1490

Notes: Sample includes relative years -3 to +2 for the nonwhite and white samples separately. Table shows estimates of δ from equation 16 in column (1). Columns (2) and (4) show estimates of β from equation 14. Columns (3) and (5) show estimates of λ from equation 15. The regressions are weighted by stacked dataset weights. Standard errors clustered at the county level. All regressions include a dummy for relative year 0. Since FSP enrollment is only reported annually, relative year corresponds to a calendar year for each county's rollout timing. Since birthweight data can be aggregated to the monthly level, relative year is constructed as the twelve months following each county's rollout timing.

Table A7: FSP rollout impacts by race, population weights

	(1) First stage	(2) RF log birthweight	(3) IV log birthweight	(4) RF share low birthweight	(5) IV share low birthweight
Panel A: Black sample					
Post rollout	0.0481***	0.00225		-0.00361	
	(0.00524)	(0.00202)		(0.00311)	
Participation rate (imputed)			0.0439		-0.0703
			(0.0391)		(0.0607)
First-stage *F*	84.2				
Baseline Mean		8.028	8.028	0.139	0.139
Observations	3297298	3297078	3297078	3297078	3297078
Counties	634	634	634	634	634
Panel B: White sample					
Post rollout	0.00876***	0.000180		-0.00121	
	(0.000531)	(0.000605)		(0.000910)	
Participation rate (imputed)			0.0203		-0.137
			(0.0685)		(0.103)
First-stage *F*	271.9				
Baseline Mean		8.106	8.106	0.0683	0.0683
Observations	8733672	8733375	8733375	8733375	8733375
Counties	1490	1490	1490	1490	1490

Notes: Sample includes relative years -3 to +2 for the nonwhite and white samples separately. Table shows estimates of δ from equation 16 in column (1). Columns (2) and (4) show estimates of β from equation 14. Columns (3) and (5) show estimates of λ from equation 15. The regressions are weighted by 1960 race-specific population. Standard errors clustered at the county level. Since FSP enrollment is only reported annually, relative year corresponds to a calendar year for each county's rollout timing. Since birthweight data can be aggregated to the monthly level, relative year is constructed as the twelve months following each county's rollout timing.

Table A8: FSP rollout impacts by race, population weights, relative year zero dummy

	(1) First stage	(2) RF log birthweight	(3) IV log birthweight	(4) RF share low birthweight	(5) IV share low birthweight
Panel A: Black sample					
Post rollout	0.0742***	0.00250		-0.00330	
	(0.00373)	(0.00239)		(0.00375)	
Participation rate (imputed)			0.0337		-0.0445
			(0.0324)		(0.0507)
First-stage *F*	212.8				
Baseline Mean		8.028	8.028	0.139	0.139
Observations	3297078	3297078	3297078	3297078	3297078
Counties	634	634	634	634	634
Panel B: White sample					
Post rollout	0.0132***	-0.0000375		-0.00113	
	(0.000604)	(0.000774)		(0.00119)	
Participation rate (imputed)			-0.00284		-0.0859
			(0.0587)		(0.0898)
First-stage *F*	238.8				
Baseline Mean		8.106	8.106	0.0683	0.0683
Observations	8733375	8733375	8733375	8733375	8733375
Counties	1490	1490	1490	1490	1490

Notes: Sample includes relative years -3 to +2 for the nonwhite and white samples separately. Table shows estimates of δ from equation 16 in column (1). Columns (2) and (4) show estimates of β from equation 14. Columns (3) and (5) show estimates of λ from equation 15. All regressions include a dummy for relative year 0. The regressions are weighted by 1960 race-specific population. Standard errors clustered at the county level. Since FSP enrollment is only reported annually, relative year corresponds to a calendar year for each county's rollout timing. Since birthweight data can be aggregated to the monthly level, relative year is constructed as the twelve months following each county's rollout timing.

