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ARTICLE

Unconditional cash transfers and compliance with public health recommendations during the COVID-19 pandemic in the United States

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Abstract

Although unconditional cash transfers (UCTs) were an important government intervention during the COVID-19 crisis worldwide, research covering UCTs' impact on compliance with public health recommendations, at an individual level, remains limited to low-income countries. This study assesses the association between UCTs' reception and compliance with public health recommendations in the United States. Longitudinal data from the Understanding Coronavirus in America panel are applied to difference-in-differences models to estimate how Economic Impact Payments' reception, associated with the CARES Act 2020, impacted a variety of pandemic health behaviours. UCTs' reception was associated with increased uptake of explicitly costly health behaviours, such as facemasks, but not with increased compliance amongst behaviours more generally. Moreover, results document stronger effects amongst poorer households. These findings have theoretical implications for how government transfers impact individual behaviour during periods of crisis and for the direction of future research.

Keywords: unconditional cash transfers; COVID-19; health behaviours; CARES Act 2020

Introduction

In recent years, social scientists have been increasingly interested in unconditional cash transfers (UCTs) and how their reception shapes behaviours and outcomes (Amorim, 2022; Sun et al., 2021). UCTs' advocates have often claimed that such policies can help societies weather new risks and crises by supporting individual behavioural adaptation, especially in the field of health (Gibson et al., 2020), citing evidence from low-income countries that UCTs' reception is associated with increased uptake of important protective behaviours such as bed nets, contraception and sanitary products (Novignon et al., 2022). Nevertheless, many remain sceptical, fearing that UCTs promote temptation goods and risky behaviours (Banerjee & Mullainathan, 2010; Somville & Vandewalle, 2018).

COVID-19's emergence led to a global expansion in UCTs' provision, with as many as 186 countries instituting some form of UCT during the pandemic (Gentilini et al., 2022), offering an ideal context to examine their impact on new behaviours' adoption during a health crisis. Whilst research in low-income countries has shown, to varying degrees, that UCTs' reception during the pandemic was associated with higher levels of compliance with COVID-related measures (Brooks et al., 2022; de Leon et al., 2023; Karlan et al., 2022; Stein et al., 2022), little work has been conducted on high-income nations, leaving a question as to whether these results translate across contexts.

To respond to these wider debates, the present article uses the first round of Emergency Impact Payments (EIPs) from the CARES Act 2020 in the US, largely distributed in mid-April 2020, to assess how UCTs shaped behaviours during the pandemic. This paper exploits waves 2–19 of Understanding Coronavirus in America (UCA), a panel following 6,338 individuals from 1 April to 23 December 2020, to provide detailed evidence on how individual behaviours evolved during the pandemic. Implementing a difference-in-differences design, this paper provides 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 two questions reflecting on competing theoretical explanations for how UCTs impact behaviours. First, do UCTs promote explicitly costly protective behaviours' uptake (for example, facemask use) during crises? Building on theoretical narratives of UCTs focussed on household budgets, articulated most prominently in economics (Cheung et al., 2022; Polec et al., 2015; Rezayatmand et al., 2013), the paper begins with the expectation that the primary channel via which UCTs can shift behaviours is by raising household liquidity, making it easier for those with tight consumption constraints to buy into new costly protective behaviour, but impacting only a narrow range of practices which come with financial costs attached. This question will focus particularly on the case of facemasks, which as will be shown later in the paper, represented a significant new cost – especially for poorer households – during the early pandemic when supply was limited.

Second, do UCTs promote compliance with a broader set of implicitly costly public health recommendations during crises? Developing out from literature on procedural justice theory (Besley, 2020; O'Donoghue et al., 2023; Tyler, 2006), the paper then considers whether UCTs might play a wider role, not only easing consumption tradeoffs but also – through a contractarian effect (Deiana et al., 2022) – lifting trust in governments and thus improving compliance with a wide range of implicitly costly public health recommendations, that is, those without any financial barrier. To that end, the paper extends its analysis to four protective behaviours (working from home; avoiding high risk individuals; avoiding public spaces; and handwashing) and four risky behaviours (attending a gathering with more than ten people; going to a bar or gathering place; close contact with non-household members; leaving the house for non-essential reasons) which – unlike facemasks – did not incur financial costs to compliance.

Ultimately, this article shows that EIPs' reception led to higher use of facemasks, especially amongst poorer households. However, no consistent evidence can be found that receiving EIPs improved compliance with other implicitly costly protective or risky behaviours. In supplementary analysis, the paper further demonstrates that EIPs' reception led to improvements both in household liquidity and subjective perceptions of facemask affordability at a similar magnitude to facemask use, lending credence to the idea that EIPs impacted behaviours primarily through household budgets rather than a contractarian effect.

Taken together, this work makes two important contributions to existing literature on UCTs, with potential lessons beyond the COVID crisis: (1) although UCTs have traditionally been conceived as a means of helping the world's poorest access basic tools of preventative health, this article shows that they can also play a meaningful role in high-income contexts and (2) despite the positive and robust results for facemasks, this article highlights limits to the scope of UCTs' potential, with their impact seemingly circumscribed to practices which come with some explicit cost barrier attached. These conclusions should help shape both policymaker's and researcher's framing of UCTs as a tool of health policy in the future.

Unconditional cash transfers: a budgetary or a contractarian tool?

