In-Kind Benefits Benefits

Evidence from Puerto Rico

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October 29, 2019

We examine the effect of providing benefits in-kind versus in cash. We leverage a policy in Puerto Rico that converted cash benefits to in-kind nutritional assistance, holding benefit generosity constant. Using a difference-in-differences strategy, we find that providing the benefits in-kind led to significant increases in food consumption and decreases in maternal anemia, but had no impact on the rate of gestational diabetes or the incidence of low birth weight. Leveraging variation in the length of early childhood exposure to in-kind versus cash benefit provision, we find long run improvements in adolescent health.

1 Introduction

While basic economics suggests that the provision of benefits in cash is efficient, most benefits are distributed in-kind. Economic rationales for this choice range from the presence of consumption externalities to political economy concerns to paternalism. In the context of nutritional assistance, which accounts for nearly 100 billion dollars annually in the United States, motivations for in-kind provision center around paternalistic concerns related to individual optimization failures and agency problems within the family that might result in expenditure choices that fail to maximize maternal or child well-being. Of further concern are the potential for consumption externalities, perhaps driven by short- or long-term health effects, that could vary depending on the form of the benefit. A necessary condition for the legitimacy of these rationales is that the provision of benefits in-kind alters expenditure patterns and generates positive consumption externalities relative to the provision of cash.

In this paper, we ask whether the form of a government benefit affects consumption patterns and, if so, whether these changes are significant enough to generate improvements in health outcomes. This question is difficult to answer given limited variation in the form of nutritional assistance over time and space. We take advantage of a previously unexplored change in the Puerto Rican Nutritional Assistance Program (NAP) that converted a cash benefit to one in which recipients were required to spend 75% of their benefits on approved food items. As the total value of the benefit remained fixed, this shift allows us to isolate the impact of providing food relative an equivalent amount of cash. The program's high participation rate, with more than one third of residents receiving assistance, makes Puerto Rico an ideal setting to study this type of shift.

While a significant body of work has demonstrated the short- and long-term effects of nutritional assistance on consumption patterns, nutrition, crime, and health, this work focuses on the *availability* of nutritional assistance and not the form.[1] Research on the effects

[1]See, for example, Gertler (2004); Fox, Hamilton and Lin (2004); Lee and Mackey-Bilaver (2006); Whitake, Philips and Orzol (2006); Baum (2007); Hoynes and Schanzenbach (2009); Almond, Hoynes and Schanzenbach (2011); Hoynes and Schanzenbach. (2012); Hoynes, Schanzenbach and Almond (2016); Hastings, Kessler

of the form of the benefit is significantly more limited. Evaluations of randomized cash out experiments conducted by the U.S. Department of Agriculture in the 1990s provide mixed evidence, with three out of four sites implying a reduction in food expenditures of between 18 and 28 cents for each dollar of food stamps cashed out (Fraker, Martini and Ohls, 1995; Ohls and Bernson, 1992; Whitmore, 2002).[2] Cunha (2014) evaluates a randomized control trial of the Mexican government's Food Assistance Program ('PAL') in which poor rural villages were randomly assigned baskets of goods or their cash equivalents. While there is no significant difference in the overall effect of the form of the benefit, the point estimates suggest that in-kind provision resulted in food consumption that was 40% higher overall, with expenditures on basket items more than 200% higher.

Closely related in context to this paper, Moffitt (1989) explores the effect of replacing food stamps with paper checks island wide in Puerto Rico in 1982, finding no effect of the cash out on food expenditures. As with the previously mentioned Mexican and USDA experiments, though, the generosity of the benefits was low and the transfer was inframarginal for most (92 percent of) households, suggesting that large differences in food expenditures should not be expected, at least under neoclassical assumptions.[3] More recent evidence from Hastings and Shapiro (2018) indicates that the neoclassical assumptions may not be appropriate because individuals appear to earmark nutritional assistance for the consumption of food. Consistent with this, they find that the marginal propensity to consume food (MPCF) out of SNAP benefits is substantially larger than out of cash, consistent with much of the earlier evidence from randomized control trials conducted by the USDA.

We make three contributions to this literature. First, we distinguish between effects of

and Shapiro (2018); Barr and Smith (2018).

[2] Fraker, Martini and Ohls (1995) argue that the cash out in Alabama had no effect on food expenditures due to the significantly shorter nature of the cash out (8 months versus 4 years) and the provision of food stamp benefits separate from other benefits (in the other sites the "cash" food stamp benefits were combined with other cash transfers such as AFDC).

[3] Moffitt also notes that changing selection into benefit receipt (generated by substantial reductions in funding for the program, changes in resource thresholds for eligibility, and changes in benefit generosity) could explain the lack of changes in food consumption. As with food stamps, there was also evidence of a black market for food stamps which limited the extent to which the benefits would constrain spending and thus reduced the effect of the cash out.

the availability of nutritional assistance and the form of the benefit in a context where the benefit is not inframarginal for the majority of recipients. Second, we do so in a large-scale context with a permanent change in benefit structure; 30-40% of Puerto Ricans receive food assistance and the benefit structure changed permanently and island-wide in 2001. The effects of a system-wide and permanent shift may differ from those observed in small or short-term experiments. Finally, we provide the only estimates of which we are aware that the provision of nutritional assistance in-kind (relative to cash) can generate positive effects on measures of short- and long-term health.

While prior evidence suggests that the FSP increased food consumption and improved short- and long-term health outcomes, it is not clear whether these effects were driven, at least partially, by providing benefits as stamps, or whether an equivalent increase in income would have generated the same effects. To shed light on this question, we turn to a natural experiment where Puerto Rico converted a cash benefit to one in which recipients were required to spend 75% of their benefits on approved food items. Here, we can examine the impact of constraining the form of the benefit in the absence of any shock to overall income. According to a simple model of household consumption decisions, this transition should increase food consumption for households with *ex ante* food consumption below the in-kind benefit amount.[4] For households with *ex ante* food consumption that exceeds the in-kind amount, neo-classical economic predictions would suggest that the transition should have no effect, although recent evidence suggests the mental accounting may result in increased food purchases, particularly because the benefits were specifically earmarked for food following the benefit shift (Hastings and Shapiro, 2018).

Using a difference-in-differences strategy, we find that providing the benefits in-kind increases fruit and vegetable consumption by 0.5 servings per day (20 percent). These improvements in nutrition led to decreases in maternal anemia of 0.3-0.7 pp (13-23 percent), but had no impact on the rate of diabetes or the incidence of low birth weight among mothers.

[4] It will also decrease consumption of other goods (unless households responded to the change by increasing labor supply).

We explore effects on longer-term health outcomes by comparing individuals with different exposures to the in-kind benefits (and presumably improved early childhood nutrition) to analogous cohorts in untreated states. Those who grew up after the shift are taller and more likely to be normal weight as adolescents.

While the results provide compelling evidence that the form of the benefit can matter, the strength of the evidence is limited by the nature of the variation. The estimates rely critically on a comparison of outcomes across time in Puerto Rico and how those outcomes evolved relative to outcomes in sets of comparison states in the U.S.

It is important to note that our results do not imply that in-kind benefits are welfare improving over cash, but rather that individual spending may not prioritize health, particularly of young children, under a cash based system. This may have additional implications for the long run social costs of cash vs in-kind benefits, particularly when low income individuals have medical costs that are covered or subsidized by the government.

In the next section we provide additional detail on the transition in benefit structure in Puerto Rico as well as anecdotal evidence of the effect of this transition on food consumption. In Section 3, we describe the data sources used to capture food consumption, and maternal, infant, and adolescent health. Section 3 outlines the empirical strategy and Section 4 contains results. We conclude the paper with a discussion of the broad takeaways from this and other literature that explores the effects of cash versus in-kind benefit provision.

