Utility Disconnection Protections and the Incidence of Energy Insecurity in the United States

Even before the COVID-19 pandemic, rates of energy insecurity in the United States were a significant and growing problem in the United States, impacting roughly 1-in-3 households (EIA, 2015). The onset of the pandemic led to an economic recession the left millions out of work and further exacerbated rates of energy insecurity, including utility disconnections, especially among already vulnerable and disadvantaged populations (Memmott et al. 2021). In response, several states put in place emergency measures to prevent utility companies from disconnecting residential customers from their electricity service. The timing and stringency of these measures varied, with some disconnection moratorium policies expiring in the first couple of months of the pandemic and others being extended through 2021.

In this proposal, we aim to exploit variation in the start and expiration dates of disconnection protection policies to measure the effect of protections on rates of energy insecurity at the household level. Specifically, we assess the efficacy of protections on rates of utility disconnection, the manner in which households allocate spending across other essential household needs, and uptake of bill payment assistance. To evaluate these effects, we analyze original longitudinal survey data of a representative sample of U.S. households at or below 200% of the Federal Poverty Line (FPL). We match survey data on household composition and energy insecurity indicators to monthly state-level disconnection protection policies, including both emergency measures adopted due to the pandemic and standard seasonal protections.

This proposal will make several important contributions to the literature. Due to data limitations and other challenges, with the exception of a few notable studies (see Cicala, 2021; Jowers et al. 2021), few analyses have measured the impact of utility disconnection protections on rates of energy insecurity. In addition, these studies analyze zip-code and county level data

rather than data at the level of households. Additionally, we measure whether protections were specifically effective in benefiting populations who have previously been identified in the literature as experiencing higher rates of energy insecurity. Finally, our analysis provides policymakers with insights into how to design effective policies to address energy-related material hardship and protect vulnerable populations.

In the submission below, we present a draft manuscript of our proposal.

Introduction

In March 2020, to control the spread of the COVID-19 virus, several U.S. states and territories implemented public health mitigation policies, such as stay-at-home orders. These mandates severely limited domestic population movement (Lasry et al. 2020) by closing businesses, schools, and other places people gather. The stay-at-home orders provided public health benefits, including reductions in COVID-19 infections and fatalities (Fowler et al. 2020), but they also contributed to reduced economic activity, a spike in unemployment (Baek et al. 2020), increased financial worries (Tull et al. 2020), and adversely affected a household's ability to pay their monthly bills (Garner et al. 2020).

In response to the economic disruption caused by stay-at-home orders and the possibility that millions of Americans would be unable to pay their energy bills under such bleak economic conditions, many U.S. states enacted temporary measures to prevent regulated electric utilities from disconnecting their residential customers. Specifically, 35 states and the District of Columbia implemented moratoria to protect their residents from utility disconnection. These measures gradually expired throughout the latter months of 2020, even as the pandemic persisted. While these moratoria were temporary, most states placed limits on when, and under what circumstances, a regulated utility could shut off service to its customers. The degree and timing of these policies, however, varied. There is little empirical evidence that utility protections significantly minimize disconnections or provide households substantial relief from energy-related material hardship, often referred to as energy insecurity.

Although an analysis of utility disconnection protections on socio-economic outcomes seems straightforward, there is little extant scholarship. Data limitations are one of the key impediments; historically, few utilities have released information about disconnections and, even

in those cases, the data are not granular enough to link them to household-level characteristics. Two recent analyses, however, have taken advantage of variation in COVID-era, emergency, state-level utility protections as well as disconnection data disclosed by utilities during the COVID-19 pandemic. Jowers et al (2021) explored housing precarity across the U.S. during the pandemic, analyzing eviction and utility disconnection moratoria and its impacts on COVID-19 infections and related deaths. In another study, Cicala (2021) analyzed data reported by utilities in the state of Illinois to evaluate patterns of disconnections at the zip-code level.

In this analysis, we similarly evaluate the impact of the state-level COVID-specific disconnection protections on household wellbeing. We capture additional granularity by studying household-level data from a nationally-representative survey of low-income households, which, unlike past work, enables us to control for important household-level characteristics. Moreover, because this study is national in scale, we can leverage the heterogeneity in the scope and duration of the state-level temporary pandemic moratoria orders, which creates an opportunity to use spatial and temporal variation to estimate the policies' effects on various household-level outcomes, including a household's energy (in)security.

We address two primary research questions. First, and most directly, do utility disconnection protections result in fewer disconnections? Second, what are the socio-economic consequences of these protections? Specifically, do the protections curtail households' need to forgo other expenses and reduce their need to rely on financial assistance to pay their energy bills?

To study these questions, we merge monthly state-level utility disconnection protections with original survey data designed and collected by the authors. The survey is a nationally representative sample of households whose income are at or below 200 percent of the federal

poverty line (FPL). We collect data from the same respondents at four points in time between April/May of 2020 and May of 2021, roughly the first year of the pandemic. The survey contains measures of household-level composition and monthly indicators of whether a respondent reported being able to pay their energy bills and if they were disconnected from their electricity service. Due to the timeline of the study, we have the unique opportunity to consider the impact of both the state-level temporary emergency disconnection moratoria as well as default seasonal protections. Through a series of regressions, we estimate the impact of these policies for low-income households. Results from our empirical analysis suggest that, on average, when protections were in place, households were less likely to be disconnected from the electric grid and forgo basic food and healthcare expenses to pay an energy bill. We also find some suggestive evidence that disconnection protections reduced a respondent's reliance on social networks and government agencies for assistance to pay an energy bill.

This paper makes several important contributions. First, our findings provide some of the first estimates of the effects of disconnection protection policies, showing a relationship between disconnection policies and household-level socio-economic outcomes. While existing studies use county and zip-code level data (see e.g., Cicala 2021; Jowers et al. 2021), the present analysis provides evidence at the household-level. Second, these findings complement other recent work (e.g., Graff et al. 2021; Memmott et al. 2021; Hernández and Laird 2021) that has investigated disparities in residential energy insecurity by demonstrating that these policies specifically can benefit socially vulnerable populations. Finally, our analysis offers insights for policymakers on how disconnection protections can serve as a policy instrument to address material hardship among low-income households both during crisis situations, such as the COVID-19 pandemic, as well as under more typical circumstances.

Supporting Literature

Energy Insecurity

Recent data indicate that energy insecurity, or an inability of a household to meet its basic energy needs, affects one in three U.S. households (EIA 2015) and is a particularly prevalent form of domestic material hardship for low-income families (Hernández 2016; Memmott et al. 2021; Graff et al. 2021). Despite increased availability of public assistance, rates of energy insecurity were high during the COVID-19 pandemic, due to the economic disruption that resulted from policies to reduce the spread of the coronavirus. According to one set of estimates, during the first year of the pandemic, 24 percent of households within 200 percent of the FPL reported that they were unable to pay a monthly energy bill and almost 8 percent had their electricity disconnected by their utility due to nonpayment (Carley and Konisky 2021).

A growing body of research shows that energy insecure households are more likely to experience deleterious physical and mental health effects (Liddell and Morris 2010), including premature death (Berko 2014; Reames et al. 2021). In addition, people experiencing this specific type of material hardship often take on financial debt (Hernández 2016) and engage in risky coping behaviors to meet their energy needs, such as using a kitchen stove as a heating source (Campbell, 2016). Moreover, when faced with energy insecurity, households may be forced to choose between paying their utility bill and forgoing other basic household expenses, like purchasing food or medicine (Bhattacharya et al. 2003; Anderson et al. 2012; Hernández and Laird, 2021). Research also documents that vulnerable populations experience higher rates of energy insecurity, including households of color, those who live in inefficient housing conditions, those who rely on an electronic medical device, those with small children residing in

the home, those without a college degree, and the chronically unemployed or underemployed (Hernández et al. 2016; Reames 2016; Mohr 2018; Graff et al. 2021).

As states mandated people stay at home to contain the spread of the coronavirus, research shows that residential energy demand and bills increased substantially (Lou et al. 2021), thereby potentially growing the energy insecure population (Graff and Carley 2020). Data from the U.S. Census Bureau's *Household Pulse Survey* reveals that, between September and October 2021, over a year into the pandemic, approximately 21 percent of U.S. households still had to forgo expenses, such as food and medical care, to pay an energy bill; about 14 percent kept their homes at unsafe temperatures to lower their energy bills; and about 12 percent of households continue to be unable to afford their energy bill (Household Pulse Survey, 2021). Furthermore, Memmott et al. (2021) found that low-income vulnerable communities were hit the hardest during this period, with Black and Hispanic households, households with young children, and those living in energy inefficient homes more likely to face energy insecurity. The study also estimated that roughly 4% of low-income households had their energy services disconnected by their utility at the beginning of the coronavirus shutdown (April/May 2020), equating to an estimated 4.3 million individuals across the United States.

Despite the growth of scholarship related to energy insecurity, there are few empirical studies that measure the impacts of domestic disconnection protections specifically. This is due primarily to a lack of disconnection data collected and reported by state governments and utility providers. As noted in the introduction, recent studies serve as important exceptions. For example, Jowers and colleagues (2021) found that the disconnection protections implemented due to the COVID-19 pandemic reduced both infections and fatalities related to the virus and Cicala (2021) reported that Black and Hispanic households in the state of Illinois were 4 to 5

times more likely to be disconnected from the electric grid and 2 to 3 times more likely to be on deferred payment plans than white households.

Disconnection Protection Policies

To address concerns about energy insecurity at the beginning of the pandemic, many states implemented emergency shutoff moratoria that prohibited regulated utilities from disconnecting customers from their energy services. Under these protections, ostensibly, residents receiving service from a regulated utility could not be shut off by their utility provider for nonpayment. The emergency orders, however, were, at times, not fully implemented. Data from a recent study reveal that, despite a state-mandated moratorium, the number of disconnections declined but did not stop altogether across all regulated utilities in the state of Indiana (Baker et al. 2021).

