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# Uncovering Retail Trading in Bitcoin: The Impact of COVID-19 Stimulus Checks

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In April 2020, the US government sent economic impact payments (EIPs) directly to households, as part of its measures to address the COVID-19 pandemic. We characterize these stimulus checks as a wealth shock for households and examine their effect on retail trading in Bitcoin. We find a significant increase in Bitcoin buy trades for the modal EIP amount of $1,200. The rise in Bitcoin trading is highest among individuals without families and at exchanges catering to nonprofessional investors. We estimate that the EIP program has a significant but modest effect on the US dollar–Bitcoin trading pair, increasing trade volume by about 3.8 percent. Trades associated with the EIPs result in a slight rise in the price of Bitcoin of 7 basis points. Nonetheless, the increase in trading is small compared to the size of the stimulus check program, representing only 0.02 percent of all EIP dollars. We repeat our analysis for other countries with similar stimulus programs and find an increase in Bitcoin buy trades in these currencies. Our findings highlight how wealth shocks affect retail trading.

JEL classification: E42, G11, G41, H31.

Keywords: Bitcoin, COVID-19, economic impact payments, stimulus checks.

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# 1 Introduction

Retail traders — that is, individuals who trade on their personal accounts — have become important drivers of financial markets in recent years. In 2020 and 2021, cryptocurrencies such as Bitcoin and Dogecoin saw surges in retail buy interest, driven by the internet and social media, and exacerbated by the circumstances of the COVID-19 pandemic (Sklar (2021)). The causes and consequences of retail trading in these new markets are not well understood.

We shed light on retail trading in the Bitcoin market by studying a wealth shock. On April 9, 2020, the United States government began making direct stimulus payments to US citizens and residents, as part of its response to contain the economic fallout from the COVID-19 pandemic. These economic impact payments (EIPs), commonly dubbed "stimulus checks," were worth up to $1,200 per person. Given the relatively small size of individual EIPs, we characterize the program as a positive wealth shock for retail investors.

Our paper is inspired by contemporary anecdotal evidence that some of this money was spent on Bitcoin. On April 16, the CEO of Coinbase, a major US-based cryptocurrency exchange, tweeted a chart showing a surge in Bitcoin transactions for $1,200 (see Figure 1). Binance US, another cryptocurrency exchange, reported a similar phenomenon. Some Bitcoin enthusiasts also took to social media to proclaim that they used their entire EIP to buy Bitcoin.[1]

We show that the EIP program has a significant impact on retail trading in Bitcoin. Using a proprietary data set of individual Bitcoin buy trades across 26 exchanges, we compare the behavior of trades for amounts at or just below $1,200 (the *treated* group), with trades for amounts just above $1,200 (the *control* group). Following the disbursement of EIPs from April 9, 2020, we find an abnormally high number of trades in the treated group, relative to the control group, and relative to trades in currencies other than US dollars.[2] This effect is significant at the 1 percent level and lasts for a period of up to three weeks, during which time most of the EIPs are disbursed. Our results hold when we include currency, exchange, and time fixed effects, suggesting that they cannot be explained by the overall state of the Bitcoin market or US economy during the pandemic period.[3] Consistent with a demand-side shock, we observe no effect for sell trades.

[1] The Coinbase CEO's tweet has since been deleted. For a contemporaneous report, see Coindesk, "Some US citizens look to be splashing their stimulus cash on cryptocurrency," https://tinyurl.com/ycrvpb5e. Examples of social media activity can be found on Reddit (https://tinyurl.com/y47h4oqm) and Twitter (https://twitter.com/BitcoinStimulus).

[2] We exclude currencies whose governments ran schemes similar to the EIP program.

[3] There were two further rounds of EIPs in the US, in December 2020 and March 2021. Our paper examines the first round of EIPs only. See https://tinyurl.com/ybkdbc95.

Our empirical strategy is based on a regression discontinuity design. By comparing treated and control groups around a given cutoff, we minimize confounding effects from contemporaneous events. Specifically, we compare a *treated* group of Bitcoin buy trades for \$1,150–1,200 to a *control* group of buy trades for \$1,200–1,250, during the period when EIPs are disbursed, and find evidence of an increase in the size of the treated group. The bandwidth of \$50 corresponds to the highest fee an exchange may charge a customer for depositing and trading an amount of this size. Our results are robust to changes in the bandwidth size. We find that the effect of EIPs is strongest on Bitcoin exchanges with a higher volume of low-value trades, consistent with our characterization of the EIP program as a wealth shock to retail traders.

Although Americans with families received EIPs larger than the modal \$1,200 amount, we find no evidence of increased Bitcoin trading at those amounts. We infer that recipients with families are less likely to use their money to buy Bitcoin, suggesting that the effect is limited to younger, single people. This is consistent with the picture of a typical Bitcoin investor painted by surveys (e.g., Henry et al. (2019)). We exploit differences in timing between the disbursement of EIPs and Federal Pandemic Unemployment Compensation (FPUC), an unemployment insurance program, and find no evidence that our results can be explained by FPUC money being spent on Bitcoin.

We next investigate the possibility that Americans may have spent only part of their EIPs on Bitcoin, by adapting our regression discontinuity methodology for amounts less than \$1,200. We find an increase in buy trades for round number amounts, in particular for \$100, \$500, \$600, and \$1,000. This is in line with evidence that agents tend to focus on round numbers when making decisions under a high degree of uncertainty (e.g., Butler and Loomes (2007)). Consistent with our earlier findings, we do not observe an increase in Bitcoin buy trades for round amounts in currencies issued by countries that did not have an EIP-type program. This suggests that our findings are related to the nature of the US EIP program, and not to marketwide or international factors.

We estimate that, between April 9 and June 5, 2020, the EIP wealth shock is associated with a 3.8 percent increase in Bitcoin-USD trading by volume, and a 0.7 percent increase by value. This increase is small compared to the overall size of the EIP program: we estimate that only around 0.02 percent of issued EIP dollars are invested in Bitcoin. However, there is heterogeneity in how the money is used is heterogeneous, with a small number of people using most or all of their EIP to buy Bitcoin. We associate the increasing in Bitcoin trading with a permanent price rise of 0.07 percent. While statistically significant, this price rise is modest compared to the 4.6 percent standard deviation in the daily price of Bitcoin. Our methodology may underestimate the true effect, since we cannot rule out individuals investing amounts other than \$1,200, or round numbers, in Bitcoin.

We show that our results are not specific to the United States. Variation in countries' fiscal responses to the COVID-19 pandemic gives rise to a quasi-natural experiment. During our sample period between January 1 and June 5, 2020, the governments of Japan, Singapore, and South Korea all made direct one-off stimulus payments to most households within their respective jurisdictions, similar to the US EIP program. We apply our regression discontinuity methodology to test whether these stimulus payments affected Bitcoin trading in the relevant domestic currency. For the Japanese yen and South Korean won, our findings are similar to those for US dollars: we find a significant increase in Bitcoin buy trades for amounts at or just below the modal stimulus check size, once payouts begin. We see no such increase in countries that did not initiate similar COVID-related stimulus programs. We do not find an increase in trading for Singapore dollars, perhaps because of thin markets and regulatory changes confounding our results. Our results for the US, Japan, and South Korea suggest that increases in Bitcoin buy trades are indeed driven by the direct stimulus payment programs in these countries and cannot be wholly explained by other factors.

Our findings shed light on the nature of retail investors and have broader implications for other types of financial markets. Retail trading in the stock market received widespread attention in early 2021, when social media drove a surge in the trading of certain stocks (Eaton et al. (2021)). Unlike retail traders in stock markets, who tend to be intermediated by brokers, cryptocurrency traders can easily place orders directly to an exchange. Our methodology could be replicated for the stock market using customer-level deposit data from individual brokers. Nonetheless, we expect EIPs may have a stronger effect on trading in the Bitcoin market than the stock market, because the characteristics of people who receive the \$1,200 EIPs — young with moderate incomes — are associated with a stronger preference for lottery-type investments (Kumar (2009)).

Our results have policy implications. Direct payments to households have been used as a fiscal policy instrument in many countries as part of broader governmental efforts to contain the economic fallout from the COVID-19 pandemic. Early evidence suggests that these payments have benefited the poorest in society by boosting consumption (see, for example, Cooney and Shaefer (2021)), consistent with our finding that only a small number of people invested substantive amounts in Bitcoin. There are three circumstances peculiar to the spring of 2020 that may explain why Bitcoin might have been preferred as an alternative investment vehicle. First, financial markets experienced significant turmoil in March 2020, which may have increased Bitcoin's appeal as an alternative asset class and potential safe haven against tail risks. Indeed, Bitcoin's price has risen since mid-2020, reaching a peak of \$64,863 on April 14, 2021 (see coinmarketcap.com). Second, the economic effects of the pandemic may have been less severe on those with the highest propensity to buy Bitcoin: that is, individuals who are young, single, well-educated, and computer-literate. Third, May 2020 saw the quadrennial Bitcoin "halvening" event, in which the nominal reward to Bitcoin miners is halved. While interest in Bitcoin, and its price, have previously tended to rise in anticipation of such events, we do not find a strong association between our findings and the halvening event.

Our study complements the literature on how households respond to unexpected increases in wealth. Several recent papers use survey data to examine how households have spent their EIPs. They find a consistent tendency among EIP recipients to save or pay down existing debt, rather than consume (see Armantier, Goldman, Koşar, Lu, et al. (2020), Baker et al. (2020), Coibion, Gorodnichenko, and Weber (2020), and Perez-Lopez and Bee (2020)). These studies, however, do not focus on whether the money is spent on investment assets. In contrast, we focus on the investment of EIPs in a specific alternative asset class and use trade-level data, rather than survey evidence, in our analysis.

The paper is organized as follows. Section 2 provides a background on the Bitcoin market in the first half of 2020 and the US Economic Impact Payment program. Section 3 describes our data and methodology, and Section 4 contains our main results. Section 5 explores round number preference in Bitcoin buy trading, and Section 6 computes the overall magnitude of the effect of the EIPs on the Bitcoin market. In Section 7 we examine Bitcoin trading related to other countries with relief programs similar to the US EIPs. Section 8 explores placebo tests, and Section 9 concludes. Charts and tables follow, with supplementary results contained in an Online Appendix.

# 2 Background

We provide a non-technical background to the Bitcoin market and the US Economic Impact Payment program. Section 2.1 describes the state of the Bitcoin market in the first half of 2020, focusing on retail investors. Section 2.2 explains the EIP program, and Section 2.3 reviews the literature on how the funds have been spent. Finally, Section 2.4 discusses the demographic characteristics of people most likely to invest in Bitcoin and suggests that such people may have been less likely to suffer economic hardship during this period, and thus are more likely to treat their EIPs as disposable income.

## 2.1 The Bitcoin market in the first half of 2020

Figure 2 plots the price of Bitcoin between January 1 and June 30, 2020. The price began to decline in late February 2020 as the magnitude of the COVID-19 crisis became clearer. The steepest sell-off came on March 12, 2020, dubbed "Black Thursday," when the World Health Organization formally declared COVID-19 a pandemic and global stock markets fell the same day. The Bitcoin price began to rise again in late March, when governments announced fiscal and monetary measures to combat the pandemic. A sharp rise in price and trading volumes followed in early May. During the latter half of 2020 and beyond — which we do not cover in this study — the price of Bitcoin climbed steeply, exceeding $64,000 in April 2021.

It is not clear to what extent the changes in price and trading volume were due to the pandemic. While Bitcoin advocates promote it as a hedge against macro-uncertainty, empirical evidence for this is, at best, mixed (Grobys (2020)). In fact, Coibion, Georgarakos, et al. (2021) find that respondents to a hypothetical exercise reduce the amount they allocate to cryptocurrencies when faced with greater uncertainty.

The brief surge in price and trading in early May was possibly catalyzed by a technical event in Bitcoin, known popularly as the "halvening". This occurred most recently on May 11, 2020, when the reward to miners for creating a new Bitcoin block was halved from 12.5 to 6.25 bitcoins. Such halvening events are hard-coded and predictable, occurring once every 210,000 blocks.[4] Nonetheless, the Bitcoin price has historically increased in anticipation of these halvening events. This may be due to self-fulfilling expectations of a price increase or because media coverage of these events generates more investor attention. Figure 2 suggests that a similar increase in the price of Bitcoin occurred in the days leading up to the halvening event. In these data, the largest one-day increase in the Bitcoin price after March 2020 is on April 30, and the third largest is on May 8, the last business day before the halvening. Figure A.1 measures investor attention in Bitcoin by plotting Google searches for the term "Bitcoin" in the United States. Investor interest peaked around the times of Black Thursday and the halvening. However, we cannot say for sure whether the price increases were due to greater investor attention, or vice versa.

## 2.2 The Economic Impact Payment program

The Coronavirus Aid, Relief, and Economic Security (CARES) Act was signed into US law on March 27, 2020. This act contains a raft of measures, including economic impact

[4] On the Bitcoin blockchain, a new block is mined approximately every 10 minutes, so a halvening occurs once every four years. The exact time of the halvening becomes more certain as it draws closer.

payments. Every US citizen (resident or not) and resident alien earning up to $75,000 per annum was eligible for a one-time EIP of $1,200, plus $500 for each qualifying child. For every dollar of income above the $75,000 threshold, she received 5 cents less, reaching zero if her annual income exceeded $99,000. Table 1 provides more details on the EIPs and the eligibility criteria for receiving them. For context, the median US annual personal income in 2019 was $35,977, so most US residents were eligible for the full EIP amount.[5]

The first EIPs were disbursed on April 9, 2020. The CARES Act stipulates that the payments should be made "as rapidly as possible," but does not specify a timeline. Taxpayers and certain welfare recipients were sent money automatically by the Internal Revenue Service (IRS). Other eligible individuals had to register on the IRS website (Marr et al. (2020) estimate that around 12 million individuals needed to do this). Individuals received a direct deposit to their bank account where possible. Otherwise they were mailed a check or prepaid debit card, which likely meant that the funds took longer to clear.

As Figure 3 shows, most payments were disbursed in the first few weeks. Nearly 7 in 10 adults are estimated to have received their EIP by the end of May (Holtzblatt and Karpman (2020)). By June 3, the IRS reported that it had made EIPs to 159 million Americans, totaling almost $267 billion, with an estimated 30–35 million payments remaining to be made (House Committee on Ways and Means (2020)). The deadline to request an EIP was November 21, 2020. In total, EIPs comprise approximately one-tenth of the total $2 trillion economic relief package authorized by the CARES Act.[6]

The US government has since disbursed two further rounds of stimulus payments: for $600 in December 2020 and $1,400 in March 2021. Again, payouts were lower for recipients with high incomes, and higher for those with dependents. The procedure was similar to that for the first round, though payments were typically made faster.[7] These subsequent rounds do not form part of our analysis in this paper because our sample ends on June 5, 2020, long before any decisions were made about further rounds of stimulus payments.

## 2.3 How the economic impact payments have been spent

Several recent papers use survey data to identify how recipients of the first round of EIPs spend the money. They generally find that much of the EIP money is saved or used to pay down existing debt, rather than financing consumption. See, for example, Armantier,

[5] EIPs were not offset by any debts due, except child support. For the text of the act, see https://www.congress.gov/116/bills/s3548/BILLS-116s3548is.pdf. For more information about IRS disbursements, see https://www.irs.gov/coronavirus/economic-impact-payment-information-center.

[6] Murphy (2021) provides a more detailed analysis of the time frame over which EIPs were paid out, and the payments methods used.

[7] See https://www.wsj.com/articles/why-cant-stimulus-payments-arrive-faster-11617631209.

Goldman, Koşar, Lu, et al. (2020), Baker et al. (2020), and Coibion, Gorodnichenko, and Weber (2020). Boutros (2020), Garner, Safir, and Schild (2020), and Perez-Lopez and Bee (2020) study responses to the US Census Bureau's Household Pulse Survey and find a strong tendency among higher-income households to save or pay down debt. Akana (2020) finds that 41 percent of survey respondents report saving at least part of their EIP. These papers tend not to disentangle investment from saving and, in any case, do not specifically ask about purchases of cryptocurrency.[8]

Other papers explore spending data, rather than surveys. Cox et al. (2020) use US households' bank account data to examine the overall impact of the pandemic. While they do not have specific data on EIPs, they model the impact of the fiscal stimulus programs and find that the EIPs likely led to increased saving by low-income households. Karger and Rajan (2021) and Misra, Singh, and Zhang (2020) explore debit card data and find differences in recipients' marginal propensities to consume EIPs, depending on geography and income. Chetty et al. (2020) use granular data on economic activity to explore various aspects of the crisis, including EIPs.

Falcettoni and Nygaard (2021) present an overview of studies on the CARES Act. There is a more general literature on how households spend unanticipated windfalls, which is too large and broad to discuss here. For more details, see the literature reviews in Baker et al. (2020), Coibion, Gorodnichenko, and Weber (2020), and Misra, Singh, and Zhang (2020).

Although the papers mentioned so far focus mainly on consumption, debt repayment, and saving, there is also evidence that EIPs are used to finance investment. Envestnet Yodlee, an account aggregation firm, finds evidence of increased securities trading from bank accounts that receive EIPs.[9] The co-CEO of Robin Hood, an investment app, reported seeing deposits of amounts equal to or multiples of the stimulus amount.[10] However, none of these papers or articles make any substantive mention of EIPs being used to buy Bitcoin or other cryptocurrencies.

[8] Armantier, Goldman, Koşar, and Klaauw (2021) examine survey evidence on the second round of EIPs and find a slightly greater proclivity to save or pay down debt. Crossley et al. (2020) ask UK adults in July 2020 how they might use a hypothetical stimulus payment. They estimate a marginal propensity to consume of 11 percent, which is lower than expected.

[9] See CNBC, "Many Americans used part of their coronavirus stimulus check to trade stocks," https://tinyurl.com/y99h664d.

[10] See CNBC TV, "Robin Hood co-CEO on users depositing stimulus checks into the app and market accessibility," https://www.youtube.com/watch?v=l1mBwJAR3Ag.

## 2.4 Demographic characteristics of Bitcoin investors

People interested in Bitcoin are more likely to be male, white, single, computer literate, and earn a higher income, compared to those not interested in Bitcoin.[11] In contrast, the economic impact of the COVID-19 pandemic tended to be worse for women, ethnic minorities, people with children, and those with low incomes (see, for example, Bauer et al. (2020) and Falk et al. (2021)). People able to work from home suffered less than those who could not. Together, these facts suggest the possibility of a negative correlation between interest in Bitcoin and economic hardship during the first half of 2020. People interested in Bitcoin are less likely to need to use the EIPs to replace lost income, and so are more inclined to invest it. This suggests that the effect of EIPs on the Bitcoin market could be larger than on, say, the stock market.

Relatedly, there is a strand of the household finance literature that examines the propensity of investors to buy lottery-type stocks. For example, Kumar (2009) shows that demand for lottery-type stocks increases during bad economic times, and young, single men are more likely to invest in such stocks. To the extent that Bitcoin returns can be thought of as lottery-like, this literature reinforces our argument that EIPs may be more likely than other forms of household income to be spent on Bitcoin. However, other demographic characteristics associated with increased demand for such stocks — such as having a low income or belonging to an ethnic minority — do not tend to be correlated with interest in Bitcoin, so we must be cautious about this interpretation.

# 3 Data and methodology

## 3.1 Overview of data

We use proprietary data on Bitcoin trades from Kaiko, a commercial provider of cryptocurrency market data. Kaiko collects tick-by-tick trade data from various cryptocurrency exchanges. Each trade observation includes a timestamp (in milliseconds), the quoted currency pair (for example, BTCUSD for trades of Bitcoin against US dollars), trade size, the price and exchange at which the trade occurred, and whether the trade is a buy or sell order. As the EIP program represents a positive wealth shock in dollar terms, we focus only on buy orders; that is, trades in which US dollars are exchanged for Bitcoin.[12]

[11] See, for example, Auer and Tercero-Lucas (2021), English, Tomova, and Levene (2020), and Henry et al. (2019), and surveys by eToro (https://tinyurl.com/y3topno8), SurveyMonkey (https://tinyurl.com/y62luusj), and Statista (https://simplemoneylyfe.com/cryptocurrency-statistics).

[12] In Section 8, we run a placebo test using sell orders and find no association with EIPs.

We restrict our sample to the period between January 1 and June 5, 2020. Since most EIPs were paid out by the end of this period (Figure 3), an extended sample period would add little value to our findings besides increasing the potential for confounding events. Our sample covers the 26 largest and most liquid exchanges that offer trading services in Bitcoin. These are listed in Table 2.

