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Mental Health Costs of Lockdowns: Evidence from Age-Specific Curfews in Turkey[†]

By Onur Altindag, Bilge Erten, and Pinar Keskin*

Using a strict, age-specific lockdown order for adults aged 65 and older in Turkey, we examine the mental health consequences of an extended period of tight mobility restrictions on senior adults. Adopting a regression discontinuity design, we find that the curfew-induced decline in mobility substantially worsened mental health outcomes, including somatic and nonsomatic symptoms of mental distress (approximately 0.2 standard deviation). Exploring potential channels, we document an increase in social and physical isolation, with no evidence of robust changes in labor market outcomes or intrahousehold conflict for this subpopulation. (*JEL* D13, I21, I18, J14, O15)

Mental health conditions account for 20 percent of all disabilities worldwide and cost approximately more than $US1 trillion annually (World Health Organization 2019). Social isolation and loneliness have shown strong correlations with depression and anxiety and can significantly predict adult morbidity and chronic diseases that lead to early mortality (Idler and Benyamini 1997, Ortega et al. 2010, Steptoe et al. 2013, Holwerda et al. 2016, Domènech-Abella et al. 2017).

Recent anecdotal evidence suggests a considerable increase in mental health disorders after the introduction of movement restrictions to slow the spread of COVID-19 (The New York Times May 12, 2020). The share of the US population who report symptoms of depression and anxiety, for example, rose to around 40 percent during the COVID-19 pandemic compared to 11 percent in early 2019 (Center for Disease Control and Prevention 2020).[1] Stay-at-home orders are associated with

*Altindag: Department of Economics, Bentley University (email: oaltindag@bentley.edu); Erten: Department of Economics, Northeastern University (email: b.erten@neu.edu); Keskin: Department of Economics, Wellesley College (email: pinar.keskin@wellesley.edu). Neale Mahoney was coeditor for this article. We gratefully acknowledge financial support from the Northeastern University CSSH COVID-19 Research and Engagement Development Initiative. We thank Emilie Courtin, James Dana, Donghee Jo, Phillip Levine, Alisa Lincoln, Mindy Marks, Stephen O'Connell, Olga Shurchkov, Berna Turam, Jeff Gulati, and the webinar participants at Ankara University, Bentley University, Grand Valley State University, LACDEV, the NBER Health Economics Meeting Fall 2020, NEUDC, Northeastern University, Washington and Lee University, and Wellesley College for their useful comments and suggestions. This study was approved by the Northeastern University IRB (20-05-12). All errors are, of course, our own.

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[1] In Spain, similarly, based on a cross-sectional survey, González-Sanguino et al. (2020) report psychological stress among 87 percent of survey participants and attribute its excessive prevalence to confinement.

an increased number of internet search queries related to mental health, most strikingly suicide intentions (Jacobson et al. 2020).[2] Mental health effects are expected to worsen over time as self- or government-imposed quarantine and other social distancing measures create increased social isolation, physical immobility, and economic uncertainty. The financial cost of treating COVID-19 related mental health conditions is staggering: one recent estimate, for example, suggests a $US1.6 trillion additional annual burden to the US healthcare system (Cutler and Summers 2020). Despite the mounting descriptive evidence coupled with increasing concern among public health and medical communities (Armitage and Nellums 2020, Brooks et al. 2020; Galea et al. 2020; Holmes et al. 2020; Panchal et al. 2020), scant empirical evidence exists to show the causal impact of restricted mobility on mental health outcomes.

We help fill this gap by quantifying the effects of binding stay-at-home orders for those aged 65 and older implemented in Turkey on individuals' mental health. The Turkish government imposed a strict stay-at-home order for the high-risk population of those 65 and older starting on March 21, 2020. Severe financial penalties were imposed for noncompliance with these mandated movement restrictions. The strict curfew orders remained in effect until June 2020, making it one of the longest confinement policies enacted worldwide to reduce COVID-19 mortality.

Our study exploits the binding age cutoff to causally estimate the impact of the age-specific curfew on mental health outcomes. We rely on the fact that individuals who were born around the age cutoff at which the curfew becomes binding have no systematic differences in predetermined characteristics and are thus comparable. To this end, we conducted a phone survey from late May to early July, targeting the specific age group of 59- to 70-year-old adults, and compared those who were just below the age cutoff of 65 and thus not affected by the stay-at-home order to those just above 65 who were affected. In addition to using the 20-item Self Reporting Questionnaire (SRQ-20) developed by the World Health Organization (WHO 1994)—a validated mental health measure used in a variety of nonclinical settings—we designed a survey to capture various channels through which the curfew can impact mental health outcomes.

Social isolation may generate adverse mental health consequences through several different channels we examined in this study. First, stay-at-home orders reduce one's contact with other individuals and restrict social interactions, leading to increased feelings of loneliness and anxiety. Since older adults are already at risk for depression and mental illness, being prohibited from seeing their close relatives or friends is likely to act as an additional stressor, making them feel lonelier, more anxious, and forgotten (Armitage and Nellums 2020; Santini et al. 2020; Newman and Zainal 2020). Second, stay-at-home orders may prevent individuals from participating in the workforce and reduce their potential income, leading to additional

[2] Similarly, individuals exposed to stay-at-home orders in the United States report increased health concerns, financial anxiety, and loneliness compared to those who are not exposed to them (Tull et al. 2020).

stress due to financial constraints (Fetzer et al. 2020a; Beland et al. 2020).[3,4] Third, being confined at home with other family members for an extended time period can also increase the likelihood of intrahousehold conflict, and in extreme cases, can give rise to physical or psychological abuse (Leslie and Wilson 2020; Ravindran and Shah 2020; Erten, Keskin, Prina 2022).[5]

We adopt a regression discontinuity design (RD) using a narrow age bandwidth and report three main results. First, our RD estimates show that the curfew reduced the number of days that individuals had gone outside in the previous week by around one day, corresponding to an approximately 47 percent decline relative to the control group. Similarly, it increased the probability of never leaving home by approximately 30 percentage points, corresponding to a 149 percent increase relative to the control group.

Second, we find that the curfew-induced reduction in mobility increases the probability of experiencing mental distress markedly, measured both by somatic indicators that capture physical symptoms of anxiety and depression and nonsomatic indicators that represent more subjective assessments of anxiety and depression. Our RD estimates imply that exposure to the curfew results in a 0.18 standard deviation increase in somatic symptoms and a 0.16 standard deviation increase in nonsomatic symptoms of mental distress.[6]

Lastly, examining potential channels, we document that social and physical isolation play a particularly important role in explaining our results. Our results indicate that exposure to the curfew results in a 8.8 percentage point (16 percent) increase in the probability of having limited social interaction with friends and family, and a 24.8 percentage point (54 percent) increase in the probability of having limited physical activity. We find no evidence of a robust significant change in labor market outcomes or intrahousehold conflict measures.

We note upfront that the order of questions asked in the survey may lead to potential priming effects. In particular, the respondents were asked about their mobility indicators prior to their mental health outcomes. If this order of questions makes mobility restrictions more salient, such priming could lead to a change in the respondents' reported mental health measures. However, it is important to note that there were a range of other questions in between mobility and mental health questions

[3] In the US context, negative effects on labor market outcomes have occurred (Forsythe et al. 2020). These studies also document a smaller and imprecisely estimated effect for the labor market outcomes of older adults (Gupta et al. 2020). Similarly, since we focus on a relatively older segment of the population, a large proportion of which is already out of the labor force, one might expect to see smaller effects on labor market outcomes.

