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

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Abstract

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 of size $1,200, which is the modal EIP amount. We find similar increases in trading for other countries that paid out stimulus checks. We estimate that the EIPs have a significant impact on the US dollar–Bitcoin trading pair, increasing buy volume by 3.8 percent, and the price by 0.6 percent. We also find that demand for Bitcoin is highly price inelastic compared to the demand for stocks. We suggest the demographic characteristics that make people more resilient to the COVID-19 economic shock — single, computer literate, and educated — are also characteristics of people who are more interested in Bitcoin.

JEL classification: E42, G11, G41, H31.

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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, as did some stocks. These developments were driven by the internet and social media, and exacerbated by the circumstances of the COVID-19 pandemic. Nonetheless, the causes and consequences of retail trading, especially 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 efforts 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.

There is 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 deposits for exactly $1,200 (see Figure 1). Binance US, another cryptocurrency exchange, reported a similar phenomenon. And some Bitcoin enthusiasts 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. Our modeling approach is based on regression discontinuity design. Using a 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). Our identification is predicated on the notion that the treated and control groups should behave similarly in response to all factors, other than the EIP program. During the period when EIPs are disbursed, 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. The 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. This is evidence that a significant number of people spend their $1,200 checks on Bitcoin.[2]

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. Consistent with a wealth shock to retail traders, we find that the effect of EIPs is strongest on Bitcoin exchanges with a higher volume of low-value trades. And, consistent with a demand-side shock, we observe no increase in selling activity.

Americans with families received EIPs larger than $1,200. However, we find no evidence of

[1] The Coinbase CEO's tweet has since been deleted. For a contemporaneous report, see P. Baker (2020). For examples of social media activity, see u/DaleWright43456 (2020) and @BitcoinStimulus (2021).

[2] There are two further rounds of EIPs in the US, in December 2020 and March 2021. Due to data limitations, our paper examines the first round only. Dantes (2021) reports little sign that the third round had a substantial impact on cryptocurrency markets.

increased Bitcoin trading at those trade sizes. 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 survey evidence on Bitcoin investors; see, for example, Auer and Tercero-Lucas (2021).

We consider the possibility that Americans may spend 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 uncertainty; see Butler and Loomes (2007). Consistent with our earlier results, we do not find a corresponding 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 affecting round number preference.

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 buy trading by volume, and a 0.7 percent increase by value. Our methodology likely underestimates the true size of the effect, since we cannot rule out individuals purchasing amounts of Bitcoin other than those we test for. While the estimated effect on trading is small compared to the overall size of the EIP program, there is heterogeneity in how the money is used, with a small number of people using most or all of their EIP to buy Bitcoin.

We associate the EIP program with a significant rise in the price of Bitcoin over the period in which payments are disbursed. This demonstrates that the demand curve for Bitcoin is downward-sloping, since the program conveyed no information about the value of Bitcoin. We estimate Bitcoin returns are 0.6 percentage points higher than they would have been in the absence of the program. With 95% confidence, the impact is between 0.03 and 1.2 percentage points. By design, our methodology produces an underestimate, so the true impact is probably close to 1 percentage point. These results show that retail trading shocks can have significant impacts on prices. Our estimated price rise is admittedly modest compared to daily fluctuations in the price of Bitcoin, perhaps due to the public nature of the shock. However, it is large relative to the amount of EIP money used to buy Bitcoin: we estimate that the price of Bitcoin is about four times more responsive to shifts in demand than stocks. This suggests that the demand curve for Bitcoin is steeply downward sloping.

We find similar results for other countries that run universal stimulus check programs. 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 ran similar programs to the US, and made direct one-off stimulus payments to most households within their respective jurisdictions. We apply our regression discontinuity methodology and find positive results for the Japanese yen and South Korean won, suggesting that increases in Bitcoin buy trades are indeed driven by the direct stimulus payment programs in these countries and cannot be explained by other factors. We do not find a significant increase in trading for Singapore dollars, perhaps because thin markets and regulatory changes confound our results.

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; see Greenwood et al. (2022). Unlike retail traders in stock markets, who tend to be intermediated by brokers, cryptocurrency traders can typically place orders directly to an exchange. Our methodology could be replicated for stock markets, using customer-level deposit data from individual brokers to test whether EIPs are used to buy stock. Still, we would expect EIPs to have a stronger effect on cryptocurrency than stock trading, because the characteristics of people who receive the \$1,200 EIPs — young with moderate incomes — are typically associated with a stronger preference for lottery-type investments.

Our results have policy implications. Direct payments to households have been used as a fiscal policy instrument in many countries as part of broader 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). This is consistent with our finding that only a small number of people invest substantive amounts of their EIPs in Bitcoin. However, there are three circumstances peculiar to the spring of 2020 that may have attracted investors to Bitcoin as an alternative investment vehicle, and so caution us against drawing strong policy conclusions. First, financial markets experienced significant turmoil, 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 as high as \$67,567 on November 7, 2021, according to Coinmarketcap (2021). Second, the economic effects of the pandemic may have been less severe for people with the highest propensity to buy Bitcoin: that is, those 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. Ahead of previous halvenings, interest in Bitcoin, and its price, have tended to rise. While the 2020 halvening was no exception, we do not find a strong association between it and our findings.

Our study complements the literature on how households respond to unexpected increases in wealth. Several recent papers use survey data to examine how households spend their EIPs. They find EIPs are commonly used to save or pay down existing debt, rather than consume. For example, see Armantier, Goldman, Koşar, Lu, et al. (2020), S. R. Baker et al. (2020), Coibion, Gorodnichenko, et al. (2020), and Perez-Lopez and Bee (2020). These studies, however, do not focus on whether the money is spent on investment assets. In this paper, 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. Our paper contributes to the nascent literature on the demographic characteristics of cryptocurrency investors; see Auer and Tercero-Lucas (2021), Benetton and Compiani (2020), and Hackethal et al. (2021).

The remainder of this 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, including relevant literature. 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 effect of the EIPs on the Bitcoin market. In Section 7 we examine Bitcoin trading in other countries with stimulus check programs. Section 8 concludes. Figures and tables follow the references. Supplementary figures and tables are contained in an Online Appendix, and are prefixed with an 'A'.

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 as a result of COVID-19, 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 plunged. The Bitcoin price began to rise again in late March after governments announced fiscal and monetary measures to combat the pandemic. A sharp rise in price and trading volumes followed in early May. After mid-2020 — beyond the period we cover in this study — the price of Bitcoin climbed steeply, exceeding $67,000 in November 2021.

It is unclear 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; see, for example, Grobys (2020). In fact, Coibion, Georgarakos, et al. (2021) find, in a hypothetical exercise, that respondents 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," which occurred on May 11, 2020. After a halvening, the reward to miners for creating a new Bitcoin block is halved, in this case from 12.5 to 6.25 bitcoins. Such events are hard-coded to occur once every 210,000 blocks, so are easily anticipated.[3] Despite this predictability, the Bitcoin price has historically increased in anticipation of these

[3]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.

halvening events, and the May 2020 halvening was no exception (Figure 2). This may be due to self-fulfilling expectations of a price increase or because media coverage of these events generates more investor attention. Interest in Bitcoin peaked around the times of Black Thursday and the halvening.[4] 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 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. Every dollar of annual income above the $75,000 threshold reduces the payment by 5 cents, reaching zero at $99,000. The median US annual personal income in 2019 was $35,977, so most US residents were eligible for the full EIP amount of $1,200. Table 1 provides more details on the EIPs and the eligibility criteria.[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 paper 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. Holtzblatt and Karpman (2020) estimate nearly 7 in 10 adults received their EIP by the end of May. By June 3, the IRS had sent EIPs to 159 million Americans, totaling almost $267 billion, with an estimated 30–35 million payments remaining to be made, according to 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. These subsequent rounds do not form part of our analysis in this paper because our data period 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

[4] See Figure A.1, which plots Google searches for the term "Bitcoin" in the US.

[5] EIPs were not offset by any debts due, except child support. For the text of the CARES Act, see Congress (2020). For more information about IRS disbursements, see Internal Revenue Service (2021).

