ONLINE APPENDIX
(Not for publication)

A Supplementary tables and figures

Figure A.1: **Tweet from Coinbase CEO on April 16, 2020**
Source: P. Baker (2020).

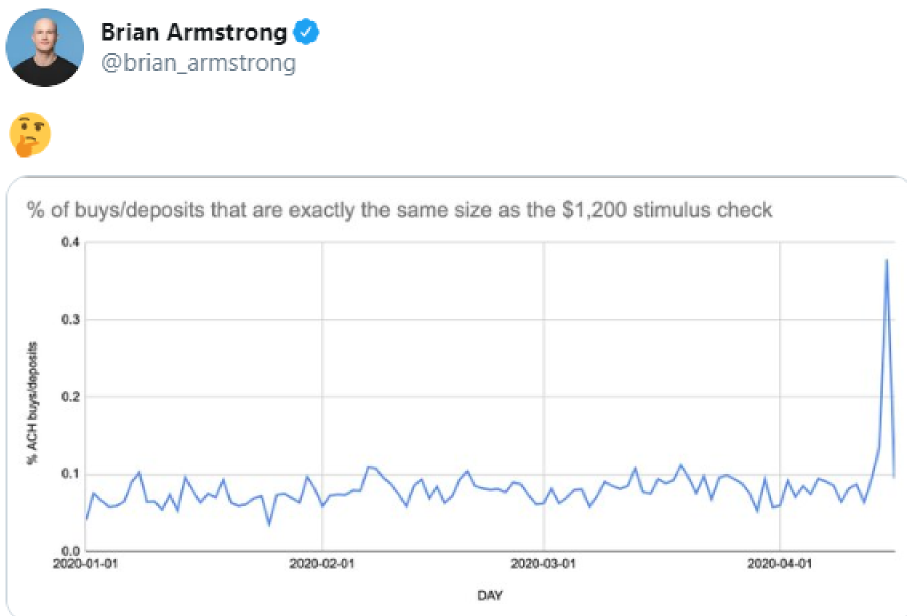

Figure A.2: **Daily Bitcoin price and exchange activity**
Data are obtained from Blockchain.com and Yahoo! Finance, and span the period January 1 to June 30, 2020. Bitcoin price is shown as the solid line and plotted on the left-hand axis. Total Bitcoin trading across cryptocurrency exchanges (in US$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

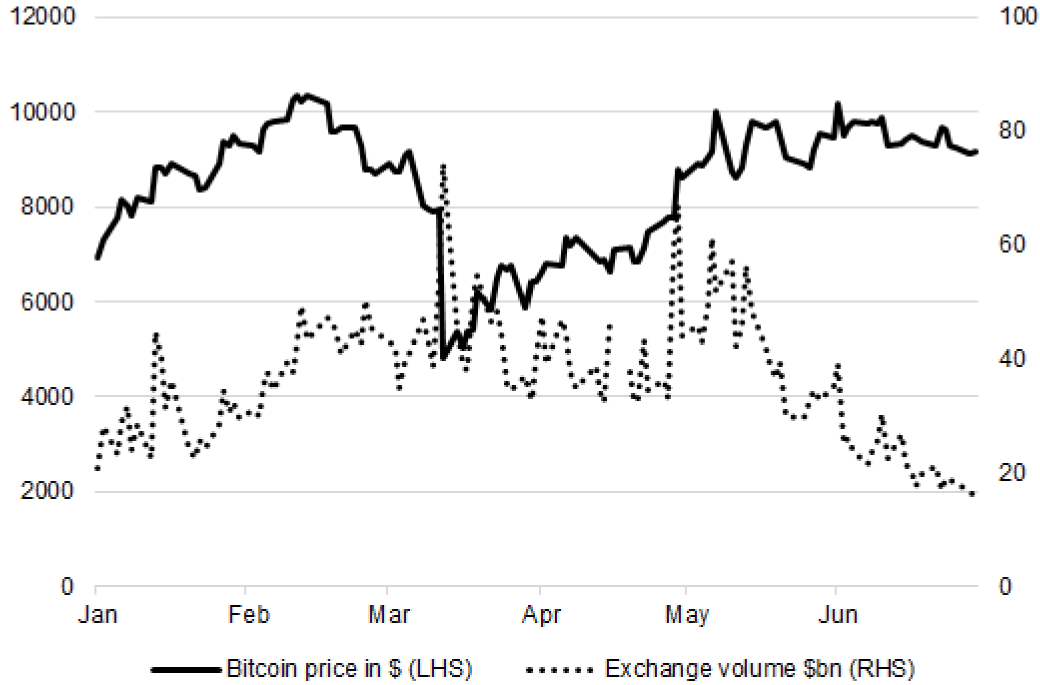

Figure A.3: **Google searches for the term "Bitcoin" in the US**
Data are obtained from Google search trends, and span the period Nov 1, 2019 to Oct 30, 2020. The chart shows a relative weekly measure of Google searches of the term "Bitcoin", with the peak of 100 on the week beginning May 10, 2020.

