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Abstract

Do emotional responses to the spread of an infectious disease affect the quality of economic decision-making? In the context of an episode of heightened public concern about Ebola in the US in October 2014, I document that worrying about the possibility of an epidemic can impair cognitive function. My analysis relies on data from cognitive tests administered as part of a wave of survey interviews by a large US panel study, which I combine with measures of local concern about Ebola based on internet search volume. For identification, I exploit temporal and spatial variation in Ebola concern caused by the emergence of four cases of Ebola that were diagnosed in the US. Using proximity to the US cases as an instrumental variable, I show that the local level of Ebola concern individuals are exposed to at the time and place of the interview reduces their scores on the cognitive test. In additional analyses, I find no indication of fear-induced selection effects that could plausibly explain these results. Moreover, proximity to subsequent Ebola locations is unrelated to test scores for interviews conducted before the emergence of the first US case. My findings indicate that emotional responses to epidemics can entail a temporary cognitive cost even for individuals for whom the actual health risk never materializes.

JEL classification: D91

Keywords: Worry, Fear, Emotions, Ebola, Epidemics, Cognitive Function

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1 Introduction

During an epidemic, the disease often does not come alone. It is accompanied by powerful emotional reponses, manifesting in feelings of worry and fear. For instance, the share of adults in the United States who reported experiencing significant worry on the previous day increased from 38 to 58 percent in March 2020 during the onset of the COVID-19 pandemic (Witters and Harters, 2020). Economists have argued that these emotions can affect behavior in important ways, e.g., by inducing temporary visceral urges to withdraw or negative anticipatory utility associated with the object of fear (e.g., Elster, 1998; Loewenstein, 2000; Caplin and Leahy, 2001). In line with these theoretical accounts, existing research on the economic consequences of epidemics emphasizes fear of contagion as a key channel that can affect patterns of economic activity by triggering avoidance behavior (e.g., Goolsbee and Syverson, 2021).

However, fear and worry might not only affect economic outcomes through deliberate changes of behavior. In this paper, I empirically investigate another, hitherto unexplored potential consequence of worrying about epidemics: it might have a direct negative effect on cognitive function, an important determinant of economic decision-making and productivity (Heckman, Stixrud, and Urzua, 2006; Burks, Carpenter, Goette, and Rustichini, 2009; Dohmen, Falk, Huffman, and Sunde, 2010; Benjamin, Brown, and Shapiro, 2013). My hypothesis is based on studies from cognitive psychology, which suggest that anxiety can shift scarce attentional resources towards the perceived threat, thus reducing the available cognitive capacity for threat-unrelated processing tasks (Mathews, 1990; Eysenck, Derakshan, Santos, and Calvo, 2007; Robinson, Vytal, Cornwell, and Grillon, 2013; Moran, 2016; Sari, Koster, and Derakshan, 2017). Worrying is associated with the experience of intrusive negative thoughts, which can induce cognitive load in the same way as disruptions due to external distractors like a honking car or an incoming phone call.[1] While the increase in focus on the threat may lead to a better response in situations with a high risk of infection, my aim in this paper is to assess whether this comes at a cognitive cost in other domains. Since many of the decisions we take and tasks we perform during an epidemic are unrelated to the threat of the disease, the economic consequences could be substantial. In particular, a decline in available cognitive resources in threat-unrelated domains implies suboptimal economic choices and reduced labor productivity. Moreover, this cognitive cost of worrying might have a much larger incidence than the disease itself because not everyone who worries will also become infected and worry is often disproportionate to the actual threat.

I test this hypothesis in the context of an episode of heightened public concern about the possibility of a local Ebola outbreak in the US in October 2014. Ebola is a life-threatening hemorrhagic fever caused by Ebola virus, which emerges periodically in local outbreaks on the African continent. In the wake of the largest outbreak to date, four cases of Ebola were diagnosed in the US and led to the implementation of contact tracing procedures in the cities of Dallas, Texas, Cleveland, Ohio, and New York, New York throughout October 2014. The US Ebola cases were imported by incoming air travelers from affected African countries and spurred considerable public concern, with almost half of all respondents of a mid-October representative survey reporting worries that they or a family member would contract Ebola. Fears varied in intensity across space and time until subsiding in early November, as people learned that the US cases were well-contained by the authorities and did not lead to further local infections. One attractive feature of this setting is that the fraction of the US

[1] See, e.g., Deck and Jahedi (2015) for evidence that cognitive load impairs cognitive ability. Furthermore, other channels could also be at play. Notably, epidemics might increase emotional arousal more generally or evoke a scarcity mindset, with similar negative effects on cognitive function (e.g., Kaufman, 1999; Mullainathan and Shafir, 2013).

population that is physically affected by Ebola cases or resulting containment measures is vanishingly small, so any effects on cognition can only be driven by psychological responses to the disease outbreak.

For my analysis, I use data from tests of fluid intelligence, which were administered as part of a wave of survey interviews by the Health and Retirement Study (HRS). The HRS is a biennial panel study that conducts interviews with questions about a wide range of topics with a representative sample of the US population aged 50 and older. Combining public and restricted HRS data sources, I obtain a sample of cognitive test scores from about 500 HRS interviews conducted in October 2014, with detailed information on their date and location. This allows to match test scores with a measure of the local level of Ebola concern test takers were exposed to in the week and media market of their interview, which I construct from data on search interest for the term "Ebola" from Google Trends.

In the first part of my analysis, I investigate the association between Ebola concern at the interview and the resulting cognitive test performance in OLS regressions with various control variables. A major concern for observational studies on the effect of epidemic-induced worry on cognitive function is reverse causality: if the propensity to worry about an epidemic is partly determined by education or cognitive ability, living in a media market with a high level of Ebola concern could be a signal for low cognitive function rather than a cause of it. To alleviate this concern, I control for demographic predictors of cognitive function like age and education, changes in life circumstances that could be related to cognitive decline like retirement, as well as cognitive test scores from previous HRS interview waves. Moreover, I add proxies for time-invariant characteristics of the interview location like the local search interest for the search topic "virus" in 2013, and I include interview week and Census region dummies. I find a strong and highly significant negative relationship between the level of Ebola concern HRS respondents are exposed to during their interview and their resulting cognitive test performance. In the most demanding specification, a one standard deviation (SD) increase in Ebola concern is associated with a decline in test scores of about 0.14 SD.

In the second part of the analysis, I use an instrumental variables (IV) strategy to strengthen the claim that the documented effect is causal. This identification strategy exploits the fact that the specific timing and geographic location of the US cases has a random component. My approach builds on Campante, Depetris-Chauvín, and Durante (2020), who show that regional differences in Ebola concern are predicted by the geographical distance to the closest US Ebola location and use this for an IV estimation of the effect of fear on the outcomes of the 2014 US midterm elections. I demonstrate the existence of a similar first-stage relationship in my sample of HRS interviews, with predictably higher levels of Ebola concern in closer proximity to a publicly known US case. This holds also when controlling for distance to close large US cities more generally, thus accounting for the higher likelihood of Ebola case imports in urban centers. With respect to the exclusion restriction, I conduct two falsification tests. First, I show that in my sample of October interviews, the instrument is unrelated to the level of Ebola search interest in August—before the first US case—and to an equivalent search interest measure during the H1N1 pandemic in 2010. This alleviates the specific concern that distance to the closest US Ebola location could be associated with systematic regional differences in attitudes towards diseases or the tendency towards internet searches during disease outbreaks. Second, I find that there is no correlation between test scores and placebo versions of the instrument, which backdate the occurrence of the US Ebola cases by one or two months, in a different sample of HRS interviews conducted in September, just before the actual emergence of the first case. These findings suggest that distance to the closest US Ebola location is a valid instrument for Ebola concern in my setting. The corresponding reduced-form and IV results support the conclusions of the OLS analysis and indicate a substantial cognitive cost of Ebola concern. Extrapolating from the point estimate of the IV coefficient, the increase in Ebola concern caused by the emergence of the US Ebola cases in October relative to the level of Ebola search interest in August, when the WHO declared the Ebola outbreak in Western Africa a public health emergency, would imply a reduction in test scores of about 12 scale points. By construction of the score scale, this corresponds to a drop in the probability of answering a test item of equivalent difficulty correctly from 90 to 71 percent. This effect survives various robustness checks, including different variants of the instrument and different approaches of constructing an Ebola concern measure from search interest data.

A remaining concern is that the composition of my sample of test scores may be distorted because potential HRS respondents react to nearby US Ebola cases for fear of contracting the disease. If individuals with positive unobservable shocks to cognitive ability in 2014 were more likely to delay their interview after the occurrence of a close case, the documented negative influence of Ebola concern on cognitive test performance could also be driven by a selection effect. To assess this possibility empirically, I test whether there is a differential effect of distance to the closest US Ebola location on the date of the interview for respondents with high and low predicted test scores, where predictions are based on all available observable determinants of cognitive ability in the HRS data. While I find that interviews in closer proximity to US Ebola locations are on average conducted a few days later, the estimates do not provide any indication that this relationship varies by cognitive ability. Therefore, the results of my analyses point to a direct causal effect of epidemic concern on cognitive function.

My findings relate to a large body of research on the economic consequences of epidemics. This topic has gained traction during the recent COVID-19 pandemic because understanding the specific mechanisms by which the economy is disrupted is crucial to effectively alleviate the adverse economic effects through targeted government measures.[2] Existing research highlights fear of contagion as a key channel that can affect patterns of economic activity by triggering avoidance behavior, thereby causing both supply and demand shocks (e.g., Eichenbaum, Rebelo, and Trabandt, 2020; Farboodi, Jarosch, and Shimer, 2021). For instance, consumers avoid spending on goods and services that involve interpersonal contact like travel, restaurant visits or physical retail shopping (e.g., Rassy and Smith, 2013; Cox, Ganong, Noel, Vavra, Wong, et al., 2020; Chen, Qian, and Wen, 2021; Goolsbee and Syverson, 2021), and workers reduce their infection risk exposure by cutting labor supply or working from home (e.g., Brynjolfsson, Horton, Ozimek, Rock, Sharma, et al., 2020).[3] Moverover, there is evidence that drops in consumer expectations about future economic conditions during an epidemic are associated with fears of the disease (Binder, 2020; Fetzer, Hensel, Hermle, and Roth, 2020). These findings are in line with theoretical accounts on the influence of emotions on economic choices and judgement (Elster, 1998; Loewenstein, 2000), which emphasize the role of experienced emotions as motivators of specific mitigation behaviors, like an urge to withdraw from a dangerous situation in the case of fear. This perspective contends that emotional responses can cause a transient increase in the relative marginal benefit of specific choice alternatives relative to others, driven in part by emotion-induced changes in the weighting and perception of subjective probabilities, the evaluation of possible outcomes, and an elevated discounting of the future.[4] In

[2]Here, I restrict attention to studies that concern the psychological aspects of epidemics. Brodeur, Gray, Islam, and Bhuiyan (2021) provide an overview of research on the economic effects of the COVID-19 pandemic with a wider focus. For other studies of the political, social and economic effects of the 2014 Ebola epidemic, see Campante, Depetris-Chauvín, and Durante (2020), Flückiger, Ludwig, and Önder (2019), González-Torres and Esposito (2020), Yarkin (2020), and Kostova, Cassell, Redd, Williams, Singh, et al. (2019).

[3]However, Balgova, Trenkle, Zimpelmann, and Pestel (2021) find no relationship between job search intensity and health concerns in an analysis based on survey data from the Netherlands during the COVID-19 pandemic.

[4]Also see Loewenstein, Weber, Hsee, and Welch (2001), who develop a psychological model of choice under risk that explains why emotional reactions to risky situations and subsequent behavior can be disproportionate to the cognitive evaluation of the same risk. Their model and the reviewed evidence illustrate the various ways by which emotions can affect economic choices and judgements.

contrast, I document an adverse effect of epidemic-induced worry on task-available cognitive capacity, which is a component of the decision-making process. Thus, my results point towards a general reduction in the quality of cognitive choice rather than a shift towards specific choice options. Since the cognitive function test underlying the main outcome variable of my analysis is completely unrelated to respondents' object of concern, changes in the relative desirability of emotion-mitigating choice options cannot explain the observed effect. Instead, my results are consistent with an analogous emotion-induced shift towards the object of concern on the level of cognitive or attentional resources. This opens up an alternative explanation for seemingly irrational observed behavior like the hoarding of goods during the COVID-19 pandemic (Baker, Farrokhnia, Meyer, Pagel, and Yannelis, 2020), which might be triggered in part by cognitive errors. It also suggests a specific new mechanism by which epidemics may disrupt the economy. Importantly, worry may affect the decision-making and productivity of all economic agents and in all domains, even if they are not physically affected by the disease itself or resulting government restrictions.

In independent research, Bogliacino, Codagnone, Montealegre, Folkvord, Gómez, et al. (2021) show that self-reported experience of various negative shocks during the COVID-19 pandemic is related to lower scores on a cognitive test. Because their analysis is correlational, they cannot rule out that this finding is driven by endogeneity in the sense that individuals with exogenously lower cognitive ability are more likely to be affected by negative shocks.[5] An additional priming intervention they conduct to substantiate a causal interpretation is ineffective. In contrast, my identification strategy relies on quasi-random variation in the date and location of Ebola cases in the US, and I provide evidence that selection effects are unlikely to play a role for my results.