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

debt, rather than finance consumption. See, for example, Armantier, Goldman, Koşar, Lu, et al. (2020), S. R. Baker et al. (2020), and Coibion, Gorodnichenko, et al. (2020). Other papers — such as Boutros (2020), Garner et al. (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) examines a survey by the Federal Reserve Bank of Philadelphia, and finds that 41 percent of respondents report saving at least part of their EIP. Using the Consumer Expenditure Survey, Parker et al. (2022) find that EIPs are spent soon after receiving them. These survey papers tend not to disentangle investment from saving and, in any case, do not specifically ask about purchases of cryptocurrency.[7]

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 et al. (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. Greenwood et al. (2022) that, around the time of the stimulus check payouts, retail buying of US stocks increased, as did the prices of retail-dominated portfolios.

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 S. R. Baker et al. (2020), Coibion, Gorodnichenko, et al. (2020), and Misra et al. (2020).

While the papers mentioned so far do not examine the use of EIPs to purchase Bitcoin, some industry surveys do specifically ask about this. Self Financial (2020) estimates people who received EIPs in the first round put $762m into Bitcoin. That is far higher than our estimate, perhaps because the survey asks about spending intention, rather than actual spending, and perhaps because our methodology is likely to underestimate the true amount invested. An online poll by The Harris Poll (2021) finds 7 percent of respondents used at least some of the first-round EIP to buy cryptocurrencies, including Bitcoin. The figure is higher among respondents who were male and those aged 44 and under. A March 2021 Mizuho Securities survey finds that 40 percent of respondents who expected to a receive a third-round EIP would use it to invest, and that about three-fifths of this money would go into Bitcoin; see Sozzi (2021). And an online poll by Rodriguez (2021) found 5 percent of respondents invested EIP money from one of the three rounds into cryptocurrency.[8]

[7] Armantier, Goldman, Koşar, and Klaauw (2021) examine a survey on the second round of EIPs and find a slightly greater proclivity to save or pay down debt.

[8] It is unclear whether the polls by Self or Mizuho were conducted exclusively online or not. As some degree of computer literacy is needed to participate in an online poll, respondents may be more likely to be interested in Bitcoin than the general population.

2.4 Demographic characteristics of Bitcoin investors

People interested in Bitcoin are more likely to be male, white, single, computer literate, and earn an above-average income, compared to those not interested in Bitcoin.[9] 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 (Papanikolaou and Schmidt (2022)). 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, coming during a recession, may be likelier 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 should 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.

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, and are listed in Table 2.

[9] This is borne out in surveys by regulators, such as Auer and Tercero-Lucas (2021), English et al. (2020), and Henry et al. (2019), and by industry surveys, such as Belger (2018), Gitlin (2018), and Graytok (2021). Hackethal et al. (2021) make similar observations about German indirect investors in cryptocurrency via structured retail products.

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.[10] Those listed as "program" currencies (USD, JPY, KRW, and SGD) are issued by jurisdictions that, during our sample period, introduced stimulus check programs. These schemes are described in more detail in Section 7.[11]

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 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 slightly 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. In this subsection, we first explain why we use $1,200 as a point of comparison, and then describe our econometric approach.

3.2.1 Focus on $1,200

The IRS provides only aggregate information on EIPs, so we do not know the size of each individual payment. Therefore, we cannot identify with certainty which Bitcoin 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 (Table A.1), suggesting that the modal EIP payment is no greater than $1,200.[12] 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.[13]

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

[11] 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.

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

[13] 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-eligible individuals; for example, expatriate US citizens

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 the entire EIP is used for this purpose. 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. This credit constraint means that it may be optimal to use the entire EIP to buy Bitcoin. 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 deposits.[14] Typically, credit on an exchange requires collateral. Suppose an investor receives an EIP and decides to use it to buy both Bitcoin and consumption goods. The investor might rationally use the EIP cash for Bitcoin trading and buy consumption goods with a credit card, which does not require collateral.[15]

Second, there are behavioral reasons why an investor may choose to invest the entire EIP in Bitcoin. Cryptocurrency 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, as described by Shiller (2019, Chapter 1). In their opinion, using central bank money to buy Bitcoin is a subversive, and thus desirable, act.[16] Putting the whole amount into Bitcoin sends a strong signal to their perceived peer group of defiance against the government. This behavioral motivation is closely related to the idea of conspicuous consumption, in which certain goods bestow status upon the purchaser; see Bagwell and Bernheim (1996). The behavioral economics literature provides other reasons why an individual might send such a signal; for example, Mandel (2009) suggests self-satisfaction, Leibenstein (1950) describe a bandwagon effect, and Braun and Wicklund (1989) discuss insecurity about personal identity. Kuchler and Stroebel (2020) review the empirical literature on how social interactions influence retail investors' choices.

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. 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. For more detail on the ACS, see US Census Bureau (2021).

[14] See Kraken (2021).

[15] For a formalization of this idea, see Telyukova (2013).

[16] See, for example, @BitcoinStimulus (2021) and u/DaleWright43456 (2020).

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 for the EIPs. We then compare the behavior of these two groups after the EIP program begins. Specifically, we test whether 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 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 individual trades 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 \$1,200, 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.[17] 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.[18]

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 stimulus check programs in those

[17] 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. See Carlson (2021). Coinbase no longer provides details about its fee structure on its website. These fees were correct as of July 2021, and certainly during our data period.

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

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.[19] 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.

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 more 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 tend 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 execution prices 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 an 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, we have an identification problem: an increase in trades for treated amounts can actually 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 exchanges with more trades financed by EIPs have higher execution prices.

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, exchanges with more trades financed by EIPs have

[19] Fuster et al. (2020) find that people are less responsive to news about future gains than they are to unanticipated realized gains.

higher execution prices for buy trades.

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. 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 CARES Act was passed on March 27 and the first EIPs disbursed on April 9, 2020. We use an event study framework to determine the effects of EIPs on Bitcoin buy trades in US dollars around these dates. 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 λ_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 λ_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 λ_t terms are not different from zero for $13 \leq t < 0$ (i.e., between the CARES Act passing and the first EIP disbursements). 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 ϵ_{jst} is normally distributed and assumed to be uncorrelated with the main regressors.

Figure 5 plots the estimated coefficients λ_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 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 -1), 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.[20]

4.2 Magnitude of effect of EIPs

We employ a difference-in-differences 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 traded 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).[21] Since EIPs are paid only in USD, they should directly affect buy trades in USD only, and not 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 currencies (JPY, KRW, SGD), along with all trades on exchanges domiciled in Japan, South Korea, and Singapore, to prevent stimulus check programs in those jurisdictions from confounding our results.

Our regression specification is given by Equation (2):

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

[20]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.

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

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.[22]

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 another 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 passed 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 α and γ are scalars, while $\boldsymbol{\beta}$ and $\boldsymbol{\delta}$ are vectors of size 2, all to be estimated.

The terms ω_i, μ_j, ν_t are fixed effects terms. The error term ϵ_{ijst} is normally distributed with mean zero.[23] 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 δ_2 is significantly positive, while Prediction P.2 says δ_1 is not significantly different from zero.

Table 4 presents our model estimates. For simplicity, we define the dummy *announced*, which is equal to 1 iff $USD_i = 1$ and the first component of $\boldsymbol{phases} = 1$. Similarly, *disbursed* is equal to 1 iff $USD_i = 1$ and the second component of $\boldsymbol{phases} = 1$. 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 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.[24]

Once again, we find no evidence of an announcement effect (Prediction P.2). 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, perhaps because of our conservative identification strategy.

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

[23] 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.

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

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 USD dummy and 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. This is because, as the bandwidth increases, we can be less confident that the treated group is mainly comprised of trades financed by EIPs.

As a placebo test, we repeat our regressions using sell orders, rather than buy orders, and find the EIPs have no effect on trade volumes. This is consistent with the EIP program being a demand-side shock. The results are available on request.

4.3 Larger EIP payments to families and leveraged trades

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.[25]

In each of these four cases, we find that the EIPs do not have a significant and robust 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.[26]

To account for the possibility that EIP recipients are leveraging up their deposits to make larger 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 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.[27]

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

[25] This is approximately equal to a Coinbase fee on a credit card transaction, as discussed in Footnote 17 and the bandwidth used in Section 4.2, rounded up for mathematical simplicity.

[26] 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.

[27] 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.

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.

Our definition of the retail statistic reflects the idea that retail traders are likely to make smaller trades than professional investors. The exact definition is somewhat arbitrary, as there is no strict point at which we can claim 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. In any case, \$5,000 is well above the modal EIP size, so the exact 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.[28] The raw ratio does not vary much between exchanges, so we take logs to increase the variation.

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.

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. 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 canceled 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 of drawing any conclusions about the announcement period from Table 6.

[28] 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. One way to do this is for the exchange owner to carry out wash trading. See, for example, Cong et al. (2021).