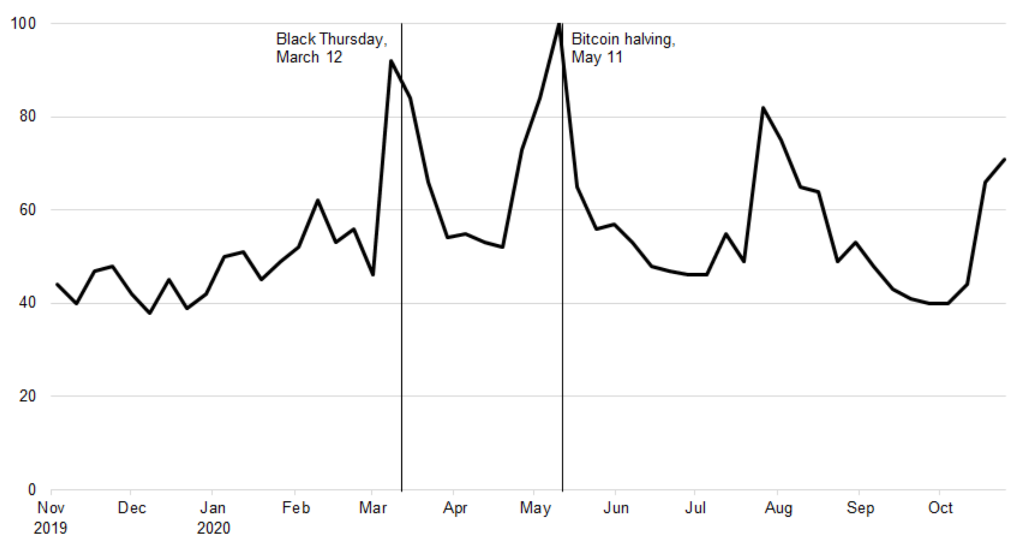

Table A.1: **Household sizes in the United States**

"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: **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.3: **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.4: **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.5: **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.6: **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.7: **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.8: **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.9: **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

B 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. Under the assumption that 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 we can consider Bitcoin traders around the world to be randomly assigned treatment, depending on whether their country introduces an EIP-like program. Therefore, we characterize our empirical setup as a quasi-natural experiment.

Gentilini et al. (2020) report that, as of June 12, 2020, five jurisdictions had introduced policies making one-off universal cash payments to households: Hong Kong, Japan, Serbia, Singapore, and South Korea.[40] Since then, Israel began its own program, and the US made two further rounds of payments. The various schemes are summarized in Table B.1. To our knowledge, no other country responded to the crisis by making direct payments to households with minimal eligibility conditions.

We study the effect of the programs in Japan, Singapore, and South Korea. We exclude Hong Kong and Israel, as well as the second and third US rounds, since those schemes did not begin to pay out until after our sample period ends.[41] In addition, we exclude Serbia since, according to our data set, there were no Bitcoin buy trades for Serbian dinar over the sample period. Throughout the paper, we treat the Hong Kong dollar and Israeli shekel as non-program currencies (see Table 4).

There are a few papers examining how beneficiaries of these programs used their money. These papers' findings tend to be similar to those that look at the US EIP program; i.e., stimulus checks had a limited effect on consumption. 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 (see Hattori et al. (2021), Kaneda et al. (2021), and Kubota et al. (2021)). These papers tend to find that responses are heterogeneous and depend on individual recipients' financial circumstances.

[40] The US is not included in the Gentilini et al. (2020) 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 for clarifying this.

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

B.1 Overview of programs in Japan, Singapore, and South Korea

B.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, meaning that residents had to actively apply. 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 for most payments to be made by the end of July 2020. For more details, see Hattori et al. (2021) and Kubota et al. (2021).

B.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 on the date of first disbursement (April 14). Individuals who had previously received government money automatically received their Solidarity Payments on April 14. This group 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 paper check would be posted on or after April 30. Additional money was available to Singaporeans, as well as some foreign permanent residents, based on age, income, and childcare responsibilities. For more details, see Ministry of Finance (2020).

B.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.[42] The funds were not paid automatically, but had to be applied for within three months.[43]

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

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

[43] For information in English, see Cha and Shin (2020) and Yonhap News Agency (2020).

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.[44] This implies that Koreans might have a higher propensity to invest any windfall in Bitcoin. On the other hand, the program's spending restrictions may well 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, although Kim et al. (2020) find little evidence that such substitution occurs.

B.2 Results

We examine Hypotheses H.1 and H.2 for each of Japanese yen, Singapore dollars, and South Korean won. For each currency, in the phase after stimulus check payments start, we expect to observe an increase in Bitcoin buy trades for amounts equal to and just under the amount paid to single individuals. Thus, we use ¥100,000, SG$600, and ₩400,000 as cutoffs in each case. As with the US program, we do not expect to see any effect during the announcement phase.