Table 3 lists the top 15 fiat currencies in our data set. These comprise over 99 percent of all trades by both value and volume.[13] Those listed as "program" currencies (USD, JPY, KRW, and SGD) are issued by jurisdictions that, during our sample period, introduced programs similar to the EIPs. These programs are described in more detail in Section 7.[14]

Figure 4 presents the distribution of BTCUSD trade sizes. Each panel shows a histogram of the daily frequency of buy trade sizes over three periods: before the CARES Act is passed (January 1 – March 26); after passage of the act but before the first payments (March 27 – April 8); and after the first disbursements (April 9 – June 5). Within each period, the distributions appear to be continuous at $1,200. This suggests that, prior to the EIPs, there is nothing special about this amount. Prima facie, there does not appear to be evidence of an increase in trading at this level after EIP disbursement. Within each period, there are clear peaks at $500 and $1,000, suggesting a preference for round number trade sizes among Bitcoin buyers, and this prominence appears to be somewhat higher during the disbursement period.

## 3.2 Methodology

We identify whether the EIPs have an impact on the Bitcoin market by comparing the daily volume of buy trades for amounts around $1,200 to those for slightly higher amounts. We control for changes in volumes of other currencies where we expect to see no EIP effect. In this section, we explain why we use $1,200 as a point of comparison, and then expand on our econometric approach.

### 3.2.1 Focus on $1,200

The IRS provides only aggregate information on EIPs, so we do not have detailed data on the size of each payment. Similarly, the Bitcoin data from Kaiko do not provide any information about the counterparties to the trade. Therefore, we cannot identify with

[13]The Chinese yuan was a major Bitcoin trading currency until 2017, but tighter regulations and enforcement have led it to fall out of use.

[14]Hong Kong and Israel introduced similar programs in July and August 2020, respectively, while the US disbursed further rounds of EIPs in December 2020 and March 2021. These programs fall outside of our sample period, so HKD and ILS are not considered program currencies.

certainty which trades are financed by EIPs. Instead, we assume that Bitcoin trades close to the modal EIP amount, occurring in the period following the disbursement of EIPs, are most likely to be financed by these payments.

We determine that the modal EIP is for $1,200, by the following argument. The CARES Act specifies that a single tax filer receives $1,200. However, a couple filing jointly, or a filer with children, may receive more (Table 1). According to the 2019 American Community Survey, the most common household type is that of a single person with no qualifying children (see Table A.1), suggesting that the modal EIP payment is no greater than $1,200.[15] A household with income above $75,000 per person may receive a smaller EIP but, as median US personal income is less than half of this, we conclude that $1,200 is the most likely modal payment.[16]

Focusing on the modal amount of $1,200 has the advantage that it is a relatively unusual payment size prior to the EIP program (Figure 4). This suggests that any abnormal changes in Bitcoin trades near the modal amount following EIP disbursement are most likely to be attributable to the program. It seems unlikely that other factors — such as, for example, an increase in retail trading during lockdowns — would cause an increase in buy trades for this particular amount of money. Our approach likely underestimates the true impact of the EIPs, since recipients may choose to use only part of their check to buy Bitcoin, and there may be some who receive larger EIPs and decide to invest it in the Bitcoin market.

Of course, even if an individual receives an EIP for $1,200 and decides to buy Bitcoin, we cannot be sure whether she invests the full amount. Indeed, a rational unconstrained economic agent may find it optimal to invest less. We argue that the presence of certain frictions — in particular, credit constraints or behavioral factors — can explain why some individuals choose to invest all of their EIP, rather than merely a part of it. We elaborate on these below.

First, it is typically harder to borrow money to buy Bitcoin than it is to borrow to purchase consumption goods. For example, at the time of writing, Kraken, a major US cryptocurrency exchange, offers customers a maximum of up to five times leverage on cash

[15] The evidence in Section 2.1 suggests that the probability of having no dependents may be even higher conditional on investing the money in Bitcoin.

[16] Caveats apply with our interpretation of the American Community Survey (ACS) data. First, the universe of ACS respondents is not identical to the population of EIP recipients. For example, expatriate US citizens are eligible for EIPs but are not generally included in the ACS. Second, the definition of a "child" in the ACS is not the same as that used by the IRS, because the ACS records only whether a child under 18 years old belonging to the householder was present. Third, an adult living in a domicile who is neither a householder nor the householder's partner (say, the householder's parent) would not be picked up by the ACS, although she would receive her own EIP. Since such an adult is likely to be a single tax filer, that should only strengthen our inference about $1,200 being the modal amount.

deposits.[17] Typically, credit on an exchange requires collateral. This credit constraint means that it may be optimal to use the entire EIP to buy Bitcoin. Suppose, for example, an investor receives an EIP and decides to increase her holdings of both Bitcoin and consumption goods. She might rationally use the EIP cash for Bitcoin trading and buy consumption goods with a credit card.[18]

Second, there are behavioral reasons why an investor may choose to invest all of her EIP. Bitcoin enthusiasts tend to have strong anti-government beliefs, and often participate in social media groups extolling the merits of Bitcoin relative to government-issued money (see Shiller (2019, Chapter 1)). In their opinion, using central bank money to buy Bitcoin is a subversive, and thus desirable, act.[19] Putting the whole amount into Bitcoin sends a strong signal of defiance against the government to their perceived peer group. This behavioral motivation is closely related to the idea of conspicuous consumption, in which certain goods bestow status upon the purchaser (Bagwell and Bernheim (1996)). Other motivations to signal investment in Bitcoin may include self-satisfaction (Mandel (2009)), a bandwagon effect (Leibenstein (1950)), and insecurity about identity (Braun and Wicklund (1989)). Kuchler and Stroebel (2020) review the empirical literature on how social interactions influence retail investors' choices.

Both these frictions are more likely to hold for retail investors than for institutional investors, who generally have access to larger pools of capital and a more rational investment mandate.

### 3.2.2 Econometric approach

Our research design is based on a regression discontinuity (RD) approach. We posit a *treated* group comprising trades relatively likely to be financed by EIP money. There is also a *control* group of trades influenced by the same factors as the treated group, except by the EIPs. We then compare the behavior of these two groups after the EIP program begins. We expect to find that the number of trades in the treated group increases, relative to those in the control group. This approach relies on two key assumptions. First, in the absence of EIPs, the treated and control groups behave similarly. Second, EIPs may affect the treated group, but do not affect the control group.

The treated and control groups are formally defined using a *cutoff* — the boundary between the two groups — along with a *bandwidth*, the size of each group. We use a cutoff of $1,200. Then, for a given bandwidth $h > 0$, the treated group comprises all trades of

[17]As of May 2021. See https://tinyurl.com/unm5bk7u.
[18]For a similar argument, see Telyukova (2013).
[19]See, for example, https://twitter.com/BitcoinStimulus and https://tinyurl.com/y47h4oqm.

size between \$1,200$-h$ and \$1,200, and the control group all trades greater than \$1,200 but no more than \$1,200 $+h$. A larger bandwidth is likely to increase the number of EIP-financed trades in the treated group, because some individuals may choose to use only part of their EIP to buy Bitcoin. However, a larger bandwidth also increases the risk of other contemporaneous factors, unrelated to EIPs, affecting our results.

We determine the bandwidth by considering the fees that cryptocurrency exchanges charge customers to deposit dollars and buy Bitcoin. While an exchange may record a deposit of exactly, say, \$1,200 (Table 1), the actual amount traded, net of fees, may be smaller, and that is what is recorded in the Kaiko data. At the time of writing, Coinbase — the main exchange used by US retail investors — charges a fee of up to around \$50 on a \$1,200 buy trade.[20] Consequently, we determine that \$50 is an appropriate choice for the bandwidth. Nonetheless, we test the sensitivity of our results to the choice of bandwidth.

Our approach is very similar, but not identical, to a standard RD design, because the outcome variable we wish to measure is the frequency of observations, rather than a score variable.[21]

### 3.3 Predictions

Our main prediction is that, during the period when EIPs are disbursed, there is an increase in BTCUSD buy trades for the treated amounts, relative to the period before the CARES Act is announced. This increase is significant relative to the number of BTCUSD trades for control amounts, and relative to Bitcoin buy trades against non-program currencies, neither of which we expect to be affected by the EIP program. We do not make comparisons with trades in other program currencies (JPY, KRW, SGD), as contemporaneous EIP-type programs in those countries may make the results difficult to interpret.

We predict that any effect on BTCUSD trades is weaker, if present at all, during the period between the announcement of the EIP program and the first disbursements. This is because the CARES Act does not specify exactly when the EIPs are to be disbursed.[22] In addition, any announcement effect may be blunted by personal budget constraints, or by investor-specific factors like limited attention or lack of faith in the government's commitment to pay the money.

[20]Coinbase charges a spread of around 0.50 percent on trades, plus a fee. For example, an individual depositing \$1,200 and using it to buy Bitcoin would pay a spread of around \$6, plus a fee of up to \$48, depending on the payment method used. For example, a fee of 3.99 percent is levied on transactions made by debit card or PayPal. These figures are correct at the time of writing; see https://tinyurl.com/rjfp7pc9.

[21]Goncharov, Ioannidou, and Schmalz (2020) also carry out a discontinuity design where the variable of interest is frequency, although their topic of study is very different from ours.

[22]Fuster, Kaplan, and Zafar (2020) find that people are less responsive to news about future gains than they are to unanticipated realized gains.

We also expect EIP disbursement has a smaller effect, if any, on BTCUSD trades for cutoffs consistent with the larger amounts received by couples and families. Survey evidence suggests that single people are most likely to invest in Bitcoin (Section 2.1). Therefore, we do not expect to see an increase in Bitcoin purchases for $2,400 (the EIP amount received by couples, before adjusting for income), nor $2,900, $3,400, $3,900, etc. (the amounts received by couples with children).

We expect the effect of EIP disbursement on Bitcoin trading to be stronger on exchanges with fewer professional traders. Professional users' trades are likely to be larger than those of retail traders, so EIPs are less likely to have an effect. In fact, many professional traders will have incomes high enough to disqualify them from receiving an EIP at all.

Finally, we make a prediction about the price of Bitcoin. If, as hypothesized, the EIPs are associated with an increase in buy trades, we would expect the price at which trades are executed to increase. However, our RD methodology is not so well-suited to detect differences in price between the treated and control groups. Most Bitcoin exchanges use limit order books to match trades. Suppose an individual uses her EIP to place a buy market order for $1,190, and that the best execution price is with a limit order to sell up to $1,210 of Bitcoin. The orders would be matched, the limit order book depleted, and any subsequent market orders for amounts up to $1,210 would occur at a higher price. In other words, an increase in trades for treated amounts can result in higher prices for control trades, depending on the depth of the limit order book. Unfortunately, we do not have access to historical limit order book data, so our predictions are restricted to actual executed trades. We predict that the EIPs may cause treated market orders to execute at a higher price than control market orders, but any effect is likely to be small.

To summarize, we make the following five predictions:

- P.1 During the period in which the US government disburses economic impact payments, there is an increase in Bitcoin buy trades in US dollars for the treated amounts, relative to control amounts, and relative to buy trades in non-program currencies.
- P.2 The effect is weaker or non-existent during the period between announcement of the EIPs and first disbursements.
- P.3 The effect is weaker or non-existent for cutoffs equal to EIP amounts paid to recipients with families.
- P.4 The effect is stronger on exchanges that are used more by non-professional traders.
- P.5 During the EIP disbursement period, buy trades for the treated amounts execute at a higher price than control amounts, and relative to buy trades in non-program currencies.

# 4 Results for \$1,200 payments

In this section, we run a series of empirical tests to identify the effect of the economic impact payments on Bitcoin buy trades. First, in Section 4.1, we carry out an event study around the start of EIP disbursement in April 2020. Next, in Section 4.2, we show that disbursement is associated with a significant increase in Bitcoin trades around \$1,200, the EIP amount paid out to single tax filers. In Section 4.3, we show that there is no increase in trades of sizes corresponding to the larger EIP amounts paid to families. Finally, in Section 4.4, we examine how the effect of EIPs on Bitcoin trades differs across the exchanges in our sample.

## 4.1 Timing of EIP impact on Bitcoin trading

The first EIPs are disbursed on April 9, 2020. We use an event study framework to determine the effects of EIPs on Bitcoin buy trades in US dollars after this date. Our specification is:

$$Y_{jst} = \alpha + treated_s \times \lambda_t + \mu_j + \nu_t + \epsilon_{jst}, \tag{1}$$

where $Y_{jst}$ is the number of Bitcoin buy trades in group $s$ on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades on that exchange and day. Trades can either belong to the *treated* group (i.e., those with size in the range \$1,150–1,200) or the *control* group (\$1,200–1,250), as explained in Section 3. The dummy variable $treated_s$ is equal to one if $s$ is the treated group, and zero otherwise. For our event study analysis, we only include trades in USD at US-domiciled exchanges, ignoring other currencies and exchanges, in order to more cleanly identify the day-by-day effect of EIPs on trading.

The coefficients of interest are the $\lambda_t$ terms, which tell us whether and when the EIPs have a significant impact on the number of treated Bitcoin trades, relative to the control group. We define $t = 0$ to be the day of disbursement, i.e., April 9, 2020. We estimate coefficients relative to the day before disbursement, so we fix $\lambda_{-1} = 0$. Then Prediction P.1 is true if the $\lambda_t$ terms are significantly greater than zero for $t \geq 0$ (i.e., once EIP disbursement begins), while Prediction P.2 is true if the $\lambda_t$ terms are not different from zero for $t < 0$ (i.e., before EIP disbursement). Our event window starts 24 days before EIP disbursement begins, and ends 24 days afterward, so we fix $\lambda_t = 0$ for $t < -24$ and $t > 24$. The other terms in Equation (1) are exchange and day fixed effects, and a constant term. The error term $\epsilon_{jst}$ is normally distributed and assumed to be uncorrelated with the main regressors.

Figure 5 plots the estimated coefficients $\lambda_t$ from $t = -24$ to $t = 24$. The vertical bars represent 90 percent confidence intervals. Prior to the passage of the CARES Act (i.e., for

$t < -13$), the coefficients are not significantly different from zero. This suggests that the treated and control groups behaved similarly prior to the EIP program. Therefore, there is evidence for parallel trends between the two groups, as assumed in Section 3.2.2.

Once EIP disbursement begins ($t = 0$), there is a significant increase in buy trades in the treated group, relative to the control group. The effect begins immediately and grows steadily until the end of April, by which time most of the EIPs have been paid out (see Figure 3). But, in the phase between the passage of the CARES Act and EIP disbursement ($t = -13$ to $-2$), there is no significant difference between trade volumes in the treated and control groups. Therefore, these results support Predictions P.1 and P.2: EIP disbursement is associated with an increase in Bitcoin buy trades, but there is no announcement effect.

We are interested in whether the response of EIP recipients to the wealth shock is driven by the "halvening" event on May 11 (see Section 2.1). Figure 2 suggests an increase in the Bitcoin price and trading volume ahead of the halvening, especially from the start of May. But Figure 5 suggests that the effect of EIPs on Bitcoin trading fades from the beginning of May (day 22). We conclude there is no evidence the halvening event magnifies the effect of the EIPs on Bitcoin trading.[23]

## 4.2 Magnitude of effect of EIPs

We employ a difference-in-difference specification to measure the size and significance of the effect of EIPs on Bitcoin trading. We split our time series into three phases: before the CARES Act (January 1 to March 26), before EIP disbursement begins (March 27 to April 8), and during disbursement (April 9 to June 5). We then test for differences in the behavior of the treated and control groups, accounting for differences in phase, the currency being exchanged for Bitcoin, and the exchange on which the trade takes place.

We include buy trades in non-program currencies, that is, currencies issued by governments that do not run EIP-type programs (Table 3).[24] Since EIPs are paid only in USD, they only directly affect buy trades in USD and should not impact trades in non-program currencies. However, factors other than the EIP program — for example, uncertainty caused by the COVID-19 crisis — are likely to affect Bitcoin investors across all currencies. Including trades in non-program currencies allows us to control for these other factors. We convert all trades to equivalent dollar amounts using the prevailing exchange rate, and include only those trades with sizes corresponding to the treated and control groups. We exclude program

[23]To test this, we extend the event study to $t = 40$ and that find the coefficient is actually significantly negative in the days immediately preceding the halvening. These results are available on request.

[24]In Section 7, we extend our analysis beyond the US and examine whether Bitcoin trading in the other program currencies responds to those programs.

currencies (JPY, KRW, SGD), along with all trades on exchanges domiciled in Japan, South Korea, and Singapore, to prevent EIP-like programs in those jurisdictions from confounding our results.

Our regression specification is given by Equation (2):

$$\begin{aligned} L_{ijst} \;&=\; \alpha \;+\; \boldsymbol{\beta} \cdot \boldsymbol{phases}_t \times USD_i \;+\; \gamma \times treated_s \\ &\quad+\; \boldsymbol{\delta} \cdot \boldsymbol{phases}_t \times USD_i \times treated_s \;+\; \omega_i + \mu_j + \nu_t + \epsilon_{ijst}. \end{aligned} \tag{2}$$

As before, $j$ denotes the exchange, $s$ indicates whether a trade belongs to the treated or control groups, and $t$ indexes the day. The index $i$ denotes the quoted currency against which a Bitcoin buy trade occurs. Our dependent variable $L_{ijst}$ is the log-odds of the proportion of buy trades in group $s$, relative to total buy trades in currency $i$ traded at exchange $j$ on day $t$.[25]

Our regression specification features several dummy variables. Like the previous model, $treated_s$ equals one if $s$ is the treated group, and zero if it is the control group. The dummy variable $USD_i$ equals 1 if the currency is USD, and 0 if it is some other currency. The term $\boldsymbol{phases}_t \in \{0,1\} \times \{0,1\}$ is a vector of length 2. The first element of $\boldsymbol{phases}_t$ is equal to 1 iff the CARES Act has been announced by day $t$ and EIP disbursement has not yet started (i.e., the phase March 27 to April 8). The second element is equal to 1 iff EIPs are paid out on day $t$ (i.e., April 9 or later). The regression coefficients $\alpha$ and $\gamma$ are scalars, while $\boldsymbol{\beta}$ and $\boldsymbol{\delta}$ are vectors of size 2, to be estimated.

The terms $\omega_i, \mu_j, \nu_t$ are fixed effects terms, while the error term $\epsilon_{ijst}$ is normally distributed with mean zero.[26] We run four different regression specifications, variously employing fixed effects for the date $t$, the traded currency $i$, and the exchange $j$. The fixed effects allow us to rule out the possibility that our findings are driven by factors such as market developments, USD-specific events other than the EIP program, or issues specific to an exchange. In each specification, we cluster standard errors by date. The variables are stationary. Prediction P.1 says the coefficient $\delta_2$ is significantly positive, while Prediction P.2 says $\delta_1$ is not significantly different from zero.

Table 4 presents our model estimates. The two components of $\boldsymbol{phases}$ are labeled *announced* and *disbursed*, respectively. In all four specifications, we find that the coefficient of the $disbursed \times treated$ interaction term is positive and significant at the 1% level. This confirms Prediction P.1: during the EIP disbursement phase, there are more BTCUSD

[25] That is, $L = \log \frac{Y}{1-Y}$, where $Y$ is the proportion of treated buy trades, as defined in Section 4.1.

[26] The fixed effects terms mean we do not need to include standalone terms for $USD_i$ or $\boldsymbol{phases}_t$. We find that inclusion of a $\boldsymbol{phases}_t \times treated_s$ interaction term does not affect the results and is not significant (results available on request). Thus, for simplicity, we do not include it in the baseline regression.

buy trades for treated amounts, relative to control amounts, and relative to other Bitcoin-currency pairs. No other dummies are significant once we introduce all three sets of fixed effects.[27]

Once again, we find no evidence of an announcement effect. EIP recipients do not buy more Bitcoin when the CARES Act passes, but only once the money is actually disbursed. The coefficient of $announced \times treated$ is slightly negative, but not significant relative to the control group.

The effect is economically large relative to the size of the treated group. The treated group accounts for 0.34 percent of all Bitcoin trades during the pre-EIP phase (January 1 to April 8), so we estimate that it rises to 0.54 percent during the disbursement phase, all else equal (based on the coefficient of 0.4733 estimated in Table 4). This is a 60 percent increase in relative volume. However, it is small compared to the overall size of the Bitcoin market. This is partly due to our conservative identification strategy.

In Table A.2, we show EIPs still have a significant effect on Bitcoin trading when we vary the bandwidth. We try various bandwidths from \$12.50 to \$100, and in every case, we find statistical significance at the 1 percent level. For brevity, in the table we use all three fixed effects and omit most of the regression coefficients, showing only the coefficients of the interaction of the *treated* dummy with the two phases. In all cases, there is a significant increase in the number of treated trades, relative to control trades, during the disbursement phase (Prediction P.1), but not the announcement phase (Prediction P.2).