[4] The retirement age in Turkey is 58 for women and 60 for men. For new entrants to the pension system after October 2008, the retirement age will gradually rise to 65 (OECD 2017). Hence, our respondents were not differentially affected by the retirement age cutoff. Using the HLFS 2019, Figure A1 illustrates graphically that the probability of retirement increases monotonically from age 63 to 68. Note that the HLFS 2019 does not contain birth month information, which is necessary to conduct an RD analysis. It is also important to remember that no specific government programs in Turkey exist that are similar to Medicare in the US for which individuals qualify once they turn 65.

[5] Although the incidence of domestic violence appears smaller among older adults, it is far from negligible, ranging from around 3 to 10 percent (Nelson 2002, Tufan 2011).

[6] These effect sizes are similar to those reported in studies that document the substantial effects of cash transfers on psychological well-being and depression (Baird et al. 2013, Haushofer and Shapiro 2016).

as shown in online Appendix C, which potentially mitigate such concerns. Adding these transition questions might offset any priming effects.[7]

We make several contributions to the existing literature. First, we show that the adverse impacts of social and physical isolation on mental health are substantial. Despite the well-known associations, most of the previous studies that document adverse mental health effects stemming from quarantine and social isolation are based on small sample sizes and fail to account for reverse causality (Brooks et al. 2020; Newman and Zainal 2020). As unobservables, such as earlier life events, childhood circumstances, and ability, might affect both social isolation and mental health outcomes, establishing a causal relationship has been difficult. Our empirical setup allows us to estimate the effects of an exogenous decline in mobility on somatic and nonsomatic mental distress indicators.

Our study more closely relates to a smaller subset of empirical studies that rely on the differential timing of lockdown measures.[8] Using two waves of cross-section data from the United States, Adams-Prassl et al. (2020) compares the respondents from states that imposed strict stay-at-home orders to those that did not before and after the lockdown. Both Brodeur et al. (2020) and Tubadji et al. (2020) use Google Trends data from the United States and Europe and compare the intensity of searching for mental health terms before and after a lockdown. While these studies suggest increased mental distress related to lockdowns, they face challenges in separately identifying the impact of mandated lockdowns from the private responses to COVID-19 cases. Our study provides an empirical setup that isolates the effects of mandated curfews and uses validated measures of mental health.

Second, our findings contribute to better understanding the costs associated with lockdowns, which go beyond financial losses. The growing literature on the optimal policy response to the pandemic often uses a susceptible-infectious-recovered (SIR) framework, assuming that different subpopulations might have different rates of infection and survival (Acemoglu et al. 2020; Alvarez et al. 2020; Brotherhood et al. 2020). Acemoglu et al. (2020), for instance, suggest that it is possible to achieve better outcomes through a simple "targeted policy that applies an aggressive lockdown" on individuals above age 65. If the policy response to COVID-19 creates a mental health crisis by placing already susceptible populations at higher risk of depression and suicide, these consequences would call for additional policy interventions to address such adverse effects. Such policy measures may include setting up mental health call centers, improving access to telehealth services, and establishing on-the-ground local support services for at-risk populations (Galea et al. 2020).

This paper is organized as follows. Section I provides a brief description of the COVID-19 lockdown in Turkey. Section II presents the data used for the analysis,

[7] In surveys to measure the Gallup-Sharecare Well-Being Index, it was observed that when subjective well-being questions are asked after political preference questions, the respondents reported substantially lower life satisfaction. In a randomized setting, Deaton (2011) shows that adding a single transition question between the political and subjective well-being questions almost fully mitigates the priming effects of political questions. The study suggests that the relevant mechanism is reorientating the respondent's attention away from politics.

[8] We provide a detailed table in the online Appendix A that summarizes some of the most recent studies on this topic (Adams-Prassl et al. 2020; Armbruster and Klotzbücher 2020; Banks and Xu 2020; Burdett et al. 2020; Daly et al. 2020; Etheridge and Spantig 2020; Fetzer et al. 2020a, b; Giuntella et al. 2020; Holman et al. 2020; Proto and Quintana-Domeque 2020).

the identification strategy, and preliminary checks for the RD analysis. Section III presents the main results, and Section IV discusses the evidence on potential causal channels. Section V concludes the paper.

I. Background

The Turkish Ministry of Health reported the first case of the novel coronavirus on March 10, 2020, and the first COVID-19-related death on March 17.[9] From this early period, the older population and individuals with underlying medical conditions were defining features of the government's response to the COVID-19 pandemic. In stark contrast to the rest of the world, the Turkish government imposed strict and long-lasting mobility restrictions exclusively on senior citizens. The first curfew decree was issued on March 21 and imposed an absolute lockdown on individuals aged 65 and older and those with certain health conditions.[10] The central government formed local support teams to provide basic needs for individuals subject to the decree and who needed assistance, while no exceptions existed that could breach the stay-at-home order.[11] The age-specific curfew along with other government measures to contain the virus, such as mask wearing in public spaces, were routinely enforced by the local security forces and the offenders were fined.[12,13]

Only after May 10, individuals who were subject to the curfew were allowed a period of four hours to walk outside their home on Sundays, which was conditional on wearing a mask and social distancing.[14] One week later, the government allowed a similar exception for six hours.[15] On May 21, senior citizens were allowed to travel to a specific location, which was conditional on staying for at least one month and not leaving their new shelter.[16] On May 29, actively employed senior citizens were exempt from the lockdown. Finally, on June 10, the curfew was relaxed and all individuals who were subject to it were allowed to be outside between 10 AM and 8 PM.

The heterodox policy response to the pandemic has stirred controversy, as a lack of empirical evidence failed to demonstrate that the decision to impose age-specific curfews would slow down the death toll or virus spread.[17] The Turkish Medical

[9] https://covid19.saglik.gov.tr/ (accessed May 4, 2021).

[10] These conditions include autoimmune disorders, chronic pulmonary disease, asthma, cardiovascular disease, hypertension, renal, and liver-related diseases. https://www.icisleri.gov.tr/65-yas-ve-ustu-ile-kronik-rahatsizligi-olanlara-sokaga-cikma-yasagi-genelgesi (last accessed May 4, 2021).

[11] In case of a health emergency, those aged 65 and older could call the emergency number 112, and an ambulance could be sent to their premises for transport to the closest hospital or health clinic for emergency treatment.

[12] According to the law, the fine for curfew offenders was set between 789–3,180 Turkish liras (https://blog.lexpera.com.tr/bulasici-hastaliklara-iliskin-tedbirlere-aykiri-davranma-sucu-tck-m-195/ (accessed May 4, 2021). Anecdotal evidence suggests that the upper limit was used to deter potential offenders (see, for example, https://www.hurriyet.com.tr/gundem/sokaga-cikma-yasagina-ragmen-kahvehanede-oyun-oynarken-yakalandilar-41492692 (accessed May 4, 2021). As a reference, the minimum monthly wage in Turkey during the same period was 2,943 Turkish liras.

[13] We also note that the lockdown policy did not place any restrictions on household visitors.

[14] https://www.goc.gov.tr/65-yas-ve-uzeri20-yas-altikronik-rahatsizligi-bulunan-kisilerin-sokaga-cikma-kisitlamasi-istisnasi-genelgesi-merkezicerik (last accessed May 4, 2021).

[15] https://www.icisleri.gov.tr/65-yas-ve-uzeri-ile-kronik-rahatsizligi-olan-vatandaslarin-sokaga-cikma-gun-ve-saatleri (laccessed May 4, 2021).

[16] https://www.icisleri.gov.tr/81-il-valiligine-65-yas-ve-uzeri-vatandaslarimiz-icin-seyahat-izin-belgesi-genelgesi (accessed May 4, 2021).