2 In-Kind Transition in Puerto Rico's Nutrition Assistance Program

In September 2001, in an effort to align more closely with federal regulations, the Puerto Rican NAP transitioned from a 100 percent cash redeemable EBT card, to one for which 75 percent of the benefits had to be spent on approved food items (Trippe et al., 2015). While 25 percent of the benefit remained redeemable in cash, the government for the first

time made it clear that 100 percent of the benefit was intended for food.[5] This shift came about as a result of pressure from the U.S. mainland to "align NAP more closely with the regulations that governed SNAP; specifically, regulations on what and where benefits could be used" (ADSEF, 2000). This pressure was motivated by a desire to "encourage and enforce the utilization of the benefit as established in Federal law and regulations — only for the acquisition of food". Interestingly, those in charge of the program expected this change to "dramatically reduce improper use of targeted funds" and "promote better nutrition for... participants" (ADSEF 2001b, p.7).

Unlike in the early 1980s benefit shift studied by Moffitt (1989), the in-kind component of the benefits after 2001 was not inframarginal for most Puerto Ricans. The newly mandated in-kind component of the benefits accounted for the majority of resources in 72 percent of recipient households, while between 35 and 40 percent of recipient households had *no other source* of income.benefits accounted for the majority of resources in 72 percent of recipient households, while between 35 and 40 percent of recipient households had *no other source* of income. Since the average Puerto Rican household spends only 18 percent (and the average welfare recipient spends only 21 percent) of their income on food, this suggests that a large portion of NAP recipients were forced to spend more on food as a result of the 2001 benefit shift.[6] The potential for trafficking of benefits to circumvent this constraint also was dramatically different in 2001 because the earlier adoption of EBT cards reduced the potential for benefit trafficking after the benefit shift.[7]

Individual expectations support the notion that the constraint was binding, with many recipients claiming that the benefit shift would force them to consume additional food. As one young mother put it "I'm going to have the cupboard full, but I will not have the light to cook what I bought... That's logical, they know it, I do not have any more money... "

[5] 25 percent was kept in cash due to concerns that some Puerto Ricans would not be able to access an approved retailer for all of their purchases.

[6] Authors' calculations using data from (Trippe et al., 2015).

[7] Consistent with this, part of the motivation for the 2001 benefit shift was to "dramatically reduce improper use of targeted funds."

Indeed, there is ample evidence from a number of sources that her case was not unique.[8] Consistent with individual expectations, food industry revenue estimates at the time imply a roughly 40 to 60 percent increase in food expenditures for NAP recipients as a result of the benefit shift.[9]

Individual reports from after the benefit shift also indicate that the food assistance was not inframarginal. Surveys of NAP recipients indicate that despite the requirement to spend 100 percent of their benefit on food (including the 25 percent cash component), only 32 percent report using any of the cash benefit on food, suggesting at least 68 percent were constrained.[10] Similarly, EBT expenditure data from after the benefit shift indicate that while over 60 percent of NAP recipients spend roughly 75 percent of their benefit on food (the in-kind share of the benefit), only 6 percent spend something in between 80 and 99 percent of their benefit on food.[11] This level of bunching suggests that many households were constrained in their expenditures on food.

3 Data

We turn to the Behavioral Risk Factor Surveillance System (BRFSS) to explore whether the expected changes in food consumption show up in terms of contemporaneous changes in nutritional intake. We use Vital Statistics Natality data to determine whether any observed changes in nutrition affect health during the critical window surrounding childbirth. Fi-

[8] Gotay, Benjamin Torres. "Empieza hoy el nuevo sistema de uso del PAN." *El Nuevo Dia* 1 Sep. 2001 (translated to English).

[9] The president of the Association of Marketing, Industry and Food Distribution (MIDA) indicated that "the economic boom experienced by the food industry is the result of a fundamental factor: the modifications to the Nutritional Assistance Program (PAN) under the formula 75-25 through the Family Card." A MIDA report suggests that the benefit shift increased food sector revenue by $300 to $400 million, 22.5 to 30 percent of the total PAN budget. Given that roughly 80 percent of benefits were spent on food after the shift, we obtain a rough estimate of the increase in food expenditure among recipients by dividing the $300 to $400 million by the implied amount spent on food prior to the shift, calculated as the share of total PAN expenditures spent on food (0.8*$1335 million), minus the estimated increase in food sector revenue ($300 to $400 million). Romn, Miguel Daz. "Prspera la industria de alimentos." 22 Oct. 2001 (translated to English).

[10] Rosado-Gonzlez, R., Puerto Rico Department of the Family, Administration for Socioeconomic Development of the Family (ADSEF). (2008). PANECO pregunta. Trujillo Alto, PR: RRG Universe and Assoc.

[11] Trippe et al. (2015) "Examination of Cash Nutrition Assistance Program Benefits in Puerto Rico." Prepared by Insight Policy Research under Contract No. AG-3198-C-14-0006. Alexandria, VA: U.S. Department of Agriculture, Food and Nutrition Service.

nally, we use the Youth Risk Behavior Survey (YRBS) to look for long run health effects on teenagers who experienced different early childhood exposure to in-kind versus cash benefit provision.

3.1 Behavioral Risk Factor Surveillance System

The BRFSS is a health survey collected by the CDC through phone interviews. It is the largest continuously collected health survey in the world, and collects information on health-related behaviors, conditions, and services. The survey asks a limited set of nutrition focused food consumption questions, which we use as a proxy for food consumption as a whole. Our key food consumption outcomes are (1) daily servings of fruits and vegetables, and (2) daily servings of fruit juice.[12] Our sample includes the BRFSS data collected from all 50 states and Puerto Rico in 1998, 2000, 2002, 2003, and 2005; these are the years in which the key questions about fruit and vegetable consumption were included in the core survey. There are an average of over 230,000 observations per year, with roughly 4,000 per year in Puerto Rico.

3.2 Vital Statistics Natality Data

We use Vital Statistics Natality data for the same period.[13] The Vital Statistics Natality data are collected from birth certificates of children born in every state (and Puerto Rico) by the CDC. Our main maternal and infant health outcomes are binary indicators for infant low birth weight, mother's diabetes, and mother's anemia. Anemia is ex-ante the most likely outcome to reveal a positive health impact from the switch to in-kind benefits. Iron deficiency is the most common cause of anemia worldwide and dietary changes, specifically increases

[12]The daily servings of fruits and vegetables variable is created by the CDC and based off individuals combined reported consumption of fruit (excluding juice), potatoes, carrots, green salad, and all other vegetables. Respondents are asked separately about fruit juice consumption. Although fruit juice consumption is sometimes reported in longer time horizons in the survey, we have adjusted all consumption variables to the level of daily intake. To account for a small number of unrealistic outliers in reported consumption, we top code each consumption response to be equal to the 99th percentile response for that variable. Respondents who refused to answer consumption questions or indicated that they did not know how much they consumed were coded as missing; these missing observations account for 3% of the data.

[13]Vital Statistics Natality data doesn't include any state level geographic identifiers after 2004.

in consumption of iron rich foods or use of iron supplements, are the most common form of iron-deficiency anemia prevention and treatment (Habershon, 1863; Camaschella, 2015). The incidence of low birth weights could also be affected by the changes in food consumption, but is less likely in this setting because of the relatively low levels of extreme malnourishment in Puerto Rico in recent decades.[14] Finally, we examine diabetes as somewhat of a placebo exercise. Diabetes is generally understood to be a slow developing, progressive, and long-term illness. While a balanced diet can decrease the risk of developing diabetes, the largest benefits come from decreasing consumption of saturated fat and cholesterol (Schoenaker et al., 2016). Decreases in total energy intake has even had some success at reversing diabetes (Lim et al., 2011). Since diabetes is closely linked with long-term over-consumption of certain food types, rather than poor nutrition in general, it is unlikely that constrained increases in food consumption would have any effect on diabetes in the short run.

The data also include month and year of birth, state or territory of birth, mother's race, mother's education, plurality of birth, and order of birth, which we use as control variables. There are an average of 2 million observations per year, with an average of roughly 55,000 births per year occurring in Puerto Rico.