For the disbursement phase, the estimated coefficient tends to decrease in the bandwidth (except when going from $h =$ \$37.50 to \$50). Statistical significance also tends to fall. As the bandwidth increases, we can be less confident that the treated group is mainly comprised of trades financed by EIPs.

## 4.3 Larger EIP payments to families

We test Prediction P.3 by repeating the analysis with cutoffs of \$2,400 (Table A.3), \$2,900 (Table A.4), \$3,400 (Table A.5), and \$3,900 (Table A.6). These cutoffs correspond to the EIPs received by couples with zero, one, two, and three children, respectively, before adjusting for household income. For each cutoff, we define treated and control groups using a bandwidth equal to 5 percent of the cutoff value.[28]

In each of these four cases, we find that the EIPs do not have a significant and robust

[27] We carry out the same tests using value rather than volume of trade and, unsurprisingly, find similar results. The details are available on request.

[28] This is approximately equal to a Coinbase fee on a credit card transaction, as discussed in Footnote 20 and the bandwidth used in Section 4.2, rounded up for mathematical simplicity.

impact on Bitcoin trading (i.e., the coefficient of $disbursed \times treated$ is not significantly different from zero). As we expected, there is no evidence that EIPs caused families to invest more in Bitcoin.[29]

To account for the possibility that EIP recipients are leveraging up their trades, we also try repeating the analysis with cutoffs equal to integer multiples of the modal EIP amount: $3,600, $4,800, and $6,000. These represent an EIP of $1,200 levered up to 3, 4, and 5 times the cash amount, respectively. In each case we do not find any significance. We conclude there is no evidence that EIP recipients are using leverage to increase the amount they can invest in Bitcoin.[30]

## 4.4 Effect of EIPs by exchange

We test Prediction P.4, which posits that the EIP effect is stronger on exchanges that have more non-professional traders, and that are domiciled in the United States. We propose a simple statistic for the non-professionalism of the user base. For a given currency $i$, exchange $j$, and day $t$, we define $retail_{ijt}$ to be the logarithm of the proportion of trades under $5,000 in size, relative to the total number of trades under $1 million.

Table 5 shows summary statistics for the $retail_{ijt}$ statistic for each exchange $j$, fixing $i$ to represent USD trades. These are computed over the phase January 1 to March 26, 2020, before the EIP program is announced. For all exchanges, the vast majority of trades below $1m are smaller than $5,000. Most of the large, retail-focused exchanges such as BinanceUS, Coinbase, and Kraken have scores consistently above -0.10, suggesting that more than 90 percent of trades below $1m are less than $5,000. A few exchanges do have days when the ratio falls lower.

The definition of the retail statistic is motivated by the idea that retail traders are likely to make smaller trades than professional investors. The exact definition is somewhat arbitrary, as there is no clear point below which we can strictly define whether a trade is made by a professional user or not. While $5,000 is not particularly large for a retail trade, using a higher number would reduce the variation in the retail statistic. We feel that $5,000 provides a good trade-off. Furthermore, it is well above the modal EIP size, so our definition of the retail statistic should not confound our results. We exclude trades above $1m from the denominator in order to limit any effect of volume manipulation by exchanges.[31]

[29]Table A.6 does exhibit significance at the 5 percent level, but only when all fixed effects are included. We conclude that the effect is not robust.

[30]The results are available on request. We do not consider 2 times leverage here, since we already examined a $2,400 cutoff in Table A.3.

[31]Lack of regulation in the cryptocurrency market has allowed some exchanges to fake volumes, in order to improve their ranking on popular comparison websites like Coinmarketcap.com. One way to do this is for

We run the regression described in Section 4.2, interacting the independent variables with the retail statistic. Table 6 shows the results. The interaction of the EIP effect ($disbursed \times treated$) with $retail_{ijt}$ is positive and statistically significant. This suggests that the effect of the EIP on Bitcoin trading is stronger on exchanges that have a larger retail user base, as predicted. The interaction term is significant at the 5 percent level, unless exchange fixed effects are introduced (model (4)). This is likely because exchange fixed effects substitute to some extent for variation in the retail ratio statistic, and thus reduce its explanatory power.

To give a sense of the economic magnitude of this effect, we can compare the exchange with the highest average retail ratio over the pre-EIP period (BitBay) to the lowest (LMAX). We estimate that, during the EIP disbursement period, the proportion of treated trades on BitBay would have been about double that on LMAX, all else held equal.

In Table 6, the coefficient of $announced \times retail$ is very large and positive, but it is almost exactly cancelled out by the coefficient of $announced \times treated \times retail$. Neither coefficient is statistically different from zero. This suggests a multicollinearity issue, perhaps because the announcement period is short. This is not a problem for Prediction P.4, but we should be wary about drawing any conclusions about the announcement period from Table 6.

# 5 Results for round trade sizes

So far, we have focused on cutoffs for the entire modal EIP amount of $1,200. We now consider the possibility that some EIP recipients may decide not to use the full EIP to buy Bitcoin, but instead keep back some of the money received for other purposes.

What does economic theory predict about how an agent responds to a wealth shock? If she were purely rational, an agent would spend her increased budget across a bundle of goods. She decides her allocation by setting the marginal rate of substitution between any two goods equal to their relative price. Upon EIP disbursement, we might expect this rational agent to purchase some Bitcoin, assuming it has some value for her. But, without knowledge of her marginal rate of substitution between Bitcoin and other goods, we cannot make empirical predictions about the sizes of these purchases. This means that, if agents have heterogeneous preferences, we should not expect to see a general increase in Bitcoin purchases of any particular size.

We take an alternative approach and abstract from perfect rationality. We posit that

the exchange owner to carry out wash trading. See, for example, Fusaro and Hougan (2019). The raw ratio does not vary much between exchanges, so we take logs to increase the variation.

EIP recipients may make cognitive shortcuts, choosing to simply invest a round number amount of dollars, rather than computing marginal rates of substitution. In Section 5.1 we discuss the theoretical justification for this approach, and in Section 5.2 we examine the evidence.

## 5.1 Literature on round number preference

There is evidence that, when agents face a high degree of uncertainty, they tend to be drawn to decisions involving round numbers. Given that returns on Bitcoin are so uncertain — and our period of study coincides with heightened macroeconomic uncertainty — an individual may find it difficult to work out the optimal investment amount. Instead, he or she may choose to focus on a round number, which feels "about right". We develop these ideas using insights from the behavioral economics literature.

Experimental evidence suggests that people are not able to consistently assign certainty-equivalent values to a lottery, giving rise to a "preference reversal" puzzle (Tversky and Thaler (1990)). Subjects' certainty-equivalent values tend not to be precise, but fall within a range (Butler and Loomes (2007)). These ranges tend to be wider when the lottery is more uncertain, and when the maximum payoff is higher (Binder (2017) and Butler and Loomes (2011)).

Faced with uncertainty, agents tend to be drawn to round numbers. In Lillard and Willis (2001) and Khaw, Stevens, and Woodford (2017), subjects are asked to estimate a probability and are shown to tend toward "focal answers" such as 0, 0.5, and 1. Griffin and Shams (2020) and Urquhart (2017) find evidence of round number effects on Bitcoin prices, though they do not examine trade sizes. The literature on round number bias in economics and finance is large — see Mitchell (2001) for an overview — but tends to focus on probabilities or prices, rather than decisions around investment amounts. One exception is Hervé and Schwienbacher (2018), who report a tendency for round number investment amounts in the French equity crowdfunding market, especially when investors face greater uncertainty. The paper argues that round number preference may be particularly strong because the crowdfunding market is marked by high uncertainty and a large number of amateur investors. These are also characteristics of the market for Bitcoin.

## 5.2 Results

We examine evidence for an increase in round number amounts below $1,200. Specifically, we look for evidence of an increase in trades for $1,000, $600, $500, and $100 after EIP disbursement begins. These amounts are chosen either because they are salient round numbers or, in the case of \$600, equal to exactly half of the modal EIP amount. Our regression equation is the same as that in Section 4.2. Again, for each cutoff, we define treated and control groups using a bandwidth equal to 5 percent of the cutoff value. We focus on Predictions P.1 and P.2.

Table 7 presents the results for the \$1,000 cutoff. In all four specifications, the coefficient of $disbursed \times treated$ is significantly positive. This suggests that EIPs do increase the number of \$1,000 Bitcoin buy trades. However, the effect is weaker than for a \$1,200 cutoff: the coefficient is smaller and is significant only at the 5 percent level. Again, we find no evidence of an announcement effect.

Table 8 examines the effect of EIPs for cutoffs of \$100, \$500, and \$600. For brevity, we show the results including all fixed effects. Once again, we find there are more trades for these amounts during EIP disbursement, relative to the period before the CARES Act, and relative to non-program currencies. These results are all significant at the 1 percent level.

When all fixed effects are used, the results for \$1,200 and \$600 have lower $p$-values (both 0.0 percent to 1 decimal place) than \$1,000 and \$500 (1.9 and 0.3 percent, respectively). This is because there is a concentration of trading at round numbers such as \$1,000 and \$500, independently of the effect of EIPs (Figure 4). This makes our identification at round number cutoffs more challenging.

The $p$-value for the \$100 cutoff is very low (0.0 percent), suggesting that trades at this level exhibit behavior from that of the other round number amounts. In addition, there appears to be an announcement effect for \$100 trades that we do not see with the other cutoffs. In other words, there is an increase in Bitcoin buy trades for \$100 as soon as the EIP program is announced. We posit that, once the CARES Act is passed, some American households are confident enough to buy small amounts of Bitcoin out of their own pocket, in anticipation of being reimbursed later in the form of EIPs. Nonetheless, the announcement effect is weaker than the disbursement effect (i.e., the coefficient of $disbursed \times treated$ is larger and more statistically significant than the coefficient of $announced \times treated$). Thus, Prediction P.2 remains valid.

There is direct evidence for round number preference in Tables 7 and 8. For the round number cutoffs (\$1,000, \$500, \$100), the coefficient on the *treated* dummy alone is positive and significant at the 1% level. This suggests, even before the CARES Act passed, there are more USD buy trades for round number amounts, relative to buy trades in other currencies. While BTCUSD trades focus on round number trade sizes in USD terms, investors in non-USD currencies do not because, when converted into their currency, this quantity is no longer a round number and has no salience.[32]

[32]There also seems to be some preference for \$600 trade sizes throughout the period. We have no expla-

Round number preference means the assumption of parallel trends is harder to justify for the round number cutoffs than for $1,200 and $600. For example, an alternative explanation for our results at round number cutoffs could be that the EIP disbursement period simply coincides with a time of heightened uncertainty, which causes traders to prefer round number trade sizes. We test this hypothesis in Section 8 and find no evidence for it.

As a robustness check, we vary the bandwidths using the same proportionate changes as in Section 4.2. In each case, we vary the bandwidth between 1.25 and 10 percent of the cutoff value.[33] With a $600 cutoff, the results are robust to changes in bandwidth (Table A.8), with the estimated coefficient declining in the bandwidth, similar to a $1,200 cutoff. However, the results are less robust to bandwidth variation for cutoffs of $1,000 and $500 (Tables A.7 and A.9), most likely because round number preference creates a challenge to identification at these cutoffs. Again, the results for the $100 cutoff are more robust to changes in bandwidth than the other rounded amounts.

The significant results at the $600 cutoff may be due to another stimulus program. Federal Pandemic Unemployment Compensation (FPUC) is a program created by the CARES Act, under which an additional $600 a week was paid to US claimants of unemployment insurance during the period March 27 to July 26, 2020. On the one hand, FPUC recipients are likely have more urgent financial needs than buying Bitcoin (Baker et al. (2020)). On the other hand, FPUC payments exceeded lost income in many parts of the United States (Ganong, Noel, and Vavra (2020)), so some recipients may have found themselves wealthier — and with more free time to learn about Bitcoin — than before the crisis. We isolate the impact of FPUC payments from EIPs by exploiting differences in the timing of the payments. FPUC payments begin immediately after the passage of the CARES Act, i.e., during the EIP announcement period. If FPUC payments are used to buy Bitcoin, we should expect to see a significant positive coefficient in Table 8 for the interaction term ($announced \times treated$). In fact, the estimated coefficient is negative. Therefore, we find no evidence, at any significance level, that FPUC payments are used to buy Bitcoin.

Finally, we run placebo tests to verify that the round number cutoffs tested here are indeed special. We repeat our regressions with cutoffs where we do not expect EIPs to have any effect: $200, $750, $4,000, and $12,000. We choose these amounts to provide a range of different values, but without treated or control groups that overlap with those we have already tested. Table A.11 shows the results of these placebo tests with bandwidths equal to 5 percent of the cutoff value. In each case, there is no significance at the 5 percent level for

nation for that.

[33]Large bandwidths run the risk of overlap between the treated group of one cutoff and the control group of another. That may mean that the regression results underestimate the EIP effect at the lower of the two cutoffs.

the *disbursed* × *treated* interaction term. Therefore, the EIP program is not associated with an increase in Bitcoin trading for these non-round amounts. Moreover, the *treated* dummy alone is not significant for these cutoffs, suggesting no evidence that these trade sizes are focal values for investors (apart from $200).

# 6 Magnitude of effect of EIPs on the Bitcoin market

In this section, we estimate, in dollar terms, the overall magnitude of the effect of the EIP program on the Bitcoin market. Using our results at various cutoffs, we first examine the effect on trade volume, and then the price, which allows us to assess Prediction P.5.

## 6.1 Impact of EIPs on Bitcoin trade volume

Define $y_{jt}$ to be the total number of trades on exchange $j$ on day $t$, and let $x_{jt} \leq y_{jt}$ be the number of trades in the treated group. Let $z_{jt} \leq x_{jt}$ be the number of treated trades that are financed by EIPs; i.e., they would not occur without the program. We cannot directly observe $z_{jt}$, so we need to find an estimator for it.

Let $\delta$ be the estimated coefficient of the *disbursed* × *treated* interaction term. Recall that the dependent variable is the log-odds of the proportion of trades in the treated group. Then we can produce an estimate $\hat{z}_{jt}$ for $z_{jt}$, defined as follows:

$$\log\left(\frac{\frac{x_{jt}}{y_{jt}}}{1-\frac{x_{jt}}{y_{jt}}}\right) \quad = \quad \delta \quad + \quad \log\left(\frac{\frac{x_{jt}-\hat{z}_{jt}}{y_{jt}-\hat{z}_{jt}}}{1-\frac{x_{jt}-\hat{z}_{jt}}{y_{jt}-\hat{z}_{jt}}}\right). \tag{3}$$

Rewriting Equation (3) in terms of $\hat{z}_{jt}$, we obtain the following simple expression:

$$\hat{z}_{jt} \quad = \quad x_{jt}(1-e^{-\delta}). \tag{4}$$

For example, we have $\delta = 0.4733$ with a cutoff of $1,200 (Table 4), so we estimate that around 37.7 percent of treated group trades in the disbursement period would not happen in the absence of EIPs. In Table 9, we report the number and value of treated trades over the disbursement period, for each cutoff (that is, $x_{jt}$). Using Equation (4), we can estimate the number and value of treated trades due to EIPs, at each cutoff. Note that, because the volume of activity is concentrated at round number amounts, the marginal impact of the EIPs is highest for the round number cutoffs.

Summing the columns of Table 9, we estimate that, during the EIP disbursement period, 219,780 trades — with a value of $58.00m — would not occur in the absence of the EIP program. At these cutoffs, these trades constitute 41.7 percent of all treated trades by volume, and 26.6 percent by value. However, the impact is much smaller when measured compared to the size of the entire Bitcoin market. We estimate that 3.8 percent of trades by number, and 0.7 percent by value, are due to the EIPs.[34] Therefore, the EIP program has a limited impact on Bitcoin trading during this period.

The numbers are even smaller when compared to the size of the EIP program. We estimate that around 0.14 percent of EIP payments by volume are used, at least in part, to buy Bitcoin, and around 0.022 percent by value.[35] In other words, given an EIP for $1,200, about 26 cents on average goes into Bitcoin. Given our priors, it is not surprising that few people use their EIPs to buy Bitcoin. However, our work suggests that the distribution is highly skewed: there is a small number of very enthusiastic Bitcoin purchasers who use all or most of their EIP to buy Bitcoin.

Our numbers are likely to underestimate the true impact of the EIP program on the Bitcoin market, because there may well be EIP recipients who decide to invest some amount other than $1,200 or a round number. Using a wider bandwidth increases the numbers, but not by much, because the estimated values of $\delta$ tend to be lower. For example, repeating the exercise with a bandwidth of 10 percent of each cutoff value suggests that 238,692 trades, with a value of $57.76m, would not occur without the EIP program. Other bandwidths yield lower results. We conclude that the EIP program does not have a substantial impact on Bitcoin trading.

## 6.2 Impact on Bitcoin prices

Prediction P.5 posits a higher price after EIP disbursement. As discussed in Section 3.3, we might not expect to see a significant difference between the prices of treated and control trades after EIP disbursement. Therefore, we take a slightly different approach and examine whether, during EIP disbursement, the prices at which buy trades are executed are higher on exchanges with a larger proportion of treated trades.

We define a new dependent variable $\log(price_{ijt})$, the logarithm of the mean execution price for all buy trades in currency $i$ on exchange $j$ at time $t$. We examine whether this is positively related to the proportion of buy trades in the treated group. In other words, we introduce $Y_{ijst}$ as an independent variable, with $i$ set to USD, and $s$ set to be the treated group. We have established that EIPs only affect trading in USD, so we restrict attention

[34] In total, there are 5,768,935 BTCUSD buy trades during the EIP disbursement period, with aggregate value $8,402.252m.

[35] We use the volume and value of EIPs paid out as of June 3, 2020 (see Figure 3). The IRS has not released more recent figures, but we do know the vast majority of EIPs were paid out by June 3.

to $i$=USD. The regression is described by Equation (5):

$$\begin{aligned}\log(price_{USD,j,t}) \;&=\; \hat{\alpha} \;+\; \hat{\boldsymbol{\beta}} \cdot \boldsymbol{phases}_t \;+\; \hat{\gamma} \times Y_{USD,j,treated,t} \\ &\quad+\; \hat{\boldsymbol{\delta}} \cdot \boldsymbol{phases}_t \times Y_{USD,j,treated,t} \;+\; \hat{\mu}_j + \hat{\nu}_{\tau(t)} + \hat{\epsilon}_{jt}.\end{aligned} \tag{5}$$

where, as before, $\boldsymbol{phases}_t$ is a dummy vector of size 2, denoting whether time $t$ falls during the EIP announcement or disbursement period. We put hats over the coefficients, to make it clear that these are not the same as those in the previous regression equation. We include fixed effects for the exchange and time, along with normally distributed error terms.

We expect any effect of the EIPs on price to be short-lived, since price differences can be arbitraged away across exchanges. With this in mind, we define time $t$ on an hourly basis, rather than daily as before, so that we can better capture temporary changes in price. For the time fixed effects, we define a projection function $\tau(t)$, so that we can explore fixed effects over different time horizons. We consider time fixed effects at the hour, day, and week level.

Table 10 shows the results with a cutoff of \$1,200. The independent variable *pct_treated* is the percentage of trades that are treated (i.e., $Y$). Models (3), (4), and (5) employ hourly, daily, and weekly fixed effects, respectively, as well as exchange fixed effects. In all five specifications, the coefficient on the interaction term $disbursed \times pct\_treated$ is positive and significant. Therefore, there is evidence for Prediction P.5: exchanges with more treated trades have higher prices over the disbursement period, all else equal. Aside from model (5), significance is at the 5 percent level or better.

The time fixed effects, even at the weekly level, produce very strong fits, measured by $R^2$. This is because time fixed effects capture all unobserved market-wide pricing factors other than EIPs, so it is not surprising they explain prices much better than EIPs alone.

Tables A.12, A.13, A.14, and A.15 give the results with cutoffs of \$1,000, \$600, \$500 and \$100, respectively. For \$1,000 and \$600, the picture is like that for \$1,200: exchanges with a higher number of treated trades during disbursement tend to have higher execution prices. For \$500 and \$100, prices are significantly higher only when we use time fixed effects at the weekly level.