UCTs continue to engender debate in the social science and policy literature (Cooper et al., 2020; Pega et al., 2022). Most discussion centres on credible mechanisms: how do UCTs change behaviours? There are broadly two narratives responding to this question.

The first is an economic narrative seeing UCTs principally as a budgetary tool, helping households to overcome the cost-barriers associated with certain behaviours by increasing their liquidity. Costs can be an important impediment to health practices' adoption (Polec et al., 2015; Rezayatmand et al., 2013). Undoubtedly, higher costs price-out some individuals, excluding any possibility of uptake. But even when individuals can afford the expense, present bias (Aue et al., 2016; Cheung et al., 2022) – the tendency to favour consumption and behaviours with immediate rather than delayed benefits – can deter many if prices are sufficiently high.

UCTs are thought to be an efficient response to such cost-barriers for three reasons. First, by lifting budget constraints, UCTs directly tackle the tradeoff between short- and long-term consumption (van der Heijden et al., 2022). Second, providing fungible cash allows households to use transfers to best reflect their needs (Aker, 2017). Third, making transfers unconditional minimises disincentives for uptake (Khan et al., 2016).

During the pandemic, few behaviours came with explicit costs, but one did: facemasks. Although facemasks were an effective preventative measure against coronavirus (Howard et al., 2021), 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 driving up prices in the early pandemic (Ahn 2021; OECD, 2020), with reports that on platforms such as Amazon facemasks' price increased by more than 500 per cent.[1] From this first theoretical perspective, UCTs might therefore be expected to promote facemasks' uptake, particularly amongst poorer households, although not necessarily other behaviours.

The second is a political narrative envisioning UCTs not only as a budgetary tool but also as a contractarian tool. Deiana et al. (2022) have argued that giving

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individuals any government transfer encourages a contractarian effect whereby individuals become more likely to comply with all government recommendations. The essence of this view – building on wider literature from procedural justice theory (Besley, 2020; 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 line of reasoning also echoes the work of scholars such as O'Donoghue et al. (2023), who argue that one reason why trust in government rose during the pandemic – as opposed to previous crises in the early 2000s and 2010s – was that government expanded welfare substantially at the beginning of the crisis.

These two views are not mutually exclusive, but they imply different outcomes. From an economic perspective, UCTs are a remedy to explicit monetary costs. From a contractarian perspective, UCTs tackle low trust in government. As such, the first view implies that UCTs have a narrow impact, changing only behaviours which depend on a good being consumed, whilst the second view predicts a wider role for UCTs, covering practically all public health recommendations.

Undercutting both perspectives, there are also grounds for scepticism as to whether UCTs are always beneficial. One critique, especially if present bias is a concern, is that individuals might principally use transfers on risky behaviours and temptation goods (Banerjee & Mullainathan, 2010; Somville & Vandewalle, 2018). In a pandemic context, there may be real concerns that individuals might use transfers on non-essential activities which bring them into closer contact with others.

Equally, there may be concerns that UCTs have short-lived effects and that once transfers end, behavioural changes revert (Altındağ & O'Connell, 2023). However, recent evidence suggests that UCTs generate long-term benefits even if transfers cease (Millán et al., 2019).

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

The existing evidence on UCTs during the coronavirus pandemic

Whilst there is an extensive literature around UCTs, most existing evidence focusses on low-income countries (Bastagli et al., 2019). Moreover, despite a historic expansion in welfare during the pandemic (Moreira & Hick, 2021; Weisstanner, 2022), little is known about UCTs' impact on COVID compliance specifically, although it is important to say that the COVID literature has extensively examined UCTs impact in other domains (Jacob et al., 2022; Kumar et al., 2023; Pilkauskas et al., 2023). To date, there have been only five significant studies into UCTs' effects on compliance with COVID-related measures, all of which show, to varying degrees, improved outcomes following cash transfers' reception.

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Two of these studies provide highly robust evidence from randomised control trials (RCT), albeit with very targeted samples. Brooks et al. (2022) conducted a RCT with female microentrepreneurs in Kenya, which showed that receiving a one-time transfer was associated with higher spending on protective equipment and greater precautionary management practices. Similarly, examining a $1000 one-time transfer amongst refugees in Uganda, Stein et al. (2022) saw no evidence for improved compliance with health recommendations generally, but in supplementary work found that those who received transfers were more likely to wear a facemask (Kimani et al., 2020).

Using individual-level observational data, two further studies have leveraged natural experiments to show that UCTs can lead to higher compliance in low-income countries. In Brazil, de Leon et al. (2023) found that individuals who qualified for the Auxilio Emergencial (a large cash transfer given 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) showed that mobile money of $15 every 3 weeks led to improved social distancing.

At a less granular level, there is one study examining the impact of UCTs on compliance in a high-income country. 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. However, it is important to underline that these results may not reflect cash transfers' individual impact.

Collectively, this pre-existing work leaves important gaps. Most obviously limited geographical coverage, and often very specific sub-populations, raises questions about external validity. But more fundamentally, these findings provoke crucial questions about the breadth of UCT's potential impact. Conflicting evidence on which practices UCTs can change makes it important to further interrogate both the consequences of and mechanism via which such policies function for health behaviours.