[17] The Turkish government does not provide detailed and consistent epidemic data, thus to the best of our knowledge, no empirical studies confirm or refute the success of the age-specific curfew policy. In addition, the

Association (TMA), for example, argues that the excessive restrictions on senior citizens' mobility adversely affected their mental health, severely disrupted their daily routines, and created a sense of unfairness among those under lockdown.[18] According to the TMA, the policy lacks the epidemiological evidence to show its effectiveness in protecting vulnerable populations at the expense of their mental well-being.[19]

In official announcements, the Turkish Ministry of Internal Affairs does not mention a specific birthday cutoff for the curfew and uses the term "age 65 and older" to indicate the senior population that is subject to lockdown, although anecdotal evidence suggests that birth year is the sole determinant.[20] To confirm the threshold in our sample, we directly asked respondents whether they were subject to the age-specific curfew the government imposed. We then ran a simulation in which we split our analysis sample into treatment and control groups using each birth year and month as the curfew threshold to estimate the average difference in exposure to the curfew between the treatment and control groups for each of the simulated thresholds.

As shown in online Appendix Figure A2 and in line with field observations, we obtain the greatest difference in being subject to the curfew between individuals who were born just before and after January 1956. The estimated coefficient indicates that individuals born before or in December 1955 are 85 percentage points more likely to claim to be subject to the curfew than those who were born in January 1956 or later. In the empirical analysis, we rely on this threshold, which provides the strongest discontinuity in exposure to the treatment, as shown by various measures in Figure 2.

II. Data and Empirical Methodology

A. *Data*

We use a unique dataset covering individuals born in Turkey between 1950 and 1961. The data were collected by KONDA Research and Consultancy, a reputable research and consultancy firm in Istanbul, Turkey. Since the firm regularly conducts nationally representative surveys to provide information on public opinion on a wide range of political issues, they have built a surveyor base throughout the country. Their regular surveys—called KONDA Barometer surveys—are conducted 11 times per year and have successfully predicted election outcomes in recent general

reported aggregate figures on deaths substantially underestimate the total case and death toll; one study showed that excess mortality is at least twice as high as the official government death counts due to COVID-19 (Altindag 2020).

[18] https://www.ttb.org.tr/415yi6z (accessed May 4, 2021).

[19] According to the Ministry of Health, the total number of confirmed COVID-19 cases was 583 in 100,000 for 50–64-year-old individuals and 553 in 100,000 for 65–79-year-olds between June 1 and June 18, 2020. The corresponding death rate for all confirmed cases was 3.19 percent for the former group while it was 13.0 percent for the latter one.

[20] Separate curfews were imposed on individuals aged 18 and 20, and government announcements indicate that year of birth determines inclusion in that age group. See, for example, https://www.icisleri.gov.tr/sokaga-cikma-yasagi-bulunan-18—20-yas-arasindaki-genclerle-ilgili-istisnalar (last accessed May 4, 2021).

Table 1—Summary Statistics for 59–70 Year-Old Individuals

	Mean	SD	Min	Max	Observations
Panel A. Pre-determined characteristics					
Completed high school	0.27	0.44	0	1	1,896
Illiterate	0.13	0.34	0	1	1,896
Female	0.43	0.50	0	1	1,909
Married	0.81	0.40	0	1	1,907
Widowed or separated	0.18	0.38	0	1	1,907
Non-Turkish	0.26	0.44	0	1	1,881
Pre-COVID-19 household size	3.29	1.73	1	10	1,909
Ever received psychological support	0.11	0.31	0	1	1,887
Has a chronic disease	0.57	0.50	0	1	1,898
Panel B. Mobility Outcomes					
Days outside last week	1.87	1.79	0	7	1,896
Under curfew	0.48	0.50	0	1	1,907
Never goes out	0.36	0.48	0	1	1,885
Panel C. Potential channels					
Paid employed	0.13	0.34	0	1	1,892
Paid or unpaid employed	0.15	0.36	0	1	1,897
Has a job but could not attend last week	0.14	0.35	0	1	1,876
Has money for usual needs	0.61	0.49	0	1	1,894
Worried about spending money	0.60	0.49	0	1	1,890
Limited social interaction	0.60	0.49	0	1	1,905
Limited physical activity	0.55	0.50	0	1	1,878
Current household size	3.42	1.83	1	10	1,909
Conflict with a household member	0.37	0.48	0	1	1,868

Notes: The table presents the means, standard deviations, minimum values, maximum values, and number of observations. The sample includes 59–70-year-old individuals born within 72 months before and after December 1955. The variables are described in online Appendix B.

elections. Given their record and well-respected position in both the Turkish[21] and international media[22] (The Economist 2008, Reuters 2011, The Economist 2019), we contacted KONDA to implement our survey instrument.

Specifically, we approached KONDA to collect survey data from their existing respondent database with the following two criteria: (i) respondents should reside in urban areas where the curfews are strictly imposed, and (ii) they should be aged between 59 and 70 to have 6 treatment and 6 control cohorts on each side of the curfew threshold. Consequently, the survey instrument was implemented in urban areas across 26 regions from May 29 to July 4 through phone interviews.[23] The average response rate for our survey was 88 percent. Although response rates are slightly lower for those not exposed to the curfew, Figure A3 shows no evidence of a significant break in nonresponse rates around the age threshold of 65.[24] Online Appendix C provides the full list of questions asked in the survey (Altindag et al. 2021).

[21] https://www.hurriyet.com.tr/gundem/hangi-anket-sirketi-secimlerin-sonucunu-dogru-bildi-29224184

[22] See, for example, https://www.economist.com/briefing/2008/07/17/flags-veils-and-sharia?story_id=E1_TTSQVVSD, https://www.reuters.com/article/us-turkey-referendum-poll/poll-shows-backing-for-turk-reforms-on-eve-of-vote-idUSTRE68A0EV20100911?feedType=RSS&feedName=everything&virtualBrandChannel=11563, https://www.economist.com/erasmus/2019/07/01/in-turkey-demography-is-a-brake-on-islamisation.

[23] On average, the respondents were subject to the curfew for 8 to 9 weeks when they were contacted.

[24] Since we did not have a universal database of telephone numbers that matched the owners' age, it was not possible to collect data through random digit dialing. Instead, we rely on the existing respondent database of

Online Appendix Table A1 compares basic demographic information from our analysis sample to the 2019 Household Labor Force Survey (HLFS), focusing on individuals born between 1950 and 1961 (Turkish Statistical Institute 2019). We observe that the average age is 64 for both samples and that the marital status indicators are quite similar. Our analysis sample has relatively fewer women. It is also composed of more educated individuals than the HLFS due to the urban sampling frame.

Online Appendix Table A1 presents the summary statistics for our analysis sample composed of a maximum of 1,909 individuals. We observe that 27 percent of the sample completed high school or above. The household size prior to the COVID-19 outbreak was 3.3, with approximately 11 percent of the respondents having ever received psychological support and 57 percent having a chronic disease.[25] We observe that 48 percent of the respondents in our sample reported being subject to the curfew, and the average number of days spent outside in the previous week was 1.9 days.

Finally, we observe that 13 percent worked for pay, and 15 percent worked either for pay or in family businesses. In addition, 14 percent had a job that they could not attend in the previous week. Approximately 60 percent experienced limited social interaction, and 55 percent experienced limited physical activity.[26] Their current household size was 3.4, and approximately 37 percent reported having a conflict with a household member over the last month.

To assess mental health outcomes, a set of mental health screening tools have been developed in nonclinical settings. These range from depression scales such as the Beck Depression Inventory (Beck et al. 1961) to more general psychological distress scales such as the K10 scale (Kessler et al. 2002). In our survey, we use the 20-Item Self Reporting Questionnaire (SRQ-20) developed by the World Health Organization (WHO 1994). Table 2 details the list of SRQ-20 questions along with their summary statistics. Among the several mental health screening tools, the SRQ-20 is one of the few specifically designed for low- and middle-income settings. The questions are designed to identify mental distress that captures typical anxiety and depression symptoms, such as poor concentration or suicidal thoughts, and less-known somatic symptoms such as digestive problems or frequent aches.[27] The questions' short format and the dichotomous answers used in the SRQ-20 render it particularly useful in settings with limited resources (van der Westhuizen et al. 2016).[28]

KONDA to draw a sample of individuals between ages 59 and 70 living in urban areas. Sampling from an existing respondent database may also explain our relatively high response rate for a phone survey.