We can estimate the total impact of EIPs on price using the results from Section 6.1. At a cutoff of \$1,200, $Y$ increases by 0.13 percent.[36] Using model (2) in Table 10, this implies an increase in price of $\exp(0.0013 \times 0.4002) - 1 = 0.05$ percent. In other words, the boost in demand for \$1,200 trades raises the price of Bitcoin by 5 basis points. Repeating this for the

[36] That is, 7,459 divided by (5,768,935-7,459) total trades.

other cutoffs at $1,000, $600 and $500 gives a total change in price of 21 basis points.[37] The effect on price is about the same — 22 basis points — using model (1) without exchange fixed effects, and much smaller if we use the models with time fixed effects.[38]

We are unable to say whether the price impact of the EIP program is permanent. Our methodology cannot preclude, for example, that agents sell their Bitcoin soon after buying it, in which case it is unlikely that the price impact is long-lasting. Makarov and Schoar (2020) estimate the price impact of changes in Bitcoin trading and conclude that about one-third of the price impact is permanent. This suggests that the EIP program permanently increases the price of Bitcoin by about 7 basis points. This is an economically small effect compared to the day-to-day volatility of the Bitcoin price.[39] We can thus conclude that the EIP program has only a marginal effect on the price of Bitcoin.

Our methodology probably underestimates the true price impact because we assume limited cross-exchange arbitrage. If it is easy to trade price differences between exchanges, then a price rise on one exchange is quickly transmitted to others. In the limit, if arbitrage were perfect, we should obtain a coefficient of zero. In fact, there are substantial barriers to trade between cryptocurrency exchanges, due to cross-border capital frictions (Makarov and Schoar (2020)) and the limited capacity of the Bitcoin blockchain to process transactions (Hautsch, Scheuch, and Voigt (2018)).

As a check, we can compare our estimated price impact to Makarov and Schoar (2020), who estimate that an increase in buy volume of 10,000 BTC in one day raises the price by 9 percent, including any temporary impact. In Section 6.1, we find an increase in net order flow of $58m over the disbursement period. Figure 5 suggests this increase is concentrated in an initial period of less than 24 days, suggesting a mean increase in daily net order flow of about $2m. The volume-weighted average price of Bitcoin over this period is $7,571. Thus, Makarov and Schoar (2020)'s results would suggest a rise in price of 24 basis points, of which one-third is permanent. This is very close to our estimate.

[37] We exclude the $100 cutoff because it suggests a negative effect on price, which has no economic interpretation.

[38] As previously discussed, time fixed effects proxy for all marketwide factors aside from EIPs, and so remove a lot of explanatory power from our regressions. For this reason, models (3), (4) and (5) are likely to underestimate the true impact of EIPs on price, and we focus on model (2) instead.

[39] Over our data period, the standard deviation of the one-day return on the Bitcoin closing price is 4.6 percent.

# 7 COVID-19 stimulus payments in other countries: A quasi-natural experiment

In response to the COVID-19 crisis, several governments around the world introduced schemes like the US Economic Impact Payment program. In this section, we analyze whether these programs influence Bitcoin trading in the relevant currencies.

Gentilini et al. (2020) report that, as of June 12, 2020, five jurisdictions had introduced policies making one-off universal cash payments to households: Hong Kong, Japan, Serbia, Singapore, and South Korea.[40] Since then, Israel began its own program, and the US made two further rounds of payments. The various schemes are summarized in Table 11. To our knowledge, no other country responded to the crisis by making direct payments to households with minimal eligibility conditions.

We characterize our empirical setup as a quasi-natural experiment. We assume a government's decision to introduce such a program is not related to other characteristics of interest, such as its citizens' propensity to invest in Bitcoin. Then Bitcoin traders around the world are randomly assigned treatment, depending on whether their country introduces an EIP-like program.

We study the effect of the programs in Japan, Singapore, and South Korea. We exclude Hong Kong and Israel, as well as the second and third US rounds, since those schemes did not begin to pay out until after our sample period ends.[41] In addition, we exclude Serbia, since our data set contains zero transactions in Serbian dinar over the sample period. Throughout the paper, we treat the Hong Kong dollar and Israeli shekel as non-program currencies (see Table 3).

There are a few papers studying how beneficiaries of these programs used their money. Findings are similar to those for the EIP program. Feldman and Heffetz (2020) study the Israeli program and show that much of the money is used to pay down debt. Kim, Koh, and Lyou (2020) study data on card transactions in Seoul and find that the payments have an immediate impact on consumption, but they do not explore spending on investment goods. Kubota, Onishi, and Toyama (2020) study Japanese bank account data. They exploit local variation in disbursement of the stimulus payments to estimate marginal propensities to consume. They find responses are heterogeneous and depend on individual recipients' financial circumstances.

[40] The US is not included in this list, as the income cut-off means that a significant proportion of households are excluded from the EIP program (Table 1). We are grateful to Ugo Gentilini at the World Bank for clarifying this.

[41] Although the Hong Kong program was announced on February 26, the first payments were not made until July 8, and our earlier results suggest that an announcement effect is unlikely.

## 7.1 Overview of programs in Japan, Singapore, and South Korea

### 7.1.1 Japan

On April 16, 2020, the Japanese Prime Minister announced, as part of a larger stimulus package, that each resident of Japan would receive a one-off tax-free "Special Cash Payment" of ¥100,000. At the time of first disbursement on April 27, this was equivalent to about US$933. All registered residents in Japan, including foreign residents, were eligible, regardless of income or wealth. Expatriate Japanese citizens were ineligible. Payments were not made automatically, so residents had to actively apply and supply bank details.[42]

Disbursements of the Special Cash Payments were handled by individual municipalities, so timing varies locally. Once a municipality opened the application process, residents had three months to apply. The government planned most payments to be made by the end of July.[43]

### 7.1.2 Singapore

On February 18, 2020, the Singapore government announced a budget, including a Care and Support Package, to combat the crisis. This included a SG$600 Solidarity Payment to all adult Singaporeans, worth US$424 at first disbursement (April 14). Individuals who had previously received government money automatically received their Solidarity Payments on April 14. This comprised around 90 percent of all potential recipients. The remainder were asked to provide their bank account details by April 23, for payment to be made on April 28. Failing that, a check would be posted on or after April 30. Additional money was available to Singaporeans based on age, income, and childcare responsibilities, as well as some foreign permanent residents.[44]

### 7.1.3 South Korea

On March 30, 2020, the South Korean president announced that the government would make one-off direct payments to all but the richest 30 percent of households. The first payments were made on May 4. A single-person household received ₩400,000, with ₩200,000 for each additional member, up to ₩1 million for a four-person household.[45] The funds were

[42] See https://kyufukin.soumu.go.jp/en/.

[43] See Tokyo Weekender, "All you need to know about how to receive your Covid-19 ¥100,000 stimulus from the government," https://tinyurl.com/y6gvvm59.

[44] For more details, see Ministry of Finance, https://tinyurl.com/4w3ubprt and https://tinyurl.com/ee3kzmbk.

[45] On May 4, 2020, there were ₩1,229 to one US dollar.

not paid automatically, but had to be applied for within three months.[46]

The Korean government prioritized the 2.8 million households on welfare, paying them in the first week via bank transfers. These comprised about 13 percent of all eligible households. Payments to other households began the following week and continued for three months. For these households, the money was transmitted in the form of credit or debit card points, regional gift certificates, or prepaid cards, as preferred by the applicant. More than 92 percent of payments were made by May 25. The money expired if not spent by August 31, and there were restrictions on where it could be used. For example, the money could not be spent at large supermarkets or entertainment venues, nor on online shopping. See Kim, Koh, and Lyou (2020) for more details.

South Korea is a particularly interesting case, because Bitcoin trading is much more widely practiced compared to the other countries in our study.[47] This suggests that Koreans might have a higher propensity to invest any windfall in Bitcoin. On the other hand, the spending restrictions could limit any impact on the Bitcoin market. It is not clear to us whether buying Bitcoin was an acceptable use of the money, but it seems unlikely. We might still expect to see an effect if households merely substitute one source of money for another, but Kim, Koh, and Lyou (2020) find little evidence that this occurs.

## 7.2 Results

For each of Japan, Singapore, and South Korea, we examine Predictions P.1 and P.2 for the relevant local currency. In the period after payments start, we expect an increase in Bitcoin buy trades in local currency for amounts equal to and just under the amount paid to single individuals. Thus, we use cutoffs ¥100,000 for Japan, SG$600 for Singapore, and ₩400,000 for South Korea. As with the US program, we do not expect to see an effect during the announcement period.

Using Equation (1), we run event studies for the three countries. In each case, we make $t = 0$ the disbursement date and set the bandwidth equal to 5 percent of the cutoff. We do not show announcement dates, since for Singapore and South Korea the programs were announced long before the first payments were made. Figure 6 shows an increase in buy trades in Japanese yen following disbursement, while Figure 7 suggests no evidence of an increase in Singapore dollar trades. As for Korean won, Figure 8 provides some evidence

[46] For more information, see Reuters, "South Korea to pay families hundreds of dollars to ease coronavirus impact," https://tinyurl.com/yyee3l8r, and Yonhap, "Payments of disaster relief money begin Monday," https://tinyurl.com/y3jf2wc5.

[47] See New York Times, "Cryptocurrency was their way out of South Korea's lowest rungs. They're still trying," https://tinyurl.com/2unmt88e, and VentureBeat, "Why South Korea is 'crypto crazy' and what that means for the rest of the world," https://tinyurl.com/yyqejajv.

of a delayed response, beginning about 10 days after disbursement began and ending after about a week. This may be due to the Korean government prioritizing payments to welfare recipients, who may be less likely to use the money to buy Bitcoin. Still, on no individual day is abnormal trading in Korean won significantly different from zero.[48]

Next, we run difference-in-difference estimations for the three countries, using Equation (2). For each currency, we try various bandwidths and display selected values. None of the currencies see a positive announcement effect, confirming Prediction P.2. Of these three currencies, the best results are for the Korean won, where the program has a positive effect on trading at various bandwidths (Table 14). For the Japanese yen, there is no significance with a bandwidth equal to 5 percent of the cutoff value (that is, ¥5,000), but there is a significant positive effect at smaller bandwidths (Table 12), perhaps due to round number preference. In contrast, the Singapore dollar sees a significant positive effect only at very large bandwidths (Table 13). We conclude that Prediction P.1 holds for Japan and South Korea, but not for Singapore.[49]

Our results suggest that the programs have a significant and positive effect on Bitcoin trading in Japan and South Korea. It seems the restrictions on how the Korean money could be used did not prevent people from diverting funds into Bitcoin. For Singapore, we observe significance only with a much larger bandwidth than we would normally use. We cannot say for sure why Singapore behaves differently from the US, Japan, and South Korea, but we suggest two reasons. First, trading volumes in Singapore dollars are much lower than for the other three currencies (Table 3). Second, regulatory changes during our sample period may have caused a regime change in Singapore dollar trading, confounding our analysis.[50]

# 8 Placebo tests

We conduct two additional tests to check that the effects we identify are truly related to the Economic Impact Payments program, and not to coincident events, such as the economic

[48]We use a window of 18 days, rather than the 24 days for US, because of the short window between the Japanese announcement and disbursement dates.

[49]We test and reject the possibility that the Singapore result is due to a rounding effect. If Singaporeans buy SG$500 of Bitcoin, then a test of SG$600 with a large bandwidth could show false positive results. However, we find no statistically significant positive effect using a cutoff of SG$500 and a bandwidth of SG$25. Results are available upon request.

[50]On January 28, 2020, the Singapore government introduced new rules requiring cryptocurrency businesses to be covered by anti-money laundering rules. There are reports of some firms pulling out of Singapore due to the new regulation. See Coindesk, "Singapore announces new AML rules for crypto businesses," https://tinyurl.com/v6yn45x6. In our data set, mean daily SGD buy trade volumes fall from 280 buys (with a total value of SG$630k) during the period prior to the budget announcement, to 82 buys (SG$111k) during the announcement period, and 170 buys (SG$303k) after disbursement.

crisis or the Bitcoin halvening event. We do this by repeating our regressions on two groups of trades that we do not expect to be affected by the EIPs: sell trades and trades in non-program currencies.

## 8.1 Sell trades

Our analysis so far has only used data on buy trades. We would not expect to see any significant effect of EIPs on sell orders, because trades should be initiated by the EIP recipient, who is buying Bitcoin in exchange for cash. This makes sell trades a good candidate for a placebo test. We repeat our regressions from Section 4.2, but use BTCUSD sell, rather than buy, orders. If we still find significant results, that suggests our findings are driven by some factor other than the EIPs.

Table 15 shows there is no significant increase in sell orders for amounts around $1,200 during the disbursement period. Table A.16 concludes the same for a cutoff of $1,000. These results suggest that our earlier findings must be driven by a factor that affects only buy orders — that is, the EIP program — and not by a factor that also affects sell orders.

## 8.2 Non-program currencies: A test for round number cutoffs

While we have established a link between $1,200 trades and EIP disbursement, the connection is slightly weaker for round number cutoffs, though still significant. This raises the possibility that agents simply have a stronger preference to buy round amounts of Bitcoin when uncertainty is higher (see Section 5.1), and this happens to coincide with EIP disbursement. If this were so, we would expect to see an increase in buy trades for round number amounts of non-program currencies at the same time. This concern is somewhat mitigated by the inclusion of the USD dummy in Equation (2). In this section we address the concern more directly, by using round number cutoffs of non-program currencies.

We test whether there is an increase in trades for round amounts of non-program currencies during the period coinciding with US disbursement. We use data on buy trades in euros and British pounds sterling, which are the non-program currencies with the highest trading volumes in our data set (see Table 3). We choose trade sizes of €1,000 and £1,000, and bandwidths of €50 and £50, respectively. The dummies *announced* and *disbursed* are defined with the same dates as the US EIP program.

As before, the regression design calls for the inclusion of all trades in non-program currencies corresponding to the treated and control group sizes (indexed by $i$ in Equation (2)). For the purposes of this section, we count euros and pounds as program currencies. It turns out that our data set contains zero trades for these sizes in any non-program currencies

(Table 3 shows the trading volumes are low). As a result, there is no variation in currency in our regressions, and so we do not use currency fixed effects. We also do not need standalone terms for the phases (that is, the $\beta$ term in Equation (2)), since the time fixed effects take care of these.

Tables A.17 and A.18 show the corresponding results. In neither case is the coefficient of the $disbursed \times treated$ interaction term significantly positive, so we reject the hypotheses of an increase in trades for €1,000 or £1,000 after April 9, 2020. This suggests the increase in USD round number trades we describe in Section 5 is most likely due to the EIPs, and not some unobserved global factor. We can conclude that the halvening event does not drive our results, although we cannot rule out that it magnifies the effect of the EIPs.

# 9 Conclusion

In this study, we demonstrate a significant and robust link between the economic impact payments paid to US citizens and residents in spring 2020, and the Bitcoin market. There is a significant increase in Bitcoin buy trades for \$1,200, the modal EIP amount, relative to other currencies. We associate the EIPs with a 3.8 percent increase in the volume of Bitcoin-USD buy trading (by volume) between April 9 and June 5, 2020. The decision to invest in Bitcoin is very heterogeneous, with only a few people choosing to invest the entire amount. We find no evidence that EIP recipients with families, nor people who received unemployment insurance, use the money to buy Bitcoin. We make use of a quasi-natural experiment and show that our results hold in Japan and South Korea, which introduced similar programs, but not in countries with no such programs.

Our findings help to understand the role that retail investors play in cryptocurrency markets. The EIP program can be thought of as a demand shock for retail investors. It is possible that there is an increase in trading of other asset classes following EIP disbursement, but the structure of the Bitcoin market — where retail trades are often executed directly on an exchange, rather than intermediated by a broker — makes it particularly conducive to our methodology.[51]

The COVID-19 crisis has impacted different people in different ways. While unemployment rose dramatically, so did the savings rate. Some people were hit very hard — for example, those working in restaurants and tourism — while others were able to adapt by working from home. Those EIP recipients who did not need the money to replace lost income or pay down debts may have chosen to invest it in Bitcoin. Nonetheless, we estimate

[51] There is evidence that retail trading of stocks increased during the pandemic, partly due to stimulus checks. See, for example, Vox, "Who gets to be reckless on Wall Street?", https://tinyurl.com/9u9m3wu7.

that only 0.02 percent of all EIP dollars were spent on Bitcoin, suggesting that policymakers should not be concerned about money being diverted to cryptocurrency markets when considering similar economic relief programs in the future.

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Figure 1: **Tweet from Coinbase CEO on April 16, 2020**
Source: https://tinyurl.com/ycrvpb5e.

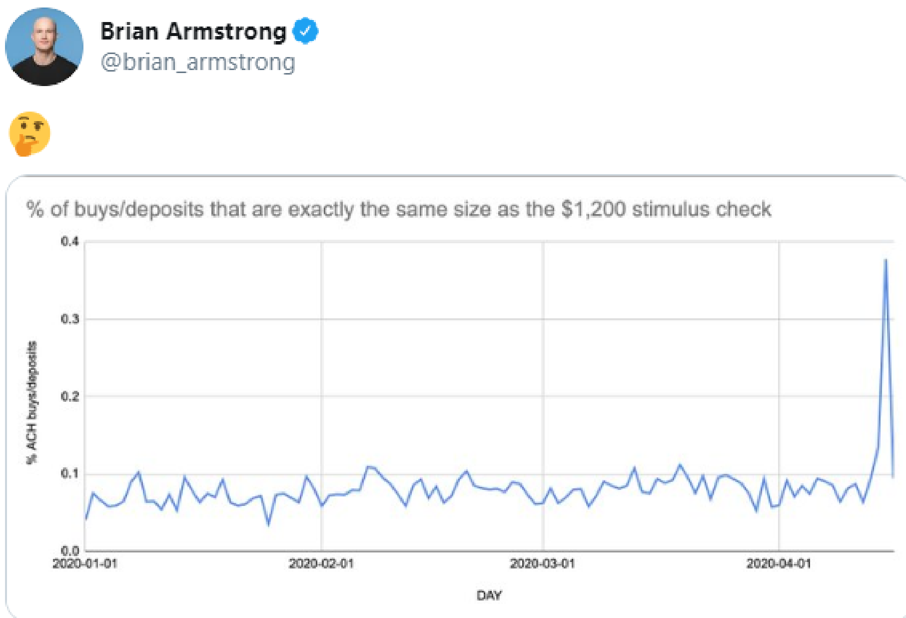


Figure 2: **Daily Bitcoin price and exchange activity**
Data are obtained from Blockchain.com and Yahoo Finance, and span the period January 1 to June 30, 2020. Bitcoin price is shown as the solid line and plotted on the left-hand axis. Total Bitcoin trading across cryptocurrency exchanges (in US$ billions) is shown as a dotted line and plotted on the right-hand axis.

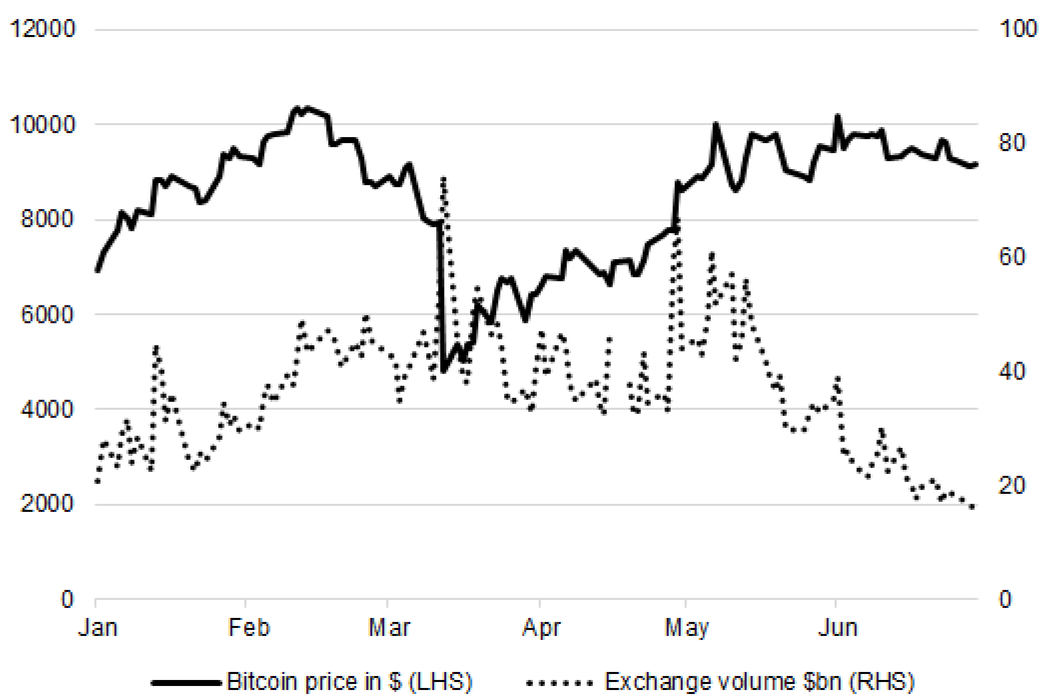

Figure 3: **Timeline of disbursement of economic impact payments**
Cumulative economic impact payments made, based on data released by the IRS on April 17, May 8, May 22, and June 3, 2020.