My findings also contribute to a strand of literature in behavioral delevelopment economics on the psychology of poverty. In an influential book, Mullainathan and Shafir (2013) hypothesize that poverty itself can impair decision-making because perceived scarcity captures and taxes cognitive resources, thereby creating a vicious cycle of poverty.[6] This idea finds support in results from priming experiments as well as analyses of natural variation in rainfall and income before and after payday for farmers in developing countries, which indicate that concerns about low levels of income and income uncertainty can reduce performance on cognitive tests (Mani, Mullainathan, Shafir, and Zhao, 2013; Lichand and Mani, 2020).[7] I extend the scope of this line of work by providing initial evidence that concerns about health can impede cognition in similar ways as financial strain. This suggests that the higher incidence of diseases and their more negative health consequences in poor countries could also be a factor for documented puzzles in the economic behavior of the poor (e.g., Banerjee and Duflo, 2007).

The remainder of the paper is structured as follows. In Section 2, I briefly outline relevant aspects of the Western African Ebola virus epidemic in 2014 and public reactions to it in the US, which provides the context of the study. Afterwards, I turn to the different data sources and methodological considerations for the construction of important analysis variables in Section 3. The results of an initial OLS analysis of the

[5]For instance, Adams-Prassl, Boneva, Golin, and Rauh (2020) document that less educated workers were more likely to lose their job during the early stages of the COVID-19 pandemic in the US and the UK. With respect to health shocks, Benson, Amato, Osler, Hosmer, and Malhotra (2021) find that the high school dropout rate is one of the strongest predictors variation in COVID-19 cases and deaths across US counties.

[6]See Dean, Schilbach, and Schofield (2019) and Kremer, Rao, and Schilbach (2019) for recent reviews that also discuss other dimensions of poverty that may impair cognition, like noise, environmental pollution, nutrition, sleep deprivation and mental health. Haushofer and Fehr (2014) survey a related body of research that explores how poverty-induced increases in stress and negative affect impact preferences, which also implies changes in economic decision-making.

[7]Recent investigations of the effect of financial strain on cognitive function yield mixed results that do not always replicate the findings of the initial studies (e.g., Carvalho, Meier, and Wang, 2016). The general state of the evidence is discussed in a recent review of the scarcity hypothesis by de Bruijn and Antonides (2021).

association between Ebola concern and cognitive function are presented in Section 4. In Section 5, I describe the identification strategy and findings of the IV analysis which forms the core of the paper. I assess the possibility that my results could be affected by selection into interview dates in Section 6. Finally, Section 7 discusses the interpretation of my main findings, and Section 8 concludes.

2 Background

The context of my analysis is an episode of heightened public concern about Ebola in the US in October 2014 after a few isolated cases emerged in the country as a consequence of the Western African Ebola virus epidemic. As outlined by Goeijenbier, van Kampen, Reusken, Koopmans, and van Gorp (2014), Ebola is a life-threatening hemorrhagic fever caused by Ebola virus. It is known for its high case fatality rate of between 50 and 90 percent and its frightening symptomatology. Typically, infected persons develop fever, vomiting and diarrhoea about four to ten days after exposure to the virus, followed by manifestations of internal and external bleeding. In lethal cases, death occurs due to circulatory shock, low blood pressure, or multi-organ failure about one to two weeks after the onset of initial symptoms. Even though chances of survival can be enhanced by early symptomatic treatment like the replacement of lost body fluids, no approved antiviral medication or vaccine for Ebola existed in the US in 2014. However, despite the severity of the disease for the infected, the objective risk of death from Ebola for individuals in developed countries is low. This is because human-to-human transmission requires direct contact with body fluids of symptomatic patients, making local outbreaks highly unlikely.

The Western African Ebola virus epidemic is the largest Ebola outbreak to date, causing a total of 11 310 reported deaths (World Health Organization, 2016). It began in a rural area in Guinea in December 2013 and spread rapidly across Guinea, Liberia, and Sierra Leone in the following year, despite multinational control efforts aimed at containing transmissions. Coltart, Lindsey, Ghinai, Johnson, and Heymann (2017) identify traditional burial practices involving physical contact with the bodies of the deceased, limited healthcare capacities with inadequate protective equipment of healthcare workers, and a fast transmission of the virus into densely populated centers due to highly mobile communities as key factors that propagated the spread of the virus during this phase of the epidemic. On August 8, 2014, the World Health Organization (WHO) declared Ebola an international public health emergency (WHO Ebola Response Team, 2014). Still, the outbreak was contained to West Africa until the official end of the epidemic in 2016, with only few isolated cases imported into other regions of the world by returning travelers from the three affected countries.

Four Ebola cases were diagnosed on US soil, all in close succession during autumn 2014. As a result, the Centers for Disease Control and Prevention (CDC) implemented contact tracing procedures at three locations within the US to identify and monitor about 450 individuals at risk of exposure to the disease.[8] The first patient was an incoming traveler from Liberia who arrived in Dallas, Texas, on September 20 (Chevalier, Chung, Smith, Weil, Hughes, et al., 2014). He presented himself in the emergency department of a local hospital with fever symptoms twice in the following days but was only tested for Ebola on his second appearance, resulting in

[8]Six additional US citizens were diagnosed with Ebola in 2014 while working with medical teams to stop the epidemic in West Africa. These patients were medically evacuated to the US for treatment in one of four specialized hospitals with biocontainment units (Rainisch, Asher, George, Clay, Smith, et al., 2015) and did not pose an infection risk for the US population at any time. Although the evacuations also received media attention, they did not provoke a comparably strong emotional response. For instance, they resulted in significantly fewer e-mail inquiries and visits to the CDC's Ebola webpages than the US-diagnosed cases (Bedrosian, Young, Smith, Cox, Manning, et al., 2016).

the first Ebola virus infection diagnosed in the US on September 30. Consequently, the patient was isolated for further treatment under special precautions, and potentially exposed contacts were traced and monitored by the CDC. The man died from Ebola on October 8. Two nurses involved in direct care of the first patient developed symptoms and were tested positive for Ebola in Dallas, Texas, on October 11 and 15, respectively. They constituted the second and third US Ebola case, leading to the tracing and monitoring of additional contact persons. Since the third patient had traveled to Cleveland, Ohio, in the days before her Ebola diagnosis while potentially being infectious, the CDC also implemented contact tracing procedures and conducted Ebola preparedness assessments of local hospitals at this location (McCarty, Basler, Karwowski, Erme, Nixon, et al., 2014). The fourth US case was a physician who returned to the City of New York, New York, after having treated Ebola patients in Guinea (Yacisin, Balter, Fine, Weiss, Ackelsberg, et al., 2015). He reported fever symptoms and tested positive for Ebola on October 23, resulting in the home confinement and monitoring of three close contact persons. The patient survived and was discharged from hospital on November 10.

Despite the occurrence of local Ebola cases, the objective risk of an epidemic outbreak in the US was considered to be extremely low by experts at that time (e.g., Gomes, Pastore y Piontti, Rossi, Chao, Longini, et al., 2014). Consistent with this assessment, there were no locally transmitted Ebola cases outside of the healthcare sector. Yet, the US Ebola cases caused considerable public concern about a major Ebola outbreak in the US. In nationally representative surveys conducted in the second week of October, almost half of the respondents reported being worried that they or a family member would contract Ebola (SteelFisher, Blendon, and Lasala-Blanco, 2015). Fears were also reflected in disproportionate behavioral reactions like private hazmat suit purchases and the shunning of returning travelers from Africa even when they had visited countries without documented Ebola cases (Bedrosian et al., 2016). SteelFisher, Blendon, and Lasala-Blanco (2015) conjecture that public concern was fueled by sensationalist media coverage, limited trust in the US federal government and health authorities, and misperceptions about the contageousness of the disease, with a substantial fraction of the population in the belief that Ebola is airborne.[9] Eventually, public concern subsided as people learned that isolated Ebola cases in the US were well-contained and did not lead to further local infections. Consequently, the level of worry about Ebola reported in national surveys saw a marked decline in early November (SteelFisher, Blendon, and Lasala-Blanco, 2015).

The episode of high public concern during October 2014 poses an ideal setting for my analysis of the effects of emotional responses to epidemic risk on cognitive function. First, as illustrated by Campante, Depetris-Chauvín, and Durante (2020), it is characterized by significant geographic and temporal variation in worry about Ebola, with predictably higher concern in close proximity to recent US Ebola cases. Second, the specific timing and geographic location of US cases has a random component. In contrast to outbreaks that originate within a country because of specific characteristics of the local environment or population, the US Ebola cases were imported by incoming air travelers. Due to minor coincidences, infected travelers could just as well have arrived on a different day in another large US city, resulting in a very different case pattern. This makes it unlikely that proximity to US Ebola cases is systematically related to regional differences in cognitive ability after accounting for distance to large US cities, thus facilitating an IV strategy to identify a causal effect. Third, public concern was disproportionate to the actual level of epidemic activity. The fraction of the US population that is physically affected by the disease or resulting containment measures is vanishingly small. There were

[9]Towers, Afzal, Bernal, Bliss, Brown, et al. (2015) provide evidence that Ebola-related news videos contributed to the propagation of fear based on parameter estimates from a model of news contagion.

also no financial ramifications for the average citizen. This implies that US Ebola cases can only affect cognitive function via the public's emotional response.

3 Data

To test the hypothesis that epidemic-induced worry impedes cognitive function in the context of the occurrence of Ebola cases in the US, I require a dataset with geographically disaggregated measures of cognitive function and Ebola concern in the US during October 2014. Since temporal and geographic variation in Ebola concern can be large, it is especially important that the data includes precise information on both the location and date of cognitive performance observations. To accomplish this, my approach is to supplement a dataset of cognitive function test scores with a measure of Ebola concern constructed from Google Trends data.

3.1 Data on Cognitive Function

My main data source is the Health and Retirement Study (HRS). The HRS is a panel study that conducts interviews about various topics related to health, economic situation, and family status with a representative sample of the US population aged 50 and older every two years. Importantly, it also includes tests of cognitive function. Interviews are either carried out either by telephone or in person at respondents' homes, with the interview mode and date determined in part by respondent availability. For the 2014 wave of the HRS, 18 747 respondents were interviewed between March 2014 and April 2015. Of these interviews, 801 were conducted in October, during the period of heightened Ebola concern described in Section 2, and will be the main focus of my analysis.

I use data on cognition and various sociodemographic characteristics from the 2010 to 2014 waves taken from the RAND HRS Longitudinal File (RAND Center for the Study of Aging, 2020) as well as additional variables from the individual wave datasets RAND HRS 2010 Fat File (RAND Center for the Study of Aging, 2017), RAND HRS 2012 Fat File (RAND Center for the Study of Aging, 2015) and RAND HRS 2014 Fat File (RAND Center for the Study of Aging, 2018). To pinpoint the circumstances of each interview, I additionally use restricted data on interview locations (Health and Retirement Study, 2019a) and interview dates (Health and Retirement Study, 2019b). In particular, I have access to information on the county in which the interview took place (item STCTYFIPS10) and the beginning date of the interview (items OIWMONTH, OIWDAY and OIWYEAR). A minor limitation of this data is that the beginning date of the interview does not always equal the date of the cognitive tests. A small fraction of interviews is suspended at some point and completed on a later date, e.g., because the respondent suffers from an acute health problem. I restrict my main analysis sample to interviews that were both started and completed in October 2014, but I do not observe whether or at what point an interview was interrupted within the month. Therefore, the date assigned to each interview is a lower bound for the day on which the cognitive tests were conducted. As a result, my analysis may falsely treat some cognitive function tests as having occurred before rather than after a given US Ebola case, which would typically imply an underestimation of the level of Ebola concern and distance to the closest US case that test was subjected to.

The HRS contains tests of three different aspects of fluid intelligence, the concept that economists often study when they are interested in cognitive ability: quantitative reasoning, verbal reasoning, and retrieval fluency (Fisher, McArdle, McCammon, Sonnega, and Weir, 2014).[10] A subset of the tests is administered in each wave. The only fluid intelligence test available in the 2014 wave for a large number of respondents is the verbal analogies task, a measure of verbal reasoning based on version III of the Woodcock–Johnson Tests of Cognitive Abilities. Each test item consists of a word pair that defines a specific logical relationship and a single third word. The respondent's task is to complete the analogy by naming the matching fourth word such that the logical relationship of the newly created pair is identical to that defined by the given word pair. Items are read out to the respondent in the form of open-ended sentences like "Mother is to Daughter as Father is to ..." without response options. In the HRS, the verbal analogies task is conducted as a block-adaptive test consisting of a total of six items. The difficulty of later items depends on respondents' performance on early items, allowing to get a relatively nuanced assessment of performance even with a short test.

The result is reported as a W score of verbal reasoning, which constitutes my main outcome measure (item OVESCORE in the RAND HRS 2014 Fat File). The W score is defined on a numeric scale in terms of the difficulty of the item that a repondent is predicted to answer correctly with exactly 50 percent probability (Jaffe, 2009). The scale is centered on a value of 500, which marks the performance of an average 10-year old. Importantly, the W score is designed as an interval score, implying that a given score difference always corresponds to the same peformance difference, irrespective of where on the scale it is. For instance, a 10 point increase in W score always implies an increase in the probability of correctly solving the old score's reference item from 50 percent to 75 percent. This interval property is crucial for my purpose because it is an implicit assumption underlying the analysis of average treatment effects (Jacob and Rothstein, 2016).

The panel structure of the HRS also allows to control for cognitive function test scores from the two previous survey waves, for which the set of eligible respondents was largely identical.[11] In these waves, the two other measures of fluid intelligence were administered: respondents were asked to solve number series tasks to test their quantitative reasoning, and their retrieval fluency was measured by the number of distinct animals they can name within a time limit of one minute. In addition, the HRS also contains cognitive tests designed to detect early signs of cognitive decline that often precede neurodegenerative diseases like dementia or Alzheimer's disease. Of these, I use the scores of a word recall test and a serial sevens test as additional controls.[12] Finally, the rich set of variables collected in the HRS also includes information on a number of demographic characteristics and on changes in general health status or lifestyle since the previous wave that could be associated with cognitive decline.