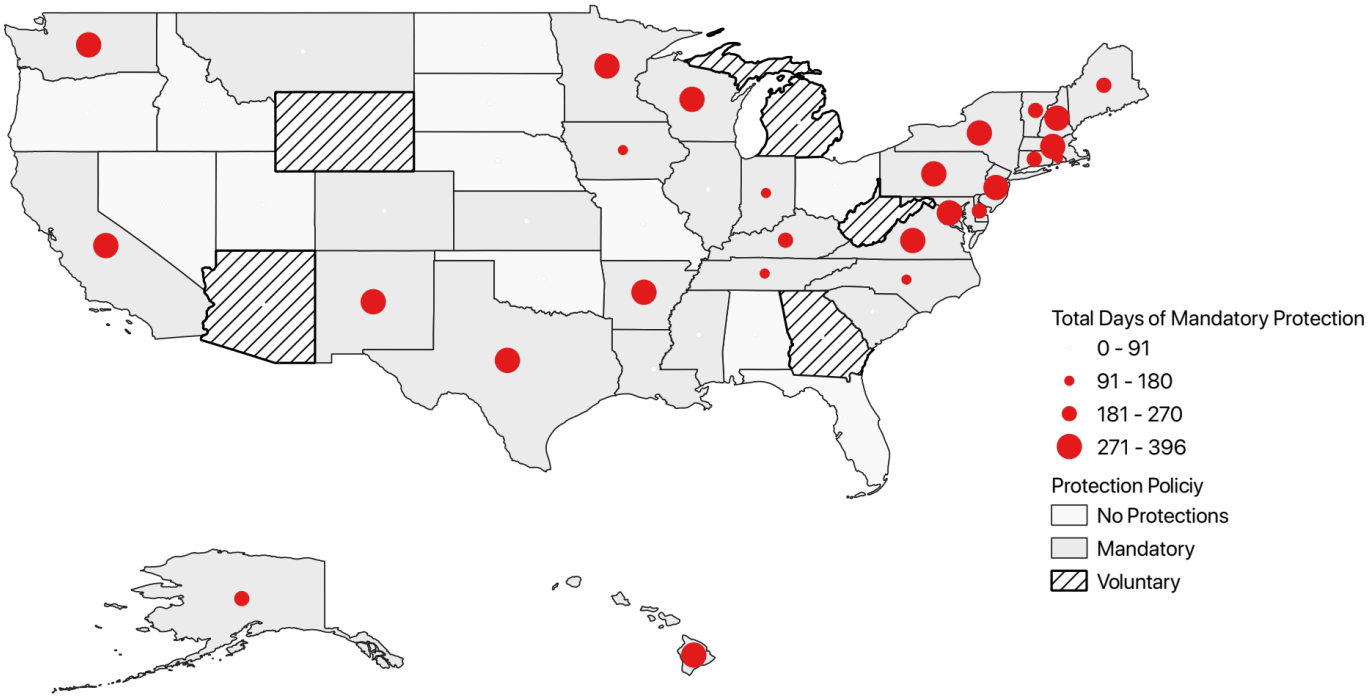
Additionally, there was heterogeneity in both the start and end dates of the moratoria. Some states (e.g., Colorado) only implemented protections in the early months of the pandemic, while other states extended protections through at least 2021 (e.g., California). Moreover, utilities subject to disconnection limitations also varied across states, since only those utilities regulated by state Public Utility Commissions (PUC) were required to abide by the emergency orders. For example, in the state of Arkansas, both investor-owned utilities (IOUs) and cooperatives are regulated by PUCs; whereas, in the state of Maryland all three utility types—IOUs, cooperatives, and municipal utilities—fall under state regulation. Thus, if residents living in Arkansas get their energy service via a municipal utility, they were not protected even though the state put in place a disconnection protection order.[1] In addition to mandatory moratoria, five

[1] Some utilities in states without emergency orders voluntarily put in place their own moratoria. Because we do not have data on these utility-initiated protections, our analysis will generate more conservative estimates of the treatment effect of the disconnections.

states implemented voluntary moratoria in which regulated utilities agreed but were not legally prohibited from shutting off customers in cases of nonpayment.

Figure 1 shows a map of the emergency utility disconnection orders implemented through January 2021, including whether a state had a mandatory utility disconnection order, voluntary agreement, or no protection in place. Thirty-three states had a mandatory protection in place in at least one month. The map reveals protections were more likely to be voluntary or nonexistent in the Southern and Plain states. Additionally, the most stringent—or those with the longest duration—protections were generally enacted in the Northeast, upper Midwest, and West Coast.

Figure 1: Map of mandatory and voluntary disconnection moratoria from May 2020 through May 2021



Note: The map identifies which states enacted mandatory, voluntary, or had no emergency COVID-19 utility disconnection protections. The size of the red bubbles indicates the duration of these policies in days, with larger red bubbles representing more days of protection for residences.

In addition to the COVID-19 emergency disconnection orders, over forty states have statutory-based utility disconnection protections that aim to limit shutoffs during specific times of the year or for vulnerable populations. There are three general categories of state-level protections: 1) seasonal protections (i.e., states prohibit regulated utilities from disconnecting electric service to residents in certain months of the year); 2) temperature protections (i.e., states prohibit regulated utilities from disconnecting electric service to residents if the temperature is above or below a certain threshold); and 3) population-based protections (i.e., states prohibit regulated utilities from disconnecting electric service to specific members of the population, including but not limited to senior citizens and those with specific medical conditions (LIHEAP 2020)). As of 2021, 27 states implement some form of seasonal protections and 24 have temperature-based protections, some of which overlap.

Often, these policies do not fully prohibit disconnections. Rather, they require customers to demonstrate eligibility for shutoff protection (Flaherty et al. 2020). For example, four states have no disconnection protections unless a household member has a physician or public health official certify that they would be adversely affected by a shutoff, requisite on providing proper documentation. And, again, it is important to emphasize that protections only apply to utilities under state jurisdiction. In all but one state, Nebraska, investor-owned utilities fall under state regulation, whereas only 11 states regulate municipal providers, and 16 states regulate cooperatives. Finally, 46 states and the District of Columbia allow customers to set up a payment plan as an alternative to disconnection (Flaherty et al. 2020), though these plans neither include long-term debt relief on the interest accrued for not paying in full nor are they adjusted based on the resident's income or ability to pay (Verclas and Hsieh 2018).

In the present analysis, we consider the effects of the COVID-specific mandatory and voluntary moratoria independently, as well as collectively with these pre-existing default seasonal protections. Our analysis specifically includes seasonal policies, which are defined at the monthly level, if the law protects all regulated customers, regardless of the amount they owe in utility fees, if they are facing financial hardship or are a qualified low-income customer. Eleven states had a seasonal protection in place in at least one month, though seven of these states had a disconnection moratorium that extended through the entirety of their seasonal protection period - Arkansas, Massachusetts, New York, Pennsylvania, Washington, Wisconsin, and Wyoming. We are not able to incorporate all seasonal disconnection protections[2] or any daily temperature-based protections[3] in our empirical evaluation because of the monthly structure of our survey data. Thus, over the duration of the study period, an individual may have been protected by a temporary emergency order or through a seasonal disconnection protection already in place. Figure 2 displays the timing of the emergency protections as well as the default seasonal protections during our study's time period for each state and Washington, D.C. (May 2020 to May 2021).

Figure 2: Mandatory (green), voluntary (red), and standard seasonal (blue) disconnection protections from May 2020 through May 2021, by state.

[2] An additional 18 states have date-based protections in place, but these policies do not protect all customers. Instead, these protections rely on household characteristics, such as an individual disability, or are contingent on meeting payment requirements. Due to data limitations discussed later in the article, these 18 states are not considered protected in our analysis.

[3] The omission of the daily temperature protections may bias the estimates in a conservative manner as households coded as unprotected may have experienced a day or days of protection from disconnection during the cold winter or hot summer months.



Note: Rectangles are monthly timelines, from May 2020 to May 2021, representing the timing and duration of each state and Washington, D.C.'s mandatory, voluntary, and default seasonal protections. Respectively, green, red, and blue lines indicate mandatory, voluntary, and seasonal protection lengths. An empty rectangle indicates that the state did not have any protection – temporary or otherwise – enacted during the study period.

Empirical Expectations

We exploit variation in COVID-19 pandemic disconnection policies as well as state-level seasonal protections to quantify the effect of utility shutoff protections. We expect these state-level utility protections to have three potential implications. First, we expect shutoff moratoria to significantly reduce disconnections. In addition, we expect that populations who tend to suffer from higher rates of energy insecurity will have benefitted the most from these protections and thus experience the largest decreases in their probability of being disconnected.

Second, disconnection moratoria should allow households to shift their spending from their energy bill to other essential goods, like food and medicine. Past research shows that low-income households are more sensitive to disruptive economic events (Rodems and Pfeffer, 2021) and those facing utility insecurity are more likely to engage in bill juggling—including strategic non- or partial- payment of other bills—to keep their electric service from being disconnected (Heflin et al. 2011). Because disconnection moratoria explicitly remove the risk that a household loses electricity service, low-income households could potentially redirect their spending to other household necessities (Ganong et al. 2020). This is especially salient for individuals and families who lost income or employment because of the economic fallout of the COVID-19 pandemic.