Using Equation (1), we run event studies for the three countries. In each case, we make $t = 0$ the disbursement date and set the bandwidth equal to 5 percent of the cutoff. Figure B.1 shows an increase in buy trades in Japanese yen following disbursement, but Figure B.2 suggests no evidence of an increase in Singapore dollar trades. As for Korean won, Figure B.3 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.[45]

Next, we run difference-in-differences estimations for the three countries. We test Equation (2) with USD_i replaced by 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 Hypothesis H.2. The Japanese yen and South Korean won see a consistently significant and positive effect during the disbursement phase, but the Singapore dollar does not. We conclude Hypothesis H.1 holds only for the Japanese yen and South Korean won.

For the Japanese yen, we find 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, albeit only at a 5 percent significance level (Table B.2). In contrast, the USD results are significant at a 1 percent level (Table 5). The difference could be explained by the decentralized disbursement of the Japanese Special Cash Payments. While the US EIPs were handled centrally by the IRS, the Japanese payments were handled locally at a prefectural level (see Hattori et al. (2021) for

[44] See, for example, Moon (2018) and Stevenson and Lee (2019).

[45] We use a window of 18 days, less than the 24 days for US, because of the short window between the Japanese announcement and disbursement dates. We do not show announcement dates in the figures, since for Singapore and South Korea the programs were announced long before the first payments were made.

details). The slight geographical differences in the timing of disbursement may confound our results somewhat.

In contrast, we find a significant positive effect for the Singapore dollar only at very large bandwidths (Table B.3).[46] We suggest two reasons why the stimulus checks appear not to have used to buy Bitcoin in Singapore. First, trading volumes in Singapore dollars are much lower than for the other three currencies (Table 4). Second, regulatory changes during our sample period may have caused a regime change in Singapore dollar trading, confounding our analysis. On January 28, 2020, the Singapore government introduced new rules requiring cryptocurrency businesses to be covered by anti-money laundering rules; see Allison (2020). This may have reduced Bitcoin trading: in our data set, mean daily SGD buy trade volumes fall from 280 buys (with a total value of SG$630k) during the phase prior to the budget announcement, to 82 buys (SG$111k) during the announcement phase, and 170 buys (SG$303k) after disbursement.

Of the three currencies considered in this section, the strongest results are for the Korean won, where the program has a highly significant and robust positive effect on trading (Table B.4). Coefficients are similar in magnitude to those for the US dollar (compare to Table 5). It seems the restrictions on how the Korean money could be used did not prevent people from diverting funds into Bitcoin. This is in apparent contrast to Kim et al. (2020), who find little evidence of substitution between use cases, though they do not examine Bitcoin trading. Possibly, enthusiasm for cryptocurrency trading in South Korea made this an exceptional case.

B.3 References

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B.4 Figures and tables

Figure B.1: **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.