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 a fully rational agent responds to a wealth shock? The increased budget would be spent across a bundle of goods. The allocation would be determined by setting the marginal rate of substitution between any two goods equal to their relative price. Assuming Bitcoin has some positive utility for this rational agent, some of the EIP will be used to purchase Bitcoin. But, without knowledge of the agent's 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 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, the agent 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, as described by Tversky and Thaler (1990). Subjects' certainty-equivalent values tend not to be precise, but fall within a range; see Butler and Loomes (2007). These ranges tend to be wider when the lottery is more uncertain, and when the maximum payoff is higher; see 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 et al. (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 is described by Equation 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 display only the results when all fixed effects are included. 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).

Unlike the other cutoffs, 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, this evidence is consistent with Prediction P.2.

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.[29]

[29] There also seems to be some preference for $600 trade sizes throughout the pre-CARES Act period. We have no explanation for that, but note it is statistically less significant than for $1,000, $500 or $100.

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 and reject this hypothesis by looking at trades in euros and pound sterling; the results are available on request.

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.[30] 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 round amounts.

The significant results at the \$600 cutoff may be due to another stimulus program created by the CARES Act. Federal Pandemic Unemployment Compensation (FPUC) paid an additional \$600 a week to US claimants of unemployment insurance during the period March 27 to July 26, 2020. On the one hand, FPUC recipients are likely to be low income and thus have more urgent financial needs than buying Bitcoin; see S. R. Baker et al. (2020). On the other hand, as Ganong et al. (2020) show, FPUC payments exceeded lost income in many parts of the United States, 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 the $disbursed \times treated$ interaction term. Therefore, the EIP program is not associated with an increase in Bitcoin trading for any of these amounts.

[30] 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.

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

Consider any cutoff trade size for the treated group (e.g., \$1,200). 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 those 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 δ be the estimated coefficient of the *disbursed* × *treated* interaction term.[31] 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:

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

Equation (3) states that the observed log-odds with the EIPs is equal to δ plus the log-odds without EIPs, and is a direct implication of the definition of δ.

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

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

Thus $1 - e^{-\delta}$ is an estimate of the proportion of treated group trades during the disbursement period that 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 those treated trades due to EIPs, by plugging in the estimated coefficients obtained from the difference-in-differences regressions in Sections 4.2 and 5.2. Note that, because the volume of activity is concentrated at round number amounts, the number of trades due to 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.[32] Therefore, the EIP program has a statistically impact but economically

[31] That is, δ is the second element of $\boldsymbol{\delta}$ estimated in Equation (2), and $Y_{j,treated,t} = x_{jt}/y_{jt}$.

[32] In total, there are 5,768,935 BTCUSD buy trades during the EIP disbursement period, with aggregate value

modest on Bitcoin trading during this period.

The numbers are small 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.[33] 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, while a wider bandwidth captures more treated trades, the estimated values of δ 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 the Bitcoin price is higher 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, because EIPs may affect the price of control trades, too. Therefore, we take a slightly different approach and examine whether, during EIP disbursement, the USD prices at which Bitcoin buy trades are executed (i.e., ask prices) are higher on exchanges with a larger proportion of treated trades. We have established that EIPs only affect trading in USD, so we restrict attention to USD buy trades.

Define p_{jt} to be the mean price of a BTCUSD buy trade at exchange j on day t. The *marketwide price* on day t is written $\bar{p}_t$ and is defined as the weighted mean price across all exchanges:

$$\bar{p}_t \equiv \frac{\sum_j p_{jt} n_{jt}}{\sum_j n_{jt}}, \tag{5}$$

where n_{jt} is the number of BTCUSD buy trades at exchange j on day t. We then define the *excess log-return* $\tilde{p}_{jt}$ as the difference between the return on exchange j and the marketwide return on day t; that is:

$$\tilde{p}_{jt} = \log\left(\frac{p_{jt}}{p_{j,t-1}}\right) - \log\left(\frac{\bar{p}_t}{\bar{p}_{t-1}}\right). \tag{6}$$

By examining the excess return relative to the marketwide average, we account for any omitted variables that affect the overall Bitcoin market.

We are interested in whether $\tilde{p}_{jt}$ is higher when there are more trades financed by EIPs at

\$8,402.252m.

[33] 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.

exchange j on day t. We can estimate the amount of EIP-financed trades by the quantity EIP_{jt}:

$$\text{EIP}_{jt} \;=\; L_{USD,j,treated,t} \;-\; L_{USD,j,control,t}, \tag{7}$$

where L is as defined in Section 4.2. When EIP_{jt} increases, we infer there are more EIP-financed trades. It is not necessary to assume EIP_{jt} has mean zero. The regression is described by Equation (8):

$$\begin{aligned} \tilde{p}_{j,t} \;=\; \hat{\alpha} \;+\; \hat{\boldsymbol{\beta}} \cdot \boldsymbol{phases}_t \;+\; \hat{\gamma} \times \Delta\text{EIP}_{jt} + \hat{\boldsymbol{\delta}} \cdot \boldsymbol{phases}_t \times \Delta\text{EIP}_{jt} \\ +\; \theta \tilde{p}_{j,t-1} \;+\; \hat{\mu}_j + \hat{\nu}_t + \hat{\epsilon}_{jt}, \end{aligned} \tag{8}$$

where Δ denotes first-differences over time t. As before, $\boldsymbol{phases}_t$ is a dummy vector of size 2, denoting whether the day t falls during the EIP announcement or disbursement period. We include fixed effects for the exchange and time, along with normally distributed error terms. We also a include a lagged excess log-return term (with coefficient θ) to allow for any momentum or reversion effects. We put hats over the coefficients, to make it clear that these are not the same as those in the previous regression equation.[34]

Table 10 shows the results with a cutoff of \$1,200. We estimate four models: (1) with no fixed effects; (2) with exchange fixed effects only; (3) with exchange and daily time fixed effects; and (4) with exchange and weekly time fixed effects. There is evidence for Prediction P.5 if the second element of $\hat{\boldsymbol{\delta}}$ — that is, the estimated coefficient on the interaction term *disbursed* × *EIP-financed trades* — is significantly positive. For models (1), (2), and (4), the estimated coefficient is indeed positive and significant at the 5% level. Therefore, there is evidence for Prediction P.5: all else equal, exchanges with more treated trades have higher returns over the disbursement period.

Interestingly, we find a strong negative coefficient on lagged excess log-return. This suggests that exchange-specific returns are mean-reverting: if an exchange sees a higher BTCUSD return than the market on day t, it is likely to underperform on day $t + 1$. Most likely, this is due to the existence of arbitrageurs between exchanges. To our knowledge, this finding is new to the literature on cryptocurrency pricing factors.[35]

In model (3), the coefficient is not significantly different to zero, and the fit, as measured by adjusted R^2, is close to perfect. Daily time fixed effects explain almost all of the variation in returns, perhaps because there is much more variation in returns across days than between exchanges. This

[34] In an earlier version of this paper — published as Federal Reserve Bank of Cleveland working paper no. 21-13 — we ran a different version of this regression. In particular, we used observations at the hourly level. We think a daily level is better, because it provides for a better comparison between exchanges. The exchanges in our data set encompass a wide variety of time zones (see Table 2), so their liquidity varies cyclically over a 24-hour day, depending on when their users are most active. And this within-exchange variation is correlated with EIP disbursement, since users of US-based exchanges are more likely to receive EIPs. To prevent this issue from confounding our results, we estimate the model with observations at a daily level.

[35] Liu et al. (2021) look at a cross-section of cryptocurrencies, using average prices across all exchanges, and find a short-term momentum factor and a long-term reversal factor. In contrast, we look at a cross-section of exchanges in a single currency and find evidence for a short-term reversal factor. See Biais et al. (2022) and Hu et al. (2019) for more discussion of cryptocurrency pricing.

means daily time fixed effects drown out any observable effect of EIPs on returns. Figure 3 helps us understand why: because the amount of EIPs varies over time, the daily time fixed effects are correlated with the variable of interest. To prevent these fixed effects from confounding our results, we focus on model (4), which still includes time fixed effects but at a weekly level.

Using the round number cutoffs described in Section 5.2, we still obtain positive coefficients, but do not have statistical significance at the 5% level.[36] This is because the identification of EIPs with these round number cutoffs is weaker than for $1,200. It is possible, for example, that as the Bitcoin price recovers during the disbursement period, there is more trading for round number amounts for reasons unrelated to EIPs. Therefore, these cutoffs provide less reliable tests of Prediction P.5 than the $1,200 cutoff.

