

# The Effect of the 2008 Economic Stimulus Payments on Nutrient Demand

Zachary A. Goodman\*

University of California, San Diego

This version: May 2021

## **Abstract**

In this paper I examine the effects of receiving an economic stimulus payment (ESP) as part of the Economic Stimulus Act of 2008 on nutrient demand. I estimate this effect by exploiting the randomized timing of ESP receipt.

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\*zgoodman@ucsd.edu. I thank Jeffrey Clemens, Gordon Dahl, Melissa Famulari, Craig McIntosh, Katherine Meckel, and numerous seminar participants for their comments and suggestions. My analyses are calculated based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of my own and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

# 1 Introduction

The US spends about \$100 billion annually on programs designed to address food insecurity including the Supplemental Nutrition Assistance Program, Special Supplemental Nutrition Program for Women, Infants, and Children, and school meal programs. These programs serve tens of millions of Americans: one in four uses a program offered by the US Department of Agriculture (USDA) Food and Nutrition Service at least once in a year (of Agriculture Food & Service, n.d.). Additionally, food insecurity and poor nutrition are often cited by politicians as justification for transfer programs. Unconditional cash transfers (UCTs) have been proposed as ways to address poverty without spending resources on means testing ( ) or increase aggregate consumption helping reducing the duration of a recession ( ). However, little is known how UCTs affect nutritional choices in developed countries.

In this paper, I examine one such UCT, economic stimulus payments (ESPs) dispersed as part of the Economic Stimulus Act of 2008 (hereafter ESA), on demand for nutrients. These ESPs were sent to households starting May 2008 in an effort to increase spending and reduce the duration of the recession caused by the 2007 financial crisis. In total, over \$100 billion in ESPs were sent to 130 million taxpayers in 2008. Fortunately, the IRS decided when each taxpayer would receive her ESP using the last two digits of her Social Security number, which is effectively random assigned.<sup>1</sup> The random timing of the ESP allows for identification of causal effects, which other authors such as Broda and Parker (2014) have exploited to estimate the impact of receiving an ESP on consumption. I use a similar approach to estimate the impact of receiving an ESP on demand for nutrients.

To estimate this effect, I calculate household-level nutrient purchases over time using data from the Nielsen Consumer Panel (NCP), which provides household-level purchase data at the barcode level for about 60,000 households over several years. I merge these panel data with barcode-level Nutrition Facts data to observe quantities of each nutrient purchased per

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<sup>1</sup>Social Security applicants are assigned the last four digits of their Social Security numbers sequentially within their geographic area and group, which determine the first seven digits of their number.

household over time. Finally, I observe when panelists receive their ESPs using a special survey constructed by Broda and Parker (2014) answered by NCP panelists before and during the period of time that ESPs were distributed.

I find that households increase purchases of...

The remainder of the paper is organized as follows. Section 2 provides a background of the policies enacted and related literature. Sections 4 and 3 introduce the methods and data used, respectively. Section 5 summarizes our results. Section 6 discusses those results, and Section 7 concludes.

## **2 Background and Related Literature**

In this section, I review the policy studied as well as the literature on UCTs in developed countries as well as the literature on demand for nutrients.

### **2.1 Economic Stimulus Act of 2008**

First I provide background on the Economic Stimulus Act of 2008. The ESA was signed into law by President George W. Bush on February 13, 2008, which promised to provide ESPs to taxpayers who filed a 2007 tax return. Each eligible taxpayer received between \$300 and \$600 if filing single or twice that amount if filing jointly, depending on tax liability in 2007 (Congress, 2008). To be eligible for an ESP, one must have earned at least \$3,000 in income in 2007 and have a sufficiently low income. Adjusted gross income beyond a threshold of \$75,000 per qualifying adult phased out ESP benefits at a rate of 5%. Additionally, each dependent child increased one's ESP by \$300. In total, the program disbursed about \$100 billion in payments to 130 million taxpayers (Parker et al., 2013).

Although signed into law in February, taxpayers did not begin to receive ESPs until May. For those taxpayers who provided direct deposit information, the IRS distributed electronic transfers of ESPs during a three-week period starting in late April. For the remaining

taxpayers, the IRS mailed paper checks during a nine-week period starting in early May through July. In advance of sending the ESP, the IRS mailed recipients a notice that an ESP would be arriving soon. The week a taxpayer received her ESP was determined by the last two digits of her Social Security number, which as mentioned earlier is assigned as good as randomly. Hence, the *timing* of receiving an ESP is random conditional on transfer method (electronic vs mail). It is important to note that the amount of the ESP is *not* random, nor is whether or not one receives an ESP. Hence, identification of causal effects in this paper comes from comparing those who receive their ESP early to those who receive their ESP later, regardless of amount, conditional on transfer method.