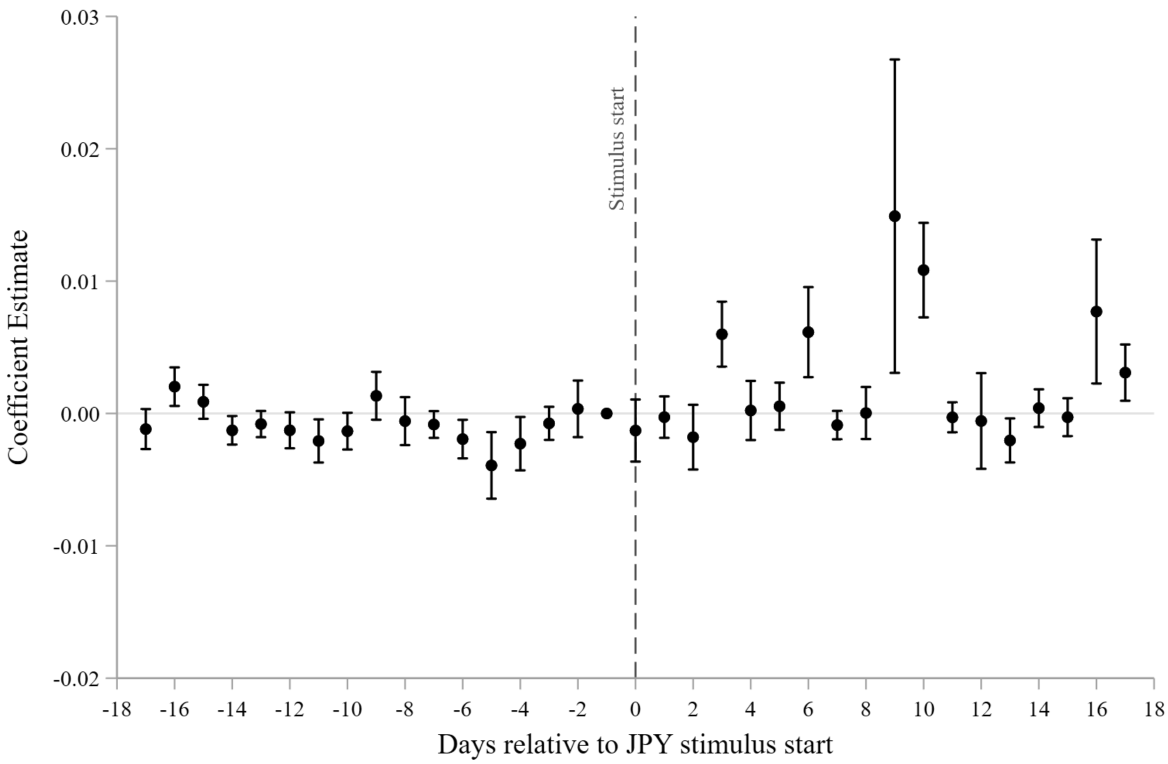

Figure B.2: **Effects of Singapore's Solidarity Payment program on BTCSGD daily trade volume: SG$600 buy trades**

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.

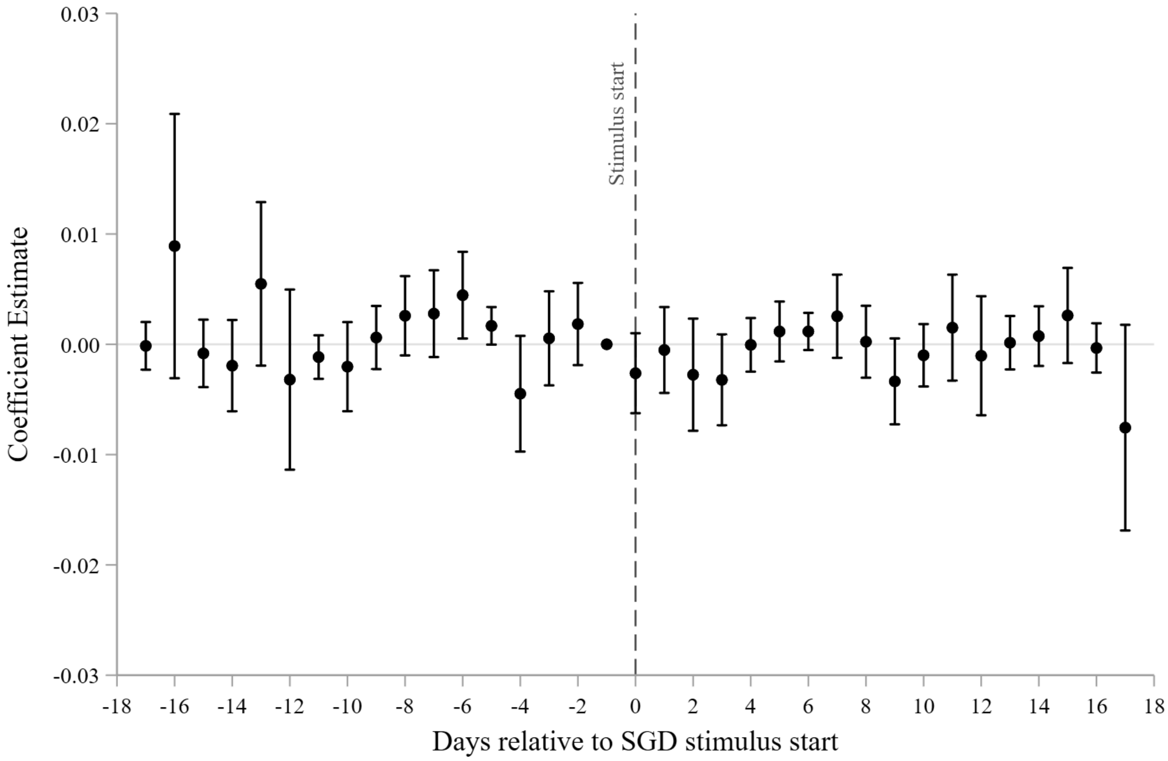

Figure B.3: **Effects of South Korea's emergency disaster relief program on BTCKRW daily trade volume: ₩400,00 buy trades**

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.