3.3 Youth Risk Behavior Survey

The Youth Risk Behavior Survey (YRBS) is a biannual CDC survey of high school students across the United States. It collects information on the health and behaviors of adolescents and contains an average of 89,000 observations per survey year during our sample period, with roughly 2,300 observations occurring in each survey year in Puerto Rico.[15] We use the 2009-2017 surveys and focus our analyses on individuals born between 1994 and 2003. Our key outcomes of interest are student height and a binary variable indicating if

[14] Risk of low birth weight has been shown to increase when mothers suffer from extremely poor nutrition when they become pregnant or from caloric deprivation during the third trimester of pregnancy. But, the same studies found no effect of moderately poor nutrition or caloric deprivation in the first two trimesters (de Bernab et al., 2004).

[15] Not every state is included in each survey year, but at least 44 states are included in each year of our sample. Puerto Rico is included in the 2009, 2015, and 2017 surveys.

students are normal weight for their age and gender.[16]

It is worth noting that Puerto Rico is somewhat different than the rest of the United States and that this is apparent in the data (Table 1). While infant and adolescent characteristics are relatively similar, levels of income and education as well as fruit and vegetable consumption are much lower in Puerto Rico, even relative to the poorest states. While not necessarily a concern in a difference-in-differences framework, the reader should keep this in mind while interpreting the results, which rely on our comparison groups providing a reasonable counterfactual for the trend that Puerto Rico would have experienced without the shift to in-kind provision of benefits.

One limitation of our data sources is that none of them allow us to directly identify those eligible for or enrolled in Puerto Ricos NAP program. Our main specifications use the full sample of data available in each year, which means that many Puerto Rican individuals are included who did not participate in NAP. As a result, we estimate an average treatment effect for all Puerto Ricans that incorporates both direct effects on the treated as well as potential spillover effects on non-participants. Spillover effects are likely given the high participation rate (30-40%) of Puerto Ricans in NAP.

4 Empirical Strategy

We explore how exposure to in-kind benefits relative to equivalent cash benefits affects food consumption and health. We use a difference-in-differences design, estimating the following equation:

$$F_{ist} = \alpha_s + \lambda_t + \beta(PR_s * Post_t) + \gamma X_{ist} + \epsilon_{ist}, \tag{1}$$

where F_{ist} is a measures of the food consumption or health of individual i in state (or territory) s in year t. The terms α_s and λ_t are state and year fixed effects. X_{ist} are individual

[16]These calculations are based off BMI and are equivalent to measuring the probability that a student is not underweight, overweight, or obese.

covariates, including age indicators interacted with gender for regressions with BRFSS and YRBS data, and mother's race, plurality of birth, and birth order for regressions with natality data. $PR_s * Post_t$ is an interaction term that equals one for observations in Puerto Rico after Puerto Rico's nutrition assistance program began requiring benefits to be spent on approved food items.[17]

In the analysis of adolescent outcomes, we follow Hoynes, Schanzenbach and Almond (2016), who find that food stamps has the largest long run effects for children exposed from age 0-5. We define treatment as the fraction of the first 5 years of life that the, now adolescent, students spent under the in-kind benefits policy. In other words, $PR_s * Post_t$ is replaced with $Frac5_t$, where $Frac5_t$ is the estimated fraction of an individual's life from birth to age five that occurred after the in-kind benefits policy change. This variable is set equal to zero for untreated Puerto Ricans (born before 1996), is between zero and one for partially treated individuals (born 1996-2001), and equal to one only for those who were exposed to the policy from birth (those born after 2001). Standard errors are clustered at the state/territory level.

The coefficient of interest, β, provides an estimate of the effect of in-kind benefits, relative to cash benefits, on each outcome variable. These estimates can be interpreted as causal under the assumption that Puerto Rico and the comparison states would have maintained similar trends if not for Puerto Rico's policy change. This assumption is supported by parallel trends between Puerto Rico and the control groups (all states and the 10 poorest states) prior to the policy change.

We also conduct supplementary analysis using the synthetic control method, which constructs the comparison group from a weighted average of the outcomes of other states. As previously mentioned, Puerto Rico has lower levels of fruit and vegetable consumption than the rest of the United States. This means that no linear combination of states is

[17]The official change happened in September 2001, but there was a ramp up period through the end of 2001. Given this and data constraints (2001 is not available in the BRFSS and YRBS data is limited to year of birth), we set 2002 as the first post year.

equivalent to Puerto Rico, and limits our ability to conduct a standard synthetic control analysis. To overcome this, we demean the data using each states pre-period outcome levels, and then create the synthetic control based on those demeaned outcomes.[18] Synthetic control estimates of food consumption, based on each state's deviation from the mean, are similar in magnitude to our main results.. More details about this estimation strategy and the results can be found in Appendix B.

5 Results

5.1 Food Consumption

While our measures of food consumption are limited, results are consistent with theory, anecdotal evidence, and the visible shifts in the raw data. We find increases in consumption of fruits, vegetables, and fruit juice that have minimal variation in magnitude or statistical significance across comparison groups (Table 2).[19] Average daily consumption of fruits and vegetables increases from 2.7 to 3.2 servings a day. These results suggest a 20-26% increase in fruits and vegetables consumed as a result of the switch to in-kind benefits. Our identification strategy is supported by event studies that demonstrate a flat (or slightly downward) trend in food consumption prior to a large increase just after the benefit shift (Figure 1).

The magnitude of our effects is large, but consistent with the food industry revenue estimates outlined previously. We can inflate our estimates by the fraction of the population enrolled in NAP to estimate the effect on consumption among benefit recipients. Given that 30-40 percent of Puerto Ricans were receiving benefits during this time period, this implies an increase of 1.5 servings of fruit and vegetables per day, which represents a consumption increase of over 50 percent, within the range of the 40 to 60 percent increase in food expenditures expected.[20]

[18] We subtract each states pre-2001 average consumption levels from each observation from that state.
[19] The effects are also robust to using a synthetic control method (Appendix Figures A1-A4).
[20] We return to a discussion of magnitude below.

5.2 Mother and Infant Health

The increases in food consumption documented above may or may not improve individual health outcomes. Mexico's PAL experiments found no differences in health for individuals receiving in-kind benefits rather than cash transfers, and other recent work suggests that individuals purchase less nutritious food with in-kind benefits than they do with cash (Cunha, 2014; Hastings, Kessler and Shapiro, 2018). However, in both of these contexts the food benefits provided were largely inframarginal and had no effect on the quantity of food consumed.

We use natality data to estimate the effect of in-kind food benefits on the contemporaneous health of mothers and newborn children. Specifically, we identify the effect of in-kind benefits on the incidence of diabetes and anemia among mothers and the incidence of low birth weight among newborn children. As discussed previously, anemia is ex-ante the most likely outcome to reveal a positive health impact from the switch to in-kind benefits given the prevalence of iron deficiency and the short-term responsiveness to dietary changes. The incidence of low birth weights could also be affected by the changes in food consumption, but is less likely given the relatively low levels of extreme malnourishment in Puerto Rico during the 1990s and 2000s.

We find no evidence of effects on low birth weight, but a substantial effect on maternal anemia (Table 2). The estimates suggest a 13-23 percent reduction in anemia. The validity of these estimates is supported by event studies that demonstrate a flat trend in anemia before 2001 followed by a relative decrease in maternal anemia in Puerto Rico after the benefit shift (Figures A7, and A8).[21] While there is no observed effect on birthweight, the associated event studies suggest caution in interpreting these estimates given the lack of parallel trends in the pre-period.

[21] Due to data restrictions, we are not able to explore heterogeneous treatment effects by education level in the natality data. The natality data includes information on mothers' education level, but a number of states changed the coding of these variables in 2003 and 2004. Because the timing of the change coincides with the post treatment period in this study, we don't conduct any health effect analysis by education subgroups.

Finally, we examine effects on maternal diabetes. While diabetes reflects underlying nutrition and health, it is generally understood to be a slow developing, progressive, and long term illness and is unlikely to be affected by short-term changes in food consumption. Differences in maternal diabetes that show up then may then reflect something else that is also responsible for effects on anemia or birth weight. Comfortingly, we find no meaningful effects on the incidence of maternal diabetes (Table 2).