3.2 Measuring Ebola Concern

I construct measures of Ebola concern for the location and date of the HRS interviews based on data on internet search interest provided by Google Trends. Such data is nowadays frequently used in economic research to study behavior in real-time or to proxy for outcomes for which no direct measure is available (e.g. Da, Engelberg, and Gao, 2011; Baker and Fradkin, 2017; Bacher-Hicks, Goodman, and Mulhern, 2021; Brodeur, Clark, Fleche, and Powdthavee, 2021).

[10] For instance, Raven's Matrices is a test of fluid intelligence that has been used in the economic literature on the psychology of poverty (e.g., Mani et al., 2013). Fluid intelligence is defined as an indvidual's general ability to reason and solve problems in novel situations that do not depend on acquired knowledge.

[11] Refer to Online Appendix A for details on the construction of control variables based on HRS data.

[12] The other cognitive tests are either too easy, like naming the current date, or only collected for a subset of respondents in different waves.

Google Trends provides data on the number of searches for a requested keyword or topic relative to the total Google search volume in a given region and time period. The data is normalized such that the highest relative search interest in the query has a value of 100. Two features of the data pose methodological challenges: First, no data is reported for region-periods for which the absolute number of searches of the keyword is below a specific threshold for reasons of privacy protection. Second, Google Trends only evaluates a random subsample of the universe of all searches, so the provided values can fluctuate, especially for regions with a small population under high temporal and geographic granularity.

I obtain data on search interest for the keyword "Ebola" by week from August to October 2014 for all media markets in the US from the Google Trends website.[13] This is the highest available level of granularity with good data quality for this keyword. To smooth out the sampling error for smaller regions, I repeat the query 100 times for each media market. In each request, I also include the US as a whole and rescale the returned time series such that the peak of the US time series—the week from October 12 to 18—has a value of 100 to make the data from different queries comparables. Missing values due to the privacy theshold introduce a selection bias because for some weeks in smaller media markets, data is only reported if the random sample has exceptionally high relative search interest. To deal with this source of bias, I code missing values as zero and then use the median across all iterations, including those with missing values. This results in an unbiased estimate of the median as long as data is reported in at least 51 iterations, which is the case for all relevant weeks and media markets for my sample of HRS respondents.[14] The resulting dataset is a media market–week panel of Ebola search interest, which I merge to the interview counties and dates of my HRS respondent sample using a crosswalk provided by Sood (2016).

In addition, I collect search interest for the keyword "anxiety" and the topic "virus (infectious agent)" in all media markets for the year 2013, and for the topic "swine flu (swine influenza)" for the period between April 25, 2009—the day the WHO declared a public health emergency after the novel H1N1 influenza virus started to spread across several US states—and August 10, 2010—the day the WHO declared the H1N1 pandemic over.[15] Again, I repeat each query 100 times and use the median value for each media market. These variables are used as covariates or placebo outcomes in the analysis.

For my analysis, I want to construct a measure of the level of Ebola concern HRS respondents feel on the day of the interview, which is not necessarily equal to the level of Ebola search interest during that week. Campante, Depetris-Chauvín, and Durante (2020) show in daily data that local Ebola search interest and Twitter activity react strongly to new US Ebola cases, but these responses are short-lived. In particular, they fade much faster than would be expected on the basis of the national surveys of Ebola worries reported in SteelFisher, Blendon, and Lasala-Blanco (2015). This suggests that Ebola concern lingers past initial search activity: individuals seem to actively search information about Ebola on the internet only when they first get worried about a nearby US Ebola case and afterwards rely on passive information acquisition through media reports, but they remain concerned. In my main specifications, I therefore use the average search interest in a given media market across all weeks between the week of the first US Ebola case and the interview week as my measure of Ebola concern. This closely follows the approach taken by Campante, Depetris-Chauvín, and

[13]Media markets are geographical areas—usually groups of counties—that receive the same television and radio signals. There are 210 media markets in the US.

[14]The results of a robustness check that uses another approach for data aggregation are presented in Section 5.4.

[15]Topics are collections of Google searches that capture a specific meaning rather than an exact search phrase. I use topics rather than keywords for "virus" to avoid capturing the alternative meaning "computer virus", and for "swine flu" to also capture searches for the common alternative name "H1N1". For "anxiety", the distinction between keyword and topic is empirically irrelevant.

Durante (2020), who use the search interest across a five-week period between the first US case and the US midterm elections on November 4.

3.3 Other Data Sources

To compute the kilometer distance of interview counties to the closest publicly known US Ebola case on the date of the interview as well as to the closest large urban center, I rely on data from the US Census Bureau. I determine the 100 largest US cities by 2014 population using population estimates reported in U.S. Census Bureau (2020). Geographic coordinates of US Ebola locations and large US cities are taken from Gazetteer Files (U.S. Census Bureau, 2014), and I use coordinates of the mean centers of population of interview counties from U.S. Census Bureau (2019). All calculated distances are geodetic distances, i.e., they correspond to the length of the shortest curve along the surface of an ellipsoidal model of the earth, based on equations derived in Vincenty (1975). They are included in regression models in log-transformed form because changes in the distance to a US Ebola case should affect Ebola concern less the farther away it is.

3.4 Main Sample

In addition to respondents with missing data on key variables, I also exclude respondents from Alaska and Hawaii because variation in geographical distance to Ebola cases in the contiguous US is unlikely to matter for their level of Ebola concern. This results in a final sample of 492 respondents from 105 media markets, covering all Census regions of the US. Descriptive statistics for this sample are reported in Table B.1.

4 OLS Estimation

I start off the analysis by investigating the association between the level of Ebola concern HRS respondents are exposed to during their interview and their resulting cognitive test performance in OLS regressions with various control variables. This exploits all variation in Ebola concern across media markets and weeks observed in the wake of the occurrence of Ebola cases in the US in October 2014. The estimation equation is

$$VerbalReasoningScore_{ict} = \alpha + \beta EbolaConcern_{mt} + \boldsymbol{\gamma}_1' \boldsymbol{X}_i + \boldsymbol{\gamma}_2' \boldsymbol{C}_c + \boldsymbol{\gamma}_3' \boldsymbol{M}_m + \delta_t + \rho_r + \epsilon_{imt}, \tag{1}$$

where $VerbalReasoningScore_{ict}$ is the cognitive function test score individual i achieves in her HRS interview in county c and week t, and $EbolaConcern_{mt}$ is the level of Ebola concern in the corresponding media market in that week. The hypothesis that worrying about Ebola is associated with lower cognitive function corresponds to $\beta < 0$. Depending on the specification, I additionally include a vector of covariates $\boldsymbol{X}_i$, which can contain demographic variables, cognitive function test scores from previous HRS waves, and interview characteristics. Similarly, $\boldsymbol{C}_c$ and $\boldsymbol{M}_m$ are vectors of covariates on the county-level and media market–level, and δ_t and ρ_r are dummies for interview week and Census region, respectively.

All regressions use cluster-robust standard errors that account for heteroscedasticity and arbitrary intra-cluster correlation within media markets. This is necessary because the current level of Ebola concern in a given media market also depends on US Ebola cases from previous weeks, implying that Ebola concern is correlated across time within media markets.

The coefficient estimates for the OLS regression model are reported in the first four columns of Table 1. In column (1), I estimate Equation 1 without any controls, finding that being exposed to higher levels of Ebola concern during the interview is associated with significantly lower test scores. Of course, the documented relationship is susceptible to endogeneity problems. Especially in light of the potential role of misperceptions about the transmission dynamics of Ebola for the spread of fear, a major concern is reverse causality: media markets in which a higher fraction of the population is relatively less educated or has lower cognitive function could develop more worry about an Ebola epidemic. Then, living in a media market with a high level of Ebola concern could be a signal for low cognitive function rather than a cause of it. To mitigate this concern, I exploit the richness of HRS data and add a variety of control variables.

Table 1. Ebola Concern and Cognitive Function: OLS Estimates

	Dependent variable: Verbal reasoning score				
	(1)	(2)	(3)	(4)	(5)
Ebola concern	−0.406***	−0.241***	−0.260***	−0.285***	
	(0.116)	(0.078)	(0.090)	(0.095)	
Ebola search interest before first US case					0.058
					(0.296)
Demographic controls	No	Yes	Yes	Yes	No
Baseline cognition controls	No	Yes	Yes	Yes	No
Interview controls	No	No	Yes	Yes	No
Location controls	No	No	Yes	Yes	No
Census region dummies	No	No	No	Yes	No
Observations	492	492	492	492	492
R^2 (adjusted)	0.037	0.440	0.437	0.437	−0.002
Mean (dependent variable)	505.089	505.089	505.089	505.089	505.089

Notes: OLS estimates for the main sample, with standard errors in parentheses. Standard errors are robust to heteroscedasticity and arbitrary intra-cluster correlation within media markets. *Ebola concern* is the average weekly relative search interest for the term "Ebola" in the media market of the interview location over all weeks between September 28 and the week of the interview. *Ebola search interest before first US case* is the relative search interest for the term "Ebola" in the media market of the interview location in the first week of August. Demographic controls include age, age squared and dummies for gender, race, education, and changes in household status (living alone, moving into a nursing home), employment status (retirement) and health (being diagnosed with dementia or a stroke) since the previous HRS wave in 2012. Baseline cognition controls are scores from tests of quantitative reasoning, retrieval fluency, word recall and the serial sevens test in 2012 and changes in these scores between 2010 and 2012. Interview controls are an indicator for a telephone interview, the number of call attempts until an interview was conducted, and interview week dummies. Location controls include the logarithm of the kilometer distance to the closest large city, Ebola search interest before the first US Ebola case, and media-market level relative search interest for the topics "anxiety" and "virus" in 2013. * denotes $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$, all for two-sided hypothesis tests.

In column (2), I control for demographic characteristics that predict performance on cognitive tests, like age and education, as well as changes in life circumstances since the previous interview wave that could be associated with cognitive decline, like being diagnosed with dementia, moving into a nursing home, starting to live alone, or retiring from work. Moreover, I include several measures of baseline cognitive ability, using scores from tests of cognitive function in the previous two HRS interview waves. In particular, I add 2012 scores and changes in scores from 2010 to 2012 of the other two HRS tests of fluid intelligence, quantitative reasoning and retrieval fluency. Analogously, I also include levels and trends of scores from a word recall and serial sevens test, both of which are aimed at detecting early warning signs of cognitive decline.

In column (3), I additionally include control variables for interview characteristics. These consist of the number of call attempts by the interviewer until an interview is conducted, a dummy for telephone interviews, and a set of dummies for the calender week of the interview. This addresses a concern that arises as a result of the HRS field operations. Since interview mode and date are partly determined by participant responsiveness, those who are in worse health or go through personal turmoil during the data collection phase tend to be interviewed later during the wave. As a result, later interview dates can be a manifestation of unexpected negative health or cognitive capacity shocks that could introduce a spurious correlation between Ebola concern and cognitive test scores because both are correlated with interview dates.[16] With interview week dummies, the coefficient on Ebola concern is identified from geographic variation only and therefore immune to this concern. A further group of control variables is directed at time-invariant influences of the interview location. To capture systematic differences across media markets in general susceptibility to fear and the tendency to search for diseases on the internet, I add measures of search interest for "anxiety" and "virus" in 2013 as well as Ebola search interest before the first US case. As a result, the coefficient of interest in this specification isolates the effect of additional Ebola searches that arise from the perceived threat of an epidemic after the emergence of cases in the US. I also include the distance to the closest large US city because this will be an important covariate in the IV estimation.

Finally, the fourth column adds Census region dummies, ruling out that an observed relationship is driven by arbitrary other regional determinants of test performance that might be correlated with Ebola concern.

The coefficient estimate shrinks after the inclusion of demographic and baseline cognition controls, but remains highly significant in all specifications. In the most demanding specification with the full set of covariates, the estimated reduction in verbal reasoning score is 4.11 W scale points, corresponding to about 0.138 SD, for a one SD increase in Ebola concern.

Comparing this effect size to that of other impediments of cognition is difficult because other studies employ different tests to assess different dimensions of cognitive functioning. Park (2020) reports a 0.055 SD decrease in student performance on standardized exams per standard deviation increase in exam day temperature. In a field experiment, Dean (2021) finds that a ten decibel increase in engine noise reduces a summary index of performance on several cognitive function tests by 0.068 SD. Taken at face value, my coefficient estimate indicates that the effect of worrying about an epidemic is about twice as large as that of these environmental factors. On the other hand, it is markedly smaller than the 0.6 to 0.7 SD increase in Raven's Matrices scores of financially constrained sugarcane farmers after the receipt of their annual harvest income that can be derived from Mani et al. (2013).

For a different way of putting the magnitude of the effect into perspective, consider the implied difference in cognitive function between a situation with and without Ebola worries. From Table B.1, the difference between the average level of Ebola concern in my sample during October 2014 and average Ebola search interest in the first week of August, which constitutes the peak of the pre-October time series, is $63.7 - 27.3 = 36.4$ units.[17] Assuming that search interest in August is driven by public interest and the October increase stems from worrying about the possibility of a US epidemic, public concern about Ebola would be associated with a reduction in verbal reasoning score of 10.4 points on the W scale. This corresponds to a drop in the probability

[16] Indeed, a regression of cognitive function on interview date across all HRS respondents in the 2014 wave yields a significantly negative relationship.

[17] The equivalent numbers for the national US are very similar.

of answering a test item of equivalent difficulty correctly from 50 to about 25 percent (Jaffe, 2009).[18] This extrapolation indicates a substantial cognitive cost of worrying about an epidemic, with the potential of adverse economic consequences both in the form of suboptimal decisions in the private sphere and reduced productivity at work.