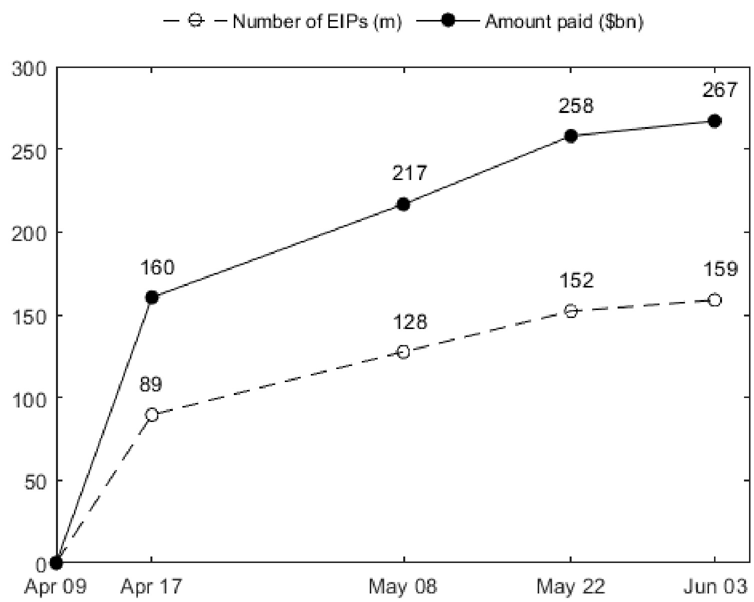

Figure 4: **Histogram of BTCUSD trades around passage of CARES Act**
Each panel shows the average number of Bitcoin trades per day in USD for individual trade amounts (rounded to the nearest ten). The first panel shows BTCUSD trades in the phase before the announcement of EIPs under the CARES Act, i.e., from January 1 to March 27, 2020. The second panel shows BTCUSD trades in the period following announcement but before the actual disbursement of EIPs, i.e., between March 28 and April 9, 2020. The third panel shows BTCUSD trades in the period following the start of EIP disbursement, i.e., from April 10 to June 5, 2020. The dashed vertical lines show the modal EIP amount $1,200. Data are from Kaiko.

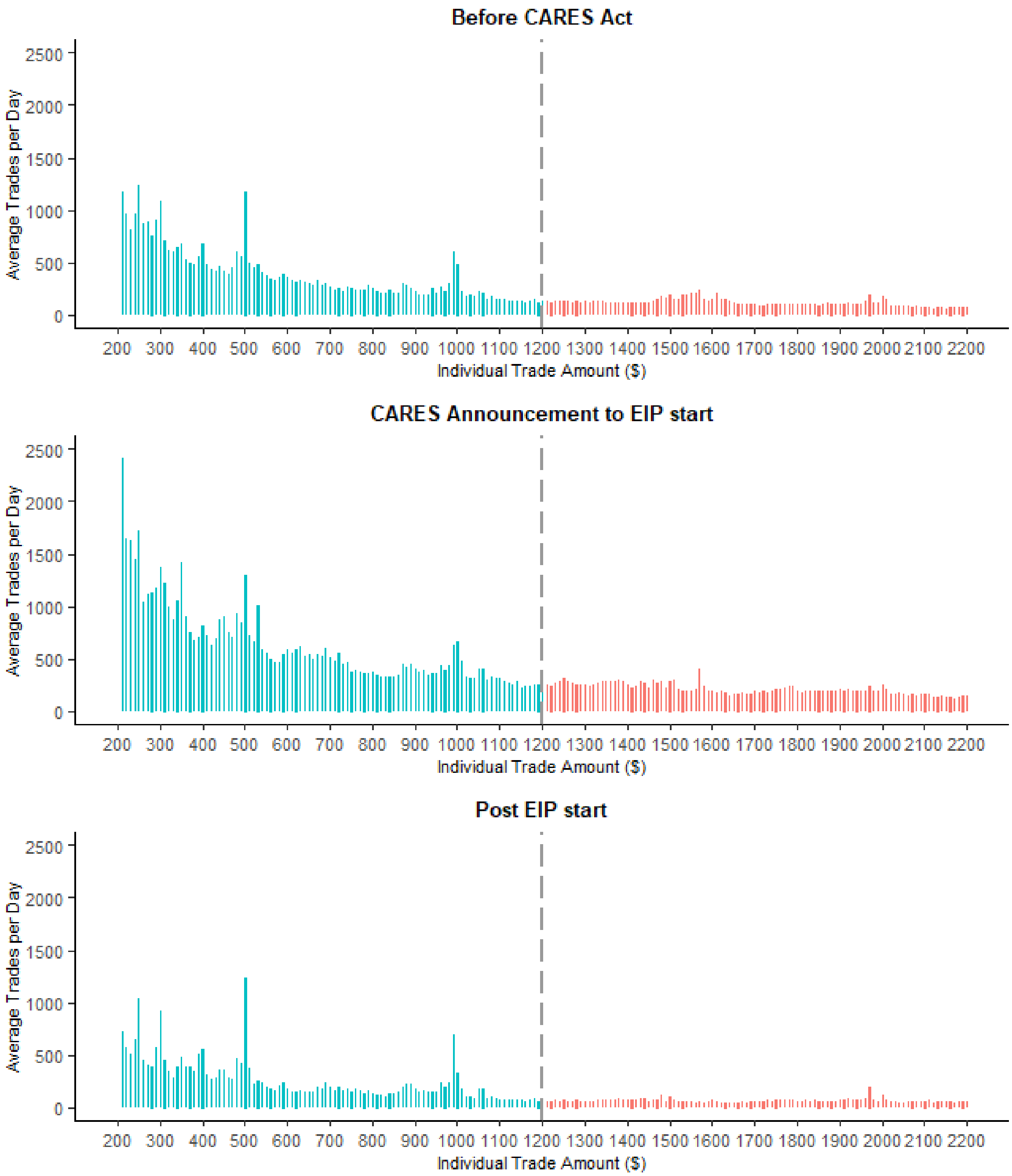

Figure 5: **Effects of economic impact payments on BTCUSD daily trade volume: \$1,200 buy trades**

Figure plots estimated treatment effects $\lambda_t$ of economic impact payments on Bitcoin buy trades using the event study specification outlined in Equation (1). We define $t = 0$ to be the first day of EIP disbursement, i.e., April 9, 2020, and estimate coefficients relative to the day before disbursement by setting $\lambda_{-1} = 0$. The outcome of interest is the number of Bitcoin buy trades in group $s$ on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades on that exchange and day. Group $s$ refers to either the *treated* group (i.e., trades with size in the range \$1,150–\$1,200) or the *control* group (trades with size in the range \$1,200–\$1,250). Only trades in USD at US-domiciled exchanges are included. The event window starts 24 days before EIP disbursement begins, and ends 24 days afterward, i.e., We fix $\lambda_t = 0$ for $t < -24$ and $t > 24$. The regression includes exchange and day fixed effects. Standard errors are clustered by date. Vertical lines represent 90% confidence intervals.

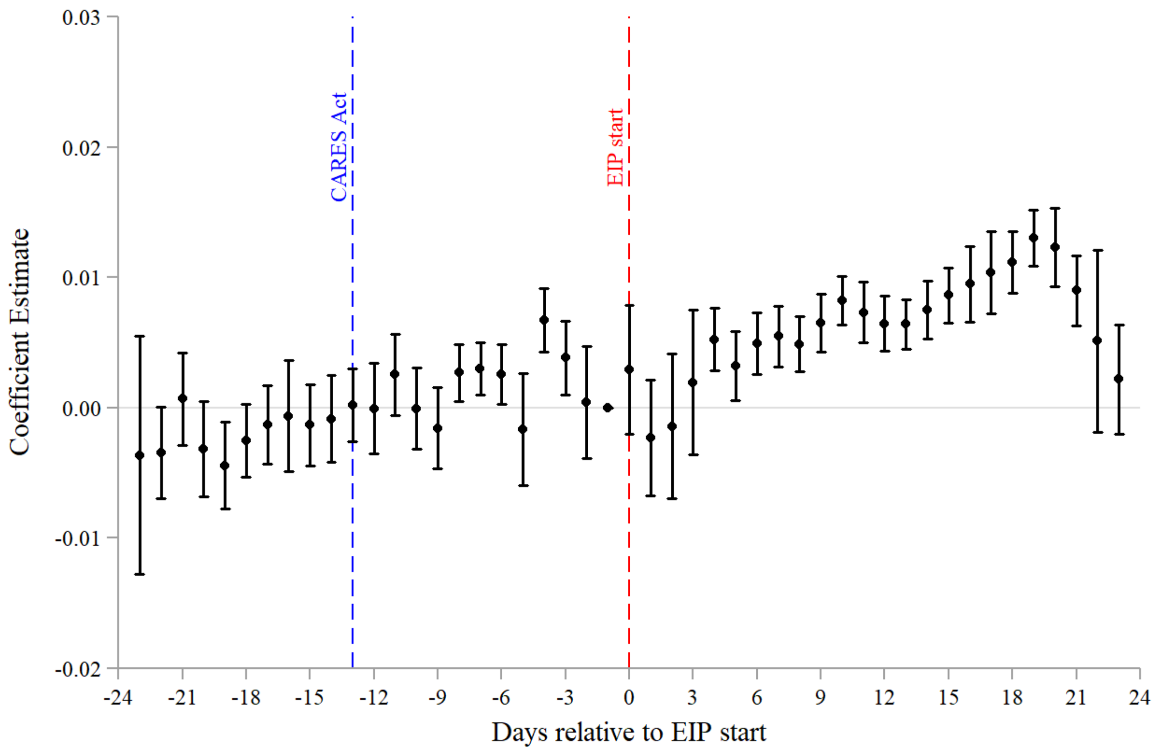

Figure 6: **Effects of Japan's Special Cash Payments program on BTCJPY daily trade volume: ¥100,000 buy trades**

Figure plots estimated treatment effects $\lambda_t$ of COVID-19 stimulus payments by the Japanese government on Bitcoin buy trades in Japanese yen (¥), using the event study specification outlined in Equation (1). We define $t = 0$ to be the start day of Japanese stimulus payments, i.e., April 27, 2020, and estimate coefficients relative to the day before disbursement by setting $\lambda_{-1} = 0$. The outcome of interest is the number of Bitcoin buy trades in group $s$ on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades on that exchange and day. Group $s$ refers to either the *treated* group (i.e., trades with size in the range ¥95,000–¥100,000) or the *control* group (trades with size in the range ¥100,000–¥105,000). Only trades in Japanese yen are included. The event window starts 18 days before Japanese stimulus payments begin, and ends 18 days afterward, i.e., we fix $\lambda_t = 0$ for $t < -18$ and $t > 18$. The regression includes exchange and day fixed effects. Standard errors are clustered by date. Vertical lines represent 90% confidence intervals.

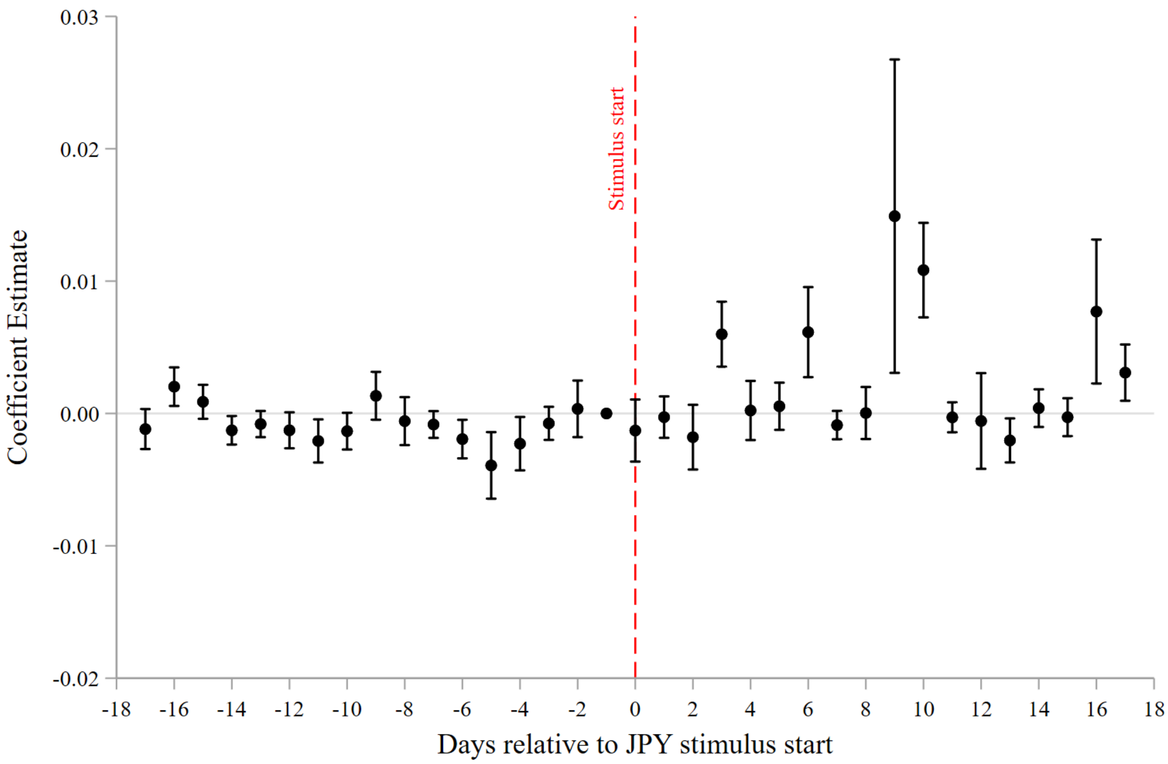

Figure 7: **Effects of Singapore's Solidarity Payment program on BTCSGD daily trade volume: SG$600 buy trades**

Figure plots estimated treatment effects $\lambda_t$ of COVID-19 stimulus payments by the Singaporean government on Bitcoin buy trades in Singapore dollars (SG$) using the event study specification outlined in Equation (1). We define $t = 0$ to be the start day of Singaporean stimulus payments, i.e., April 14, 2020, and estimate coefficients relative to the day before disbursement by setting $\lambda_{-1} = 0$. The outcome of interest is the number of Bitcoin buy trades in group $s$ on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades on that exchange and day. Group $s$ refers to either the *treated* group (i.e., trades with size in the range SG$570–SG$600) or the *control* group (trades with size in the range SG$600–SG$630). Only trades in Singapore dollars are included. The event window starts 18 days before Singaporean stimulus payments begin, and ends 18 days afterward. We fix $\lambda_t = 0$ for $t < -18$ and $t > 18$. The regression includes exchange and day fixed effects. Standard errors are clustered by date. Vertical lines represent 90% confidence intervals.

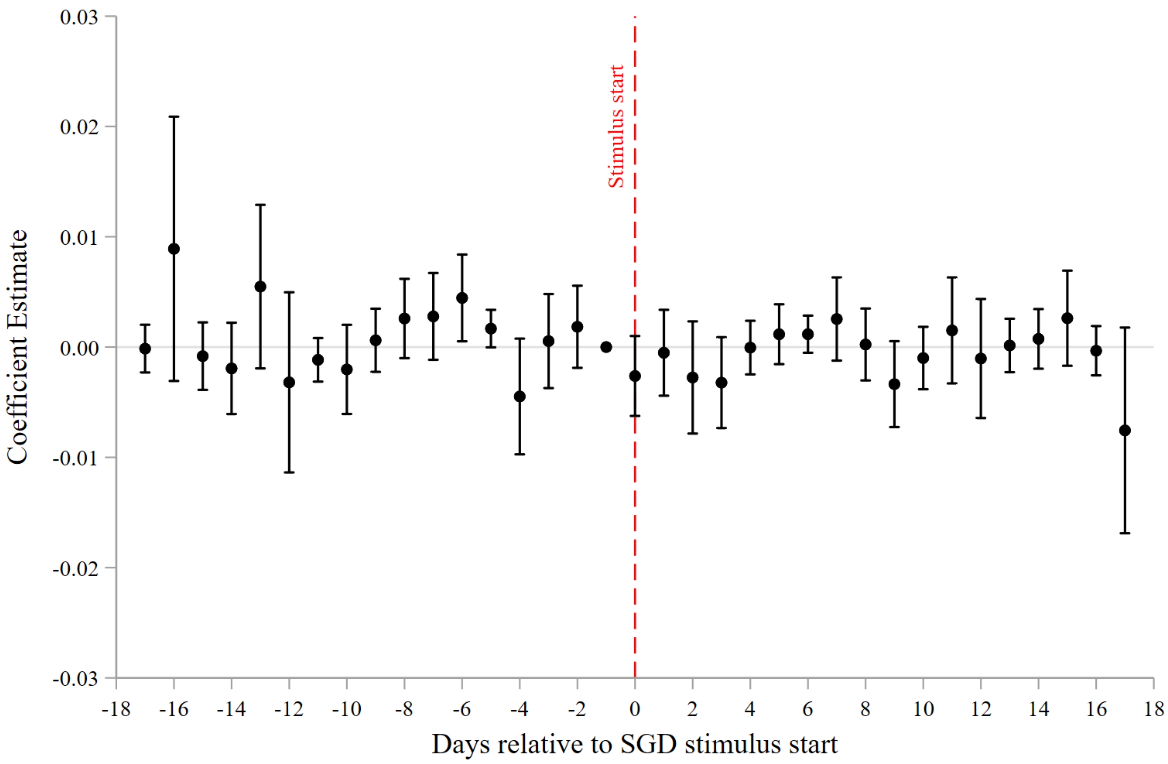

Figure 8: **Effects of South Korea's emergency disaster relief program on BTCKRW daily trade volume: ₩400,00 buy trades**

Figure plots estimated treatment effects $\lambda_t$ of COVID-19 stimulus payments by the South Korean government on Bitcoin buy trades in South Korean won (₩) using the event study specification outlined in Equation (1). We define $t = 0$ to be the start day of South Korean stimulus payments, i.e., May 4, 2020, and estimate coefficients relative to the day before disbursement by setting $\lambda_{-1} = 0$. The outcome of interest is the number of Bitcoin buy trades in group $s$ on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades on that exchange and day. Group $s$ refers to either the *treated* group (i.e., trades with size in the range ₩380,000–₩400,000) or the *control* group (trades with size in the range ₩400,000–₩420,000). Only trades in South Korean won are included. The event window starts 18 days before South Korean stimulus payments begin, and ends 18 days afterward. We fix $\lambda_t = 0$ for $t < -18$ and $t > 18$. The regression includes exchange and day fixed effects. Standard errors are clustered by date. Vertical lines represent 90% confidence intervals.