## 2.2 Unconditional cash transfers in the United States

Here I review papers that examine causal effects of UCTs in the US Johnson, Parker, and Souleles (2006) use the quasi-random timing of tax rebate checks sent to households as part of the Economic Growth and Tax Relief Reconciliation Act of 2001 to estimate the effects of income shocks on consumption. Using a unique set of questions added to the Consumer Expenditure Survey after passage of the 2001 Tax Act, they observe when households receive their payments. Similar to the ESPs distributed in 2008, the timing of payments in 2001 depended on one's SSN, and hence the timing of receipt is as good as randomly assigned. The authors find that households spent about two-thirds of their rebates over the six months following receipt on nondurable goods.

Broda and Parker (2014) conduct the survey of Nielsen Consumer Panel households that I use in this paper to observe if and when households receive their stimulus payments, how much they received, and some information about preferences and how the household intends to spend their payments. The authors find that households do not respond to news about the incoming payment but that aggregate spending rises by 10% the week after the payment is received. Additionally, spending remains higher for the three months following receipt of the payment, and the response is greatest for households that report low liquid wealth

and low income. In another paper, Parker et al. (2013) add additional survey questions to the Consumer Expenditure Survey related to receipt of 2008 ESPs. The authors find that households spent between 50 - 90% of the payments, with about a third of that spent on nondurable goods.

Kaplan and Violante (2014a) construct a model that can explain the smaller consumption response in 2008 vs 2001, which they detail in Kaplan and Violante (2014b). The authors describe “wealthy hand-to-mouth” households who hold large amount of wealth mostly as illiquid assets. These households display large marginal propensities to consume upon receiving a positive income shock but not upon receiving news of the shock, which runs counter to conventional hypotheses that uses a one-asset framework and ignore the liquidity of such an asset. The key theoretical takeaway that I examine in this work is that liquidity constrained households, even those with significant savings, may experience consumption changes upon receiving an ESP.

## 2.3 Demand for nutrients

Here I review empirical work that estimates the elasticity of demand for nutrients and in general evidence that the demand for “healthy” nutrients (non-sugar carbohydrates, fiber, unsaturated fats) is more elastic than the demand for “unhealthy” nutrients (saturated fats, sugar, sodium).<sup>2</sup>.

## 3 Data

I identify the causal effects of UCTs on nutrient demand using several data sources. I start with data from the Nielsen Homescan Survey, a nationally-representative panel that allows me to observe household-level food purchases and demographic data for about 60,000

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<sup>2</sup>*Healthy* in this context is guided by recommendations from US government agencies, such as the USDA’s *Dietary Guidelines for Americans 2020*, which are guided by current average levels of nutrient consumption in the US.

households in 2008. Households in the panel are provided a barcode scanner by Nielsen, who asks households to scan all items purchased. To incentivize households to scan items, Nielsen offers a rewards catalog, which provides panelists higher value rewards the longer they remain in the sample.<sup>3</sup>

When scanning barcodes, Nielsen panelists are asked to provide information about the store where the product was purchased and the price of the item. Prices are automatically recorded for products purchased at a Nielsen partner retail outlet as the average price during the week the panelist purchased the product. Panelists are asked to manually input the price of products made at non-partner retail outlets. For barcodeless products like some produce and bakery items, Nielsen provides a reference booklet with barcodes associated with a photo and description of a product.<sup>4</sup> In practice, households may choose to omit reporting certain purchases or forget to scan and do not report all purchases (Einav, Leibtag, & Nevo, 2010). Hence I report coefficient estimates as percent changes. Provided the degree of underreporting in the homescan data is not correlated with ESP timing, the treatment effect estimates should not be biased by this measurement error of all purchases.

To observe nutrition information, I match the UPC codes associated with each Nielsen product with barcode-level nutrition data. I then collapse these data to observe total quantities of each nutrient purchased per household per week. The nutrition information come from three sources. The first source is from Syndigo, who license their nutrition dataset covering over 220,000 products containing information from the Nutrition Facts panel, ingredients list, and general product attributes like brand, size, and description.<sup>5</sup> The second source is the US Department of Agriculture’s FoodData Central, which provides product-specific nutrient information. The final source is from images of products provided by major online retailers, from which I hand-record nutrition information from pictures of the Nutrition Facts

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<sup>3</sup>The median household stays in the sample for about seven years.

<sup>4</sup>“Reference card goods” are underreported in the Nielsen data, and Nielsen provides sampling weights for researchers who choose to include reference card goods in their analysis that upweight households that scan reference card items.

<sup>5</sup>Over 2,000 consumer applications have licensed these data, as have other researchers such as Dubois, Griffith, and Nevo (2014) who also match these data to Nielsen products.

panel.