Table A9: Birthweight – AHS and replication

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	AHS				Replication			
White:								
Average FSP (0/1)	2.039	2.635	2.089	2.175	2.0305	2.2701	1.8597	2.1877
(SE)	(0.947)	(0.896)	(1.039)	(0.975)	0.9474	0.9161	1.0379	1.0405
Observations	97,785	97,785	97,785	97,785	96156.0	96156.0	96096.0	96096.0
Mean of dependent variable	3,350	3,350	3,350	3,350	3350.6229	3350.6229	3350.6116	3350.6116
Black:								
Average FSP (0/1)	3.454	4.120	5.466	1.665	2.8028	3.6793	4.7631	1.6541
(SE)	(2.660)	(2.317)	(2.579)	(2.330)	2.7507	2.3606	2.5797	2.3504
Observations	27,374	27,374	27,374	27,374	26726.0	26726.0	26667.0	26669.0
Mean of dependent variable	3,097	3,097	3,097	3,097	3097.2587	3097.2587	3097.2126	3097.2207
Controls included:								
1960 CCDB × linear time	X	X	X		X	X	X	
REIS controls	X	X	X	X	X	X	X	X
County per capita real income	X	X	X	X	X	X	X	X
Year-quarter fixed effects	X	X	X	X	X	X	X	X
County fixed effects	X	X	X	X	X	X	X	X
State × linear time		X				X		
State × year fixed effects			X				X	
County × linear time				X				X

Notes: Each parameter is from a separate regression of the outcome variable on the food stamp implementation dummy. The treatment is assigned as of three months prior to birth (proxy for beginning of the third trimester). The estimation sample includes means by race-county-quarter for years including 1968 through 1977 where cells with fewer than 25 births are dropped. In addition to the fixed effects, controls include 1960 county variables (log of population, percentage of land in farming, percentage of population black, urban) each interacted with a linear time trend, per capita county transfer income (public assistance, medical care, and retirement and disability benefits), and county real per capita income. Estimates are weighted using the number of births in the cell and are clustered on county. Standard errors are in parentheses. Note that race group is determined by the race of the mother in each birth observation.

Table A10: Share low birthweight – AHS and replication

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	AHS				Replication			
White:								
Average FSP (0/1)	-0.0006	-0.0006	-0.0006	-0.0006	-0.0006	-0.0005	-0.0005	-0.0005
(SE)	(0.0003)	(0.0003)	(0.0003)	(0.0004)	0.0003	0.0003	0.0003	0.0004
Observations	97,785	97,785	97,785	97,785	96156.0	96156.0	96096.0	96096.0
Mean of dependent variable	0.06	0.06	0.06	0.06	0.0646	0.0646	0.0646	0.0646
Black:								
Average FSP (0/1)	-0.0015	-0.0016	-0.0019	-0.0009	-0.0012	-0.0015	-0.0015	-0.0008
(SE)	(0.0010)	(0.0010)	(0.0012)	(0.0012)	0.0010	0.0011	0.0012	0.0012
Observations	27,374	27,374	27,374	27,374	26726.0	26726.0	26667.0	26669.0
Mean of dependent variable	0.13	0.13	0.13	0.13	0.1304	0.1304	0.1305	0.1305
Controls included:								
1960 CCDB × linear time	X	X	X		X	X	X	
REIS controls	X	X	X	X	X	X	X	X
County per capita real income	X	X	X	X	X	X	X	X
Year-quarter fixed effects	X	X	X	X	X	X	X	X
County fixed effects	X	X	X	X	X	X	X	X
State × linear time		X				X		
State × year fixed effects			X				X	
County × linear time				X				X

Notes: Each parameter is from a separate regression of the outcome variable on the food stamp implementation dummy. The treatment is assigned as of three months prior to birth (proxy for beginning of the third trimester). The estimation sample includes means by race-county-quarter for years including 1968 through 1977 where cells with fewer than 25 births are dropped. In addition to the fixed effects, controls include 1960 county variables (log of population, percentage of land in farming, percentage of population black, urban) each interacted with a linear time trend, per capita county transfer income (public assistance, medical care, and retirement and disability benefits), and county real per capita income. Estimates are weighted using the number of births in the cell and are clustered on county. Standard errors are in parentheses. Note that race group is determined by the race of the mother in each birth observation.