Column (5) of Table 1 contains estimates from a placebo check. In particular, I verify that there is no correlation between the cognitive function of my HRS respondent sample and Ebola search interest before the first US Ebola case, when geographic variation in search interest is unlikely to be driven by worry about an epidemic in the US. To this end, I regress the cognition test scores of October HRS interviews on Ebola search interest in the media market of the interview in the first week of August, when the WHO declared Ebola an international health emergency. This addresses the possibility that respondents with lower cognitive function happen to come from media markets with systematically higher public interest in Ebola or a higher propensity to search for Ebola on Google, for reasons other than worry. Such a correlation could confound the estimate of β in the regression model of Equation 1. Therefore, it is reassuring that the coefficient estimate of this exercise is close to zero and far from statistically significant.

5 Instrumental Variables Estimation

The previous section documents a robust and economically meaningful association between the level of Ebola concern participants are exposed to at the time and location of their interview and their resulting cognitive test scores. It also alleviates the most obvious endogeneity concerns. Yet, it is difficult to justify a causal interpretation of the observed relationship based on selection on observables alone. While controlling for individuals' test scores in 2010 and 2012 rules out reverse causality based on stable differences in cognitive ability, Ebola concern could still be related to unobserved local factors that accelerate cognitive decline. For instance, the strength of age-associated cognitive impairment and Ebola concern could both be correlated with local residents' media consumption, which would imply a downward bias. On the other hand, measurement error in the independent variable might induce attenuation bias towards zero.

To overcome these remaining challenges, I implement an instrumental variables strategy that follows Campante, Depetris-Chauvín, and Durante (2020). The underlying idea is that people will be more worried about Ebola if they live in close proximity to a recent US Ebola case. Since the specific location and timing of Ebola cases in large US cities is quasi-random, the distance to the actual cases then provides a source of exogenous variation in Ebola concern.

To exploit this, I construct a variable $EbolaDistance_{ct}$, which is the logarithm of the kilometer distance between the county of the HRS interview of individual i and the closest location with a relationship to a US Ebola case that was publicly known on interview day. The three Ebola locations are Dallas, Texas, where the first and second US Ebola case were diagnosed on September 30 and October 11, respectively, Cleveland, Ohio, which the third US Ebola case had visited immediately before her diagnosis on October 15, and the City of New York, New York, where the fourth and last US case was diagnosed on October 23. If (i) the relationship between the newly constructed variable and my measure of Ebola concern is strong and (ii) it satisfies the exclusion

[18] The corresponding drop in success probability for an easier test item that is answered correctly at a rate of 90 percent in the absence of concern about Ebola is 15 percentage points.

restriction, i.e., it is uncorrelated with other determinants of cognitive function after adjusting for covariates, then distance to the closest US Ebola location will be a valid instrument for Ebola concern. Consequently, two-stage least squares estimation of Equation 1 will yield an unbiased estimate of the causal effect of Ebola concern on cognitive function. In contrast to the OLS analysis, this identification strategy only uses the share of observed variation in Ebola concern that is the result of the exogenous shock posed by the emergence of close US Ebola cases.

To increase statistical precision and adjust for potential sources of correlation between the instrument and the outcome, I include the same control variables as in the OLS estimation in Section 4. Importantly, the covariates include distance to the closest large US city to account for the higher case probability that stems from their higher travel volume. This control variable is computed in just the same manner as the instrument, but considers the 100 largest US cities by 2014 population irrespective of their relationship to a US Ebola case.[19]

In the following subsections, I will first show that distance to US Ebola cases is a strong predictor of Ebola concern. I will then present two falsification tests conducted to assess the validity of the exclusion restriction. Afterwards, I will report the results of the IV estimation and wrap up by summarizing the outcomes of various robustness checks.

5.1 Ebola Concern and Distance to US Ebola Locations

To convey a first impression of the relationship between proximity to Ebola cases and Ebola concern, the maps in Figure 1 depict the geographic distribution of Ebola concern across all media markets of the contiguous US. For each of the five weeks of October 2014 separately, deciles of the distribution of Ebola concern are colored in different shades of blue, with darker tones indicating media markets with higher levels of concern. The three Ebola locations appear on the maps as red dots starting from the week in which the respective case was diagnosed. Clearly, proximity to Ebola locations is associated with higher levels of worry in all weeks of October.

Table 2 contains estimates from corresponding first-stage regressions in my sample of HRS interview respondents, allowing for a formal assessment of the strength of the relationship between distance to close US Ebola locations and Ebola concern. As a natural benchmark, I first regress the level of Ebola concern in the media market of the interview on distance to the closest large US city. The estimated coefficient is displayed in column (1). It is close to zero and statistically insignificant, implying that distance to large cities alone does not predict Ebola concern.

Columns (2) to (4) then present the first-stage estimates from regressions on the proposed instrument. I include distance to the closest US Ebola case once as the only independent variable, once only with adjustment for distance to the closest large city, and once with the full set of IV covariates. In all cases, the coefficient is large and highly significant. A one unit increase in logarithmized distance to the closest US Ebola case, which is equivalent to 0.9 SD, increases Ebola concern by about 9 units. This corresponds to one fourth of the total increase in public concern about Ebola in October relative to August 2014. As indicated by the R^2 in column (2), proximity to US Ebola cases explains 30 percent of the variation in Ebola concern in my sample.

[19]Note that the three US Ebola locations are themselves among the 100 largest cities in the US, taking the 1st, 9th, and 48th place in the ranking.

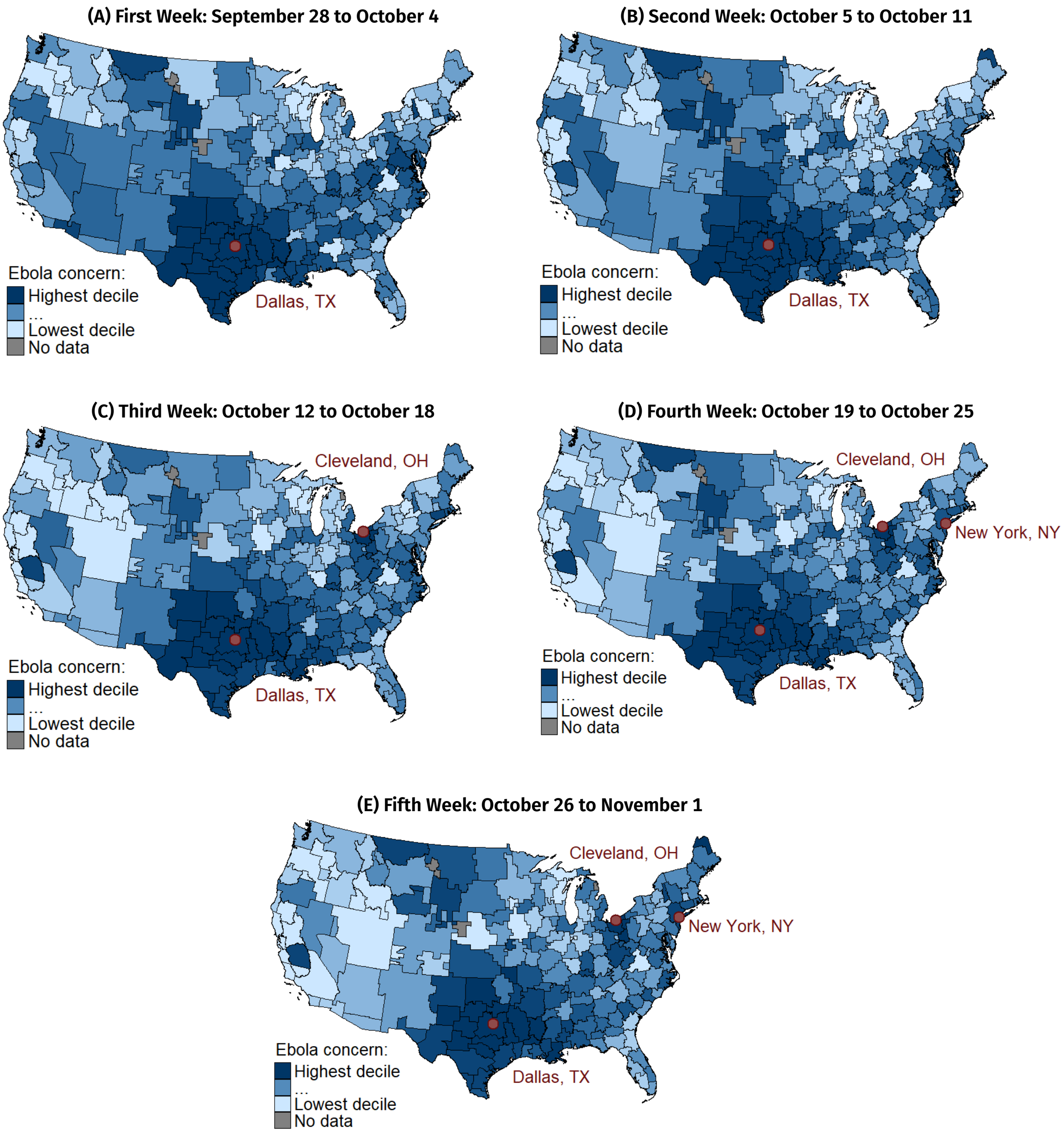

Figure 1. Ebola Case Locations and Geographic Variation in Ebola Concern Across Media Markets by Week

Notes: Choropleth maps of the geographic variation in the main measure of Ebola concern for each week in October 2014. The level of Ebola concern in a given media market and week is defined as the average weekly relative search interest for the term “Ebola” in that media market over all weeks since September 28. Each panel shows the spatial distribution of Ebola concern within a specified week, with media markets in higher deciles colored in darker shades of blue. Media markets for which the Ebola concern measure cannot be constructed because of insufficient search interest data are colored in gray. Locations with a relationship to a US Ebola case that was publicly known by the end of that week are marked with red dots: The first and second US case were diagnosed in Dallas, TX, on September 30 and October 11, respectively. The third Ebola diagnosis, with connections to both Dallas, TX, and Cleveland, OH, became publicly known on October 15. The fourth and last US case was diagnosed in the City of New York, NY, on October 23.

Table 2. Ebola Concern and Distance to US Ebola Locations: First-Stage Estimates

	Dependent variable: Ebola concern			
	(1)	(2)	(3)	(4)
Distance to closest large city	0.110		0.549	−0.292
	(0.874)		(0.730)	(0.448)
Distance to closest Ebola location		−8.773***	−8.853***	−9.189***
		(2.782)	(2.769)	(2.602)
Demographic controls	No	No	No	Yes
Baseline cognition controls	No	No	No	Yes
Interview controls	No	No	No	Yes
Location controls	No	No	No	Yes
Census region dummies	No	No	No	Yes
Observations	492	492	492	492
R^2 (adjusted)	−0.002	0.299	0.301	0.660
Mean (dependent variable)	63.698	63.698	63.698	63.698
Effective F-statistic		9.945	10.221	12.474

Notes: First-stage estimates for the main sample, with standard errors in parentheses. Standard errors are robust to heteroscedasticity and arbitrary intra-cluster correlation within media markets. *Ebola concern* is the average weekly relative search interest for the term "Ebola" in the media market of the interview location over all weeks between September 28 and the week of the interview. *Distance to closest large city* is the logarithm of the kilometer distance to the closest of the largest 100 US cities by 2014 population. *Distance to closest Ebola location* is the logarithm of the kilometer distance to the closest location with a relationship to a US Ebola case that was publicly known on the day of the interview. Demographic controls include age, age squared and dummies for gender, race, education, and changes in household status (living alone, moving into a nursing home), employment status (retirement) and health (being diagnosed with dementia or a stroke) since the previous HRS wave in 2012. Baseline cognition controls are scores from tests of quantitative reasoning, retrieval fluency, word recall and the serial sevens test in 2012 and changes in these scores between 2010 and 2012. Interview controls are an indicator for a telephone interview, the number of call attempts until an interview was conducted, and interview week dummies. Location controls include Ebola concern before the first US Ebola case and media market–level relative search interest for the topics "anxiety" and "virus" in 2013. The effective F-statistic reported in the bottom of the table accounts for the use of cluster-robust standard errors (Montiel Olea and Pflueger, 2013). * denotes $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$, all for two-sided hypothesis tests.

In the bottom row of the table, I report the effective F-statistic, which is a diagnostic tool to detect potential problems related to weak instruments in settings with cluster-robust standard errors (Montiel Olea and Pflueger, 2013). Weak instruments denote an insufficiently strong relationship between the instruments and the endogenous variable. This is problematic because (i) it can induce an asymptotic bias that distorts the IV estimate towards the OLS estimate, and (ii) it can lead to size distortion in hypothesis tests of the IV coefficients, implying that the probability of falsely rejecting the null hypothesis is larger than the nominal size of the test. The effective F-statistic speaks to the likelihood of both of these problems.

For the preferred first-stage specification in column (4), the effective F-statistic is 12.474, which exceeds the standard Staiger and Stock (1997) rule of thumb threshold of 10. In simulations based on a sample of 230 recent IV specifications from the *American Economic Review*, Andrews, Stock, and Sun (2019) do not observe any bias or size distortions for specifications with effective F-statistics above this threshold, suggesting that my first stage is strong enough to rely on conventional hypothesis tests for inference. However, it is below the critical value suggested by Montiel Olea and Pflueger (2013) to formally reject an approximate asymptotic bias of 10 percent relative to a "worst case benchmark" at the 5 percent level, which is at 23.109.[20] For my instrument, I can only reject a worst case bias of 30 percent, for which the critical value is 12.039.