I follow an imputation process similar to that of Dubois, Griffith, and Nevo (2014) to label each purchased product with nutrient information. I begin by dropping non-food products, alcohol, tobacco, weight-loss/diet aids, and reference card goods.<sup>6</sup> I then match Nielsen products to Syndigo nutrients, covering about TODO% of purchased products. I next match products without a direct match to those that share the same product description, brand, product module,<sup>7</sup> size type,<sup>8</sup> flavor, variety, type, formula, and style, which adds TODO% to the matched data. After that, I allow matches across brands, including storebrand goods, which do not have matches in the Syndigo data. This step matches TODO% of purchased products. The next step relaxes the flavor, variety, type, formula, and style restrictions, labeling TODO% of products. I then impute with product module for ones that have sufficient labeled observations, which covers TODO% of products. Finally, I manually label the remaining TODO% of products using the USDA and online retailer data sources.

After aggregating to the household-week level, I topcode each nutrient measure at the 99th percentile for all values greater than the 99th percentile to reduce the influence of outliers.

Finally, I use the survey constructed by Broda and Parker (2014) to observe when panelists received their ESPs.

## 4 Methods

In this section, I describe the empirical strategies used in this paper.

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<sup>6</sup>Reference card goods do not have barcodes and are generally underreported by panelists. Additionally, they are reported as *counts* as opposed to weights, thereby making it difficult to label these products with correct nutrition information.

<sup>7</sup>Nielsen defines over 1,000 product categories called *product modules*.

<sup>8</sup>“Size type” is whether the product is measured in counts versus volume versus weight, which is generally (but not always) consistent within product module.

## 4.1 Effect on payment on nutrient demand

I use a stacked event study design to estimate the impact of receiving an ESP on purchases. The primary specification is as follows:

$$y_{it} = \text{ESP}_{i(t)} + \alpha_i + \gamma_t + \epsilon_{it} \quad (1)$$

where  $y_{it}$  is the total purchases of a food or nutrient  $y$  by household  $i$  during week  $t$ ,  $\alpha_i$  is a household fixed effect,  $\gamma_t$  are week fixed effects, and  $\epsilon_{it}$  is an unobserved error term.  $\text{ESP}_{i(t)}$  is a set of indicators for week since the household received its ESP, and I omit the week prior to when the household receives its check such that all coefficients are relative to that week. Identification of the indicator dummies comes from timing of the ESPs, which is assigned as-good-as random conditional on ESP type (paper check or direct deposit) and receiving the payment when expected, as described in detail in Section 2.1. Under unconfoundedness and SUTVA, the coefficients  $\text{ESP}_{i(t)}$  for  $i(t) \geq 0$  identify the average causal effect of receiving an ESP on purchases of  $y$ . I use the inverse hyperbolic sine of levels of  $y$  such that the coefficients can be interpreted as percent changes.

## 4.2 Heterogeneity

According to the Permanent Income Hypothesis, those with sufficient liquidity should not change their nutrient purchases dramatically upon receiving an ESP. To examine heterogeneous treatment effects of the ESP, I adjust 1 to include an interaction term:

$$y_{it} = \text{ESP}_{i(t)} + \beta I_i * \text{ESP}_{i(t)} + \alpha_i + \gamma_t + \epsilon_{it} \quad (2)$$

where  $I_i$  is an indicator equal to 1 when a household has a time-invariant characteristic



of interest, such as having at least two months of income saved in liquid assets. Note that time-invariant, in this setting, requires that the characteristic be constant during the periods studied, which is assumed to be true for all Nielsen-provided demographic information, which is updated annually.

## **5 Results**

In this section I present results using the methodology discussed in Section 4 estimated using data described in Section 3.

### **5.1 Balance check**

I begin by examining whether I can predict household-level demographic information using timing of the household's ESP. Doing so would suggest that the timing of the ESP was not random.

### **5.2 Effect on panelist behaviors**

I next estimate the effects of receiving an ESP on panelist behaviors, such as number of trips taken, items scanned, and total amount spent using equation 1.

### **5.3 Effect on nutrient demand**

Next I examine the effects of receiving an ESP on nutrients purchased.

### **5.4 Treatment effect heterogeneity**

Here I investigate heterogeneous treatment effect estimates along household characteristic dimensions.

I start by estimating Equation 2 using as outcomes the inverse hyperbolic sine of grams of sugar and ounces of regular soda purchased. For the interaction terms, I use dummies for

## **6 Discussion**

### **6.1 Limitations**

The methods used in this paper have some limitations. First, I rely on panelists' reported ESP receipt date, which may contain measurement error and bias the coefficients towards zero. Second, the results are specific to the time period studied, namely the beginning of the worst economic recession in recent history. It is possible that the results may look very different if, for example, payments were made during an expansionary period. Third, the estimates are specific to a one-time transfer. Further research is warranted for estimating the impact of repeat transfers on household nutrition, which would help policymakers predict nutrition-related health impacts of transfer programs like UBI.

## **7 Conclusion**

In this paper, I

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## Appendix