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# Unconditional Cash Transfers Impact on Health Behaviours During a Crisis: A Natural Experiment Using the CARES Act 2020

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## Unconditional Cash Transfers Impact on Health Behaviours During a Crisis: A Natural Experiment Using the CARES Act 2020

### Highlights

- Reception of unconditional cash transfers (UCTs) was associated with higher use of facemasks during the COVID-19 pandemic.
- UCTs' impact on facemask uptake was stronger amongst poorer households and unaligned voters.
- UCTs did not improve compliance with other public health recommendations, such as: working from home, avoiding high risk individuals, avoiding public spaces, and hand-washing.
- UCTs did not affect the long-term consumption of demerit temptation goods such as alcohol.

### **Abstract**

*Unconditional cash transfers (UCTs) were an important government intervention during the COVID-19 pandemic, with as many as 191 countries adopting some form of UCT between March and June 2020. However, the extant literature evaluating UCTs' impact on individual health behaviours and compliance with public health recommendations has remained limited to low-income countries. To fill this gap, the present article exploits natural variation in Economic Impact Payments' (EIPs) reception in the United States – associated with the CARES Act 2020 – to evaluate UCTs' impact in a high-income country during a crisis. Drawing on the first 19 waves of the Understanding Coronavirus in America (UCA) panel – in an event studies design using the interacted-weighted estimator – this paper is able to robustly assess EIPs' effects.*

*Firstly, this article shows that EIPs' reception was associated with a significant increase in facemasks' use, with effects being stronger amongst poorer households. When all eligible households are included, this work estimates that individuals were 4.7-percentage points more likely to wear a mask following EIPs' reception. When the sample is restricted to the bottom income quartile, this effect rises to 6.3-percentage points. Second, the paper examines EIPs' impact on compliance with other public health recommendations – working from home, avoiding high risk individuals, avoiding public spaces, and handwashing – finding no evidence that UCTs promote their uptake, suggesting that the mechanism is specific to facemasks. Thirdly, the article considers whether EIPs encouraged demerit temptation goods' consumption, focusing on alcohol. Ultimately, no robust evidence can be seen for EIPs increasing alcohol consumption overall.*

*In robustness checks, the paper also explores partisanship's role influencing UCTs' impact. Overall, this supplementary work shows few differences. However, for facemasks the paper finds that EIPs' were most impactful amongst unaligned voters, suggesting that UCTs are best positioned to change the behaviour of those without strong political affiliations.*

**Keywords:** Unconditional Cash Transfers, COVID-19, Health Behaviours, CARES Act 2020

## Introduction

Unconditional cash transfers (UCTs) are widely recognised as an effective means of improving population health (Gibson et al 2020; Yoshino et al 2023). Although debate persists – with particular concern that transfers may principally be used on temptation goods (Banerjee & Mullainathan 2010; Somville & Vandewalle 2018) – UCTs have been robustly associated with uptake of protective health behaviours for which there is a cost-barrier, with prominent examples including: bed nets, contraception, and sanitary products (Khan et al 2016; Novignon et al 2022; Sibson et al 2018).

COVID-19's emergence led to a large expansion in UCTs' provision across the globe, with as many as 191 countries instituting some form of UCT between March and June 2020 (Gentilini et al 2020), offering an ideal context to examine their impact on health behaviours during a crisis. However, the extant literature exploring UCTs' role in shaping protective health behaviours during the pandemic has remained limited to low and middle-income countries (Brooks et al 2022; de Leon et al 2023; Karlan et al 2022; Stein et al 2022). Although a few papers (Jacob et al 2022; Kumar et al 2023; Pilkauskas et al 2023) have looked at UCTs' role at an individual level in high-income countries, their impact on health behaviours has not been examined. This leaves an open question as to whether UCTs were an effective tool in shaping behavioural outcomes in high-income countries during the pandemic.

To fill this gap, the present article uses Emergency Impact Payments (EIPs) from the CARES Act 2020 in the US, to assess how UCTs impacted behaviours during the COVID-19 pandemic. This paper exploits the first 19 waves of Understanding Coronavirus in America (UCA), a panel following 6716 individuals from 10<sup>th</sup> March to 23<sup>rd</sup> December 2020 who were resurveyed approximately every two weeks, to provide detailed evidence on how individual behaviours evolved during the pandemic. Implementing Sun & Abraham's (2021) IW estimator in an event-studies design, this paper provides robust estimates for EIPs' impact, using individuals in eligible households who never received the transfer as the control group.

To frame this work, the paper is structured around three questions which reflect on specific theoretical claims within the literature concerning UCTs. First, did UCTs encourage the uptake of facemasks? This article begins with a focus on facemasks as a health behaviour which is likely to be impacted by UCTs' reception because households had to internalise some of this new behaviour's expense by buying their own masks (OECD 2020).

Second, Did UCTs improve compliance with public health recommendations more generally? In line with Deiana et al's (2022) claims that welfare lifted compliance with public health recommendations more generally during the pandemic, this article studies whether UCTs impacted behaviours for which the costs of compliance were not explicit. Specifically, the paper examines: working from home, avoiding high risk individuals, avoiding public spaces, and handwashing.

Finally, mirroring concerns from the literature around UCTs' unintended consequences, this paper asks: did UCTs encourage the consumption of demerit temptation goods? In particular, the article will focus on the case of alcohol consumption.

Ultimately, this article shows that UCTs' reception was associated with higher use of facemasks. When all eligible households are included, this work estimates that individuals were approximately 4.7-percentage points more likely to wear a mask following EIPs' reception. When the sample is restricted down to the bottom income quartile, this effect rises to 6.3-percentage points. However, no evidence can be found that receiving UCTs leads either to improved compliance with other public health recommendations or increased consumption of temptation goods in the long-term. This suggests that the mechanism is specific to facemasks.

In additional robustness checks the paper reruns this analysis, stratifying results by partisanship. This work relies on a smaller sample of individuals ( $n = 5305$ ) – those who answered both Understanding America's 2019 End of Year Survey and UCA – because partisanship was not directly asked about in UCA. Whilst generally results are not substantially different, this supplementary work shows that UCTs' impact on facemask was stronger amongst unaligned voters (i.e. those who are neither Republicans nor Democrats). This suggests that UCTs are best positioned to change the behaviour of those without strong political affiliations.

Overall, this article makes a novel contribution to the wider literature by demonstrating UCTs' impact in a high-income country during a crisis. This work rules out the most pessimistic view of UCTs by showing their positive impact on facemask uptake whilst also demonstrating that they did not overall impact on temptation goods' consumption. However, this article is equally able to rule out the most optimistic view. It is clear that UCTs do not generally lead to higher uptake of protective health behaviours.

## **I. Unconditional Cash Transfers: A Theoretical Background**

UCTs continue to engender debate in the wider literature and public health policy (Cooper et al 2020; Pega et al 2022). Most of the discussion centres on credible mechanisms: how do UCTs change behaviours? This paper will not directly resolve this issue, but will use it to place this article's work in dialogue with the wider literature. There are broadly two schools of thought in responding to this question.

The first school argues that UCTs act principally as an economic tool, helping to overcome cost-barriers associated with certain behaviours. Costs can be an important impediment to preventative health practices' adoption (Krist et al 2010; Polec et al 2015; Rezayatmand 2013). Undoubtedly, higher costs price-out some individuals, excluding any possibility of take-up. But even when individuals can afford the expense, present bias (Cheung et al 2022; Mahajan et al 2020; O'Donoghue & Rabin 2015) – the tendency to favour consumption with immediate benefits, rather than consumption with delayed benefits – can deter many if prices are sufficiently high. Present bias can be significantly more acute in poorer households where the

trade-off between immediate and delayed consumption is sharper (Aue et al 2016; De Bruijn & Antonides 2022).

UCTs are thought to be a more efficient response to such cost-barriers – compared to other potential solutions – for three principal reasons. First, by lifting individual budget constraints, UCTs directly tackle the trade-off between short and long-term consumption (Pan & Singhal; van der Heijden 2022). Second, giving individuals fungible cash allows households to use the transfer to best reflect their needs (Aker 2017; Hagen-Zanker & Himmelstine 2015; Haushofer & Shapiro 2016). Third, making transfers unconditional helps to minimise disincentives for uptake (Baird 2013; Daalen 2022; Khan 2016).