With respect to test size distortions due to weak instruments, the effective F-statistic can be used along with the critical values of Stock and Yogo (2005) in the single instrument case (Andrews, Stock, and Sun, 2019). Here, I can reject at the 5 percent level that the maximum size of a hypothesis test on the coefficient is more than 15 percent based on a Stock-Yogo critical value of 8.96, implying that the worst case size distortion is no more than 10 percentage points. At the same time, I cannot reject that the maximum size of a hypothesis test on the coefficient is more than 10 percent, for which the respective critical value is 16.38.

To sum up, while the strength of my instrument seems sufficient based on rule of thumbs typically used in applied economics, the inability to reject intermediate levels of bias from a worst-case perspective leaves some room for concern. Therefore, I will additionally report the results of IV inference procedures that are robust to weak instruments.

5.2 Falsification Tests for the Exclusion Restriction

The validity of distance to the closest US Ebola case as an instrument for Ebola concern in Equation 1 relies on the exclusion restriction. It requires that the instrument is uncorrelated with the second-stage error term, conditional on the control variables. Intuitively, distance to the closest US Ebola location should only affect cognitive performance in the interview through its effect on Ebola concern. While the exclusion restriction is not formally testable in the just-identified case, I report the results of two types of falsification tests in Table 3.

First, I check whether distance to the closest US Ebola case is associated with systematic regional differences in attitudes towards diseases or the tendency towards internet searches during epidemics. Such a correlation would cast doubt on the exclusion restriction because it seems plausible that attitudes towards diseases are related to cognitive function, as illustrated by the case of misperceptions. To test this, I check whether the instrument predicts Ebola search interest before the first US case and the level of concern about

[20] Intuitively, the worst case benchmark used in Montiel Olea and Pflueger (2013) corresponds to a situation when instruments are completely uninformative and first-stage and second-stage errors are perfectly correlated. The resulting critical values concern the the null hypothesis that the approximate asymptotic bias of the IV estimator exceeds a fraction of the bias of this benchmark for at least one value of the parameter space.

Table 3. Distance to US Ebola Locations and Selected Outcomes: Falsification Tests

	Main sample		Placebo sample	
Dependent variable:	Ebola search interest before first US case	Swine flu concern	Verbal reasoning score	
	(1)	(2)	(3)	(4)
Distance to closest Ebola location	0.029 (0.404)	0.273 (0.337)		
Distance to closest Ebola location (placebo)			−0.150 (0.766)	
Distance to closest Ebola location (placebo 2)				0.161 (0.664)
Demographic controls	Yes	Yes	Yes	Yes
Baseline cognition controls	Yes	Yes	Yes	Yes
Interview controls	Yes	Yes	Yes	Yes
Location controls	Yes	Yes	Yes	Yes
Census region dummies	Yes	Yes	Yes	Yes
Observations	492	492	563	563
R^2 (adjusted)	0.273	0.379	0.417	0.417
Mean (dependent variable)	27.326	40.767	504.963	504.963

Notes: OLS estimates for the main sample and a placebo sample of HRS 2014 respondents interviewed in September (excluding September 30), with standard errors in parentheses. Standard errors are robust to heteroscedasticity and arbitrary intra-cluster correlation within media markets. *Ebola search interest before first US case* is the relative search interest for the term “Ebola” in the media market of the interview location in first week of August. *Swine flu concern* is the relative search interest for the topic “swine flu (swine influenza)” in the media market of the interview location between April 25, 2009, and August 10, 2010. *Distance to closest Ebola location* is the logarithm of the kilometer distance to the closest location with a relationship to a US Ebola case that was publicly known on the day of the interview. *Distance to closest Ebola location (placebo)* is the logarithm of the kilometer distance to the closest of the three cities that have a connection to one of the four Ebola case diagnosed in the US between September 30 and October 23, even though the interview date precedes these cases. *Distance to closest Ebola location (placebo 2)* is the logarithm of the kilometer distance to the closest location that would have a relationship to a publicly known US Ebola case on the day of the interview if all US Ebola cases happened exactly 30 days earlier. Demographic controls include age, age squared and dummies for gender, race, education, and changes in household status (living alone, moving into a nursing home), employment status (retirement) and health (being diagnosed with dementia or a stroke) since the previous HRS wave in 2012. Baseline cognition controls are scores from tests of quantitative reasoning, retrieval fluency, word recall and the serial sevens test in 2012 and changes in these scores between 2010 and 2012. Interview controls are an indicator for a telephone interview, the number of call attempts until an interview was conducted, and interview week dummies. Location controls include the logarithm of the kilometer distance to the closest large city, Ebola search interest before the first US Ebola case (except in column (1)), and media-market level relative search interest for the topics “anxiety” and “virus” in 2013. * denotes $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$, all for two-sided hypothesis tests.

another recent epidemic, the swine flu pandemic of 2009/2010, in my main sample of October interviews. As demonstrated by the small and insignificant coefficient estimates in columns (1) and (2) of the table, there is no relationship between distance to US Ebola locations and internet searces for diseases before the emergency of Ebola cases in the US.

In a second, more direct falsification exercise, I test whether the instrument is related to cognitive function in a placebo sample of HRS interviews that should be unaffected by Ebola concern in October. In particular, I check whether there is a relationship between placebo variants of the instrument and the cognitive test scores of HRS respondents interviewed in September. Based on their interview dates, these respondents should be similar to October respondents, but were interviewed before the emergence of the first US Ebola case. Therefore, I expect that their cognitive function test scores will be unrelated to their distance to the eventual US Ebola locations if the exclusion restriction holds. I compute two different placebo versions of the proposed instrument that keep the locations of US cases unchanged but backdate their occurrence by different amounts of time. For the placebo instrument in column (3), I assume that all US Ebola cases happened before September 2014. In contrast, the placebo instrument used in column (4) assumes that all Ebola cases happened exactly 30 days earlier than their actual dates. In both cases, the estimated coefficient is small and insignificant, suggesting that my proposed instrument does not simply pick up pre-existing differences in cognitive function that are unrelated to US Ebola cases.[21]

Overall, my falsification tests provide no indication for potential violations of the exclusion restriction.

5.3 IV Estimates

The main results of my IV analysis are shown in Table 4. Column (1) estimates the reduced form. Under the maintained assumption that the exclusion restriction holds, the reduced form provides a test of the effect of worry on cognitive function that is valid irrespective of any weak instrument concerns because it does not rely on a strong first-stage relationship. In line with the hypothesis that worrying about an Ebola epidemic impairs cognitive performance, cognitive function test performance increases significantly with distance to the closest US Ebola location, conditional on the full set of controls. Specifically, a one unit increase in logarithmized distance implies an increase in verbal reasoning score of about 3.03 points on the W scale.

The corresponding IV estimate is presented in column (2). The estimated coefficient implies that a one SD increase in Ebola concern causes a reduction in verbal reasoning score of 4.74 points on the W scale. This is equivalent to a reduction in the probability of answering a test item of equivalent difficulty correctly from 50 to 36.6 or 90 to 84 percent, respectively (Jaffe, 2009). Comparing the level of Ebola concern in October 2014 with the counterfactual situation in which public concern is equal to the pre-October search interest peak, the extrapolated effect of worrying about a US Ebola epidemic would even amount to a 12 point drop in cognitive test performance.

In the bottom of the table, I also report a weak-instrument-robust confidence set based on test inversion of the cluster-robust version of the Anderson-Rubin test, using a significance level of 5 percent (Anderson and Rubin, 1949; Chernozhukov and Hansen, 2008; Andrews, Stock, and Sun, 2019). Like the reduced form estimates, this procedure does not rely on a strong first stage, so the resulting confidence interval survives any

[21] In unreported regressions, I verify that this conclusion carries over to regressions on the sample of all HRS 2014 interviews conducted before the first US case.

Table 4. Ebola Concern and Cognitive Function: IV Estimates

	Dependent variable: Verbal reasoning score		
	Reduced form	IV	OLS
	(1)	(2)	(3)
Distance to closest Ebola location	3.026***		
	(1.136)		
Ebola concern		−0.329**	−0.285***
		(0.132)	(0.095)
Demographic controls	Yes	Yes	Yes
Baseline cognition controls	Yes	Yes	Yes
Interview controls	Yes	Yes	Yes
Location controls	Yes	Yes	Yes
Census region dummies	Yes	Yes	Yes
Observations	492	492	492
R^2 (adjusted)	0.432	0.437	0.437
Mean (dependent variable)	505.089	505.089	505.089
Effective F-statistic		12.474	
Weak-instrument-robust confidence set		[-0.744, -0.104]	

Notes: Reduced-form and IV estimates for the main sample, with standard errors in parentheses. Standard errors are robust to heteroscedasticity and arbitrary intra-cluster correlation within media markets. *Distance to closest Ebola location* is the logarithm of the kilometer distance to the closest location with a relationship to a US Ebola case that was publicly known on the day of the interview. *Ebola concern* is the average weekly relative search interest for the term "Ebola" in the media market of the interview location over all weeks between September 28 and the week of the interview. Demographic controls include age, age squared and dummies for gender, race, education, and changes in household status (living alone, moving into a nursing home), employment status (retirement) and health (being diagnosed with dementia or a stroke) since the previous HRS wave in 2012. Baseline cognition controls are scores from tests of quantitative reasoning, retrieval fluency, word recall and the serial sevens test in 2012 and changes in these scores between 2010 and 2012. Interview controls are an indicator for a telephone interview, the number of call attempts until an interview was conducted, and interview week dummies. Location controls include the logarithm of the kilometer distance to the closest large city, Ebola search interest before the first US Ebola case, and media-market level relative search interest for the topics "anxiety" and "virus" in 2013. The effective F-statistic for the first stage reported in the bottom of the table accounts for the use of cluster-robust standard errors. The weak-instrument-robust confidence set reported in the bottom of the table is constructed by inverting the cluster-robust version of an Anderson-Rubin test at the 5 percent level for the coefficient on *Ebola concern*. Column (3) reproduces the OLS estimates from Table 1. * denotes $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$, all for two-sided hypothesis tests.

weak instrument concerns. Therefore, it is reassuring that the null hypothesis of no effect is clearly rejected also using this alternative inference approach.

Column (3) reproduces the OLS estimates from Table 1. The IV estimate is approximately 15 percent larger, suggesting that the OLS results cannot be explained by remaining endogeneity problems. On the contrary, any potential downward endogeneity bias in the OLS estimates seems to be outweighed by the effect of measurement error in Ebola concern. The findings of the IV analysis therefore corroborate the conclusion that worrying about an epidemic induces a substantial cognitive cost.

5.4 Robustness

In Online Appendix C, I report estimates from various additional analyses designed to test the robustness of the OLS and IV estimation results. I briefly summarize these analyses here.

First, I verify that my findings are not an artefact of the way my measure of local Ebola concern on interview day is constructed from the Google Trends search interest data. The Ebola concern measure employed in the main text, which is based on the average weekly search interest across all weeks between the first US Ebola case and the interview week, assumes that worries outlast the period in which individuals actively search for information about Ebola on the internet. In Table C.1, I re-run the analyses under the assumption that Ebola concern equals search interest in the week of the interview. A downside of this measure is that measurement error due to US Ebola cases after the interview day in the same week plays a larger role here. Consequently, the resulting OLS estimate is close to zero, whereas the first stage and IV estimates are very similar to the respective main text coefficients. I steer a middle course between both approaches in table Table C.2, which contains estimates based on measuring Ebola concern as the average search interest in the interview week and the preceding week.[22] For this third measure of Ebola concern, all estimates closely resemble those reported in the main text. The insignificance of the specific way Ebola concern is measured is also reflected in the empirical distributions of the three measures within my sample (see Table B.1). While the dispersion naturally decreases for measures that average search interest over a larger number of weeks, the sample means all approximate a value of 60.

Second, I assess the implication of a different way of aggregating the data from individual Google Trends queries into weekly search interest in the first place. Instead of the median, I now use the average across all 100 iterations.[23] The coefficient estimates of this robustness check, displayed in Table C.3, are almost identical to those reported in the main text.

Third, I re-run the estimations for the subsample of HRS respondents that are interviewed in metro areas. This should further alleviate concerns that the probability of US Ebola cases is lower in rural locations, but it comes at the cost of losing 20 percent of the sample. The estimated IV coefficient, presented in Table C.4, shrinks by one third in size and is only marginally statistically significant, but still qualitatively in line with the main results.

[22] For one respondent with an early October interview, Ebola search interest data for the preceding September week in the respective media market is missing in more than 49 iterations of data collection. As a result, this measure of Ebola concern suffers from the selection bias described in Section 3.2 for the affected respondent. I exclude this respondent from the analysis.

[23] For media market-weeks where data is missing for some queries because of the Google Trends privacy threshold, I use the midpoint between a lower and an upper bound on the average, where the lower bound results from setting missing values to zero and the upper bound from setting missing values to the lowest observed value. This only concerns search interest before the first US case for a few main sample respondents, not the main regressor of interest.

Finally, I check whether my results are robust to the use of alternative instruments. In their analyses of daily Twitter activity about Ebola in October 2014, Campante, Depetris-Chauvín, and Durante (2020) show that Ebola concern reacts most strongly to the first three US Ebola cases in Dallas, Texas, and Cleveland, Ohio. In contrast, the last US Ebola case in the City of New York, New York, on October 23 does not provoke a strong reaction any more. This is intuitively plausible because every Ebola case that does not cause a US epidemic is good news about the contagiosity of the virus and the ability of the authorities to contain it. It also implies that proximity to early US Ebola cases only could also be a suitable instrument for Ebola concern. Based on this observation, I report results from IV analyses that use distance to the first or the closest of the first two US Ebola locations as alternative instruments in Table C.5 and Table C.6. As expected, the alternative instruments have a stronger first stage. In particular, the effective F-statistic is well beyond all relevant critical values for distance to the first US Ebola case. The corresponding reduced form and IV estimates are qualitatively in line with those reported in the main text, with estimated IV coefficients of −0.253 and −0.316. All estimates are significantly different from zero at least at the 5 percent level. This indicates that the results of the IV analysis do not depend on a specific way of formalizing the idea that proximity to recent US Ebola cases increases worry. Moreover, it dispels any remaining concerns about weak instruments.