We can estimate the overall impact of EIPs on price using the results from Sections 4.2 and 5.2, which provide estimates of ΔEIP for each cutoff. That is, the estimated total cumulative impact on Bitcoin log-returns is $\sum_c \delta_c \hat{\hat{\delta}}_c$, where c indexes the cutoffs, δ_c is the second element of $\boldsymbol{\delta}$ estimated from Equation (2), and $\hat{\hat{\delta}}_c$ is the second element of $\hat{\hat{\boldsymbol{\delta}}}$ estimated from Equation (8). For each regression output, we use the outputs from model (4). Our estimate for the cumulative total impact on the Bitcoin return is 0.63 percentage points. In other words, on June 5, 2020, the Bitcoin price is 0.63pp higher than it would have been in the absence of an EIP program.

Our estimated price impact of 0.63 percentage points is not inconsiderable, bearing in mind several mitigating factors. First, our methodology tends to underestimate the true impact, because we only test for certain cutoffs. Second, most of the EIPs are paid out early in the disbursement period, so the initial price impact may be larger than measured here. Third, the EIP shock affects only retail investors: a similar shock that affects institutional investors could have an even larger price impact. Fourth, the shock is publicly observable, since the CARES Act and stimulus check program were widely covered among US news services. This suggests that Bitcoin prices ought not to be too sensitive to the increased trading.[37] We compute a 95% confidence interval for the price impact of $[0.03, 1.24]$ percentage points. We conclude the true impact is probably close to 1 percentage point.

Another reason our methodology probably underestimates the true price impact is that our methodology assumes 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 measure a price impact of zero. The literature suggests are substantial barriers to trade between cryptocurrency exchanges. For example, Makarov and Schoar (2020) discuss cross-border capital controls, while Hautsch et al. (2018) show that the limited settlement capacity of the Bitcoin blockchain constrains the ability of traders to realize arbitrage opportunities. While the significant reversion term in our results suggests arbitrageurs

[36]See Tables A.12, A.13, A.14, and A.15 in the Online Appendix. For $1,000, we do have significance at the 10% level. Significance appears to rise monotonically with cutoff.

[37]Having said that, the Bitcoin price can be sensitive to news and market liquidity. Over our data period, the standard deviation of the one-day return on the Bitcoin closing price is 4.6 percentage points, much larger than the estimated cumulative impact of the EIP program.

are somewhat effective, the reversion is only partial, and with a one-day lag.

Our results have nothing to say about the extent to which the EIP program permanently affects the Bitcoin price. Our methodology cannot preclude, for example, that agents sell their Bitcoin soon after our data set ends, in which case the price impact is unlikely to be 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. Assuming the same here, this suggests that the EIP program permanently increases the price of Bitcoin by about 0.21 percentage points.

6.3 Price elasticity of demand for Bitcoin

Our results allow us to estimate the price elasticity of demand for Bitcoin over the EIP episode. As the average market cap of Bitcoin over the disbursement period was $156bn, we estimate a price elasticity of demand of 0.06. In other words, an increase in demand of 1 percentage point raises the price by about 17 percent. Demand for Bitcoin is thus much less elastic than for stocks, suggesting our estimated increase in price is actually very large given the amount of EIP money spent on Bitcoin.[38]

We conclude that the demand curve for Bitcoin is steeply downward sloping, at least in the short term. Given the high volatility of the Bitcoin price, it is not too surprising that demand should be relatively unresponsive to price. A downward-sloping demand curve could be explained by a lack of direct substitutes for Bitcoin, or by the fact it is traded in segmented markets with limited arbitrage; see Wurgler and Zhuravskaya (2002). Downward-sloping demand for stocks can also be explained by asymmetric information, but that seems unlikely in our case, since any such information would affect the execution prices of both treated and control trades.

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 similar to the US Economic Impact Payment program. In this section, we analyze whether these programs influenced Bitcoin trading.

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.[39] 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.

[38]Greenwood et al. (2022), using a different approach, compute the price elasticity of stocks during the first round of EIPs to be 0.23 (or, more precisely, they give the inverse elasticity as 4.40). Their estimate is in line with the wider literature on the demand curve for stocks. Like us, Ilk et al. (2021) find that Bitcoin demand is relatively inelastic. Benetton and Compiani (2020) and Jermann (2021) also compute elasticities of demand for Bitcoin, using different approaches and definitions.

[39]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.

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.[40] 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 examining how beneficiaries of these programs used their money. Findings are similar to studies on the US EIP program. Feldman and Heffetz (2020) study the Israeli scheme and show that much of the money is used to pay down debt. Kim et al. (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. In Japan, several papers exploit local variation in disbursement of the stimulus payments to estimate marginal propensities to consume. The common finding is that responses are heterogeneous and depend on individual recipients' financial circumstances. See Hattori et al. (2021), Kaneda et al. (2021), and Kubota et al. (2021).

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. 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. For more details, see Hattori et al. (2021) and Kubota et al. (2021).

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

[40] 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.

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. For more details, see Ministry of Finance (2020).

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.[41] The funds were not paid automatically, but had to be applied for within three months.[42]

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 et al. (2020) for more details.

South Korea is a particularly interesting case, because Bitcoin trading there is much more widespread than in the other countries in our study.[43] This implies that Koreans might have a higher propensity to invest any windfall in Bitcoin. On the other hand, the program's spending restrictions could limit any impact on the Bitcoin market. It seems unlikely that buying Bitcoin would have been possible under the program. Nonetheless, we might still expect to see an effect if households substitute one source of money for another. Kim et al. (2020) find little evidence that substitution occurs.

7.2 Results

We examine Predictions P.1 and P.2 for each of Japanese yen, Singapore dollars, and South Korean won. 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 ¥100,000, SG$600, and ₩400,000 for cutoffs. 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 $t = 0$

[41] On May 4, 2020, there were ₩1,229 to one US dollar.
[42] For information in English, see Cha and Shin (2020) and Yonhap News Agency (2020).
[43] See Moon (2018) and Stevenson and Lee (2019).

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, but Figure 7 suggests no evidence of an increase in Singapore dollar trades. As for Korean won, Figure 8 provides some evidence 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.[44]

Next, we run difference-in-differences estimations for the three countries. We Equation (2) with the USD_i replaced with a dummy for the relevant currency. In each case, we try various bandwidths and display selected values. None of the three currencies see a positive announcement effect, confirming Prediction P.2. Of these three currencies, the strongest 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.[45]

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 suggest two reasons why Singapore behaves differently from the US, Japan, and South Korea. 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.[46]

8 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

[44] 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.

[45] 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.

[46] 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 Allison (2020). 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.

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, leading to a statistically significant, but economically small, price rise. The decision to invest in Bitcoin is 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 stimulus check schemes, but not in countries without 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.

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 that only a small fraction 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: P. Baker (2020).

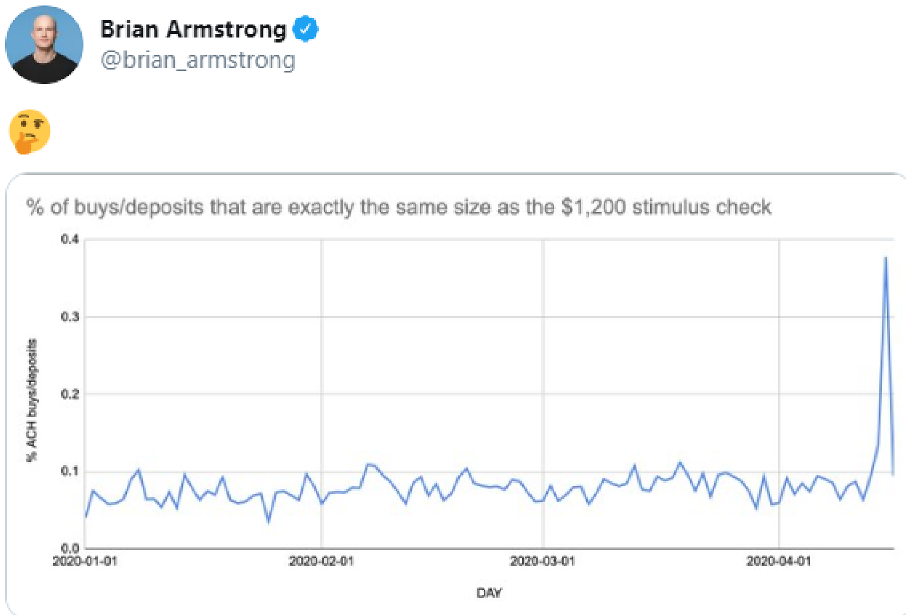

Figure 2: **Daily Bitcoin price and exchange activity**
Data are obtained from Blockchain.com and Yahoo! Finance, and span the period January 1 to June 30, 2020. Bitcoin price is shown as the solid line and plotted on the left-hand axis. Total Bitcoin trading across cryptocurrency exchanges (in US$ billions)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