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 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 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 years, an additional $500 was received. For every $100 above the threshold, an individual received 5 per cent less, up to a maximum income of $99,000 and $198,000 for single and joint filers, respectively. In general, individuals did not apply for EIPs, rather, the Internal

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Revenue Service (IRS) identified eligible individuals, although the option existed to make an online application.

What makes it possible to use the CARES Act to evaluate UCTs' impact is that some eligible households never received transfers. Detailed work by Clark et al. (2023), using data from the IRS and US Census Bureau, estimates that at least 8 per cent of eligible individuals never received EIPs from the CARES Act and amongst EIPs' recipients, only 55 per cent received it in the first week of their distribution – the second week of April 2020. Importantly, Clark et al. show that there were not significant demographic differences between recipients and non-recipients, supporting the idea that EIPs' reception was to a large extent arbitrary, driven by human error when identifying eligibility. There were, however, differences 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 methodological challenges, the CARES Act offers a relatively unique opportunity to estimate UCTs' impact given out to a wide segment of the population in a high-income country.

Data and methods

Understanding Coronavirus in America

This paper uses Understanding Coronavirus in America (UCA), an extension of the pre-existing Understanding America Study, a longitudinal online panel following a nationally representative sample of US residents since 2014 (Alattar et al., 2018), chosen via address-based sampling. Although the main Understanding America Study follows 14,000 individuals, UCA was based on a smaller sample of 8,815, with a response rate of approximately 82 per cent in each wave. UCA provides a rich source of questions covering health behaviours during the pandemic alongside detailed demographic information which can be used to supplement any analysis.

Importantly in the case of income, Understanding America releases data on individuals' annual household income in brackets (up to annual household incomes of $150,000, above which respondents are all put in the same bracket). For this article's purposes, respondents in the same bracket are all recorded as having the midpoint income. This approach is sufficient in terms of identifying eligibility for EIPs, but not to actively include income in the estimation strategy, for example, by treating EIPs as a proportion of household income.

This paper's analysis relies upon waves 2–19 of UCA, covering 1 April to 23 December 2020 – with each new wave fielded 2 weeks apart (for a full calendar see Table 1A). Wave 1 is excluded because the full battery of questions relating to compliance with public health recommendations was not used. Consequently, those who received their EIP in wave 2 are also excluded because they lack a pre-treatment period. 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 – on the basis of their 2019 household

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income – were excluded from the sample. This leaves a final sample of 6,338 individuals.

Dependent variables

This article's first dependent variable is a self-reported indicator of whether a respondent wore a facemask in the last week. Respondents were asked '*Which of the following have you done in the last 7 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 answered '*Yes*' (coded as 1) or '*No*' (coded as 0).

This question appeared alongside a battery of other behaviours, four of which are used to assess whether EIPs promoted protective behaviours 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 sanitiser several times per day*').

Additionally, to assess individuals' propensity to engage in risky behaviours, the paper makes use of a battery of questions introduced in wave 2. Respondents were asked '*In the last 7 days, have you done the following*'. Participants were then asked about a series of behaviours to which they could answer '*Yes*' (coded as 1) or '*No*' (coded as 0). In this paper four are used: went to a gathering of more than ten people ('*Attended a gathering with more than ten people, such as a reunion, wedding, funeral, birthday party, concert or religious service*'), went to a public meeting space ('*Gone out to a bar, club or other place where people gather*'), had close contact with non-household residents ('*Had close contact (within 6 feet) with people who do not live with you*') and left the house for non-essential reasons ('*Remained in your residence at all times, except for essential activities or exercise*'). The coding of the last item is reversed to make it consistent with the other three.

Other items were included across these two batteries 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*').

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 cases where respondents never answer '*Yes*' they are always coded as 0. To model dynamic effects, the number of waves before/after EIPs' receipt is also recorded. The intuition of this approach is that although EIPs represent a transient one-time shock, this indicator can trace EIPs' evolving impact over time, comparing the paths of treated against never-treated individuals.

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

First, to account for differences in state-level mask mandates, a binary indicator is included – on the basis of 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 1.

Table 1. Summary statistics

Variable	Overall	Control	Treatment (pre-/post-EIP)	Treatment (pre-EIP)	Treatment (post-EIP)
Dependent variables					
Wearing a facemask	0.867	0.837	0.873	0.730	0.904
Working from home	0.445	0.487	0.436	0.490	0.425
Avoiding high-risk individuals	0.793	0.772	0.798	0.847	0.787
Avoiding public spaces	0.765	0.734	0.771	0.878	0.748
Handwashing	0.934	0.890	0.943	0.950	0.941
Gathering of ten-plus people	0.141	0.123	0.144	0.042	0.166
Public meeting space	0.076	0.081	0.075	0.024	0.086
Non-household contact	0.586	0.507	0.601	0.421	0.639
Non-essential activities	0.495	0.475	0.511	0.714	0.468
Independent variables					
Emergency Impact Payment	0.688	0.00	0.823	0.00	1
Stay-at-home order	0.999	0.999	0.999	0.998	1.00
Compulsory mask ordinance	0.837	0.873	0.829	0.496	0.901
Risk of infection	0.229	0.226	0.229	0.245	0.226
Risk of death	0.203	0.195	0.205	0.243	0.196
Diagnosed with COVID	0.006	0.007	0.005	0.005	0.005