[25] Less than 7 percent of the respondents reported that they were living alone at home.

[26] Limited social interaction is a dummy variable that takes the value of one if the respondent reported that his/her social interaction with friends and family has been extremely limited or very limited in the last month compared to pre-COVID times. Limited physical activity is a dummy variable that takes the value of one if the respondent reported that his/her physical activity (e.g., walking, running, doing sports, etc.) has been extremely limited or very limited in the last month compared to pre-COVID times.

[27] The American Psychiatric Association also suggests that anxiety and depression symptoms include not only classic psychological signs such as loss of interest but also somatic symptoms such as general aches and pains or trembling (American Psychiatric Association 2013).

[28] The SRQ-20 has been cross-validated across many countries, including Brazil (Iacoponi and de Jesus Mari 1989), China (Chen et al. 2009), Vietnam (Giang et al. 2006), and India (Patel et al. 2008), and has been shown to be a reliable tool for measuring mental health distress in low- and middle-income contexts.

Table 2—List of SRQ-20 Questions and their Summary Statistics

	Mean	SD	Min	Max	Observations
Over the last 4 weeks					
have you often had headaches?	0.47	0.50	0	1	1,904
has your appetite been poor?	0.38	0.49	0	1	1,903
have you slept badly?	0.49	0.50	0	1	1,904
have you been easily frightened?	0.31	0.46	0	1	1,897
have you had shaking hands?	0.19	0.39	0	1	1,894
have you felt nervous, tense, or worried?	0.66	0.47	0	1	1,891
has your digestion been poor?	0.32	0.47	0	1	1,890
have you had trouble in thinking clearly?	0.41	0.49	0	1	1,889
have you felt unhappy?	0.57	0.50	0	1	1,890
have you cried more often than usual?	0.18	0.38	0	1	1,877
have you found it difficult to enjoy your daily activities?	0.52	0.50	0	1	1,887
have you found it difficult to make decisions?	0.36	0.48	0	1	1,888
has your daily work suffered?	0.46	0.50	0	1	1,887
have you been unable to play a useful part in life?	0.22	0.41	0	1	1,883
have you lost interest in things?	0.38	0.49	0	1	1,880
have you felt that you are a worthless person?	0.21	0.41	0	1	1,879
has the thought of ending your life been on your mind?	0.04	0.20	0	1	1,882
have you felt tired all the time?	0.50	0.50	0	1	1,895
have you had uncomfortable feelings in your stomach?	0.42	0.49	0	1	1,899
have you gotten tired easily?	0.56	0.50	0	1	1,895

Notes: The table presents the list of questions included in the SRQ-20 inventory, and their summary statistics, including the means, standard deviations, minimum values, maximum values, and number of observations. The sample includes 59–70-year-old individuals born within 72 months before and after December 1955. The variables are described in online Appendix B.

Following Anderson (2008) and Erten and Keskin (2020), we construct three summary indices: (i) a mental distress index, which is an average of the z-scores of 20 mental health indicators; (ii) a somatic symptoms of distress index, which is an average of 4 indicators related to the body and are therefore more objective measures of anxiety and depression; and (iii) a nonsomatic symptoms of distress index, which is an average of the remaining 16 indicators that represent more subjective assessments of anxiety and depression. We create these indices to have a mean of 0 and a standard deviation of 1, following Anderson (2008). The variables that compose each index are described in online Appendix A. Higher index values reflect higher mental distress levels.

In addition, we include a more standard measure of mental distress using SRQ-20 by summing "yes" answers to the questions included in the SRQ-20 inventory. This variable provides an alternative measure of mental distress, with higher values capturing higher distress levels.[29]

[29] In the psychology literature, some studies used certain cutoff points of the SRQ score varying between 0 and 20 to identify the presence of mental disorders. However, as WHO (1994) has described, it is necessary to conduct an empirical validation against a sample of in-depth psychiatric interviews in order to determine the country- and culture-specific cutoff point (Harpham et al. 2003). Since we do not have a study that conducted such a validation in Turkey, we opted for not using any specific cutoff point.

B. *Identification*

As explained in Section I, COVID-19 lockdowns were strictly imposed on individuals born before or in December 1955, while those born in January 1956 or later were exempt. The context thus offers an ideal setting to implement an RD design to estimate the curfew's impact on a range of outcomes.

Our RD design leverages the quasi-random assignment of curfew around the age cutoff to estimate the reduced-form (RF) effects of the curfew on our outcomes of interest. The causal interpretation of both estimates relies on the identifying assumption that around the vicinity of the curfew age cutoff, the assignment to curfew is as good as random. Our identifying assumption is that these two cohorts born one month apart do not exhibit any systematic differences other than whether they were exposed to the curfew or not. For our RF estimates, we use the following specification:

$$(1) \qquad y_i = \alpha + \beta z_i + f(x_i) + \epsilon_i, \qquad \forall x_i \in (c-h, c+h),$$

where y_i captures the outcome of interest, which is regressed on a treatment indicator z_i that equals one for individuals who were born before January 1956 and zero otherwise. Note, x_i is the forcing variable defined as the number of months that the respondent is older than the index month of the curfew threshold. The function $f(x_i)$ is a continuous local linear function fit separately on each side of the threshold point c. The standard errors are clustered at the month-year of birth to account for the correlation in outcomes across individuals who were born in the same year-month cell (Lee and Card 2008). We additionally control for month of birth fixed effects, province fixed effects, surveyor fixed effects, as well as indicator variables for education levels, ethnicity, and gender. For the regression sample restriction, we use the Imbens and Kalyanaraman (2012) procedure to choose the optimal bandwidth h.[30] As discussed in Section III, our results are robust to a range of bandwidths and a quadratic control function.

To address concerns related to multiple hypothesis tests, we report p-values that account for the false discovery rate (FDR) (Benjamini et al. 2006; Anderson 2008). It is necessary to correct for FDR in our study since we examine multiple outcomes within the same domain, which increases the likelihood that some of our estimates are statistically significant by chance alone. We use the FDR approach presented in Anderson (2008), based on the methodology in Benjamini et al. (2006). This procedure converts p-values into q-values, which control the expected proportion of rejections that are Type I errors, or false discoveries. Following Field et al. (2021), we make these adjustments for multiple hypothesis testing within domains defined by the set of outcomes considered in each table (e.g., we adjust within all mobility outcomes in Table 3), or each panel within tables (e.g., we adjust within all labor market outcomes in panel A of Table 5).[31]

[30] We use a uniform kernel in our estimations. The results are highly similar when we use a triangular kernel.

[31] Other standard routines for q-value conversion developed by Simes (1986) and Romano and Wolf (2016) yield similar results, which are available from the authors upon request.