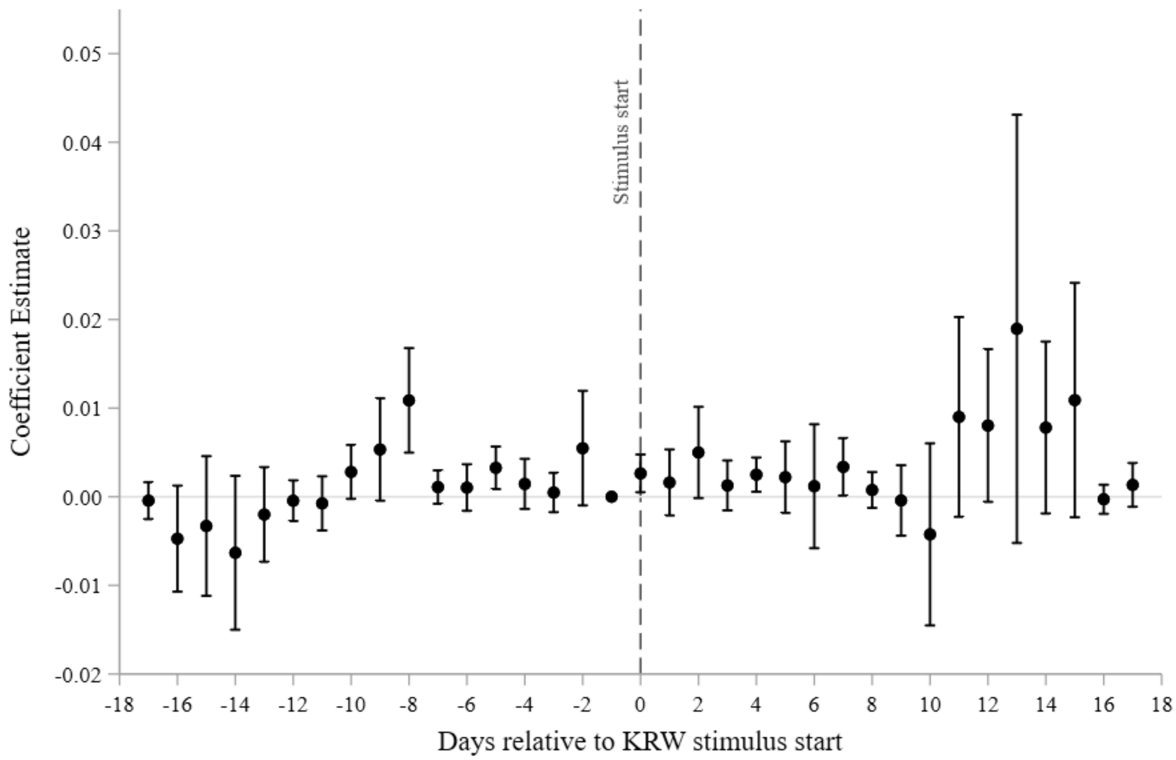

Table B.1: **Summary of direct payment programs in response to COVID-19 around the world**

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 B.2: **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 B.3: **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 B.4: **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

C Robustness and placebo checks

C.1 Robustness of results to changes in bandwidth

In Table C.1, we show EIPs still have a significant effect on Bitcoin trading when we vary the bandwidth, with a $1,200 cutoff. 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 (Hypothesis H.1) but not the announcement phase (Hypothesis H.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.

We carry out the same robustness check for the round number cutoffs described in Section 5.2. In each case, we vary the bandwidths using the same proportionate changes as in Table C.1; that is, we test bandwidths between 1.25 and 10 percent of the cutoff value.[47] With a $600 cutoff, the results are robust to changes in bandwidth (Table C.3), 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 C.2 and C.4), most likely because round number preference creates a challenge to identification at these cutoffs. The results for the $100 cutoff are more robust than the other round amounts (Table C.5).

C.2 Sell orders as a placebo test

As a placebo test, we repeat our regressions from Section 4.2 using sell orders, rather than buy orders. We would not expect to see any significant effect of EIPs on sell orders, because trades should be initiated by the EIP recipient, who is buying Bitcoin in exchange for cash. This makes sell trades a good candidate for a placebo test. If we were to obtain statistically significant results, that would suggest that some factor other than the EIP program is responsible for the observed change in Bitcoin trading behavior. Tables C.6 and C.7 show there is no significant increase in sell orders for amounts around $1,200 and $1,000, respectively, during the disbursement period.

C.3 Placebo tests on the period before the CARES Act was passed

We run our regression model on the period before the CARES Act was passed; i.e., the phase between January 1 and March 26, 2020. If our baseline results are really due to the EIPs and not some other factor, we would expect to see no significant effect on trading during this phase. We

[47]Note that larger bandwidths risk 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.

thank an anonymous referee for this suggestion.

Our regression design requires a control period to make a comparison. For example, in the baseline regression, we compare the second (announcement) and third (disbursement) phases to the first phase (pre-CARES Act). We split up the first phase (i.e., Jan 1 – Mar 26) into three new phases and run our model. Since it is not obvious what split to use, Table C.8 shows the results with three different ways of splitting up the pre-CARES Act phase. For example, the first column displays the results of a model that uses Jan 1–31 as the benchmark first phase, Feb 1–14 as the second (analogous to the 'announcement' phase in our baseline model), and Feb 15 – Mar 26 as the third phase (analogous to the 'disbursement' phase in our baseline model).