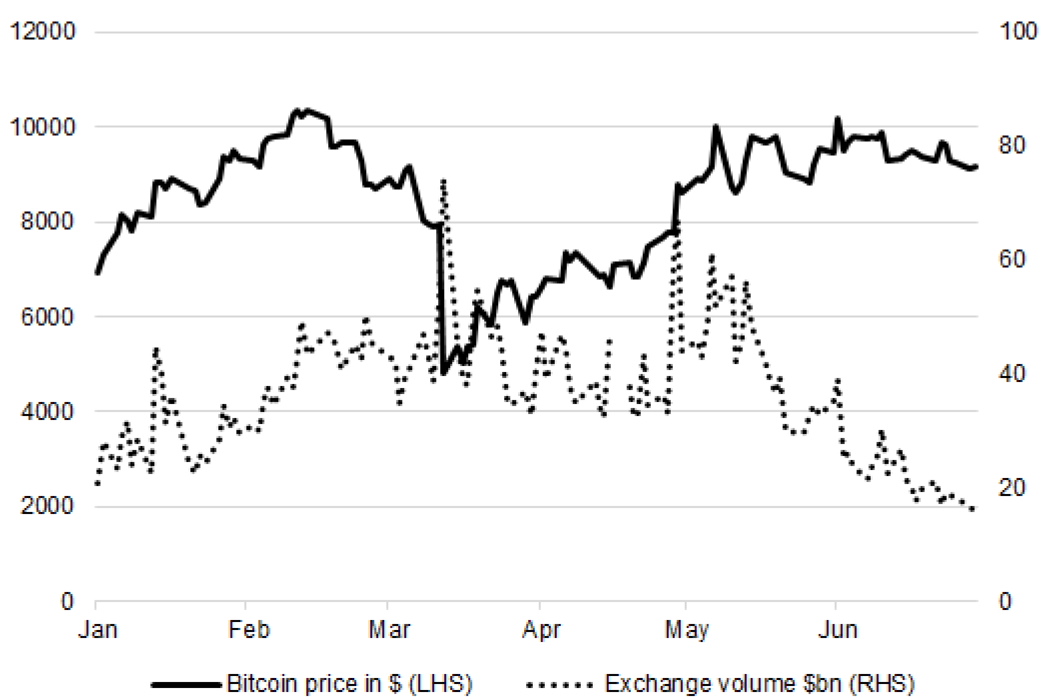

Figure 3: **Timeline of disbursement of economic impact payments**
Cumulative economic impact payments made, based on data released by the IRS on April 17, May 8, May 22, and June 3, 2020.

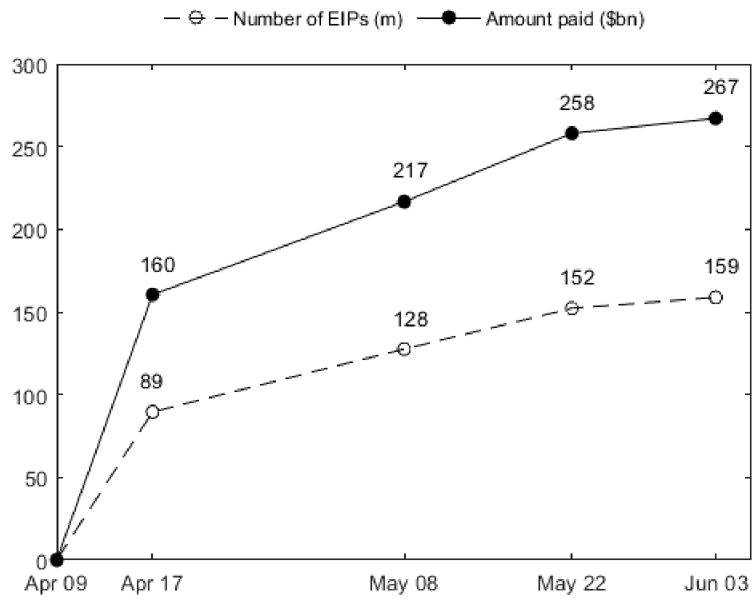

Figure 4: **Histogram of BTCUSD trades around passage of CARES Act**
Each panel shows the average number of Bitcoin trades per day in USD for individual trade amounts (rounded to the nearest ten). The first panel shows BTCUSD trades in the phase before the announcement of EIPs under the CARES Act, i.e., 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.

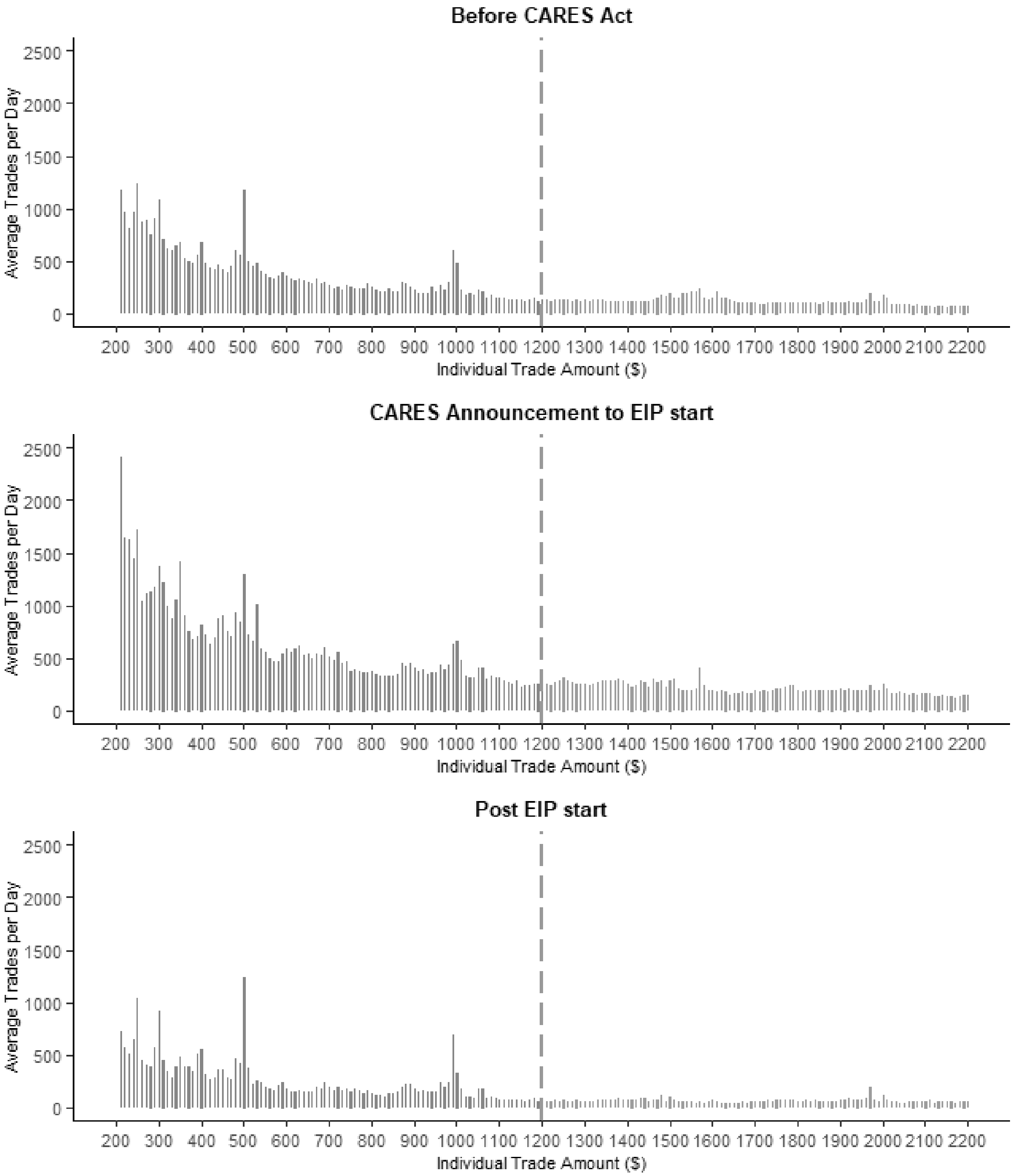

Figure 5: **Effects of economic impact payments on BTCUSD daily trade volume: $1,200 buy trades**

Figure plots estimated treatment effects λ_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.