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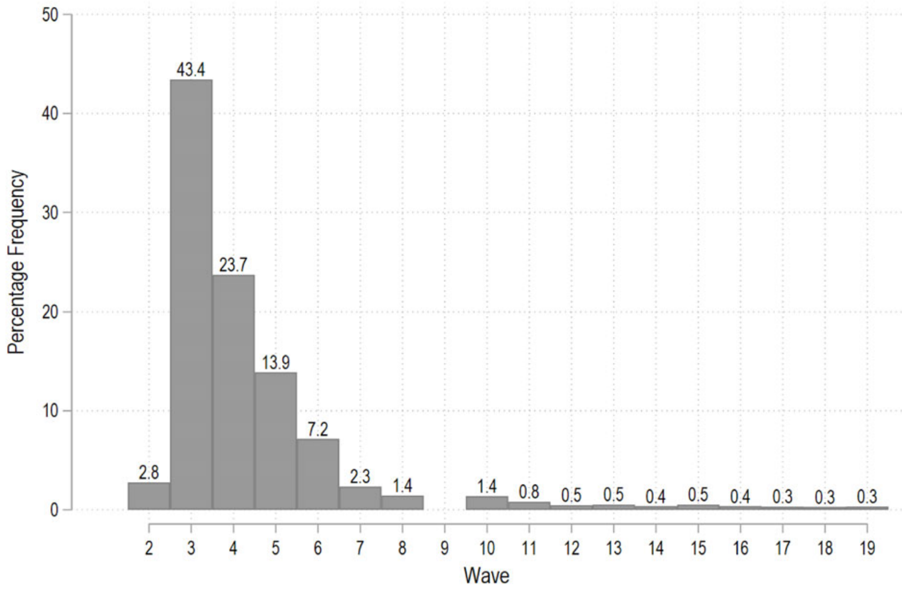

Figure 1. Percentage frequency (amongst the treatment group) of when emergency impact payments were received.

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

Event study design with IW estimator

Staggered treatment timings present a challenge for difference-in-differences designs (Goodman-Bacon, 2021). The comparison between already-treated and newly treated units makes it necessary to assume homogeneity in an intervention's impact across cohorts which may be implausible. Therefore, this paper does not use the commonly applied two-way fixed effects estimator, which is sensitive to differences in treatment timing. Instead, Sun and Abraham's (2021) interacted-weighted (IW) estimator is used, which is robust to heterogenous treatment effects across cohorts. The IW estimator has distinct advantages compared with alternative estimators (see Roth et al., 2023) tackling a similar problem. First, the IW estimator provides easily interpretable results by taking the weighted average of underlying cohort effects. Second, the IW estimator can flexibly incorporate covariates, unlike most other approaches, which abstract away from potential confounders.

To begin, each individual is defined as belonging to a treatment cohort depending on when they received their EIP, meaning there are sixteen treatment cohorts and a control group who never received EIPs, $E_i \in \{3, \ldots, 19, \infty\}$. Figure 1 shows treatment cohorts' distribution across waves. For completeness, those who received their EIP in wave 2 are also included in this figure although excluded from the rest of the analysis.

Then, relative time-periods l are defined for each individual i at each wave t on the basis of the wave T_i in which they first received treatment. As EIPs' reception was concentrated in earlier waves – with 43 per cent of treated individuals receiving

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EIPs in wave 3 – relative time periods are binned together to avoid lags reliant exclusively on smaller cohorts. As such, all relative time periods 4 or more waves before treatment are binned together, summarised by the following expression:

$$l = \begin{cases} -4 \; for \; t - T_i \leq -4 \\ T_i - t \; for -4 < t - T_i \leq 16 \end{cases}$$

Subsequently, Sun and Abraham's three-step procedure is followed. (1) Cohort average treatment effects on the treated (CATTs) are estimated using an interacted specification. This estimation strategy can be described as follows:

$$y_{istd} = \sum_{e \in \{3,4,5 \ldots, 19\}} \sum_{l=-4, \; l \neq -1}^{16} \delta_{el} 1\{E_i = e\} \cdot EIP^l_{istd} + \theta X_{istd} + \alpha_i + \tau_{st} + \varphi_d + \mu_{istd}$$

where EIP^l_{istd} is a vector of lags and leads for receiving EIPs, X_{istd} is a vector of covariates, α_i are individual fixed-effects, τ_t are state-wave fixed effects and φ_d are day (i.e. calendar-date) fixed effects. To further account for potential sources of bias, the main specification controls were for whether a compulsory mask ordinance in place in a respondent's state, whether the state had a stay-at-home order in place, perceived risk of infection, perceived risk of dying from COVID-19 and having been diagnosed with COVID-19. Finally, δ_{el} provides an estimate for the effect of treatment for each cohort at each relative time period – holding one period prior to EIPs' reception as the reference point – comparing each cohort only with never-treated units.

(2) Each cohort's sample share e across cohorts is then estimated by its sample analogue. (3) Finally, dynamic IW estimates, $\widehat{v_l}$, 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 EIPs' static effect across cohorts.

Given strong theoretical reasons to believe that UCTs' effects are more pronounced amongst poorer households – where EIPs' relative impact on household income were greater – this specification is also stratified by income, estimating separately for households above and below the median sample income of $45,000.