As an additional robustness check, we split the pre-CARES Act phase into two, rather than three, phases and run the regression models. We do this to increase the number of observations in each period, thus increasing the likelihood of obtaining significant results. Table C.9 shows the results. In none of the models is there a significant coefficient on either of the two interaction terms. We conclude there is no evidence of any effect during the pre-CARES Act phase. It appears that the proportion of treated trades, relative to control trades, remains broadly constant throughout this period of time.

C.4 Placebo tests on non-program currencies

The results for the round number cutoffs are weaker (albeit still statistically significant) than for $1,200. This raises the possibility that our results could be due to agents having stronger round number preferences when uncertainty is high (see Section 5.1), and our disbursement phase happens to coincide with a period of high uncertainty. If this were so, the EIP program would be irrelevant. That suggests we would expect to see a simultaneous increase in buy trades for round number amounts of other currencies that are not subject to a stimulus check program.

The concern described above is somewhat mitigated by the strong results for $1,200, as well as by the inclusion of the USD dummy in Equation (2). Moreover, we already address the concern to some extent in our baseline specification, because the dummies *announced* and *disbursed* are equal to 1 if and only if the date is in the relevant phase <u>and</u> the transaction is in USD. Therefore, our baseline results show there is an increase in USD trades in the treated group <u>relative to</u> any corresponding change in trades in other currencies. Nonetheless, we can address the concern more directly by testing trades in other currencies.

We test whether there is an increase in trades for round amounts of non-program currencies during the period coinciding with US EIP disbursement. We use data on buy trades in euros and British pounds sterling, as these are the non-program currencies with the highest trading volumes in our data set (see Table 4). We choose trade sizes of €1,000 and £1,000, and bandwidths of €50 and £50, respectively. The dummies *announced* and *disbursed* are defined with the same dates as the US EIP program. For the purposes of these two placebo tests only, we relabel EUR and GBP as program currencies.

As before, the regression design calls for the inclusion of all trades in non-program currencies corresponding to the treated and control group sizes, indexed by i in Equation (2). It turns out that our data set contains zero trades for these sizes in any non-program currencies.[48] This means there is no variation in currency in our regressions, and so we do not use currency fixed effects. We also do not need standalone terms for the phases since the time fixed effects take care of these.

Tables C.10 and C.11 show the results for euros and pounds, respectively. In neither case is the coefficient of the $disbursed \times treated$ interaction term significantly positive, so we reject the hypotheses of an increase in trades for €1,000 or £1,000 after April 9, 2020. This suggests the increase in USD round number trades we describe in Section 5 is most likely due to the EIPs, and not some unobserved global factor, like an increase in uncertainty or the halvening event, affecting round number preference. We conclude that such events do not drive our results, although we cannot rule out that they may magnify the effect of the EIPs.

C.5 Placebo tests on other round number cutoffs

We run placebo tests to verify that the round number cutoffs tested here are indeed special. There are three reasons to do this. First, the analysis in Section 5.2 could raise concerns that any amount we check would yield statistical significance, perhaps because volume is simply declining in trade size, as Figure 1 shows. Second, EIP recipients with families, who received larger payments, might have rounded down rather than invest the entire EIP in Bitcoin.[49]

We run our model for cutoffs of \$1,500, \$2,000, \$2,500, and \$3,000. The results are in Tables C.12, C.13, C.14, and C.15, respectively. In each case, we run the usual four models with various fixed effects, with bandwidths equal to 5 percent of the cutoff value. There is no robust evidence of any effect at these cutoffs.

As additional tests, we repeat our regressions with cutoffs for which 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 C.16 shows the results of these placebo tests.[50] In each case, there is no significance at the 5 percent level for the $disbursed \times treated$ interaction term. Interestingly, the *treated* dummy alone is not significant for these cutoffs (except for \$200), suggesting that these trade sizes are not focal values for investors.

We conclude that the cutoffs we test in the paper are indeed special, and that there is no evidence of EIP recipients with families investing part of their checks in Bitcoin. Of course, it is possible that EIP recipients with families invested smaller round numbers, such as \$500 and \$1,000, for which we do find significant results (see Section 5.2). We cannot rule this out, although the

[48] In other words, if we convert all trades in non-program currencies to EUR and GBP, there are no trades in either of the ranges € [950,1050] and £[950,1050]. This occurs because, as Table 4 shows, trading volumes are low in these non-program currencies.

[49] We thank two anonymous referees for suggesting these tests. The negative relationship between volume and trade size explains why, in almost all of our regression results, the *treated* dummy has a positive coefficient, sometimes significantly so. However, it cannot by itself explain why the interaction term $disbursed \times treated$ is significantly positive, unless there is some reason why volume decreases faster in trade size during the disbursement phase.