Third, we expect people covered by disconnection protections to have reduced their reliance on financial assistance to help pay off their energy bills. Under "normal" circumstances, energy insecure households often receive financial help from friends, family members, churches, or local nonprofits to avoid disconnections. Additionally, households sometimes seek assistance from more formal entities, including local government assistance programs, but these programs vary in their generosity, eligibility requirements, and availability. However, those with limited social networks are less likely to receive assistance during times of need (Middlemiss and Gillard

2015); therefore, without an immediate threat of disconnection, we hypothesize that households are less likely to reach out to informal social networks or apply to more formal government programs to pay their energy bills.

To summarize, we expect that the disconnection protection policies implemented in many states reduced the prevalence of disconnections among households that are served by a regulated utility, when compared to similar households in states without such protections. Moreover, we posit that these households are less likely to have forgone other important household necessities or to have solicited financial assistance to pay an energy bill. In the next section, we describe our research design to test these expectations.

Methodological Approach

Data

The data we analyze in this paper come from an original, panel survey of low-income households that we designed to examine energy insecurity during the pandemic. During each of four waves of the survey, we asked respondents a range of questions about their household composition, their economic circumstances, and to report several indicators of energy insecurity. We also asked respondents about their utility provider to match utility data to state disconnection moratoria.[4]

[4] The survey item capturing a respondent's utility type was included in wave 2 of the survey. For respondents that participated in wave 1 of the survey, we were able to assign their utility type using a couple of approaches. First, using geospatial files in QGIS, we assign a respondent a utility type if the household's zip code is fully contained within the service territory of single IOU, municipal utility, or cooperative utility. In cases where multiple utilities operate in a given zip code, we assign a utility type if more than half of the utilities in a single zip code are a single type (i.e., three utilities service a single zip code, and if two of three utilities in zip code are IOUs, assign it as an IOU). We drop the remaining 352 observations, which amounts to approximately 1.5 percent of the sample.

YouGov, a private polling and data analytics firm, administered the survey online. To create a nationally-representative sample from its standing panel, YouGov employs a two-stage process wherein the firm generates a target sample by drawing a random sample from a target population that is derived from the general population (for this case, using data from the 2017 American Community Survey). The firm then uses a matching algorithm to select potential respondents from its panel of approximately two million U.S. participants to generate a representative sample (Vavreck and Rivers, 2008). Scholars have validated extensively the underlying methodology that YouGov uses (see, e.g., Vavreck and Rivers, 2008; Ansolabehere and Rivers, 2013; Ansolabehere and Schaffner, 2014; Liu et al. 2010).

Because energy insecurity is more prevalent among low-income families (Ross et al. 2018), we designed the survey to focus on households with incomes at or below 200 percent of the FPL. Not only has past research has used 200 percent of the FPL as an indicator of low-income U.S. households (e.g., She and Livermore, 2007), but this income threshold is particularly relevant in the context of the present study because federal energy bill assistance programs, such as the Low Income Home Energy Assistance Program (LIHEAP), often set eligibility at 150 percent of the FPL, enabling us to consider households both above and below the threshold.

Table 1 summarizes the timing and sample size for each of the four waves of the panel survey. The first wave of the survey (n=2,831 respondents) was between April 30 and May 25, 2020;[5] the second wave of the survey between August 4 and August 20, 2020 (n=2,247

[5] Wave 1 combines data from two identical surveys that were fielded simultaneously, one national and one in the state of Indiana. Most of the sample is derived from the national sample (n=2,381) with the balance from the Indiana sample (n=450). The Indiana sample integrated into the national sample are respondents that were invited to participate in subsequent waves of the national survey (another 1,550 respondents from the original Indiana sample are excluded). The national sample is weighted to account for the Indiana oversample.

respondents); the third wave of the survey between January 15 and January 22, 2021 (n=1,670 respondents); and the fourth wave of the survey between May 24 and June 5, 2021 (n=1,378 respondents). The reduction in sample size over the course of the survey was anticipated. We pre-determined the general sample size for each wave to maintain a sufficiently large and nationally representative survey in each wave of the survey. YouGov generates post-stratified weights using propensity scores based on gender, race and ethnicity, age, geographic region, and education levels; we employ these weights in our analysis. We used an unbalanced panel in our analysis to preserve the original sample population and avoid potential issues that can arise due to survey attrition (Cheng and Trivedi, 2015).[6]

Table 1: Dates that each survey wave was administered, along with survey sample size and the number of months covered by each wave

	Wave 1	**Wave 2**	**Wave 3**	**Wave 4**
Survey Administration Date	4/30/2020 – 5/25/2020	8/4/2020 – 8/20/2020	1/15/2021 – 1/22/2021	5/24/2021 – 6/5/2021
Sample Size	2,831	2,247	1,670	1,378
Months Covered	April/May 2020	June-August 2020	September 2020-January 2021	February-May 2021

In each of the four survey waves, we asked respondents to reflect on the previous months and to report in which months they experienced certain events, such as utility disconnection, making trade-offs across food and medical care versus paying energy bills, and uptake of financial assistance. While such recall questions may be prone to error, we believe that respondents are likely to remember the general timing of these conditions and circumstances.[7] We also matched each survey respondent to identify their electric utility service provider, which

[6] We also provide an alternative estimation which uses a balanced panel of respondents who participated in all four waves. Results are consistent with the unbalanced panel and can be found an Appendix Table A1.

[7] To address potential recall bias, we also provide an alternative estimation which aggregates our data to the level of a survey wave and regress our outcome variables on the number of days that a respondent was protected during that wave. These models are further described in our Robustness Checks section.

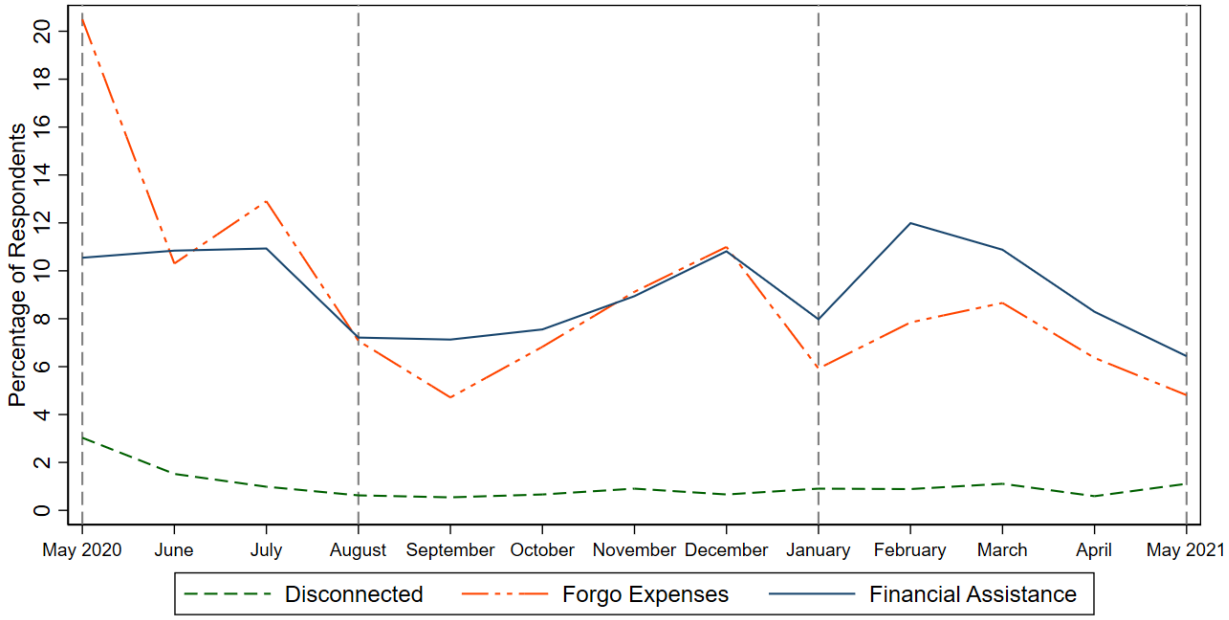
we then used to determine if their household was covered by a disconnection protection policy, as further described below.

Outcome variables

We employ three main outcome variables in the analysis, each based on measurement from the survey. To address the first research question, we use a variable that measures whether a survey respondent reported that their household was disconnected from its utility service in any month from May 2020 through May 2021. We use two other survey items to address the second research question of whether the presence of disconnection protections allowed residents to shift resources to other household needs and to reduce their reliance on financial assistance. The first variable captures whether respondents reported forgoing basic expenses, like food and medical care, to pay their energy bills in each month, and the second measures whether the respondent reported receiving financial assistance to pay their energy bill (e.g., from a government agency, their energy provider, a friend or family member, a faith-based organization, a nonprofit, a payday lender, or a loan from a banking institution) in each month.

During our study period, about 1.7 percent of respondents had their electricity service disconnected by their utility, and approximately 10 percent of respondents had to forgo basic household expenses to pay an energy bill and about 10 percent received financial assistance to pay an energy bill. For all three outcome variables, rates were higher among respondents who were not protected by a disconnection moratorium. Figure 3 graphs the outcome variables over the study period and shows that all three measures were at their highest in the first month of the pandemic (e.g., when lock-down orders were first put into place), and then fluctuated thereafter with some evidence of seasonal effects.

Figure 3: Distribution of survey respondents' being disconnected (green, dashed), having to forgo basic household expenses (red, alternating solid and dash), and receiving energy assistance from informal social networks (blue, solid), monthly from May 2020-May 2021.



Notes: The vertical lines represent the final month of each survey period (May 2020, August 2020, January 2021, and May 2021).