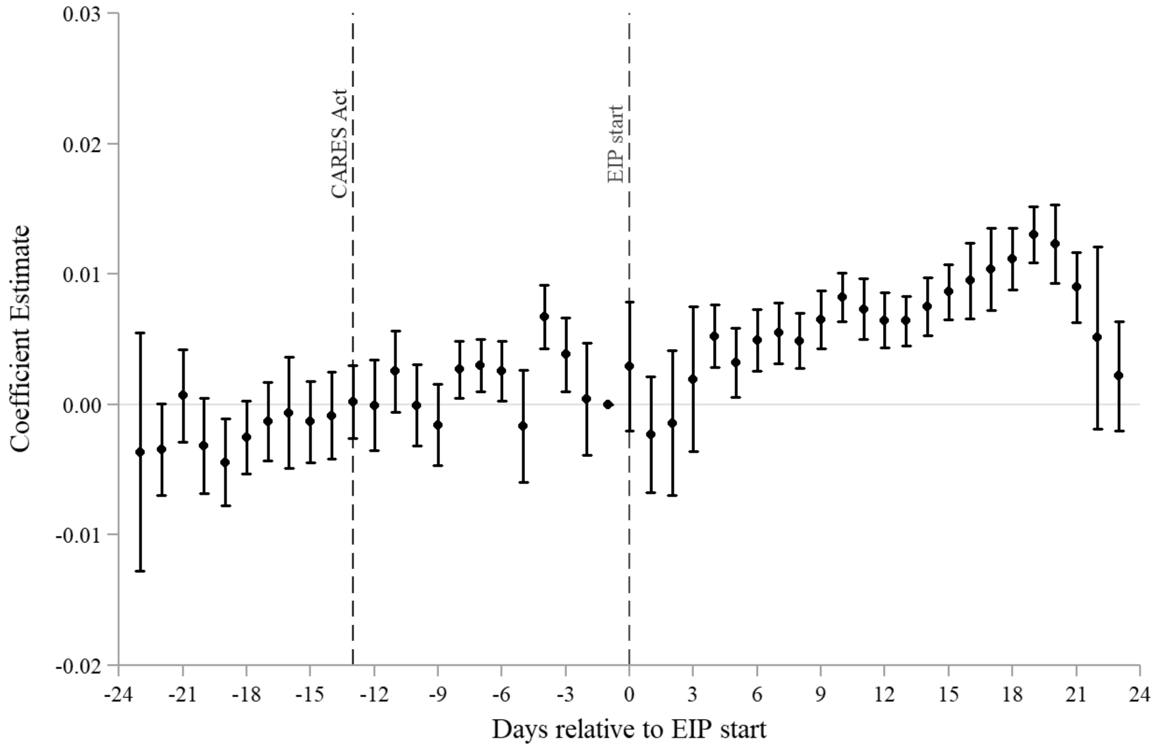

Figure 6: **Effects of Japan's Special Cash Payments program on BTCJPY daily trade volume: ¥100,000 buy trades**

Figure plots estimated treatment effects λ_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.

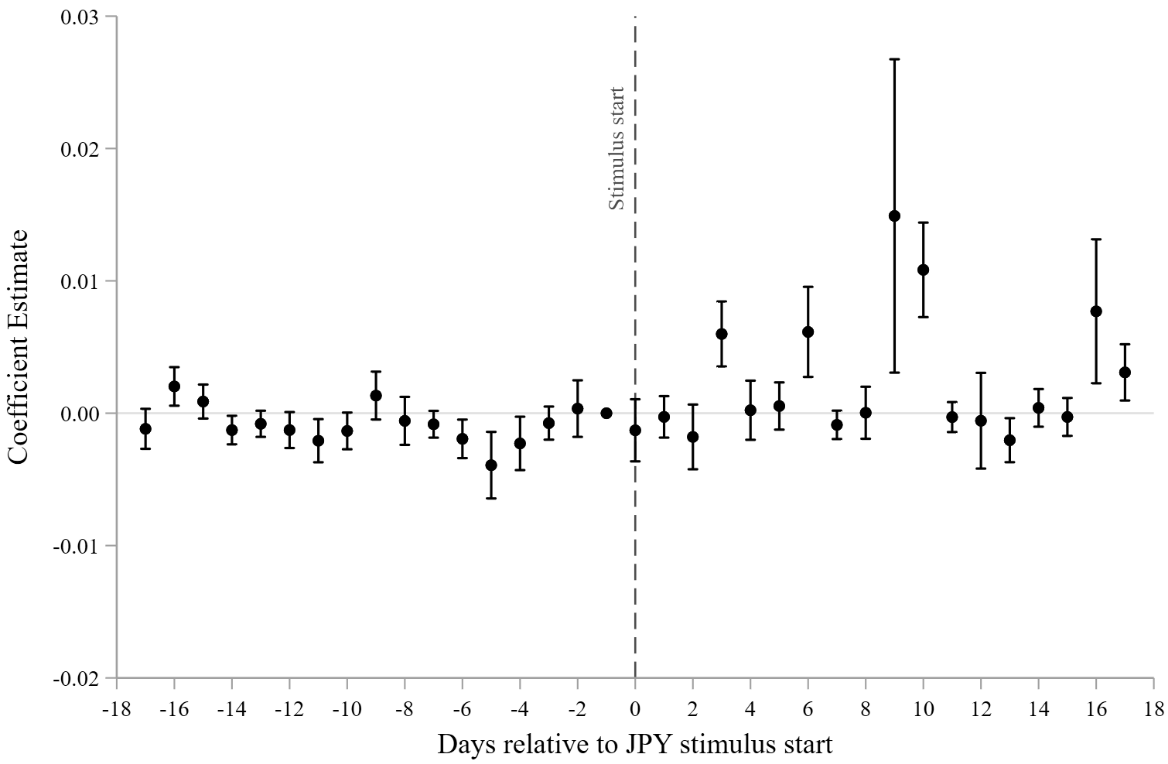

Figure 7: **Effects of Singapore's Solidarity Payment program on BTCSGD daily trade volume: SG$600 buy trades**

Figure plots estimated treatment effects λ_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.

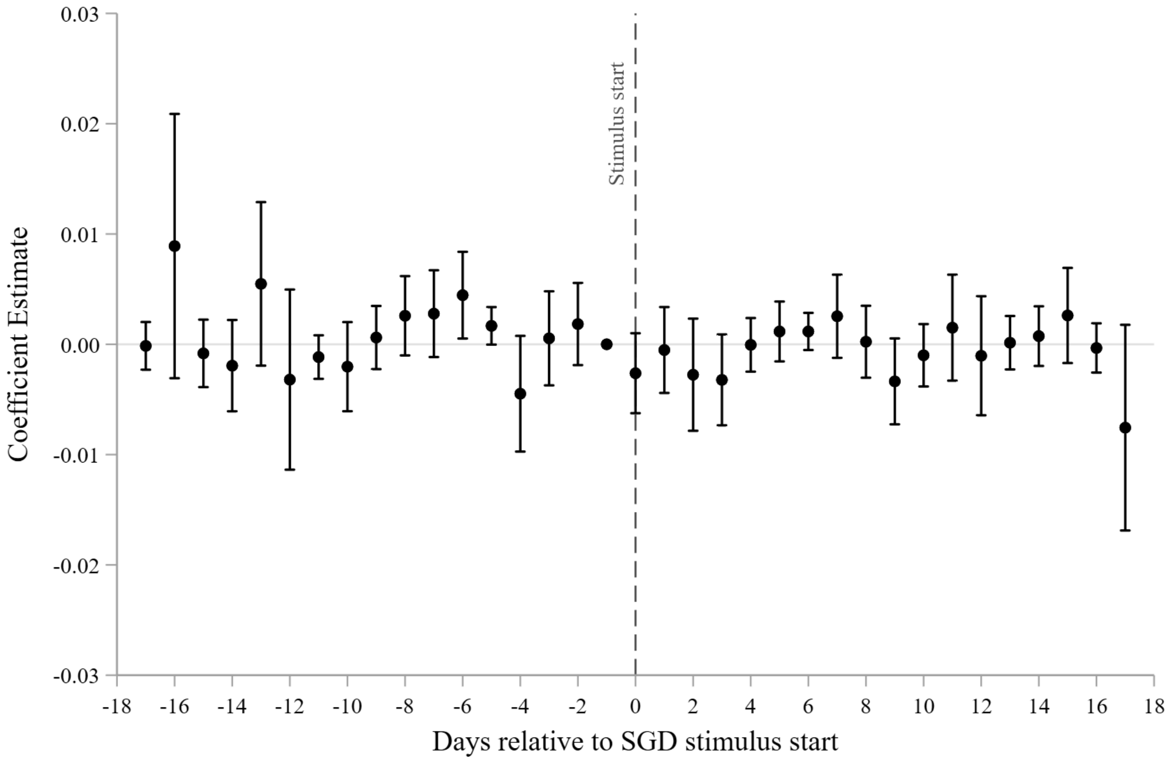

Figure 8: **Effects of South Korea's emergency disaster relief program on BTCKRW daily trade volume: ₩400,00 buy trades**

Figure plots estimated treatment effects λ_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.