[50] For brevity, we present the results of the models with all fixed effects.

weight of evidence suggests it is likelier that these results are due to recipients of $1,200 EIPs.

C.6 Tables

Table C.1: **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 C.2: **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 C.3: **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 C.4: **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 C.5: **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 C.6: **Impact of economic impact payments on BTCUSD sell trade volume: $1,200 sell trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin sell trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin sell trades in currency i within group s (treated/control) on exchange j on day t, expressed as a proportion of the total number of sell trades for that same currency, exchange, and day. The sample comprises Bitcoin sell trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day t and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8), 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% level, respectively.

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

Table C.7: **Impact of economic impact payments on BTCUSD sell trade volume: $1,000 sell trades**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin sell trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin sell trades in currency i within group s (treated/control) on exchange j on day t, expressed as a proportion of the total number of sell trades for that same currency, exchange, and day. The sample comprises Bitcoin sell trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day t and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8), 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	-0.3739***	-0.2404**	-0.1960*	-0.0719
	(0.0964)	(0.1137)	(0.1126)	(0.1209)
disbursed	0.0110	0.1477**	0.1517**	-0.0360
	(0.0474)	(0.0745)	(0.0756)	(0.0858)
treated	0.0628	0.0493	0.0528	0.0447
	(0.0673)	(0.0660)	(0.0663)	(0.0643)
announced × treated	-0.1711*	-0.1588*	-0.1611*	-0.1643*
	(0.0923)	(0.0914)	(0.0916)	(0.0900)
disbursed × treated	0.0942	0.1000	0.0991	0.0991
	(0.0926)	(0.0911)	(0.0914)	(0.0895)
Fixed effects				
date	No	Yes	Yes	Yes
currency	No	No	Yes	Yes
exchange	No	No	No	Yes
Fit statistics				
Observations	7,658	7,658	7,658	7,658
R^2	0.012	0.081	0.086	0.242

Table C.8: **Placebo tests during pre-CARES Act phase: three sub-phases**

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 March 26, 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 '2nd phase' is equal to 1 iff the trade is in USD and t lies between the dates denoting the start of the 2nd phase (inclusive) and the start of the 3rd phase (exclusive). The dummy '3rd phase' is equal to 1 iff the trade is in USD and t is equal to or later than the start of the 3rd phase. 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		
	2nd phase: Feb 1 3rd phase: Feb 15	2nd phase: Feb 15 3rd phase: Mar 1	2nd phase: Mar 1 3rd phase: Mar 15
Model:	(1)	(2)	(3)
2nd phase	0.6047***	0.1479	0.2318
	(0.1322)	(0.1443)	(0.1711)
3rd phase	0.6967***	0.5930***	0.6034***
	(0.1170)	(0.1045)	(0.0944)
treated	0.0407	0.0461	0.0587
	(0.0659)	(0.0535)	(0.0427)
2nd phase × treated	-0.0215	0.0661	0.1574
	(0.1039)	(0.0747)	(0.1215)
3rd phase × treated	-0.0039	-0.0242	-0.0625
	(0.0767)	(0.0688)	(0.0625)
date	Yes	Yes	Yes
currency	Yes	Yes	Yes
exchange	Yes	Yes	Yes
Observations	5,414	5,414	5,414
R^2	0.422	0.414	0.415

Table C.9: **Placebo tests during pre-CARES Act phase: two sub-phases**

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 March 26, 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 '2nd phase' is equal to 1 iff the trade is in USD and t is equal to or later than the start of the 2nd phase. 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		
	2nd phase: Feb 1	2nd phase: Feb 15	2nd phase: Mar 1
Model:	(1)	(2)	(3)
2nd phase	0.6859***	0.5298***	0.5523***
	(0.1144)	(0.1040)	(0.0925)
treated	0.0407	0.0461	0.0587
	(0.0659)	(0.0535)	(0.0427)
2nd phase × treated	-0.0055	-0.0146	-0.0356
	(0.0752)	(0.0662)	(0.0611)
date	Yes	Yes	Yes
currency	Yes	Yes	Yes
exchange	Yes	Yes	Yes
Observations	5,414	5,414	5,414
R^2	0.422	0.404	0.413

Table C.10: **Impact of economic impact payments on BTCEUR buy volume: €1,000 buy trades**

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

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

Table C.11: **Impact of economic impact payments on BTCGBP buy volume: £1,000 buy trades**