5.3 Adolescent Health

Given the observed short-term effects on food consumption and maternal health, a natural question is whether these effects persist, perhaps suggesting an important role of nutrition in explaining the effects observed in Hoynes, Schanzenbach and Almond (2016). Using the YRBS data on high school students age 14-18, we are able to look at the effect of the policy on long run health outcomes. Following Hoynes, Schanzenbach and Almond (2016), treatment is defined as the proportion of time an individual was exposed to in-kind benefits from age 0-5. We show that additional exposure to in-kind benefits results in increased individual height and the likelihood of being classified as normal weight. For each year of in-kind benefit exposure from age 0 to 5, height increases by 0.04 to 0.06 inches. Relative to those who never received in-kind benefits during this critical period, exposure for the first five years of life results in an increase in adolescent height of $\frac{1}{6}$ to $\frac{1}{3}$ of an inch. Full exposure from age 0 to 5 also results in a 4-5 percentage point (6-7%) increase in the fraction of high school students considered to be normal weight (Table 2).

Figures 7 and 10 provide graphical evidence of the effects, demonstrating the relationship between the age at exposure to in-kind benefits and height or normal weight in adolescence. Given the nature of treatment, the presentation is somewhat non-standard, following Hoynes, Schanzenbach and Almond (2016). The x-axis presents the number of years between the year of the transition to in-kind benefits (2001) and an individual's year of birth. In other words, negative values represent individuals who were born after the transition to in-kind benefits in 2001. Those individuals with a value of 0 or less are "fully treated" in that the in-kind

benefits were available from the year of their birth. As we move to the right the age at transition to in-kind benefits increases (and therefore childhood in-kind benefit exposure decreases). As observed in the figure, the earlier in an individual's life that the transition to in-kind benefits occurred (and presumably food consumption increased), the larger the increase in height or likelihood of being of normal weight in adolescence. The effects on height and being of normal weight in adolescence are largest at or prior to conception and decrease between conception and age 5. Consistent with our estimates representing a causal effect of the transition to in-kind benefit provision, the timing of the transition prior to conception has no effect on the size of the reduction (i.e., the effect of the availability of in-kind benefits is the same for those born one or two years after the transition).[22]

When combined with previous results, this results suggests that increases in food consumption during early childhood, caused by the switch to in-kind benefits, translate into improved health through adolescence.

5.4 Magnitude of Effects

If the policy change only affected the Puerto Ricans receiving NAP benefits, we could inflate our results by the fraction of the population enrolled in NAP to estimate average effects for recipients. Given that 30-40 percent of Puerto Ricans were receiving benefits during this time period, this implies an average treatment effect for recipients of 1.5 servings of fruits and vegetables per day, which represents an over 50 percent increase in consumption. Similar calculations suggest reductions in maternal anemia of 1-1.7 percentage points (30-50 percent) as well as substantial increases in height (7-12 percent) and the likelihood of being normal weight (14-18 percent) among receipients.

However, we find it unlikely that those receiving NAP were the only ones affected. Puerto Rico is more densely populated than any state, and has a culture that puts a strong focus on family and community. Since the shift in NAP policy constrained roughly 1 out of every 3 people to purchase more food, we expect that a non-trivial portion of that food would

[22]Unfortunately, the timing of the policy change and the availability of data limits our ability to explore effects on cohorts born more than one or two years after the transition.

be shared with family and neighbors, either through direct gifts of food or by sharing prepared meals with individuals outside the household. Additionally, we know that the policy change had meaningful implications for food distributors across Puerto Rico. One newspaper wrote that,"the commercial food sector, which lobbied intensely [in favor of the in-kind restrictions], has registered significant increases in sales, product of the captive market that provide 75% of the $1.8 billion that the PAN distributes annually in Puerto Rico. A specific estimate of how much sales have risen was not available, but the head of the Socioeconomic Development Administration (Adsef) of the Family, Gretchen Coll, says it is 'very much.'"It is possible that the policy change affected the general equilibrium in a number of ways, including food prices, the types of foods supplied/consumed, and the location of sellers.

These predictions of spillover are supported by the data. When we run our analysis separately across individuals with different education levels (Table 7) we find meaningful increases in food consumption at every education level, including individuals with college degrees who have low rates of NAP eligibility. Of course, not all of this effect on high education individuals is due to spillover. A significant share of college educated individuals are eligible to receive benefits. Estimates using the 2000 Puerto Rico census suggest that about 13% of those with bachelor's degrees had incomes below the federal poverty line (FPL), compared to 48% of those with only a high-school degree and 63% of high school drop outs.

Some of the effect on high education individuals could be an artifact of our limited food consumption measures, rather than the result of spillovers. As discussed in section 3.1, respondents do not report their complete food consumption, only consumption of fruits, vegetables, and fruit juice. If highly educated individuals have a higher marginal propensity to consume foods from the reported food categories, then our measures of their food consumption will be inflated relative to individuals with lower education levels. Prior to 2001, Puerto Ricans with college degrees drank 9% more fruit juice and ate 22% more fruits and vegetables than those with only a high school diploma on average. It is likely that, when constrained to spend NAP benefits on food, better-educated recipients will have a higher marginal propen-

sity to consume fruits and vegetables than their lower educated counterparts. If this is the case, it explains part of the relatively large effect sizes for highly educated groups with low NAP participation rates.

Despite being a contributing factor, differences in marginal propensity to consume fruits and vegetables does not convincingly explain the magnitude of the effect on highly educated individuals. The fraction of high school graduates below the FPL is 3.7 times larger than the fraction of college graduate below the FPL. All else equal, we would expect roughly one-fourth as many college graduates to be receiving benefits, and expect average treatment effects for those with bachelor's degrees to be 27.1% of average treatment effects for high school dropouts. Contrary to this, we estimate that increases in average fruit and vegetable consumption for college graduates is 66% of the size of the increase for high school dropouts. Even after adjustments allowing college graduates to have a 22% higher marginal propensity to consume fruits and vegetables (equivalent to the average difference in consumption between groups before the policy change), implied TOT effects for college graduates are still 2 times higher than we would expect in the absence of spillovers.[23] In order for the ratio of effect sizes relative to program eligibility to be fully explained by differences in marginal propensity to consume fruits and vegetables, college graduates below FPL would have to consume 2.5X more fruits and vegetables with each constrained NAP dollar of benefits than their counterparts with a high school diploma. While possible, we find it more likely that part of the effect is caused by the higher propensity of college-educated NAP recipients to consume fruits and vegetables while the rest of the difference is the result of spillover effects.

The presence of spillover effects has important implications for how we interpret the reduced form and scaled effects of the shift to in-kind benefits. While we discuss above the average treatment effects for recipientsimplied by NAP participation rates, these inflated

[23]Inflating the effect size of each education category by the fraction of individuals in that education category who have incomes below FPL gives estimated TOT effects of (.623/.48)=1.30 for high school graduates and (.415/.13)=3.19 for college graduates (Food consumption results by education level are shown in Table 7). Inflating 1.30 by 22% yields 1.6 as the expected TOT for college graduates, but the calculated TOT estimate for college graduates (3.19) is 2 times higher than this prediction.

estimates are only accurate under the assumption of no spillover effects and are biased upward when spillovers are present. Because spillovers are likely, our main specifications and results focus on the reduced form effect of the policy shift for the population, accounting for direct effects for NAP recipients as well as any spillover and general equilibrium impacts.

The alternative explanation is that food consumption increases were completely unrelated to the NAP policy change. If some other factor increased food consumption for the entire population of Puerto Rico, irrespective of participation in NAP, this would explain why the ratio of effects across education levels is smaller than those implied by eligibility ratios.