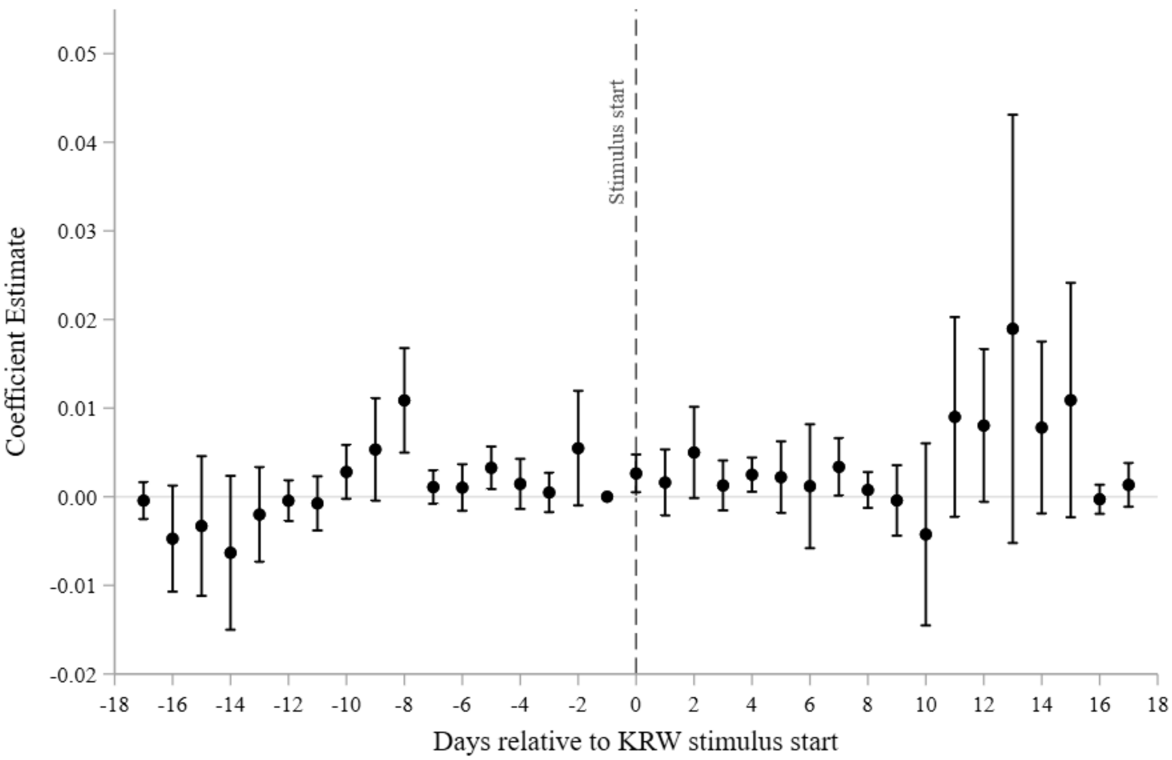

Table 1: **Calculation of economic impact payments**

Source: US Internal Revenue Service (IRS). More details can be found at Internal Revenue Service (2021). "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), and the trade is in USD. The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day t (i.e., April 9 or later) and the trade is in USD. 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), and the trade is in USD. The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day t (i.e., April 9 or later) and the trade is in USD. 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), and the trade is in USD. The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day t (i.e., April 9 or later) and the trade is in USD. 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), and the trade is in USD. The dummy *disbursed* is equal to 1 iff EIPs are being paid out on day t (i.e., April 9 or later) and the trade is in USD. 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**
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, where the coefficients δ are those obtained from logistic regressions with full fixed effects and a bandwidth equal to 5% of the cutoff. See Equations (3) and (4).

	Trade size				
	\$1,200	\$1,000	\$600	\$500	\$100
Estimated δ	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 return: \$1,200 buy trades**
Table presents difference-in-differences estimates of the effect of EIPs on Bitcoin return based on the specification outlined in Equation (8). The sample comprises Bitcoin buy trades in USD between January 1 to June 5, 2020. The dependent variable is the excess log-return of BTCUSD buy trades, as defined in Equation (6). The dummy *announced* is equal to 1 iff the CARES Act has been announced 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 (i.e., April 9 or later). The scalar variable *EIP-financed trades* is the estimated difference between the log-odds of the proportion of BTCUSD buy trades for treated amounts (between \$1,150 and \$1,200) and the log-odds of the proportion for control amounts (between \$1,200 and \$1,250), as defined in Equation (7). The regressions include exchange and date fixed effects. Standard errors are clustered by date and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	Excess log-return			
Model:	(1)	(2)	(3)	(4)
Variables				
announced	0.0011	0.0012	0.0000	0.0055***
	(0.0007)	(0.0009)	(0.0005)	(0.0017)
disbursed	-0.0000	0.0002	-0.0000	0.0026
	(0.0010)	(0.0008)	(0.0004)	(0.0017)
EIP-financed trades	-0.0017	-0.0017	-0.0000	-0.0019
	(0.0011)	(0.0011)	(0.0002)	(0.0011)
lagged excess log-return	-0.2763***	-0.2772***	-0.1303	-0.2834***
	(0.0066)	(0.0064)	(0.0815)	(0.0070)
announced × EIP-financed trades	0.0028	0.0028	-0.0000	0.0028
	(0.0021)	(0.0021)	(0.0004)	(0.0021)
disbursed × EIP-financed trades	0.0048**	0.0048**	0.0002	0.0050**
	(0.0020)	(0.0021)	(0.0004)	(0.0021)
Fixed effects				
exchange	No	Yes	Yes	Yes
day	No	No	Yes	No
week	No	No	No	Yes
Fit statistics				
Observations	1,796	1,796	1,796	1,796
Adjusted R^2	0.077	0.070	0.986	0.069

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), and the trade is in JPY. The dummy *disbursed* is equal to 1 iff Japanese stimulus payments are being paid out on day t (i.e., April 27 or later) and the trade is in JPY. 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), and the trade is in SGD. The dummy *disbursed* is equal to 1 iff Singaporean stimulus payments are being paid out on day t (i.e., April 14 or later) and the trade is in SGD. 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), and the trade is in KRW. 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) and the trade is in KRW. 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

ONLINE APPENDIX (Not for publication)

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.

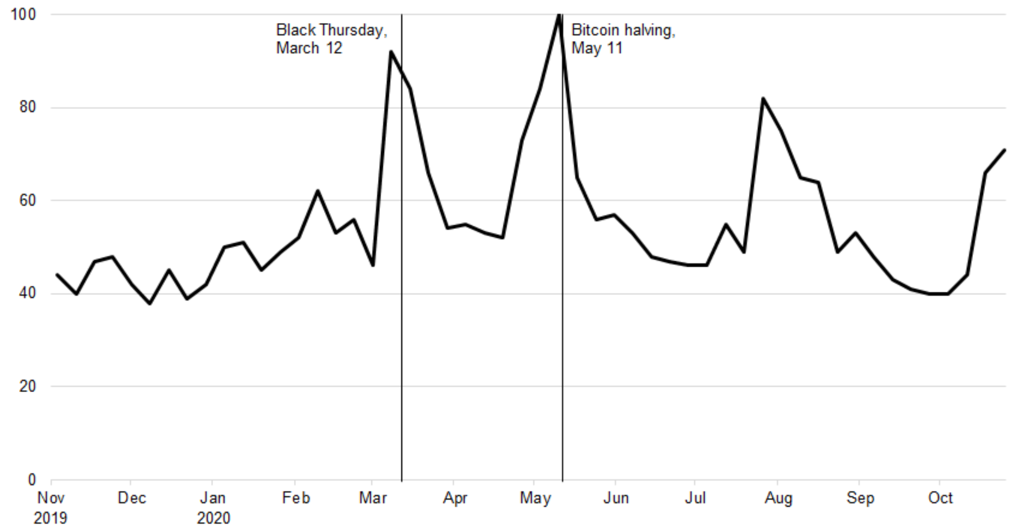

Table A.1: **Household sizes in the United States**
"Single" refers to households comprising a single adult person and no children. "Couple" refers to households with at least two adult persons (married or cohabiting). "Children" are defined as own children of the householder, under 18 years old. The data come from the 2019 American Community Survey. More details can be found at US Census Bureau (2021).