During the pandemic, few behaviours came with explicit costs, but one did: facemasks. Although facemasks were a cost-efficient preventive measure against coronavirus (Bartsch et al 2022; Howard et al 2021; Izadi et al 2023) they required households to internalise some of the expense by buying their own masks. This issue was compounded by higher global demand for facemasks and supply-chain problems substantially driving up prices in the early pandemic (Ahn 2021; Fung & Roberts 2021; Goel & Haruna 2021; OECD 2020; Oxenham 2020) – with reports that on platforms such as Amazon facemasks' price increased by over 500-percent (Warkentin 2021) [1]. From this first theoretical perspective, UCTs might be expected to promote facemasks' uptake, particularly amongst the poorest households who face the sharpest trade-off between long and short-term consumption, although not necessarily other behaviours.

The second school of thought sees UCTs not only as an economic tool but also as a political tool. Deiana et al (2022) have argued that giving individuals any government transfer – in their case they examined food stamps in Italy during the pandemic – might encourage a *contractarian effect* whereby individuals become more likely to comply with all health recommendations. The essence of this view – which builds on wider work coming from economics, sociology, political science, and social psychology (Besley 2020; Feld & Frey 2007; Tyler 2006) – is that receiving welfare makes individuals perceive institutional and political processes as fairer, thus encouraging them to comply with all instructions from government.

This political view of UCTs does not exclude the economic one, but it implies different outcomes. From an exclusively economic perspective, UCTs are a remedy to explicit monetary costs. From a political perspective, UCTs are an intervention against low-trust in government. As such, the first view implies that UCTs will have a limited impact, changing only behaviours which depend on some good being consumed. Whilst the second view predicts a much wider role for UCTs, covering practically all public health recommendations, and working especially well for those least trusting of government.

Undercutting both these perspectives, there are also grounds for scepticism around whether UCTs are always beneficial. One major critique of UCTs, especially if present bias is a concern, is that individuals might principally use transfers on temptation goods (Banerjee & Mullainathan 2010; Somville & Vandewalle 2018). However, recent studies have found only

limited evidence that UCTs' provision leads to increases in temptation goods' consumption (Al Izzati et al 2023; Brune et al 2022; Evans and Popova 2017; Handa et al 2018a).

Equally, there may be concerns that UCTs are a short-lived solution and that once transfers cease behavioural changes will revert (Altındağ & O'Connell 2023). Although, again, recent evidence from both high and low-income countries suggests that UCTs generate long-term benefits even if transfers cease (Aizer et al 2016; Handa et al 2018b; Millán et al 2019; Parker & Vogl 2018).

These competing theoretical perspectives might lead to divergent predictions about UCTs' impact on health behaviours in a crisis context. Optimistically, it could be argued that the political and economic mechanisms – theorised in the existing literature – would work together to promote compliance with a broad range of protective health behaviours into the medium/long-term, whilst minimising temptation goods' uptake. Pessimistically, the persistent problem of present bias might lead to a scenario in which UCTs do nothing to promote protective health behaviours and individuals use these transfers exclusively on temptation goods. Ultimately, this is a field without strong priors and therefore empirical evidence is required to arbitrate between these positions.

## **II. The Existing Evidence on UCTs During the Coronavirus Pandemic**

Although there is an extensive literature around UCTs in general, most existing evidence is focused on low-income countries (Bastagli 2019; Shah & Gennetian 2023). Moreover, despite a historic expansion in welfare during the pandemic (Cantillon 2021; Moreira & Hick 2021; Weisstanner 2022), comparatively little is known about UCTs' impact during the COVID-19 crisis specifically.

To date, there have been five significant studies into UCTs' effects on health behaviours and compliance with public health recommendations during the pandemic:

- Brooks et al (2022) conducted a randomised control trial with female microentrepreneurs in Kenya, which showed that receiving a one-time unconditional transfer was associated with higher spending on PPE (personal protective equipment) and greater precautionary management practices.
- Examining a \$1000 one-time transfer amongst refugees in Uganda, Stein et al (2022) found no evidence for improved compliance with health recommendation generally as a result of UCTs, but in supplementary work discovered that those who received the transfer were approximately 6-percentage points more likely to wear a facemask (Kimani et al 2020).

- In Brazil, de Leon et al (2023) found – using cross-sectional data with a regression discontinuity design – that individuals who qualified for the Auxilio Emergencial (a large cash transfer given to low-income households during the pandemic in monthly instalments) were less likely to have contracted COVID-19, which the authors attribute to reduced working hours.
- In Ghana, Karlan et al (2022) used a difference-in-difference design to show that reception of a mobile money transfer – in which individuals received the equivalent of \$15 every three weeks – led to improved social distancing.
- Wright et al (2020) found evidence, at an aggregated level, that counties in the US which received larger transfers from the CARES Act 2020 experienced a significantly greater decline in population movement – based on mobile phone signal noise – although it is important to underline that these results may not reflect cash transfers’ individual impact.

This paper seeks to contribute to the debate around UCTs by examining EIPs’ impact on health behaviours in the US. In so doing, this paper is the first to estimate UCTs’ effects on health behaviours at an individual level in a high-income country during the pandemic.

### **III. The CARES Act 2020**

The CARES (Coronavirus Aid, Relief, and Economic Security) Act 2020 provides a useful empirical setting to understand UCTs’ impact on behaviours during the pandemic. Signed into law on 27<sup>th</sup> March 2020 by President Trump, the CARES Act was the first major stimulus package adopted by the US during the crisis, subsequently followed by the COVID-related Tax Relief Act in December 2020 and the American Rescue Plan Act in March 2021.

The CARES Act included a tax-free, one-time, unconditional cash transfer to all qualifying US households, called Emergency Impact Payments (Bhutta et al 2020). Married couples who filed their taxes jointly and surviving spouses, with a gross household income under \$150,000, received a cheque for \$2400. Single filers, with a gross household income under \$75,000, received a cheque for \$1200. For every dependent child in the household under the age of 17 an additional \$500 was received. For every \$100 above the threshold to receive the full amount, an individual received 5-percent less. In general individuals didn’t apply for EIPs, rather the IRS identified eligible individuals, although the option existed to make an application via an online portal.

What makes it possible to use the CARES Act as a natural experiment is that some eligible households never received their cheque. Detailed work by Clark et al (2023) using data from the IRS and US Census Bureau, estimates that at least 8-percent of eligible individuals never received EIPs from the CARES Act. Amongst EIPs’ recipients: 55-percent received it in the first week of their distribution (the second week of April 2020), another 40-percent received the cheque in the following five weeks, and 5-percent received their cheque more than six



weeks later. Importantly, Clark et al show that there were not significant demographic differences between those who did and did not receive payments, supporting the idea that EIPs' reception was quasi-random. There were differences between groups in terms of treatment timings, with younger individuals and those with children receiving cheques faster. Clark et al attribute this difference to the fact that younger individuals were more likely to file their taxes online whilst those with children were more likely to already be receiving other benefits making it easier for the IRS to assess both groups' eligibility.

Despite the methodological difficulties of obtaining a causal estimate with staggered treatment timings (Goodman-Bacon 2021), the CARES Act offers a relatively unique opportunity to estimate the impact of UCTs given out to a wide segment of the population, in a high-income country.

## **IV. Data and Methods**

### **IV.1. Understanding Coronavirus in America**

This paper uses Understanding Coronavirus in America (UCA) [2], an extension of the pre-existing Understanding America Study, an online panel which has followed a nationally representative sample of US residents since 2014 (Alattar et al 2018). The analysis will rely upon waves 1 to 19 of UCA, covering the period from 10<sup>th</sup> March to 23<sup>rd</sup> December 2020 – with each new wave being fielded approximately two weeks after the last (for a full calendar of waves see table A1 in the online appendix). Although further waves were collected, the analysis is ended in December 2020 to avoid including respondents who received a second round of EIPs associated with the Tax Relief Act 2020. To simplify this paper's work, individuals who were not eligible for the full amount – i.e. married couples/surviving spouses with a gross household income above \$150,000 or single filers with a gross household income above \$75,000 – were excluded from the sample. This leaves a total sample of 6716 individuals. Baseline sample characteristics are shown in table 1.