Before proceeding, it is important to acknowledge that a causal interpretation of these results relies on two key assumptions. First, the parallel trends assumption: in the absence of EIPs, control and treatment groups would have evolved in the same way. Second, the anticipation assumption: treated individuals should not react to the treatment prior to reception. Although difficult to formally test, it is possible to gain some intuition about these assumptions' plausibility by studying relative lags. If lags significantly diverge from 0, this is a clear indication that one or both of these assumptions has been violated. Therefore, the paper examines not only static, but also dynamic, outcomes of this estimation.

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Improving robustness with matching

One potential critique of the above approach is that despite controlling for individual time-invariant characteristics with fixed effects, control and treatment units may nonetheless diverge due to covariate imbalance. To address this concern, coarsened exact matching is used to find balanced treatment and control groups in terms of age, household income, race (whether the respondent identifies as white, Black, Asian, Pacific Islander or Native American), whether the respondent is Hispanic, whether the respondent is married and the number of children in the household. Baseline sample characteristics both before and after the matching process are presented in Table 2. Throughout the paper, results are presented with and without matching to leverage both the greater precision obtained through a larger sample and the improved robustness achieved through matching.

Results

Facemasks

In response to the first research question, this section begins by presenting IW estimates for EIPs' effect on the probability of wearing a facemask (see Table 3). When all eligible households are included, the overall ATT for receiving EIPs on the probability of wearing a mask is approximately 3 percentage points without matching and 5 percentage points with matching. When the sample is restricted to households earning below $45,000, EIPs' effect on wearing a facemask rises to roughly 5 percentage points with and 6 percentage points with matching. When only households earning above $45,000 are included, EIPs' reception has no significant impact.

To provide evidence for the parallel trends assumption, the paper next studies the relative lags from dynamic models to examine whether there is any observable divergence in respondents' propensity to wear facemasks, between the control and treatment groups, prior to EIPs' reception (see Fig. 2). For both matched the unmatched samples, there is no obvious violation of the parallel trends assumptions, nor evidence for pre-trends which could bias results.

One concern is that UCTs might prove a short-term solution to changing behaviours (Altındağ & O'Connell, 2023). However, examining relative leads, it is clear that EIPs' effect on facemask use does not weaken over time. Focussing on unmatched results for those with incomes below $45,000, EIPs' impact on facemask use sixteen relative time periods (approximately 8 months) later remains positive at 4–5 percentage points. Whilst important not to overinterpret individual timepoints, this picture provides evidence that EIPs were associated with a durable change in facemask use.

Other protective behaviours

Did UCTs improve protective behaviours' uptake more generally? Aside from their clear effect on facemasks, EIPs may also have encouraged wider compliance with public health recommendations in line with contractarian arguments. To respond to this question, the same analysis with IW estimators is run examining four protective

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Table 2. Baseline sample characteristics

Variable	Full sample				Matched sample			
	N	All ($n = 6{,}338$)	Control ($n = 1{,}464$)	Treatment ($n = 4{,}874$)	*N*	All ($n = 2{,}142$)	Control ($n = 1{,}071$)	Treatment ($n = 1{,}071$)
Age (years)	6333	50.0	45.0	51.5	2142	46.7	46.8	46.7
Household income (per thousand dollars)	6338	55.2	50.3	56.6	2142	54.5	54.3	54.6
Male (percentage)	6337	39.0	38.0	39.3	2142	39.7	40.3	39.1
High school diploma (percentage)	6336	93.7	91.2	94.5	2142	93.4	92.5	94.3
Undergraduate degree (percentage)	6336	34.9	35.0	34.9	2142	36.1	37.3	35.0
Married (percentage)	6333	55.8	47.7	58.2	2142	48.7	48.7	48.7
Number of children	6338	0.68	0.69	0.67	2142	0.51	0.51	0.51
White (percentage)	6281	81.7	76.9	83.1	2142	86.2	86.2	86.2
Black (percentage)	6281	10.9	13.6	10.1	2142	9.06	9.05	9.06
Asian (percentage)	6281	6.02	7.87	5.46	2142	3.92	3.92	3.92
Hispanic (percentage)	6337	17.9	24.2	16.0	2142	18.3	18.3	18.3
Native American (percentage)	6281	5.68	6.28	5.50	2142	1.77	1.77	1.77
Pacific Islander (percentage)	6281	1.89	2.35	1.76	2142	0.19	0.19	0.19

Table 3. IW estimates for wearing a facemask

	Full sample			Matched sample		
	All households (1)	Below \$45,000 (2)	Above \$45,000 (3)	All households (4)	Below \$45,000 (5)	Above \$45,000 (6)
EIP	0.031* (0.013)	0.046** (0.017)	−0.001 (0.020)	0.045* (0.020)	0.064* (0.027)	0.013 (0.030)
Stay-at-home order	−0.016	−0.094	0.149	0.069	−0.053	0.214
	(0.089)	(0.115)	(0.136)	(0.236)	(0.189)	(0.291)
Compulsory mask ordinance	0.017	0.030	0.007	0.027	0.047	0.019
	(0.013)	(0.019)	(0.018)	(0.023)	(0.033)	(0.034)
Risk of infection	0.013	0.012	0.013	0.023	0.021	0.029
	(0.010)	(0.014)	(0.013)	(0.018)	(0.026)	(0.025)
Risk of death	0.004	0.004	0.000	0.014	0.004	0.030
	(0.011)	(0.015)	(0.016)	(0.022)	(0.032)	(0.029)
Diagnosed with COVID	0.004	−0.012	0.028	−0.003	−0.037	0.057
	(0.013)	(0.019)	(0.018)	(0.024)	(0.038)	(0.030)
Fixed-effects	Individual; Day; State-Wave					
N	86,216	43,216	43,000	26,779	13,730	13,048
R-squared	0.524	0.536	0.538	0.568	0.599	0.591

Notes: *p < 0.05;**p < 0.01; *** p < 0.001. Standard errors are clustered by individual.