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. 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
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
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. 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)
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
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 (converted to the equivalent USD), that is, currencies issued by governments that did not run EIP-type programs. 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)
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
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 (converted to the equivalent USD), that is, currencies issued by governments that did not run EIP-type programs. 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)
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
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. 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)
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
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. 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
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
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. 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
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
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. 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
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
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. 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
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
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. 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
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
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 return: $1,000 buy trades**
Table presents difference-in-differences estimates of the effect of EIPs on Bitcoin return based on the specification outlined in Equation (8). The dependent variable is the excess log-return of BTCUSD buy trades, as defined in Equation (6). The scalar variable *EIP-financed trades* is the estimated difference between the log-odds of the proportion of BTCUSD buy trades for treated amounts (between $950 and $1,000) and the log-odds of the proportion for control amounts (between $1,000 and $1,050), as defined in Equation (7). The regressions include exchange and date fixed effects. Standard errors are clustered by date and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	Excess log-return			
Model:	(1)	(2)	(3)	(4)
announced	0.0000	0.0002	0.0010	0.0061**
	(0.0012)	(0.0015)	(0.0012)	(0.0024)
disbursed	0.0007	0.0014	0.0011	0.0049*
	(0.0007)	(0.0010)	(0.0012)	(0.0026)
EIP-financed trades	-0.0018	-0.0018	-0.0002	-0.0020
	(0.0013)	(0.0013)	(0.0003)	(0.0013)
lagged excess log-return	-0.2883***	-0.2887***	-0.1600*	-0.2933***
	(0.0037)	(0.0039)	(0.0769)	(0.0030)
announced × EIP-financed trades	-0.0046	-0.0045	0.0002	-0.0044
	(0.0032)	(0.0032)	(0.0006)	(0.0032)
disbursed × EIP-financed trades	0.0054*	0.0053*	0.0002	0.0053*
	(0.0030)	(0.0030)	(0.0004)	(0.0030)
Fixed effects				
exchange	No	Yes	Yes	Yes
day	No	No	Yes	No
week	No	No	No	Yes
Observations	1,897	1,897	1,897	1,897
Adjusted R^2	0.083	0.077	0.958	0.080

Table A.13: **Effect of economic impact payments on return: $600 buy trades**
Table presents difference-in-differences estimates of the effect of EIPs on Bitcoin return based on the specification outlined in Equation (8). The dependent variable is the excess log-return of BTCUSD buy trades, as defined in Equation (6). The scalar variable *EIP-financed trades* is the estimated difference between the log-odds of the proportion of BTCUSD buy trades for treated amounts (between $570 and $600) and the log-odds of the proportion for control amounts (between $600 and $630), as defined in Equation (7). The regressions include exchange and date fixed effects. Standard errors are clustered by date and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	Excess log-return			
Model:	(1)	(2)	(3)	(4)
announced	0.0009	0.0010	0.0009	0.0062**
	(0.0010)	(0.0013)	(0.0012)	(0.0026)
disbursed	0.0014**	0.0019*	0.0013	0.0063**
	(0.0005)	(0.0009)	(0.0012)	(0.0030)
EIP-financed trades	-0.0017	-0.0017	-0.0001	-0.0018
	(0.0021)	(0.0021)	(0.0002)	(0.0021)
lagged excess log-return	-0.2844***	-0.2846***	-0.1566*	-0.2901***
	(0.0036)	(0.0037)	(0.0826)	(0.0034)
announced × EIP-financed trades	-0.0034	-0.0034	-0.0003	-0.0034
	(0.0034)	(0.0035)	(0.0003)	(0.0035)
disbursed × EIP-financed trades	0.0028	0.0028	0.0002	0.0028
	(0.0023)	(0.0023)	(0.0003)	(0.0023)
Fixed effects				
exchange	No	Yes	Yes	Yes
day	No	No	Yes	No
week	No	No	No	Yes
Observations	1,834	1,834	1,834	1,834
Adjusted R^2	0.078	0.071	0.959	0.074

Table A.14: **Effect of economic impact payments on return: $500 buy trades**

Table presents difference-in-differences estimates of the effect of EIPs on Bitcoin return based on the specification outlined in Equation (8). The dependent variable is the excess log-return of BTCUSD buy trades, as defined in Equation (6). The scalar variable *EIP-financed trades* is the estimated difference between the log-odds of the proportion of BTCUSD buy trades for treated amounts (between $475 and $500) and the log-odds of the proportion for control amounts (between $500 and $525), as defined in Equation (7). The regressions include exchange and date fixed effects. Standard errors are clustered by date and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	Excess log-return			
Model:	(1)	(2)	(3)	(4)
announced	0.0009	0.0012	0.0010	0.0052**
	(0.0007)	(0.0010)	(0.0013)	(0.0018)
disbursed	0.0008	0.0013	0.0012	0.0043*
	(0.0006)	(0.0010)	(0.0013)	(0.0021)
EIP-financed trades	-0.0033*	-0.0033*	0.0009	-0.0034*
	(0.0017)	(0.0017)	(0.0011)	(0.0017)
lagged excess log-return	-0.2875***	-0.2878***	-0.1478*	-0.2924***
	(0.0044)	(0.0044)	(0.0722)	(0.0035)
announced × EIP-financed trades	-0.0001	-0.0001	-0.0015	-0.0002
	(0.0026)	(0.0026)	(0.0009)	(0.0026)
disbursed × EIP-financed trades	0.0026	0.0026	-0.0009	0.0025
	(0.0025)	(0.0025)	(0.0009)	(0.0025)
Fixed effects				
exchange	No	Yes	Yes	Yes
day	No	No	Yes	No
week	No	No	No	Yes
Observations	1,857	1,857	1,857	1,857
Adjusted R^2	0.082	0.075	0.959	0.075

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

Table presents difference-in-differences estimates of the effect of EIPs on Bitcoin return based on the specification outlined in Equation (8). The dependent variable is the excess log-return of BTCUSD buy trades, as defined in Equation (6). The scalar variable *EIP-financed trades* is the estimated difference between the log-odds of the proportion of BTCUSD buy trades for treated amounts (between $95 and $100) and the log-odds of the proportion for control amounts (between $100 and $105), as defined in Equation (7). The regressions include exchange and date fixed effects. Standard errors are clustered by date and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	Excess log-return			
Model:	(1)	(2)	(3)	(4)
announced	0.0005	0.0006	0.0007	0.0050***
	(0.0009)	(0.0009)	(0.0010)	(0.0016)
disbursed	0.0015***	0.0022**	0.0013	0.0054**
	(0.0005)	(0.0009)	(0.0010)	(0.0021)
EIP-financed trades	-0.0002	-0.0002	-0.0000	-0.0000
	(0.0010)	(0.0010)	(0.0003)	(0.0009)
lagged excess log-return	-0.2806***	-0.2810***	-0.1650*	-0.2863***
	(0.0071)	(0.0072)	(0.0831)	(0.0073)
announced × EIP-financed trades	0.0005	0.0004	-0.0004	0.0004
	(0.0016)	(0.0016)	(0.0003)	(0.0015)
disbursed × EIP-financed trades	0.0015	0.0014	-0.0003	0.0012
	(0.0025)	(0.0025)	(0.0004)	(0.0026)
Fixed effects				
exchange	No	Yes	Yes	Yes
day	No	No	Yes	No
week	No	No	No	Yes
Observations	1,930	1,930	1,930	1,930
Adjusted R^2	0.073	0.066	0.955	0.069