**Table 1: Baseline Sample Characteristics**

<b>Variable</b>	<b>N</b>	<b>All (n = 6716)</b>	<b>Control (n = 1703)</b>	<b>Treatment (n = 5013)</b>
Age (years)	6710	49.8	45.6	51.3
Household Income (per thousand dollars)	6716	54.9	50.4	56.4
Male (percentage)	6715	39.2	38.6	39.4
High School Diploma (percentage)	6714	93.7	91.4	94.4
Undergraduate Degree (percentage)	6714	34.5	34.0	34.7
Married (percentage)	6711	55.6	47.7	58.3
Number of Children	6716	0.664	0.643	0.671
Republican (percentage)	5305	32.6	25.7	34.4
Democrat (percentage)	5305	38.6	40.9	38.0
White (percentage)	6656	81.6	77.3	83.0
Black (percentage)	6656	11.0	13.2	10.2
Asian (percentage)	6656	6.10	7.84	5.51
Hispanic (percentage)	6714	18.1	23.9	16.1
Native America (percentage)	6656	5.84	6.65	5.57
Pacific Islander (percentage)	6656	1.98	2.49	1.81

**Notes:** The column labelled N reports the number of respondents for whom information exists on each item. The column labelled All shows results for all respondents regardless of treatment status. The column labelled Control shows the results for those who never received EIPs. The column labelled Treatment shows the results for those who received EIPs. Children are defined as being any member of the household under the age of 17. Ethnic groups are not mutually exclusive. Statistics on Republicans and Democrats are based on self-reported alignment, not voter registration.

## IV.2. Dependent Variables

This article's first dependent variable of interest is a self-reported dichotomous indicator of whether a respondent wore a facemask in the last seven days. Respondents were asked "*Which of the following have you done in the last seven days to keep yourself safe from coronavirus in addition to what you normally do? Only consider actions that you took or decisions that you made personally. Worn a face mask*". Respondents had the option to answer "Yes" (coded as 1) or "No" (coded as 0).

Importantly, this question appeared alongside a battery of other behaviours, four of which will be used to assess whether EIPs promoted compliance more generally. These four are: working from home ("*Worked or studied at home*"), avoiding high risk individuals ("*Avoided contact with people who could be high-risk*"), avoiding public spaces ("*Avoided public spaces, gatherings, or crowds*"), and handwashing ("*Washed your hands with soap or used hand sanitizer several times per day*"). Other items were included in the battery but are not analysed in this paper either because they are not protective behaviours (i.e. "*Prayer*"), or the questions were not included in every wave (i.e. "*Visiting a Chinese restaurant*"), or they were behaviours which potentially affected only a small number of respondents (i.e. "*Postponing air travel*").

To assess EIPs' impact on temptation goods' consumption, this paper will also make use of a question related to respondents' alcohol consumption. Respondents were asked "*Out of the past seven days, what is your best estimate of the number of days that you did each of the following activities? Drank alcohol*". Respondents reported a number between 0 and 7. To simplify interpreting these results' magnitude, the variable is standardised such that every increase of 1 represent a 1 standard deviation change. Although other temptation goods were asked about in the survey, they are not analysed here either because they are highly vulnerable to social desirability bias (i.e. "*Used cannabis products such as marijuana*") or because they weren't asked about in every wave (i.e. "*Smoked all or part of a cigarette*").

## IV.3. Independent Variables

The paper's main predictor is a binary indicator of whether a respondent had received their EIP or not. From wave 2 onwards, respondents were asked "In the past month, did you or anyone in your household receive any of the following government benefits? Economic stimulus funds". The first wave that a respondent answers "Yes" to this question is identified, then that individual is coded as 0 in all previous waves and as 1 from that wave onwards. In the case where a respondent never answers "Yes" they are always coded as 0. To subsequently model dynamic effects, the number of waves before/after EIPs' receipt are also recorded. The intuition of this approach is that although EIPs represent a transient one-time shock, this indicator can be used to trace EIPs' evolving impact on health behaviours over time, comparing the paths of treated individuals against never-treated individuals.

Additional control variables are also included to reflect the fact that changes in behaviours may be a consequence of: (1) local policy differences, (2) evolving risk-perception (3) differences in exposure to COVID-19.

First, to account for differences in state-level mask mandates, a binary indicator is included – based on data from the Oxford COVID-19 Government Response Tracker (Hale et al 2021) – which is coded as 1 if the state in which the respondent resides had a compulsory mask ordinance in place on the day of their response and 0 otherwise. An additional dichotomous indicator is also included which records whether a respondent was subject to a stay-at-home order at the time of their response, coded as 1 if the state in which the respondent resides had a stay-at-home order in place on the day of their response and 0 otherwise.

Second, to evaluate risk-perception, two variables are included which ask participants to assess their risk of infection from COVID-19 (in percentage terms) and their risk of dying from coronavirus (if contracted). Both variables are normalised between 0 and 1.

Finally, a dichotomous variable is also included to indicate whether an individual has been diagnosed with COVID-19, equal to 1 if they have received a positive diagnosis and 0 otherwise. Summary statistics for all these variables are presented in table 2.

**Table 2: Descriptive Statistics**

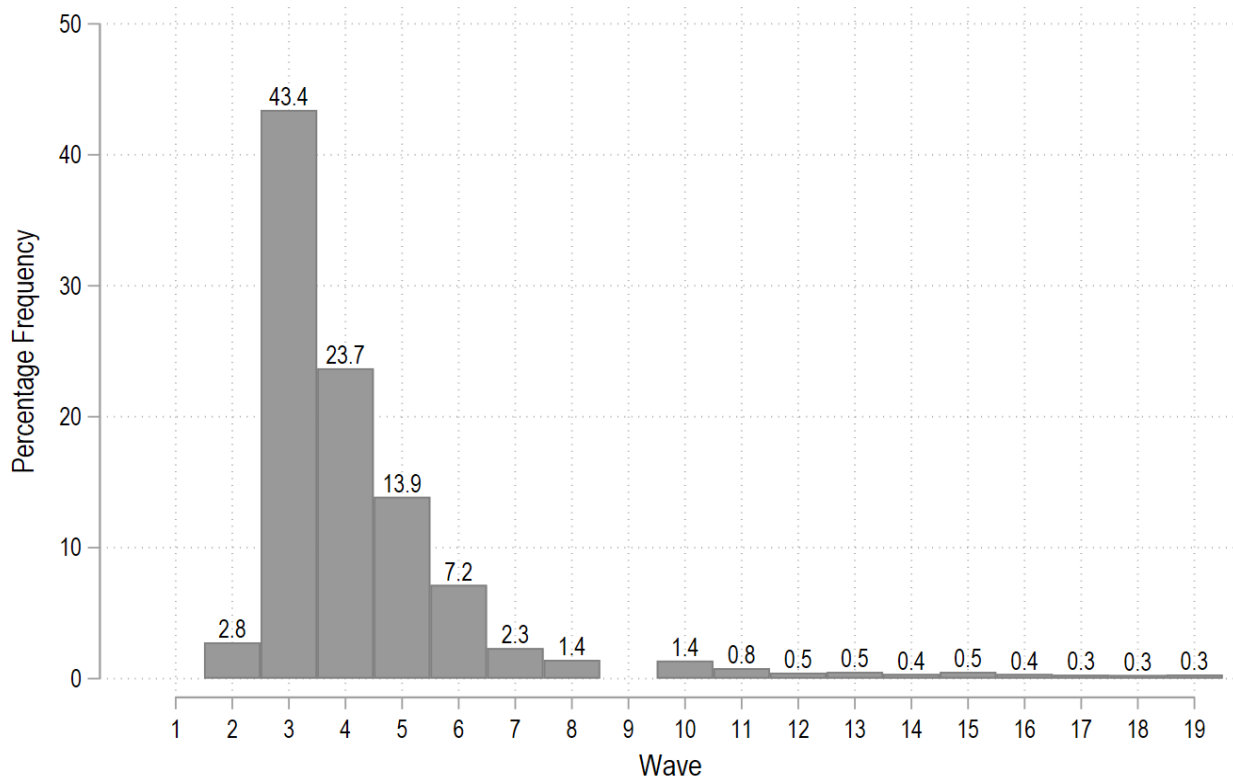
<b>Variable</b>	<b>Mean</b>	<b>Std. Devia- tion</b>	<b>Percentage Frequency</b>	<b>Min</b>	<b>Max</b>
<i>Dependent Variables</i>					
Wearing a Facemask	0.823	0.382	82.3	0	1
Working from Home	0.434	0.496	43.4	0	1
Avoiding High Risk Individuals	0.781	0.414	78.1	0	1
Avoiding Public Spaces	0.752	0.432	75.2	0	1
Handwashing	0.932	0.252	93.2	0	1
Alcohol (Number of Days a Week Consumed, Standardised)	0.629	1		0	3.42
<i>Independent Variables</i>					
Emergency Impact Payment	0.837	0.369	83.7	0	1
Compulsory Mask Ordinance	0.787	0.410	78.7	0	1
Stay-At-Home Order	0.969	0.172	96.9	0	1
Risk of Infection	0.227	0.218		0	1
Risk of Death	0.200	0.257		0	1
Diagnosed With Covid	0.005	0.073	0.50	0	1