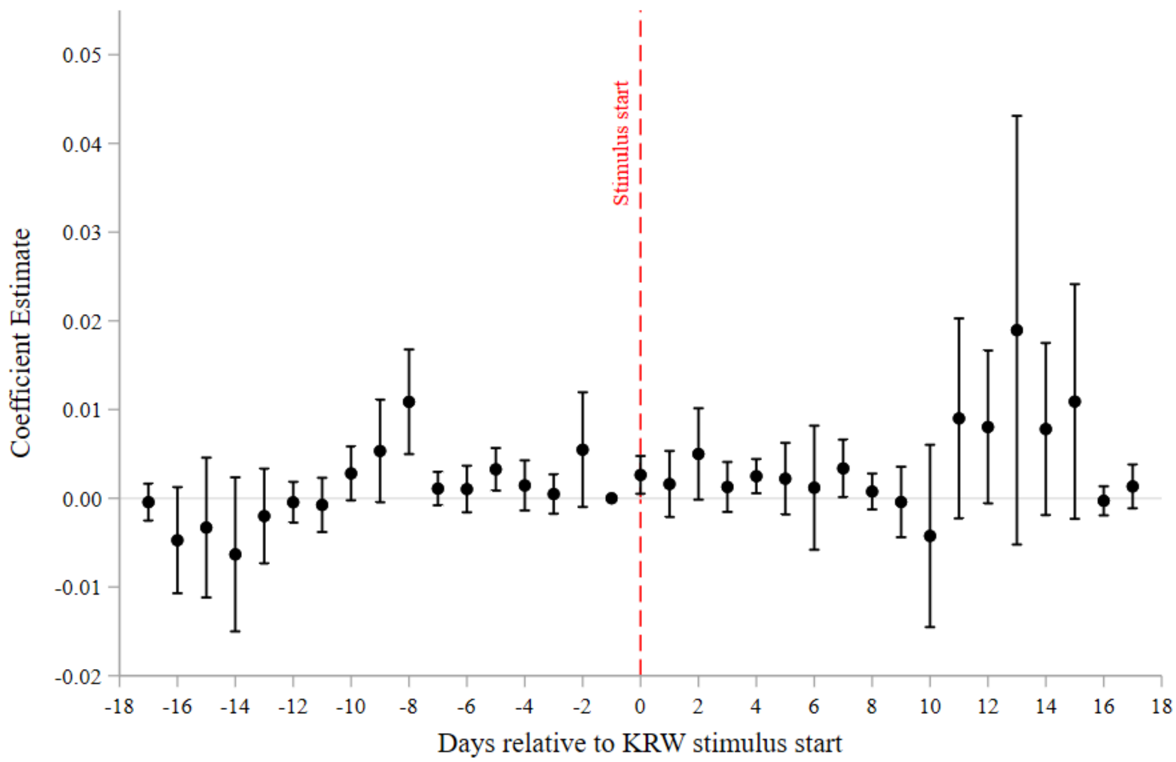

Table 1: **Calculation of economic impact payments**
Source: US Internal Revenue Service (IRS). More details can be found at https://tinyurl.com/2pmvwa3n. "Single" refers to a household comprising a single adult and no children. "Head of household" is a single adult caring living with a dependent. "Couple" refers to households with at least two adults (married or cohabiting). "Children" are defined as own children of the householder, living in the household, and under 18 years old. For every dollar of household income above the amounts given in the second column, payments were reduced by 5 cents.

| | Full payment | Max income for full payment | Min income for zero payment |
|---|---|---|---|
| Single | $1,200 | $75,000 | $99,000 |
| Head of household | $1,200 | $112,500 | $136,500 |
| Couple with joint return | $2,400 | $150,000 | $198,000 |
| Each qualifying child | +$500 | +$10,000 | +$10,000 |

Table 2: **Bitcoin exchanges: descriptive statistics**
Data refer to Bitcoin buy trades between January 1 and June 5, 2020. Trade values are expressed in US$ at the prevailing exchange rates. Data on Bitcoin trades are obtained from Kaiko. Other information is from exchanges' own websites.

| Exchange | Domicile | Trade USD | USD Trading | | All Currencies Trading | |
|---|---|---|---|---|---|---|
| | | | Volume (1000s) | Volume $m | Volume (1000s) | Volume $m |
| Binance | Malta | No | 0 | 0 | 237 | 52 |
| BinanceUS | US | Yes | 634 | 327 | 634 | 327 |
| Bitbank | Japan | No | 0 | 0 | 1,594 | 1,253 |
| BitBay | Poland | Yes | 8 | 2 | 45 | 63 |
| Bitfinex | H Kong | Yes | 5,181 | 6,221 | 6,799 | 7,051 |
| bitFlyer | Japan | Yes | 124 | 113 | 3,165 | 5,032 |
| Bithumb | S Korea | No | 0 | 0 | 4,317 | 3,763 |
| Bitlish | UK | Yes | 8 | 25 | 31 | 45 |
| Bitstamp | Lux'burg | Yes | 1,716 | 6,589 | 2,848 | 8,657 |
| Bittrex | US | Yes | 856 | 659 | 865 | 665 |
| BTC-Alpha | UK | Yes | 1,784 | 943 | 1,784 | 943 |
| Btcbox | Japan | No | 0 | 0 | 1,204 | 309 |
| CEX.IO | UK | Yes | 231 | 169 | 360 | 237 |
| Coinbase | US | Yes | 8,527 | 11,409 | 11,855 | 14,063 |
| Coincheck | Japan | No | 0 | 0 | 3,165 | 1,274 |
| Coinone | S Korea | No | 0 | 0 | 1,753 | 2,036 |
| Gemini | US | Yes | 736 | 1,441 | 736 | 1,441 |
| Kraken | US | Yes | 1,893 | 5,070 | 4,831 | 10,073 |
| LGOMarkets | US | Yes | 71 | 241 | 71 | 241 |
| LMAX | UK | Yes | 607 | 3,010 | 646 | 3,128 |
| OkCoin | US | Yes | 388 | 593 | 396 | 599 |
| Quoine | Japan | Yes | 441 | 469 | 38,435 | 18,109 |
| TheRockTrading | Italy | No | 0 | 0 | 64 | 34 |
| TideBit | H Kong | Yes | 233 | 304 | 244 | 327 |
| UPbit | S Korea | No | 0 | 0 | 4,757 | 4,455 |
| Zaif | Japan | No | 0 | 0 | 2,384 | 779 |

Table 3: **Fiat currencies traded on Bitcoin exchanges**

Table shows Bitcoin buy trades between January 1 and June 5, 2020. Values are expressed in US$ at the prevailing exchange rates. "Program currency" means the issuing government ran a scheme similar to the US economic impact payments; i.e., a COVID-19 related economic stimulus program in which the majority of households received direct one-off payments during this period.

| Currency | Volume | | Program currency |
|---|---|---|---|
| | $m | Trades (1000s) | |
| US dollar | 37,021 | 23,437 | Yes |
| Japanese yen | 24,016 | 49,495 | Yes |
| Euro | 9,151 | 8,071 | No |
| Korean won | 7,623 | 10,827 | Yes |
| Polish zloty | 1,909 | 531 | No |
| British pound | 824 | 1,337 | No |
| Turkish lira | 738 | 911 | No |
| Mexican peso | 182 | 459 | No |
| Singapore dollar | 58 | 38 | Yes |
| Canadian dollar | 57 | 76 | No |
| HK dollar | 23 | 15 | No |
| Russian rouble | 17 | 239 | No |
| Swiss franc | 14 | 23 | No |
| Australian dollar | 8 | 19 | No |
| Israeli shekel | 7 | 8 | No |

Table 4: **Effect of economic impact payments on BTCUSD trade volume: $1,200 buy trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that do not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). The dummy *treated* is equal to one for treated trades (between $1,150 and $1,200 in size) and zero for control trades (between $1,200 and $1,250). The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | | |
|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) |
| *Variables* | | | | |
| announced | -0.2321** | -0.2702*** | -0.2849*** | 0.1156 |
| | (0.0909) | (0.0740) | (0.0747) | (0.1066) |
| disbursed | -0.3917*** | -0.1036 | -0.0863 | 0.0538 |
| | (0.0565) | (0.0885) | (0.0930) | (0.1299) |
| treated | 0.4337*** | 0.4356*** | -0.0052 | -0.0042 |
| | (0.0667) | (0.0700) | (0.0528) | (0.0510) |
| announced × treated | -0.0132 | 0.0016 | -0.1264 | -0.1331 |
| | (0.0971) | (0.0977) | (0.0968) | (0.0946) |
| disbursed × treated | 0.4949*** | 0.4876*** | 0.4841*** | 0.4733*** |
| | (0.0791) | (0.0829) | (0.0774) | (0.0701) |
| *Fixed effects* | | | | |
| date | No | Yes | Yes | Yes |
| currency | No | No | Yes | Yes |
| exchange | No | No | No | Yes |
| *Fit statistics* | | | | |
| Observations | 5,355 | 5,355 | 5,355 | 5,355 |
| $R^2$ | 0.015 | 0.058 | 0.112 | 0.197 |

Table 5: **Summary statistics for retail ratio**

Table shows summary statistics for *retail ratio*, defined as the logarithm of the ratio of number of Bitcoin buy trades under $5,000 to those under $1m. The ratio is computed daily on each exchange, and summary statistics are computed across time. The sample comprises Bitcoin buy trades in USD prior to EIP announcement between January 1 to March 26, 2020.

| *Exchange* | Min | Max | Median | Mean | St dev |
|---|---|---|---|---|---|
| BinanceUS | -0.05 | 0 | -0.02 | -0.02 | 0.01 |
| BitBay | -0.22 | 0 | 0 | -0.01 | 0.03 |
| Bitfinex | -0.14 | -0.01 | -0.06 | -0.06 | 0.03 |
| bitFlyer | -0.21 | 0 | -0.03 | -0.04 | 0.03 |
| Bitlish | -2.71 | 0 | 0 | -0.27 | 0.58 |
| Bitstamp | -0.37 | -0.08 | -0.19 | -0.19 | 0.05 |
| Bittrex | -0.05 | 0 | -0.02 | -0.02 | 0.01 |
| BTC-Alpha | -0.09 | 0 | -0.01 | -0.02 | 0.02 |
| CEX.IO | -0.07 | 0 | -0.02 | -0.02 | 0.01 |
| Coinbase | -0.11 | -0.02 | -0.06 | -0.06 | 0.02 |
| Gemini | -0.18 | -0.03 | -0.09 | -0.09 | 0.03 |
| Kraken | -0.22 | -0.06 | -0.15 | -0.15 | 0.03 |
| LGOMarkets | -0.97 | 0 | -0.18 | -0.21 | 0.13 |
| LMAX | -0.66 | -0.18 | -0.37 | -0.37 | 0.10 |
| OkCoin | -0.15 | 0 | -0.06 | -0.06 | 0.03 |
| Quoine | -0.09 | -0.01 | -0.05 | -0.05 | 0.02 |
| TideBit | -1.56 | 0 | 0 | -0.11 | 0.33 |

Table 6: **Effect of professionalism of exchange user base on EIP effect: $1,200 buy trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that do not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). The dummy *treated* is equal to one for treated trades (between $1,150 and $1,200 in size) and zero for control trades (between $1,200 and $1,250). The scalar variable *retail* is the logarithm of ratio of number of Bitcoin buy trades under $5,000 to those under $1m, for a given currency, exchange and day. The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | | |
|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) |
| *Variables* | | | | |
| announced | 2.297 | 2.074 | 2.077 | 1.586 |
| | (1.682) | (1.451) | (1.453) | (1.033) |
| disbursed | -0.3767*** | -0.1938 | -0.1562 | -0.1404 |
| | (0.0705) | (0.1343) | (0.1369) | (0.1541) |
| treated | 0.4656*** | 0.4772*** | -0.0647 | -0.0341 |
| | (0.0890) | (0.0920) | (0.0694) | (0.0679) |
| retail | -0.0758 | -0.1924 | -0.2217 | 1.027*** |
| | (0.1506) | (0.1715) | (0.1716) | (0.2588) |
| announced × treated | -2.709 | -2.474* | -2.645* | -1.883* |
| | (1.703) | (1.443) | (1.447) | (0.9566) |
| disbursed × treated | 0.6004*** | 0.6083*** | 0.6105*** | 0.5462*** |
| | (0.1155) | (0.1208) | (0.1118) | (0.1135) |
| announced × retail | 41.63 | 35.28 | 35.11 | 18.26 |
| | (41.49) | (30.7) | (30.57) | (16.57) |
| disbursed × retail | 0.2152 | 0.3928 | 0.3416 | -0.0274 |
| | (0.4937) | (0.5949) | (0.5900) | (0.6605) |
| treated × retail | 0.6895 | 0.7960 | -0.1329 | 0.0861 |
| | (0.4829) | (0.5171) | (0.4339) | (0.4928) |
| announced × treated × retail | -44.34 | -37.89 | -39.8 | -23.64 |
| | (41.75) | (31.47) | (31.34) | (16.89) |
| disbursed × treated × retail | 2.11** | 2.328** | 2.05** | 1.913* |
| | (0.9019) | (0.9902) | (0.8615) | (1.024) |
| *Fixed effects* | | | | |
| date | No | Yes | Yes | Yes |
| currency | No | No | Yes | Yes |
| exchange | No | No | No | Yes |
| *Fit statistics* | | | | |
| Observations | 5,727 | 5,727 | 5,727 | 5,727 |
| $R^2$ | 0.040 | 0.143 | 0.155 | 0.319 |

Table 7: **Effect of economic impact payments on BTCUSD trade volume: $1,000 buy trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that do not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). The dummy *treated* is equal to one for treated trades (between $950 and $1,000 in size) and zero for control trades (between $1,000 and $1,050). The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | | |
|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) |
| *Variables* | | | | |
| announced | -0.5359*** | -0.3627*** | -0.3623*** | -0.1011 |
| | (0.0918) | (0.0881) | (0.0876) | (0.1140) |
| disbursed | -0.1438* | 0.0822 | 0.0842 | -0.1113 |
| | (0.0760) | (0.0977) | (0.0965) | (0.1186) |
| treated | 0.5833*** | 0.5824*** | 0.5690*** | 0.5534*** |
| | (0.0581) | (0.0584) | (0.0634) | (0.0627) |
| announced × treated | -0.0339 | -0.0282 | -0.0308 | -0.0162 |
| | (0.1029) | (0.1016) | (0.1066) | (0.1023) |
| disbursed × treated | 0.1832** | 0.1769** | 0.1778** | 0.1981** |
| | (0.0853) | (0.0863) | (0.0877) | (0.0845) |
| *Fixed effects* | | | | |
| date | No | Yes | Yes | Yes |
| currency | No | No | Yes | Yes |
| exchange | No | No | No | Yes |
| *Fit statistics* | | | | |
| Observations | 6,161 | 6,161 | 6,161 | 6,161 |
| $R^2$ | 0.035 | 0.109 | 0.109 | 0.282 |

Table 8: **Effect of economic impact payments on BTCUSD trade volume: other round number trade sizes**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that do not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. We consider three different cutoffs at \$100, \$500 (to test round number preference), and \$600 (representing half the modal \$1,200 EIP). The bandwidth is set to 5% of the cutoff value in each case. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). The dummy *treated* is equal to one for treated trades (i.e., trades for amounts lower than the cutoff by a quantity up to the bandwidth value) and zero for control trades (i.e., trades for amounts greater than the cutoff by a quantity up to the bandwidth value). The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | |
|---|---|---|---|
| Cutoff: | \$100 | \$500 | \$600 |
| Bandwidth: | \$5 | \$25 | \$30 |
| *Variables* | | | |
| announced | -0.5364*** | -0.1430 | 0.1934 |
| | (0.1276) | (0.1298) | (0.1250) |
| disbursed | -0.6532*** | -0.0867 | -0.1418 |
| | (0.1014) | (0.0857) | (0.1185) |
| treated | 0.7408*** | 0.3798*** | 0.1257** |
| | (0.0828) | (0.0395) | (0.0550) |
| announced × treated | 0.3998*** | -0.0499 | -0.0570 |
| | (0.1267) | (0.0957) | (0.0950) |
| disbursed × treated | 0.9649*** | 0.1880*** | 0.4267*** |
| | (0.1161) | (0.0633) | (0.0548) |
| *Fixed effects* | | | |
| date | Yes | Yes | Yes |
| currency | Yes | Yes | Yes |
| exchange | Yes | Yes | Yes |
| *Fit statistics* | | | |
| Observations | 6,080 | 6,115 | 5,981 |
| $R^2$ | 0.391 | 0.166 | 0.243 |

Table 9: **Estimates of dollar impact of economic impact payments on Bitcoin trade sizes**
The coefficients $\delta$ are those obtained from logistic regression with full fixed effects and a bandwidth equal to 5% of the trade size (i.e., cutoff). Trading volumes and values relate to the EIP disbursement period, April 9 to June 5, 2020, and are obtained directly from the Kaiko data. We estimate a proportion $1-e^{-\delta}$ of these trades are financed by EIPs. See Equations (3) and (4).

| | **Trade size** | | | | |
|---|---|---|---|---|---|
| | $1,200 | $1,000 | $600 | $500 | $100 |
| Estimated $\delta$ | 0.4733 | 0.1981 | 0.4267 | 0.1880 | 0.9649 |
| Proportion of trades due to EIPs ($=1-e^{-\delta}$) | 0.3771 | 0.1797 | 0.3474 | 0.1791 | 0.2601 |
| Total number of trades in treated group | 19,781 | 95,389 | 35,194 | 112,804 | 264,342 |
| Total value of trades $m in treated group | 23.25 | 93.28 | 20.59 | 55.16 | 25.63 |
| Est. number due to EIPs | 7,459 | 17,143 | 12,224 | 19,333 | 163,622 |
| Est. value due to EIPs $m | 8.77 | 16.76 | 7.15 | 9.45 | 15.86 |

Table 10: **Effect of economic impact payments on Bitcoin price: $1,200 buy trades**
Table presents difference-in-differences estimates of the effect of EIPs on Bitcoin price based on the specification outlined in Equation (5). The dependent variable is the logarithm of the mean execution price for all Bitcoin buy trades in USD within exchange $j$ at hour $t$. The sample comprises Bitcoin buy trades in USD between January 1 to June 5, 2020. The dummy *announced* is equal to 1 iff the CARES Act is announced by hour $t$ and EIP disbursement has not yet started (i.e., between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out at hour $t$ (i.e., April 9 or later). The scalar variable *pct_treated* is the number of BTCUSD buy trades for treated amounts (between $1,150 and $1,200) on exchange $j$ at hour $t$, expressed as a proportion of the total number of BTCUSD buy trades for that same exchange and time. The regressions include hourly, daily, weekly, and exchange fixed-effects. Standard errors are clustered by hour and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

| Dependent Variable: | Logarithm of mean price | | | | |
|---|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) | (5) |
| *Variables* | | | | | |
| announced | -0.1226*** | -0.1792*** | -0.0001 | -0.0002 | -0.0066*** |
| | (0.0039) | (0.0039) | (0.0001) | (0.0002) | (0.0009) |
| disbursed | 0.0364*** | 0.0398*** | 0.0005*** | 0.0000 | -0.0045*** |
| | (0.0042) | (0.0049) | (0.0000) | (0.0001) | (0.0005) |
| pct_treated | -0.3300*** | -0.2877*** | -0.0081*** | -0.0466*** | -0.0094 |
| | (0.1163) | (0.1075) | (0.0015) | (0.0172) | (0.0251) |
| announced | -0.9715 | -0.9075 | 0.0012 | -0.0208 | 0.0417 |
| × pct_treated | (0.6716) | (0.6869) | (0.0032) | (0.0500) | (0.0518) |
| disbursed | 0.4301** | 0.4002** | 0.0117*** | 0.0592*** | 0.0812* |
| × pct_treated | (0.1961) | (0.1894) | (0.0024) | (0.0217) | (0.0474) |
| *Fixed-effects* | | | | | |
| exchange | No | Yes | Yes | Yes | Yes |
| time: hourly | No | No | Yes | No | No |
| time: daily | No | No | No | Yes | No |
| time: weekly | No | No | No | No | Yes |
| *Fit statistics* | | | | | |
| Observations | 53,991 | 53,991 | 53,991 | 53,991 | 53,991 |
| $R^2$ | 0.068 | 0.105 | 0.999 | 0.987 | 0.917 |

Table 11: **Summary of direct payment programs in response to COVID-19 around the world**

This table summarizes all schemes where a sovereign government has made direct payments to households in its country with minimal eligibility conditions, in response to the COVID-19 crisis. "Announcement" is date when scheme is first announced by government or passed in legislation. "Disbursement" is date of first payment. All dates are 2020 unless otherwise stated. We convert to US dollars using exchange rates on respective disbursement dates. Amounts are those paid to a single recipient with no children, and an income low enough to qualify for the full payment amount. We exclude schemes that do not pay money directly to the majority of the country's citizens. Hong Kong, Israel, Serbia, and US rounds 2 and 3 are not used in our analysis, but are included in this table for information. List last checked on June 30, 2021.

| **Country** | **Date** | | **Amount** | |
|---|---|---|---|---|
| | Announcement | Disbursement | Local currency | US dollars |
| US, 1st round | Mar 27 | Apr 9 | $1,200 | $1,200 |
| Japan | Apr 16 | Apr 27 | ¥100,000 | $933 |
| Singapore | Feb 18 | Apr 14 | SG$600 | $424 |
| South Korea | Mar 30 | May 4 | ₩400,000 | $326 |
| Hong Kong | Feb 26 | Jul 8 | HK$10,000 | $1,290 |
| Israel | Jul 29 | Early Aug | NIS 750 | $220 |
| Serbia | Mar 29 | May 15 | RSD 11,759 | $108 |
| US, 2nd round | Dec 27 | Dec 29 | $600 | $600 |
| US, 3rd round | Mar 11, 2021 | Mar 17, 2021 | $1,400 | $1,400 |

Table 12: **Effect of Japan's Special Cash Payments program on BTCJPY trade volume: ¥100,000 buy trades**