Primary Independent Variable

The treatment variable measures whether a respondent was covered under a state disconnection protection in each month. We employ three iterations of treatment, respectively. The first measures whether a respondent was covered by a COVID-related temporary mandatory disconnection moratorium, which we expect will drive much of the variation in the effect of protections, as mandatory protections are designed to prohibit any disconnections among protected populations. The second measures whether a respondent was covered by a COVID-related temporary voluntary or mandatory disconnection moratorium. We expect that adding voluntary protections, wherein utility companies agree not to shutoff respondents, will account for additional variation in our outcome variables. Finally, our third measure includes whether a respondent was covered by a COVID-related temporary mandatory or voluntary protection or a

default, seasonal protection. We think that including seasonal protections alongside the temporary COVID-related protections best captures the effect of disconnection protections.

To measure protection, we code a respondent as a "1" if their state had a disconnection protection in place and their utility service fell under state regulation and a "0" if the respondent was not covered in that month.[8] In our sample, which ranges from May 2020 through May 2021, about 56% of respondents were protected under a moratorium in at least one month.

The nature of our survey data do not enable us to formally check for parallel trends before the imposition of policies, but we can evaluate whether household characteristics differ among those in our survey population who were and were not covered by a COVID-19 temporary, mandatory, or voluntary disconnection moratorium in each month. Because there is within-state variation in protection based on type of utility, we are not concerned with state-level dispersion of policies. Instead, we consider household-level characteristics in our analyses to ensure that the households covered and not covered by moratoria are comparable. Table 2 compares the means of respondents based on key underlying characteristics—race, employment status, education, whether a household's income was at or below 100% of the FPL, whether the household had children under 5 in the household, and household size—for those who were and were not covered by a moratorium in May of 2020.[9] We do not find substantial difference in means, except in the case of utility provider type. This is a function of state regulatory policies, wherein IOUs are far more likely to fall under state regulation, and cooperative and municipal utilities are less so. Importantly, the results of Table 2 show a similar, or relatively balanced,

[8] A state is coded as covered if a protection was in place for 15 or more days. We use 15 as a cut-off because it represents respondents being protected against utility disconnection for at least half the days in a month.

[9] We also provide a balancing table of all observations in each month from May 2020 through May 2021 in Appendix Table A2.

distribution in sociodemographic characteristics that have previously been associated with energy insecurity – race, income, and having young children in the household – between the two groups of households. Thus, we would not expect differential rates of utility disconnection or energy-related financial hardship among those who were covered by a disconnection moratorium and those who were not based on pre-coverage characteristics.

Table 2: Distribution of pre-treatment control variables among respondents who were covered or not covered under state disconnection protection in May 2020

	Not Covered			**Covered**			
	count	mean	sd	count	mean	sd	Mean diff.
Black	1,021	0.17	0.37	1,321	0.15	0.35	0.02
Hispanic	1,021	0.15	0.36	1,321	0.12	0.32	0.03
Unemployed	1,021	0.20	0.40	1,321	0.20	0.40	0.00
Education	1,021	2.84	1.26	1,321	3.00	1.33	0.16
Under 100% FPL	1,021	0.36	0.48	1,321	0.36	0.48	0.00
Household member under 5	1,021	0.18	0.39	1,321	0.16	0.36	0.02
Household size	1,021	2.91	1.74	1,321	2.74	1.79	0.17
Cooperative	1,021	0.26	0.44	1,321	0.08	0.27	0.18
Muni	1,021	0.32	0.47	1,321	0.08	0.27	0.24
IOU	1,021	0.40	0.49	1,321	0.84	0.37	0.44

Control variables

In addition to our main regressors, we control for a number of household characteristics that may otherwise confound the relationship between disconnection protection policies and the energy insecurity outcomes. Specifically, we use three variables to control for whether a respondent received government assistance in the previous month, all of which we think would make a household less likely to experience energy insecurity. The first variable measures whether a respondent reported having received a COVID-19 recovery check issued by the U.S. Congress; the second measures whether a respondent noted having received funding from one of the two major federal energy assistance programs (i.e., the Weatherization Assistance Program (WAP) or LIHEAP); and the third variable indicates whether a respondent reported having

received another form of government assistance (specifically, assistance from the Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), Supplementary Security Income (SSI) or Social Security Disability Income (SSDI), Medicaid or Medicare, Veterans Benefit, or unemployment insurance). We also include several variables measuring sociodemographic characteristics that past scholarship has indicated are associated with energy insecurity: the respondent's race (Drehobl and Ross 2016), employment status (Memmott et al. 2021), educational obtainment (Hernández et al 2016), income (Hernández et al. 2014), household size (i.e., how many members are in the household) (Murray and Mills 2014), and whether the household has children under the age of 5 years old (Hernández 2016). Finally, we control for the utility type from which the respondent receives service. We present all variable definitions in Table 3.

Table 3: Descriptive statistics and variable definitions for all the variables used in the regression models, using survey weights

Variable	Description	Observations	Min	Max	Mean	SD
Disconnected	A binary variable set to 1 if the respondent was disconnected in the previous month	22,712	0	1	0.02	0.13
Forgo expenses	A binary variable to set to 1 if the respondent indicated that they had to forgo a basic household expenses to pay for an energy bill in the previous month	22,712	0	1	0.10	0.30
Financial Assistance	A binary variable set to if the respondent indicated that they received assistance paying their energy bill from a government agency, energy provider, a friend or family member, a faith-based organization, a nonprofit, a payday lender, or a loan from a banking institution	22,712	0	1	0.10	0.30
Mandatory	A binary variable set to 1 if the respondent was covered by a mandatory disconnection moratorium for at least 15 days in a given month	22,712	0	1	0.39	0.49
Mandatory + Voluntary	A binary variable set to 1 if the respondent was covered by a mandatory or voluntary disconnection moratorium for at least 15 days in a given month	22,712	0	1	0.41	0.49
Mandatory + Seasonal + Voluntary	A binary variable set to 1 if the respondent was covered by a mandatory, voluntary, or seasonal disconnection moratorium for at least 15 days in a given month	22,712	0	1	0.42	0.50
Recovery check (lagged one month)	A binary variable indicating whether a respondent received a CARES ACT Stimulus check in the previous month	22,699	0	1	0.30	0.66
WAP/LIHEAP (lagged one month)	A binary variable indicating whether a respondent received WAP or LIHEAP in the previous month	22,699	0	1	0.05	0.21
		22,699	0	1	0.38	0.48

Other Government Assistance (lagged one month)	A binary variable indicating whether a respondent received SNAP, TANF, SSI, SSDI, Medicaid or Medicare, Veterans Benefits, or unemployment insurance in the previous month					
Black	A binary variable set to 1 if the respondent indicated that they identify as Black	22,712	0	1	0.17	0.38
Hispanic	A binary variable set to 1 if the respondent indicated that they identify as Hispanic	22,712	0	1	0.19	0.40
Unemployed	A binary variable set to 1 if the respondent indicated that they were unemployed in the given month	22,712	1	1	0.17	0.37
Education	The level of education a respondent has obtained, ranging from no high school through a post graduate education	22,712	1	6	2.69	1.34
Under 100% FPL	A binary variable set to 1 if a respondent is under 100% of the Federal Poverty Line.	22,712	0	1	0.40	0.49
Household size	The number of individuals residing in an individual's household, ranging from 1 through 20	22,712	1	20	2.77	1.79
Children under 5	A binary variable set to 1 if the household has at least 1 child under 5 living in the household	22,712	0	1	0.15	0.36
Cooperative	A binary variable set to 1 if the respondent gets their utility services provided by a Cooperative	22,712	0	1	0.15	0.36
IOU	A binary variable set to 1 if the respondent gets their utility services provided by an Investor-Owned Utility	22,712	0	1	0.64	0.48

Model

To test our empirical expectations, we estimate a series of two-way fixed effects regression models which exploit heterogeneity in protections across states and over time, and our main treatment variable is whether a respondent was covered by disconnection protection policies. Specifically, we estimate the following regression:

$$Y_{ist} = \alpha + \beta_1 Policy_{ist} + B_2 GA_{ist-1} + \beta_3 X'_{ist} + \gamma_s + \delta_t + \epsilon_{ist},$$

where Y_{ist} represents one of our three binary outcome variables for a respondent *i* in state *s* and in month *t*. $Policy$ is a binary variable indicating if a respondent was covered by a disconnection protection,[10] GA represents whether a respondent received government assistance in the previous month, and X' is a vector of sociodemographic control variables. The model includes state and month fixed effects to control for unobserved variation across states (i.e., economic conditions) and over time (i.e., seasonal variation in temperature). In addition to the three primary models,

[10] In our baseline models, we consider a respondent as covered if a protection was in place for their utility for at least half of a month (i.e., 15 days). In subsequent models, we also consider the number of days covered, with substantively similar results.

we also assess the heterogeneity of our results along sociodemographic indicators, including households of color, households at or below 100% of the FPL, those who are unemployed, and households with children under the age of 5 years old. We use a linear probability model to estimate the main models.[11]

Following the presentation of our main results below, we also report the results of several robustness checks to further test for parallel trends and to address potential concerns that states adopted moratoria in response to prior rates of energy insecurity, differential within month coverage of protection policies, and recall bias in our self-reported outcome data.

Results

Table 4 presents the regression results with the first dependent variable: whether a household was disconnected from its utility service in any given month. For each dependent variable, we first measure utility protection as the temporary mandatory COVID-19 moratoria, and then add the voluntary protections followed by adding the default, standard seasonal protections.

The model estimates show that respondents covered by disconnection protections were less likely to report having been disconnected from their service, with minor variation in the effect sizes across the three models. To estimate the magnitude of the effects, we additionally estimate average marginal effects (AME) for several of our models. Based on the results in Table 4, AME estimation suggests that being protected by a mandatory moratorium reduced the likelihood of a household having their energy shutoff by about 2.4 percent, controlling for other factors.

[11] We also estimate logistic regression models. Results, which can be found in the appendix in Table A3, yield consistent results with our linear probability model.