#### IV.4. Event Study Design with IW Estimator

To overcome the methodological difficulties of staggered treatment timings (Goodman-Bacon 2021), this paper will not use the commonly applied two-way fixed effects estimator. Instead, Sun & Abraham's (2021) IW (interacted-weighted) estimator will be used which is robust to heterogeneous treatment effects across cohorts.

First, each individual is defined as belonging to a treatment cohort depending on when they received their EIP, meaning there are seventeen treatment cohorts and a control group of individuals who never received EIPs,  $E_i \in \{1, 2, \dots, 17, \infty\}$ . Second, a series of relative time-periods  $l$  are defined for each unit  $i$  at each wave  $t$  based on the wave  $T_i$  in which they first received treatment. As EIPs' reception was relatively concentrated in a small number of waves (see figure 1) - with approximately 43-percent of the treatment group receiving EIPs just in wave 3 and approximately 88-percent receiving EIPs between waves 3 to 6 inclusive – relative time-periods are binned together at the beginning and end to avoid leads and lags which rely exclusively on smaller treatment cohorts to obtain an estimate. As such, all relative time-periods 5 or more waves before treatment are binned together and all relative time-periods 16 or more waves after treatment are binned together. This procedure can be summarised by the follow expression:

**Figure 1: Percentage Frequency (amongst the Treatment Group) of When Emergency Impact Payments were Received**



**Notes:** The figure shows the percentage of respondents (amongst the treatment group) who received Emergency Impact Payments for the first time at each wave.

$$l = \begin{cases} -5 & \text{for } t - T_i \leq -5 \\ T_i - t & \text{for } -5 < t - T_i < 16 \\ 16 & \text{for } t - T_i \geq 16 \end{cases}$$

Sun & Abraham's, three step procedure is then followed. (1) Cohort Average Treatment Effects on the Treated (CATTs) are estimated using an interacted specification. This estimation strategy can be described as follows:

$$y_{it} = \sum_{e \in \{1,2,3,\dots,17\}} \sum_{l=-5, l \neq -1}^{16} \delta_{el} 1\{E_i = e\} \cdot EIP_{it}^l + \theta X_{it} + \alpha_i + \tau_t + \mu_{it}$$

Where  $EIP_{it}^l$  is a vector of lags and leads for receiving EIPs,  $X_{it}$  is a vector of covariates,  $\alpha_i$  are individual fixed-effects, and  $\tau_t$  are wave fixed effects. For the model which estimates the probability of wearing a facemask, the specification controls for whether the state has a compulsory mask ordinance in place, the perceived risk of infection, the perceived risk of dying (from COVID-19), and having been diagnosed with COVID-19. All other models use the same covariates, except that the compulsory mask ordinance dummy is substituted for a dummy recording whether a state has a stay-at-home order in place. Crucially, this first step provides  $\delta_{el}$ , an estimate for the effect of treatment for each cohort at each relative time-period – holding one relative time-period prior to receiving the EIP as the reference period – comparing each cohort only to never-treated units.

(2) Then, the sample share of each cohort  $e$  across cohorts is estimated by its sample analog. (3) Finally, dynamic IW estimates,  $\hat{v}_i$ , are formed by taking the weighted averages of  $\widehat{\delta}_{el}$  from step 1 using sample cohort shares from step 2 as weights. Thanks to this estimator's flexibility, it is also possible to aggregate together the weighted leads to provide an overall ATT analogous to the static effect of receiving EIPs across different cohorts, which will be presented alongside the dynamic results [3].

Given that there are strong theoretical reasons to believe that EIPs' effects will be more pronounced amongst poorer households – where EIPs' relative impact on household income will be greater – this specification will, in the main paper, be estimated separately for: all eligible households, the bottom half of households (according to income), and the bottom quartile of households. Advocates of the view that UCTs function as a political tool may also believe that UCTs' impact will be influenced by individuals' underlying political identification. Therefore, in additional robustness checks the analysis will also be re-run stratified by partisanship.

## V. Results

### V.1. Facemasks

In response to the first research question, this section begins by presenting the IW estimates for EIPs' effect on the probability of wearing a facemask (see table 2 for full results and figure 3 for a graphical representation). When all eligible households are included, the overall ATT (i.e. the aggregation of all relative leads) for receiving EIPs on the probability of wearing a mask is approximately 4.7-percentage points. When the sample is restricted to only the bottom half of households (according to income), EIPs' effect on wearing a facemask rises to roughly 5.7-percentage points. When only households in the bottom income quartile are included, receiving EIPs is estimated to make individuals about 6.3-percentage points more likely to wear a facemask.

**Table 3: IW Estimates for Facemasks**

	All Households (1)	Bottom Half (2)	Bottom Quartile (3)
<b>ATT</b>	0.047*** (0.012)	0.057*** (0.017)	0.063** (0.018)
<b>Emergency Impact Payment (ref = Time: -1)</b>			
<i>Time: &lt;= -5</i>	-0.036 (0.019)	-0.039 (0.028)	-0.051 (0.036)
<i>Time: -4</i>	-0.055 ** (0.019)	-0.089 *** (0.026)	-0.107 *** (0.032)
<i>Time: -3</i>	-0.036 * (0.015)	-0.055 * (0.022)	-0.078 ** (0.027)
<i>Time: -2</i>	-0.029 ** (0.011)	-0.046 ** (0.015)	-0.070 *** (0.018)
<i>Time: +0</i>	0.028 ** (0.009)	0.036 ** (0.012)	0.026 (0.015)
<i>Time: +1</i>	0.046 *** (0.012)	0.058 *** (0.015)	0.056 ** (0.018)
<i>Time: +2</i>	0.059 *** (0.013)	0.079 *** (0.016)	0.079 *** (0.019)
<i>Time: +3</i>	0.049 *** (0.013)	0.067 *** (0.017)	0.062 ** (0.021)