Table 3—Effects of Curfew on Mobility Outcomes

	±17	±30	±45	±60
Days outside last week				
Born before 1955	−1.011	−1.106	−1.09	−1.023
	(0.308)	(0.230)	(0.180)	(0.166)
	[0.002]	[<0.001]	[<0.001]	[<0.001]
	⟨0.003⟩	⟨0.001⟩	⟨0.001⟩	⟨0.001⟩
Observations	506	832	1,214	1,601
Control group mean	2.30	2.33	2.30	2.40
Under curfew				
Born before 1955	0.609	0.663	0.708	0.723
	(0.071)	(0.057)	(0.045)	(0.036)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]
	⟨0.001⟩	⟨0.001⟩	⟨0.001⟩	⟨0.001⟩
Observations	511	837	1,222	1,610
Control group mean	0.12	0.09	0.09	0.08
Never goes out				
Born before 1955	0.213	0.301	0.297	0.245
	(0.079)	(0.045)	(0.036)	(0.034)
	[0.011]	[<0.001]	[<0.001]	[<0.001]
	⟨0.004⟩	⟨0.001⟩	⟨0.001⟩	⟨0.001⟩
Observations	500	825	1,203	1,591
Control group mean	0.17	0.18	0.20	0.18

Notes: This table presents first-stage estimates of the effect of being born before December 1955 on the mobility outcomes of individuals. The variable descriptions are provided in online Appendix B. The first column presents results for individuals born within 17 months of the age threshold, December 1955. The second through fourth columns expand the sample to include individuals within 30, 45, and 60 months of the age threshold. The specification includes month fixed effects, province fixed effects, surveyor fixed effects, as well as indicator variables for education levels, ethnicity, and gender. Standard errors, clustered at the month-year cohort level, are in parentheses. Corresponding p-values and Anderson's (2008) sharpened q-values are in square and angle brackets, respectively.

One could be concerned that exposure to the curfew affects the perceived likelihood of getting sick in addition to its effects on mobility. However, this concern is highly unlikely given that those just below the cutoff point have heard the same information about COVID-19's higher risks for older people and people with chronic diseases. Nevertheless, if this concern was valid, the estimates capture the combined effect of the lockdown and fear related to COVID-19.

The optimal bandwidth according to Imbens and Kalyanaraman (2012) (IK)'s method is 45 for the variable capturing days spent outside during the previous week. For brevity, we will use the RD estimates for this optimal bandwidth in interpreting the magnitudes of the results. For robustness, we also present our results using the corresponding optimal bandwidth calculated by Calonico et al. (2014) algorithm, which is 17 months. In addition, we use two additional constant bandwidths of 30 and 60 months around the discontinuity in our main tables. The results are highly similar with different bandwidths and control functions.

C. *Preliminary Checks*

We conduct two standard checks to validate our RD design (Imbens and Lemiuex 2008). The first is to test whether the forcing variable is subject to manipulation

around the predetermined threshold (McCrary 2008). In our specific setup, rejecting a one-sided null hypothesis would indicate that individuals falsify their birthday to exempt themselves from the curfew. This, however, is highly unlikely because we asked individuals to read their birthday from their national IDs, which is the standard tool that local security forces use to enforce the curfew. Figure A4 provides visual evidence that the local quadratic fits on the left- and righthand side of the age threshold for the curfew do not exhibit any jumps in observation frequency. A formal test provided in McCrary (2008) also fails to reject the null hypothesis.[32]

Second, we examine whether the predetermined covariates are balanced around the discontinuity. In Figure 1, each graph plots local averages of the outcome in one-month bins against the forcing variable. We find no evidence of a significant break at the discontinuity for indicator variables of whether the respondents completed high school, whether they are illiterate, whether they are female, whether they are married, whether they are widowed or separated, whether they are of non-Turkish ethnic origin, whether they have ever received psychological support, whether they have a chronic disease, and their household size prior to COVID-19.[33]

III. Effects of the Curfew on Mobility and Mental Health Outcomes

A. *Mobility Outcomes*

We begin by testing the curfew's effect on mobility outcomes. Panel A of Figure 2 plots local averages of three mobility outcomes in monthly bins against the respondent's month and year of birth with a cutoff of December 1955.[34] As described in 1, the curfew required those born before this date to stay at home, whereas younger cohorts were free to leave their homes at any time. Local linear smoothed fits on each side of the cutoff are overlaid on each figure. Figure 2, panel A shows a clear downward shift at the discontinuity with an approximately 1-day decline in the number of days that respondents went out during the week prior to the interview. Similarly, Figure 2, panel B also reveals a clear jump around the discontinuity in the self-reported probability of being subject to the curfew. Similarly, in Figure 2 panel C, the probability of never going outside—the likelihood of never leaving home—increases abruptly around the age threshold. Compared to the control group averages, all of these indicators show a substantial decline in the mobility of respondents older than 65 years of age at the time the curfew was imposed.

In Table 3, we present the corresponding first-stage estimates using the main estimating equation at various bandwidths. Crossing the treatment threshold reduces the number of days the respondents went outside in the previous week by 1–1.1

[32] To conduct the test, we use our first-stage optimal bandwidth of 45 months. The test yields a p-value of 0.8.

[33] In regression-based tests reported in online Appendix Table A2, we note that none of the predetermined covariates display any evidence of a statistically significant jump at the discontinuity across different bandwidths. SUR tests of the coefficients' joint significance result in p-values ranging from 0.25 to 0.75, depending on the bandwidth.

[34] Online Appendix Figure A5 provides a residualized version of Figure 2 after regressing outcomes on fixed effects and controls, displaying very similar results.

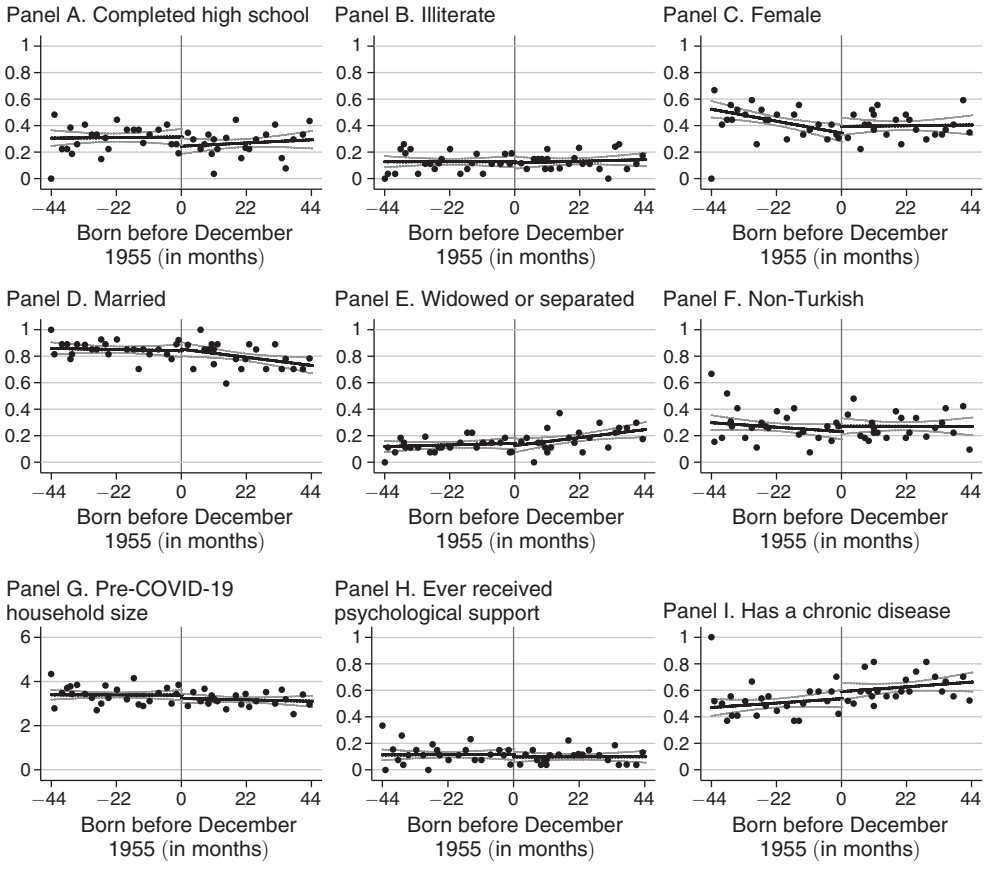

Figure 1. Balanced Covariates

Notes: The sample includes all individuals born before and after 44 months around the cutoff point, December 1955. The figures plot predetermined covariates in monthly bins against the month-year of birth of being born in December 1955. The vertical line in each graph represents the cutoff point. Gray lines show 95 percent confidence intervals around the mean level. Variable definitions are listed in online Appendix B.

days. The estimates are robust to different bandwidths and roughly correspond to a 47 percent decline in mobility relative to the control group mean.[35]

Related indicators exhibit similarly large declines in mobility: being born before 1955 increases reporting of being subject to the curfew by 71 percentage points and raises the probability of never going out by 30 percentage points. Relative to the control group means, these estimates correspond to an approximately 7-fold increase in the probability of reporting being subject to the curfew and a 149 percent increase in the probability of never leaving home. These estimates are robust to

[35] The results are very similar if we examine the curfew's effects on the number of times the respondent went outside over the last month in a week on average, or the number of times the respondent went outside in the last week apart from the legally permitted times.