Table presents difference-in-differences GLM estimates of the effect of COVID-19 stimulus payments by the Japanese government on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in Japanese yen and non-program currencies, that is, currencies issued by governments that do not run EIP-type programs. Trades in non-program currencies are converted to the equivalent JPY amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the Japanese stimulus program is announced by day $t$ and payment has not yet started (i.e., the phase between April 16 to April 26). The dummy *disbursed* is equal to 1 iff Japanese stimulus payments are being paid out on day $t$ (i.e., April 27 or later). The dummy *treated* is equal to one for treated trades (between ¥95,000 and ¥100,000 in size) and zero for control trades (between ¥100,000 and ¥105,000). The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | | | |
|---|---|---|---|---|---|
| Bandwidth ($h$): | ¥500 | ¥1,000 | ¥1,250 | ¥2,000 | ¥5,000 |
| *Variables* | | | | | |
| announced | -0.0350 | -0.0784 | -0.0916 | -0.2304** | -0.2707** |
| | (0.1261) | (0.1101) | (0.1029) | (0.1153) | (0.1307) |
| disbursed | 0.5740*** | 0.3485*** | 0.3619*** | 0.2571 | 0.3039* |
| | (0.1572) | (0.1290) | (0.1258) | (0.1647) | (0.1693) |
| treated | 0.0379 | -0.1107 | -0.1305 | -0.1410 | 0.1462 |
| | (0.0747) | (0.0893) | (0.0928) | (0.1078) | (0.1030) |
| announced × treated | -0.0381 | 0.0747 | 0.1208 | 0.2917** | 0.1909 |
| | (0.1381) | (0.1483) | (0.1381) | (0.1426) | (0.1171) |
| disbursed × treated | 0.2731** | 0.3280** | 0.3817** | 0.2552 | -0.1781 |
| | (0.1184) | (0.1417) | (0.1564) | (0.1753) | (0.1867) |
| *Fit statistics* | | | | | |
| Observations | 3,287 | 3,826 | 3,996 | 4,378 | 5,306 |
| $R^2$ | 0.603 | 0.428 | 0.410 | 0.392 | 0.245 |

Table 13: **Effect of Singapore's Solidarity Payment program on BTCSGD trade volume: SG$600 buy trades**

Table presents difference-in-differences GLM estimates of the effect of COVID-19 stimulus payments by the Singaporean government on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in Singapore dollars and non-program currencies, that is, currencies issued by governments that do not run EIP-type programs. Trades in non-program currencies are converted to the equivalent SGD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the Singaporean stimulus program is announced by day $t$ and payment has not yet started (i.e., the phase between February 18 to April 13). The dummy *disbursed* is equal to 1 iff Singaporean stimulus payments are being paid out on day $t$ (i.e., April 14 or later). The dummy *treated* is equal to one for treated trades (between SG$570 and SG$600 in size) and zero for control trades (between SG$600 and SG$630). The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | | | |
|---|---|---|---|---|---|
| Bandwidth ($h$): | SG$30 | SG$42 | SG$60 | SG$90 | SG$120 |
| *Variables* | | | | | |
| announced | 0.3066 | 0.3096 | 0.2256 | 0.0199 | 0.0314 |
| | (0.2439) | (0.1880) | (0.1365) | (0.0989) | (0.0987) |
| disbursed | 0.5634*** | 0.5146*** | 0.4138*** | 0.1669* | 0.2044** |
| | (0.2113) | (0.1637) | (0.1294) | (0.0919) | (0.0924) |
| treated | 0.2102** | 0.1364* | 0.0718 | 0.0069 | 0.0687 |
| | (0.1042) | (0.0824) | (0.0695) | (0.0527) | (0.0454) |
| announced × treated | -0.1175 | -0.0602 | -0.0287 | 0.0785 | 0.1129 |
| | (0.1573) | (0.1324) | (0.1098) | (0.0902) | (0.0959) |
| disbursed × treated | -0.3137** | -0.1692* | -0.0106 | 0.1633** | 0.1108* |
| | (0.1271) | (0.0990) | (0.0863) | (0.0663) | (0.0641) |
| *Fit statistics* | | | | | |
| Observations | 3,681 | 3,937 | 4,241 | 4,504 | 4,668 |
| $R^2$ | 0.408 | 0.309 | 0.202 | 0.151 | 0.133 |

Table 14: **Effects of South Korea's emergency disaster relief program on BTCKRW trade volume: ₩400,000 buy trades**

Table presents difference-in-differences GLM estimates of the effect of COVID-19 stimulus payments by the South Korean government on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in KRW and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent KRW amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the South Korean stimulus program is announced by day $t$ and payment has not yet started (i.e., the phase between March 30 to May 4). The dummy *disbursed* is equal to 1 iff South Korean stimulus payments are being paid out on day $t$ (i.e., May 5 or later). The dummy *treated* is equal to one for treated trades (between ₩380,000 and ₩400,000 in size) and zero for control trades (between ₩400,000 and ₩420,000). The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | | | |
|---|---|---|---|---|---|
| Bandwidth ($h$): | ₩5,000 | ₩8,000 | ₩20,000 | ₩28,000 | ₩40,000 |
| *Variables* | | | | | |
| announced | -0.1574 | -0.1044 | 0.1479 | 0.2194** | 0.2035** |
| | (0.1260) | (0.1186) | (0.0952) | (0.0921) | (0.0845) |
| disbursed | -0.0814 | -0.0680 | 0.0267 | -0.0287 | -0.0699 |
| | (0.1679) | (0.1465) | (0.0999) | (0.0872) | (0.0724) |
| treated | 0.0725 | 0.0207 | -0.0145 | -0.0117 | 0.0529 |
| | (0.0673) | (0.0636) | (0.0642) | (0.0569) | (0.0484) |
| announced × treated | 0.0808 | 0.0399 | -0.1241 | -0.2273** | -0.2461*** |
| | (0.0759) | (0.0900) | (0.1111) | (0.1057) | (0.0895) |
| disbursed × treated | 0.1734 | 0.4045*** | 0.4275*** | 0.3913*** | 0.2205*** |
| | (0.1154) | (0.1235) | (0.0988) | (0.0805) | (0.0666) |
| *Fit statistics* | | | | | |
| Observations | 2,562 | 2,849 | 3,345 | 3,526 | 3,677 |
| $R^2$ | 0.345 | 0.367 | 0.254 | 0.197 | 0.187 |

Table 15: **Impact of economic impact payments on BTCUSD sell trade volume: \$1,200 sell trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin sell trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin sell trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of sell trades for that same currency, exchange, and day. The sample comprises Bitcoin sell trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). The dummy *treated* is equal to one for treated trades (between \$1,150 and \$1,200 in size) and zero for control trades (between \$1,200 and \$1,250). The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | | |
|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) |
| *Variables* | | | | |
| announced | -0.2500*** | -0.2829*** | -0.3257*** | 0.1325* |
| | (0.0942) | (0.0704) | (0.0743) | (0.0798) |
| disbursed | -0.1610*** | 0.1204 | 0.1369* | 0.2309** |
| | (0.0555) | (0.0751) | (0.0796) | (0.1119) |
| treated | 0.0016 | 0.0008 | -0.0004 | -0.0050 |
| | (0.0649) | (0.0654) | (0.0652) | (0.0643) |
| announced × treated | -0.1475* | -0.1337 | -0.1288 | -0.1375* |
| | (0.0848) | (0.0834) | (0.0823) | (0.0794) |
| disbursed × treated | -0.0230 | -0.0167 | -0.0101 | 0.0024 |
| | (0.0753) | (0.0750) | (0.0743) | (0.0729) |
| *Fixed effects* | | | | |
| date | No | Yes | Yes | Yes |
| currency | No | No | Yes | Yes |
| exchange | No | No | No | Yes |
| *Fit statistics* | | | | |
| Observations | 7,156 | 7,156 | 7,156 | 7,156 |
| $R^2$ | 0.003 | 0.036 | 0.046 | 0.124 |

# ONLINE APPENDIX

Figure A.1: **Google searches for the term "Bitcoin" in the US**
Data are obtained from Google search trends, and span the period Nov 1, 2019 to Oct 30, 2020. The chart shows a relative weekly measure of Google searches of the term "Bitcoin", with the peak of 100 on the week beginning May 10, 2020.

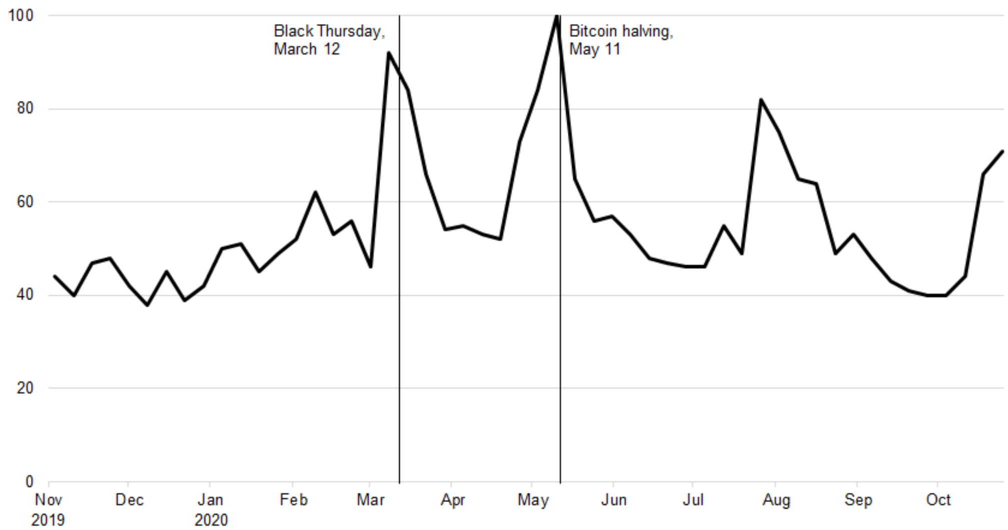


Table A.1: **Household sizes in the United States**
"Single" refers to households comprising a single adult person and no children. "Couple" refers to households with at least two adult persons (married or cohabiting). "Children" are defined as own children of the householder, under 18 years old. The data come from the 2019 American Community Survey. More details can be found at: https://data.census.gov/cedsci/

| Household makeup | Frequency |
|---|---|
| Single with no children | 46,995,583 |
| Single with one or more children | 7,989,572 |
| Couple with no children | 40,442,821 |
| Couple with one or more children | 25,348,072 |

Table A.2: **Robustness of results to changes in bandwidth: $1,200 buy trades**
Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). The dummy *treated* is equal to one for treated trades (i.e., trades for amounts lower than the cutoff by a quantity up to the bandwidth value) and zero for control trades (i.e., trades for amounts greater than the cutoff by a quantity up to the bandwidth value). We use various bandwidths, ranging from $12.50 to $100, around the $1,200 cutoff. The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. For brevity, we show only the coefficients of the interactions of the *treated* dummy with *announced* and *disbursed*, as these are the results of interest.

| Dependent Variable: | Log-odds of relative daily trade volume | | | | | | |
|---|---|---|---|---|---|---|---|
| Bandwidth: | $12.50 | $25 | $37.50 | $67.50 | $75 | $87.50 | $100 |
| *Variables* | | | | | | | |
| announced | 0.0521 | -0.0996 | -0.1656* | -0.8713* | -0.7905* | -0.7397* | -0.6447* |
| × treated | (0.1158) | (0.0860) | (0.0887) | (0.4549) | (0.4208) | (0.3941) | (0.3718) |
| disbursed | 0.5248*** | 0.5101*** | 0.4467*** | 0.3498*** | 0.3225*** | 0.2529*** | 0.2508*** |
| × treated | (0.0913) | (0.0708) | (0.0710) | (0.0682) | (0.0725) | (0.0652) | (0.0659) |
| *Fixed effects* | | | | | | | |
| date | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| currency | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| exchange | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| *Fit statistics* | | | | | | | |
| Observations | 4,603 | 5,203 | 5,526 | 5,864 | 5,977 | 6,075 | 6,254 |
| $R^2$ | 0.286 | 0.209 | 0.248 | 0.204 | 0.194 | 0.191 | 0.193 |

Table A.3: **Impact of economic impact payments to couples with no children on BTCUSD trade volume: $2,400 buy trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). We use a cutoff of $2,400, the likely modal amount paid to couples without children, and a bandwidth equal to 5% of the cutoff amount. Thus, the dummy *treated* is equal to one for treated trades (between $2,280 and $2,400 in size) and zero for control trades (between $2,400 and $2,520). The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | | |
|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) |
| *Variables* | | | | |
| announced | 0.3753 | 0.5810 | 0.5810 | 0.7048 |
| | (0.5179) | (0.5667) | (0.5667) | (0.6092) |
| disbursed | 0.1219* | 0.1657 | 0.1657 | 0.0465 |
| | (0.0692) | (0.1437) | (0.1437) | (0.1447) |
| treated | 0.1721 | 0.1748 | 0.1748 | 0.1348 |
| | (0.1141) | (0.1139) | (0.1139) | (0.0989) |
| announced × treated | -1.0300* | -1.0030* | -1.0030* | -0.7956** |
| | (0.5363) | (0.5109) | (0.5109) | (0.3108) |
| disbursed × treated | -0.2768** | -0.2811** | -0.2811** | -0.2261** |
| | (0.1265) | (0.1264) | (0.1264) | (0.1119) |
| *Fixed effects* | | | | |
| date | No | Yes | Yes | Yes |
| currency | No | No | Yes | Yes |
| exchange | No | No | No | Yes |
| *Fit statistics* | | | | |
| Observations | 4,098 | 4,098 | 4,098 | 4,098 |
| $R^2$ | 0.002 | 0.056 | 0.056 | 0.484 |

Table A.4: **Impact of economic impact payments to couples with one child on BTCUSD trade volume: $2,900 buy trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). We use a cutoff of $2,900, the likely modal amount paid to couples with one child, and a bandwidth equal to 5% of the cutoff amount. Thus, the dummy *treated* is equal to one for treated trades (between $2,755 and $2,900 in size) and zero for control trades (between $2,900 and $3,045). The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | | |
|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) |
| *Variables* | | | | |
| announced | -0.3954*** | -0.4053*** | -0.4053*** | -0.2473** |
| | (0.0691) | (0.0705) | (0.0705) | (0.1129) |
| disbursed | -0.0830 | -0.0583 | -0.0583 | -0.3214*** |
| | (0.0597) | (0.0575) | (0.0575) | (0.0883) |
| treated | -0.2120** | -0.2157** | -0.2157** | -0.1937** |
| | (0.0888) | (0.0874) | (0.0874) | (0.0803) |
| announced × treated | 0.1147 | 0.1156 | 0.1156 | 0.0977 |
| | (0.1545) | (0.1516) | (0.1516) | (0.1407) |
| disbursed × treated | 0.0496 | 0.0458 | 0.0458 | 0.0247 |
| | (0.1053) | (0.1026) | (0.1026) | (0.0976) |
| *Fixed effects* | | | | |
| date | No | Yes | Yes | Yes |
| currency | No | No | Yes | Yes |
| exchange | No | No | No | Yes |
| *Fit statistics* | | | | |
| Observations | 4,129 | 4,129 | 4,129 | 4,129 |
| $R^2$ | 0.005 | 0.040 | 0.040 | 0.376 |

Table A.5: **Impact of economic impact payments to couples with two children on BTCUSD trade volume: $3,400 buy trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). We use a cutoff of $3,400, the likely modal amount paid to couples with two children, and a bandwidth equal to 5% of the cutoff amount. Thus, the dummy *treated* is equal to one for treated trades (between $3,230 and $3,400 in size) and zero for control trades (between $3,400 and $3,570). The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | | |
|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) |
| *Variables* | | | | |
| announced | -0.1061 | -0.4075*** | -0.4075*** | -0.1052 |
| | (0.0706) | (0.0645) | (0.0645) | (0.0705) |
| disbursed | 0.1671 | -0.0094 | -0.0094 | 0.2347** |
| | (0.1176) | (0.1133) | (0.1133) | (0.1111) |
| treated | -0.0091 | -0.0081 | -0.0081 | -0.0458 |
| | (0.0522) | (0.0521) | (0.0521) | (0.0489) |
| announced × treated | -0.0652 | -0.0645 | -0.0645 | -0.0151 |
| | (0.0854) | (0.0845) | (0.0845) | (0.0804) |
| disbursed × treated | -0.2851** | -0.2862** | -0.2862** | -0.2408* |
| | (0.1251) | (0.1247) | (0.1247) | (0.1243) |
| *Fixed effects* | | | | |
| date | No | Yes | Yes | Yes |
| currency | No | No | Yes | Yes |
| exchange | No | No | No | Yes |
| *Fit statistics* | | | | |
| Observations | 3,981 | 3,981 | 3,981 | 3,981 |
| $R^2$ | 0.004 | 0.078 | 0.078 | 0.454 |

Table A.6: **Impact of economic impact payments to couples with three children on BTCUSD trade volume: $3,900 buy trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). We use a cutoff of $3,900, the likely modal amount paid to couples with three children, and a bandwidth equal to 5% of the cutoff amount. Thus, the dummy *treated* is equal to one for treated trades (between $3,705 and $3,900 in size) and zero for control trades (between $3,900 and $4,095). The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | | |
|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) |
| *Variables* | | | | |
| announced | -0.5186*** | -0.5148*** | -0.5148*** | 0.0225 |
| | (0.0512) | (0.1188) | (0.1188) | (0.2555) |
| disbursed | -0.1102 | -0.3616*** | -0.3616*** | -0.0585 |
| | (0.0673) | (0.1371) | (0.1371) | (0.1041) |
| treated | 0.0878 | 0.0691 | 0.0691 | 0.0027 |
| | (0.1067) | (0.0963) | (0.0963) | (0.0523) |
| announced × treated | 0.9809 | 0.9681 | 0.9681 | 0.7259** |
| | (0.6252) | (0.5920) | (0.5920) | (0.3234) |
| disbursed × treated | 0.2571 | 0.2777 | 0.2777 | 0.3565** |
| | (0.1867) | (0.1826) | (0.1826) | (0.1632) |
| *Fixed effects* | | | | |
| date | No | Yes | Yes | Yes |
| currency | No | No | Yes | Yes |
| exchange | No | No | No | Yes |
| *Fit statistics* | | | | |
| Observations | 3,983 | 3,983 | 3,983 | 3,983 |
| $R^2$ | 0.002 | 0.080 | 0.080 | 0.782 |

Table A.7: **Robustness of results to changes in bandwidth: $1,000 buy trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). The dummy *treated* is equal to one for treated trades (i.e., trades for amounts lower than the cutoff by a quantity up to the bandwidth value) and zero for control trades (i.e., trades for amounts greater than the cutoff by a quantity up to the bandwidth value). We use various bandwidths, ranging from $12.50 to $100, around the $1,000 cutoff. The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. For brevity, we show only the coefficients of the interactions of the *treated* dummy with *announced* and *disbursed*, as these are the results of interest.

| Dependent Variable: | Log-odds of relative daily trade volume | | | | | | |
|---|---|---|---|---|---|---|---|
| Bandwidth: | $12.50 | $25 | $37.50 | $62.50 | $75 | $87.50 | $100 |
| *Variables* | | | | | | | |
| announced | -0.2875*** | -0.0984 | -0.0389 | -0.2220* | -0.2276** | -0.1069 | -0.0311 |
| × treated | (0.0935) | (0.0878) | (0.1204) | (0.1163) | (0.1046) | (0.0942) | (0.0984) |
| disbursed | -0.3166*** | 0.1561* | 0.1871** | -0.0556 | -0.0405 | -0.0209 | 0.0423 |
| × treated | (0.0978) | (0.0855) | (0.0844) | (0.0749) | (0.0723) | (0.0707) | (0.0664) |
| *Fixed effects* | | | | | | | |
| date | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| currency | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| exchange | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| *Fit statistics* | | | | | | | |
| Observations | 5,172 | 5,668 | 5,931 | 6,161 | 6,248 | 6,528 | 6,830 |
| $R^2$ | 0.400 | 0.297 | 0.268 | 0.251 | 0.249 | 0.241 | 0.238 |

Table A.8: **Robustness of results to changes in bandwidth: $600 buy trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). The dummy *treated* is equal to one for treated trades (i.e., trades for amounts lower than the cutoff by a quantity up to the bandwidth value) and zero for control trades (i.e., trades for amounts greater than the cutoff by a quantity up to the bandwidth value). We use various bandwidths, ranging from $7.50 to $60, around the $600 cutoff. The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. For brevity, we show only the coefficients of the interactions of the *treated* dummy with *announced* and *disbursed*, as these are the results of interest.