The results in Table 4 additionally reveal that, even when controlling for policy protections, race and other vulnerable households are correlated with higher rates of self-reported disconnections. Specifically, we find that Black households, Hispanic households, households with children under 5 years old, larger households, and those that are served by cooperative utilities, relative to municipal utilities, were all more likely to be disconnected from their electricity service.

Table 4: Linear probability model predicting whether a respondent reported having their utility service disconnected.

	Model 1: Disconnected	Model 2: Disconnected	Model 3: Disconnected
Mandatory	-0.024***		
	(0.006)		
Mandatory + Voluntary		-0.023***	
		(0.006)	
Mandatory + Voluntary + Seasonal			-0.022***
			(0.006)
Stimulus lag	0.003	0.003	0.003
	(0.003)	(0.003)	(0.003)
WAP/LIHEAP lag	-0.002	-0.002	-0.001
	(0.005)	(0.005)	(0.005)
Other Government Assistance Lag	-0.001	-0.001	-0.001
	(0.003)	(0.003)	(0.003)
Black	0.019***	0.020***	0.020***
	(0.004)	(0.004)	(0.004)
Hispanic	0.012**	0.012**	0.012**
	(0.005)	(0.005)	(0.005)
Unemployed	0.002	0.002	0.002
	(0.004)	(0.004)	(0.004)
Education	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)
Under 100% FPL	-0.003	-0.003	-0.003
	(0.003)	(0.003)	(0.003)
Children under 5	0.042***	0.042***	0.042***
	(0.007)	(0.007)	(0.007)
Household size	0.002*	0.002*	0.002*
	(0.001)	(0.001)	(0.001)
IOU	0.005	0.006	0.005
	(0.003)	(0.004)	(0.003)
Cooperative	0.023***	0.024***	0.024***
	(0.006)	(0.006)	(0.007)
State FE?	Yes	Yes	Yes
Month FE?	Yes	Yes	Yes
Observations	22,699	22,699	22,699

Cells contain OLS regression coefficients, with robust standard errors in parentheses. Levels of statistical significance: * $p < 0.1$, ** $p < 0.05$ *** $p < 0.01$

In Table 5, we display the results of our estimation of the effect of disconnection protections on the likelihood that a household forgoes other basic household expenses (Models 1-3) and receives financial assistance to pay an energy bill (Models 4-6). These results indicate

that disconnection protections decreased the probability that a household reported having to forgo other basic expenses, which suggests that when people are less concerned about being disconnected, they can allocate their resources toward other household necessities such as medical care and food. Estimation of AMEs suggests that the likelihood of forgoing basic household expenses declined by about 2.4 percent for respondents who were protected by a mandatory disconnection moratorium. With respect to receiving financial assistance to help in paying an energy bill, the coefficients in Models 4 through 6 are negative but none reach a standard level of statistical significance. These estimates suggest that there may be an effect consistent with our expectations, but it is not definitive.

Table 5: Linear probability model predicting whether a respondent had to forgo a basic household expense, or received financial assistance to pay an energy bill in each month.

	Model 1: Forgo expenses	Model 2: Forgo expenses	Model 3: Forgo expenses	Model 4: Social assistance	Model 5: Social assistance	Model 6: Social assistance
Mandatory	-0.024**			-0.015		
	(0.009)			(0.010)		
Mandatory + Voluntary		-0.023**			-0.014	
		(0.009)			(0.006)	
Mandatory + Voluntary + Seasonal			-0.022**			-0.012
			(0.009)			(0.009)
Stimulus lag	0.004	0.004	0.005	-0.011*	-0.011*	-0.010*
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
WAP/LIHEAP lag	-0.007	-0.007	-0.005	0.018	0.018	0.016
	(0.013)	(0.013)	(0.013)	(0.015)	(0.015)	(0.015)
Other Government Assistance Lag	0.024***	0.024***	0.023***	0.005	0.005	0.005
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Black	0.014*	0.014*	0.014*	0.022***	0.022***	0.022***
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)
Hispanic	0.031***	0.031***	0.031***	-0.011	-0.011	-0.011
	(0.010)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)
Unemployed	0.027***	0.027***	0.027***	0.011	0.011	0.011
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
Education	0.002	0.002	0.002	0.006***	0.006***	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Under 100% FPL	0.041***	0.041***	0.041***	0.039***	0.039***	0.039***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Children under 5	0.035***	0.035***	0.035***	0.065***	0.065***	0.065***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Household size	0.013***	0.013***	0.013***	0.000	0.000	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
IOU	-0.008	-0.007	-0.008	-0.010	-0.010	-0.011
	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Cooperative	0.007	0.008	0.008	0.002	0.002	0.002
	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)
State FE?	Yes	Yes	Yes	Yes	Yes	Yes
Month FE?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,699	22,699	22,699	22,699	22,699	22,699

Cells contain OLS regression coefficients, with robust standard errors in parentheses. Levels of statistical significance: * p < 0.1, ** p < 0.05 *** p < 0.01

Regarding the estimates for control variables, we find that, even when controlling for disconnection protections, Black households, households that are under 100 percent FPL, and those that have children under 5 years old were more likely to seek financial assistance to pay their energy bills; whereas those that were served by an IOU were less likely to seek financial assistance. We additionally find that larger households, Hispanic households, those that were unemployed during the first year of the pandemic, those with incomes that are at or below 100 percent FPL, and households with children under 5 were all more likely to have to forgo expenses even when disconnection protections were in place.

Across the models, we find that adding voluntary and seasonal protections marginally weakened the estimated impact of disconnection protections on all indicators of household energy insecurity. While these differences are small, it is worth noting that we would expect the estimates to become stronger when including additional protections. Therefore, the results may suggest that mandatory moratoria were the most binding of the three types of the policies, and the addition of voluntary and seasonal protections introduced more cases of households who were disconnected despite being "protected," which resulted in a diminished effect size. The comparison of these coefficient sizes should be interpreted with caution, however, because we cannot rule out that there is no statistical difference between the models.

Heterogeneity of Results

Based on the results of our main models as well as outcomes from extant literature, we subsequently split our survey sample into sub-populations to examine possible heterogeneity in the effects of the disconnection protections on the likelihood of being disconnected for especially energy insecure populations. Results, displayed in Table 6, show that being covered by a

disconnection protection policy reduced the likelihood of disconnections for Black households, Hispanic households, households at or below 100% of the FPL, and households with children under the age of 5. The results are consistent across all three treatment measurements. These results suggest that the policies benefitted certain vulnerable groups, though had no effect for respondents that indicated they were unemployed at the time of the survey. The largest marginal effects of mandatory protections correspond to households with young children (AME of -10.1%), Hispanic households (AME of -7.2%), and Black households (AME of -6.2%).

Table 6. Split-sample Linear probability model predicting whether a respondent had their utility service disconnected in each month for Black respondents, Hispanic respondents, those with income under 100% of FPL, those who are unemployed, and respondents with children under 5 years old in the household

	Black	Hispanic	Under 100% FPL	Unemployed	Children under 5
Mandatory	-0.062***	-0.072***	-0.023***	-0.024	-0.101***
	(0.018)	(0.024)	(0.009)	(0.017)	(0.031)
Mandatory + Voluntary	-0.056***	-0.070***	-0.021**	-0.023	-0.092***
	(0.018)	(0.024)	(0.009)	(0.016)	(0.027)
Mandatory + Voluntary + Seasonal	-0.048***	-0.072***	-0.020***	-0.021	-0.088***
	(0.016)	(0.021)	(0.008)	(0.016)	(0.027)
Controls	Yes	Yes	Yes	Yes	Yes
State FE?	Yes	Yes	Yes	Yes	Yes
Month FE?	Yes	Yes	Yes	Yes	Yes
Observations	3,349	3,323	7,992	2,857	2,564

Cells contain OLS regression coefficients from separate models with different measurement of disconnection protections (all with full controls and state and month fixed effects included), with robust standard errors in parentheses. Levels of statistical significance: * $p < 0.10$, **$p < 0.05$, *** $p < 0.01$

We further estimated a split-sample regression of the effect of disconnection protections on the likelihood that vulnerable groups had to forgo expenses or receive financial assistance to pay an energy bill. As seen in Table 7, similar to our previous split-sample analysis, we find that being protected by a mandatory disconnection protection significantly reduced the likelihood that Black households, households with young children, and those at or below 100% of the FPL had to forgo basic expenses to pay an energy bill. We also find that when voluntary and seasonal

default protections are included in the treatment variable, Hispanic households also reduced their likelihood of forgoing medicine and food.

Table 7: Split-sample Linear probability model predicting whether a respondent had to forgo basic household expenses to pay an energy bill in each month for Black respondents, Hispanic respondents, those with income under 100% of FPL, those who are unemployed, and respondents with children under 5 years old in the household

	Black	Hispanic	Under 100% FPL	Unemployed	Children under 5
Mandatory	-0.053**	-0.045	-0.063***	-0.026	-0.070**
	(0.025)	(0.034)	(0.017)	(0.028)	(0.036)
Mandatory + Voluntary	-0.049**	-0.041	-0.054***	-0.029	-0.055*
	(0.023)	(0.032)	(0.017)	(0.027)	(0.033)
Mandatory + Voluntary + Seasonal	-0.050**	-0.049**	-0.062*	-0.038	-0.047
	(0.022)	(0.017)	(0.033)	(0.029)	(0.040)
Controls	Yes	Yes	Yes	Yes	Yes
State FE?	Yes	Yes	Yes	Yes	Yes
Month FE?	Yes	Yes	Yes	Yes	Yes
Observations	3,349	2,323	7,992	3,059	2,752

Cells contain OLS regression coefficients from separate models with different measurement of disconnection protections (all with full controls and state and month fixed effects included), with robust standard errors in parentheses. Levels of statistical significance: * $p < 0.10$, **$p < 0.05$, *** $p < 0.01$

By contrast, as illustrated in Table 8, disconnection protections did not significantly reduce the likelihood that most vulnerable groups received financial assistance. The exception is for households that are under 100 percent of the FPL. These results mirror those found in our primary empirical specification using the complete survey sample, whereby disconnection policies are correlated with reducing both disconnections across all low-income households and the need to forego basic expenses to pay an energy bill with no statistical impact on a household's likelihood of accessing financial assistance.