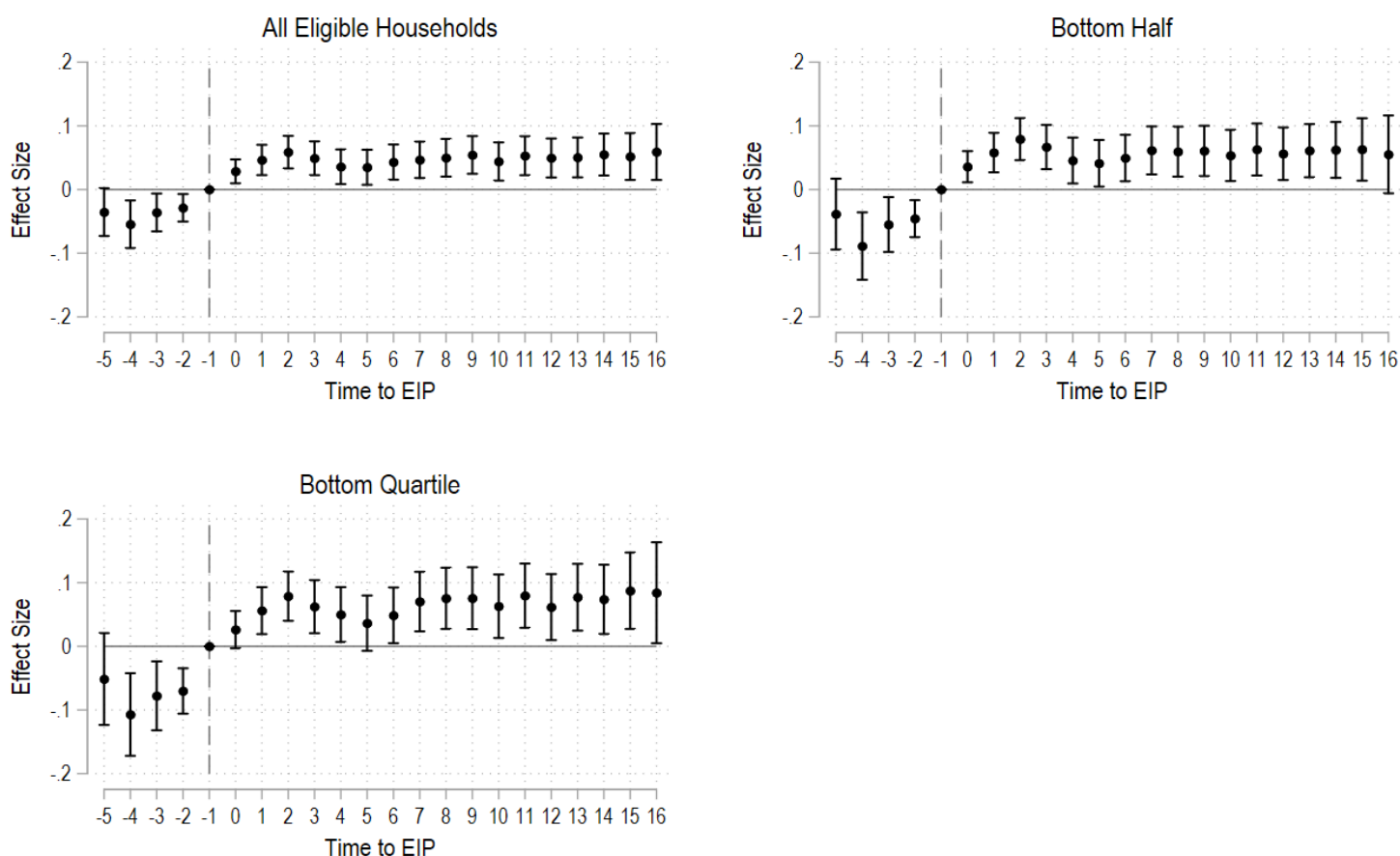
<i>Time: +4</i>	0.036 ** (0.014)	0.046 * (0.018)	0.050 * (0.022)
<i>Time: +5</i>	0.035 * (0.014)	0.041 * (0.018)	0.036 (0.022)
<i>Time: +6</i>	0.043 ** (0.014)	0.049 ** (0.018)	0.049 * (0.022)
<i>Time: +7</i>	0.047 ** (0.014)	0.061 ** (0.019)	0.070 ** (0.023)
<i>Time: +8</i>	0.050 *** (0.015)	0.059 ** (0.020)	0.075 ** (0.024)
<i>Time: +9</i>	0.054 *** (0.015)	0.061 ** (0.020)	0.076 ** (0.024)
<i>Time: +10</i>	0.044 ** (0.015)	0.053 ** (0.020)	0.063 * (0.025)
<i>Time: +11</i>	0.053 *** (0.015)	0.063 ** (0.020)	0.079 ** (0.025)
<i>Time: +12</i>	0.050 ** (0.015)	0.056 ** (0.021)	0.062 * (0.026)
<i>Time: +13</i>	0.050 ** (0.016)	0.061 ** (0.021)	0.077 ** (0.026)
<i>Time: +14</i>	0.055 *** (0.016)	0.062 ** (0.022)	0.074 ** (0.027)
<i>Time: +15</i>	0.052 ** (0.018)	0.063 * (0.025)	0.087 ** (0.030)
<i>Time: &gt;= +16</i>	0.059 ** (0.022)	0.055 (0.031)	0.084 * (0.040)
<b>Compulsory Mask Ordinance</b>	0.104 *** (0.006)	0.107 *** (0.008)	0.115 *** (0.011)
<b>Risk of Infection</b>	0.014 (0.009)	0.021 (0.013)	0.018 (0.018)
<b>Risk of Death</b>	0.015 (0.010)	0.016 (0.013)	-0.007 (0.017)

<b>Diagnosed With Covid</b>	0.009 (0.013)	-0.020 (0.019)	-0.037 (0.025)
<b>Individual Fixed Effects</b>	Yes	Yes	Yes
<b>Wave Fixed Effects</b>	Yes	Yes	Yes
<b>N</b>	93718	47082	27215
<b>R-Squared</b>	0.582	0.571	0.562

**Notes:** \*  $p < .05$ ; \*\*  $p < .01$ ; \*\*\* $<0.001$ . Robust standard errors are reported in parentheses. Standard errors are clustered by individual. The first row shows the Average Treatment Effect on the Treated (ATT), calculated as an aggregation of all relative leads. Relative-periods less than or equal to -5 are binned together and relative-periods greater than or equal to +16 are binned together. The dependent variable is the probability of wearing a facemask.

To contextualise the magnitude of this effect, results for other covariates are also presented. Particularly noteworthy should be the estimates for a Compulsory Mask Ordinance. These results suggest that implementing a Compulsory Mask Ordinance is associated with an: 10.4-percentage point increase when all households are considered, 10.7-percentage point increase when only the bottom half are included, and 11.5-percentage points when only the bottom quartile are considered. This would imply that whilst overall EIPs are not as effective as Compulsory Mask Ordinances in inducing facemasks' uptake, they can make an important contribution to shifting compliance especially amongst the poorest households.

**Figure 2: IW Estimates for Facemasks with 95-Percent Confidence Intervals**



**Notes:** The figure shows IW estimates and 95-percent confidence intervals for the effect of receiving Emergency Impact Payments (EIPs) at each relative time-period before/after reception on the probability of wearing a face-mask. Relative time-periods 5 or more waves prior to EIPs' first reception are binned together. Relative time-periods 16 or more waves after reception are binned together. The top left panel shows estimates when all eligible households are included. The top right panel shows estimates when only the bottom half of households (according to income) are used in the estimation. The bottom left panel shows results when only the bottom income quartile is used.

One concern expressed in the wider literature, is that UCTs might prove to be a short-term solution to changing health behaviours (Altındağ & O'Connell 2023). However, looking at the relative leads, it is clear that the effect of receiving EIPs on the probability of wearing a face-mask does not weaken over time. At relative time-period 0 (i.e. immediately following EIPs' reception) the effect of receiving EIPs on the probability of wearing a facemask is: 2.8-percentage points when all households are included; 3.6-percentage points when the sample is restricted to the poorest half of household; 2.6-percentage points when only the bottom income quartile is considered. By relative time-period 16 (approximately 8-months later) EIPs' impact rises to approximately: 5.9-percentage points when all households are included, 5.5-percentage points when only the poorest half is considered, and 8.4-percentage points when the sample is restricted to the bottom income quartile. Whilst it is perhaps important to hesitate in saying that the effect of EIPs strengthened over time – remembering that the staggered nature of the treatment means that there are also compositional changes happening across relative time-periods – this picture certainly provides evidence that EIPs' effect was not declining.