In sum, the results of these robustness checks show that the main findings are not sensitive to specific data management or analysis choices.

6 Do Interview Characteristics Respond to US Ebola Cases?

One unaddressed threat to the internal validity of my findings arises because interview dates are party dictated by the availability of HRS participants rather than predetermined. This opens up the possibility that interview characteristics are endogenous to local Ebola concern. In particular, respondents from locations close to US Ebola cases might postpone or cancel their interview for fear of infection. If individuals with positive unobservable shocks to cognitive ability in 2014 are more likely to respond to nearby Ebola cases by delaying their interview, the documented negative effect of Ebola concern on cognitive test performance could also be explained by a selection effect. However, note that such a relationship seems implausible in light of the role of misperceptions about Ebola for the spread of fear, described in Section 2. Assuming that the likelihood of holding wrong beliefs about Ebola decreases in cognitive ability, respondents with positive shocks to cognitive functioning should be less likely to avoid an interview for fear of infection if they live close to Ebola cases.

Nontheless, I also assess the plausibility of selection due to differential avoidance behavior by cognitive ability empirically. As a first step, I look for indications that the emergence of nearby US Ebola cases affects interview characteristics. Table 5 presents estimates from regressions of various interview characteristics on distance to the closest US Ebola location, using the same set of covariates as the previous analyses. First, I check whether the likelihood of telephone interviews decreases with distance to US Ebola cases, as would be expected if respondents avoid face-to-face interviews for fear of getting infected. The respective coefficient estimate is reported in column (1). In contrast to the prediction, a one unit increase in logarithmized distance is associated with an 8 percentage point higher likelihood of a telephone interview. This estimate is significantly different from zero at the 10 percent level.

Hesitant respondents should also require more call attempts by the interviewer until they agree to be interviewed. This prediction is tested in column (2) and finds support in the data. In particular, the number of

Table 5. Distance to US Ebola Locations and Interview Characteristics

	Main sample		All interviews after first US case		
Dependent variable:	Telephone interview	Number of call attempts	Interview date (days since wave start)		
	(1)	(2)	(3)	(4)	(5)
Distance to closest Ebola location	0.079*	−1.316***	−7.854***	−7.590**	−8.574***
	(0.043)	(0.479)	(1.955)	(3.181)	(3.314)
Distance to closest Ebola location × High predicted score				−0.648	
				(2.162)	
Distance to closest Ebola location × High predicted score (LASSO)					1.422
					(2.096)
Demographic controls	Yes	Yes	Yes	Yes	Yes
Baseline cognition controls	Yes	Yes	Yes	Yes	Yes
Interview controls	Yes	Yes	Yes	Yes	Yes
Location controls	Yes	Yes	Yes	Yes	Yes
Census region dummies	Yes	Yes	Yes	Yes	Yes
Observations	492	492	1341	1341	1341
R^2 (adjusted)	0.047	0.104	0.358	0.360	0.358
Mean (dependent variable)	0.431	15.927	274.518	274.518	274.518

Notes: OLS estimates for two different samples of HRS 2014 participants who were interviewed after the first US Ebola case, with standard errors in parentheses. Standard errors are robust to heteroscedasticity and arbitrary intra-cluster correlation within media markets. In columns (4) and (5), I report block bootstrap standard errors that additionally account for the presence of generated regressors. *Distance to closest Ebola location* is the logarithm of the kilometer distance to the closest location with a relationship to a US Ebola case that was publicly known on the day of the interview. *High predicted score* and *High predicted score (LASSO)* are indicators for respondents with an above-median predicted verbal reasoning score, based on their covariates and coefficient estimates of an OLS or LASSO regression of verbal reasoning score on these covariates in the sample of all HRS 2014 respondents interviewed before the first US Ebola case. Demographic controls include age, age squared and dummies for gender, race, education, and changes in household status (living alone, moving into a nursing home), employment status (retirement) and health (being diagnosed with dementia or a stroke) since the previous HRS wave in 2012. Baseline cognition controls are scores from tests of quantitative reasoning, retrieval fluency, word recall and the serial sevens test in 2012 and changes in these scores between 2010 and 2012. Interview controls are an indicator for a telephone interview (except in column (1)), the number of call attempts until an interview was conducted (except in column (2)), and interview week dummies (except in column (3) to (5)). Location controls include the logarithm of the kilometer distance to the closest large city, Ebola search interest before the first US Ebola case, and media-market level relative search interest for the topics “anxiety” and “virus” in 2013. Columns (4) and (5) additionally include the main effect of *High predicted score* and *High predicted score (LASSO)*, respectively. * denotes $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$, all for two-sided hypothesis tests.

interactions between interviewer and respondent until the interview is conducted decrease by about 1.3 calls for each unit increase in logarithmized distance, which is highly significant.

To check for delayed interviews, I extend the sample to all HRS 2014 respondents with interviews dates after the first US Ebola case on September 30. Column (3) contains the coefficient estimate from a regression of interview date, measured in days, on distance to the closest Ebola location in this extended sample. On average, interviews are conducted about 8 days earlier for a one unit increase in logarithmized distance to US Ebola cases.

In Table B.2, I show that there are no comparable differences in interview characteristics by distance to the three subsequent Ebola locations in the sample of all HRS interviews conducted before the first US case. Thus, the data does not provide an indication that the significant effects detected in Table 5 could be an artefact of location-specific data collection procedures like differences in the start dates of the interview period. Yet, the directions of the observed differences also do not unequivocally support the idea that they are driven by respondent reactions to Ebola cases, as this is difficult to reconcile with a decrease in telephone interviews in proximity to Ebola locations. Arguably, switching to a telephone interview would be the easiest way to eliminate the risk of an Ebola infection even for individuals who hold wrong beliefs about the transmission dynamics of the disease. In line with this reasoning, HRS employees expressed in private communication to me that they are not aware of protocol changes or respondent concerns about contracting Ebola during interviews conducted in 2014.

For my purposes, the relevant question is whether changes in interview characteristics by distance to Ebola locations can explain my results. In this respect, it is reassuring that I control for interview mode and the number of contact attempts in the main specifications. Delayed interviews in proximity to Ebola cases, on the other hand, would only change the interpretation of my results if they were positively correlated with respondents' cognitive capacity in 2014. Therefore, I next investigate whether the strength of the documented relationship between nearby Ebola cases and interview dates is related to observable determinants of cognitive function in the comprehensive HRS data.[24] To this end, I implement the following two-stage estimation procedure. First, I model verbal reasoning score as a function of observable respondent characteristics in the sample of all HRS 2014 interviews before the first US Ebola case. The estimation equation is

$$VerbalReasoningScore_{jc} = \alpha + \boldsymbol{\eta}_1'\boldsymbol{X}_j + \boldsymbol{\eta}_2'\boldsymbol{C}_c + \boldsymbol{\eta}_3'\boldsymbol{M}_m + \rho_r + \epsilon_{jc}, \tag{2}$$

where respondents are denoted by the subscript j rather than i to indicate the use of a different sample. As predictors, I include the full set of demographic, baseline cognition and location controls as well as census region dummies, but not interview characteristics, which might themselves depend on Ebola distance in the second stage. The resulting model can explain 41.4 percent of the variation in actual cognitive test scores. Then, I use the estimated coefficients from this model to predict test scores in the sample of HRS interviews conducted after the first US Ebola case, and generate an indicator variable $High\widehat{PredictedScore}_{ic}$ that is equal to one for respondents with an above-median predicted test score in this sample. The result is an easily interpretable summary measure of counterfactual cognitive ability in the absence of US Ebola cases.

The second stage builds on the regression model underlying column (3) of Table 5, which I augment with the constructed indicator for high predicted test scores as a main and interaction effect with Ebola distance.

[24] Note that I cannot simply use the realized verbal reasoning test score of October respondents because I expect this to itself be affected by distance to Ebola locations via the hypothesized effect of Ebola concern.

The respective estimation equation is thus given by

$$
\begin{aligned}
InterviewDate_{ict} &= \alpha + \beta_1 EbolaDistance_{ct} + \beta_2 EbolaDistance_{ct} \times \widehat{HighPredictedScore}_{ic} \\
&\quad + \kappa_0 \widehat{HighPredictedScore}_{ic} + \boldsymbol{\kappa}_1' \boldsymbol{X}_i + \boldsymbol{\kappa}_2' \boldsymbol{C}_c + \boldsymbol{\kappa}_3' \boldsymbol{M}_m + \rho_r + \epsilon_{ict},
\end{aligned}
\tag{3}
$$

where the interaction effect coefficient β_2 reveals whether selection into later interview dates differs by cognitive ability.[25] The resulting coefficient estimates of the second stage are reported in column (4) of the table. The main effect of Ebola distance, which corresponds to the average delay of the interview per unit of logarithmized distance for respondents with below-median predicted cognitive performance, is similar to the equivalent estimate for the whole sample in column (3). The interaction effect, though noisily estimated, is close to zero. This suggests that potential selection effects are not related to cognitive ability.

Column (5) displays second-stage estimates from a variant of the estimation procedure that replaces OLS by an alternative regression method in the prediction stage. OLS minimizes the in-sample sum of squared errors, so using it for out-of-sample predictions poses the risk of overfitting: with a large number of included predictors, coefficients may pick up specific random components of the sample at hand that are not present in other samples. Therefore, I repeat the prediction stage using LASSO regression, which is a machine learning algorithm that selects a linear model from a pool of potential covariates to minimize an estimate of out-of-sample prediction error.[26] The pool of available covariates for the algorithm to choose from is equal to the set of regressors in the OLS prediction regression, and the tuning parameter is determined by means of cross-validation with 10 folds using the "one standard-error rule" discussed in Hastie, Tibshirani, and Friedman (2009). In this case, the LASSO regression only selects 13 out of 39 available variables, but the interpretation of the resulting second-stage point estimates does not change. If anything, they even suggests a weakly positive interaction effect, implying a weaker association between Ebola distance and interview dates for respondents with high predicted cognitive test scores.

All in all, the empirical investigation provides no indication that changes in interview characteristics are related to cognitive function, suggesting that selection into interview dates is unlikely to explain my main results.

7 Discussion

I have established a robust negative association between the local level of Ebola concern and performance in a test of fluid intelligence in a sample of HRS participants. The results of an IV strategy confirm the findings of

[25]The variable $\widehat{HighPredictedScore}_{ic}$ is itself estimated and therefore subject to sampling uncertainty which is not accounted for in standard covariance estimates (Pagan, 1984). To correct the standard errors for the presence of a generated regressor and allow for arbitrary intra-cluster correlation within media markets, I implement a two-step block bootstrap approach that follows Ashraf and Galor (2013). Each bootstrap replicate is generated in the following way, resembling the two-stage estimation procedure: First, a random sample of HRS respondents is drawn with replacement from the sample of all HRS respondents with interview dates before the first US Ebola case. I run the prediction regression (Equation 2) on this random sample and save the resulting OLS coefficients. Second, I draw a random sample of media markets with replacement from the sample of the all HRS respondents interviewed after the first US Ebola case, thereby accounting for clustering within media markets. I use the saved OLS coefficients from the prediction regression to predict the counterfactual verbal reasoning scores in this random sample of post-September HRS interviews and construct the indicator variable for above-median predicted test scores. I then estimate the second-stage OLS regression (Equation 3), which yields the bootstrap coefficient estimates of this replicate. This process of two-step block bootstrap sampling and OLS estimation is repeated 10 000 times. The standard deviations of the resulting sample of 10 000 bootstrap coefficient estimates from the second-stage regression are the bootstrap standard errors of the point estimates of these coefficients.

[26]See Mullainathan and Spiess (2017) for a discussion on how machine learning algorithms can be used for prediction tasks in economics and Athey and Imbens (2019) for a general overview of machine learning methods for economists.

the OLS analysis and lead to the conclusion that the relationship is causal. I have also shown that this cannot be explained by selection effects due to differential avoidance behavior by cognitive ability, but rather implies a direct effect of Ebola concern on cognitive test performance. Regarding the mechanisms behind these results, two points for discussion emerge from my analysis.

The first point pertains to the interpretation of my Ebola concern measures, which are constructed from data on internet search volume. While especially the reduced-form and IV estimates provide strong evidence that cognitive costs are ultimately caused by the emergence of nearby Ebola cases, it is less clear whether the effect is driven by emotional responses to these cases in the form of worry or fear or an increase in factual public interest.

There is reason to believe that Ebola concern captures emotional expressions. This fits well with evidence from nationally representative surveys that many people were in fact worried about Ebola and with anecdotal evidence of fear-based behavioral overreactions, summarized in Section 2. Moreover, the strong dependence of the level of Ebola concern on geographical distance to the closest case location—both within the US and with respect to Ebola cases in other countries—suggests that the perceived threat of infection plays an important role. In particular, it seems difficult to explain why the WHO's declaration of an international public health emergency after almost 2000 Ebola cases and 1000 deaths in Africa would be objectively less interesting than the occurrence of a single Ebola case in the US several weeks later, if not because of a lower perceived personal threat.[27] Yet, the nationwide level of Ebola concern is more than twice as high after the first US case than after the WHO announcement. The reading that internet activity reveals emotional reactions is also supported by van Lent, Sungur, Kunneman, van de Velde, and Das (2017), who carry out a content analysis of a random sample of tweets about Ebola that were posted in the Netherlands between March and November 2014. They find explicit expressions of fear in one fifth of all analysed tweets, and the fraction and number of fearful tweets increases substantially in October, when initial Ebola cases in nearby European countries become public. Therefore, this is my preferred interpretation.