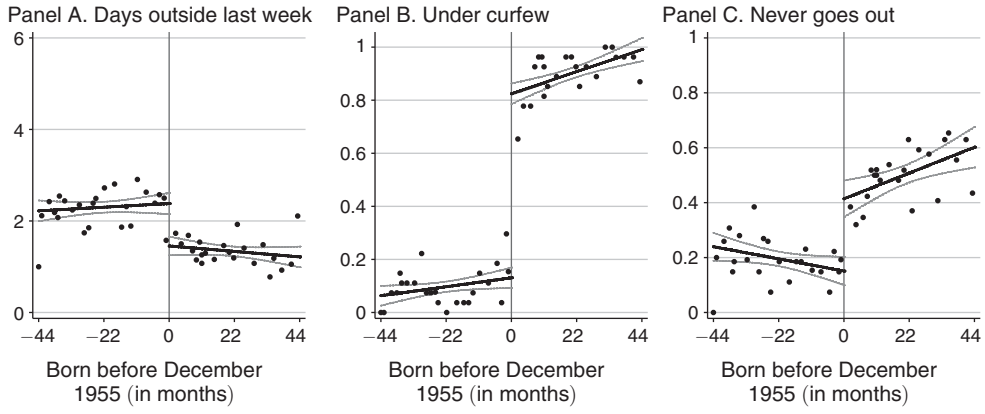

FIGURE 2. RD TREATMENT EFFECTS ON MOBILITY OUTCOMES

Notes: The figures plot the non-residualized values of the number of days spent outside last week, the probability of being subject to the curfew, and the probability of never going outside against the month-year of birth of being born in December 1955. The sample includes all individuals born before and after 44 months around the cutoff point, December 1955. The vertical line in each graph represents the cut-off point, December 1955. Gray lines show 95 percent confidence intervals around the mean level. Variable definitions are listed in online Appendix B.

different bandwidths and control functions, as shown in online Appendix Tables A3 and A8.

B. *Mental Health Outcomes*

We next examine the curfew's impact on mental health outcomes. Following recent RD studies,[36] we graphically illustrate the average of each index as a function of monthly distance from being born in December 1955. In particular, we residualize the outcomes by regressing them on fixed effects and controls, and then plot local averages of these residualized outcomes in monthly bins against the respondent's month and year of birth with a cutoff of December 1955 in Figure 3. These graphs suggest a sharp increase in all mental distress indicators around the discontinuity.[37]

Table 4 quantifies the magnitude of these effects by reporting the reduced-form RD treatment effects of being born before December 1955 with a linear control function in the month-year of birth on each side of the discontinuity across different bandwidths. Remarkably, the RD estimates show a substantial impact of the curfew on all measures of mental distress; the first-row estimates imply a 0.21 standard deviation increase in the mental distress index.[38] We estimate similar effects for the

[36] See, for example, Asher and Novosad (2020).

[37] Online Appendix Figure A6, which provides an unresidualized version of Figure 3, shows very similar results.

[38] One could consider scaling the magnitudes of these reduced-form estimates by different mobility measures to get a sense of the treatment effects on the complier population. Scaling by reporting that the respondent was under curfew results in larger two-stage-least-squares estimates than the reduced-form estimates since the estimated coefficient for differential likelihood of being under curfew is 0.7 for the optimal bandwidth of 45 months. In particular, complying with the curfew leads to a 0.29 standard deviation increase in the mental distress index. In contrast, scaling with respect to days spent outside last week results in slightly smaller estimates as the reduced-form coefficients

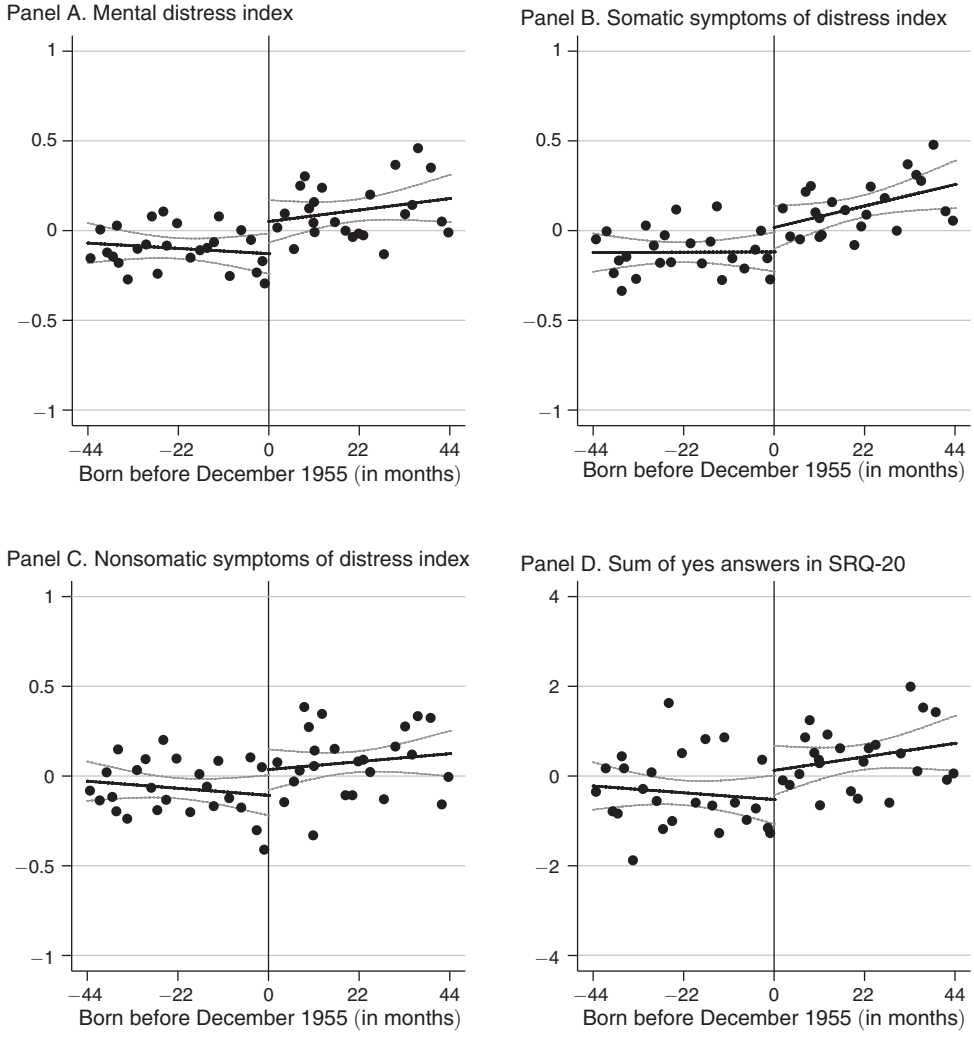

Figure 3. RD Treatment Effects on Mental Health Outcomes

Notes: The figures plot the residualized values (after controlling for all variables in the main specification other than distance to the cutoff) of the indices of mental distress outcomes over the month-year of birth of being born in December 1955. The sample includes all individuals born before and after 44 months around the cutoff point, December 1955. The vertical line in each graph represents the cut-off point, December 1955. Gray lines show 95 percent confidence intervals around the mean level. Variable definitions are listed in online Appendix B.

more objective measure of depression—the somatic symptoms index (0.18 standard deviation), which includes only physical symptoms of depression. The corresponding effect size of the nonsomatic symptoms index is an increase of 0.16 standard deviation. Finally, the RD estimates indicate that the curfew had a positive impact of

are divided by the first-stage estimates of around 1.1 days. For instance, a one-day reduction in days spent outside due to the curfew results in a 0.19 standard deviation increase in mental distress index.