Table 8: Split-sample Linear probability model predicting whether a respondent received financial assistance to pay an energy bill in each month for Black respondents, Hispanic respondents, those with income under 100% of FPL, those who are unemployed, and respondents with children under 5 years old in the household

	Black	Hispanic	Under 100% FPL	Unemployed	Children under 5
Mandatory	-0.003	-0.037	-0.028*	-0.030	0.014
	(0.026)	(0.031)	(0.017)	(0.030)	(0.036)
Mandatory + Voluntary	0.014	-0.032	-0.026	-0.020	0.014
	(0.024)	(0.028)	(0.016)	(0.028)	(0.033)
Mandatory + Voluntary + Seasonal	-0.005	-0.032	-0.027*	-0.020	0.014

	(0.023)	(0.029)	(0.016)	(0.028)	(0.033)
Controls	Yes	Yes	Yes	Yes	Yes
State FE?	Yes	Yes	Yes	Yes	Yes
Month FE?	Yes	Yes	Yes	Yes	Yes
Observations	3,349	2,323	7,992	3,059	2,752

Cells contain OLS regression coefficients from separate models with different measurement of disconnection protections (all with full controls and state and month fixed effects included), with robust standard errors in parentheses. Levels of statistical significance: * p < 0.10, **p < 0.05, *** p < 0.01

Robustness Checks

To address limitations of our research design and alleviate concerns about several threats to validity, we conducted a series of robustness checks. First, and most importantly, the data do not allow us to measure pre-treatment parallel trends in energy insecurity between respondents who were and were not covered by disconnection protections. In addition to the covariate balancing measures displayed in Table 1, we also estimated models with interactive state and month fixed effects to capture any state-specific time trends. Results are displayed in Table 9. If there was a threat to pre-treatment trends, we would expect that our empirical model would lose statistical significance if the treatment, disconnection protections, were correlated with unobserved heterogeneity across states and over time. Instead, the results of the interactive fixed effects model show that the effect of protections on the likelihood of being disconnected and forgoing household expenses remains statistically significant, and the coefficients remain fairly similar to the original specification, which we believe assuages concerns about our research design's lack of testable pre-treatment data.

Table 9: Linear probability model with interactive state and month fixed effects predicting whether a respondent had their utility service disconnected, had to forgo a basic household expense, or received financial assistance to pay an energy bill in each month.

	Model 1: Disconnected	Model 2: Disconnected	Model 3: Disconnected	Model4: Forgo expenses	Model 5: Forgo expenses	Model 6: Forgo expenses	Model 7: Social assistance	Model 8: Social assistance	Model 9: Social assistance
Mandatory	-0.028***			-0.024**			-0.016		
	(0.007)			(0.011)			(0.012)		

Mandatory + Voluntary		-0.027*** (0.007)			-0.019* (0.011)			-0.015 (0.011)	
Mandatory + Voluntary + Seasonal			-0.026*** (0.007)			-0.019* (0.011)			-0.015 (0.011)
Stimulus lag	0.003	0.003	0.003	0.014***	0.004	0.005	-0.010*	-0.010*	-0.010*
	(0.003)	(0.003)	(0.003)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
WAP/LIHEAP lag	-0.002	-0.002	-0.001	-0.006	-0.007	-0.004	0.019	0.019	0.017
	(0.005)	(0.005)	(0.005)	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)
Other Government Assistance Lag	-0.001 (0.003)	-0.001 (0.003)	-0.001 (0.003)	0.023*** (0.006)	0.024*** (0.006)	0.023*** (0.006)	0.005 (0.006)	0.005 (0.006)	0.005 (0.006)
Black	0.019***	0.020***	0.020***	0.014**	0.014**	0.014**	0.022***	0.022***	0.022***
	(0.004)	(0.004)	(0.004)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)
Hispanic	0.011**	0.011**	0.011**	0.032***	0.032***	0.032***	-0.011	-0.011	-0.011
	(0.005)	(0.005)	(0.005)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Unemployed	0.002	0.002	0.002	0.026***	0.026***	0.026***	0.011	0.011	0.0011
	(0.004)	(0.004)	(0.004)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
Education	0.000	0.000	0.000	0.002	0.002	0.002	0.006***	0.006***	0.006***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Under 100% FPL	-0.003	-0.003	-0.003	0.041***	0.041***	0.041***	0.039***	0.039***	0.039***
	(0.003)	(0.003)	(0.003)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Children under 5	0.042***	0.042***	0.042***	0.034***	0.034***	0.034***	0.064***	0.064***	0.064***
	(0.007)	(0.007)	(0.007)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Household size	0.002*	0.002*	0.002*	0.013***	0.013***	0.013***	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
IOU	0.006*	0.006*	0.006*	-0.008	-0.008	-0.009	-0.010	-0.009	-0.010
	(0.003)	(0.003)	(0.003)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Cooperative	0.023***	0.023***	0.024***	0.008	0.008	0.008	0.002	0.002	0.002
	(0.007)	(0.006)	(0.006)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)
Month X State FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,699	22,699	22,699	22,699	22,699	22,699	22,699	22,699	22,699

Cells contain OLS regression coefficients, with robust standard errors in parentheses. Levels of statistical significance: * p < 0.1, ** p < 0.05, *** p < 0.01

An additional concern is that states with higher rates of residential utility disconnections before the start of the pandemic would be more likely to adopt a temporary mandatory moratorium. To address this concern, we estimated a model that regresses a binary variable measuring whether a household was disconnected in the year prior to the pandemic (May 2020 to May 2021)[12] on two independent variables: 1) whether a respondent was ever covered under a temporary mandatory moratorium in any month, and 2) the number of days that the respondent

[12] In our first wave survey, we asked respondents a series of questions about their household circumstances in the year prior, including whether they were unable to pay energy bills, received a notice for disconnection, or were disconnected.

was covered under a temporary mandatory moratorium during our study period. If states were more likely to adopt a disconnection moratorium because residents faced higher pre-pandemic rates of disconnection, we would expect that adoption and days of protection would be statistically significant and positive predictors of disconnections in the previous year. Instead, the model displayed in the Appendix, Table A4, shows that coverage is negatively correlated with pre-pandemic disconnections, and results are not statistically significant.

A third potential threat to validity is the coding of the coverage of COVID-era disconnection moratoria. Although most respondents (about 98%) were under disconnection moratoria that spanned every day in a month, some protections expired or began in the middle of a month. For example, in Illinois, moratorium coverage for the month of July expired on July 26th. Our original specification codes a respondent as being covered in month *t* if they are protected for 15 or more days of the month and coded as not covered if they are protected for 14 days or less. This means that if a respondent lived in a state where they were covered by a disconnection moratorium until the 15th of the month, and got disconnected on the 16th or later, they would have been disconnected despite being coded as protected by a moratorium. This coding decision likely biases our estimates downward, yielding a more conservative set of results than we may estimate otherwise. To examine this issue, we estimated an alternative model that uses the number of days covered under a disconnection protection in each month as the main regressor as opposed to a monthly binary coverage variable. The results of the continuous, daily coverage model (see Table A5 in the appendix) are consistent with the binary coverage model, yielding almost identical results in terms of the cumulative marginal effects of being protected in each individual day of the month.

A fourth concern is the potential issue of recall bias among respondents. Specifically, it is possible that respondents mis-remembered the precise month in which an event took place. For example, a participant in wave 2 might have indicated that their household was disconnected in July when that disconnection, in fact, took place in August. If the household was covered under a disconnection moratorium that expired at the end of July, this would impact our estimates. To mitigate potential concerns about this type of recall bias, we aggregated our data to the level of a survey wave and regressed our outcome variables on the number of days that a respondent was protected during that wave (e.g., if a respondent was covered in all of June and July, but not August, that respondent is coded as being covered for 61 days of a 91-day time period). Results can be found in Appendix Table A6. If recall bias affected our estimates, we would expect our findings would be weaker (i.e., the coefficients to be smaller). Instead, we find results that are largely consistent with our main findings.

There are additional limitations to our study. As noted previously, a lack of publicly available utility or administrative data related to utility disconnections limits our analysis. First, there are no measures of rates of disconnections or other energy-related material hardship at the household-level prior to our study period. While the robustness checks discussed above should assuage some concerns, as noted previously, we cannot definitively rule out differential pre-treatment trends and, thus, we encourage some caution in the interpretation of our results.

The lack of data collection related to utility disconnections has persisted—with a few state-level exceptions (e.g., Illinois)—throughout the pandemic. For this reason, we also cannot corroborate our self-reported findings with publicly-reported utility or administrative data. Despite this limitation, self-reported data allows us to study the effects of disconnection protections at the household-level, which has not previously been done on a national scale.

Finally, we cannot perfectly measure disconnection protections for each respondent in each month. Although we include date-based default, seasonal protections in the final iteration of our models, several states have additional protections in place based on temperature and vulnerable members of the population. We do not include policies that use daily temperatures as the mechanism to determine protection since our models are estimated at the monthly level and there is no accurate way to estimate daily phenomena at a month scale. Additionally, we do not include protections based on household characteristics, such as age or medical information, since our survey data do not offer enough detail (e.g., if a person received a note from a doctor citing medical reasons that the utility provider could not disconnect service) to accurately capture and code these protections. As a result, the protection measure is likely underestimating the number of respondents who are protected; thereby providing conservative estimates of treatment's impact on disconnections, forgoing expenses, and use of financial assistance.