Table A11: Birthweight, AHS specification to stacked dataset estimates

	AHS Replication									Stacked Dataset							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
A: White																	**Baseline**
FSP	2.0305	2.2701	1.8597	2.1877	0.5873	-0.3973	0.7654	1.2160	0.5415	2.1515	0.5227	-1.0781	-6.1652	1.2124	-1.8668	2.1310	-0.3590
(SE)	0.9334	0.9025	1.0379	1.0405	0.9884	1.4582	1.8286	1.3976	1.5344	1.6291	1.9398	3.1411	3.8547	1.9047	5.2223	1.8233	5.3984
Observations	96096	96096	96096	96096	96336	47903	27628	48064	27716	5696449	3414521	4492357	2692145	8733375	3235041	8733375	3235041
Mean of dependent variable	3350.6116	3350.6116	3350.6116	3350.6116	3350.5244	3349.4646	3348.0520	3349.3810	3347.9014	3336.0420	3340.7999	3336.1364	3340.8502	3336.4970	3336.6498	3336.4970	3336.6498
B: Black																	
FSP	2.8028	3.6793	4.7631	1.6541	3.8146	4.6249	3.0124	3.8997	4.8083	6.0398	7.1010	13.8975	14.7988	7.7961	10.5709	8.4461	15.6713
(SE)	2.7024	2.3190	2.5797	2.3505	4.0782	3.5455	4.8642	3.0116	3.9201	5.4158	5.5845	7.4041	8.7452	5.2478	10.4876	5.1296	10.6761
Observations	26669	26669	26667	26669	26841	11569	6674	11667	6733	1382615	857719	976590	605469	3297078	1148298	3297078	1148298
Mean of dependent variable	3097.2207	3097.2207	3097.2126	3097.2207	3097.0414	3096.9879	3094.1629	3096.8027	3094.0635	3099.0406	3096.1352	3099.0220	3096.1210	3099.2244	3099.2454	3099.2244	3099.2454
Specification details:																	
Rollout July 1969 or later only						X	X	X	X	X	X	X	X	X	X	X	X
Data relative years -3 to +2							X		X		X		X	X	X	X	X
Birth count weights	X	X	X	X	X	X	X	X	X	X	X			X		X	
Baseline weights												X	X		X		X
Baseline sample limitations														X	X	X	X
Baseline treatment definition																X	X
Controls included:																	
Time, County FEs	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
1960 CCDB linear time	X	X	X			X	X										
REIS controls	X	X	X	X		X	X										
County per capita real income	X	X	X	X		X	X										
Year-quarter fixed effects	X	X	X	X		X	X										
County fixed effects	X	X	X	X		X	X										
State linear time		X															
State year fixed effects			X			X	X										
County linear time				X													

Notes: Each parameter is from a separate regression of the outcome variable on the food stamp implementation dummy. The treatment is assigned as of three months prior to birth (proxy for beginning of the third trimester) in columns (1)-(15) and as of FSP rollout in columns (16)-(17). The estimation sample is race-county-quarter for years including 1968 through 1977 where cells with fewer than 25 births are dropped. This differs including/after column (10) in the stacked dataset regressions which use sample at the race-county-month due to the stacking based upon unique rollout month. Specifically, columns (10)-(17) use stacked datasets. Similarly, unique quarter and county fixed effects are included across regressions, except for stacked dataset regressions which include unique month-by-dataset and fips-by-dataset fixed effects instead. Additional controls included in columns (3) and (6), (7) include state-year fixed effects, 1960 county variables (log of population, percentage of land in farming, percentage of population black, urban) each interacted with a linear time trend, per capita county transfer income (public assistance, medical care, and retirement and disability benefits), and county real per capita income. Additional controls used across AHS replication columns (1), (2), and (4) are also detailed in the table rows describing each specification. Estimates are all clustered on county. Standard errors are in the row directly below the estimate. Note that race group is determined by the race of the mother in each birth observation.