Table 4—Effects of Curfew on Mental Health Outcomes

	±17	±30	±45	±60
Mental distress index				
Born before 1955	0.264	0.359	0.205	0.238
	(0.188)	(0.115)	(0.094)	(0.077)
	[0.169]	[0.003]	[0.032]	[0.003]
	⟨0.422⟩	⟨0.011⟩	⟨0.095⟩	⟨0.011⟩
Observations	475	777	1,133	1,485
Somatic symptoms of distress index				
Born before 1955	0.343	0.296	0.175	0.198
	(0.195)	(0.121)	(0.085)	(0.080)
	[0.087]	[0.018]	[0.043]	[0.015]
	⟨0.422⟩	⟨0.019⟩	⟨0.095⟩	⟨0.016⟩
Observations	503	824	1,203	1,580
Nonsomatic symptoms of distress index				
Born before 1955	0.153	0.272	0.158	0.188
	(0.182)	(0.118)	(0.092)	(0.073)
	[0.404]	[0.025]	[0.088]	[0.011]
	⟨0.422⟩	⟨0.019⟩	⟨0.095⟩	⟨0.016⟩
Observations	478	781	1,137	1,491
Sum of "yes" answers in SRQ-20				
Born before 1955	1.163	1.236	0.734	0.816
	(0.936)	(0.517)	(0.440)	(0.384)
	[0.222]	[0.020]	[0.098]	[0.036]
	⟨0.422⟩	⟨0.019⟩	⟨0.095⟩	⟨0.021⟩
Observations	475	777	1,133	1,485
Control group mean	7.03	7.17	7.05	7.16

Notes: This table presents regression discontinuity estimates of the effect of the curfew on mental health outcomes. See the online Appendix B for details of index construction. The first column presents results for individuals born within 17 months of the age threshold, December 1955. The second through fourth columns expand the sample to include individuals within 30, 45, and 60 months of the age threshold. The specification includes month fixed effects, province fixed effects, surveyor fixed effects, as well as indicator variables for education levels, ethnicity, and gender. Standard errors, clustered at the month-year cohort level, are in parentheses. Corresponding *p*-values and Anderson's (2008) sharpened *q*-values are in square and angle brackets, respectively.

0.7 on the sum of "yes" answers in SRQ-20 inventory reported by the respondents. This corresponds to a 10.4 percent increase relative to the control group mean. These estimates are robust to different bandwidths and control functions, as shown in online Appendix Tables A4 and A8.[39,40]

[39] When we examine the effects of the curfew on individuals' self-perception of their health, panel A of online Appendix Table A5 shows that those exposed to the curfew report a higher probability of having poor physical health compared to those not exposed. Interestingly, we find no evidence that exposure to the curfew has a significant impact on the probability of reporting poor mental health. Hence, despite a significant worsening in their mental distress outcomes as measured by SRQ-questions, those exposed to the curfew do not recognize these symptoms as a deterioration in their mental health.

[40] Moreover, we asked respondents two questions about their life satisfaction using the Cantril Scale, which is frequently used in Gallup Surveys. In reporting scale scores, Gallup refers to those respondents who have poor ratings of their current life situation (4 and below) and negative ratings for the next five years (4 and below) as "suffering," or having well-being that is at high risk (Gallup 2013). In panel B of online Appendix Table A5, we also examine whether the curfew had a significant impact on the probability of reporting higher suffering measured by these indicators, but we find no evidence of a significant change in this measure.

IV. Effects of the Curfew on Potential Causal Channels

We proceed by examining potential causal channels through which the curfew negatively impacted mental health outcomes. We divide our analysis into three subsections by focusing on the curfew's effects on the following outcomes: (i) employment and income, (ii) social and physical isolation, and (iii) household conflict.

A. *Employment and Income Outcomes*

If exposure to the curfew prevents one from going to work, it can negatively impact one's ability to work outside the home and earn a living. Such negative labor market impacts can lead to additional anxiety and a deterioration of mental health outcomes. We explore this mechanism by testing whether the curfew negatively impacts employment and income outcomes.

In panel A of Table 5, we find no evidence that the curfew significantly impacts labor market outcomes or household finances after adjusting for multiple hypothesis testing. These include whether the respondent was working in paid, or paid/unpaid employment, or whether the respondent has a job that he/she cannot attend. Similarly, we find no evidence of the curfew significantly impacting having enough money to meet usual needs, or being worried about spending money.[41] Hence, we conclude that the employment and income channel does not seem to explain our results.

B. *Social and Physical Isolation Outcomes*

Confinement may severely limit an individual's social interaction and physical mobility. Social isolation, loneliness, and disconnectedness from the community may lead to mental health problems among the senior population. Moreover, continuous confinement within the same physical space and a lack of physical mobility and exercise could further magnify the risk of a mental breakdown.

In panel B of Table 5, the RD estimates show that the curfew had a positive impact on having a very limited or extremely limited social interaction and physical activity in the last month compared to pre-COVID times.[42] In particular, exposure to the curfew results in a 8.8 percentage point increase in the probability of having limited social interaction with friends and family, corresponding to a 16 percent increase relative to the control group. Similarly, exposure to the curfew leads to a 24.8 percentage point increase in the probability of having limited physical activity such as walking, running, or participating in sports, corresponding to a 54 percent increase compared to the control group. Overall, we conclude that the social and physical isolation channel can potentially explain our results.

[41] In our survey, we also asked respondents about their monthly income more explicitly. However, many respondents answered this question by reporting that they have retirement income. Since retirement income varies widely across individuals, we do not have a consistent measure of household income.

[42] The results are very similar if we only focus on experiencing an extreme limitation in social interaction and physical activity over the same time period.

Table 5—Effects of Curfew on Potential Channels

	±17	±30	±45	±60
Panel A. Employment and income outcomes				
Paid employed				
Born before 1955	−0.032	−0.083	−0.071	−0.064
	(0.074)	(0.048)	(0.037)	(0.032)
	[0.671]	[0.090]	[0.059]	[0.044]
	⟨1.000⟩	⟨0.812⟩	⟨0.423⟩	⟨0.285⟩
Observations	506	832	1,215	1,597
Control group mean	0.16	0.15	0.16	0.18
Paid or unpaid employed				
Born before 1955	−0.020	−0.031	−0.025	−0.039
	(0.083)	(0.052)	(0.041)	(0.034)
	[0.814]	[0.549]	[0.542]	[0.263]
	⟨1.000⟩	⟨1.000⟩	⟨1.000⟩	⟨0.358⟩
Observations	506	832	1,216	1,601
Control group mean	0.18	0.18	0.19	0.21
Has a job but could not attend last week				
Born before 1955	−0.016	−0.010	−0.022	−0.042
	(0.090)	(0.047)	(0.035)	(0.032)
	[0.864]	[0.836]	[0.527]	[0.187]
	⟨1.000⟩	⟨1.000⟩	⟨1.000⟩	⟨0.333⟩
Observations	500	825	1,207	1,586
Control group mean	0.14	0.16	0.16	0.18
Has enough money for usual needs				
Born before 1955	0.028	0.044	0.063	0.074
	(0.096)	(0.062)	(0.058)	(0.048)
	[0.775]	[0.481]	[0.276]	[0.121]
	⟨1.000⟩	⟨1.000⟩	⟨1.000⟩	⟨0.320⟩
Observations	507	832	1,218	1,601
Control group mean	0.58	0.56	0.58	0.57
Worried about spending money				
Born before 1955	−0.120	−0.045	−0.021	−0.030
	(0.066)	(0.052)	(0.047)	(0.042)
	[0.080]	[0.393]	[0.660]	[0.478]
	⟨0.670⟩	⟨1.000⟩	⟨1.000⟩	⟨0.434⟩
Observations	506	829	1,213	1,597
Control group mean	0.62	0.59	0.60	0.62