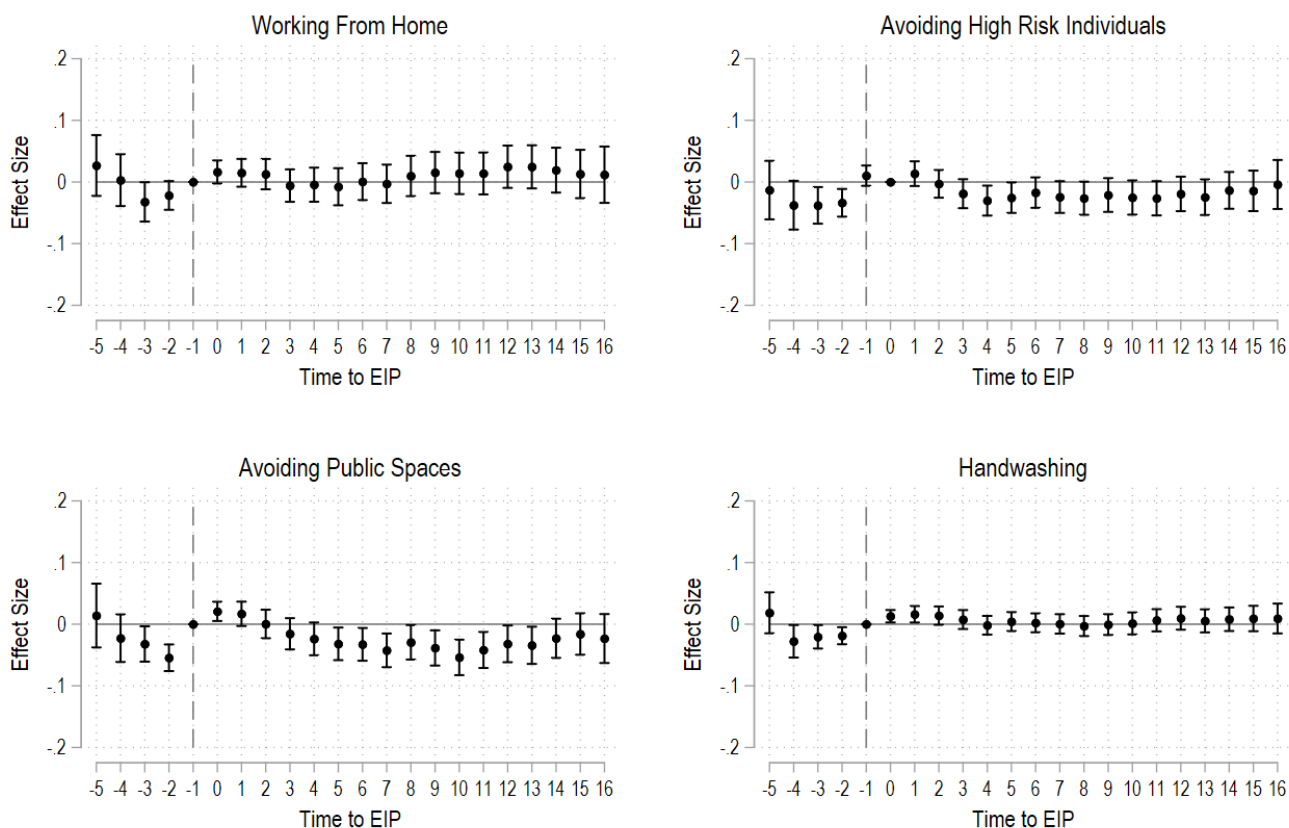
These results robustly show that the reception of EIPs from the CARES Act was associated with greater uptake of facemasks, especially amongst poorer households. It is of course important to underline that these results indicate merely an average across cohorts, and it is entirely possible that the context and timing of EIPs' reception played some role in both the magnitude and direction of the effect. Although such an issue is difficult to study with only a few large treatment cohorts, to provide some intuition of how the effect may vary across time, the ATT for each treatment cohort is shown graphically in the online appendix. Nevertheless, at least within the context they were received, EIPs promoted facemasks' use.

## **V.2. Other Protective Behaviours**

Did UCTs improve compliance with public health recommendations more generally? Given their clear effect on the uptake of facemasks, it seems plausible to suggest that EIPs may also have helped encourage wider compliance with public health recommendations – in line with Deiana et al's (2022) claim that government welfare may promote a contractarian effect during periods of crisis. To respond to this second research question, the same analysis with IW estimators is run – although this time controls for compulsory mask ordinances are replaced with a control for having a stay-at-home order – examining four behaviours: working from home, avoiding high risk individuals, avoiding public spaces, and handwashing. These results are graphically displayed below (see figure 3), but full results can be found in the online appendix. No evidence can be seen that EIPs significantly improved compliance with other public health recommendations. In the case of avoiding public spaces, recipients of EIPs actually seem to

have become slightly less compliant overall. This suggests that the mechanism at play is specific to facemasks and does not apply to protective behaviours more generally.

**Figure 3: IW Estimates for Other Behaviours, With 95% Confidence Intervals**

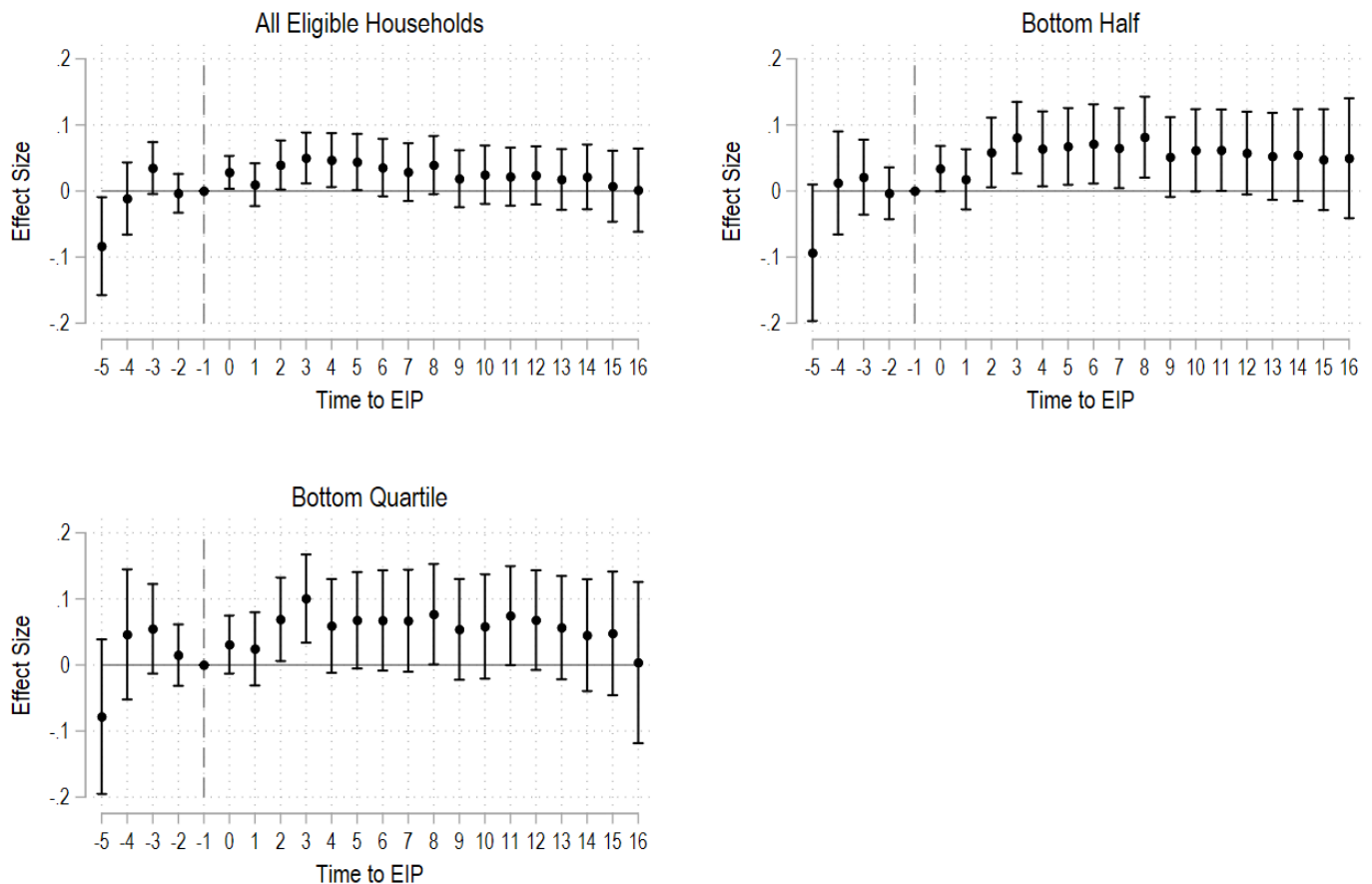


**Notes:** The figure shows IW estimates and 95-percent confidence intervals for the effect of receiving Emergency Impact Payments (EIPs) at each relative time-period before/after reception on the probability of: working from home (top-left panel), avoiding high risk individuals (top-right panel); avoiding public spaces (bottom right panel), handwashing (bottom-right panel). Relative time-periods 5 or more waves prior to EIPs' first reception are binned together. Relative time-periods 16 or more waves after reception are binned together. The top left panel shows estimates when all eligible households are included.