Discussion

Our study provides new evidence on the socioeconomic impacts of utility disconnection protections. We match utility disconnection data with an original survey on energy insecurity. This unique dataset allows us to exploit state variation in moratoria that states implement as a result of the COVID-19 economic shock and more accurately estimate the effects of disconnection protections.

We find that the disconnection moratoria had a substantial impact on disconnections, since those in states without protections, or who were not covered by their state moratorium in a given month, faced a far greater likelihood of being disconnected than those who were covered by a moratorium. Our primary models suggest that, even when controlling for the presence of a

disconnection protections, vulnerable households – specifically Black households, Hispanic households, and households with children under 5 years old – are all more likely to be disconnected from the electric grid. However, subgroup analysis suggests that moratoria were effective in reducing disconnections for these same vulnerable populations, (i.e., Black households, Hispanic households, households with children under the age of 5 years old, and those with incomes at or below 100% of the FPL). The combination of these results suggest that utility protections, including mandatory disconnection moratoria, help but may not entirely overcome the documented disparate impacts of energy insecurity felt by households of color and those with young children.

Our primary models also find that utility disconnection moratoria decreased the likelihood that a household had to forgo basic household expenses, such as food or medical care, to pay an energy bill and avoid the threat of utility disconnection. Finally, we find a negative relationship between disconnection moratoria and a household receiving social assistance to pay their energy bill, though results are not statistically significant.

The results presented here have several implications for policymakers. First, disconnection moratoria are effective in reducing incidence of utility disconnections and other energy-related material hardship. Protections are particularly helpful for vulnerable populations, yet more government and utility assistance might be required to overcome the current racial disparities that have been documented by previous energy insecure literature. This finding is especially important as rates of energy insecurity are likely to rise in the future as climate change increases average temperatures and extreme weather events (Graff et al. 2021).

Second, we find that the effects of moratoria on energy insecurity are largely driven by mandatory protections as opposed to voluntary or seasonal protections. This finding suggests that

if lawmakers want to reduce energy insecurity and related material hardship, they should consider making their standard protections statutorily mandatory for regulated utilities. Unlike mandatory disconnection protections, voluntary policies yield the decision about household disconnection to the utilities, who have a vested interest in recovering their costs. In addition, ongoing seasonal protections are complicated and often do not offer full protection of vulnerable populations. Further, it is likely that effect sizes were at least partially driven by the fact that these mandatory moratoria were not burdensome to customers – e.g., they did not require documentation or require households show that your families' economic circumstances have deteriorated – which is not true of many of the standard seasonal protections (LIHEAP 2020). Past research has shown that detailed eligibility requirements reduce program take-up (Graff and Pirog 2019).

Finally, our findings show that our current stable of welfare programs, including standard programs, like Medicaid, as well as energy specific programs, like LIHEAP, do not statistically reduce one's likelihood of being disconnected or a household's need to forgo medical and food expenditures. This finding stresses the importance of funding and expanding energy assistance programs that are accessible and available to low-income populations to avoid the most deleterious impacts of energy insecurity. Additional support for these programs may also help relieve some households in the U.S. of the chronic cycle of energy insecurity.

Our analysis provides important new information on the impact of state level disconnection protections on the energy security and financial stability of low-income populations. These results have important implications for both advocates and government officials. Our study shows the importance of designing utility disconnection policies to protect

the most vulnerable populations and reduce energy insecurity, especially during periods of economic crisis.

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Appendix

Table A1: Linear regression model predicting whether a respondent had their utility service disconnected, had to forgo a basic household expense, or received financial assistance to pay an energy bill in each month, among a balanced panel of respondents

	Model 1: Disconnected	Model 2: Disconnected	Model 3: Disconnected	Model 4: Forgo expenses	Model 5: Forgo expenses	Model 6: Forgo expenses	Model 7: Social assistance	Model 8: Social assistance	Model 9: Social assistance
Mandatory	-0.024***			-0.024**			-0.019		
	(0.007)			(0.011)			(0.012)		
Mandatory + Voluntary		-0.020***			-0.022**			-0.019	
		(0.007)			(0.011)			(0.012)	
Mandatory + Voluntary + Seasonal			-0.020***			-0.020*			-0.014
			(0.007)			(0.011)			(0.011)
Stimulus Lag	0.002	0.002	0.002	0.007	0.007	0.006	-0.008	-0.008	-0.008
	(0.002)	(0.002)	(0.002)	(0.008)	(0.008)	(0.008)	(0.006)	(0.006)	(0.006)
WAP/LIHEAP Lag	-0.003	-0.002	-0.002	-0.012	-0.012	-0.011	0.017	0.017	0.016
	(0.007)	(0.007)	(0.07)	(0.014)	(0.014)	(0.014)	(0.018)	(0.018)	(0.018)
Other Government Assistance Lag	-0.001	-0.001	-0.001	0.026***	0.026***	0.025***	0.006	0.006	0.007
	(0.003)	(0.003)	(0.003)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Black	0.022***	0.022***	0.022***	0.017*	0.017*	0.017*	0.012	0.012	0.012
	(0.005)	(0.005)	(0.005)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Hispanic	0.005	0.005	0.005	0.026**	0.026**	0.026**	-0.018*	-0.018*	-0.018*
	(0.005)	(0.005)	(0.005)	(0.011)	(0.011)	(0.011)	(0.009)	(0.009)	(0.009)
Unemployed	-0.002	-0.002	-0.002	0.022**	0.023**	0.023**	-0.012	-0.012	-0.011
	(0.004)	(0.004)	(0.004)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Education	0.001	0.001	0.001	0.007***	0.007***	0.007***	0.008***	0.008***	0.008***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Under 100% FPL	-0.006*	-0.005*	-0.005*	0.043***	0.043***	0.043***	0.042***	0.042***	0.042***
	(0.003)	(0.003)	(0.003)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Children under 5	0.043***	0.043***	0.043***	0.014	0.014	0.014	0.070***	0.069***	0.069***
	(0.010)	(0.010)	(0.010)	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)
Household size	0.006***	0.006***	0.006***	0.019***	0.019***	0.019***	0.004	0.004	0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
IOU	0.008**	0.008**	0.008**	-0.008	-0.008	-0.009	0.010	0.010	0.008
	(0.003)	(0.003)	(0.003)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)
Cooperative	0.038***	0.038***	0.038***	0.025**	0.026***	0.026***	0.038***	0.038***	0.038***
	(0.008)	(0.008)	(0.008)	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)
State FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,594	17,594	17,594	17,594	17,594	17,594	17,594	17,594	17,594

Cells contain OLS regression coefficients, with robust standard errors in parentheses. Levels of statistical significance: * $p < 0.1$, $p < 0.05$, *** $p < 0.01$

Table A2: Distribution of pre-treatment control variables among respondents who were covered or not covered under state disconnection protection in each month from May 2020 through May 2021

	Not Covered			**Covered**			
	count	mean	sd	count	mean	sd	Mean diff.
Black	12,821	0.15	0.36	9,981	0.14	0.35	0.01
Hispanic	12,821	0.09	0.29	9,981	0.12	0.32	0.03
Unemployed	12,821	0.13	0.33	9,981	0.14	0.35	0.01
Education	12,821	2.98	1.32	9,981	3.03	1.35	0.05
Under 100% FPL	12,821	0.35	0.48	9,981	0.36	0.48	0.01
Household member under 5	12,821	0.12	0.33	9,981	0.12	0.33	0.00
Household size	12,821	2.61	1.68	9,981	2.58	1.68	0.03
Cooperative	12,821	0.21	0.41	9,981	0.07	0.25	0.14
Muni	12,821	0.22	0.41	9,981	0.10	0.30	0.12
IOU	12,821	0.55	0.50	9,981	0.83	0.38	0.33

Table A3: Logistic regression model predicting whether a respondent had their utility service disconnected, had to forgo a basic household expense, or received financial assistance to pay an energy bill in each month