(continued)

C. *Household Conflict Outcomes*

While being confined to the home reduces time spent with people outside of the home, it tends to result in an increase in time spent with household members. This additional time could mechanically increase the probability of having conflict with a household member. Moreover, the additional stress of social isolation could also increase the probability of experiencing a conflict at home.

In panel C of Table 5, the RD estimates indicate no evidence of change in the current household size, implying no significant impact of the curfew on household composition. We also find no evidence of a significant change in the probability of having a conflict with a household member driven by home confinement. However, we note as a caveat that the precision of estimates for the household conflict effect do not allow us to rule out meaningful results. Overall, the household conflict channel

Table 5—Effects of Curfew on Potential Channels (continued)

	±17	±30	±45	±60
Panel B. Social and physical isolation outcomes				
Limited social interaction				
Born before 1955	0.222	0.074	0.088	0.083
	(0.071)	(0.058)	(0.051)	(0.042)
	[0.004]	[0.211]	[0.088]	[0.049]
	⟨0.002⟩	⟨0.119⟩	⟨0.047⟩	⟨0.026⟩
Observations	509	835	1,220	1,608
Control group mean	0.61	0.57	0.56	0.53
Limited physical activity				
Born before 1955	0.365	0.203	0.248	0.219
	(0.092)	(0.064)	(0.054)	(0.047)
	[<0.001]	[0.003]	[<0.001]	[<0.001]
	⟨0.001⟩	⟨0.006⟩	⟨0.001⟩	⟨0.001⟩
Observations	499	822	1,201	1,585
Control group mean	0.45	0.45	0.46	0.45
Panel C. Household conflict outcomes				
Household size				
Born before 1955	−0.098	0.032	−0.047	0.013
	(0.244)	(0.183)	(0.161)	(0.146)
	[0.690]	[0.860]	[0.771]	[0.930]
	⟨1.000⟩	⟨0.756⟩	⟨0.784⟩	⟨1.000⟩
Observations	511	838	1,224	1,612
Control group mean	3.53	3.50	3.50	3.54
Conflict with a household member				
Born before 1955	0.035	0.091	0.052	0.023
	(0.093)	(0.055)	(0.042)	(0.037)
	[0.705]	[0.105]	[0.220]	[0.523]
	⟨1.000⟩	⟨0.265⟩	⟨0.784⟩	⟨1.000⟩
Observations	498	820	1,200	1,579
Control group mean	0.34	0.36	0.38	0.38

Notes: This table presents regression discontinuity estimates of the effect of the curfew on potential channels. The variable descriptions are provided in online Appendix B. The first column presents results for individuals born within 17 months of the age threshold, December 1955. The second through fourth columns expand the sample to include individuals within 30, 45, and 60 months of the age threshold. The specification includes month fixed effects, province fixed effects, surveyor fixed effects, as well as indicator variables for education levels, ethnicity, and gender. Standard errors, clustered at the month-year cohort level, are in parentheses. Corresponding p-values and Anderson's (2008) sharpened q-values are in square and angle brackets, respectively.

does not appear to explain our results. The graphical illustration of these potential channels presented in online Appendix Figure A7 are consistent the results shown in Table 5.

Individual Beliefs and Practices.—Finally, we explore some potential consequences of the curfew for individual beliefs and practices. For example, age-specific curfews might create a sense of social unfairness among individuals subject to them. As shown in Table 6, individuals subject to the curfew are substantially less likely to support the curfew policy. In particular, exposure to the curfew reduced support for the 65 and over age-specific curfew by 14.6 percent compared to the control group. In contrast, we find no evidence of a robust significant change for satisfaction with the government's overall COVID-19 policy response. Last, we examine the change in religious practices and religiosity as a coping mechanism under social isolation.

Table 6—Effects of Curfew on Political Support for Curfew and COVID-19-Specific Policies

	±17	±30	±45	±60
Supports the 65+ age-specific curfew				
Born before 1955	−0.222	−0.129	−0.115	−0.086
	(0.088)	(0.049)	(0.041)	(0.034)
	[0.017]	[0.010]	[0.006]	[0.014]
	⟨0.036⟩	⟨0.022⟩	⟨0.012⟩	⟨0.029⟩
Observations	505	828	1,207	1,590
Control group mean	0.83	0.80	0.79	0.81
Satisfied with the government's COVID-19 policy response				
Born before 1955	−0.122	−0.093	−0.032	−0.002
	(0.099)	(0.051)	(0.047)	(0.038)
	[0.227]	[0.071]	[0.490]	[0.958]
	⟨0.129⟩	⟨0.037⟩	⟨0.325⟩	⟨0.919⟩
Observations	501	822	1,197	1,579
Control group mean	0.74	0.70	0.69	0.69

Notes: This table presents regression discontinuity estimates of the effect of the curfew on supporting the 65+ age-specific curfew and being satisfied with the government's COVID-19 policy response using a linear control function. The variable descriptions are provided in online Appendix B. The first column presents results for individuals born within 17 months of the age threshold, December 1955. The second through fourth columns expand the sample to include individuals within 30, 45, and 60 months of the age threshold. The specification includes month fixed effects, province fixed effects, surveyor fixed effects, as well as indicator variables for education levels, ethnicity, and gender. Standard errors, clustered at the month-year cohort level, are in parentheses. Corresponding p-values and Anderson's (2008) sharpened q-values are in square and angle brackets, respectively.

The estimates provided in online Appendix Table A7 show no evidence of the curfew significantly impacting religious beliefs and practices.

Note that these estimates are robust to using this study's different bandwidths and control functions as shown in online Appendix Tables A6 and A8.

V. Conclusion

The policy response to the COVID-19 pandemic has involved lockdown orders with different degrees of strictness. While the lockdown policies generally brought benefits in reducing the spread of the virus, they also posed potential costs on certain subpopulations. While macroeconomic models incorporating the SIR framework often recommend age-specific lockdowns targeting adults age 65 and older, they often neglect the mental health consequences of these movement restrictions.

Using a rather unique setup in Turkey, which imposed a strict curfew for the high-risk population group of those aged 65 and over on March 21, 2020, we implement an RD design comparing those just under the binding age cutoff to those above it using data from a detailed phone survey covering 59- to 70-year-old adults.

Our findings reveal that the curfew had striking mental health consequences. We find that the curfew reduced the number of days spent outside the week prior to the interview by approximately one day. In turn, the curfew-induced reduction in mobility increases the probability of experiencing mental distress substantially, with approximately 0.2 standard deviation increases in somatic and nonsomatic

symptoms of mental distress. These sizable effects are all the more concerning since older adults are already more susceptible to a higher risk of depression and suicide.

These mental health consequences of strict lockdown policies call for a rethinking of how additional policy measures—ranging from mental health call centers and telehealth services to on-the-ground local support for senior adults—can be used to alleviate the mental health burden on susceptible populations.

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