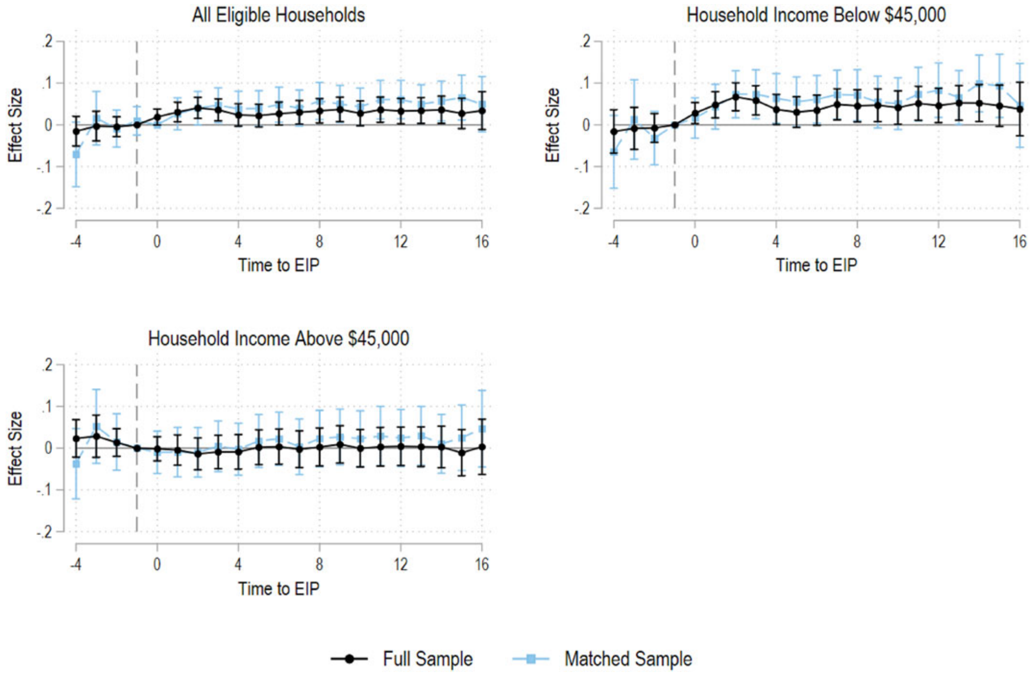

Figure 2. IW estimates for wearing a facemask with 95% confidence intervals.

Notes: The figure shows IW estimates and 95-percent confidence intervals for the effect of receiving EIPs on the probability of wearing a facemask.

behaviours: working from home, avoiding high risk individuals, avoiding public spaces and handwashing. Results are graphically displayed below (see Fig. 3 and Tables 2A–3A in Appendix A). Looking at the full sample, no evidence can be seen that EIPs significantly improved compliance with other protective health behaviours. Matching does not substantially change results, except for handwashing where positive results are sometimes obtained, although inconsistently across models. Altogether, this suggests that the mechanism at play is specific to facemasks and does not apply to protective behaviours more generally.

Risky behaviours

Do EIP recipients engage in more risky behaviours? If present bias is a concern, it seems reasonable to suggest that individuals might use transfers on risky behaviours involving non-essential activities leading them to be in close proximity with others. To better understand this issue, four risky behaviours are examined: attending a gathering with more than ten people; going to a public meeting space (such as a bar); close contact with non-household residents; and leaving the house for non-essential reasons. Results are shown in the bottom panel of Fig. 3 (see also Tables 4A–5A).

For going to a public meeting space and leaving the house for non-essential reasons, no significant effects can be observed. However, using the full sample, EIPs' reception is associated with a 3 percentage point increase in meeting in groups of ten and 5 percentage points more likely to meet non-household residents. However, these results become insignificant once matching is applied, suggesting these effects may simply be the product of imbalance.

Alternative estimation strategies

An important threat to identification in this analysis comes from the self-reported treatment status. To allay concerns that results might be biased by respondents systematically misreporting EIPs' reception, the paper re-estimates with an alternative control group (see Table 6A). The data are restricted to just the first six waves. Those who reported never receiving EIPs are excluded from the sample. The same basic specification is then re-run using the comparison between those who received EIPs sometime before wave 7 and the not-yet-treated units who received EIPs from wave 7 onwards. Under this alternative approach: the results for facemasks remain substantively unchanged; the results for meeting in groups of ten and meeting with non-household members becomes insignificant; a small positive effect can be observed for handwashing; and the results for all other outcomes remain insignificant. Taken together, these findings suggest that the initial results are not simply driven by misreporting, but represent EIPs' substantive impact.