5.5 Limitations and Threats

The primary internal validity concern for our empirical strategy is that Puerto Rico implemented the benefit change when food consumption was increasing and Puerto Rican's were becoming healthier for some other reason. For example, Puerto Rico may have adopted the benefit shift as part of a larger initiative to improve a variety of services for mothers and young children. If this were the case, we might observe improved nutrition and behavior due to a comprehensive effort to help these cohorts and not because of the benefit shift. But the purpose for the shift was not internally motivated. In fact, it came about as a result of pressure from the U.S. mainland to "align NAP more closely with the regulations that governed SNAP; specifically, regulations on what and where benefits could be used" (ADSEF, 2000). This pressure was motivated by a desire to "encourage and enforce the utilization of the benefit as established in Federal law and regulations only for the acquisition of food." Interestingly, those in charge of the program expected this change to "dramatically reduce improper use of targeted funds" and "promote better nutrition for... participants" (ADSEF 2001b, p.7). The externally motivated benefit shift is consistent with the evidence provided in Figure 1, which demonstrates a flat trend in fruit and vegetable consumption followed by a sharp jump between 2000 and 2002.

Still, it is possible that the benefit shift coincided with something else that generated

improvements in nutrition and health around or after the point of the benefit shift. To address this concern we conducted an extensive review of Puerto Rican policies and events during this time period.[24] We uncovered few policy shifts or events that seem likely to have generated the observed results. The greatest potential confounds are the 2001 recession and migration out of Puerto Rico.

While the 2001 recession coincides with the timing of the benefit shift, it seems unlikely to have generated our results as it had similar effects on Puerto Rico and our sets of comparison states. If anything, the recession was somewhat more pronounced in Puerto Rico, which we would expect to negatively affect nutrition and birth outcomes. Regardless, the recession was rather short lived and thus seems unlikely to account for the persistent effects we observe.

Another potential explanation for our estimates is differential migration out of Puerto Rico. Migration out of Puerto Rico could account for our results if impoverished families or mothers began leaving the island in greater numbers around the time of the benefit shift. While out-migration did increase somewhat in the early 2000s, the numbers can account for only a tiny fraction of our observed effects. Further, there was no "jump" in out migration that occurred around 2001 that could account for the observed improvements in food consumption and birth outcomes.

6 Discussion and Conclusion

While prior evidence suggests that the FSP increased food consumption and improved short- and long-term health outcomes, it is not clear whether these effects were driven, at least partially, by constraining households' consumption decisions, or whether an equivalent increase in income would have generated the same effects. To shed light on this question, we leverage a natural experiment where Puerto Rico converted a cash benefit to one in which

[24] This task was undertaken with the assistance of a Puerto Rican legal researcher with extensive experience searching Spanish-language periodicals and reports.

recipients were required to spend 75% of their benefits on approved food items. This allows us to examine the impact of constraining household consumption decisions in the absence of any shock to overall income.

Using a difference-in-differences strategy, we find that providing the benefits in-kind increases fruit and vegetable consumption by .5 servings per day (20 percent). These improvements in nutrition led to decreases in maternal anemia of 0.3-0.7 pp (13-23 percent) and resulted in those who grew up after the shift being taller and more likely to be normal weight as adolescents.

Our results conflict somewhat with a body of work that suggest more modest effects of the form of nutritional assistance on food expenditures or health. Most of these results can be reconciled when one considers the degree to which program participants' food consumption levels are constrained by the generosity of the in-kind benefit. Given the lack of a binding constraint and the subsequent minimal effects on food consumption and nutrition it is perhaps not surprising that prior studies were unable to detect effects on health. Unlike in most prior cash out evaluations, the majority of benefit recipients in Puerto Rico were constrained by the shift to in-kind benefits. This resulted in large increases in food consumption and subsequent improvements in health.

While the results provide compelling evidence that the form of the benefit can matter, the analyses are not without limitations. First, the strength of the evidence is limited by the nature of the variation. The estimates rely critically on a comparison of outcomes across time in Puerto Rico and how those outcomes evolved relative to outcomes in sets of comparison states in the U.S. While we are unaware of other policy changes or events that could have generated these effects, we present the results with this caution in mind. Second, it is important to emphasize that the results do not imply that in-kind benefits are welfare improving over cash, but rather that individual spending may not prioritize health, particularly of young children, under a cash-based system. This may have additional implications for the long run costs of cash vs in-kind benefits, particularly when low income

individuals have medical costs that are covered or subsidized by the government. A more holistic understanding of the costs and benefits of provision of benefits in-kind or in cash is outside the scope of this work.

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7 Figures and Tables

Table 1: Summary Statistics

	(1) All Comparison States	(2) 10 Poorest States	(3) Puerto Rico
Panel A: BRFSS			
Female	0.60	0.63	0.64
Age	48.68	48.77	49.15
Daily Servings of Fruit and Vegetables	3.83	3.71	2.97
Daily Servings of Fruit Juice	0.65	0.61	0.90
Income Below 10k	0.05	0.07	0.36
Income Above 50k	0.35	0.29	0.05
Graduated High School	0.96	0.93	0.78
Graduated College	0.31	0.26	0.22
Observations	1,829,113	308,772	29,924
Panel B: Natality			
Mother's Diabetes	0.03	0.03	0.02
Mother's Anemia	0.02	0.03	0.03
Low Birth Weight	0.08	0.09	0.11
Plural Birth	0.03	0.03	0.02
Observations	27,591,343	3,574,871	389,729
Panel C: YRBS			
Female	0.51	0.52	0.50
Age	15.77	15.79	15.74
Hispanic	0.19	0.18	0.20
Weight (lbs)	146.91	150.45	147.04
Normal Weight	0.67	0.62	0.65
Height (in)	66.57	66.60	66.33
Observations	449,867	57,916	7,412

Note: Table presents descriptive statistics for each data set used in analysis. Statistics are shown separately for all states, the 10 poorest states, and Puerto Rico as indicated by column titles.

Table 2: Main Outcomes

	BRFSS		Natality			YRBS	
	(1) Fruit and Vegetables	(2) Fruit Juice	(3) Low Birth Weight	(4) Mother's Anemia	(5) Mother's Diabetes	(6) Normal Weight	(7) Height in Inches
Panel A: All States							
PR*Post	0.5306***	0.2290***	0.0005*	-0.0035***	-0.0006	0.0385***	0.1904***
	(0.0167)	(0.0046)	(0.0003)	(0.0008)	(0.0006)	(0.0034)	(0.0464)
Observations	1,236,218	1,219,453	27,555,353	26,350,409	27,294,011	454,397	454,397
Panel B: 10 Poorest States							
PR*Post	0.5576***	0.2253***	-0.0008	-0.0067***	-0.0001	0.0514***	0.3216**
	(0.0546)	(0.0120)	(0.0007)	(0.0016)	(0.0007)	(0.0054)	(0.1021)
Observations	222,938	217,407	3,924,711	3,769,091	3,899,538	57,501	57,501
PR Pre-Treatment Average	2.67	0.81	.11	.03	.02	0.68	65.46

Note: Each coefficient is the result of a separate regression. Panel A displays the results when all states are included in the comparison group, while panel B restricts the comparison group to include only the 10 poorest states. Outcomes are indicated by column titles, with the data source for each outcome indicated by its multi-column header. All standard errors are clustered at the state level. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

Table 3: Natality - With or without controls

	(1) No Controls	(2) All Controls
Panel A: Low Birthweight		
Low Birthweight: Poor States	-0.0003	-0.0008
	(0.0009)	(0.0007)
Low Birthweight: All States	0.0004*	0.0005*
	(0.0002)	(0.0003)
Panel B: Anemia		
Anemia: Poor States	-0.0068***	-0.0067***
	(0.0016)	(0.0016)
Anemia: All States	-0.0037***	-0.0035***
	(0.0008)	(0.0008)
Panel B: Diabetes		
Diabetes: Poor States	-0.0018	-0.0001
	(0.0012)	(0.0007)
Diabetes: All States	-0.0016**	-0.0006
	(0.0008)	(0.0006)

Note: Each coefficient is the result of a unique regression, where the outcome variable and comparison set is indicated by the row label. Both columns include state and year fixed effects. The second column also includes all controls used in our main analysis (education level, birth month, race, plural birth, and birth order). Standard errors are clustered at the state level. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Table 4: Natality by Marital Status