	Model 1: Disconnected	Model 2: Disconnected	Model 3: Disconnected	Model 4: Forgo expenses	Model 5: Forgo expenses	Model 6: Forgo expenses	Model 7: Social assistance	Model 8: Social assistance	Model 9: Social assistance
Mandatory	-0.950***			-0.205**			-0.140		
	(0.266)			(0.101)			(0.102)		
Voluntary + Voluntary		-0.878***			-0.204**			-0.121	
		(0.264)			(0.097)			(0.099)	
Mandatory + Voluntary + Seasonal			-0.840***			-0.202**			-0.099
			(0.266)			(0.098)			(0.099)
Stimulus Lag	0.101	0.098	0.105	0.036	0.035	0.046	-0.142**	-0.142**	-0.133*
	(0.165)	(0.133)	(0.164)	(0.064)	(0.064)	(0.063)	(0.071)	(0.071)	(0.070)
WAP/LIHEAP Lag	-0.050	-0.056	-0.008	-0.085	-0.85	-0.058	0.189	0.189	0.171
	(0.383)	(0.383)	(0.371)	(0.161)	(0.161)	(0.158)	(0.160)	(0.160)	(0.160)
Other Government Assistance Lag	-0.084	-0.082	-0.109	0.292***	0.293***	0.280***	0.060	0.060	0.058
	(0.173)	(0.172)	(0.174)	(0.067)	(0.067)	(0.067)	(0.066)	(0.066)	(0.066)
Black	1.224***	1.225***	1.228***	0.192**	0.194**	0.193**	0.250***	0.251***	0.250***
	(0.206)	(0.207)	(0.207)	(0.089)	(0.089)	(0.089)	(0.081)	(0.081)	(0.081)
Hispanic	0.650***	0.651***	0.652***	0.356***	0.355***	0.353***	-0.120	-0.120	-0.119
	(0.222)	(0.222)	(0.222)	(0.095)	(0.094)	(0.094)	(0.104)	(0.104)	(0.104)
Unemployed	0.269	0.267	0.263	0.296***	0.295**	0.297***	0.098	0.098	0.099
	(0.207)	(0.207)	(0.207)	(0.085)	(0.085)	(0.085)	(0.088)	(0.088)	(0.088)
Education	0.056	0.055	0.054	0.035	0.035	0.035	0.072***	0.073***	0.072***
	(0.056)	(0.056)	(0.056)	(0.025)	(0.025)	(0.025)	(0.024)	(0.024)	(0.024)
Under 100% FPL	-0.056	-0.057	-0.056	0.489***	0.490***	0.490***	0.440***	0.441***	0.440***
	(0.177)	(0.177)	(0.177)	(0.068)	(0.068)	(0.068)	(0.066)	(0.066)	(0.066)
Children under 5	1.691***	1.689**	1.690***	0.341***	0.341***	0.341***	0.638***	0.638***	0.638***
	(0.185)	(0.184)	(0.184)	(0.097)	(0097)	(0.097)	(0.098)	(0.097)	(0.097)
Household size	0.106***	0.105***	0.105***	0.126***	0.126***	0.123***	0.002	0.002	0.002
	(0.027)	(0.028)	(0.028)	(0.018)	(0.018)	(0.018)	(0.022)	(0.021)	(0.021)
IOU	0.027	0.010	0.002	-0.084	-0.078	-0.078	-0.115	-0.119	-0.126
	(0.199)	(0.199)	(0.199)	(0.090)	(0.091)	(0.091)	(0.084)	(0.084)	(0.084)
Cooperative	1.009***	1.026***	1.004***	0.116	0.118	0.119	0.025	0.028	0.030
	(0.236)	(0.239)	(0.239)	(0.111)	(0.111)	(0.111)	(0.117)	(0.117)	(0.117)
State FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,396	20,396	20,396	22,570	22,570	22,570	22,401	22,401	22,401

Cells contain Logistic regression coefficients, with robust standard errors in parentheses. Levels of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Linear regression model predicting whether a respondent had their utility service disconnected in the previous year

	Model 1: Disconnected in previous year	Model 2: Disconnected in previous year
Mandatory Ever Covered	-0.098	
	(0.082)	
Mandatory Days Covered		-0.003
		(0.003)
Black	0.121**	0.121**
	(0.053)	(0.053)
Hispanic	0.068	0.068
	(0.057)	(0.057)
Unemployed	0.118**	0.118**
	(0.049)	(0.049)
Education	-0.010	-0.010
	(0.011)	(0.011)
Under 100% FPL	0.021	0.021
	(0.032)	(0.032)
Children under 5	0.181***	0.181***
	(0.057)	(0.057)
Household size	0.023	0.023
	(0.021)	(0.021)
IOU	-0.078	-0.077
	(0.049)	(0.049)
Cooperative	-0.047	-0.047
	(0.061)	(0.061)
State FE?	Yes	Yes
Month FE?	Yes	Yes
Observations	2,342	2,342

Cells contain OLS regression coefficients, with robust standard errors in parentheses. Levels of statistical significance: * $p < 0.1$, $p < 0.05$, *** $p < 0.01$

Table A5: Linear regression model predicting whether a respondent had their utility service disconnected, had to forgo a basic household expense, or received financial assistance to pay an energy bill in each month, using days of coverage as the treatment variable

	Model 1: Disconnected	Model2: Disconnected	Model 3: Disconnected	Model 4: Forgo expenses	Model 5: Forgo expenses	Model 6: Forgo expenses	Model 7: Social assistance	Model 8: Social assistance	Model 9: Social assistance
Mandatory days	-0.001***			-0.001**			-0.000		
	(0.000)			(0.000)			(0.000)		
Mandatory + Voluntary days		-0.001***			-0.001**			-0.000	
		(0.000)			(0.000)			(0.000)	
Mandatory + Voluntary + Seasonal days			-0.001***			-0.001**			-0.000
			(0.000)			(0.000)			(0.000)
Stimulus Lag	0.003	0.003	0.003	0.004	0.004	0.005	-0.011*	-0.011*	-0.010*
	(0.003)	(0.003)	(0.003)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
WAP/LIHEAP Lag	-0.002	-0.002	-0.001	-0.007	-0.007	-0.005	0.018	0.018	0.016
	(0.005)	(0.005)	(0.005)	(0.013)	(0.013)	(0.013)	(0.015)	(0.015)	(0.015)
Other Government Assistance Lag	-0.001	-0.001	-0.001	0.024***	0.024***	0.023***	0.005	0.005	0.005
	(0.005)	(0.005)	(0.003)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Black	0.019***	0.020***	0.020***	0.014*	0.014*	0.014*	0.022***	0.022***	0.022***
	(0.004)	(0.004)	(0.004)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)
Hispanic	0.012**	0.012**	0.012**	0.031***	0.031***	0.031***	-0.011	-0.011	-0.011
	(0.005)	(0.005)	(0.005)	(0.010)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)
Unemployed	0.002	0.002	0.002	0.027***	0.027***	0.027***	0.001	0.011	0.011
	(0.004)	(0.004)	(0.004)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
Education	0.000	0.000	0.000	0.002	0.002	0.002	0.006***	0.006***	0.006***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Under 100% FPL	-0.003	-0.003	-0.003	0.041***	0.041***	0.041***	0.039***	0.039***	0.039***
	(0.003)	(0.003)	(0.003)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Children under 5	0.042***	0.042***	0.042***	0.035***	0.035***	0.035***	0.065***	0.065***	0.065***
	(0.007)	(0.007)	(0.007)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Household size	0.002*	0.002*	0.002*	0.013***	0.013***	0.013***	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
IOU	0.005	0.005	0.005	-0.008	-0.007	-0.007	-0.010	-0.010	-0.011
	(0.003)	(0.003)	(0.003)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)
Cooperative	0.023***	0.024***	0.024***	0.007	0.007	0.007	0.002	0.002	0.002
	(0.006)	(0.006)	(0.006)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)
State FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,699	22,699	22,699	22,699	22,699	22,699	22,699	22,699	22,699

Cells contain OLS regression coefficients, with robust standard errors in parentheses. Levels of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Linear regression model predicting whether a respondent had their utility service disconnected, had to forgo a basic household expense, or received financial assistance to pay an energy bill in each wave, using cumulative days of coverage during the wave as the treatment variable

	Model 1: Disconnected	Model 2: Disconnected	Model 3: Disconnected	Model 4: Forgo expenses	Model 5: Forgo expenses	Model 6: Forgo expenses	Model 7: Social assistance	Model 8: Social assistance	Model 9: Social assistance
Mandatory days	-0.000***			-0.001***			-0.000*		
	(0.000)			(0.000)			(0.000)		
Mandatory + Voluntary days		-0.000***			-0.001***			-0.000*	
		(0.000)			(0.000)			(0.000)	
Mandatory + Voluntary + Seasonal days			-0.000***			-0.001***			-0.000
			(0.000)			(0.000)			(0.000)
Stimulus Lag	0.005	0.005	0.005	0.041**	0.041**	0.041**	0.000	0.000	0.000
	(0.007)	(0.007)	(0.007)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
WAP/LIHEAP Lag	-0.011	-0.011	-0.011	-0.005	-0.005	-0.005	0.049*	0.049*	0.049*
	(0.010)	(0.010)	(0.010)	(0.024)	(0.024)	(0.024)	(0.029)	(0.029)	(0.029)
Other Government Assistance Lag	0.006	0.006	0.006	0.036***	0.036***	0.036***	0.015	0.015	0.016
	(0.007)	(0.007)	(0.007)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Black	0.028***	0.028***	0.028***	0.034**	0.034**	0.034**	0.043***	0.043***	0.043***
	(0.009)	(0.009)	(0.009)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Hispanic	0.027**	0.027**	0.027**	0.085***	0.085***	0.085***	0.023	0.023	0.023
	(0.011)	(0.011)	(0.011)	(0.021)	(0.021)	(0.021)	(0.020)	(0.020)	(0.020)
Unemployed	0.022**	0.022**	0.022**	0.047***	0.048***	0.048***	0.029*	0.029*	0.029*
	(0.010)	(0.010)	(0.010)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Education	-0.001	-0.001	-0.001	0.005	0.005	0.005	0.004	0.004	0.004
	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Under 100% FPL	-0.008	-0.007	-0.007	0.057***	0.057***	0.057***	0.046***	0.046***	0.046***
	(0.007)	(0.007)	(0.007)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Children under 5	0.077***	0.077***	0.077***	0.094***	0.094***	0.093***	0.148***	0.148***	0.148***
	(0.015)	(0.015)	(0.015)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
Household size	0.002	0.002	0.002	0.014***	0.014***	0.014***	-0.003	-0.003	-0.003
	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
IOU	0.005	0.005	0.006	-0.015	-0.015	-0.015	-0.012	-0.011	-0.012
	(0.008)	(0.009)	(0.009)	(0.017)	(0.017)	(0.017)	(0.016)	(0.017)	(0.017)
Cooperative	0.030**	0.030**	0.031**	0.006	0.007	0.008	0.002	0.002	0.004
	(0.013)	(0.013)	(0.013)	(0.020)	(0.020)	(0.020)	(0.021)	(0.021)	(0.021)
State FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,574	7,574	7,574	7,574	7,574	7,574	7,574	7,574	7,574

Cells contain OLS regression coefficients, with robust standard errors in parentheses. Levels of statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$