Table A12: Share low birthweight, AHS specification to stacked dataset estimates

	AHS Replication									Stacked Dataset							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
A: White																	**Baseline**
FSP	-0.00056	-0.00051	-0.00049	-0.00054	-0.00033	0.00026	-0.00011	-0.00025	-0.00071	-0.00065	-0.00128	-0.00025	0.00098	-0.00162	0.00036	-0.00202	-0.00088
(SE)	0.00027	0.00029	0.00030	0.00038	0.00031	0.00043	0.00078	0.00042	0.00061	0.00059	0.00085	0.00102	0.00148	0.00087	0.00205	0.00085	0.00209
Observations	96096	96096	96096	96096	96336	47903	27628	48064	27716	5696449	3414521	4492357	2692145	8733375	3235041	8733375	3235041
Mean of dependent variable	0.0645	0.0645	0.0645	0.0645	0.0646	0.0645	0.0646	0.0645	0.0646	0.0669	0.0657	0.0668	0.0657	0.0662	0.0662	0.0662	0.0662
B: Black																	
FSP	-0.00123	-0.00147	-0.00154	-0.00077	-0.00188	-0.00043	-0.00038	-0.00054	-0.00116	0.00017	-0.00077	-0.00241	-0.00202	-0.00190	-0.00971	-0.00256	-0.01155
(SE)	0.00102	0.00105	0.00118	0.00122	0.00114	0.00189	0.00259	0.00161	0.00222	0.00193	0.00283	0.00397	0.00471	0.00273	0.00542	0.00269	0.00540
Observations	26669	26669	26667	26669	26841	11569	6674	11667	6733	1382615	857719	976590	605469	3297078	1148298	3297078	1148298
Mean of dependent variable	0.1305	0.1305	0.1305	0.1305	0.1305	0.1306	0.1314	0.1307	0.1315	0.1316	0.1308	0.1316	0.1308	0.1318	0.1317	0.1318	0.1317
Specification details:																	
Rollout July 1969 or later only						X	X	X	X	X	X	X	X	X	X	X	X
Data relative years -3 to +2							X		X		X		X	X	X	X	X
Birth count weights	X	X	X	X	X	X	X	X	X	X	X			X		X	
Baseline weights												X	X		X		X
Baseline sample limitations														X	X	X	X
Baseline treatment definition																X	X
Controls included:																	
Time, County FEs	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
1960 CCDB linear time	X	X	X			X	X										
REIS controls	X	X	X	X		X	X										
County per capita real income	X	X	X	X		X	X										
Year-quarter fixed effects	X	X	X	X		X	X										
County fixed effects	X	X	X	X		X	X										
State linear time		X															
State year fixed effects			X			X	X										
County linear time				X													

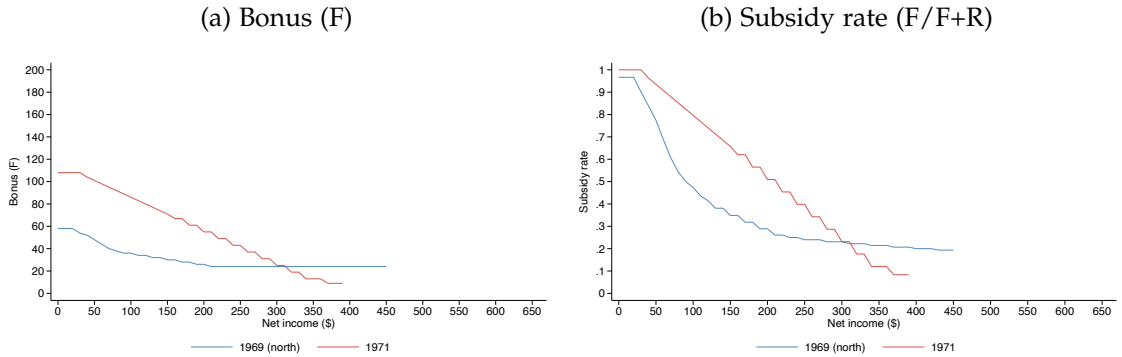
Notes: Each parameter is from a separate regression of the outcome variable on the food stamp implementation dummy. The treatment is assigned as of three months prior to birth (proxy for beginning of the third trimester) in columns (1)-(15) and as of FSP rollout in columns (16)-(17). The estimation sample is race-county-quarter for years including 1968 through 1977 where cells with fewer than 25 births are dropped. This differs including/after column (10) in the stacked dataset regressions which use sample at the race-county-month due to the stacking based upon unique rollout month. Specifically, columns (10)-(17) use stacked datasets. Similarly, unique quarter and county fixed effects are included across regressions, except for stacked dataset regressions which include unique month-by-dataset and fips-by-dataset fixed effects instead. Additional controls included in columns (3) and (6), (7) include state-year fixed effects, 1960 county variables (log of population, percentage of land in farming, percentage of population black, urban) each interacted with a linear time trend, per capita county transfer income (public assistance, medical care, and retirement and disability benefits), and county real per capita income. Additional controls used across AHS replication columns (1), (2), and (4) are also detailed in the table rows describing each specification. Estimates are all clustered on county. Standard errors are in the row directly below the estimate. Note that race group is determined by the race of the mother in each birth observation.