### V.3. Temptation Goods

This paper is also interested in examining a third research question: did UCTs encourage the consumption of demerit temptation goods? To partially respond to this question, this final section estimates the effects of receiving EIPs on the number of days a respondent consumed alcohol. Overall, it is not clear that receiving EIPs led to an increase in alcohol consumption.

**Figure 4: IW Estimates for Number of Days of Alcohol Consumption, With 95% Confidence Intervals**



**Notes:** The figure shows IW estimates and 95-percent confidence intervals for the effect of receiving Emergency Impact Payments (EIPs) at each relative time-period before/after reception on alcohol consumption (standardised). Relative time-periods 5 or more waves prior to EIPs' first reception are binned together. Relative time-periods 16 or more waves after reception are binned together. The top left panel shows estimates when all eligible households are included. The top right panel shows estimates when only the bottom half of households (according to income) are used in the estimation. The bottom left panel shows results when only the bottom income quartile is used.

When all households are included, the overall ATT narrowly misses significance at the 90-percent level of confidence ( $p = 0.109$ ), however some of the early leads are significant and positive. At relative time-periods 0 to 5 the impact of EIPs is significantly estimated at being between 0.02 and 0.05 standard deviations higher (with the exception of relative time-period 1 which is insignificant and has a much lower point-estimate). Moreover, some of the later leads show similar point-estimates, again just missing significance. This might be evidence of a small short-term effect of EIPs on alcohol consumption.

When the sample is restricted to the poorest half and quartile, this early effect seems to strengthen. For the sample including the bottom half (according to income) the effect rises to 0.06 standard deviations overall and is significant at the 95-percent level. However, for the bottom income quartile, although the overall ATT is similar, the results are insignificant. Moreover, the fact that some early lags – for the poorest income quartile – are significantly

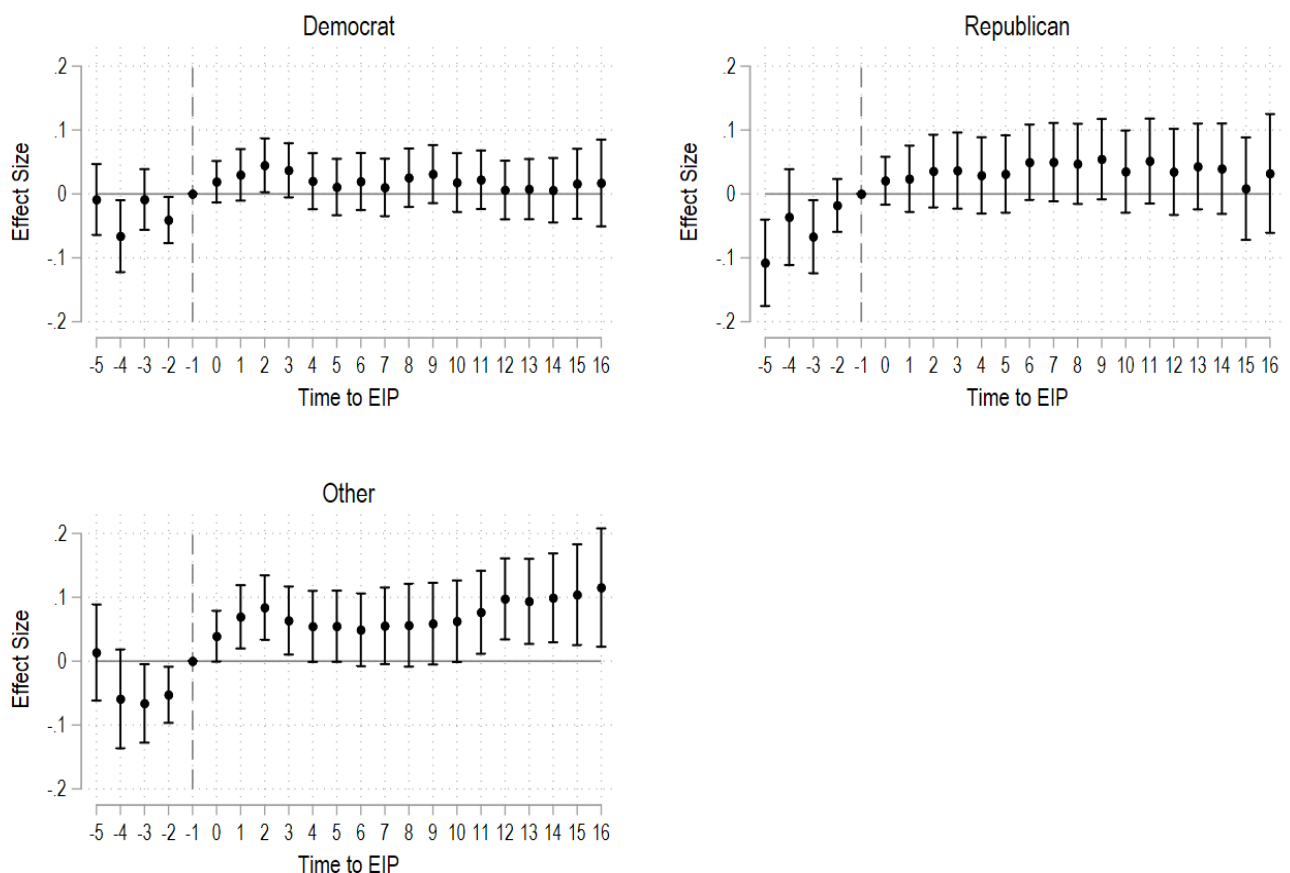
above 0, may throw into question whether EIPs are truly responsible for the early rise in alcohol consumption observed immediately after their receipt.

In summary, EIPs' may have some short-term impact on alcohol consumption, although the evidence is unclear. This is certainly not a large effect – between 0.02 and 0.05 standard deviations for the whole sample. However, this effect may be stronger amongst poorer households.

#### V.4. Additional Robustness Checks – Variation by Partisanship

Partisanship was not directly asked about in UCA. However, given protective health behaviours' polarised nature in the US during the pandemic – and its theoretical importance – it is critical to also consider how partisanship might have influenced EIPs' impact. Using Understanding America's 2019 End of Year Survey, individuals' partisanship was recorded based on the following question “*Regardless of if or how you are registered to vote, are you more closely aligned with ...*”. Respondents were coded as being Democrat, Republican or Other. This leaves a smaller sample of 5305 individuals. For every outcome, the results were then rerun stratified by partisanship (see tables 7A-12A in the online appendix).

**Figure 5: IW Estimates for Facemasks with 95-Percent Confidence Intervals, Stratified by Partisanship**



**Notes:** The figure shows IW estimates and 95-percent confidence intervals for the effect of receiving Emergency Impact Payments (EIPs) at each relative time-period before/after reception on the probability of wearing a facemask. Relative time-periods 5 or more waves prior to EIPs' first reception are binned together. Relative time-periods 16 or more waves after reception are binned together. The top left panel shows estimates when all eligible households are included. The top left panel shows estimates when only Democrats are used in the estimation. The top right panel shows results when only Republicans are used. The bottom left panel shows results when those who do not feel aligned with Republicans or Democrats are used.

The most important result to note is that for facemasks EIPs' impact was clearly strongest amongst unaligned voters (i.e. those who were neither Republicans nor Democrats). The results are insignificant for Republicans and Democrats. However, for unaligned voters EIPs' reception was associated with a 7-percentage point increase in facemasks' uptake. This perhaps suggests that whilst UCT's may not themselves be a principally political tool – i.e. their effectiveness is contingent on explicit costs' presence – their impact can be influenced by political factors.

## Discussion

This paper sought to fill an empirical gap in the literature, studying UCTs' impact on health behaviours and compliance with public health recommendations during the pandemic in a high-income country. To that end, three research questions were posed to frame the work. First, did UCTs encourage the uptake of facemasks? Second, did UCTs improve compliance with public health recommendations more generally? Third, did UCTs encourage the consumption of demerit temptation goods?