	(1) Low Birth Weight	(2) Anemia	(3) Diabetes
Panel A: All States			
PR*Post*Married	-0.001***	-0.007***	0.001
	(0.000)	(0.001)	(0.001)
PR*Post*(Married or Cohabiting)	-0.001***	-0.004***	-0.001
	(0.000)	(0.001)	(0.001)
PR*Post*(Not Married)	0.002***	-0.000	-0.001*
	(0.001)	(0.001)	(0.000)
PR*Post*(Not Married or Cohabiting)	0.005***	-0.000	-0.000
	(0.001)	(0.001)	(0.000)
Panel B: 10 Poorest States			
PR*Post*Married	-0.002**	-0.009***	0.001
	(0.001)	(0.001)	(0.001)
PR*Post*(Married or Cohabiting)	-0.001*	-0.007***	-0.000
	(0.001)	(0.001)	(0.001)
PR*Post*(Not Married)	0.000	-0.004	-0.001
	(0.001)	(0.003)	(0.001)
PR*Post*(Not Married or Cohabiting)	0.002*	-0.004	0.000
	(0.001)	(0.003)	(0.001)
PR Pre-Treatment Average	.11	.03	.02

Note: This table shows Natality results by marital status. Each coefficient is the result of a separate regression. Outcomes, comparison groups, and marital status are indicated by column titles, panel labels, and row labels respectively. All standard errors are clustered at the state level. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

Table 5: BRFSS by Marital Status

	(1) All	(2) Married	(3) Married or Cohabiting	(4) Not Married	(5) Not Married or Cohabiting
Panel A: All States					
Daily Servings of Fruits & Vegetables	0.531***	0.445***	0.458***	0.637***	0.480***
	(0.017)	(0.017)	(0.016)	(0.019)	(0.017)
Daily Servings of Fruit Juice	0.229***	0.219***	0.225***	0.243***	0.221***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	1,219,453	666,219	696,818	549,617	1,017,215
Panel B: 10 Poorest States					
Daily Servings of Fruits & Vegetables	0.558***	0.472***	0.480***	0.662***	0.504***
	(0.055)	(0.045)	(0.046)	(0.066)	(0.052)
Daily Servings of Fruit Juice	0.225***	0.207***	0.215***	0.250***	0.215***
	(0.012)	(0.011)	(0.011)	(0.016)	(0.012)
Observations	217,407	114,749	118,974	102,113	180,736

Note: Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

7.1 BRFSS Figures

Figure 1

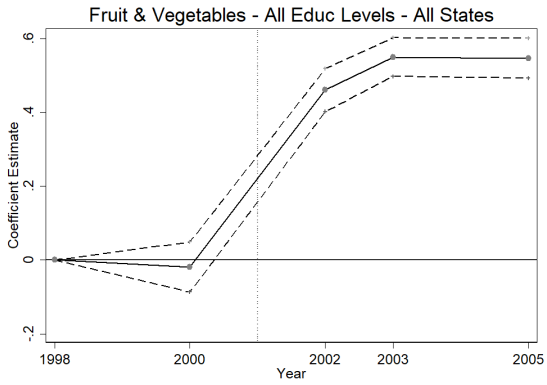


Figure 2

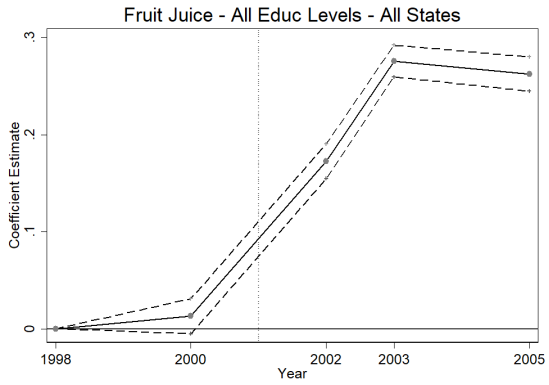


Figure 3

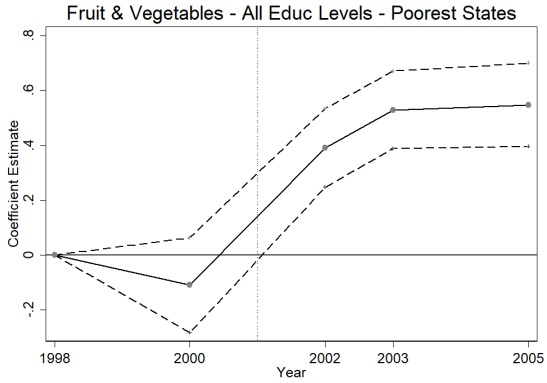
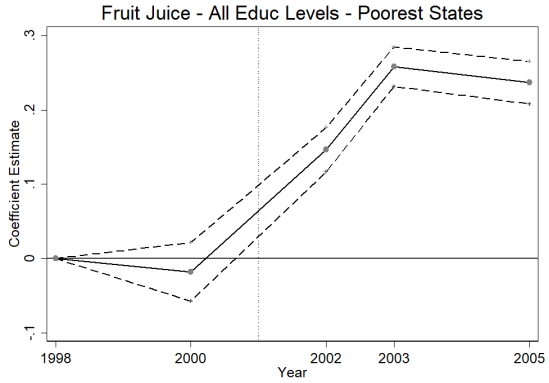