Table A13: Ln(birthweight), AHS specification to stacked dataset estimates

							Stacked Dataset							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
A: White														**Baseline**
FSP	0.0006	0.0002	-0.0001	0.0003	0.0004	0.0002	0.0007	0.0002	-0.0003	-0.0019	0.0004	-0.0006	0.0006	-0.0002
(SE)	0.0003	0.0003	0.0004	0.0006	0.0004	0.0005	0.0005	0.0006	0.0009	0.0012	0.0006	0.0016	0.0006	0.0017
Observations	96096	96336	47903	27628	48064	27716	5696449	3414521	4492357	2692145	8733375	3235041	8733375	3235041
Mean of dependent variable	8.1164	8.1164	8.1161	8.1156	8.1160	8.1156	8.1110	8.1125	8.1110	8.1125	8.1099	8.1099	8.1099	8.1099
B: Black														
FSP	0.0016	0.0013	0.0015	0.0010	0.0013	0.0016	0.0018	0.0023	0.0045	0.0048	0.0025	0.0030	0.0027	0.0047
(SE)	0.0008	0.0013	0.0011	0.0016	0.0010	0.0013	0.0018	0.0018	0.0025	0.0029	0.0017	0.0037	0.0017	0.0037
Observations	26667	26841	11569	6674	11667	6733	1382615	857719	976590	605469	3297078	1148298	3297078	1148298
Mean of dependent variable	8.0374	8.0374	8.0373	8.0364	8.0373	8.0364	8.0359	8.0350	8.0358	8.0350	8.0322	8.0322	8.0322	8.0322
Specification details:														
Rollout July 1969 or later only			X	X	X	X	X	X	X	X	X	X	X	X
Data relative years -3 to +2				X		X		X		X	X	X	X	X
Birth count weights	X	X	X	X	X	X	X	X			X		X	
Baseline weights									X	X		X		X
Baseline sample limitations											X	X	X	X
Baseline treatment definition													X	X
Controls included:														
Time, County FEs	X	X	X	X	X	X	X	X	X	X	X	X	X	X
1960 CCDB linear time	X		X	X										
REIS controls	X		X	X										
County per capita real income	X		X	X										
Year-quarter fixed effects	X		X	X										
County fixed effects	X		X	X										
State year fixed effects	X		X	X										

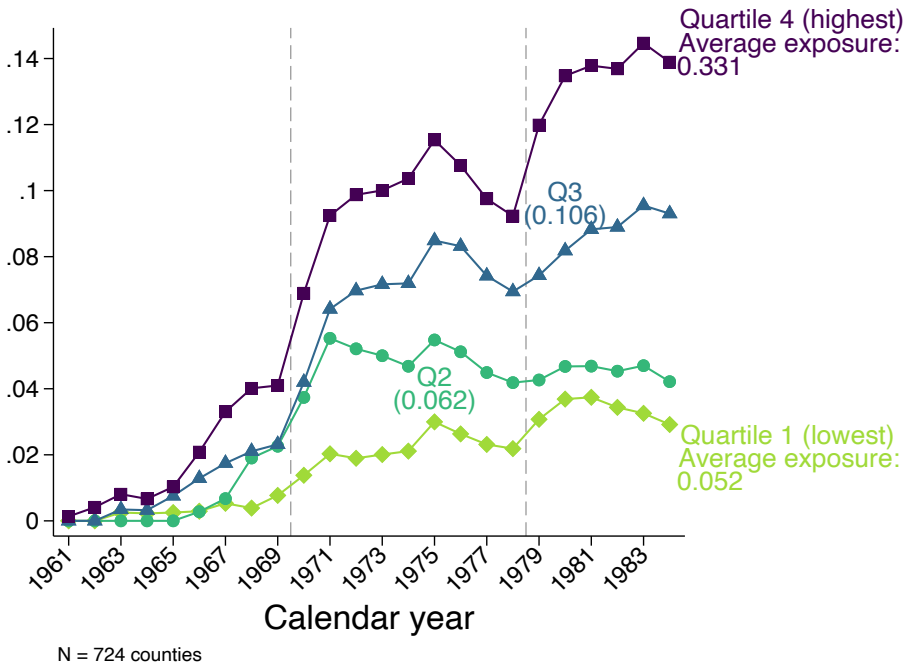
Each parameter is from a separate regression of the outcome variable on the food stamp implementation dummy. The treatment is assigned as of three months prior to birth (proxy for beginning of the third trimester) in columns (1)-(12) and as of FSP rollout in columns (13)-(14). The estimation sample is race-county-quarter for years including 1968 through 1977 where cells with fewer than 25 births are dropped. This differs including/after column (7) in the stacked dataset regressions which use sample at the race-county-month due to the stacking based upon unique rollout month. Specifically, columns (7)-(14) use stacked datasets. Similarly, unique quarter and county fixed effects are included across regressions, except for stacked dataset regressions which include unique month-by-dataset and fips-by-dataset fixed effects instead. Additional controls included in columns (1) and (3), (4) include state-year fixed effects, 1960 county variables (log of population, percentage of land in farming, percentage of population black, urban) each interacted with a linear time trend, per capita county transfer income (public assistance, medical care, and retirement and disability benefits), and county real per capita income. Column (1) specifications are analogous to column (3) and (7) in the replication of AHS Table 1, but the authors do not use a logged birthweight outcome. Estimates are all clustered on county. Standard errors are in the row directly below the estimate. Note that race group is determined by the race of the mother in each birth observation.