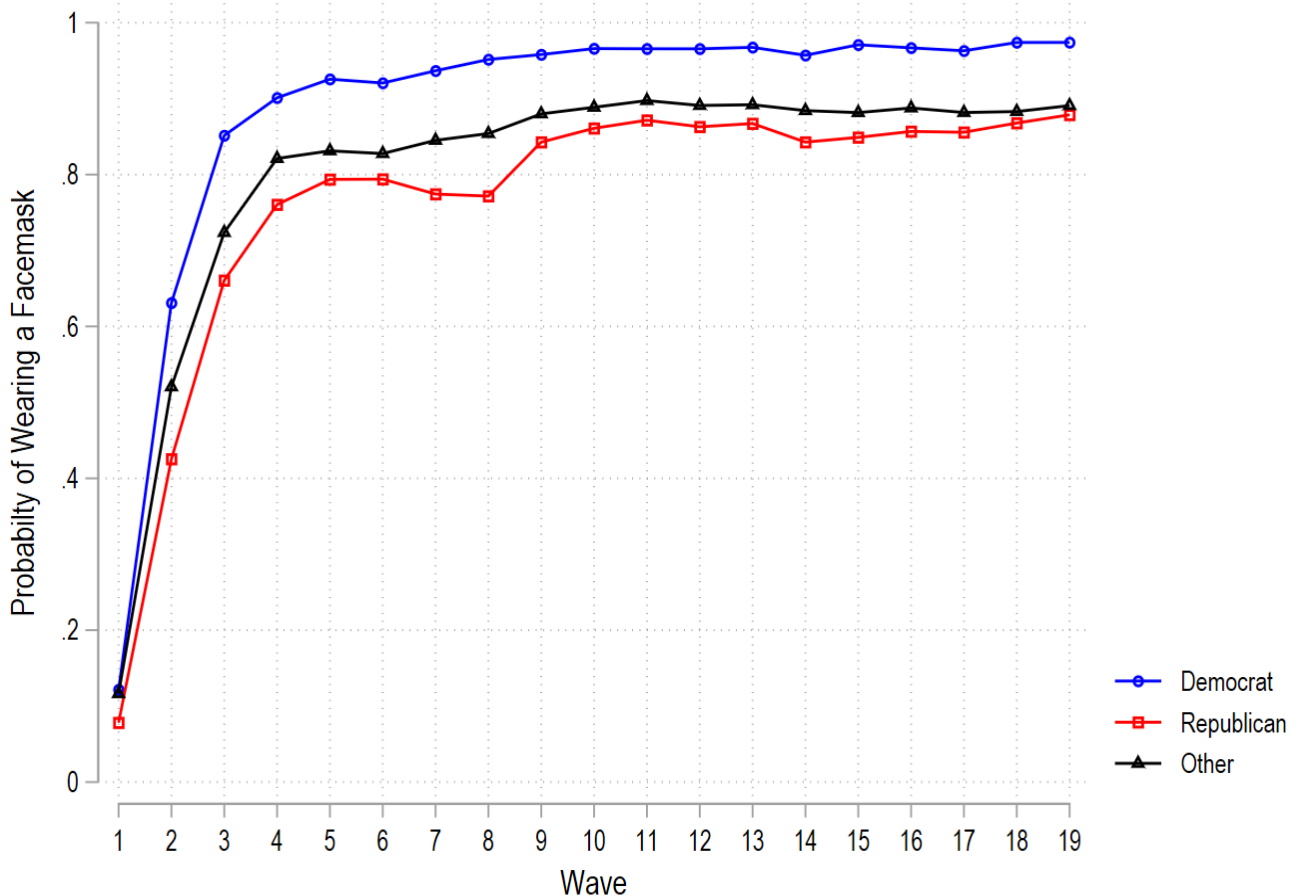
In answering these questions this paper provides a robust analysis of UCTs' consequences for health behaviours. Firstly, this paper rules out the most pessimistic view about UCTs. Concerns that UCTs wouldn't work at all because individuals would use the money on temptation goods alone (Banerjee & Mullainathan 2010; Somville & Vandewalle 2018) proved to be unfounded. A statistically significant and positive effect can be observed for EIPs on the probability of wearing a facemask, particularly amongst poorer households. Similarly, suggestions that UCTs' impact would be short term (Altındağ & O'Connell 2023) – that once the money stopped coming individuals would revert to their normal behaviours – were not borne out. Even 8 months after the reception of EIPs, it is still possible to observe a significant difference between those who did and did not receive cheques. This perhaps suggests that even if UCTs only impact a limited set of behaviours, where they do have an impact, they have the power to be habit-forming, with individuals continuing to keep-up health behaviours even once the money runs out.

However, it is also possible to exclude the most optimistic vision of how UCTs shape behaviours during a crisis. No general contractarian effect (Deiana et al 2022) can be observed, in which the reception of welfare lifted compliance with all public health recommendations. This suggests that UCTs impact was specific to facemasks and it seems likely that this effect was linked to the fact that EIPs were received in a context where: (1) households had to buy their own masks and (2) firms were raising the price of facemasks in response to a sudden demand shock coupled with supply-chain problems (Ahn 2021; Fung & Roberts 2021; Goel & Haruna 2021; OECD 2020; Oxenham 2020).

Crucially, saying that UCTs are principally an economic rather than political tool – i.e. that their impact is limited to behaviours which require some cost barrier to overcome – is not to say that UCTs' effect isn't influenced by political factors. Additional robustness checks showed that UCTs' impact on facemasks was strongest amongst the politically unaligned –

those who consider themselves neither Republicans nor Democrats. Descriptively, it seems that Republicans' facemask use remained low whilst for Democrats it remained high regardless of receiving EIPs (see figure 6). This perhaps suggests that UCTs are most successful at changing behaviours when individuals are not heavily influenced by political leaders and/or parties who either promote a certain behaviour (in which case individuals will comply regardless of any interventions) or who discourage a certain behaviour (in which case individuals won't comply, no matter the intervention's scale).

**Figure 6: Probability of Wearing a Facemask by Wave and Partisanship**



**Notes:** The figure shows the mean average probability for wearing a facemask by wave and partisanship.

Globally, these results are largely inline with much of the highest quality evidence emerging from low-income countries. Both Brooks et al (2022) and Stein et al (2022)/ Kimani et al (2020), in Kenya and Uganda respectively, found that reception of one-time UCTs led to improved use of facemasks without significantly impacting other behaviours. This perhaps suggests that UCT's impact is not vastly different across country-contexts.

It is important to underline that this paper can only consider the effect of cash transfers in the form which they were given out as part of the CARES Act 2020. It is entirely conceivable that had the intervention been designed differently, the results may also have been different. For example, if the UCT had been given out as a series of smaller transfers – rather than as a single



lump-sum – then some impact on other behaviours might have been seen, as was the case for Karlan et al (2022) in Ghana and de Leon et al (2023) in Brazil. However, such concerns lie beyond the scope of what this paper is able to analyse.

Despite its limitation, this article makes an important contribution to the literature on UCTs. Whilst most previous papers, concerned with the period of the COVID-19 pandemic, have estimated UCTs' impact in low-income countries – and often with very specific sub-populations such as microentrepreneurs and refugees – this paper is able to provide an estimate for the effect of UCTs on health behaviours amongst a representative sample in a high-income country. In so doing, this work shows that UCTs had a specific impact on mask-wearing whilst leaving other behaviours largely unaffected.

## Notes

[1] Whilst it is difficult to provide precise estimates – due to the diversity of suppliers and debates about different facemasks' equivalency – in the case of *McQueen et al v. Amazon.com, Inc*, argued before the Northern District of California Court, the plaintiffs presented evidence that facemasks' price increased in excess of 500-percent on Amazon (see <https://digitalcommons.law.scu.edu/cgi/viewcontent.cgi?article=3205&context=historical> for the original filing).

[2] For an overview of the Understanding Coronavirus in America Study see <https://covid19pulse.usc.edu/>

[3] For the IW estimator, I use the implementation provided by the *feols* package in R.

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