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

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

Table C.12: **Effect of economic impact payments on BTCUSD trade volume: $1,500 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 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 $1,425 and $1,500 in size) and zero for control trades (between $1,500 and $1,575). 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)
announced	-0.1252	-0.3636**	-0.3967**	-0.1833
	(0.0763)	(0.1575)	(0.1534)	(0.1794)
disbursed	-0.2542***	-0.4114***	-0.3905***	-0.1204
	(0.0826)	(0.0806)	(0.0845)	(0.1060)
treated	0.4906***	0.4743***	0.2232***	0.2486***
	(0.0607)	(0.0581)	(0.0693)	(0.0668)
announced × treated	0.0347	0.0983	0.0349	0.1233
	(0.1752)	(0.1741)	(0.1771)	(0.1933)
disbursed × treated	0.0838	0.0923	0.1179	-0.0208
	(0.0807)	(0.0785)	(0.0767)	(0.0836)
date	No	Yes	Yes	Yes
currency	No	No	Yes	Yes
exchange	No	No	No	Yes
Observations	3,735	3,735	3,735	3,735
R^2	0.021	0.139	0.161	0.322

Table C.13: **Effect of economic impact payments on BTCUSD trade volume: $2,000 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 $1,900 and $2,000 in size) and zero for control trades (between $2,000 and $2,100). 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)
announced	0.2225	0.3356	0.4164	0.1635
	(0.2733)	(0.2669)	(0.2707)	(0.2724)
disbursed	0.0534	0.6837***	0.6641***	0.2551
	(0.2068)	(0.2023)	(0.2050)	(0.2089)
treated	0.5829***	0.5767***	0.7429***	0.6197***
	(0.0799)	(0.0757)	(0.0784)	(0.0797)
announced × treated	-0.1933	-0.1173	-0.0583	0.2984
	(0.2682)	(0.2357)	(0.2353)	(0.2225)
disbursed × treated	-0.0046	0.0082	-0.0083	0.1519
	(0.2023)	(0.1985)	(0.1976)	(0.1930)
date	No	Yes	Yes	Yes
currency	No	No	Yes	Yes
exchange	No	No	No	Yes
Observations	3,907	3,907	3,907	3,907
R^2	0.007	0.056	0.057	0.468

Table C.14: **Effect of economic impact payments on BTCUSD trade volume: $2,500 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 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 $2,375 and $2,500 in size) and zero for control trades (between $2,500 and $2,625). 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)
announced	-0.3603***	-0.3783***	-0.4200***	-0.4570**
	(0.1230)	(0.1360)	(0.1312)	(0.1811)
disbursed	-0.0392	0.0634	0.0645	-0.0148
	(0.1249)	(0.1263)	(0.1238)	(0.1758)
treated	0.2006**	0.2025**	0.0331	0.0045
	(0.0875)	(0.0851)	(0.0885)	(0.0890)
announced × treated	0.0299	0.0882	0.0244	0.2237
	(0.1469)	(0.1516)	(0.1606)	(0.1452)
disbursed × treated	0.1170	0.1036	0.1172	0.1904
	(0.1434)	(0.1418)	(0.1455)	(0.1399)
date	No	Yes	Yes	Yes
currency	No	No	Yes	Yes
exchange	No	No	No	Yes
Observations	4,479	4,479	4,479	4,479
R^2	0.003	0.040	0.052	0.280

Table C.15: **Effect of economic impact payments on BTCUSD trade volume: $3,000 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 $2,850 and $3,000 in size) and zero for control trades (between $3,000 and $3,150). 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)
announced	-0.5139***	-0.5689***	-0.5471***	-0.5870***
	(0.0952)	(0.1148)	(0.1190)	(0.0981)
disbursed	-0.2050***	-0.1115	-0.1130	-0.4141***
	(0.0723)	(0.0940)	(0.0929)	(0.1118)
treated	0.2347***	0.2284***	0.3036***	0.2379**
	(0.0803)	(0.0774)	(0.1037)	(0.1040)
announced × treated	0.2050	0.2804**	0.3191**	0.4312***
	(0.1448)	(0.1380)	(0.1340)	(0.1297)
disbursed × treated	0.2097	0.2134	0.2106	0.2719**
	(0.1326)	(0.1300)	(0.1345)	(0.1241)
date	No	Yes	Yes	Yes
currency	No	No	Yes	Yes
exchange	No	No	No	Yes
Observations	3,631	3,631	3,631	3,631
R^2	0.004	0.056	0.060	0.345

Table C.16: **Effect of economic impact payments on BTCUSD trade volume: arbitrary round number cutoffs**

Table presents difference-in-differences GLM estimates of the effect of EIPs on Bitcoin trading, based on the specification outlined in Equation (2). The dependent variable is the number of Bitcoin buy trades in currency i within group s (treated/control) on exchange j on day t, expressed as a proportion of the total number of buy trades for that same currency, exchange, and day. The sample comprises Bitcoin buy trades between January 1 to June 5, 2020 in USD and non-program currencies, that is, currencies issued by governments that did not run EIP-type programs. Trades in non-program currencies are converted to the equivalent USD amount at the prevailing exchange rate. The dummy *announced* is equal to 1 iff the CARES Act is announced by day t and EIP disbursement has not yet started (i.e., the phase between March 27 to April 8), 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. 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 dummy *treated* is equal to one for treated trades and zero for control trades. 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