Figure A39: Benefit schedules transfers and subsidy rate



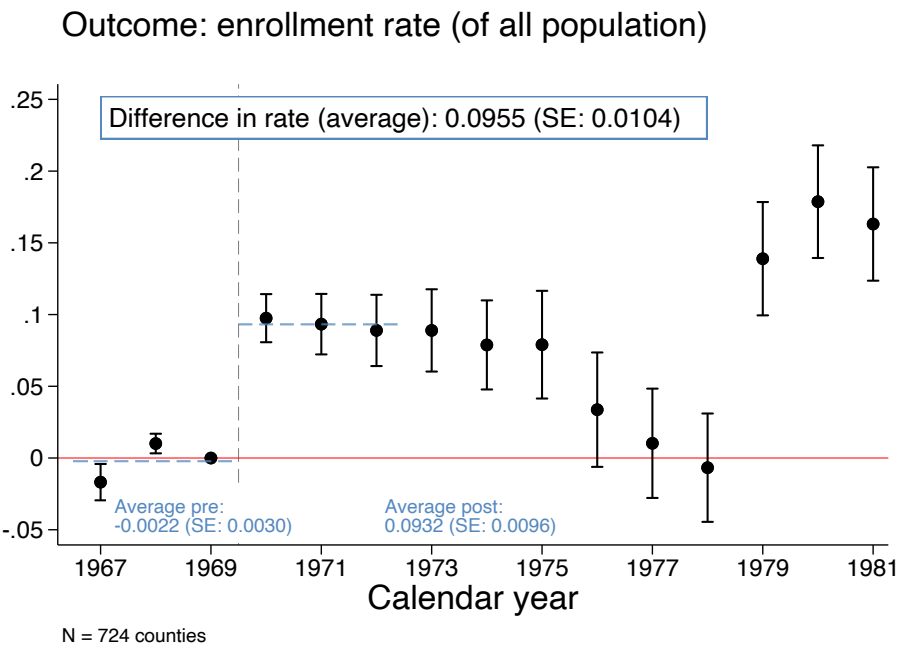
Notes: Panel (a) shows the bonus/transfer, in dollars, across benefits schedules in years 1969 to 1971 for a family of four. Panel (b) shows the subsidy rate across these years. Income bins have not been adjusted and thus the figures reflect the benefits received by a family of four within each income bin in terms of a given year's dollars. There were separate benefits schedules for the North and the South, and this figure depicts the schedule that applied to the North. Note that each point represents a $10-wide net income bin. For example, in panel (a), the point at net income $300 shows the bonus, in dollars, received by a family of four with monthly net income between $290.00-$299.99.

Figure A40: FSP participation by quartile of 1968 exposure measure



Notes: Figures show population-weighted average county-level outcomes over time by population-weighted quartile of the 1968 exposure measure. Darker shades in the series color scale correspond with increasing quartile of exposure. Vertical lines indicate the final pre-period before the 1970 increase in subsidization rate and the 1979 purchase requirement reform. The average exposure measure within each quartile is displayed within series labels. The exposure measure is winsorized the 99th percentile Sample is limited to counties where FSP was rolled out 1967 or earlier, and further limited to the panel that is balanced for the participation and birthweight variables (724 counties). Note that weighted quartiles are defined over the panel-specific defined set of counties.

Figure A41: 1970 subsidy increase first stage



Notes: Figure shows the results of estimating the first stage equation (10) using a continuous 1968 exposure measure on FSP enrollment, 1969 baseline year, and years-since-rollout-by-calendar year fixed effects. The 1968 exposure measure is winsorized at 99th percentile. Sample is limited to counties that rolled out in 1967 or earlier, and further limited to the panel that is balanced for the participation and birthweight variables (724 counties). Regressions are all weighted by 1960 county-level population. Standard errors are clustered at the county level.