Additional analysis of household liquidity

These findings point towards household liquidity as an important mechanism. Such a narrative explains both why EIPs improve compliance only when an explicit cost barrier exists (as with facemasks) and why their effects are stronger amongst poorer households. Although it is difficult to formally test whether UCTs' impact on

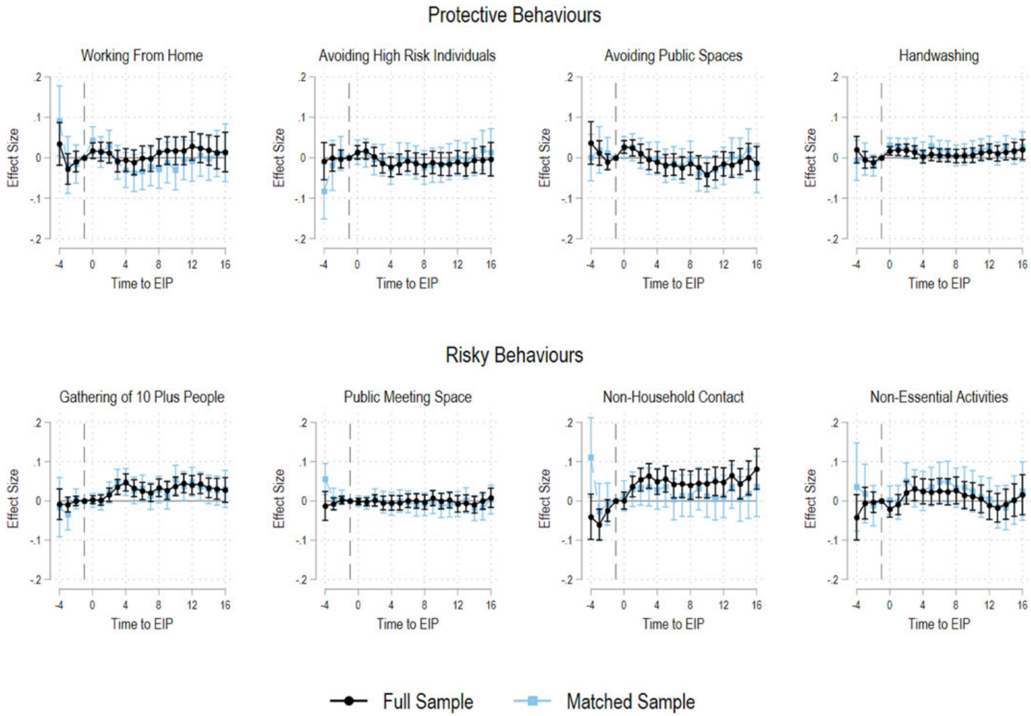

Figure 3. IW estimates for other behaviours, with 95% confidence intervals.

Notes: The figure shows IW estimates and 95-percent confidence intervals for the effect of receiving EIPs on the probability of engaging in protective behaviours (top-panel) and risky behaviours (bottom panel).

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household liquidity is the primary channel via which they change behaviours, it is possible to contextualise this paper's main results in light of how they impacted respondents' reported liquidity.

To that end, this paper makes use of a question, asked from wave 2 onwards, which assessed whether participants were able to pay an unexpected expense exclusively with savings. Participants were asked '*Suppose you have an emergency expense that costs $400. On the basis of your current financial situation, how would you pay this expense? If you would use more than one method, please select all that apply. With the money currently in my checking/savings account or with cash*'. Respondents could answer '*Yes*' (coded as 1) or '*No*' (coded as 0).

The specification is re-run using household liquidity as the outcome (see Table 7A for results). Amongst the entire sample, reception of EIPs is associated with a 5 percentage point increase in the likelihood that a respondent could pay an unexpected $400 expense exclusively with savings. When the sample is restricted to households earning below $45,000, this effect rises to 7 percentage points (Table 7A and Fig. 4).

This outcome provides an indication that household liquidity is a plausible mechanism to explain how UCTs affect pandemic behaviours. Comparing these findings with previous estimates for behaviours, it is possible to see that EIPs' impact on liquidity is inline or marginally higher than that for behaviours, a result which is consistent with the view that household liquidity is a relevant first step in ultimately impacting compliance with public health recommendations.

However, some difficulty remains in demonstrating a direct link between increased liquidity and facemask use. It is necessary to assume that higher liquidity led respondents to perceive facemasks as more financially accessible, which in turn increased usage. Although UCA lacks good pre-treatment data on respondents' motivations to use facemasks, there is post-treatment evidence thanks to a battery of questions introduced in wave 8 (after most transfers had been handed out) exploring respondent motivations around facemask use, including an explicit question about cost-barriers. Appendix B therefore provides further evidence on the mechanisms linking EIP reception and mask use. Employing an ex post matching design, the appendix shows strong differences in perceptions of facemasks' financial accessibility between those who had and had not received EIPs even as late as wave 19.

Discussion

This paper sought to fill an important gap in the literature, studying UCTs' impact on compliance with public health recommendations during the pandemic in a high-income country. To that end, two research questions were posed to frame the work. First, do UCTs promote the uptake of explicitly costly protective behaviours? Second, do UCTs promote compliance with implicitly costly public health recommendations during crises?