| Dependent Variable: | Log-odds of relative daily trade volume | | | | | | |
|---|---|---|---|---|---|---|---|
| Bandwidth: | $7.50 | $15 | $22.50 | $37.50 | $45 | $52.50 | $60 |
| *Variables* | | | | | | | |
| announced | 0.0554 | 0.0177 | -0.0019 | -0.0247 | -0.0379 | -0.0397 | -0.0227 |
| × treated | (0.1029) | (0.1050) | (0.0973) | (0.0960) | (0.0923) | (0.0993) | (0.0835) |
| disbursed | 0.5459*** | 0.5741*** | 0.4914*** | 0.4128*** | 0.3522*** | 0.3564*** | 0.3908*** |
| × treated | (0.0681) | (0.0583) | (0.0530) | (0.0508) | (0.0488) | (0.0456) | (0.0463) |
| *Fixed effects* | | | | | | | |
| date | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| currency | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| exchange | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| *Fit statistics* | | | | | | | |
| Observations | 5,016 | 5,562 | 5,824 | 6,081 | 6,194 | 6,477 | 6,799 |
| $R^2$ | 0.247 | 0.241 | 0.349 | 0.231 | 0.217 | 0.213 | 0.217 |

Table A.9: **Robustness of results to changes in bandwidth: $500 buy trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). The dummy *treated* is equal to one for treated trades (i.e., trades for amounts lower than the cutoff by a quantity up to the bandwidth value) and zero for control trades (i.e., trades for amounts greater than the cutoff by a quantity up to the bandwidth value). We use various bandwidths, ranging from $6.25 to $50, around the $500 cutoff. The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. For brevity, we show only the coefficients of the interactions of the *treated* dummy with *announced* and *disbursed*, as these are the results of interest.

| Dependent Variable: | Log-odds of relative daily trade volume | | | | | | |
|---|---|---|---|---|---|---|---|
| Bandwidth: | $6.25 | $12.50 | $18.75 | $31.25 | $37.50 | $43.75 | $50 |
| *Variables* | | | | | | | |
| announced | 0.1080 | 0.1002 | 0.0701 | 0.0072 | 0.0615 | 0.1627** | 0.2452*** |
| × treated | (0.1718) | (0.1038) | (0.0944) | (0.0869) | (0.0811) | (0.0631) | (0.0724) |
| disbursed | -0.2713** | 0.0961 | 0.1448* | 0.1928*** | 0.1764*** | 0.2176*** | 0.2003*** |
| × treated | (0.1190) | (0.0832) | (0.0736) | (0.0617) | (0.0577) | (0.0586) | (0.0479) |
| *Fixed effects* | | | | | | | |
| date | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| currency | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| exchange | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| *Fit statistics* | | | | | | | |
| Observations | 5,209 | 5,696 | 5,934 | 6,213 | 6,322 | 6,602 | 6,971 |
| $R^2$ | 0.224 | 0.217 | 0.188 | 0.186 | 0.184 | 0.190 | 0.195 |

Table A.10: **Robustness of results to changes in bandwidth: $100 buy trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). The dummy *treated* is equal to one for treated trades (i.e., trades for amounts lower than the cutoff by a quantity up to the bandwidth value) and zero for control trades (i.e., trades for amounts greater than the cutoff by a quantity up to the bandwidth value). We use various bandwidths, ranging from $1.25 to $10, around the $100 cutoff. The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. For brevity, we show only the coefficients of the interactions of the *treated* dummy with *announced* and *disbursed*, as these are the results of interest.

| Dependent Variable: | Log-odds of relative daily trade volume | | | | | | |
|---|---|---|---|---|---|---|---|
| Bandwidth: | $1.25 | $2.50 | $3.75 | $6.25 | $7.50 | $8.75 | $10 |
| *Variables* | | | | | | | |
| announced | 0.4332*** | 0.4076*** | 0.3622*** | 0.3774*** | 0.2882*** | 0.3422*** | 0.3598*** |
| × treated | (0.1015) | (0.1546) | (0.1267) | (0.1115) | (0.1007) | (0.1011) | (0.0912) |
| disbursed | 0.6872*** | 0.7554*** | 0.9649*** | 0.9623*** | 0.8766*** | 0.8545*** | 0.7406*** |
| × treated | (0.1444) | (0.1422) | (0.1161) | (0.1058) | (0.0987) | (0.0889) | (0.0903) |
| *Fixed effects* | | | | | | | |
| date | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| currency | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| exchange | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| *Fit statistics* | | | | | | | |
| Observations | 5,320 | 5,723 | 5,949 | 6,164 | 6,249 | 6,537 | 6,882 |
| $R^2$ | 0.351 | 0.363 | 0.366 | 0.407 | 0.389 | 0.404 | 0.403 |

Table A.11: **Effect of economic impact payments on BTCUSD trade volume: placebo tests**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). The dummy *treated* is equal to one for treated trades (i.e., trades for amounts lower than the cutoff by a quantity up to the bandwidth value) and zero for control trades (i.e., trades for amounts greater than the cutoff by a quantity up to the bandwidth value). We consider four arbitrarily chosen cutoffs that we do not expect to be affected by the US EIP program. In each case, we use a bandwidth equal to 5% of the cutoff value. The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | | |
|---|---|---|---|---|
| Cutoff: | \$200 | \$750 | \$4,000 | \$12,000 |
| Bandwidth: | \$10 | \$37.50 | \$200 | \$600 |
| *Variables* | | | | |
| announced | 0.3110** | -0.1373 | -0.0760 | 0.1334 |
| | (0.1223) | (0.1716) | (0.0853) | (0.1286) |
| disbursed | 0.0239 | 0.1681* | 0.1190 | -0.0143 |
| | (0.1096) | (0.0964) | (0.0751) | (0.1787) |
| treated | 0.5043*** | 0.0516 | 0.0954* | 0.0750 |
| | (0.0553) | (0.0559) | (0.0497) | (0.0715) |
| announced | -0.5201*** | 0.2044 | 0.0528 | -0.1504 |
| × treated | (0.0743) | (0.1803) | (0.0787) | (0.1388) |
| disbursed | 0.0924 | -0.0278 | 0.1359* | 0.1083 |
| × treated | (0.0764) | (0.0859) | (0.0804) | (0.0973) |
| *Fixed effects* | | | | |
| date | Yes | Yes | Yes | Yes |
| currency | Yes | Yes | Yes | Yes |
| exchange | Yes | Yes | Yes | Yes |
| *Fit statistics* | | | | |
| Observations | 5,766 | 5,933 | 4,858 | 3,413 |
| $R^2$ | 0.269 | 0.244 | 0.366 | 0.406 |

Table A.12: **Effect of economic impact payments on price: $1,000 buy trades**

Table presents difference-in-differences estimates of the effect of EIPs on Bitcoin price based on the specification outlined in Equation (5). The dependent variable is the logarithm of the mean execution price for all Bitcoin buy trades in USD within exchange $j$ at hour $t$. The sample comprises Bitcoin buy trades in USD between January 1 to June 5, 2020. The dummy *announced* is equal to 1 iff the CARES Act is announced by hour $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out at hour $t$ (i.e., April 9 or later). The scalar variable *pct_treated* is the number of BTCUSD buy trades for treated amounts (between $950 and $1,000) on exchange $j$ at hour $t$, expressed as a proportion of the total number of BTCUSD buy trades for that same exchange and time. The regressions include hourly, daily, weekly, and exchange fixed-effects. Standard errors are clustered by hour, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

| Dependent Variable: | Logarithm of mean price | | | | |
|---|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) | (5) |
| *Variables* | | | | | |
| announced | -0.1151*** | -0.1744*** | 0.0000 | -0.0003 | -0.0061*** |
| | (0.0041) | (0.0036) | (0.0001) | (0.0002) | (0.0009) |
| disbursed | 0.0320*** | 0.0364*** | 0.0005*** | -0.0000 | -0.0059*** |
| | (0.0046) | (0.0054) | (0.0000) | (0.0002) | (0.0007) |
| pct_treated | 0.4047*** | 0.4543*** | 0.0008 | -0.0039 | -0.0181** |
| | (0.0446) | (0.0486) | (0.0018) | (0.0034) | (0.0090) |
| announced | -1.267*** | -0.7031*** | 0.0004 | 0.0135 | -0.0630* |
| × pct_treated | (0.4370) | (0.1889) | (0.0039) | (0.0177) | (0.0330) |
| disbursed | 0.3218** | 0.3074** | -0.0011 | 0.0180** | 0.1254*** |
| × pct_treated | (0.1326) | (0.1419) | (0.0019) | (0.0090) | (0.0355) |
| *Fixed-effects* | | | | | |
| exchange | No | Yes | Yes | Yes | Yes |
| time: hourly | No | No | Yes | No | No |
| time: daily | No | No | No | Yes | No |
| time: weekly | No | No | No | No | Yes |
| *Fit statistics* | | | | | |
| Observations | 53,991 | 53,991 | 53,991 | 53,991 | 53,991 |
| $R^2$ | 0.073 | 0.109 | 0.998 | 0.987 | 0.917 |

Table A.13: **Effect of economic impact payments on price: $600 buy trades**
Table presents difference-in-differences estimates of the effect of EIPs on Bitcoin price based on the specification outlined in Equation (5). The dependent variable is the logarithm of the mean execution price for all Bitcoin buy trades in USD within exchange $j$ at hour $t$. The sample comprises Bitcoin buy trades in USD between January 1 to June 5, 2020. The dummy *announced* is equal to 1 iff the CARES Act is announced by hour $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out at hour $t$ (i.e., April 9 or later). The scalar variable *pct_treated* is the number of BTCUSD buy trades for treated amounts (between $570 and $600) on exchange $j$ at hour $t$, expressed as a proportion of the total number of BTCUSD buy trades for that same exchange and time. The regressions include hourly, daily, weekly, and exchange fixed-effects. Standard errors are clustered by hour, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

| Dependent Variable: | Logarithm of mean price | | | | |
|---|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) | (5) |
| *Variables* | | | | | |
| announced | -0.1135*** | -0.1720*** | -0.0000 | 0.0004 | -0.0039*** |
| | (0.0036) | (0.0036) | (0.0001) | (0.0002) | (0.0009) |
| disbursed | 0.0371*** | 0.0398*** | 0.0005*** | 0.0000 | -0.0041*** |
| | (0.0043) | (0.0050) | (0.0000) | (0.0002) | (0.0005) |
| pct_treated | 0.1523*** | 0.1913*** | -0.0022* | -0.0138** | 0.0667*** |
| | (0.0457) | (0.0465) | (0.0013) | (0.0061) | (0.0116) |
| announced | -2.582*** | -1.931*** | -0.0051 | -0.1507*** | -0.4984*** |
| × pct_treated | (0.3077) | (0.2639) | (0.0036) | (0.0347) | (0.0733) |
| disbursed | 0.2111** | 0.2124** | 0.0012 | 0.0238** | 0.0781*** |
| × pct_treated | (0.1008) | (0.1027) | (0.0017) | (0.0098) | (0.0256) |
| *Fixed-effects* | | | | | |
| exchange | No | Yes | Yes | Yes | Yes |
| time: hourly | No | No | Yes | No | No |
| time: daily | No | No | No | Yes | No |
| time: weekly | No | No | No | No | Yes |
| *Fit statistics* | | | | | |
| Observations | 53,991 | 53,991 | 53,991 | 53,991 | 53,991 |
| $R^2$ | 0.069 | 0.106 | 0.998 | 0.987 | 0.917 |

Table A.14: **Effect of economic impact payments on price: $500 buy trades**
Table presents difference-in-differences estimates of the effect of EIPs on Bitcoin price based on the specification outlined in Equation (5). The dependent variable is the logarithm of the mean execution price for all Bitcoin buy trades in USD within exchange $j$ at hour $t$. The sample comprises Bitcoin buy trades in USD between January 1 to June 5, 2020. The dummy *announced* is equal to 1 iff the CARES Act is announced by hour $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out at hour $t$ (i.e., April 9 or later). The scalar variable *pct_treated* is the number of BTCUSD buy trades for treated amounts (between $475 and $500) on exchange $j$ at hour $t$, expressed as a proportion of the total number of BTCUSD buy trades for that same exchange and time. The regressions include hourly, daily, weekly, and exchange fixed-effects. Standard errors are clustered by hour, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

| Dependent Variable: | Logarithm of mean price | | | | |
|---|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) | (5) |
| *Variables* | | | | | |
| announced | -0.1182*** | -0.1775*** | -0.0000 | -0.0003* | -0.0058*** |
| | (0.0039) | (0.0036) | (0.0001) | (0.0002) | (0.0010) |
| disbursed | 0.0367*** | 0.0403*** | 0.0005*** | 0.0000 | -0.0050*** |
| | (0.0042) | (0.0050) | (0.0000) | (0.0002) | (0.0005) |
| pct_treated | 0.1174*** | 0.1654*** | 0.0004 | -0.0105** | -0.0308*** |
| | (0.0391) | (0.0431) | (0.0014) | (0.0045) | (0.0101) |
| announced | -0.8605*** | -0.3880*** | -0.0060 | 0.0110 | -0.0813 |
| × pct_treated | (0.3226) | (0.1317) | (0.0050) | (0.0116) | (0.0630) |
| disbursed | 0.0779 | 0.0713 | 0.0024* | 0.0095 | 0.0555*** |
| × pct_treated | (0.0599) | (0.0632) | (0.0013) | (0.0082) | (0.0172) |
| *Fixed-effects* | | | | | |
| exchange | No | Yes | Yes | Yes | Yes |
| time: hourly | No | No | Yes | No | No |
| time: daily | No | No | No | Yes | No |
| time: weekly | No | No | No | No | Yes |
| *Fit statistics* | | | | | |
| Observations | 53,991 | 53,991 | 53,991 | 53,991 | 53,991 |
| $R^2$ | 0.068 | 0.104 | 0.998 | 0.987 | 0.917 |

Table A.15: **Effect of economic impact payments on price: $100 buy trades**

Table presents difference-in-differences estimates of the effect of EIPs on Bitcoin price based on the specification outlined in Equation (5). The dependent variable is the logarithm of the mean execution price for all Bitcoin buy trades in USD within exchange $j$ at hour $t$. The sample comprises Bitcoin buy trades in USD between January 1 to June 5, 2020. The dummy *announced* is equal to 1 iff the CARES Act is announced by hour $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out at hour $t$ (i.e., April 9 or later). The scalar variable *pct_treated* is the number of BTCUSD buy trades for treated amounts (between $95 and $100) on exchange $j$ at hour $t$, expressed as a proportion of the total number of BTCUSD buy trades for that same exchange and time. The regressions include hourly, daily, weekly, and exchange fixed-effects. Standard errors are clustered by hour, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

| Dependent Variable: | Logarithm of mean price | | | | |
|---|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) | (5) |
| *Variables* | | | | | |
| announced | -0.1196*** | -0.1806*** | -0.0000 | -0.0005*** | -0.0086*** |
| | (0.0034) | (0.0034) | (0.0001) | (0.0002) | (0.0009) |
| disbursed | 0.0363*** | 0.0415*** | 0.0005*** | 0.0003** | -0.0057*** |
| | (0.0043) | (0.0051) | (0.0000) | (0.0001) | (0.0005) |
| pct_treated | 0.2997*** | 0.3463*** | -0.0009* | 0.0002 | 0.0039 |
| | (0.0199) | (0.0220) | (0.0005) | (0.0016) | (0.0050) |
| announced | -0.2156*** | 0.0840 | -0.0032 | 0.0222*** | 0.1462*** |
| × pct_treated | (0.0701) | (0.0742) | (0.0021) | (0.0058) | (0.0208) |
| disbursed | -0.1152*** | -0.1517*** | -0.0005 | -0.0022 | 0.0365*** |
| × pct_treated | (0.0220) | (0.0236) | (0.0005) | (0.0018) | (0.0057) |
| *Fixed-effects* | | | | | |
| exchange | No | Yes | Yes | Yes | Yes |
| time: hourly | No | No | Yes | No | No |
| time: daily | No | No | No | Yes | No |
| time: weekly | No | No | No | No | Yes |
| *Fit statistics* | | | | | |
| Observations | 53,991 | 53,991 | 53,991 | 53,991 | 53,991 |
| $R^2$ | 0.076 | 0.115 | 0.998 | 0.987 | 0.917 |

Table A.16: **Impact of economic impact payments on BTCUSD sell trade volume: $1,000 sell trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin sell trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin sell trades in currency $i$ within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of sell trades for that same currency, exchange, and day. The sample comprises Bitcoin sell trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). The dummy *treated* is equal to one for treated trades (between $950 and $1,000 in size) and zero for control trades (between $1,000 and $1,050). The regressions include date, currency, and exchange fixed-effects. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | | |
|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) |
| *Variables* | | | | |
| announced | -0.3739*** | -0.2404** | -0.1960* | -0.0719 |
| | (0.0964) | (0.1137) | (0.1126) | (0.1209) |
| disbursed | 0.0110 | 0.1477** | 0.1517** | -0.0360 |
| | (0.0474) | (0.0745) | (0.0756) | (0.0858) |
| treated | 0.0628 | 0.0493 | 0.0528 | 0.0447 |
| | (0.0673) | (0.0660) | (0.0663) | (0.0643) |
| announced × treated | -0.1711* | -0.1588* | -0.1611* | -0.1643* |
| | (0.0923) | (0.0914) | (0.0916) | (0.0900) |
| disbursed × treated | 0.0942 | 0.1000 | 0.0991 | 0.0991 |
| | (0.0926) | (0.0911) | (0.0914) | (0.0895) |
| *Fixed effects* | | | | |
| date | No | Yes | Yes | Yes |
| currency | No | No | Yes | Yes |
| exchange | No | No | No | Yes |
| *Fit statistics* | | | | |
| Observations | 7,658 | 7,658 | 7,658 | 7,658 |
| $R^2$ | 0.012 | 0.081 | 0.086 | 0.242 |

Table A.17: **Impact of economic impact payments on BTCEUR buy volume: €1,000 buy trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in euros within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades in euros for that same exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). The dummy *treated* is equal to one for treated trades (between €950 and €1,000 in size) and zero for control trades (between €1,000 and €1,050). Our data set includes no trades in non-program currencies for these amounts, so we are forced to include only BTCEUR buy trades in our sample. The regressions include date, currency, and exchange fixed-effects. Currency fixed-effects make no difference, due to the lack of trades in non-program currencies. Time fixed-effects make standalone terms for *announced* and *disbursed* redundant. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | |
|---|---|---|---|
| Model: | (1) | (2) | (3) |
| *Variables* | | | |
| treated | 0.3174** | 0.3174** | 0.2111** |
| | (0.1486) | (0.1486) | (0.0960) |
| announced × treated | -0.4883*** | -0.4883*** | -0.2544** |
| | (0.1598) | (0.1598) | (0.1217) |
| disbursed × treated | 0.0118 | 0.0118 | 0.0926 |
| | (0.1721) | (0.1721) | (0.1234) |
| *Fixed effects* | | | |
| date | Yes | Yes | Yes |
| currency | No | Yes | Yes |
| exchange | No | No | Yes |
| *Fit statistics* | | | |
| Observations | 2,557 | 2,557 | 2,557 |
| $R^2$ | 0.083 | 0.083 | 0.484 |

Table A.18: **Impact of economic impact payments on BTCGBP buy volume: £1,000 buy trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in pounds within group $s$ (treated/control) on exchange $j$ on day $t$, expressed as a proportion of the total number of buy trades in pounds for that same exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020. The dummy *announced* is equal to 1 iff the CARES Act is announced by day $t$ and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8). The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day $t$ (i.e., April 9 or later). The dummy *treated* is equal to one for treated trades (between £950 and £1,000 in size) and zero for control trades (between £1,000 and £1,050). Our data set includes no trades in non-program currencies for these amounts, so we are forced to include only BTCGBP buy trades in our sample. The regressions include date, currency, and exchange fixed-effects. Currency fixed-effects make no difference, due to the lack of trades in non-program currencies. Time fixed-effects make standalone terms for *announced* and *disbursed* redundant. Standard errors are clustered by date, and are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| Dependent Variable: | Log-odds of relative daily trade volume | | |
|---|---|---|---|
| Model: | (1) | (2) | (3) |
| *Variables* | | | |
| treated | 0.3733 | 0.3733 | 0.2239** |
| | (0.2620) | (0.2620) | (0.0888) |
| announced × treated | -0.2924 | -0.2924 | -0.2815 |
| | (0.3465) | (0.3465) | (0.1990) |
| disbursed × treated | 0.1866 | 0.1866 | 0.4672 |
| | (0.3198) | (0.3198) | (0.4821) |
| *Fixed effects* | | | |
| date | Yes | Yes | Yes |
| currency | No | Yes | Yes |
| exchange | No | No | Yes |
| *Fit statistics* | | | |
| Observations | 973 | 973 | 973 |
| $R^2$ | 0.162 | 0.162 | 0.915 |