To the extent that my measures of Ebola concern merely pick up variation in factual public interest, my results suggest that the resulting attentional capture nontheless leads to similar levels of mental preoccupation. This would be more in line with the scarcity hypothesis advanced by Mullainathan and Shafir (2013), which posits that the perception of pressing needs like a scarcity of income or time taxes cognitive capacities by inducing a narrow attentional focus on the perceived problem.

Alternatively, respondents might consciously choose to allocate less cognitive effort to the interview because nearby Ebola cases change the marginal benefit of other cognitively demanding activities. For instance, local levels of concern about Ebola might affect the utility of consuming and reflecting about the latest news. This would be in the spirit of Altmann, Grunewald, and Radbruch (2021), who show that relative economic incentives affect the allocation of attention among different choice domains to the detriment of neglected domains. However, this seems unlikely in this case because (i) cognitive effort contributes little to solving verbal analogies, and (ii) it is hard to imagine how a reduction in mental focus on the interview would benefit competing activities. In particular, the interview will not be noticably shorter if respondents perform worse on the cognitive tests and fruitfully engaging in any other activity simultaneously with the cognitive tests seems close to impossible.

[27] A similar argument could be made with respect to the difference in information value between an Ebola case in a neighboring and a more distant US state.

A second discussion point concerns the importance of contextual factors of the 2014 US Ebola episode for the size of the documented effect. As described in Section 2, the occurrence of Ebola cases in the US led to a surge in sensationalist media reporting about Ebola. At the same time, politicians strategically exploited the situation to appeal to voters in light of the upcoming 2014 midterm elections, likely contributing to the spread of fear (Campante, Depetris-Chauvín, and Durante, 2020). I cannot disentangle whether the cognitive costs of US Ebola cases are primarily attributable to concern about the perceived threat of the cases themselves or to its amplification by these other factors. This suggests caution with respect to the generalizability of my findings to other settings, as similar levels of epidemic activity will not necessarily induce similar levels of public concern.

8 Conclusion

My findings suggest the possibility of an unexplored economic cost of epidemics: negative emotional responses to the threat of the disease could cause suboptimal choices and reduced cognitive productivity by taking a toll on individuals' cognitive functioning. The cognitive cost of worrying about an epidemic could in principle affect everyone, even those who are unaffected by the disease itself or resulting government interventions, and in all domains of decision-making. However, those who have more to fear, e.g., because they work in occupations that require interpersonal contact, might be hit hardest.

For policy makers, this implies that it is important to not only contain the epidemic itself, but also to curb excessive fears that may arise from it. In particular, information campaigns can prevent misperceptions about the danger of the disease. On the personal level, it seems wise to foresee the cognitive consequences of epidemic-induced worry and restrain one's ambitions and plans accordingly. For instance, one could defer important life decisions until fear has subsided rather than taking the risk of a suboptimal choice during the onset of a frightening epidemic.

Future research should investigate the specific economic consequences of epidemic-induced cognitive costs, especially with respect to cognitive errors in individual decision-making and reductions in labor productivity. Finding out which domains and individuals are most affected would pave the way for the development of strategies and targeted interventions to mitigate any negative impact. Moreover, it would be interesting to learn more about the underlying mechanisms of the documented reductions in cognitive functioning. The hypothesized underlying cognitive processes imply a reallocation of scarce attentional resources towards the perceived threat. It remains an open question whether this also leads to measurably better health outcomes such as a lower risk of infection.

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Online Appendices

Online Appendix A HRS Data Appendix

This appendix describes the source and construction of HRS variables that are not explicated in detail in the main text. The main regression specifications include a wide range of control variables that are constructed from survey items in the RAND HRS Longitudinal File and the RAND HRS Fat Files for the 2010, 2012 and 2014 wave. HRS control variables can be categorized into three groups. The respective source items of each described variable are given in parentheses and refer to the RAND HRS Longitudinal File unless noted otherwise.

Demographic controls include age, age squared (both based on item R12AGEY_E) and dummies for gender, race, education, and changes in household status, employment status and health since the previous HRS wave in 2012. I control for gender by including a dummy variable equal to one for female respondents (based on item RAGENDER). Race and ethnicity are captured by seperate dummies for identifying as White Hispanic, Black, other race Hispanic or other race non-Hispanic, such that White non-Hispanic is the omitted category (based on items RARACEM and RAHISPAN). To account for education, I include four dummies that mark respondents' highest attained education category (based on item RAEDUC): one for passing the General Educational Development test, one for completing at most high school, one for having done some college, and one for holding at least a college degree, with less than high school forming the base category. The remaining dummy variables indicate changes in personal circumstances since the last HRS wave in 2012 that could be associated with cognitive decline. In particular, I construct a dummies for having started to live alone (based on item H11HHRES and H12HHRES), having moved into a nursing home (based on item R11NHMLIV and R12NHMLIV), reporting to have retired from work (based on item R11RETEMP and R12RETEMP) and having been diagnosed with dementia (item R12DEMENS) or a stroke (item R12STROKS).[28] For each categorical variable, I also include a dummy equal to one if one of the underlying survey items is missing for the respondent, to avoid losing observations due to missing control variables.

Baseline cognition controls are the scores of four different cognitive function tests conducted in the 2012 HRS wave and the changes in these scores between the 2010 and 2012 wave. In particular, I include the W scores of a quantitative reasoning test (based on item R10NSSCRE and R11NSSCRE) and the number of correct answers in a test of retrieval fluency (where the 2012 score is constructed as the difference between item ND196 and ND198 in the RAND HRS 2012 Fat File, and the 2010 score as the difference between item MD196 and MD198 in the RAND HRS 2010 Fat File) to control for baseline differences in fluid intelligence across respondents.[29] The remaining test scores are the number of correct answers in a word recall test (based on item R10TR20 and R11TR20) and the number of correct subtractions in the serial sevens test (based on item R10SER7 and R11SER7), both of which are tests aimed at detecting early warning signs of cognitive decline. For the latter two tests, the HRS imputes missing values based on non-changing baseline demographics, wave-specific demographic, economic and health status variables, and previous and current wave cognitive function measures. I also use imputed values as control variables in my analysis because the imputation procedure relies on pre-Ebola characteristics only.

[28] An equivalent dummy for having been diagnosed with Alzheimer's disease since the last wave (item R12ALZHES) is collinear with the other control variables and therefore omitted.

[29] For a small number of respondents, retrieval fluency scores are negative because the count of correct answers is exceeded by the number of incorrect answers. I allow for negative scores in my analyses. However, the results are very similar if negative scores are truncated at zero instead.

The last group of HRS control variables are interview controls. These consist of an indicator for telephone interviews, for which in-person interviews are the omitted category (item OB084 in the RAND HRS 2014 Fat File), and the number of call or contact attempts by the interviewer until an interview is conducted (item O085 in the RAND HRS 2014 Fat File). In addition, this group also includes interview week dummies constructed from restricted data on the beginning date of the interview. Interview weeks are defined to start on Sundays for consistency with the weekly search interest data provided by Google Trends, which follows the same convention.

Online Appendix B Additional Tables

Table B.1. Descriptive Statistics for the Main Sample

Variable	Mean	SD	Min.	Median	Max.	Obs.
Verbal reasoning score	505.089	29.831	435	498	560	492
Ebola concern	63.698	14.426	(s)	(s)	(s)	492
Distance to closest Ebola location	6.819	0.901	(s)	(s)	(s)	492
Female	0.622	0.485	0	1	1	492
Age	66.057	10.185	42	64	95	492
White Hispanic	0.077	0.267	0	0	1	492
Black	0.207	0.406	0	0	1	492
Other race Hispanic	0.022	0.148	0	0	1	492
Other race non-Hispanic	0.053	0.224	0	0	1	492
General Education Development test	0.053	0.224	0	0	1	492
High school degree	0.252	0.435	0	0	1	492
Some college	0.264	0.441	0	0	1	492
College degree	0.274	0.447	0	0	1	492
Started living alone (since 2012)	0.075	0.264	0	0	1	492
Moved into nursing home (since 2012)	0.012	0.110	0	0	1	492
Retired (since 2012)	0.075	0.264	0	0	1	492
Diagnosed with dementia (since 2012)	0.014	0.119	0	0	1	492
Dementia status missing	<0.010	<0.100	0	0	1	492
Diagnosed with a stroke (since 2012)	0.018	0.134	0	0	1	492
Stroke status missing	<0.010	<0.100	0	0	1	492
Quantitative reasoning score (2012)	518.795	32.607	409	519	584	492
Change in quantitative reasoning score (2010 to 2012)	21.023	35.017	−74.2	17.8	146.8	492
Retrieval fluency score (2012)	17.518	7.210	−20	17	66	492
Change in retrieval fluency score (2010 to 2012)	0.571	7.006	−35	0	53	492
Word recall score (2012)	10.382	3.158	2	10	20	492
Change in word recall score (2010 to 2012)	−0.301	3.294	−13	0	11	492
Serial sevens score (2012)	3.636	1.561	0	4	5	492
Change in serial sevens score (2010 to 2012)	−0.024	1.470	−5	0	5	492
Telephone interview	0.431	0.496	0	0	1	492
Number of call attempts	15.927	10.304	3	14	83	492
Interview day (within October 2014)	14.559	9.188	1	13	31	492
Distance to closest large city	3.695	1.541	(s)	(s)	(s)	492
Ebola search interest before first US case	27.326	5.950	(s)	(s)	(s)	492
Search interest for “anxiety” (2013)	55.243	6.001	(s)	(s)	(s)	492
Search interest for “virus” (2013)	50.765	5.873	(s)	(s)	(s)	492
Swine flu concern (2009 and 2010)	40.767	5.056	(s)	(s)	(s)	492
Ebola concern (interview week only)	61.633	29.641	(s)	(s)	(s)	492
Ebola concern (last two weeks)	58.837	22.807	(s)	(s)	(s)	491
Ebola concern (average)	63.771	14.477	(s)	(s)	(s)	492
Distance to first Ebola location	7.124	0.666	(s)	(s)	(s)	492
Distance to closest Ebola location (excl. New York)	6.889	0.744	(s)	(s)	(s)	492

Notes: Mean, standard deviation, minimum, median, maximum, and number of observations of important variables for the main estimation sample. All distance variables are in log-transformed kilometers. (s) indicates statistics which are suppressed to preclude the possibility of inferential identification of small geographical areas, in line with the HRS disclosure rules for analyses based on restricted data. Some statistics for indicator variables are censored at the request of HRS to avoid disclosure of small cell sizes.

Table B.2. Distance to US Ebola Locations and Interview Characteristics: Placebo Regressions

	Placebo sample		All interviews before first US case
Dependent variable:	Telephone interview	Number of call attempts	Interview date (days since wave start)
	(1)	(2)	(3)
Distance to closest Ebola location (placebo)	0.010	0.001	0.366
	(0.025)	(0.482)	(1.045)
Demographic controls	Yes	Yes	Yes
Baseline cognition controls	Yes	Yes	Yes
Interview controls	Yes	Yes	Yes
Location controls	Yes	Yes	Yes
Census region dummies	Yes	Yes	Yes
Observations	563	563	11979
R^2 (adjusted)	0.165	0.276	0.331
Mean (dependent variable)	0.309	13.840	78.339

Notes: OLS estimates for two different samples of HRS 2014 participants who were interviewed before the first US Ebola case, with standard errors in parentheses. Standard errors are robust to heteroscedasticity and arbitrary intra-cluster correlation within media markets. The placebo sample used in columns (1) and (2) is the sample of all HRS 2014 respondents interviewed in September (excluding September 30th), while the sample used in column (3) also includes respondents from earlier months. *Distance to closest Ebola location (placebo)* is the logarithm of the kilometer distance to the closest of the three cities that have a connection to one of the four Ebola case diagnosed in the US between September 30 and October 23, even though the interview date precedes these cases. Demographic controls include age, age squared and dummies for gender, race, education, and changes in household status (living alone, moving into a nursing home), employment status (retirement) and health (being diagnosed with dementia or a stroke) since the previous HRS wave in 2012. Baseline cognition controls are scores from tests of quantitative reasoning, retrieval fluency, word recall and the serial sevens test in 2012 and changes in these scores between 2010 and 2012. Interview controls are an indicator for a telephone interview (except in column (1)), the number of call attempts until an interview was conducted (except in column (2)), and interview week dummies (except in column (3)). Location controls include the logarithm of the kilometer distance to the closest large city, Ebola search interest before the first US Ebola case, and media-market level relative search interest for the topics "anxiety" and "virus" in 2013. * denotes $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$, all for two-sided hypothesis tests.