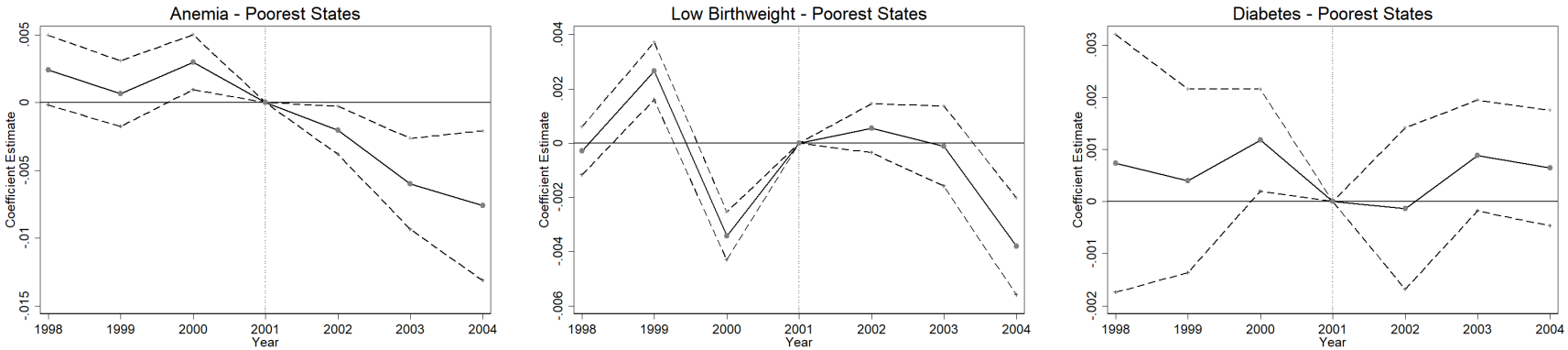
Figure 4



7.2 Natality Figures

Figure 5: Natality Outcomes: All States

Figure 6: Natality Outcomes: 10 Poorest States



7.3 YRBS Figures

Figure 7: All States

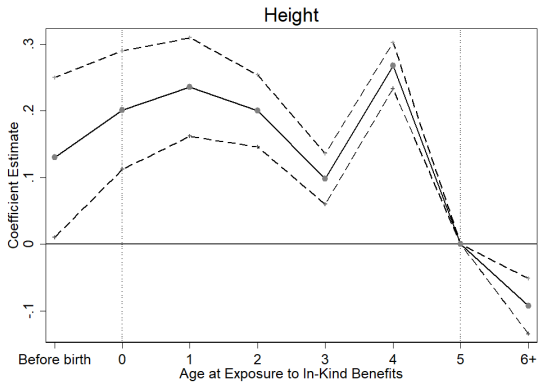


Figure 8: All States

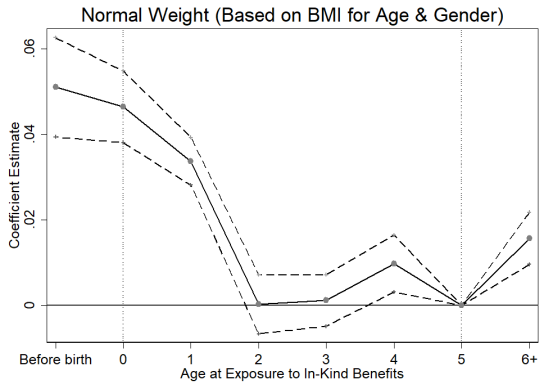


Figure 9: 10 Poorest States

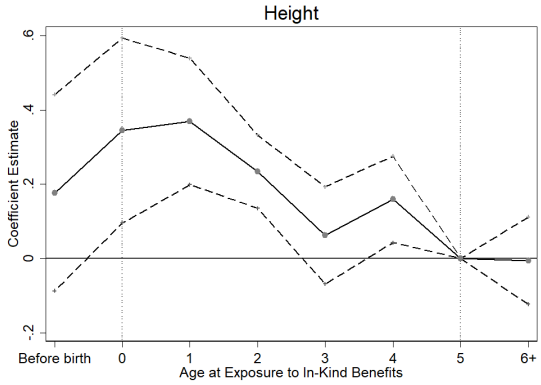
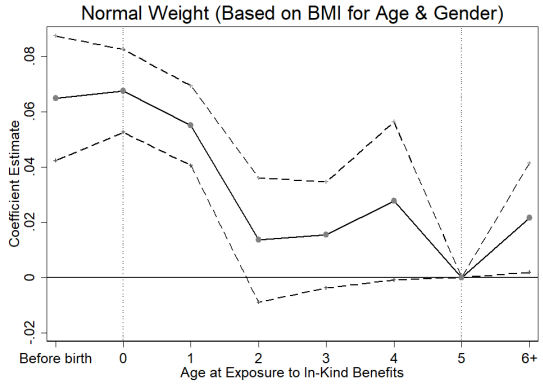


Figure 10: 10 Poorest States



A Additional Tables and Figures

Table 6: BRFSS Results by Education Level

	(1) All	(2) Dropout	(3) High School Diploma	(4) Some College	(5) Bachelor's Degree
Panel A: All States					
Daily Servings of Fruits & Vegetables	0.531***	0.587***	0.623***	0.559***	0.415***
	(0.017)	(0.019)	(0.017)	(0.018)	(0.020)
Percent Change	19.9	24.3	24.3	20.7	13.3
Daily Servings of Fruit Juice	0.229***	0.183***	0.269***	0.236***	0.216***
	(0.005)	(0.008)	(0.005)	(0.006)	(0.006)
Percent Change	28.3	25.4	33.3	27.8	24.5
Observations	1,219,453	134,941	380,692	329,336	371,905
Panel B: 10 Poorest States					
Daily Servings of Fruits & Vegetables	0.558***	0.633***	0.638***	0.567***	0.452***
	(0.055)	(0.030)	(0.051)	(0.051)	(0.080)
Percent Change	20.9	26.2	24.9	21.0	14.5
Daily Servings of Fruit Juice	0.225***	0.202***	0.267***	0.226***	0.205***
	(0.012)	(0.012)	(0.011)	(0.016)	(0.018)
Percent Change	27.8	28.1	33.0	26.6	23.3
Observations	217,407	37,256	71,225	52,943	55,463
PR Pre-Treatment Average	0.81	0.72	0.81	0.85	0.88

Note: Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

Table 7: Natality - Composition of Births

	Education				Race		Other	
	(1) No HS Diploma	(2) HS Diploma	(3) Some College	(4) Earned BA	(5) White	(6) Black	(7) First Child	(8) Number of Births
Panel A: All States								
PR*Post	-0.032***	0.024***	0.010***	-0.003	-0.009***	0.016***	0.013***	-8780.438***
	(0.003)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(625.515)
Observations	21,461,549	21,754,521	21,461,549	21,461,549	27,981,072	27,981,072	27,836,044	364
Panel B: 10 Poorest States								
PR*Post	-0.034***	0.028***	0.004**	0.002	-0.019***	0.024***	0.019***	-7723.250***
	(0.005)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(874.788)
Observations	3,027,519	3,062,206	3,027,519	3,027,519	3,062,206	3,062,206	3,054,231	77
PR Pre-Treatment Average	.27	.29	.23	.21	.92	.08	.43	58790

Note: Each coefficient is the result of a unique regression, where the outcome variable is indicated by the column title. Regressions include state and year fixed effects and no other controls. Regressions for education outcomes exclude 9 comparison states that adjusted their coding of education categories in the post period. Standard errors are clustered at the state level. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

B Synthetic Control

Table A1: Main Outcomes

	BRFSS		Natality		
	(1) Fruit and Vegetables	(2) Fruit Juice	(3) Low Birth Weight	(4) Mother's Anemia	(5) Mother's Diabetes
Demeaned Outcomes					
Puerto*Post	0.480	0.232***	0.003	-0.002	-0.001
	(0.115)	(0.038)	(0.885)	(0.256)	(0.269)
	[0.019]	[0.019]	[0.192]	[0.605]	[.904]
Not Demeaned					
Puerto*Post			0.005	-0.004*	-0.003
			(0.788)	(0.093)	(0.192)
			[0.038]	[0.349]	[0.596]
PR Pre-Treatment Average	2.7	0.8	.11	.03	.02

Note: Natality post period is defined as starting 2002. P-Values (Calculated from Post-treatment RMSE divided by Pre-treatment RMSE) are in parentheses and significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01). Standard Random Inference P-Values are in brackets.

B.1 BRFSS Results

Figure A1

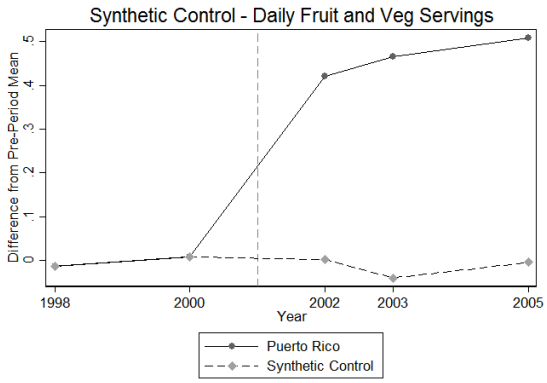


Figure A2

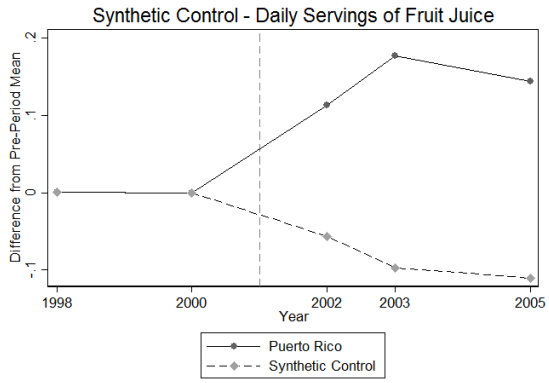


Figure A3

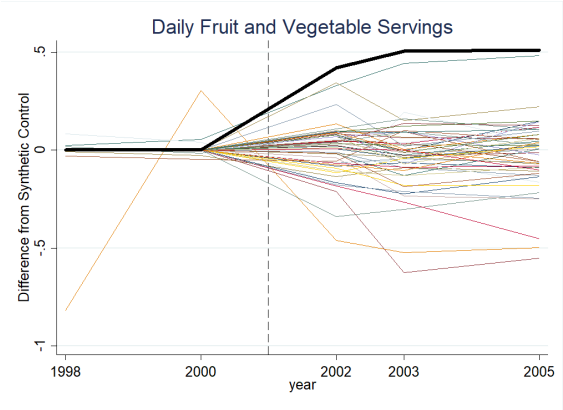
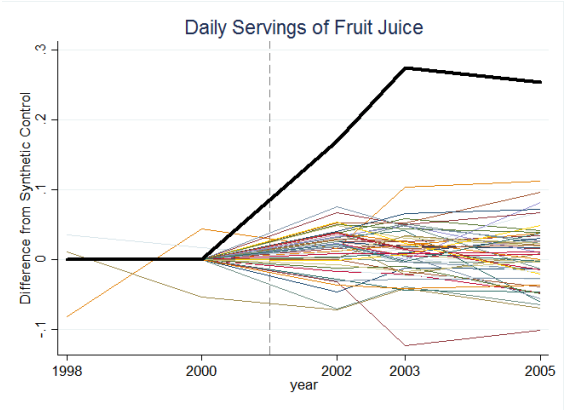