In answering these questions, this paper provides a robust analysis of UCTs' consequences for compliance with public health recommendations during the COVID crisis. Firstly, this paper rules out the most pessimistic view about UCTs. Concerns that UCTs wouldn't work at all (Banerjee & Mullainathan, 2010; Somville

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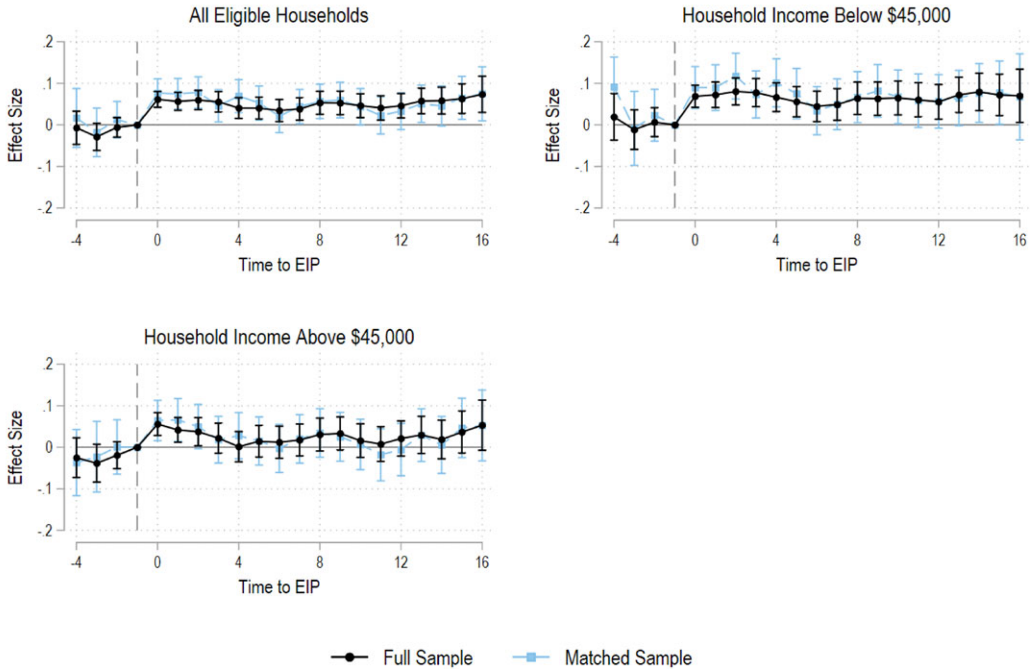

Figure 4. IW estimates for ability to pay $400 expense with savings.

Notes: The figure shows IW estimates and 95-percent confidence intervals for the effect of receiving EIPs on the probability a respondent can pay a $400 expense with savings.

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& Vandewalle, 2018) proved to be unfounded. A significant and positive effect can be observed for EIPs on the likelihood of wearing a facemask, particularly amongst poorer households.

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 welfare's reception lifted compliance with all public health recommendations. This suggests that the beneficial effects of UCTs are specific to facemasks and it seems likely that, much as with bed-nets, contraception and sanitary products in low-income countries, this effect is linked to the fact that households had to internalise the costs of this new behaviour (OECD, 2020).

Globally, these results are in line with much of the highest quality evidence emerging from low-income countries. Brooks et al. (2022) and Kimani et al. (2020), looking at 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 underscore that this paper does have limitations. First, this paper can only consider cash transfers in the context in which they were received. The US differs in important ways from other high-income countries, especially in Europe where government welfare plays a larger role in daily life (Moreira & Hick, 2021; Weisstanner, 2022). It is imaginable that in countries where the state is generally more generous to its citizens, a one-time UCT might be less effective. In that vein, it could also be argued that EIP design may have shaped their impact, for example, if transfers had been given in a series of instalments rather than as one lump-sum then results may well have been different. Whilst the paper cannot tackle these issues directly, they should form the basis for future avenues of research.

Second, the use of declarative data may bias these results especially if respondents misreport behaviours. Although there are risks associated with self-reported data, they still provide invaluable evidence in terms of understanding the micro-level consequences of and mechanisms behind UCTs during the pandemic, something which is harder to assess with coarse aggregated data used by papers such as Wright et al. (2020).

Despite these limitations, this article makes an important contribution to the literature on UCTs. Whilst most previous papers concerned with 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 compliance with pandemic-related measures amongst a representative sample in a high-income country.

This novel evidence should help inform future thinking about how UCTs can make societies more resilient to new risks and crises by shaping individual behaviours, especially in high-income countries. Much as with basic health practices in low-income contexts, UCTs can play an important role in promoting costly new health behaviours in high-income countries. However, UCTs are not a panacea and their scope for changing health behaviours, at least in the context of the CARES Act, seems largely limited to practices which come with some explicit cost attached.

Supplementary material. To view supplementary material for this article, please visit https://doi.org/10.1017/S0047279425101074.

Data availability statement. The analysis conducted in this paper relies on two sources of data. For the Understanding Coronavirus in America Study, the data underlying this article cannot be shared publicly due to privacy concerns. However, the data can be accessed upon request by making an application at the following link: https://uasdata.usc.edu/index.php. For the Oxford COVID-19 Government Response Tracker, the data underlying this article are available in GitHub, at https://github.com/OxCGRT/covid-policy-dataset. All code used in this analysis is available upon request.

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Competing interests. The author declares none.

Note

1 Whilst 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*, the plaintiffs presented evidence that facemasks' price increased by 500 per cent on Amazon (see https://digitalcommons.law.scu.edu/cgi/viewcontent.cgi?article = 3205&context = historical).

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