Online Appendix C Robustness

Table C.1. Alternative Measures of Ebola Concern: Search Interest in the Interview Week

Dependent variable:	Ebola concern (interview week only)	Verbal reasoning score	
	First stage	IV	OLS
	(1)	(2)	(3)
Distance to closest Ebola location	−8.701***		
	(1.993)		
Ebola concern (interview week only)		−0.348***	−0.065
		(0.132)	(0.075)
Demographic controls	Yes	Yes	Yes
Baseline cognition controls	Yes	Yes	Yes
Interview controls	Yes	Yes	Yes
Location controls	Yes	Yes	Yes
Census region dummies	Yes	Yes	Yes
Observations	492	492	492
R^2 (adjusted)	0.861	0.413	0.427
Mean (dependent variable)	61.633	505.089	505.089
Effective F-statistic	19.056	19.056	
Weak-instrument-robust confidence set		[-0.688, -0.101]	

Notes: OLS, first-stage and IV estimates for the main sample, with standard errors in parentheses. Standard errors are robust to heteroscedasticity and arbitrary intra-cluster correlation within media markets. *Ebola concern (interview week only)* is the relative search interest for the term "Ebola" in the media market of the interview location in the week of the interview. *Distance to closest Ebola location* is the logarithm of the kilometer distance to the closest location with a relationship to a US Ebola case that was publicly known on the day of the interview. Demographic controls include age, age squared and dummies for gender, race, education, and changes in household status (living alone, moving into a nursing home), employment status (retirement) and health (being diagnosed with dementia or a stroke) since the previous HRS wave in 2012. Baseline cognition controls are scores from tests of quantitative reasoning, retrieval fluency, word recall and the serial sevens test in 2012 and changes in these scores between 2010 and 2012. Interview controls are an indicator for a telephone interview, the number of call attempts until an interview was conducted, and interview week dummies. Location controls include the logarithm of the kilometer distance to the closest large city, Ebola search interest before the first US Ebola case, and media-market level relative search interest for the topics "anxiety" and "virus" in 2013. The effective F-statistic for the first stage reported in the bottom of the table accounts for the use of cluster-robust standard errors. The weak-instrument-robust confidence set reported in the bottom of the table is constructed by inverting the cluster-robust version of an Anderson-Rubin test at the 5 percent level for the coefficient on *Ebola concern (interview week only)*. * denotes $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$, all for two-sided hypothesis tests.

Table C.2. Alternative Measures of Ebola Concern: Search Interest in the Last Two Weeks

Dependent variable:	Ebola concern (last two weeks)	Verbal reasoning score	
	First stage	IV	OLS
	(1)	(2)	(3)
Distance to closest Ebola location	−8.490***		
	(1.364)		
Ebola concern (last two weeks)		−0.359***	−0.247**
		(0.132)	(0.100)
Demographic controls	Yes	Yes	Yes
Baseline cognition controls	Yes	Yes	Yes
Interview controls	Yes	Yes	Yes
Location controls	Yes	Yes	Yes
Census region dummies	Yes	Yes	Yes
Observations	491	491	491
R^2 (adjusted)	0.873	0.433	0.434
Mean (dependent variable)	58.837	505.106	505.106
Effective F-statistic	38.745	38.745	
Weak-instrument-robust confidence set		[-0.646, -0.104]	

Notes: OLS, first-stage and IV estimates for the main sample, with standard errors in parentheses. Standard errors are robust to heteroscedasticity and arbitrary intra-cluster correlation within media markets. *Ebola concern (last two weeks)* is the average weekly relative search interest for the term "Ebola" in the media market of the interview location over the week of the interview and the preceding week. *Distance to closest Ebola location* is the logarithm of the kilometer distance to the closest location with a relationship to a US Ebola case that was publicly known on the day of the interview. Demographic controls include age, age squared and dummies for gender, race, education, and changes in household status (living alone, moving into a nursing home), employment status (retirement) and health (being diagnosed with dementia or a stroke) since the previous HRS wave in 2012. Baseline cognition controls are scores from tests of quantitative reasoning, retrieval fluency, word recall and the serial sevens test in 2012 and changes in these scores between 2010 and 2012. Interview controls are an indicator for a telephone interview, the number of call attempts until an interview was conducted, and interview week dummies. Location controls include the logarithm of the kilometer distance to the closest large city, Ebola search interest before the first US Ebola case, and media-market level relative search interest for the topics "anxiety" and "virus" in 2013. The effective F-statistic for the first stage reported in the bottom of the table accounts for the use of cluster-robust standard errors. The weak-instrument-robust confidence set reported in the bottom of the table is constructed by inverting the cluster-robust version of an Anderson-Rubin test at the 5 percent level for the coefficient on *Ebola concern (last two weeks).* * denotes $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$, all for two-sided hypothesis tests.

Table C.3. Alternative Aggregation of Google Trends Search Interest Data Queries

Dependent variable:	Ebola concern (average)	Verbal reasoning score		
	First stage	Reduced form	IV	OLS
	(1)	(2)	(3)	(4)
Distance to closest Ebola location	−9.299***	3.009***		
	(2.586)	(1.128)		
Ebola concern (average)			−0.324**	−0.278***
			(0.130)	(0.093)
Demographic controls	Yes	Yes	Yes	Yes
Baseline cognition controls	Yes	Yes	Yes	Yes
Interview controls	Yes	Yes	Yes	Yes
Location controls	Yes	Yes	Yes	Yes
Census region dummies	Yes	Yes	Yes	Yes
Observations	492	492	492	492
R^2 (adjusted)	0.659	0.432	0.437	0.437
Mean (dependent variable)	63.771	505.089	505.089	505.089
Effective F-statistic	12.929		12.929	
Weak-instrument-robust confidence set			[-0.719, -0.103]	

Notes: OLS, first-stage, reduced form and IV estimates for the main sample, with standard errors in parentheses. Standard errors are robust to heteroscedasticity and arbitrary intra-cluster correlation within media markets. *Ebola concern (average)* is the average weekly relative search interest for the term "Ebola" in the media market of the interview location over all weeks between September 28th and the week of the interview, using the average rather than the median of 100 queries on the Google Trends website. *Distance to closest Ebola location* is the logarithm of the kilometer distance to the closest location with a relationship to a US Ebola case that was publicly known on the day of the interview. Demographic controls include age, age squared and dummies for gender, race, education, and changes in household status (living alone, moving into a nursing home), employment status (retirement) and health (being diagnosed with dementia or a stroke) since the previous HRS wave in 2012. Baseline cognition controls are scores from tests of quantitative reasoning, retrieval fluency, word recall and the serial sevens test in 2012 and changes in these scores between 2010 and 2012. Interview controls are an indicator for a telephone interview, the number of call attempts until an interview was conducted, and interview week dummies. Location controls include the logarithm of the kilometer distance to the closest large city, Ebola search interest before the first US Ebola case, and media-market level relative search interest for the topics "anxiety" and "virus" in 2013. All control variables based on Google Trends data use the average rather than the median of 100 queries on the Google Trends website. The effective F-statistic for the first stage reported in the bottom of the table accounts for the use of cluster-robust standard errors. The weak-instrument-robust confidence set reported in the bottom of the table is constructed by inverting the cluster-robust version of an Anderson-Rubin test at the 5 percent level for the coefficient on *Ebola concern (average)*. * denotes $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$, all for two-sided hypothesis tests.

Table C.4. Estimates for the Subsample of Metropolitan Area Interviews

Dependent variable:	Ebola concern	Verbal reasoning score		
	First stage	Reduced form	IV	OLS
	(1)	(2)	(3)	(4)
Distance to closest Ebola location	−9.347***	2.061*		
	(2.739)	(1.235)		
Ebola concern			−0.220*	−0.255***
			(0.122)	(0.092)
Demographic controls	Yes	Yes	Yes	Yes
Baseline cognition controls	Yes	Yes	Yes	Yes
Interview controls	Yes	Yes	Yes	Yes
Location controls	Yes	Yes	Yes	Yes
Census region dummies	Yes	Yes	Yes	Yes
Observations	392	392	392	392
R^2 (adjusted)	0.643	0.459	0.465	0.467
Mean (dependent variable)	63.784	504.617	504.617	504.617
Effective F-statistic	11.647		11.647	
Weak-instrument-robust confidence set			[-0.524, 0.045]	

Notes: OLS, first-stage, reduced form and IV estimates for the subsample of HRS respondents who were interviewed in a metropolitan area in October 2014, with standard errors in parentheses. Standard errors are robust to heteroscedasticity and arbitrary intra-cluster correlation within media markets. *Ebola concern* is the average weekly relative search interest for the term "Ebola" in the media market of the interview location over all weeks between September 28 and the week of the interview. *Distance to closest Ebola location* is the logarithm of the kilometer distance to the closest location with a relationship to a US Ebola case that was publicly known on the day of the interview. Demographic controls include age, age squared and dummies for gender, race, education, and changes in household status (living alone, moving into a nursing home), employment status (retirement) and health (being diagnosed with dementia or a stroke) since the previous HRS wave in 2012. Baseline cognition controls are scores from tests of quantitative reasoning, retrieval fluency, word recall and the serial sevens test in 2012 and changes in these scores between 2010 and 2012. Interview controls are an indicator for a telephone interview, the number of call attempts until an interview was conducted, and interview week dummies. Location controls include the logarithm of the kilometer distance to the closest large city, Ebola search interest before the first US Ebola case, and media-market level relative search interest for the topics "anxiety" and "virus" in 2013. The effective F-statistic for the first stage reported in the bottom of the table accounts for the use of cluster-robust standard errors. The weak-instrument-robust confidence set reported in the bottom of the table is constructed by inverting the cluster-robust version of an Anderson-Rubin test at the 5 percent level for the coefficient on *Ebola concern*. * denotes $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$, all for two-sided hypothesis tests.

Table C.5. Alternative Instruments: Distance to the first US Ebola Location

Dependent variable:	Ebola concern	Verbal reasoning score	
	First stage	Reduced form	IV
	(1)	(2)	(3)
Distance to first Ebola location	−14.578***	4.600***	
	(1.326)	(1.503)	
Ebola concern			−0.316***
			(0.104)
Demographic controls	Yes	Yes	Yes
Baseline cognition controls	Yes	Yes	Yes
Interview controls	Yes	Yes	Yes
Location controls	Yes	Yes	Yes
Census region dummies	Yes	Yes	Yes
Observations	492	492	492
R^2 (adjusted)	0.758	0.434	0.437
Mean (dependent variable)	63.698	505.089	505.089
Effective F-statistic	120.790		120.790
Weak-instrument-robust confidence set			[-0.526, -0.121]

Notes: First-stage, reduced form and IV estimates for the main sample, with standard errors in parentheses. Standard errors are robust to heteroscedasticity and arbitrary intra-cluster correlation within media markets. *Ebola concern* is the average weekly relative search interest for the term "Ebola" in the media market of the interview location over all weeks between September 28 and the week of the interview. *Distance to first Ebola location* is the logarithm of the kilometer distance to Dallas, TX, the location at which the first US Ebola case was diagnosed on September 30. Demographic controls include age, age squared and dummies for gender, race, education, and changes in household status (living alone, moving into a nursing home), employment status (retirement) and health (being diagnosed with dementia or a stroke) since the previous HRS wave in 2012. Baseline cognition controls are scores from tests of quantitative reasoning, retrieval fluency, word recall and the serial sevens test in 2012 and changes in these scores between 2010 and 2012. Interview controls are an indicator for a telephone interview, the number of call attempts until an interview was conducted, and interview week dummies. Location controls include the logarithm of the kilometer distance to the closest large city, Ebola search interest before the first US Ebola case, and media-market level relative search interest for the topics "anxiety" and "virus" in 2013. The effective F-statistic for the first stage reported in the bottom of the table accounts for the use of cluster-robust standard errors. The weak-instrument-robust confidence set reported in the bottom of the table is constructed by inverting the cluster-robust version of an Anderson-Rubin test at the 5 percent level for the coefficient on *Ebola concern*. * denotes $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$, all for two-sided hypothesis tests.

Table C.6. Alternative Instruments: Distance to the closest of the first two US Ebola Locations

Dependent variable:	Ebola concern	Verbal reasoning score	
	First stage	Reduced form	IV
	(1)	(2)	(3)
Distance to closest Ebola location (excl. New York)	−12.173***	3.076**	
	(2.020)	(1.411)	
Ebola concern			−0.253**
			(0.109)
Demographic controls	Yes	Yes	Yes
Baseline cognition controls	Yes	Yes	Yes
Interview controls	Yes	Yes	Yes
Location controls	Yes	Yes	Yes
Census region dummies	Yes	Yes	Yes
Observations	492	492	492
R^2 (adjusted)	0.709	0.431	0.437
Mean (dependent variable)	63.698	505.089	505.089
Effective F-statistic	36.328		36.328
Weak-instrument-robust confidence set			[-0.474, -0.032]

Notes: First-stage, reduced form and IV estimates for the main sample, with standard errors in parentheses. Standard errors are robust to heteroscedasticity and arbitrary intra-cluster correlation within media markets. *Ebola concern* is the average weekly relative search interest for the term "Ebola" in the media market of the interview location over all weeks between September 28 and the week of the interview. *Distance to closest Ebola location (excl. New York)* is the logarithm of the kilometer distance to the closest location with a relationship to a US Ebola case that was publicly known on the day of the interview, excluding the location of the last US Ebola case diagnosed on October 23. Demographic controls include age, age squared and dummies for gender, race, education, and changes in household status (living alone, moving into a nursing home), employment status (retirement) and health (being diagnosed with dementia or a stroke) since the previous HRS wave in 2012. Baseline cognition controls are scores from tests of quantitative reasoning, retrieval fluency, word recall and the serial sevens test in 2012 and changes in these scores between 2010 and 2012. Interview controls are an indicator for a telephone interview, the number of call attempts until an interview was conducted, and interview week dummies. Location controls include the logarithm of the kilometer distance to the closest large city, Ebola search interest before the first US Ebola case, and media-market level relative search interest for the topics "anxiety" and "virus" in 2013. The effective F-statistic for the first stage reported in the bottom of the table accounts for the use of cluster-robust standard errors. The weak-instrument-robust confidence set reported in the bottom of the table is constructed by inverting the cluster-robust version of an Anderson-Rubin test at the 5 percent level for the coefficient on *Ebola concern*. * denotes $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$, all for two-sided hypothesis tests.