Figure A4



B.2 Natality Results

B.2.1 Demeaned Outcomes

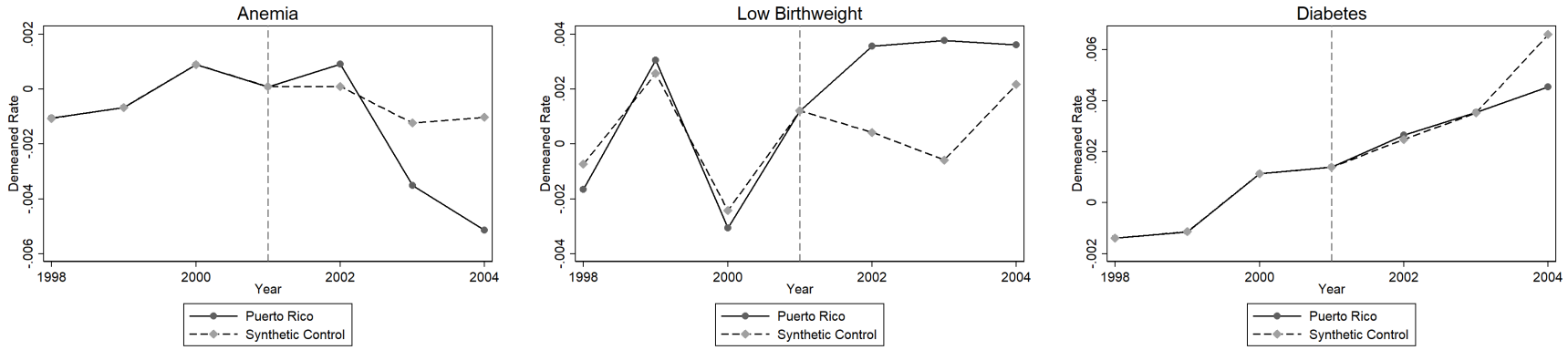


Figure A5

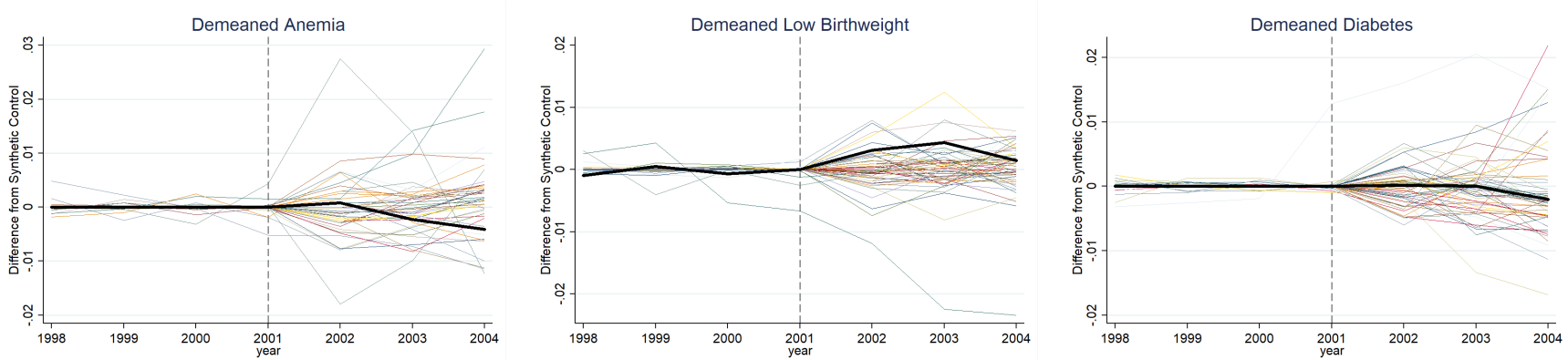


Figure A6

B.2.2 Not Demeaned Outcomes

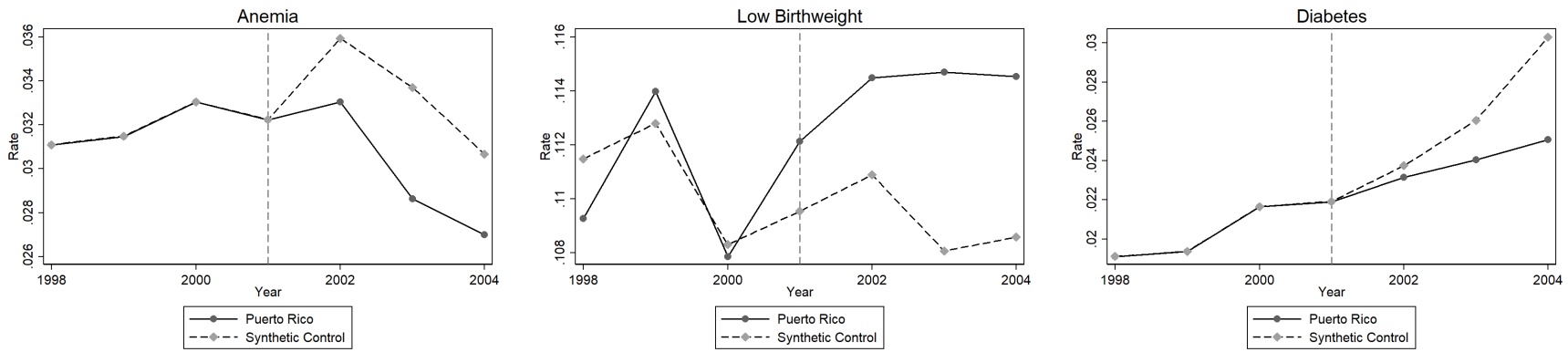


Figure A7

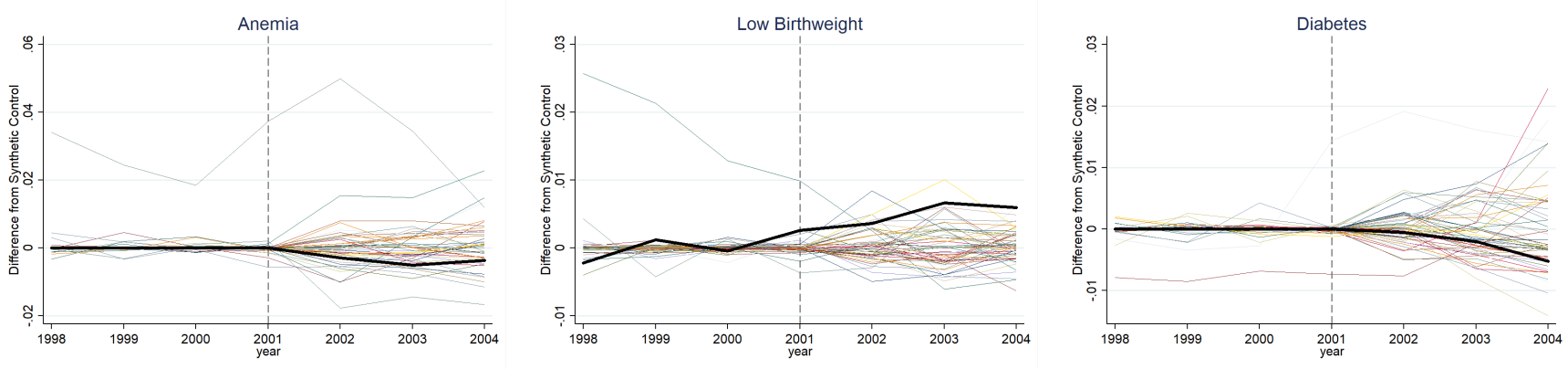


Figure A8